AMERICAN INVESTMENT COUNCIL
Growth Through Long-Term Investment

March 4, 2024

The Honorable Lina Khan
Chair
Federal Trade Commission
600 Pennsylvania Avenue NW
Washington, D.C. 20580

Dear Chair Khan:

I write on behalf of the American Investment Council (AIC), an advocacy and resource organization dedicated to developing and providing information to policymakers about the private investment industry and its contributions to both the long-term growth of the U.S. economy and retirement security of American workers. AIC's members include the world's leading private equity and private credit funds. In service of its mission, AIC has engaged with the Federal Trade Commission (FTC) regarding recent rulemaking proposals, filing several detailed comment letters aimed at correcting the Commission's apparent misimpressions about the private equity industry.[1]

The Federal Trade Commission has the important mission of supporting a vibrant economy that "protect[s] the public" from "deceptive or unfair business practices." We are writing to express our concern that the FTC is unfairly targeting productive private equity investments that support quality, affordable health care in the U.S. and fund the development of new treatments and cures. Several weeks ago, the Commission announced that it would host a virtual public workshop to discuss private equity in health care markets on March 5, 2024. AIC welcomes the Commission's engagement in this area. We are concerned, however, that the workshop will be little more than a series of one-sided presentations setting forth (to quote from the Commission's press release) the participants' "concerns" about the purportedly "harmful effects" of private equity investment.[2] The event page for the workshop explains that the Commission has "become increasingly concerned about the effects of private equity investment in [the health care] sector" and that it plans to feature those "who have experienced, first-hand, the effects of" that investment.[3] The implication is that these "experience[s]" will be uniformly negative.

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[1] American Investment Council, Comment Letter on Draft Merger Guidelines (Sept. 18, 2023), https://www.investmentcouncil.org/wp-content/uploads/2023/09/AIC-Merger-Guidlines-Commet-Letter.pdf; American Investment Council, Comment Letter on Proposed Revisions to Hart-Scott-Rodino Premerger Notification Requirements (Sept. 27, 2023), https://www.investmentcouncil.org/wp-content/uploads/2023/09/American-Investment-Council-Comments-re-Proposed-HSR-Amendments-9.27.2023.pdf.

[2] FTC, FTC to Host Virtual Workshop on Private Equity in Health Care (Feb. 14, 2024), https://www.ftc.gov/news-events/news/press-releases/2024/02/ftc-host-virtual-workshop-private-equity-health-care.

[3] FTC, Private Capital, Public Impact: An FTC Workshop on Private Equity in Health Care, https://www.ftc.gov/news-events/events/2024/03/private-capital-public-impact-ftc-workshop-private-equity-health-care.

Moreover, the Commission published an agenda and speaker list on February 28—less than a week before the workshop. Reviewing the listed topics and witnesses has confirmed AIC's initial impressions. Certain speakers consistently focus on a select few investments that have not succeeded instead of portraying the full benefit of private equity investments, which align quality, affordable health care with an economic benefit for millions of Americans who depend on private equity investment for well-paying jobs and retirement security. It appears that the FTC has not invited even a single speaker who can discuss the benefits of private equity investments in supporting quality, affordable health care, let alone a balanced number of speakers.

Private equity investments consistently support quality, affordable health care for patients across America. Private equity-backed hospitals employ a higher ratio of doctors, nurses, and pharmacists compared to their non-private equity-backed counterparts, according to research from Indiana and Georgetown Universities.[4] The same study found that wages increase significantly at private equity-backed hospitals. Private equity investments have enabled the development of treatments for several life-threatening conditions, such as Leukemia, Alzheimer's, heart disease, HIV, and breast cancer, and for several debilitating conditions, including rheumatoid arthritis, diabetes, and ulcerative colitis.[5] If the Commission wants solutions to protect American consumers, based on balanced data and information it should create a working group to bring together various voices to truly understand a policy issue.

AIC respectfully submits that the current FTC's blinkered approach is misguided. The Commission was established as an independent, nonpartisan agency which reflects Congress's expectation that its decisions will be grounded in impartial economic and policy analysis. Rather than send the message that it has already made up its mind, the Commission should instead use the workshop to solicit and engage with a diverse set of perspectives on the important questions at issue, many of which are empirical in nature.

AIC stands willing to assist the Commission in identifying witnesses who would speak to the many positive impacts of private equity investment in the health care sector. AIC would also be happy to direct the Commission to empirical work supporting those assessments. As a start, AIC is providing an extensive literature review that addresses the impacts of private equity investment in general and specifically in the health care sector.[6] For example, these materials explain how private equity investment has helped streamline health care costs and built out urgent care facilities in underserved rural communities.

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4 Gao, Janet and Sevilir, Merih and Kim, Yongseok, Private Equity in the Hospital Industry, Sept. 15, 2021, European Corporate Governance Institute – Finance Working Paper No. 787/2021, Available at SSRN: https://ssrn.com/abstract=3924517.

5 American Investment Council, Improving Medical Technologies: Private equity's role in life sciences, March 2022, Available at: https://www.investmentcouncil.org/wp-content/uploads/2022/03/aic-life-sciences-report2.pdf; and American Investment Council, Private Equity is Improving Health Care Across America, Available at: https://www.investmentcouncil.org/wp-content/uploads/2021/03/private-equity-health-care.pdf.

6 AIC Literature Review available here.

AIC trusts that the Commission's approach to these critical issues will reflect an evenhanded assessment of the data, rather than animus or preconceived notions about the effects of private equity investment in one of the nation's most important sectors.

Sincerely,

s/ Rebekah Goshorn Jurata

Rebekah Goshorn Jurata
General Counsel
American Investment Council

AIC Prepared Literature Review
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CHAPTER 3

Congressional request: Private equity and Medicare

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Congressional request: Private equity and Medicare

Chapter summary

In March 2020, the chairman of the Committee on Ways and Means asked the Commission to examine the role that private equity (PE) plays in the Medicare program. *Private equity* refers broadly to any activity where investors buy an ownership, or equity, stake in companies or other financial assets that are not traded on public stock or bond exchanges. One type of PE activity that has drawn growing attention in recent years involves investment firms that purchase companies and then try to improve their operational and financial performance so they can later be sold for a substantial profit. These types of acquisitions have become increasingly common in many parts of the economy, including the health care sector.

The advantages and disadvantages of PE investment in health care have long been a topic of debate. Supporters argue that PE firms improve the performance of the companies they acquire, generate better returns than other types of investments, and provide a way for health care companies to obtain capital. Opponents argue that PE firms can weaken the long-term health of the companies they acquire by weighing them down with debt, increase health care costs by using market power to obtain higher payment rates, and do little to improve quality.

In this chapter

Committee questions and our responses

What are current gaps in Medicare data that create issues in tracking private equity investments in Medicare? Are there levers that facilitate or allow for the collection of PE-related information in the current Change of Ownership (CHOW) process administered by the Centers for Medicare & Medicaid Services?

Understanding which individuals or entities own a Medicare provider and their track record of operations could help to improve oversight and safeguard patient care. Transparency of ownership information may help not only beneficiaries and their families as they select health care providers but also researchers as they analyze the effects of PE backing. CMS primarily collects data on provider ownership to support the enrollment process, payment, and fraud prevention, rather than research on the prevalence of different types of ownership. Observers have noted for many years that the ownership data submitted to CMS are incomplete and sometimes inaccurate. One particular obstacle is capturing accurate ownership data for providers (such as nursing homes and some hospitals) that are part of complex corporate structures with multiple levels and subsidiaries. As a result, CMS's ownership data typically do not indicate a parent organization atop a hierarchy of legal entities. More complete ownership data and greater transparency of ownership are highly important. However, under constrained resources, the feasibility of CMS identifying parent organizations for large numbers of Medicare providers and suppliers is a difficult challenge.

What are private equity funds' business models when investing in health care? How do these strategies vary by health care setting?

We examined PE business models in three key sectors: hospitals, nursing homes, and physician practices. PE firms have made investments in each sector but have a limited presence: We found that PE firms own about 4 percent of hospitals and 11 percent of nursing homes. We do not have a comparable figure for physician practices. At least 2 percent of practices were acquired by PE firms from 2013 to 2016, but that figure does not account for previous PE acquisitions and appears to have grown since then.

Because there is no single comprehensive source of ownership information, researchers compile data about PE ownership from proprietary datasets and public announcements. As a result, the estimated numbers of health care providers with PE backing are likely too low.

PE firms use several common strategies to make the providers they own in these sectors more profitable. Many of these strategies are also used by for-profit providers that are not PE owned. Some of those strategies focus on increasing revenues (such as providing more services, shifting toward a more highly compensated mix of services and procedures, or raising prices where possible), while others focus on reducing costs (such as taking advantage of economies of scale and lowering labor costs). Other strategies are more relevant to individual sectors, such as selling off a nursing home's real estate or creating larger physician practices by acquiring a large "platform" practice and then buying smaller practices in the same market.

How has private equity investment in health care affected Medicare costs and the beneficiary and provider experience?

For hospitals, where it was easier to identify the relatively small number of PE-owned facilities from public sources compared with other sectors, we found that PE-owned facilities tended to have lower costs and lower patient satisfaction than other for-profit and nonprofit hospitals. However, the differences among the three groups were relatively small and may not be caused by PE ownership.

For nursing homes, the research literature is somewhat dated and the findings on the effects of PE ownership on financial and quality of care indicators are mixed.

For physician practices, there is minimal peer-reviewed, empirical evidence of the impact of PE ownership on Medicare spending, quality of care, and patients' experience.

To what extent are private equity firms investing in companies that participate in Medicare Advantage, and is it possible to evaluate the effects of such investments on Medicare costs?

We found that PE funds own about 2 percent of the companies (6 out of 309) offering Medicare Advantage (MA) plans in January 2021. The plans offered by those PE-owned companies account for a little less than 2 percent of overall MA enrollment. We also identified another 25 companies that have received other types of PE investment, largely venture capital. These companies are often startup firms that focus exclusively on the MA program, and many target specific niche markets, such as beneficiaries living in nursing homes. This group of companies accounted for about 1 percent of overall MA enrollment.

In addition, PE firms (again, largely venture capital firms) have invested in a range of companies that work for MA plan sponsors. Many of these companies provide services or care management to enrollees, and several are paid using value-based contracts where they bear some financial risk for enrollees' overall health costs.

We did not find any research that examines the effects of PE investments in MA companies on Medicare costs, and we believe that such an analysis would be very difficult to conduct due to various data limitations. ■

Background

The term *private equity* (PE) refers broadly to any activity where investors buy an ownership, or equity, stake in companies or other financial assets that are not traded on public stock or bond exchanges. One type of PE activity that has drawn growing attention in recent years involves investment firms that purchase companies and then try to improve their operational and financial performance so they can later be sold for a profit. These types of acquisitions have become increasingly common in many parts of the economy, including the health care sector.

In March 2020, the chairman of the Committee on Ways and Means asked the Commission to examine the effects of private equity on the Medicare program. The request asked the Commission to answer four questions, to the extent feasible:

1. What are current gaps in Medicare data that create issues in tracking private equity investments in Medicare? Are there levers that facilitate or allow for the collection of PE-related information in the current Change of Ownership (CHOW) process administered by the Centers for Medicare & Medicaid Services?
2. What are private equity funds' business models when investing in health care? How do these strategies vary by health care setting?
3. How has private equity investment in health care affected Medicare costs and the beneficiary and provider experience?
4. To what extent are private equity firms investing in companies that participate in Medicare Advantage, and is it possible to evaluate the effects of such investments on Medicare costs?

This chapter provides our responses to the questions specified in the request. The request expressed interest in a quantitative analysis of the effect of PE ownership, if feasible, but this kind of analysis is often quite difficult to carry out due to the lack of good data about which providers are owned by PE firms, which we discuss in more detail in this chapter. As a result, the work in this chapter is based primarily on a combination of literature review and interviews with outside experts such as representatives of PE firms, researchers, and consultants.

What do we mean when we use the term *private equity*?

The term *private equity* refers broadly to any activity where investors buy an ownership stake, or equity, in companies or other financial assets that are not traded on public exchanges like the stock and bond markets.[1] The term sometimes generates confusion because it encompasses a wide range of investment activities that can differ in important respects. For example, the financial sector considers all of the following types of investment to be private equity:

- ***Venture capital (VC)*** involves investments in startup companies that are developing new technologies or business models. These companies often need capital for activities such as research and development, but they have not yet demonstrated that they can be profitable and thus cannot obtain capital by borrowing from a bank or issuing bonds. VC investors provide capital for startup companies in exchange for a partial ownership stake. These investments carry a high degree of risk since the companies involved are new and unproven, but VC investors can earn significant profits from companies that later become successful.
- ***Growth capital*** involves investments in companies that have moved beyond the startup phase—they have demonstrated that they can be profitable—but need capital to expand their operations. As with VC, growth capital investors typically receive a partial ownership stake when they invest in a company (although some may purchase a majority stake), and the company's existing management usually remains in place. However, these investments are considered less risky than venture capital because they involve companies that have shown their viability.
- ***Buyouts*** involve investments in established companies, which can be either privately owned or publicly traded. Unlike the two categories above, buyout funds purchase at least a majority ownership stake when they invest in a company. When a buyout fund takes full ownership of a company that had been publicly traded, the company is "taken private," meaning that it becomes a privately owned entity and its shares are no longer bought and sold on the stock market. The buyout fund takes full control of the company and can either retain or replace the company's management. In many instances, the

company's management team will also take a partial ownership stake. Buyout funds will spend some of their own money to buy a company, but they usually finance more than half of the cost of the acquisition by borrowing money. The use of borrowed money, or debt, to help finance an investment is often referred to as leverage because it allows the borrower to use less of its money to make a given investment, which potentially enables the borrower to earn much greater returns (while also potentially exposing the borrower to much greater losses). Since buyout funds rely heavily on borrowed money to purchase a company, their acquisitions are sometimes referred to as leveraged buyouts.

Within the health care sector, the growing prominence of PE firms in recent years largely reflects the actions of companies that have been acquired through buyouts. For example, some of the physician staffing companies that have engaged in the controversial practice of "surprise billing," where providers such as emergency department (ED) physicians and anesthesiologists bill for services using out-of-network rates, have been owned by PE funds that pursue buyouts. As a result, we focused primarily on buyouts in responding to the congressional request and will use the term *private equity* to refer to them specifically unless noted otherwise.

Private equity investments have been growing

The amount of public equity in the U.S. dwarfs the amount of private equity. In 2019, public market capitalization totaled over $37 trillion, compared with aggregate North American PE assets under management—including buyouts, venture capital, growth capital, private debt, real estate, and other types of investments—of about $3 trillion (McKinsey & Company 2020, Siblis Research 2020). (Those figures pertain to the overall economy, not just the health care sector.) Stock exchanges remain the key source of investment funds among very large corporations and growth companies with large capital requirements because public exchanges have been perceived as the lowest cost way to access sizable amounts of financing (Moon 2006, Rosov 2018).

Nevertheless, over the past several decades, the importance of private equity in the U.S. economy has grown dramatically. Between 1996 and 2012, the number of companies listed on U.S. public stock exchanges fell from more than 8,000 to about 4,100 (Doidge et al. 2017).[2] Meanwhile, between 2006 and 2017, the number of PE-backed U.S. firms grew from around 4,000 to about 8,000 (McKinsey & Company 2019). One reason for the decline in public listings is that the average size of listed firms increased. However, the trend also reflects the fact that listing one's company on a public exchange may no longer be as important for obtaining access to capital as in prior years.

Buyouts are the leading category of PE investment. As of 2019, total North American PE buyout assets under management totaled $1.24 trillion—nearly three times the size of venture capital, the next-largest category (McKinsey & Company 2020). PE firms have been around since at least the 1970s, but the use of leveraged buyouts as a method of acquiring companies first became more noticeable in the 1980s (Kaplan and Stromberg 2009). The crash of junk bonds in the late 1980s and early 1990s led to the default of a few high-profile firms acquired using leveraged buyouts, and there were few PE acquisitions of publicly traded companies in the 1990s. Nevertheless, PE firms continued to purchase divisions of public firms and private companies. After declining in the early 2000s with the collapse of the "dot-com bubble," PE buyouts of public firms reemerged in the mid-2000s.

Several reasons account for the rise of PE leveraged buyouts. First, the use of debt (borrowed money) has had a lower cost of capital than investor equity because of lower risk and because interest payments on loans can be deducted from corporate income taxes.[3] Interest rates have also remained low since the 2008 financial crisis. Relative to publicly traded markets, private investments (including PE buyouts) are subject to fewer disclosure and regulatory requirements of securities law. Further, under accounting rule changes, public and private pension funds have been required to recognize their unfunded liabilities, many of which are substantial. To help make up those shortfalls, some pension funds have sought investments with higher returns, and PE firms have been perceived as offering such returns. PE investments have also been seen as a way to diversify the portfolio of institutional investors such as pension funds.

Key elements of the private equity model

The PE firms that specialize in buyouts vary greatly in size and in the types of companies that they purchase, but they nonetheless have a number of common features, and their investment activities follow a distinctive life cycle. In this section, we briefly outline the basic elements of the PE model.

Raising money from investors

The life cycle of private equity investment begins with a PE firm raising money from outside investors and pooling it into an investment fund. Each investment fund operates for a specific period of time, usually around 10 years (Mercer 2015). Most PE firms raise money for new investment funds every few years and thus oversee multiple funds. According to one report, PE firms managed an average of 4.5 funds in 2019 (Bain & Company 2020b).

The Securities and Exchange Commission (SEC) limits participation in PE funds to "accredited" and "qualified" investors—including institutional groups such as pension funds, university endowments, foundations, banks, and insurance companies, as well as individuals who meet asset, income, or other criteria that deem them sophisticated enough to not need the protections provided by the registration and disclosure requirements of publicly traded companies (Securities and Exchange Commission 2020a). Institutional investors account for more than 90 percent of the money invested in PE funds (Securities and Exchange Commission 2021). PE funds are subject to fewer regulatory requirements than other parts of the financial sector—for example, under an exception to a 1982 rule, funds that are limited to accredited investors received safe harbor from registration requirements for securities offerings (De Fontenay 2017). The SEC's limits on participation in PE funds are based on the rationale that the ability to invest in PE funds should be restricted to relatively sophisticated groups that can better assess the potential risks and rewards of these types of assets. In addition, PE funds often require investors to contribute a substantial minimum amount, which can range anywhere from $100,000 to $10 million or more depending on the size of the fund (Jones 2018). The median amount of time that PE firms needed to raise money for the investment funds that were launched in 2019 was 10.5 months (Bain & Company 2020b).

When investors participate in a PE fund, they agree to provide a specified amount of money to support the fund's investment activities and operating costs. The investors do not provide this money upfront. Instead, the PE firm periodically makes "capital calls" that require investors to provide funding when the firm is ready to make a specific investment. Investors usually have 10 days to provide the money (Altegris Advisors 2019). As a result, a significant portion of the money that has been pledged to a PE fund may not be in use at a given point in time, especially in the early years of a fund's life span. Investors cannot withdraw their money from a PE fund before the end of the fund's life span, which makes PE funds a much more long-term and illiquid (i.e., difficult to convert to cash) form of investing compared with traditional stocks or bonds.[4]

In 2019, PE firms operating in the U.S. raised a total of $301 billion across 202 investment funds, for an average size of $1.5 billion. However, that average is inflated because it includes six "mega funds" that each raised more than $10 billion. The average size of the funds that were launched between 2016 and 2018 was smaller, around $900 million (Lykken 2020).

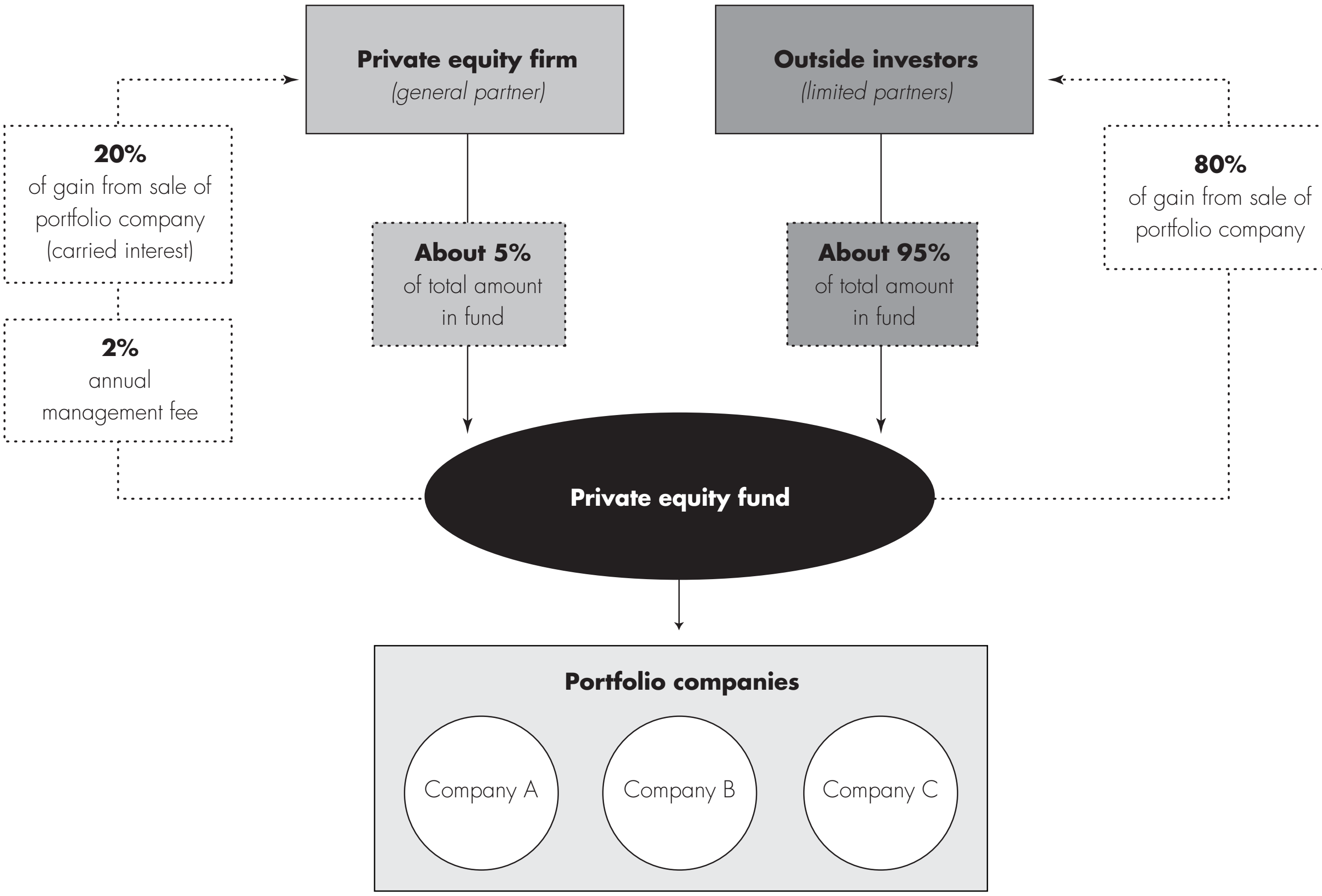
PE funds are structured as limited partnerships, with the PE firm typically serving as the fund's general partner (GP) (Figure 3-1, p. 78). The legal agreement that governs the partnership may set broad guidelines about the fund's investment activity (for example, requiring it to invest in a mix of economic sectors and geographic regions), but within that framework the GP has broad control over the fund's activity (Altegris Advisors 2019). The GP also invests some of its own money in the fund, usually between 1 percent and 5 percent of the overall total (Jacobius 2017). The fund's outside investors serve as limited partners; although they account for the vast majority of the money committed to the fund, they are passive investors and play no role in the fund's activities.

Buying and selling portfolio companies

Once a new investment fund has been set up, the PE firm that manages the fund buys and sells companies with the goal of improving their operational and financial performance, increasing their value, and later selling them for a profit (Figure 3-2, p. 79). Once these companies have been acquired, they are referred to as portfolio companies. These acquisitions usually occur during the first three to five years of a fund's life span, which is often called the investment period.[5] PE firms will often make between 10 and 20 acquisitions during a fund's life span, with the fund's rules typically barring the firm from using more than 15 percent to 20 percent of the overall capital for any one investment (Witkowsky 2020). The amount spent on a single acquisition can vary anywhere from less than $25 million to billions of dollars (Mercer 2015). Many acquisitions in health care are relatively small and fall below the threshold where parties to a merger or acquisition must report their plans to federal antitrust authorities before completing the transaction.[6]

FIGURE 3–1

Typical structure of a private equity fund



Source: MedPAC analysis.

PE firms rely heavily on borrowed money to finance their acquisitions. Depending on the permissiveness of the lending environment, borrowed money can account for as much as 70 percent of the cost of an acquisition (Mercer 2015). The PE fund provides the remaining amount. In a typical leveraged buyout, the assets of the company that is being acquired are used as collateral for the loan, and the company that is being acquired, rather than the PE firm or the PE fund, becomes responsible for making payments on the loan once the buyout is completed.

PE firms prefer using borrowed money instead of the investment fund's capital for two reasons. First, borrowing money magnifies the potential return on an investment because the PE fund can use less of its money to acquire a company while still generating a comparable profit from its eventual sale. (Borrowing money also magnifies the potential losses from an investment, but one controversial feature of PE funds is that they are not usually responsible for the debts of their portfolio companies in a bankruptcy. This arrangement lets PE funds reap the benefits of using borrowed money while limiting their exposure to the capital they have invested in the portfolio company.) Second, the corporate income tax provides an incentive to borrow money because the costs of servicing debt reduce a company's tax liability.

FIGURE 3–2

Typical life cycle of private equity investments

Note: PE (private equity).

Source: MedPAC analysis of Government Accountability Office. 2010. *Nursing homes: Complexity of private investment purchases demonstrates need for CMS to improve the usability and completeness of ownership data.* GAO–10–710. Washington, DC: GAO.

Since PE firms acquire companies during the first 3 to 5 years of an investment fund and must sell the companies before the fund reaches the end of its life span (usually 10 years), a PE firm will usually control a portfolio company for somewhere between 3 and 7 years. During this time, the PE firm will try to improve the portfolio company's operational and financial performance—for example, by increasing its revenues or lowering its costs. Since the PE firm owns the portfolio company (or at least a majority stake), the PE firm has a much greater degree of control than it would with a partial ownership stake in a publicly traded company and can make significant changes to the portfolio company's management team and/or business strategy (Mercer 2015).

Once an investment fund enters the second half of its life span, the PE firm's attention begins to shift from buying portfolio companies to selling them. This phase is sometimes known as a fund's liquidation period. There may not be a clear boundary between the end of the investment period and the start of the liquidation period; a fund might acquire one company while selling another company. The sale of a portfolio company usually happens in one of four ways:

- the PE fund sells the company to a strategic acquirer (such as a competing company in the same industry);
- the PE fund sells the company to another PE investment fund;
- the PE fund converts the company into a publicly traded entity through an initial public offering of stock (which then allows the PE fund to sell its shares in the company); or
- the portfolio company repays the PE fund for its investment (effectively buying itself back from the PE firm, often by borrowing money) (Altegris Advisors 2019).

Once a portfolio company has been sold, the PE fund typically distributes the proceeds to the fund's investors instead of reinvesting them, even if the fund has not yet reached the end of its life span. Although PE firms aim to achieve substantial returns for their investors, the profits (or losses) from the sale of an individual portfolio company will depend on the extent to which the PE firm was able to improve the company's performance and find an attractive exit.

PE firms may also employ strategies that generate profits from portfolio companies before selling them. For example, the PE firm might require a portfolio company to complete a dividend recapitalization—where the company borrows money and uses the proceeds to make a special dividend payment to its owners (i.e., the investors in the PE fund). Another strategy is to direct the portfolio company to sell some of its real estate holdings and distribute some of the proceeds from the sale to the PE fund's investors. This strategy has been used in several

PE investments in the hospital and nursing home sectors. A third strategy is to require the portfolio company to pay substantial management or consulting fees to the PE firm or a related subsidiary. Although these strategies can enable a PE fund to generate some profits well before a portfolio company is sold, they have also been criticized for weakening the underlying financial health of portfolio companies (Appelbaum and Batt 2020, Coleman-Lochner and Ronalds-Hannon 2019, Whoriskey and Keating 2018).

Critics have argued that PE ownership can be harmful to companies because PE firms typically own the companies for a relatively short period of time and require them to take on more debt. These features, they suggest, give PE firms an incentive to focus on strategies that generate short-term profits but may weaken a company's long-term health. In contrast, the PE firm representatives that we interviewed argued that, relative to publicly traded companies and their focus on quarterly earnings, PE firms can be more flexible and nimble, and are often "patient capital" that make it easier for companies to pursue strategies that may take time to fully pay off. These representatives also said PE firms do not want to undermine their companies' long-term health because that would make it harder to sell them for a profit.

PE firms are typically paid based on the "2 and 20" model

The limited partners in a PE investment fund (the outside investors) have traditionally paid the general partner (the PE firm) for managing their investments using an approach known as the "2 and 20" model. The PE firm receives two types of payments under this model.

The first payments are annual management fees that equal 2 percent of the total amount that investors have committed to the fund (Altegris Advisors 2019). However, these fees may be somewhat lower for large investment funds and funds managed by PE firms with weaker track records (Khoury and Peghini 2019). Once the investment period ends, these fees may also decrease because they may be based on the amounts the fund currently has invested, rather than the amounts that were originally committed (Mercer 2015).

The second payments are a share of the profits that the PE firm receives when it sells one of the fund's portfolio companies. These payments are frequently referred to as "carried interest" and typically equal 20 percent of the profits from the sale.[7] However, the PE firm does not receive carried interest unless the profits exceed a minimum threshold, which is known as the hurdle rate and typically ranges from 6 percent to 10 percent (Altegris Advisors 2019). These payments appear to account for most of the profits that PE firms receive.

Returns on private equity are similar to returns from mutual funds that invest in smaller companies

There is a debate as to whether PE investments have historically generated better returns than investments in publicly traded stocks. For example, one study found that PE funds outperformed public equity before 2006 by 3 percent to 4 percent (Harris et al. 2015). However, another study recently argued that the higher return may just be a function of the comparison group, and it found that the premium is diminished if the comparison group consisted of smaller companies rather than index funds of large corporations (Phalippou 2020). While there is disagreement regarding the historic premium earned by PE before 2006, there is greater agreement that PE returns have been similar to public equity returns over the past decade. For example, the PE firm Bain Capital recently reported that "Since 2009, when the global economy limped out of the worst recession in generations, U.S. public equity returns have essentially matched returns from U.S. buyouts at around 15%" (Bain & Company 2020b). Phalippou also found similar returns for private and public equity in recent years (Phalippou 2020).

The decline in PE returns relative to public equity should not be surprising. Because of a historical perception that PE had higher returns (and provided additional portfolio diversification), there was a large expansion in institutional investments in PE funds. Institutional investors wanted to replicate the success of some high-profile PE investors such as the Yale University Endowment (Bary 2019). As the amount of capital searching for acquisitions grew, the prices paid for companies (expressed as a multiple of their cash flow) increased (Bain & Company 2020b). As the purchase price increases, the expected return should decrease relative to alternative investments. Despite the lack of superior returns in recent years, institutional investors continue to allocate dollars to PE funds, resulting in PE firms holding "record levels" of uninvested capital (known as "dry powder") (Bain & Company 2021).

The similarity in the returns for private and public equity raises the question of why investments in PE funds have continued to grow. One possible explanation is that PE fund performance varies widely, with funds in the top quartile performing significantly better than the median or average PE fund (Bain & Company 2020b, Mercer 2015). Some PE firms have shown that they can consistently generate above-average returns, and those firms appear to be attracting an increasing share of the money being committed to PE investment funds (Bain & Company 2021, Bain & Company 2020b).

PE involvement in the health care sector has been growing

While PE buyouts have been evident in the economy since the 1980s, their role in health care became more noticeable only over the past two decades. More recently, the share of PE deal values devoted to health care buyouts in 2019 was roughly proportional to health care's share of the U.S. gross domestic product. One major PE firm estimates that in 2019, PE buyout deals involving North American health care providers totaled $46.7 billion, up from $29.6 billion in the prior year (Bain & Company 2020a).

Purchases of and investments in health care providers accounted for about 60 percent of all health care–related buyout transactions in 2019—96 deals, up from 84 in 2018 (Bain & Company 2020a). PE funds invested in retail health; behavioral health and substance abuse centers; home health and hospice care; and physician practice management in specialties that have been more fragmented, such as radiology, gastroenterology, ophthalmology, and dermatology. After PE deals involving providers, the most common transactions involved buyouts of biopharma-related firms, medical technology firms, and companies that provide services to health plans. Health care information technology was also the focus of many buyout deals, including firms that facilitate pharmaceutical drug trials, develop electronic health record software for behavioral health, and oversee revenue cycle management (e.g., debt collection).

One major reason health care has become a focus of PE investment in the U.S. is the projected demand for services related to the aging population. Before the current pandemic, the combination of stable and often growing demand for health care, the use of insurance, and the prominence of fee-for-service (FFS) payment meant predictable cash flow to health care providers. Meanwhile, the fragmented structure of categories of health care providers (such as certain specialists) and changes in technology make health care an investment target for PE funds that can consolidate providers into larger bargaining entities. The growth of PE investment has also been driven by an extended period of low interest rates, which has encouraged investors to find other ways to generate attractive returns.

Many Medicare providers have complex business structures that make it difficult to identify ownership and control

Understanding which individuals or entities own a Medicare provider and what their track record of operations is could help to improve oversight and safeguard patient care. Transparent ownership information may also help beneficiaries and their families as they select health care providers. In particular, safety, quality, and compliance with federal regulations at nursing homes have been longstanding problems, and some operators have been repeat offenders in providing substandard care (Hawes et al. 2012).[8] Today, about 60 percent of nursing homes are owned by chains (primarily smaller, regional for-profit entities), and PE firms own approximately 11 percent of facilities (Harrington et al. 2021).[9] Changes over time in how providers structure their organizations have made it difficult to identify nursing homes' owners or chains with common underlying ownership which, in turn, makes it difficult to enforce regulations (Wells and Harrington 2013).

In the request, the Commission was asked to identify gaps in Medicare data and in CMS's Change of Ownership (CHOW) approval process that make it difficult to track PE investments. Here we review CMS's enrollment process and the information it collects in the Provider Enrollment, Chain, and Ownership System (PECOS), including CHOW data.

CMS collects data on provider ownership for Medicare's enrollment process. Data from PECOS are used to support payment, fraud prevention, and law enforcement, but also to populate other data sets such as CMS's public provider enrollment files and consumer provider comparison tools. CMS has not typically used PECOS data for program analysis or to research the prevalence of ownership types such as private equity. Applicants self-report ownership details to PECOS and CMS has no centralized data source with which to verify that information. As a result, there have been longstanding issues associated with the accuracy and completeness of PECOS's ownership data.

Across many types of owners, health care providers and suppliers have changed the ways in which they structure themselves so as to limit their legal liability. Providers that have common ownership are now structured in ways that do not make this ownership obvious. Thus, it is extremely difficult to capture within a data set and lay out an ownership hierarchy among a web of interrelated entities, and CMS's ownership data typically do not indicate a parent organization atop a hierarchy of legal entities.

We were able to identify PE investors in PECOS data for some providers but not for others. When we were able to identify PE ownership, it was because we had information from public data sources such as research reports or websites that identified PE relationships. Typically, the names of PE-backed portfolio companies were listed as owners rather than the PE funds themselves. We cannot say whether enrollment information for providers with PE investors is more complete and accurate, less so, or similar in its completeness and accuracy compared with providers that do not have PE backing.

Medicare's process for enrolling providers and suppliers

One way for CMS to protect beneficiaries and reduce improper Medicare payments is to have strong safeguards for enrolling or contracting with providers and health care organizations. CMS enters into contracts with MA plan sponsors and the agency enrolls FFS Medicare providers and suppliers. Under the MA program, private plan sponsors sign contracts with CMS that identify the parent organization that will bear risk for plan members' medical spending. Sponsors must verify that information annually. A sponsor must also provide evidence of insurance licenses that demonstrate that the states in which it operates believe the company has sufficient financial assets to bear the risk. Under traditional, or FFS, Medicare, the program typically does not require providers to bear risk, and CMS enrolls many times more providers than MA has plan sponsors.[10]

To become an FFS provider or supplier, a health care entity or individual practitioner must apply to enroll in Medicare, undergo background reviews and/or certification surveys, and be approved to receive a Medicare billing number. (CMS refers to facilities that bill Medicare under Part A, such as hospitals and skilled nursing facilities, as "providers." Physicians, physician group practices, and other entities that furnish services under Medicare Part B are called "suppliers.") Providers and suppliers apply online through PECOS or by paper to their appropriate Medicare administrative contractor (MAC) or the National Supplier Clearinghouse (NSC).[11] Most types of institutional providers and certain organizations that bill under Part B (such as ambulatory surgical centers) must be surveyed by state agencies or an approved accreditation organization, which then makes recommendations about approval to CMS's regional offices (ROs). CMS ROs make the final decisions regarding eligibility for Medicare billing. Enrolled providers and suppliers must generally resubmit and recertify the accuracy of their enrollment information to CMS every five years or upon CMS request to retain billing privileges (called "revalidation").[12]

All Part A providers and Part B suppliers must report to CMS within 30 days any change in ownership or in control of the provider. However, Part A providers and certain Part B suppliers (such as ambulatory surgical centers that are subject to survey and certification) may need to update their PECOS data through the CHOW process. CMS defines CHOWs differently depending on the type of legal entity involved.

- In partnerships, CHOWs include the removal, addition, or substitution of a partner as permitted under state law.
- In sole proprietorships, CHOWs include transfer of title and property to another party.
- In corporations, a CHOW is typically the merger or consolidation of the provider corporation with another organization that leads to the creation of a new corporation. A corporate asset transfer would be considered a CHOW, but the transfer of corporate stock into an existing provider corporation would not.

A CHOW usually results in the transfer of the provider's Medicare billing number and provider agreement to the new owner.[13] Typically, there is also a change to the provider's tax identification number. Both the buyer and seller must report the CHOW through PECOS, and the transaction must be approved by the applicable CMS RO. If approved, CMS automatically reassigns the provider's Medicare number to the new owner unless the buyer rejects assignment in its filing.[14] After the CHOW registration is complete, only the buyer is permitted to submit claims to Medicare. Failure to report a transaction in a timely manner can result in the deactivation of billing privileges or the entire revocation of the provider's Medicare number.

Medicare Part B suppliers that are not subject to survey and certification requirements (such as physician group practices) do not undergo or register CHOWs, but they must still report changes in ownership as changes to the PECOS information within 30 days.[15] In the event of, say, the sale of a group practice, the purchaser must enroll as a new Part B supplier to receive its own Medicare billing number.

The Affordable Care Act of 2010 (ACA) included provisions that permitted CMS to screen providers and suppliers more closely and aimed to increase ownership transparency, particularly for nursing homes.[16] Section 6101 of the ACA expanded reporting requirements for the identities of direct and indirect controlling interests in the operations and management of skilled nursing facilities and nursing facilities (Hawes et al. 2012, Maxwell 2016). The ACA provisions also aimed to provide consumers with greater transparency about ownership on lookup tools such as CMS's Care Compare (https://www.medicare.gov/care-compare/).

Today, not only nursing homes but most categories of facilities and physician groups must report within PECOS every individual or organization with: (1) at least a 5 percent direct or indirect ownership interest or managerial control (including providers' mortgage holders); (2) any general or limited partnership interest; or (3) operational or managerial control. In addition, corporations must report all officers and directors. Applicants for initial Medicare enrollment or revalidation are required to submit a diagram of the entity's organizational structure, identifying the relationships among entities with ownership or managerial interests (Centers for Medicare & Medicaid Services 2020). Under a recent program integrity rule, CMS's authority was expanded to revoke or deny Medicare billing privileges to providers based not only on certain adverse actions conducted by a provider or supplier itself but also on actions by its affiliations—including those with 5 percent or more direct or indirect ownership, a general or limited partnership interest, those with day-to-day managerial control, and corporate officers or directors (Centers for Medicare & Medicaid Services 2019).[17]

Changes in the structure of health care organizations

Just as the legal structure of a corporation shields its shareholders and officers from the corporation's liabilities, many health care businesses have restructured themselves to do the same. Over the past several decades, an increasing number of nursing homes, hospitals, and other providers have restructured from one organization into several single-purpose entities (SPEs) that permit investors to pool resources while limiting their liability (Casson and McMillen 2003). For example, a health system with several hospitals might register each hospital as its own limited liability company (LLC) to curb potential effects on the entire system when there is litigation against one hospital for harm or malpractice. One attorney we interviewed referred to this strategy as the "taxi cab model" in which each cab is registered as its own LLC to prevent a plaintiff from suing the entire fleet.

Nursing homes are especially reliant on Medicaid and Medicare payments for the bulk of their revenues. Enrolling each facility in a chain as its own LLC limits the risk to the entire chain if CMS excludes one facility from the programs. The owner could sell the one facility without devaluing the others. Attorneys have advised nursing home owners to establish SPEs for their facilities' real estate separately from companies that lease and operate facilities because "numerous SPEs may be less attractive as defendants than a single company with multiple operating interests and multiple real estate holdings" (Casson and McMillen 2003). Different companies use different restructuring approaches. Some subdivide down to two SPEs for each facility (an operator and the owner of real estate), while others form subsidiaries to jointly hold the real estate or operating companies for several facilities. Since 2008, real estate investment trusts have formed that hold diverse portfolios of nursing home properties as well as the properties of assisted living facilities, hospitals, ambulatory surgical centers, and medical offices. Some owners of Medicare providers also own related-party companies that provide services to the facilities under contract. In addition, it is common for nursing home owners to hire management companies as contractors to operate the facility on their behalf.

Many providers with and without PE ownership have restructured health care businesses in these ways. However, PE funds may be more likely than less financially savvy owners to protect their investments through restructuring.

Based on our interviews with attorneys who advise PE investors, some stakeholders believe that CMS's enrollment system displays a lack of understanding about how health care providers are structured today.

For example, in the case of PE funds, identifying all individuals with an ownership stake of at least 5 percent would include limited partners such as pension funds and wealthy individuals even though they are typically passive investors. Meanwhile, if a nursing home owner awarded a management contract and gave the contractor wide latitude over day-to-day operations, the owner would be required to submit updated enrollment information but the update would not prompt as much review as a CHOW (Markenson and Woffenden 2019). As another example, health care providers have restructured into LLCs, which have characteristics of both partnerships and corporations. Medicare guidance lays out what defines a CHOW for partnerships and corporations, but does not formally address how to treat LLCs.[18] In the opinion of some interviewees, CMS needs to make its enrollment applications and instructions clearer about what constitutes a CHOW for businesses as they are structured today.

Gaps in data about ownership of Medicare providers

For many years, the Department of Health and Human Services Office of Inspector General (OIG) has found PECOS's ownership data incomplete and sometimes inaccurate (Maxwell 2016). Providers and suppliers self-report ownership to PECOS and CMS has no central data source with which to verify the information. OIG attributes PECOS's shortcoming in part to gaps in the efforts of the MACs and the NSC to verify key pieces of provider information during the enrollment and revalidation processes (Office of Inspector General 2016). According to an attorney we interviewed who counsels providers on regulatory filings, applicants sometimes provide incomplete information about ownership and management interests. Unless the MACs know what to look for and follow up to ask, applicants do not volunteer more information. In addition, because providers often use a complex structure of LLCs, the hierarchy of control and nature of relationships among related parties can be hard to unpack. A 2010 study by the Government Accountability Office (GAO) found that for nursing homes with common chain ownership, PECOS did not capture the hierarchy of control among their interrelated LLCs (Government Accountability Office 2010). Our own look at current PECOS data for various providers—including some with and others without PE backing—confirmed that the same issues persist. (See text box for an example of the structure of one hospital chain.)

States have their own processes for licensing providers and enrolling them for the administration of Medicaid and other programs. While a few states have more extensive transparency requirements around ownership, many do not.[19] One issue commonly raised is that as one state enrolls a provider, it may not know of deficiencies at facilities in other states that have common ownership. One state licensing and certification official we interviewed told us that his state focuses on verifying information for a provider's operating company, not the owner of the real estate or the management company. He noted that his office simply does not have the resources to track down all organizations and individuals that have a direct or indirect ownership stake or a role in managing facilities. In his experience, he had been able to devote attention to tracking down ownership details only when facilities provided systematically poor care and received deficiency violations or when facilities experienced financial distress.

Because of recent high-profile bankruptcies of nursing home chains affecting facilities in several states, some state governments have taken steps to tighten requirements for licensing and disclosure. For example, in 2019, Kansas passed a law requiring applicants for nursing home licenses to disclose "every other licensed property he or she owns or has ever owned, either within Kansas or elsewhere in the United States" (Spanko 2019). The law applies to ownership stakes in both operating and real estate companies. That same year, Ohio put regulations in place requiring more disclosure about a nursing home license applicant's financial status and history (Flynn 2019). We do not yet know about the effects of those changes. One state—Virginia—has long required audited financial statements and cost reports from nursing home licensees.

Researchers, advocates, and policymakers have pressed for policies to improve the information on health care provider ownership, with the goal of making it more understandable, accurate, and available to consumers, regulators, and researchers. For example, in the wake of the coronavirus pandemic and the devastating effects it has had on nursing home residents and staff, a group of nursing home experts made several recommendations "to make ownership, management, and financing more transparent and accountable to improve U.S. nursing home care" (Harrington et al. 2021). Among their recommendations were for CMS to "augment PECOS reporting to include all parent, management, and property companies, and other related party entities and ensure

Example of a hospital chain's complex ownership structure

Some providers have complex ownership structures and related-party transactions. In the hospital-chain example that follows, we are not aware of any ownership by PE investment funds. Nevertheless, the case demonstrates how ownership, managerial control, and cash flow among related parties can be difficult to track.

Prime Healthcare Services Inc. (PHS) is a privately held for-profit company founded in 2001 that operates a chain of 31 acute care hospitals. The founder, Dr. Prem Reddy, also formed Prime Healthcare Foundation (PHF), a nonprofit entity that operates 15 hospitals donated to PHF by PHS. Some suggest the PHS strategy is to acquire and improve the profitability of financially distressed or underperforming emergency department–centered hospitals in or near large metropolitan areas (Al-Muslim 2020, FitchRatings 2020).

Members of the same family control PHS's for-profit hospitals, PHF's nonprofit hospitals, management companies that provide services to the hospitals, and real estate companies leasing facilities to the hospitals (Prime Healthcare Foundation 2019). PHS holds variable interest in medical groups and owns subsidiaries Prime Healthcare Management Inc. (PHM) and Prime Healthcare Management II Inc. (PHM II). The latter two entities provide management, consulting, and support services to hospitals owned by PHS and PHF (Department of Justice 2018). Prime A, a company with ownership in common with PHS, holds title to two hospital facilities and leases them to PHS (Ernst & Young 2019). Prime A also rents property to PHM. PHS and PHF purchase services from three other related parties: Bio-Med Inc. (which repairs and maintains medical equipment), Hospital Business Services (which provides administrative services), and PrimEra Technologies (which provides coding and revenue cycle management services).

For this case, Provider Enrollment, Chain, and Ownership System data we examined could not provide sufficient detail to understand the various Prime relationships or hierarchy of control. Instead, the information we found came from various public disclosures around financial transactions and a settlement agreement. Indeed, it would be difficult to construct a government database that captures the entirety of these ownership relationships and related-party transactions. It is also possible that any rules set up to limit types of ownership could be circumvented through contracts with related entities that provide real estate or management services. ■

enforcement of Section 6101 of the ACA, including that companies provide a complete organizational chart." They also called for more scrutiny of the ownership and management of nursing homes at purchase or when there is a CHOW by recommending that CMS specify minimum federal criteria that would "prevent individual or corporate owners from purchasing, operating, or managing additional facilities if they have a history of owning or operating other facilities with chronically low staffing and poor-quality care in any state."

Access to more complete ownership data and a clearer line of sight into the top of a provider's or supplier's ownership hierarchy are important for several reasons. First, such information could improve CMS's ability to evaluate the past business conduct of a parent organization across all the providers and suppliers it owns as the agency decides whether to extend billing privileges. Making ownership data available to researchers would improve their ability to analyze whether factors such as PE ownership affect health care spending, access, and quality of care with more confidence than they do today. Greater ownership transparency may also be useful to consumers as they choose where to seek care. However, given constrained resources and complex ownership structures, CMS and state agencies may find it infeasible to identify parent owners for the large number of providers and suppliers that enroll in Medicare. Legal structures may continue to evolve in ways that make it difficult to trace ownership, and privacy protections also limit the amount of ownership information that CMS is permitted to make public.

Business models for PE investments in health care

All PE firms try to generate profits by using the same basic strategy: identify and acquire undervalued or underperforming companies, make them more valuable by improving their operational and financial performance, and then sell them after three to seven years for a profit. However, there is often little publicly available information about the business models that PE firms use to increase the value of their portfolio companies since those companies are privately held and are not subject to the disclosure requirements that apply to publicly traded companies.

We relied on a combination of literature reviews and interviews with outside experts (such as representatives of PE firms, physicians, consultants, and researchers) to examine the business models that PE firms use when they invest in three types of health care providers that are particularly significant to Medicare beneficiaries: hospitals, nursing homes, and physician practices. Given the breadth of PE investment in the health care sector, our findings are necessarily somewhat qualitative and difficult to generalize to other types of providers.

Private equity has invested in all three sectors but has a limited presence

We found that PE firms have acquired providers in all three sectors (hospitals, nursing homes, and physician practices), but the share of providers that are PE-owned was relatively small. Identifying PE-owned providers is difficult due to the opacity of ownership structures and the lack of a single data source to identify ownership. Researchers who want to identify PE ownership must first assemble data from various proprietary (e.g., PitchBook) and public data sources. The volume and size of deals and the number of PE firms and providers in the sector compound the challenge of assembling a data set identifying PE ownership. Given these difficulties, researchers likely undercount PE-owned providers, although researchers typically use other available research to help validate the number of PE-owned providers in a sector.

Hospitals

For-profit hospitals can be owned directly by physicians, individual investors, PE firms, publicly traded corporations, or a mixture of these investors. Through publicly available resources, we identified 115 hospitals that were owned by PE firms at the start of 2020, representing only about 4 percent of traditional hospitals.[20] Other for-profit entities (such as publicly traded corporations and physician practices) own another 22 percent of traditional hospitals. The remaining 74 percent of hospitals are nonprofit or government-owned facilities.

Many hospitals have shifted back and forth among these ownership models. The most prominent example of shifting ownership is HCA Healthcare, which owns 184 hospitals, representing over 20 percent of all for-profit traditional hospitals. HCA went private in 1989, returned to being a publicly traded company in 1992, went private again in 2006 as part of a leveraged buyout led by PE firms, and became a publicly traded company again in 2010 (Wicklund 2010). However, members of the Frist family had leadership roles in the company throughout these changes, and this continuity of leadership may limit the effects of PE ownership cycling in and out of the company's capital structure. Similarly, the Steward Health Care system was formed in 2010 with PE financing (Hechinger and Willmer 2020). In 2020, the system sold its hospital real estate to a real estate investment trust, and a group of physicians bought the hospital operations from the PE fund (Steward Health Care 2020). While the system's ownership structure has changed over time, the same individual has continued to serve as its chief executive officer. The assumption of substantial lease obligations following the real estate sale may increase pressure on the operating company to generate positive cash flows, but the continuity of management may limit the degree to which operations change with ownership.[21]

The HCA and Steward models both involve acquiring hospitals and operating them under private ownership. A more controversial acquisition was a PE firm's 2018 purchase of Hahnemann University Hospital in Philadelphia from the Tenet system, where the PE firm quickly closed the hospital in 2019. However, it is not clear whether the hospital—which was losing money—would have remained open if it had been owned by a publicly traded company, a different PE firm, or a single family.

Nursing homes

PE investment in nursing homes dates to the late 1990s (Pradhan and Weech-Maldonado 2011). GAO found that almost 1,900 nursing homes were acquired by private investment firms between 1998 and 2008 (Government Accountability Office 2010). Some of the acquisitions that GAO identified involved a nursing home's operations and real estate, while other acquisitions involved only the real estate.

Some early research on private equity and nursing homes identified two phases of PE investment in the first decade of the 2000s (Stevenson and Grabowski 2008). The first phase was limited and focused on efforts by larger for-profit chains between 2000 and 2003 to sell selected facilities in Florida in response to liability costs and liability insurance premiums that were much higher than average. The second phase was broader and included facilities from some of the nation's largest nursing home chains. While investors looked for operational inefficiencies to improve in this phase, they also "began to recognize value in the real estate assets of some of the larger chains, especially in a climate with access to relatively inexpensive capital" (Stevenson and Grabowski 2008). They noted that the predictable cash flow from government payers to the nursing home sector plus the untapped value of some companies' real estate holdings made certain nursing home chains attractive investment opportunities.

Since the first decade of the 2000s, PE firms have continued to invest in nursing homes, reflecting the persistence of favorable conditions such as low interest rates, an aging population, reliable government payers, and favorable tax treatment of earnings. One recent article, citing data from PitchBook, noted a recent uptick in PE acquisitions, with nearly 190 nursing home deals totaling about $5.3 billion since the start of 2015, up from 116 deals totaling over $1 billion from 2010 to 2014 (Laise 2020). Although estimates of the number of PE-owned facilities vary, about 11 percent of nursing facilities nationwide are PE owned (Harrington et al. 2021). PE-owned nursing homes are a subset of for-profit facilities, which account for about 70 percent of all nursing homes in the U.S.

Physician practices

Physician practices are a target of private equity in part because the market for physician services is fragmented. Most physicians work in small practices: In 2018, over 56 percent of nonfederal physicians were in a practice of 10 or fewer physicians. This share has declined slowly, primarily due to a move away from physicians operating as solo practitioners (Kane 2019a). At the same time, the share of physicians in midsize practices (11 to 49 physicians) has remained steady, while the share joining groups of 50 or more or who are direct hospital employees or contractors has grown.

The structure of the market for physician services is changing rapidly through both horizontal consolidation among practices and vertical integration of practices and health systems or health plans. For the first time, in 2018, the share of employed physicians was slightly larger than the share of physician practice owners (47 percent versus nearly 46 percent) (Kane 2019b). Between 2016 and 2018, the share of all physicians affiliated with health systems grew from 40 percent to 51 percent (Furukawa et al. 2020).[22] As hospitals have acquired increasing numbers of physician practices, large health plans have responded in kind, perhaps to assert their own market power or to defensively counter the market power of health systems. PE firms compete with health systems and plans for physician practices and may contribute to the increasing pace of consolidation. We do not know of evidence that indicates whether practices acquired by PE behave differently from practices acquired by health systems or plans.

Information about the extent of PE investments in physician practices is lacking, and identifying deals is challenging because not all deals are publicized and PE firms and practices commonly use nondisclosure agreements (American Medical Association 2019). Nevertheless, some researchers have begun developing databases on PE acquisitions by combining proprietary information about practice deals with other sources of data. Building such data sets is painstaking; researchers often must resort to online search engines to verify PE deals and then attempt to match the practice name and location with additional information. According to several researchers we spoke with, proprietary data on deals are more likely to include acquisitions of larger practices than smaller practices. Data limitations mean that the number of PE-affiliated practices and physicians described in the literature are likely to be underestimates.

One study examining the 2013 to 2016 period found PE investments in just 355 practices (Table 3-1, p. 88). That figure accounts for about 2 percent of the approximately 18,000 practices in the U.S. (data not shown), but it does not take into account practices that had already been acquired by PE firms, including some very large physician staffing companies that employ tens of thousands of

TABLE 3–1

Physician groups with private equity investments, 2013–2016

	2013	2014	2015	2016	Total	Share of total
Number of practices by specialty type						
Primary care*	13	22	13	23	71	20%
Anesthesiology	10	20	15	24	69	19
Multispecialty	15	15	19	19	68	19
Emergency medicine	10	6	10	17	43	12
Dermatology	1	5	11	18	35	10
Ophthalmology	0	2	2	7	11	3
Radiology	0	0	2	6	8	2
Orthopedic surgery	0	0	2	3	5	1
Other specialty practices	10	2	14	19	45	13
Total practices	59	72	88	136	355	100
Number of physicians by specialty type						
Anesthesiology	246	593	458	597	1,894	33
Primary care*	163	367	300	216	1,046	18
Emergency medicine	150	184	148	419	901	16
Dermatology	11	26	86	211	334	6
Radiology	4	13	159	76	252	4
Ophthalmology	6	35	68	25	134	2
Orthopedic surgery	0	13	43	74	130	2
Urgent care	41	16	32	35	124	2
Other specialties	222	166	282	229	899	16
Total physicians	843	1,413	1,576	1,882	5,714	100

Note: Components may not sum to totals because of rounding.
*Primary care includes family practice, internal medicine, and pediatrics.

Source: Zhu et al. 2020.

clinicians. The number of deals rose each year from 59 practices in 2013 to 136 in 2016. Acquired practices had a mean of four office sites and six physicians per site (Zhu et al. 2020). Out of about one million active physicians, just over 5,700 (less than 1 percent) were associated with affected practices. The most common types of practices with PE deals were primary care, anesthesiology, multispecialty, emergency medicine, and dermatology. Interest in specialties such as dermatology, ophthalmology, behavioral health, and women's health expanded after 2016 (data not shown) (Brown et al. 2020, Bruch et al. 2020a, Chen et al. 2020, O'Donnell et al. 2020, Tan et al. 2019).

Practices that provide services such as emergency medicine and anesthesiology for hospitals were among the first wave of consolidations involving PE investment over the past 10 to 15 years. Several of the largest PE firms own physician staffing companies that were built by aggregating practices of hospitalists, emergency medicine physicians, anesthesiologists, radiologists, pathologists, and other specialists into multispecialty groups that focus on hospital services.[23] Other PE-backed single-specialty groups (for example, of anesthesiologists or radiologists) are among the largest regional entities providing those services to hospitals. PE funds (including venture capital in addition to buyout funds) have invested in primary care practices as well, but the incentives around those acquisitions may be different because many of those practices appear to be positioning themselves for risk sharing and value-based contracts. Other PE investments in primary care groups aim to ultimately fold them into larger multispecialty practices or target specific niches such as direct primary care and self-pay concierge care. More recently, single-specialty practices in ophthalmology, dermatology, orthopedic surgery, behavioral health, obstetrics-gynecology, and gastroenterology have attracted larger numbers of "middle-market" PE funds.[24] Those practices are expanding by hiring new clinicians and acquiring other practices to become larger local and regional groups.

PE firms use some common strategies to make providers more profitable

Our research found that the business models that PE firms use in the hospital, nursing home, and physician sectors use many of the same strategies. In this section, we highlight strategies that are used in at least two of those sectors, looking first at strategies focused on increasing revenues and second at strategies focused on reducing costs. However, it is worth keeping in mind that many of these strategies are commonly used by other for-profit providers in these sectors and are not unique to PE-backed providers.

Strategies that focus on increasing revenues

One strategy that PE-owned providers can use to increase revenues is to simply provide more services. For example, the researchers we interviewed noted that PE-owned nursing homes can try to boost their occupancy rates, while PE-owned physician practices may take steps such as hiring additional clinicians, expanding their office hours, and using branding and advertising to attract more patients.

Providers can also try to furnish a more profitable mix of services or expand the volume of lucrative services. Nursing homes can improve their payer mix by serving more Medicare and private-pay patients and fewer Medicaid patients or by providing services with higher margins. PE firms seek to acquire physician practices that own ambulatory surgical centers or have the potential to generate additional income from highly reimbursed elective procedures and ancillary services (Casalino et al. 2019, O'Donnell et al. 2020). For example, referrals within large practices allow dermatology and ophthalmology groups to keep revenues from higher paying services such as Mohs surgeries, intravitreal injections, and cataract and retinal procedures within their practice (Chen et al. 2020, Tan et al. 2019). In addition, PE-backed practices may offer self-pay services such as cosmetic injections or laser refractive surgery (O'Donnell et al. 2020).

Another strategy for increasing revenues is to raise prices. One study found that hospitals tended to increase their charges after being acquired by PE firms (Bruch et al. 2020b). Higher charges may increase profits from out-of-network patients and from insurers that pay for outpatient services based on a percentage of charges. Another study found that PE firms often aim to aggregate large numbers of physicians who have a common specialty to gain bargaining leverage over commercial payment rates (O'Donnell et al. 2020).[25] This strategy has little immediate, direct impact on Medicare beneficiaries or spending because Medicare's prices are set administratively rather than negotiated. However, a potential indirect effect is that providers may, over time, prefer commercial patients for whom they are more highly reimbursed.

For many types of clinicians, demanding higher commercial prices comes with a tradeoff—they may lose volume if insurers and patients turn to other providers. However, for certain specialties such as emergency medicine, patients cannot meaningfully choose among providers.[26] When hospitals contract with outside companies to deliver these services, the clinicians have inherent bargaining leverage because the hospital contracts for their services separately from the group's payment arrangement with insurers (Cooper et al. 2020a). So long as the hospital continues to contract for staffing services, excluding the staffing company's clinicians from a commercial insurer's network would likely not affect their volume of care. Some of the largest physician staffing companies have used this leverage in their negotiations with insurers, but the strategy has risks for the companies. Patients with commercial insurance have sometimes been left with unexpectedly large bills for receiving care from out-of-network clinicians who work at in-network hospitals and ambulatory surgical centers (Cooper et al. 2020b, Duffy et al. 2020). In turn, the issue of surprise billing has drawn public attention and raised questions about staffing firms' future profitability now that the Congress has restricted these billing practices (Gottfried 2020).[27]

PE firms also arrange for providers to work with related entities that share common ownership. For example, a PE firm may require nursing homes to buy goods and services from other companies that the PE firm owns, a practice known as "related party transactions." There may be several related companies, with each one focused on a separate aspect of the nursing home's operations (e.g., staffing, therapies, purchasing), resulting in a corporate structure that has multiple limited liability corporations under the same parent company.[28] While this approach can make it harder to understand the corporate structure and to litigate, one expert stressed that related parties are not problematic on their face and can be more efficient. Because transactions between health care entities, whether related or unrelated, must take into account the fair market value or risk running afoul of the federal Anti-Kickback Statute and state equivalents, the use of related parties becomes a concern only when a nursing home must pay above a fair market price for goods and services from related parties.

In the physician sector, PE firms may expand a practice by adding on subspecialty practices that give it more control over referrals. Competition for referrals from providers in other PE-backed practices may also lead to defensive consolidation. One ophthalmologist told us that his practice's referrals were being "chipped away" by rival practices that had partnered with PE funds, motivating his group to look for PE backing.

Strategies that focus on reducing costs

Consolidating providers within a given sector also allows PE firms to lower costs by taking advantage of economies of scale, a strategy particularly useful for physician practices (O'Donnell et al. 2020). For example, PE owners may consolidate "back office" services such as scheduling, coding and billing, revenue cycle management, and payroll. Smaller independent practices may not have expertise at managing administrative services efficiently; joining with larger practices and conducting some administrative functions centrally may lower their costs. An infusion of capital from PE investors may support investment in information technology to centralize quality measurement, reporting, and marketing at more favorable vendor pricing. PE capital may also allow practices to move to common electronic health records and potentially improve clinical workflow. One consultant we interviewed pointed out that PE funds offer smaller independent practices access to capital at lower borrowing rates than they would be able to obtain through other sources such as local banks. PE acquisitions in the hospital and nursing home sectors offer many of the same opportunities to realize economies of scale.

Another common strategy is to reduce labor costs. One study of the 2006 leveraged buyout of HCA found that it had slower cost growth than comparable hospitals after the leveraged buyout in part due to slower staffing growth (Kim and McCue 2012). We also found that PE-owned hospitals tended to have lower costs than both other for-profit and nonprofit hospitals. (See Table 3-2, p. 97; we explain this analysis in more detail in the next section.) In the nursing home sector, PE-owned facilities may attempt to lower their costs by reducing staff and/or changing the mix of staff.[29] PE owners may be able to reduce labor costs to some extent if a nursing home's staffing exceeds federal or state minimum standards. However, according to one researcher we interviewed, many nursing homes are already at minimum nursing staffing levels when they are acquired by private equity, so cutting nursing staff further may not be feasible. In that case, the PE owners would still have latitude to reduce non-nursing staff costs, which may reduce quality of life for patients without reducing measured quality of care or affecting federally reported staffing measures.

PE firms may also try to lower labor costs when they acquire physician practices by substituting less expensive clinicians (such as physician assistants) for physicians or reducing staffing (Brown et al. 2020, Hafner and Palmer 2017). Use of these approaches is likely to vary. For example, one physician told us that his ophthalmology practice had sought a PE backer that would not reduce its workforce and that the practice had continued to pay staff during the coronavirus pandemic even though revenues were lower. However, others have had different experiences. For example, major physician staffing companies reportedly cut clinician hours and asked for voluntary furloughs as elective hospital procedures declined during the pandemic (Arnsdorf 2020).

However, PE firms also use strategies that can increase costs for providers. For example, providers that are acquired through leveraged buyouts are typically required to spend more on debt service. PE firms may also sell a provider's real estate to another company and have the provider sign a long-term lease, making the provider responsible for the lease payments. (This practice is more common for nursing homes and is discussed in more detail later in the chapter.)

Finally, PE firms often require nursing homes and physician practices to pay monitoring or management fees. These fees compensate the PE firm for the costs of overseeing and managing the provider's operations and allow PE firms to generate some returns before they exit an investment. According to one PE investor we interviewed, the management fees for a PE-owned nursing home typically equal 5 percent to 6 percent of its gross revenues. However, it is worth noting that the fees paid by portfolio companies are generally used to reduce the management fees that the limited partners in a PE fund are required to pay the general partner.

Some PE strategies are more relevant to a particular sector

Although PE investments in hospitals, nursing homes, and physician practices have a number of common features, there are other strategies that are largely used in only one of those sectors.

Separation of real estate and operations

Nursing homes and some hospitals can be profitable investments because the investor can sell the real estate to a related company or to a third party. The proceeds from real estate sales can be disbursed as profits to the PE fund, and the facility then has to pay rent.

Starting in 2003, PE firms made several deals to purchase nursing home chains where they separated the chains' real estate and operations. Investors would buy a company, finance the deal with the chain's real estate assets (for example, by leasing its properties to help pay off debt assumed in the acquisition), and hire a separate operating company to manage the assets. The operators of the nursing homes thus became tenants instead of owners and assumed responsibility for paying the rent and all expenses of the properties, including insurance, operating expenses, and property taxes. (These types of leases are known as "triple net" leases.) The practice of separating real estate and operations is common across the industry and not limited to PE-owned facilities.[30]

Complex corporate structures

Like the hospital chain structure described above, nursing homes with a common owner can also have complex structures that make ownership, managerial control, and cash flow difficult to track. Though this complexity is not necessarily limited to PE, private equity owners may restructure a chain by establishing a holding company that owns the entire chain, having separate LLCs for the operation of each individual facility that is part of the chain, separate LLCs that own the real estate, and a separate company that leases properties from a real estate holding company and subleases to operating companies (Government Accountability Office 2010). The text box (pp. 92–93) explores one example of this complex structure in a PE-owned nursing home chain (Bos and Harrington 2017).

A separate set of considerations—state laws restricting the corporate practice of medicine (CPOM)—affect how PE firms structure their investments in physician practices. CPOM laws vary by state and allow certain exceptions. However, most require practices to be organized as professional corporations or professional limited liability companies—both referred to here as professional service companies (PSCs)—with owners, shareholders, and/or board members who are licensed medical providers (American Medical Association 2015). Such laws were enacted out of concern that corporate ownership's obligations to shareholders may not align with a physician's responsibilities to his or her patients and could lead to interference in the physician's independent medical judgment (American Medical Association 2019). When PE firms invest in practices, the organizational structures they set up must avoid appearing to influence physicians' behavior since that could trigger enforcement of CPOM laws or raise concerns about inducement of services under the Anti-Kickback Statute or the False Claims Act. One reason that some physicians find PE ownership appealing is that investors may be less involved in day-to-day operations compared with acquisition by a health system.

Although PE firms use a variety of structures, in states with CPOM laws, investors typically establish a relationship with a trusted medical provider who is the owner and manager of a PSC that retains ownership of a practice's clinical assets (Figure 3-4, p. 94). The PSC employs practice physicians and makes decisions on hiring and firing, credentialing, and peer review. The PE firm holds majority equity in a management services organization (MSO) that takes ownership of the practice's nonclinical assets and provides administrative and financial services to the PSC under a management services agreement (Genecov 2019). The PSC pays fees for management services to the MSO; these fees are set at fair market value, but that amount likely varies by practice. One or more representatives of the PE firm may sit on an advisory board or joint operating committee to coordinate the two entities. In states without CPOM laws, the PE firm's operating company may hold a more direct ownership stake in the clinical side of the practice but may still arrange a management services agreement for nonclinical support.

The impact of private equity ownership on the Golden Living nursing home chain

The private equity (PE) firm Fillmore Capital Partners acquired the Beverly Enterprises nursing home chain in a leveraged buyout in 2006 and renamed the company Golden Living. Following this acquisition, researchers examined changes in the chain's strategy and operations over the next 12 years (Bos and Harrington 2017). Several of those strategies predate the PE acquisition and were commonly used across the nursing home industry. The key strategies that Golden Living used are consistent with those identified in the literature on approaches that PE owners use to create value, including:

Sale of unprofitable facilities. Starting in 2001 before the PE acquisition and continuing after, Golden Living sold off more than 150 nursing homes. Divesture was common across the industry at the time due to high liability costs in some states and changes in Medicare policy that limited per day payments.

Addition of other services and lines of business. Mainly after 2004, the company started to invest in new profitable services and lines of business, including a rehabilitation therapy company (Aegis Therapies), a hospice company (Asera Care), and a staffing company (Aedon Staffing) that targeted Medicare and private-pay patients.[31] Golden Living often served as the "launch customer" for new lines of business.

Tighter corporate control over individual facilities. Following the PE acquisition, local managers of the chain's facilities were given a smaller span of control, and the use of performance-related pay was introduced.

Changes in staffing. Researchers compared the chain's staffing levels pre- and postpurchase. The skill mix (the proportion of higher educated nurses when compared with lower educated nurses) was significantly higher from 2009 onward. Total staffing levels in California were lower during PE ownership but they had higher staffing levels for registered nurses than other facilities.

Corporate restructuring. Fillmore Capital created one LLC, Pearl Senior Care, to purchase Golden Living (Figure 3-3). Pearl Senior Care in turn owned another LLC, Drumm Investors, which in turn owned Golden Horizons (which operated the facilities) and Geary Property Holdings (which owned the facilities and their real estate), legally separating the operations from the buildings and the land. Postpurchase, the chain's nursing facilities leased their buildings and land. The individual Golden Living nursing homes were also split into separate LLCs. The PE owner stated that its lenders required the company to use separate LLCs to limit risk in the event of bankruptcy or litigation. The authors note that this complex structure, with separate management and property companies and multiple ownership levels, was not unique to PE-owned nursing homes and was commonly used by large nursing home chains by 2008. ■

(continued next page)

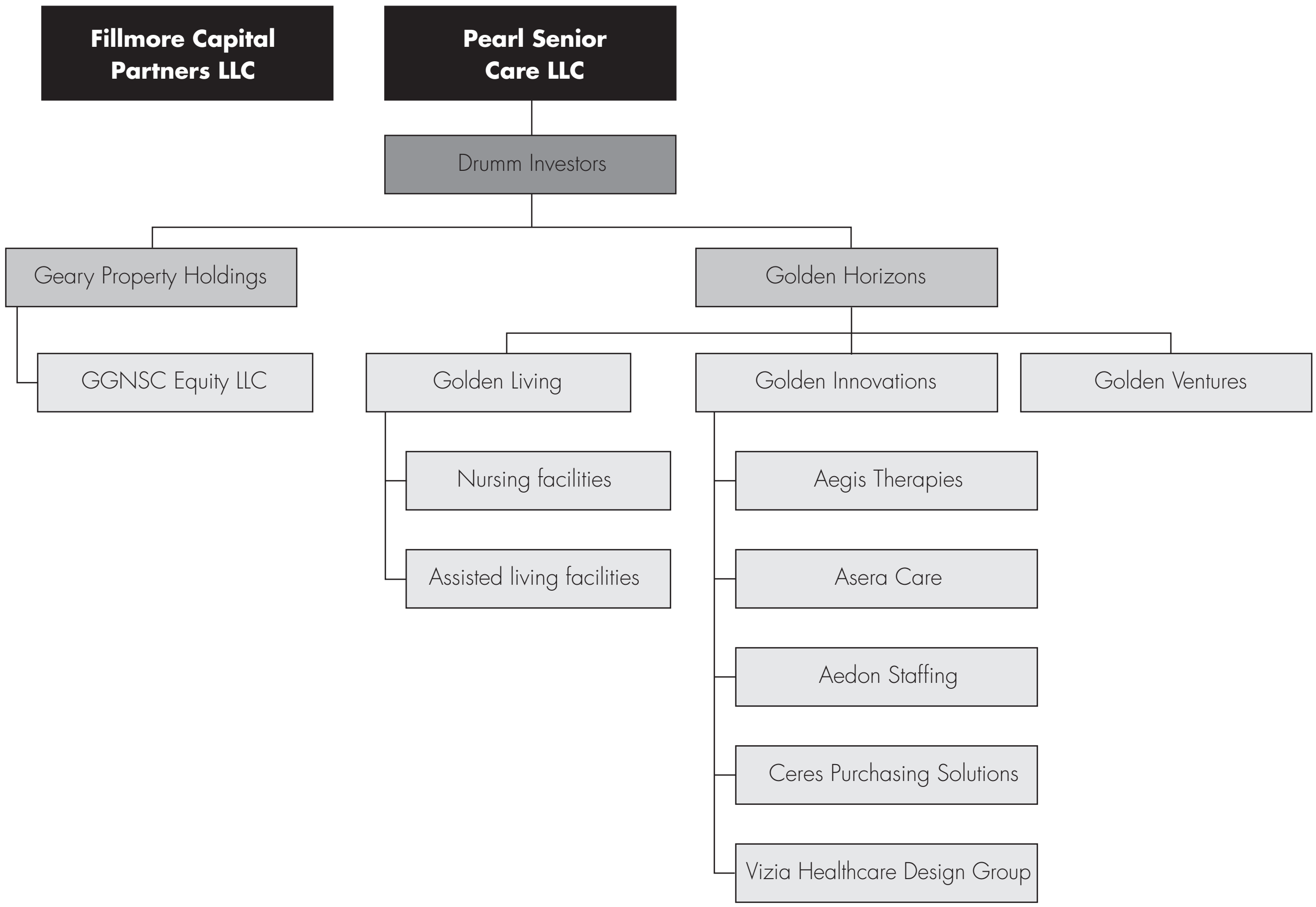
The use of platform and add-on acquisitions to consolidate physician practices

PE firms use a variety of approaches to build regional group practices, but they often first invest in a large, well-established practice (known as a platform acquisition), which then acquires smaller practices in the same or a related specialty (add-on or tuck-in acquisitions). Under this approach, the platform practice builds into a larger local or regional practice group with greater scale economies for centralized business services (such as billing) and potentially more influence over referral patterns and commercial payment rates. PE investors use a combination of investor capital and debt to finance acquisitions, and the debt becomes the obligation of the practice (Casalino 2020). Because PE firms have a limited time horizon in which to provide returns to investors, they generally aim to exit ownership of portfolio practices after three to seven years (Casalino et al. 2019). Competition for

The impact of private equity ownership on the Golden Living nursing home chain (cont.)

FIGURE 3–3

The Golden Living nursing home chain had a complex corporate structure after its acquisition by a PE firm



Note: PE (private equity), LLC (limited liability company). This figure, taken from "What Happens to a Nursing Home Chain When Private Equity Takes Over? A Longitudinal Case Study," depicts Golden Living's corporate structure at the time of the case study's publication in 2017. While Fillmore Capital Partners still owns Golden Living, some of the company names and ownership arrangements have changed since the publication of the case study. For example, Asera Care, a hospice provider, was sold to Amedysis in June 2020.

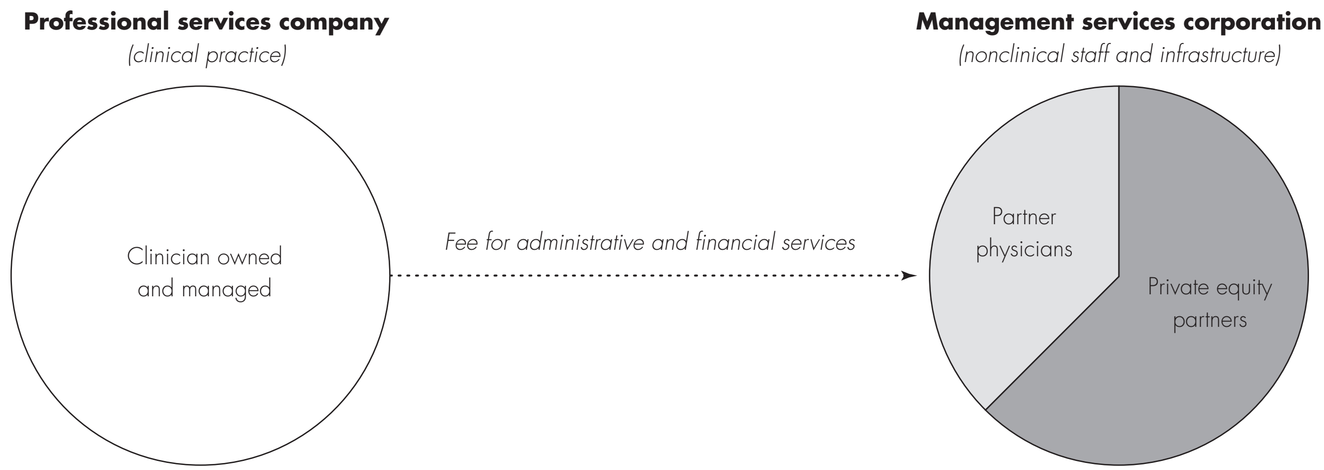
Source: Bos and Harrington (2017).

physicians among hospital-based health systems, health plans, larger physician groups, and other PE companies may all offer exit opportunities for the PE firm.

Sequential "roll-ups" (acquisitions) of physician practices by PE firms, health systems, and insurers often are too small individually to trigger antitrust reporting requirements, yet they can result in large practice groups with market power. According to one former member of the Federal Trade Commission, the median size of recent buyouts of health care firms has been $60 million to $70 million, well below reporting requirements. In his

FIGURE 3–4

In states with laws against the corporate practice of medicine, PE firms control management service organizations rather than clinical practices



Note: PE (private equity).

Source: MedPAC analysis based on Gilreath et al. 2019.

opinion, PE firms can use this strategy to "quietly increase market power and reduce competition," leading to a higher valuation when the company is later sold (Chopra 2020). A recent analysis documented that among group practices that initially had 100 or more physicians, about half of their growth resulted from acquisitions of small groups with 10 or fewer physicians. Another one-third of growth resulted from hiring new physicians (Capps et al. 2017).

PE firms provide upfront payments to physician owners that compensate them for the practice's future stream of operating earnings and are calculated as a multiple of the practice's earnings before interest, taxes, depreciation, and amortization (EBITDA). Owners of a large platform practice may receive a multiple of 8 to 12 times EBITDA (sometimes even higher), while owners of add-on acquisitions receive multiples that are considerably lower (Casalino et al. 2019, Helm 2019).[32] After the add-on practice has been absorbed into the larger entity, its value increases to the same level as the platform practice (8 to 12 times EBITDA). This increase in the value of add-on practices provides an opportunity for higher returns when the PE firm sells its stake in the MSO in three to seven years.

Rollover equity

Part of the PE firm's upfront payment for a practice reflects prospective reductions in regular compensation to the practice's physician owners (Helm 2019). Typically, a medical practice distributes end-of-the-year profits among its partners so that the practice itself does not pay taxes (Gilreath et al. 2019). PE deals replace this approach with salaries that are typically about 30 percent lower than the physician-owners' prior compensation (Shryock 2019). However, as part of the PE deal, founding physicians or other key practice owners also receive "rollover equity"—a minority ownership stake (e.g., 20 percent to 40 percent) to keep physicians' incentives aligned with those of the PE investor (Casalino et al. 2019). The PE firm's exit from a practice also provides physicians with rollover equity a chance at getting "a second bite at the apple"—a share of the profits from selling their stake to a new owner.

The future of PE investment in hospitals, nursing homes, and physician practices

While the regulatory, demographic, and payment conditions that have made health care an attractive investment remain, parts of the sector are facing significant disruptions due to the coronavirus pandemic. Postponement and cancellation of elective procedures and in-person office visits in March and April 2020 reduced revenues of hospitals and physician practices. Many health care providers received federal assistance in 2020, allowing some providers (e.g., many hospitals) to see an increase in profitability in 2020. However, other providers (e.g., some nursing homes) struggled financially in 2020 despite federal support. COVID-19 infections and related deaths severely affected residents of nursing homes, and even though most residents have now been vaccinated, nursing home occupancy rates are expected to recover slowly. During 2020, the number of PE deals declined by one-seventh, but the value of PE investments in health care fell by about one-third (PitchBook 2021). Analysts attribute this decline to PE funds looking for bargains and sellers holding out for higher deal valuations once the pandemic has waned.

Going forward, we expect private equity to play a limited role in the hospital industry. In 2020, Cerberus Capital Management sold its interest in the Steward hospital chain (which owns 35 hospitals) to a group led by Steward physicians. Also in 2020, the publicly traded Quorum hospital chain filed for bankruptcy and was taken over by its creditors, which included PE funds. The net effect was that PE firms continue to own about 4 percent of general and acute care hospitals. Despite the fact that private equity firms have large amounts of capital to be deployed (called "dry powder"), we do not expect PE firms to acquire a large number of nonprofit or publicly traded hospitals. Most nonprofit hospitals have had strong all-payer profits in recent years and do not have need for outside capital. In addition, most publicly traded hospitals have seen their stock prices rise substantially in recent years, making them less attractive acquisition targets. Because there is little need for PE capital and no clear competitive advantage of PE ownership over other ownership structures, we do not expect PE firms to acquire large numbers of hospitals in the near future. The pace of acquisitions is more likely to be slow, reflecting incremental acquisitions by PE firms, publicly traded hospitals, and nonprofit systems. During January 2021, nonprofit health systems appeared to be making most hospital acquisitions (Hansard 2021).

PE firms have been more active in acquiring nursing homes, but it is not clear whether that level of interest will continue. Even before the pandemic, PE ownership of health care providers, including nursing homes, was receiving renewed attention from policymakers. The impact of the coronavirus on the lives and welfare of residents and staff has intensified media coverage of nursing homes, with some reports focusing on acquisitions by PE firms during the pandemic and conditions in PE-owned facilities.[33] One study found that PE-owned facilities were less likely to have at least a one-week supply of N95 masks and medical gowns than facilities that did not have PE owners, but found no statistically significant differences in staffing levels, COVID-19 cases or deaths, or deaths from any cause between PE-owned nursing homes and facilities with other types of ownership (Braun et al. 2020). Another study found that PE-owned nursing homes were associated with a decreased probability of resident and staff cases of COVID-19 and shortages of personal protective equipment (PPE) (Gandhi et al. 2020a). Facilities previously owned by PE firms were associated with an increased probability of PPE shortages and resident outbreaks.

At an industry conference in February 2021, investors noted that the coronavirus pandemic, combined with increased scrutiny of PE ownership of nursing homes by policymakers, will likely contribute to waning PE interest in nursing homes (Spanko 2021). Where there is still interest, investors will pay close attention to the quality of the nursing home operator in a post-coronavirus world, and "turnaround" projects will be less attractive. One investor noted that how well an operator has weathered the pandemic will likely be an important signal to investors: "While buildings in different parts of the country saw wildly varied COVID-19 situations at different points in the year, they all received the same fire hose of federal support—and it will become immediately clear to curious observers how any given operator decided to deploy that money" (Spanko 2021).

PE interest in physician practices remains strong. In some specialties, PE investors hope to gain from an expected rebound in patient volumes (Hansard 2021). Practices that receive a larger proportion of their revenues through capitated payments fared relatively well during the pandemic, and financial analysts expect that PE deals with them will grow (PitchBook 2021). Other analysts have expressed concern that some physician practices, especially those in primary care, are experiencing continued economic difficulty, which may accelerate the pace of PE deals by investors seeking to acquire practices in financial distress at lower prices (Bruch et al. 2021a). Although the market for physician services is changing as hospital systems and insurers acquire practices, it remains fragmented. Consolidating practices offers PE firms opportunities to lower some costs through economies of scale and to expand revenues through higher volume, higher commercial payment rates, and a more lucrative mix of services.

Effects of PE investment on Medicare costs, beneficiary experience, and provider experience

Estimating the effects of PE ownership first requires the accurate identification of PE-owned providers, but, as previously discussed, that process is time consuming and difficult. Given the complexity of identifying PE ownership, we used published literature, supplemented with other sources, to examine the effects of PE ownership on hospitals, nursing homes, and physician practices. Empirical literature on the effects of PE ownership on hospitals, which have had relatively few but high-profile PE owners, is relatively scant. We supplemented that literature with a cross-sectional analysis that compared PE-owned hospitals with hospitals that have other ownership structures. In contrast to hospitals, the nursing home sector has a longer history of PE ownership and more extensive literature examining its effects. We reviewed and summarized this literature on the impacts on costs and quality. For physicians, who have seen more recent PE interest, we reviewed the literature on and interviewed physicians about their experiences with PE acquisition. Empirical information about the impact of PE ownership of physician practices on Medicare spending, quality of care, and patient experience is minimal, but researchers have hypothesized about some possible effects based on PE business strategies.[34]

Hospitals

We conducted a cross-sectional analysis of how PE-owned hospitals compare with other hospitals and report on a study that examined how hospitals change when their ownership changes. Our analysis and the literature suggest that PE owners induce an increase in hospital charges and that PE-owned hospitals tend to have lower costs and lower patient satisfaction. However, the differences between hospitals owned by private equity and other hospitals are not large, and there is a substantial overlap in the distribution of costs and patient satisfaction among PE-owned hospitals and other hospitals. While PE ownership may influence provider costs and patient experience, it will not have a large direct effect on Medicare costs due to the program's use of prospective payment rates.

PE-owned hospitals tended to have lower costs and lower patient satisfaction

We tested whether there are any differences in the cost structures for PE-owned hospitals versus other hospitals by examining hospital costs per discharge in 2018 after adjusting for local wage rates, patient mix, and other factors.[35] We limited our analysis to hospitals with over 500 Medicare discharges during the year to create some stability in measures of costs per discharge. We also examined the hospitals' profit margins and the share of patients rating the hospital a 9 or 10 in their overall satisfaction of the hospital.

PE-owned hospitals tended to have lower costs and patient satisfaction than both other for-profit and nonprofit hospitals (Table 3-2).[36] Lower patient satisfaction is consistent with results from a similar analysis of 2018 data (Bruch et al. 2021b). The lower costs at PE-owned hospitals contributed to their higher Medicare margins. However, the PE-owned hospitals had relatively low all-payer margins in 2018. Those margins could in part reflect their payer mix, which was more heavily weighted toward Medicare and Medicaid. While there are differences in median performance, we also present the 25th and 75th percentiles of performance. There is a great deal of overlap across the categories, suggesting that different types of ownership are not associated with consistently large differences across any of the metrics we examined.

We also examined risk-adjusted mortality 30 days after discharge and risk-adjusted readmission rates 30 days after discharge using models developed by 3M™. We did not find any statistically significant differences in mortality across the three groups of hospitals, and the relative performance of the groups depended on whether we examined means or medians (data not shown). Readmissions at PE-owned and other for-profit hospitals were 104 percent of the national median using the 3M measure. However, the readmission measure should be viewed with some caution as the demographic characteristics of the patients may affect readmissions.

The cross-sectional differences we see could be because PE firms tend to buy hospitals that already have relatively

TABLE 3–2

Performance of PE-owned hospitals, 2018

Characteristics	PE hospitals	Other for profit	Government/ nonprofit
Number of hospitals (with over 500 Medicare discharges)	79	455	1,851
	Medians (25th to 75th percentiles)		
Cost per discharge as a share of the national median	90%[ab] (80 to 102%)	92%[b] (84 to 103%)	102% (92 to 113%)
Median share of patients rating the hospital a 9 or 10 (out of 10)	64%[ab] (58 to 68%)	68%[b] (63 to 74%)	72% (67 to 76%)
Median Medicare margin in 2018	2%[b] (–6 to 11%)	0%[b] (–10 to 8%)	–9% (–19 to 0%)
Median total (all-payer) margin in 2018	5%[a] (–3 to 12%)	10%[b] (1 to 19%)	4% (0 to 10%)
Median share of patients for whom Medicare is the primary payer	39% (29 to 46%)	35% (27 to 44%)	36% (28 to 44%)
Median share of patients for whom Medicaid is the primary payer	11%[ab] (4 to 19%)	5%[b] (2 to 10%)	7% (3 to 13%)

Note: PE (private equity). Sample is limited to hospitals with 2018 cost report data and over 500 Medicare discharges in 2018. Relative values are the median for the group as a share of the median of all hospitals. Per case costs are standardized for area wage rates, case-mix severity, prevalence of outlier and transfer cases, interest expenses, low-income shares, and teaching intensity. Patient ratings are from the Hospital Consumer Assessment of Healthcare Providers and Systems®. See our March 2021 report to the Congress for methodological details. Twenty of the 79 hospitals owned by PE firms were in the Steward system, which ceased to be owned by PE in 2020.
[a] Indicates a statistically significant difference from other for-profit hospitals using a $p < .05$ criterion using a Tukey test to account for multiple comparisons.
[b] Indicates a statistically significant difference from nonprofit hospitals using a $p < .05$ criterion using a Tukey test to account for multiple comparisons.

Source: MedPAC analysis of Medicare cost report and Hospital Compare data.

low cost structures and low patient satisfaction or because PE ownership results in lower costs and satisfaction. We cannot show causation through the cross-sectional analysis.

Changes in charges, profits, and quality metrics following PE acquisitions

A recent study by Bruch and others examined changes in charges and quality metrics after hospitals were acquired by private equity (Bruch et al. 2020b). Most of the PE-owned hospitals examined in the study were HCA hospitals that were acquired in a single transaction in 2006.

Bruch and colleagues found charges (list prices) increased following acquisitions and found mixed evidence of quality changes. The HCA hospitals showed some improvements in process measures after their ownership changed, but other hospitals acquired by PE firms failed to improve in any process measures and reported declining performance on one process measure. The mixed findings on quality make it difficult to attribute the quality changes to ownership changes, especially given the consistent hospital management at HCA. The HCA hospitals could have initiated process changes independently of the PE acquisition, and it was those efforts, rather than ownership changes, that drove improvements in process metrics. The

Bruch study did not evaluate whether the assumed quality effects of HCA going private in 2006 were reversed when it switched back to being publicly traded in 2010. The movement of HCA in and out of PE ownership illustrates the difficulty of determining the long-term effect of PE ownership, which itself is not designed to last for a long period.

Nursing homes

The literature on the effects of PE ownership on nursing homes is comparatively extensive, reflecting the long history of PE involvement in the industry, the number of nursing homes with PE owners, and the public policy interest in the effect of PE ownership.[37] While PE ownership could lead to lower quality of care or quality of life due to greater efforts to reduce costs or the debt that providers assume in the acquisition, researchers also point out that PE owners could make changes that improve quality, operational efficiency, and profitability (Huang and Bowblis 2019).

Studies measuring the effect of PE ownership generally attempt to measure its average impact and distinguish any PE-specific effects from the general effects of for-profit ownership. Beyond that, however, studies vary on several key dimensions, such as the period covered (the length of the look-back period before the PE purchase and the length of the observation period after the purchase), the nursing homes examined in the study (some use data from a single state, while others are national in scope), and the method and data sources used to identify PE-owned providers. As discussed above, there is no single data source that identifies PE-owned health care providers. Researchers must decide what counts as PE ownership and use multiple data sources in a complicated and time-consuming process to identify PE-owned nursing homes. Studies also differ in their choice of impact measures (e.g., staffing, quality metrics, mortality). Measures of staffing at the facility level are commonly used because (1) staffing is widely considered an important input into the quality of care, (2) staffing is under the control of nursing home operators, and (3) administrative data on staffing are generally available. Finally, these studies vary in whether or how they account for underlying differences between nursing homes acquired by PE and other nursing homes or differences in the residents served, which can bias results.

Overall, the findings in the literature on the average effects of PE ownership on nursing home quality and costs are mixed. For example, studies have found different effects of PE ownership on staffing levels and mix. A summary of the findings of studies published since 2012 is shown in Table 3-3. Note that most of the studies look at periods before 2010, although two working papers use more recent data.

Physician practices

According to the peer-reviewed literature and our interviews with physicians, physician experiences with PE investment have been highly variable, primarily due to differences among specialties, physicians, practice sizes, and PE firms (Casalino 2020, Casalino et al. 2019, Gondi and Song 2019, Zhu and Polsky 2021). When a PE firm acquires a physician practice, a key downside is the physicians' loss of control over the future of the practice. This uncertainty may particularly affect early and mid-career physicians who expect to practice longer than older physicians. Physicians also sacrifice future revenue because they are selling a portion of their future revenue stream. Another issue is that physicians risk losing some of their autonomy. For example, private equity firms may cut staff, change the hours of operation, and require physicians to obtain approval to purchase new equipment. Because PE investors want to rapidly increase profits, they may create incentives for physicians to change their clinical behavior. For example, dermatologists reported pressure to increase the volume of procedures and direct pathology specimens and surgical referrals to employees of the practice (Resneck 2018). A dermatologist told us that the PE firm that acquired his practice pressured clinicians to see more patients and perform more procedures, such as biopsies and Mohs surgeries.

On the other hand, researchers and physicians also cite benefits from PE investment (Casalino 2020, Casalino et al. 2019, Gondi and Song 2019). PE deals are often lucrative for older physicians who are seeking to exit practice ownership (Gondi and Song 2019). The large upfront payments from these deals replace physicians' future income but are taxed at capital gains rates, which are lower than income tax rates. PE buyouts may also be attractive to younger physicians who are looking for a better work-life balance and freedom from administrative and financial responsibilities (Casalino 2020).

In addition, rapid changes in the health care market (e.g., vertical and horizontal integration of providers, movement toward value-based care, and changes in information technology) have created an environment of uncertainty and higher expenses for independent

TABLE 3–3

Overview of key studies on the effects of private equity ownership of nursing homes

Paper title (author and year)	Summary of findings	Study population and dates
Does Private Equity Investment in Healthcare Benefit Patients? Evidence from Nursing Homes (Gupta et al. 2021)	Among patients with Medicare-covered stays, PE ownership increased mortality and spending. Researchers also observed worsening mobility and elevated use of antipsychotic medications, declines in nurse availability per patient, and declines in compliance with federal and state standards of care. Operating costs post-acquisition shifted toward non–patient care items such as monitoring fees, interest, and lease payments.	National data for 2000–2017
Private Equity, Consumers, and Competition: Evidence from the Nursing Home Industry (Gandhi et al. 2020b)	The effect of PE ownership was heterogenous with respect to levels of local market concentration: In highly competitive markets, PE owners increased staffing, while in less competitive markets they reduced staffing. Following introduction of the 5-Star Quality Rating System, PE-owned facilities increased staffing more than their non-PE counterparts, and PE facilities shifted staffing more toward RNs in response to the rating system's emphasis on RN staffing.	National data for 1993–2017
Private Equity Ownership and Nursing Home Quality: An Instrumental Variables Approach (Huang and Bowblis 2019)	Private equity ownership does not lead to lower quality, measured using 17 resident-level quality metrics, for long-stay nursing home residents in a period of 4 to 5 years following acquisition.	Ohio only for 2005–2010
What Happens to a Nursing Home Chain When Private Equity Takes Over? A Longitudinal Case Study (Bos and Harrington 2017)	PE owners continued and reinforced several strategies that were already put in place before the takeover, including a focus on keeping staffing levels low. The new PE owners added restructuring, rebranding, and investment strategies such as establishing new companies, where the nursing home chain served as an essential "launch customer."	A single multi-state nursing home chain from 2000–2012
Private Investment Purchase and Nursing Home Financial Health (Orfaly Cadigan et al. 2015)	PE acquisition had little impact on financial outcomes except for liquidity, the only measure with a change after acquisition that did not begin in the pre-acquisition period. At baseline, acquired nursing homes looked different than non-acquired nursing homes: They had higher occupancy, lower Medicaid/higher Medicare share of residents, lower operating expenses, higher total revenue, greater liquidity, and higher profits.	National data for 1998–2010
Private Equity Ownership of Nursing Homes: Implications for Quality (Pradhan et al. 2014)	PE nursing homes in Florida had lower RN staffing and higher LPN and CNA staffing compared with other for-profit nursing homes. The change in nurse staffing pattern was reflected in the lower skill mix of PE nursing homes post-acquisition. PE-owned facilities reported worse results on pressure sore prevention and restorative ambulation and had significantly higher numbers of deficiencies and pressure ulcer risk prevalence.	Florida only for 2000–2007
Private Equity Ownership and Nursing Home Financial Performance (Pradhan et al. 2013)	Compared with other for-profit nursing homes, PE nursing homes had higher operating revenues and costs, operating margins, and total margins and no significant differences in payer mix.	National data for 2000–2007
Nurse Staffing and Deficiencies in the Largest For-Profit Nursing Home Chains and Chains Owned by Private Equity Companies (Harrington et al. 2012)	Chains purchased by PE companies showed little change in staffing levels, but the number of deficiencies and serious deficiencies increased in some postpurchase years compared with the prepurchase period.	National data for 2003–2008

Note: PE (private equity), RN (registered nurse), LPN (licensed practical nurse), CNA (certified nursing assistant).

Source: Bos and Harrington (2017), Gandhi et al. (2020b), Gupta et al. (2021), Harrington et al. (2012), Huang and Bowblis (2019), Orfaly Cadigan et al. (2015), Pradhan et al. (2014), Pradhan et al. (2013).

practices. PE investment offers these practices "shelter from the storm" by providing them with access to capital and expertise in financial management, operations, and practice acquisition (Casalino et al. 2019, Gondi and Song 2019). PE acquisition can also help subspecialty practices maintain their access to referrals. For example, retinal specialists depend on general ophthalmologists for referrals. By combining with general ophthalmologists in a PE-owned practice, retinal specialists can secure a steady stream of referrals (Casalino 2020).

Some physicians report that practice operations and clinical decision-making have not been affected by PE ownership (Casalino 2020, Gondi and Song 2019). Among the physicians we interviewed, those who performed considerable due diligence and selected a PE firm that shared their practice's values generally had positive experiences.

We found minimal peer-reviewed, empirical evidence about the impact of PE ownership of physician practices on spending, quality of care, and patients' experience.[38] The pressure that some PE firms apply to clinicians to increase revenue by performing more procedures and ancillary services (e.g., imaging) could lead to higher spending (Casalino 2020, Casalino et al. 2019, Gondi and Song 2019, Zhu and Polsky 2021). In addition, ophthalmology practices owned by PE investors have an incentive to use more expensive drugs, which have higher profit margins (O'Donnell et al. 2020).

Physicians' views differ about the impact of private equity on quality of care and patients' experience. Concerns about potentially harmful effects on quality include the following:

- The pressure on PE-owned practices to achieve high returns on investment in a short time may come at the expense of investing in quality and safety (Gondi and Song 2019).
- The focus on increasing procedures may lead to inappropriate services and reduced quality (Casalino 2020).
- Care may be delivered by nonphysician practitioners, such as physician assistants (PAs), without adequate physician supervision (Gondi and Song 2019); one physician told us that he had difficulty supervising PAs because of their high patient volume, and he did not feel comfortable with the care they provided.
- The emphasis on keeping referrals within the practice may not be consistent with patients' needs or preferences (Gondi and Song 2019).

However, some physicians report that patient care and practice patterns do not change as a result of PE ownership (Gondi and Song 2019). During our interviews, some physicians stated that PE firms are committed to providing patients with a positive experience so they can attract new patients. Another view is that PE acquisitions can improve quality of care because physicians no longer need to focus on running a business (Casalino 2020).

Summary of effects of PE ownership

Our review of the evidence on the effects of PE ownership on hospitals, nursing homes, and physicians is summarized below.

- ***Hospitals.*** Our cross-sectional analysis found that PE-owned hospitals tended to have lower costs and lower patient satisfaction, but the differences between hospitals owned by private equity and other hospitals were not large. This association could be due to the type of hospitals that PE firms buy (e.g., hospitals with a low purchase price) or the effect of PE ownership on hospitals (PE firms pushing down costs). Our cross-sectional analysis cannot differentiate between these two possibilities. Longitudinal analysis in the literature suggests that following acquisitions by PE firms, hospitals tend to increase their charges at a higher rate than the average. While PE ownership may influence provider charges, it will not have a large direct effect on Medicare costs due to the program's use of prospective payment rates. In addition, the effect of PE acquisitions on the quality of care is not clear given that we do not have consistent evidence that PE ownership has large effects on quality metrics.
- ***Nursing homes.*** Studies on PE ownership of nursing homes have examined a variety of quality and financial outcomes, and findings are generally mixed. One recent study found that PE ownership had no effect on total revenue or costs but found evidence of a shift in operating costs away from staffing toward monitoring fees, interest, and lease payments (Gupta et al. 2020). Another recent study found that, in highly competitive markets, PE-owned nursing homes increased staffing, while in less competitive markets they reduced staffing (Gandhi et al. 2020b).

- ***Physicians.*** PE investment in physician practices is relatively new, and the literature estimating the impact of PE ownership of physician practices on spending, quality of care, and patient experience is scant. The pressure that some PE firms apply to clinicians to increase revenue by performing more procedures and ancillary services (e.g., imaging) could lead to higher spending (Casalino 2020, Casalino et al. 2019, Gondi and Song 2019).

PE involvement with the Medicare Advantage program

Under the Medicare Advantage (MA) program, Medicare contracts with private plans to deliver Part A and Part B benefits to eligible beneficiaries. (Most MA plans also provide Part D drug coverage.) The share of beneficiaries enrolled in MA plans has increased steadily for more than a decade. In 2020, 43 percent of all beneficiaries with both Part A and Part B coverage were in MA, and that number is widely expected to continue growing in the coming years.

The size and scope of the MA program may provide PE firms with a wider range of investment opportunities compared with an individual provider sector. We therefore tried to assess PE activity on two levels: (1) investment in MA plan sponsors (the health insurers that offer plans) and (2) investment in related companies that work for plan sponsors (such as a company that helps manage care for enrollees with complex health needs). In addition, we examined other types of PE investment besides buyouts—such as venture capital (VC) and growth capital—because they appear to play a larger role in this area than in the three provider sectors that we already examined.

In addition, although the congressional request specifically refers to MA, we also included other private plans that provide Part A and Part B benefits but are not part of the MA program—cost plans, Medicare–Medicaid Plans, and the Program of All-Inclusive Care for the Elderly (PACE)—to provide a fuller picture of PE involvement.[39]

PE investment in MA plan sponsors

We examined PE investment in MA plan sponsors using January 2021 information from CMS on the parent organization and tax status for each plan. The parent organization is the plan's ultimate owner—"the legal entity that exercises a controlling interest . . . directly or through a subsidiary or subsidiaries, and which is not itself a subsidiary of any other legal entity" (Centers for Medicare & Medicaid Services 2021a).[40] CMS also requires plans to indicate whether they are for-profit and nonprofit entities.

In January 2021, there were 309 distinct parent organizations offering Medicare health plans, with 26.6 million enrollees (Table 3-4, p. 102). Among them, 123 parent organizations operated at least one plan on a for-profit basis, and those for-profit plans had 19.9 million enrollees (about 75 percent of total enrollment). The number of parent organizations operating nonprofit plans was larger, but those plans accounted for only about 25 percent of total enrollment.

We conducted an internet search of the parent organizations with for-profit plans between December 2020 and February 2021 to determine (1) whether the organization was publicly traded or privately owned and (2) whether the organizations that are privately owned have received any investment from PE firms. Only 12 parent organizations were publicly traded, but they accounted for about 90 percent of enrollment in for-profit plans (18.0 million out of 19.9 million) and roughly two-thirds of total enrollment (under "Detail on for-profit companies" in Table 3-4, p. 102). The subset of publicly traded parent organizations is dominated by six large companies—Anthem, Centene, Cigna, CVS Health, Humana, and UnitedHealth—that collectively have 17.7 million enrollees (data not shown). The remaining 111 parent organizations that operate for-profit plans are privately owned and account for about 7 percent of total enrollment.

We found six parent organizations that are currently owned by PE firms as the result of buyouts. (Given the lack of comprehensive data on PE investment activity, there could be other PE-owned organizations that we were unable to identify.) In 2021, those organizations offer a total of 133 plans, including employer plans, and have about 497,000 enrollees, which represents about 1.7 percent of total enrollment. The bulk of those enrollees—about 450,000—are in MA plans that two organizations operate in Puerto Rico. In February 2021, one of those organizations announced it would sell its MA plans in Puerto Rico to Anthem (Tepper 2021). Once that transaction has been completed, PE-owned organizations will account for less than 1 percent of total health plan enrollment.

TABLE 3–4

Privately owned for-profit companies account for a relatively small share of Medicare health plan enrollment, 2021

	Parent organizations	Plans	Enrollees (in millions)	Share of enrollees
Type of company				
For profit	123	4,750	19.9	74.8%
Nonprofit	208	1,582	6.7	25.2
Total	309*	6,332	26.6	100.0
Detail on for-profit companies:				
Publicly owned	12	3,676	18.0	67.6
Privately owned	111	1,074	1.9	7.3

Note: The figures in this table are based on January 2021 enrollment in health plans that provide Part A and Part B benefits, which includes all types of Medicare Advantage plans, cost plans, Medicare–Medicaid Plans, and the Program of All-Inclusive Care for the Elderly. We counted plans using unique combinations of contract number and plan number. The table does not include stand-alone Part D prescription drug plans. Components may not sum to totals due to rounding. *There are 22 parent organizations that have both for-profit and nonprofit divisions. These parent organizations are counted in both the "For profit" and "Nonprofit" rows. The total unduplicated number of parent organizations that offer health plans is thus 309 instead of 331.

Source: MedPAC analysis of CMS health plan enrollment data and research on health plan ownership.

In addition to buyouts, we identified 25 parent organizations where PE firms have made other investments that are either active or have recently concluded. These investments appear to be venture capital for new companies or growth capital for more established companies that want to expand. In 2021, these organizations offer 262 plans and have about 264,000 enrollees, which equals about 1 percent of total enrollment. (As with the buyouts, there may be other recipients of PE investment that we could not identify due to data limitations.) Many of these investments appear to be targeted at three types of plan sponsors: startup health insurers focused on MA and/or the ACA exchanges, provider-sponsored institutional special needs plans, and PACE.

Startup health insurers focused on MA and/or the ACA exchanges

During the past decade, several new health insurers have formed to participate in the MA program and the ACA health insurance exchanges. Some companies—such as Alignment Healthcare, Clover Health, and Devoted Health—focus exclusively on MA and have no other lines of business. Other companies, such as Oscar Health, focus primarily on the exchanges but have expanded into MA, and at least one company, Bright Health, has significant enrollment in both sectors. None of these startup insurers operate Medicaid managed care plans or have indicated that they plan to do so.

Four of these companies—Bright Health, Clover Health, Devoted Health, and Oscar Health—have touted their use of information technology as a feature that distinguishes them from traditional insurers (for example, by enabling them to improve the beneficiary experience or better identify beneficiaries who need preventive care). These companies present themselves as startup tech companies as much as startup health insurers, and they are sometimes referred to as "insurtechs" (Accenture Insurance 2019, Muoio 2019). All four companies have raised substantial amounts of venture capital, ranging from about $800 million to $1.6 billion. Alignment Healthcare, Clover Health, and Oscar Health became publicly traded companies earlier this year, and Bright Health also plans to become publicly traded this year (Minemyer 2021, Schubarth 2021, Vaidya 2021, Wilhelm 2021).

Provider-sponsored institutional special needs plans

Institutional special needs plans (I–SNPs) are specialized MA plans that restrict their enrollment to beneficiaries who need the level of care provided in a long-term care facility for 90 days or longer. The sector has always been relatively small due to limited interest from plan sponsors and nursing homes. In 2021, there are a total of 172 I–SNPs, with about 91,000 enrollees.[41] UnitedHealth has long been the primary sponsor of I–SNPs; its plans cover about 65 percent of all I–SNP enrollees. The second-largest sponsor, Anthem, accounts for only 7 percent of the market.

However, over the past five years, a growing number of nursing homes have started becoming plan sponsors in their own right—as opposed to simply participating in the provider networks of MA plans—and offering an I–SNP to the residents of their facilities. For nursing homes, these provider-sponsored I–SNPs are viewed as a way to get more control over their revenues (the share of residents enrolled in MA plans has been growing, but MA payment rates for skilled nursing care are generally lower than FFS rates) and retain any profits generated by the I–SNP model, which focuses on reducing hospital admissions by providing more primary care in the nursing home.

PE firms have invested in companies that help launch and operate these new I–SNPs. These companies first recruit nursing homes in a geographic region, usually a metropolitan area or state, to participate in the I–SNP. These plans are often structured as joint ventures between the PE-backed companies and the nursing homes. As part of this process, these companies reach an agreement with the nursing homes on the amount of capital that each side will invest in the plan and how its profits and losses will be shared. According to one consultant we interviewed, these risk-sharing arrangements vary across nursing homes, even among the facilities that participate in the same plan. The PE-backed companies also provide funding to help the participating nursing homes obtain an insurance license, if needed, and meet state insurance requirements to maintain sufficient capital reserves. The companies also perform many of the plan's administrative functions, such as assembling provider networks and paying claims. One of these companies, AllyAlign Health, has developed 25 plans that collectively have about 10,000 enrollees.

Representatives for one of these companies believed that PE funding had played an important role in facilitating the company's expansion. The company had used the funding for a variety of purposes, including developing case management software that was better suited for institutional settings and hiring more capable staff. These representatives felt that PE funding was helping the company expand its operations much more rapidly than it would have if it had relied solely on the profits generated by its existing plans. These representatives also stated that the company could not have obtained a similar amount of capital from a traditional commercial bank.

Program of All-Inclusive Care for the Elderly

PACE is another type of specialized plan that serves beneficiaries who need the level of care provided in a nursing home. Unlike I–SNPs, which largely serve beneficiaries who are already in nursing homes, PACE targets beneficiaries who still live in the community. PACE uses a distinctive model of care based on adult day-care centers that are staffed by an interdisciplinary team that provides therapy and medical services. Almost all PACE enrollees are dually eligible for Medicare and Medicaid, and PACE plans cover all Medicare and Medicaid services. PACE plans are typically small, and overall enrollment is fairly low (about 50,000).

For many years, PACE plans were required to operate as nonprofit entities, but CMS lifted this restriction in 2015 after a statutorily mandated demonstration found that for-profit PACE plans provided care that was comparable in quality (Centers for Medicare & Medicaid Services 2015). Since then, there has been some PE investment in for-profit PACE plans. The most notable example is probably InnovAge, a nonprofit PACE plan in Colorado that was acquired by a PE firm in 2016 and converted into a for-profit company (Lagasse 2016). Since then, InnovAge has acquired other plans in several states and become the largest PACE sponsor in the country, accounting for about 12 percent of total PACE enrollment. The company became publicly traded earlier this year (InnovAge 2021). Another example of PE investment is WelbeHealth, which has received VC funding and entered the PACE market in 2019. Unlike InnovAge, which has grown primarily by acquiring existing plans, WelbeHealth has focused on developing new PACE plans.

PE investment in companies that work for MA plan sponsors

In addition to investing in certain MA plan sponsors, PE firms have also invested in an array of related companies that perform a variety of functions for plan sponsors. Many of these related companies either provide services directly to MA enrollees or provide care management (or both), and some are paid using value-based arrangements where the company bears some degree of financial risk for an enrollee's overall spending. Most of these companies are relatively new, so VC funding and growth capital appear to play a larger role than leveraged buyouts.

In this section, we provide some examples of the companies that have received funding from PE firms. We cannot offer a comprehensive overview given the limits on the available data about both PE investment activity and the extent of the relationships between these companies and MA plan sponsors, but we highlight some areas that have attracted investment in recent years.

Primary care

PE firms have invested in companies that are using several distinct business models to revamp the delivery of primary care. One set of companies operates their own networks of primary care clinics that focus largely or entirely on serving MA enrollees. These companies are paid by MA plan sponsors on a capitated basis and agree to take full financial risk for the overall Medicare costs of the enrollees they serve. Two companies that use this model and have received VC funding are Oak Street Health and Iora Health.[42] According to the companies' websites, as of March 2021, Oak Street operated a total of 89 clinics in 13 states, while Iora Health had 47 clinics in 8 states. Oak Street became a publicly traded company in July 2020 (Reuter 2020). At the time of its IPO, the company had contracts with 23 plan sponsors, with Humana accounting for about half of its capitated revenues, and it served 55,000 MA enrollees where it was paid on a capitated basis (Securities and Exchange Commission 2020b). Iora Health remains privately owned, and information on its relationships with MA plan sponsors is not available.

A second set of PE-backed companies, such as Aledade and agilon health, form joint ventures with physician practices that want to participate in value-based contracts with health plans. These companies do not buy the practices; instead, through the joint ventures, they bear some of the financial risk from the value-based contracts and support the practices in several ways, such as by providing better information technology, performing utilization management, and managing relationships with outside specialists. In 2020, Aledade-affiliated practices served about 100,000 MA enrollees through value-based contracts, although the amount of risk the practices bear under those contracts is unclear (Landi 2021).

Another PE-backed company, Cano Health, uses both of these models. As of January 2021, the company served about 85,000 MA enrollees where it was paid on a capitated basis. Like Oak Street, the company has relationships with numerous MA plan sponsors, but Humana accounts for the majority of its capitated enrollees (Cano Health 2021). The company became publicly traded in 2020 (Cano Health 2020).

A third set of companies focus on delivering primary care in beneficiaries' homes to improve their health and avoid expensive emergency room visits and inpatient stays. These companies use their own providers (usually nurse practitioners and physician assistants) to deliver the in-home care and often focus on serving beneficiaries with complex health conditions. Several companies that use this model—such as ConcertoCare, DispatchHealth, Landmark Health, and Ready Responders—have received funding from VC firms. Some of the companies, such as Landmark Health, participate in value-based contracts, while others may be paid by plans on an FFS basis. Earlier this year, UnitedHealth's Optum subsidiary agreed to buy Landmark Health (Donlan 2021).

Many of these companies (in all three models) participate in other Medicare value-based programs. For example, Oak Street Health, Iora Health, agilon health, Cano Health, and Landmark Health have expanded into FFS Medicare by participating in CMS's direct contracting model (Center for Medicare & Medicaid Innovation 2020). In contrast, Aledade originally focused on developing accountable care organizations in the Medicare Shared Savings Program before expanding into value-based contracts with MA plans.

Post-acute care

PE firms have also invested in companies such as CareCentrix and naviHealth that manage the use of post-acute care on behalf of MA plan sponsors. These companies assess enrollees' care needs, encourage the use of less expensive care when appropriate (such as home health instead of skilled nursing care), and try to reduce the number of hospital readmissions. Both companies also participate in value-based contracts. Each company has been publicly traded or PE owned at different points. CareCentrix is currently owned by a PE firm, while naviHealth is now owned by UnitedHealth's Optum subsidiary, which bought it from a PE firm in 2020 (Landi 2020b).

Chronic kidney disease and end-stage renal disease

Policymakers have recently made two changes to Medicare that affect beneficiaries with chronic kidney disease (CKD) or end-stage renal disease (ESRD). The first change was the enactment of the 21st Century Cures

Act, which allowed beneficiaries with ESRD to enroll in MA plans starting in 2021. (Before that, beneficiaries who developed ESRD after enrolling in an MA plan could remain in the plan, but those who already had ESRD were prohibited from newly enrolling in a plan.) The second change was CMS's development of the Kidney Care Choices model, which aims to improve care for beneficiaries with CKD and ESRD (for example, by slowing the progression from CKD to ESRD and encouraging the use of home dialysis when possible). The model was also scheduled to start in 2021 but has been delayed to 2022.

These policy changes have led VC firms to invest in startup companies that focus on managing care for the CKD and ESRD populations. At least four companies in this sector—Cricket Health, Monogram Health, Somatus, and Strive Health—have received VC funding. Each company works with MA plans and has expressed interest in participating in value-based contracts, but the full extent of their relationships is unclear. One leading MA plan sponsor, Humana, has signed contracts with Monogram Health, Somatus, and Strive Health to care for CKD/ESRD enrollees in selected states.

Collection of diagnosis codes

Medicare payments to MA plans are risk adjusted to account for differences in enrollees' health status. The risk adjustment system that CMS has developed relies partly on the diagnosis codes from inpatient, outpatient, and physician claims, which gives MA plan sponsors an incentive to document all valid diagnosis codes for their enrollees. PE firms have invested in companies such as Cotiviti, Signify Health, and Vatica Health that help plan sponsors collect diagnosis codes. (Signify Health became a publicly traded company earlier this year.) These companies perform activities such as analyzing claims data to identify instances where diagnosis codes might be missing, using information technology to collect diagnosis codes directly from physicians' electronic health records, and conducting in-home health assessments. (Some of these companies also have other lines of business, such as helping providers participate in bundled payment programs and helping plans collect quality data.) Collecting more diagnosis codes increases Medicare payment to plans, although it is unclear whether PE-owned companies allow plan sponsors to collect more codes than they would by using other approaches, such as collecting codes themselves.

In addition, the value-based contracts that many companies described in this section sign with MA plan sponsors may also encourage the collection of more diagnosis codes. For example, companies that sign "full-risk" contracts with MA plan sponsors may be paid using capitated rates that equal a share of the plan's Medicare revenues. This arrangement gives the company with the value-based contract an incentive to collect more diagnosis codes because doing so generates more revenue for the plan sponsor, which in turn leads to more revenue for the downstream company.

Some MA plan sponsors also make investments in outside companies

We have focused on instances where PE firms invest in companies that work for MA plan sponsors, but it is worth noting that plan sponsors can also be investors in their own right. Several plan sponsors have their own VC arms, including for-profit sponsors (UnitedHealth's Optum Ventures), nonprofit sponsors (Intermountain Ventures, Kaiser Permanente Ventures, UPMC Enterprises), and a mix of for-profit and nonprofit sponsors (the Blue Cross/Blue Shield affiliates' Blue Venture Fund). As one might expect, these funds invest in startup companies that could benefit health plans and have focused on areas such as information technology and care management. For example, they have invested in some of the companies discussed in this section: CareCentrix (Blue Venture Fund), DispatchHealth (Optum Ventures), naviHealth (Blue Venture Fund), and Somatus (Blue Venture Fund, Optum Ventures). Plan sponsors that do not have their own VC arms also make investments: For example, Centene recently invested in a company working to improve the interoperability of health care data (Landi 2020a).

In addition, the second-largest MA plan sponsor, Humana, has participated in several buyouts led by PE firms. In 2018, Humana and two PE firms acquired the post-acute care company Kindred Healthcare, which operated long-term care hospitals (LTCHs), inpatient rehabilitation facilities (IRFs), home health agencies, and hospices. As part of the deal, Kindred Healthcare was split into two separate companies. The first company, which kept the Kindred Healthcare name, operates the LTCHs and IRFs and is owned entirely by the PE firms. The second company, called Kindred at Home, operates the home health agencies and hospices and is jointly owned by the PE firms (60 percent) and Humana (40 percent). Humana has the right to buy out the PE firms and take full ownership (Kindred Healthcare 2018, Mullaney 2018).

Later that year, Humana and the same PE firms purchased Curo Health Services, a hospice provider, and added it to Kindred at Home (Holly 2018).

Finally, in 2020, Humana and one of the PE firms involved in the Kindred and Curo acquisitions started a joint venture to develop a network of primary care centers focused on serving Medicare beneficiaries. The centers will be managed by a Humana subsidiary. The PE firm has a majority stake in the joint venture and can require Humana to buy it out over the next 5 to 10 years (Humana 2020).

Effect of MA-related investments on Medicare costs

We are not aware of any research that evaluates the effect that PE investment in MA-related companies has on Medicare costs. Under the MA payment system, those investments would not change Medicare spending unless they had an impact on plan bids, quality bonuses, or risk scores. Conducting that type of analysis would be challenging for several reasons. For example, CMS collects information on each plan's ultimate owner—the parent organization—but does not know which organizations are owned by PE firms. The agency also does not collect information on plan sponsors' contracting arrangements with other companies (which means, for example, that there is no database that identifies which plans use PE-backed companies to provide care management for enrollees with complex health needs). In addition, researchers would probably need to use encounter data to assess whether PE-backed companies had any effect on enrollees' service use. However, the existing encounter data are incomplete and may not provide an accurate picture of utilization patterns, especially in key areas like post-acute care.

Conclusion

Private equity firms raise capital from entities such as pension funds and endowments and invest those funds in ways that they hope will generate attractive returns. Their investments can take many forms, but the approach that has generated the most debate is the leveraged buyout, which relies heavily on borrowed money and aims to generate returns within a relatively short time.

The amounts that investors have committed to PE funds have increased in recent years, and PE funds' investment activity has grown accordingly. We found that PE funds have been active in all four sectors we examined in this chapter—hospitals, nursing homes, physician practices, and Medicare Advantage. However, their presence was relatively limited: PE firms owned roughly 4 percent of hospitals, 11 percent of nursing homes, and 2 percent of MA plan sponsors. At least 2 percent of physician practices were acquired from 2013 to 2016, but that figure does not take into account previous PE acquisitions, and it appears to have grown since then.

There is relatively little research on the effects of private equity in the sectors we examined, due in part to the challenges of identifying PE-owned providers, and the findings that are available appear to be mixed. However, we expect to see further research on this issue in the coming years, especially on acquisitions of physician practices, and those studies may provide new insights into the effects of PE investment in health care.

The debate about the merits of private equity involves many issues that lie outside Medicare's purview, such as federal antitrust policy, whether PE firms should bear responsibility for the debt of their portfolio companies, and the tax treatment of carried interest. Even within health care, one major concern—that private equity may consolidate providers to create market power and negotiate higher payment rates—may have limited relevance for Medicare because the program largely sets its own payment rates. Nevertheless, Medicare could be affected in other ways, such as the volume and mix of services that are provided, and the program's payment policies are often an important consideration for PE firms. Investment activity in specific sectors or markets may indicate areas where payment policies should be reexamined (for example, by addressing site-of-service differences in payment rates that make it more profitable to deliver certain services in a higher cost setting) and may highlight areas that could potentially result in lower costs or better quality (such as efforts to develop value-based payment models). ■

Endnotes

1 Some PE firms also make loans in addition to equity investments.

2 Similarly, between 2001 and 2012, the number of initial public offerings (IPOs) in the U.S. averaged 99 per year, compared with 310 IPOs annually between 1980 and 2000 (De Fontenay 2017).

3 These interest payments used to be fully deductible, but in 2017, the Tax Cuts and Jobs Act limited the deduction to make the treatment of debt and equity financing more comparable. Between 2018 and 2021, the deduction is capped at 30 percent of a company's earnings before interest, taxes, depreciation, and amortization (EBITDA). Starting in 2022, the deduction will be capped at 30 percent of a different metric—a company's earnings before interest and taxes (EBIT). Since EBIT is lower than EBITDA, this change will further reduce the amount of interest that companies can deduct.

4 There is also a relatively small secondary market where an investor can sell its ownership stake in a PE fund to another investor before the fund has reached the end of its life span.

5 There can be some overlap between the period when a PE firm is raising money for a new fund and the period when the fund begins making its investments. In these instances, the PE firm has raised some money for the new fund but has not yet reached its overall fundraising target.

6 Under the Hart-Scott-Rodino Antitrust Improvements Act of 1976, firms are generally exempt from this "premerger notification" requirement for deals valued below a dollar threshold (Wollman 2019). The threshold was set at $50 million in 2000 and is adjusted annually by the rate of change in the gross national product. For 2020, the threshold was $94 million.

7 The term *carried interest* apparently traces back to the shipping industry, where captains would receive a share of the profits on the cargo they carried.

8 For this reason, CMS established a category of providers, Special Focus Facilities, to increase oversight of poorly performing nursing homes (Centers for Medicare & Medicaid Services 2021b).

9 In 2016, 69 percent of the nearly 15,500 nursing homes in the U.S. were for-profit entities. Fifty-eight percent of all nursing home were owned by chains (Harrington et al. 2018).

10 However, CMS does require some types of providers and suppliers to demonstrate that they have certain levels of financial assets to operate. For example, when a home health agency initially enrolls, it must demonstrate that it has sufficient initial reserve operating funds to operate for its first three months. Similarly, although there are some exemptions, suppliers of durable medical equipment, prosthetics, orthotics, and supplies must post surety bonds to enroll in Medicare.

11 The NSC processes applications for suppliers of durable medical equipment, prosthetics, orthotics, and supplies. MACs process applications of all other providers and suppliers. The MACs and the NSC are responsible for verifying the provider's name, address, tax identifiers, license, and any history of adverse actions, license revocations, or felony convictions.

12 Suppliers of durable medical equipment, prosthetics, orthotics, and supplies must be revalidated every three years.

13 However, for home health agencies, if an individual or organization acquires more than a 50 percent direct ownership interest within the first 36 months of the agency's initial enrollment (or a previous CHOW), the prospective owner must apply as a new enrollee absent a regulatory exception.

14 Buyers that reject assignment must apply as an initial applicant to Medicare and may be subject to a full initial accreditation survey.

15 Other changes in enrollment information must be reported to CMS within 90 days.

16 The ACA authorized CMS to expand screening requirements for enrolling all types of providers and suppliers in Medicare and Medicaid, not just nursing homes. For example, CMS places providers in risk categories and conducts more extensive review of applicants in high-risk categories (such as new home health agencies), including site visits and fingerprinting to conduct felony checks.

17 This expanded authority was intended, in part, to prevent providers or suppliers who committed fraud and abuse and then left the program with unpaid debt to Medicare from reenrolling while shifting their activities to an affiliated entity.

18 CMS often regards the transfer of an asset as a CHOW, but not the transfer of a membership interest (Markenson and Woffenden 2019). This distinction means the purchase or sale of a Medicare provider by a PE firm should require a CHOW submission to PECOS, but the entry or exit of investors in the associated PE investment fund would not.

19 We reviewed several state online tools that list provider ownership data. For nursing homes, many states send consumers to CMS's Care Compare tool, which makes a limited amount of ownership information available. CMS does not make comparable ownership information available for general hospitals. A few state websites provided more detailed facility information. For example, California's Department of Public Health posts a data set that lists, for each licensed facility, the names of individuals or organizations with any share of ownership of the licensee as well as the property owner, management company, and administrator. However, the data are not fully populated for all facilities.

20 *Traditional hospitals* refer to general and surgical hospitals that are not small rural critical access hospitals. We identified ownership by conducting an internet search on for-profit hospitals. The list of hospitals we identified may not be complete. In addition, some long-term care hospitals that provide post-acute care are owned by PE firms and are not included in our universe of general and surgical hospitals.

21 However, some research has suggested that adding physician ownership may result in a more favorable selection of patients. For example, see (O'Neill and Hartz 2012).

22 Health systems are defined here as organizations that had at least one acute care hospital and one physician group and were connected through common ownership or joint management. An affiliation was defined as common ownership or a joint management agreement.

23 Two such firms, TeamHealth (owned by PE firm Blackstone) and Envision (owned by KKR), have been at the center of the recent controversy over surprise billing (Gottfried 2020).

24 The term "middle-market" refers to firms that make smaller investments in lesser known companies. Definitions of middle-market PE investors differ, but PitchBook defines them as funds with $100 million to $5 billion of capital commitments.

25 This strategy is similar to the "physician rollup" approach used by physician practice management (PPM) companies in the 1990s (Robinson 1998). Most publicly traded PPMs went bankrupt, which one prominent economist attributed to the industry trying to grow "mindlessly fast in a fatal pas de deux with a financial market that egged the industry on with unrealistic expectations about future earnings" (Reinhardt 2000). Because more recent deals are structured differently from PPMs—including shared equity with physician owners—they may be less likely to fail (Casalino et al. 2019).

26 Pathologists, emergency medicine physicians, anesthesiologists, radiologists, hospitalists, neonatologists, and a limited number of other specialists are thought to be in this category.

27 To address these situations, the Congress included the No Surprises Act in its fiscal year 2021 omnibus spending bill. Beginning in 2022, commercial insurers may charge patients only in-network cost sharing for all out-of-network emergency facility and professional services. The law sets up a system of arbitration to determine the amounts that insurers pay facilities and clinicians. See Adler and colleagues (2021) for more details.

28 For an example, see https://www.washingtonpost.com/business/2020/12/31/brius-nursing-home/. A related concern is that these complex corporate structures make it difficult to identify a nursing home's ultimate owner and to look for quality of care issues across a chain's facilities.

29 Labor in nursing homes is a mix of therapy staff and nursing staff, such as more costly registered nurses (RNs) and less costly licensed practical nurses (LPNs) or certified nursing assistants. Federal requirements for nursing home staffing state that a nursing home must have 24 hours of licensed nurse (RN or LPN) coverage every day, including one RN on duty for at least 8 consecutive hours. Some states have higher or more specific staffing requirements. According to a recent study, granular staffing data from the Payroll-Based Journal (PBJ) "suggest that a large proportion of nursing homes often have daily staffing below CMS's case-mix-adjusted expected staffing levels" and that "for each staffing type and across all ownership categories, the mean PBJ-reported hours per resident day were lower than reported in CASPER [the Certification and Survey Provider Enhanced Reports]," which contain facility-reported staffing data (Geng et al. 2019). Analysis in a recent New York Times article found that the PBJ data may also overstate patient-care staffing depending on how a nursing home records the time of RNs in administrative positions (Silver-Greenberg and Gebeloff 2021).

30 The separation of a nursing home's assets and operations may involve a real estate investment trust (REIT), which is a public or private corporation that invests in real estate with exemptions for corporate income tax provided it meets "requirements related to sources of income and assets, payment of dividends, and diversification of ownership" (Harrington et al. 2011). In addition to the corporate tax benefits, REITs can be advantageous because they have "rental agreements in which, in addition to basic rental charges, the nursing home operating companies pay a proportion of their income to the REITs, allowing nursing homes to shift profits to the REITs and further reduce their corporate taxes (Harrington et al. 2011). REITs also offer liability protection when nursing home operators are sued because the real estate assets are legally separate from the operator.

31 The divestment described here is intended to show the effects of restructuring and rebranding at that time. While Fillmore Capital Partners retains ownership, some of the company names and ownership arrangements have changed since publication of the source article. For example, in June 2020, AseraCare Hospice was acquired by Amedysis Inc.

32 Casalino and colleagues describe PE payments to physician owners of add-on acquisitions of two to four times EBITDA or less (Casalino et al. 2019). Helm describes the same types of payments as 30 percent to 40 percent less than those paid for the platform practice (Helm 2019).

33 For example, see Americans for Financial Reform (2020), Goldstein et al. (2020), Laise (2020), Spanko (2020), and Tan and Chason (2020).

34 Cooper and colleagues examined whether a large emergency physician staffing company that engages in out-of-network billing—EmCare, today a subsidiary of Envision Healthcare—affects commercial insurance payments for physician and hospital services (Cooper et al. 2020a). After EmCare entered into a contract with a hospital and began billing for ED services, insurance payments and patient cost-sharing for ED physicians doubled and hospital facility payments also increased, driven by higher use of imaging and a rise in admissions. The authors used data from 2011 through 2015, which included a two-year period during which EmCare was owned by a PE firm (from 2011 to 2013) (Harvard T.H. Chan School of Public Health 2019). Because EmCare was owned by a PE company for only about half of the period studied, it is unclear whether EmCare's impact on payments was related to PE ownership.

35 For a discussion of our methodology for standardizing hospital costs see our March 2020 report to the Congress (Medicare Payment Advisory Commission 2020).

36 We conducted two checks of the robustness of our findings by examining (1) 2018 costs for all hospitals, including those with fewer than 500 discharges, and (2) 2019 costs for hospitals with more than 500 discharges. We found similar results to those described in the chapter.

37 There is a large volume of literature on the effects of PE ownership of nursing homes generally on the quality of patient care and on the relationship between staffing and quality of care. For the latter see (Bostick et al. 2006).

38 See endnote 34.

39 Compared with MA plans, relatively few beneficiaries are enrolled in these other types of private plans. In January 2021, there were 25.9 million enrollees in MA plans and a total of 694,000 enrollees in cost plans, Medicare–Medicaid Plans, and PACE.

40 For example, CVS Health Corporation is listed as the parent organization on a total of 42 contracts. However, none of the legal entities that signed those contracts with CMS have "CVS" in their name. All of those entities were part of Aetna before CVS acquired it in 2018; most of them still have "Aetna" in their name, and some even have the names of other companies that Aetna acquired in earlier years, such as "Coventry" or "First Health."

41 We counted plans based on the combination of contract numbers and plan numbers, but this approach arguably overstates the size of the I–SNP sector because many plans have very few enrollees. Only 96 plans have more than 100 enrollees.

42 Another privately owned company—ChenMed—uses this model, but we could not find any evidence that it has received PE funding.

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Abstract

We examine the survival, profitability, and employment profiles of private equity (PE) acquired hospitals. Target hospitals sustain their survival rates and improve in profitability. Although employment and wage expenditures substantially decline, the effect differs across employee types: The decline in core medical workers is temporary and quickly reversed, while the decline in administrative workers and their wages persists. These changes are more pronounced for nonprofit targets, acquisitions into larger systems, and PEs with healthcare industry expertise. We do not find patient outcomes to worsen at acquired hospitals. Our results suggest that PE acquirers improve hospitals' operational efficiency without compromising healthcare quality.

Keywords: Private Equity, Hospital Acquisitions, Employment, Operational Efficiency, Real Patient Outcomes

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1 Introduction

It is estimated that private equity (PE) firms invested around $200 billion into the U.S. healthcare industry over the past decade, a large fraction of which is invested in hospitals.[1] There are opposing views regarding the growing presence of PE firms in the hospital industry. Proponents claim that they provide hospitals with much needed managerial expertise and operational reform, which help turn around struggling hospitals. Opponents voice concerns that PE firms load hospitals with debt, sell assets, lay off workers, and even close hospitals. This debate is particularly important given the economic significance of the healthcare industry. This industry contributes to nearly 20% of total U.S. GDP, provides critical healthcare to local communities, and ranks among the top ten job providers in the U.S. In this paper, we seek to shed light on this current debate by examining various outcomes at hospitals acquired by PE firms.

We study the survival, operating performance, and employee profiles at PE-acquired hospitals. We first examine closure rates and profitability outcomes after PE acquisitions, and then track the variation in employment. In this investigation, we separately examine the changes in core medical workers and administrative workers, because the former is key to providing high-quality healthcare, while the latter is a main source of hospitals' wasteful spending (Shrank et al., 2019). Finally, we look into changes in patient composition as well as mortality rates and readmission rates. Overall, our evidence suggests that PE acquisitions are associated with substantial profitability improvement and cost-cutting at target hospitals. The cost-cutting reflects PEs' operational expertise (Jensen, 2019; Kaplan and Stromberg, 2009), as it is focused on administrative expenditures but not on core medical functions. We also document little changes in patient composition or outcomes.

We compile a sample of 1,218 M&A deals in the hospital industry over the period spanning from 2001 to 2018. Our focus is on 281 deals where the acquirer is a for-profit organization, either a PE firm, a PE-owned hospital or a hospital with no PE ownership. These deals involve 610 unique target hospitals. We analyze PE-acquired hospitals relative to a control group of non-acquired hospitals that are closely matched by loca-

[1] Source: A city's only hospital cut services. How locals fought back. *Wall Street Journal*, Aug. 2020.

tion, time, and pre-event characteristics. We also benchmark the effects of PE buyers against non-PE, for-profit buyers by comparing the outcomes of the hospitals they acquire. These comparisons help address concerns that our results may capture differences across location, hospital type, or the selection of targets by for-profit acquirers in general.

We track target and control hospitals over two horizons around the events: a short-run horizon where we compare outcomes during the four-year pre-event window ([-4, -1]) with those in the four-year post-event window ([0, 4]), and a long-run horizon where we contrast the pre-event window with years [5, 8] after the event. This choice is motivated by the consideration that restructuring events often involve large scale transformation and take a long time to implement. Looking only at the short-run effects could mask important implications of reforms conducted at acquired hospitals. Indeed, prior studies on the roles of PEs examine both the short-run effects following PE buyouts (e.g., Kaplan 1989; Davis et al. 2014; Lerner et al. 2011; Bernstein 2022) and long-term consequences (e.g., Kaplan and Stromberg 2009). Kaplan and Stromberg (2009) report that holding periods of PE firms have increased substantially since the 1990s, documenting that only 12% of deals are exited within 24 months, while in half of the deals, PE firms maintain ownership for over 6 years.

We start our analysis with the survival and profitability of acquired hospitals, evaluating the concerns raised in the popular press that PE firms tend to close hospitals. We find no evidence of excessive closure of PE-acquired hospitals. PE-acquired hospitals also significantly improve their operating profitability. The profitability boost persists over the 8-year window following PE acquisitions. It is thus unlikely to be a manifestation of PE firms' window-dressing efforts in the short-term while sacrificing the long term potential of acquired hospitals. These results help alleviate the concerns that PE owners excessively close down hospitals, leading to the loss of health care and jobs in the community.

The survival results do not imply, however, that all jobs are preserved at PE-acquired hospitals. In fact, an important driver of M&A transactions is to eliminate excess employment at acquired firms or overlapping employment between merging parties. We find that employment at PE-acquired hospitals declines by 7% over the first four-year event window and this reduction reaches 10% over the long-term horizon. As a natural consequence, the total wage bill at PE-acquired hospitals goes down by 7% over the first four years and become 11% lower than its pre-acquisition level at the end of year 8 following the deal. These findings suggest substantial cost-savings at PE-acquired hospitals.

Employment cut can be a double-edged sword. On the one hand, it helps reduce costs and improve profitability. On the other hand, laying off essential medical workers can compromise health care quality and the long-run viability of acquired hospitals. Therefore, we examine employment outcomes separately for administrative workers and core workers. We define core workers as nurses, pharmacists and physicians—those critical in the delivery of health care.

We find that the number of core workers at PE-acquired hospitals temporarily drops over the first event window but bounces back over the second event window. In other words, at the end of our eight-year horizon, the number of core workers at PE-acquired hospitals does not differ from its pre-acquisition level. One interpretation of this finding is that the initial restructuring following an acquisition leads to a temporary decline in core workers. After the initial stage, the operational environment stabilizes and the hospital regains core workers back to the pre-event level. We confirm this finding using an alternative measure, the ratio of core workers relative to the number of patients.

Our examination of administrative workers reveals different dynamics from that of core workers. We observe a large decline in administrative workers, by 18%, at PE-acquired hospitals over the first event window. Different from core workers, the drop in administrative workers does not revert back, but stays at 22% under the pre-acquisition level by the of the second event window. Such reductions also show up when we scale administrative workers using total patient counts.

Tracking the evolution of core and administrative workers at PE target hospitals, we find that the above-documented changes do not start taking place prior to PE acquisitions, but occur after the acquisitions. The declines in administrative workers persist for many years after the takeover, while the reductions in core workers are short-lived. The lack of pre-event trends alleviates the concern that PE firms may select targets that already started implementing improvements along those observable dimensions prior to being acquired.

We perform several additional analyses to bolster our inferences. First, we perform a "placebo" test looking at the changes that occur at hospitals acquired by non-PE buyers. We find that target hospitals of non-PEs exhibit worse survival likelihood compared to their control group. After the acquisition, these hospitals also reduce their employment and wages, but such reductions are relatively small and statistically insignificant over the long run. In terms of worker composition, we observe a persistent decline in core workers, but not in administrative workers at non-PE target hospitals. By the end of the eight-year period, core workers are nearly 25% lower than their pre-acquisition levels. There is no reduction in administrative workers across either the short-run or the long-run event window. Results from this set of analysis suggest that the operational changes documented above are unique to PEs, but not present for other for-profit acquirers in general.

Next, we examine wage rates offered to core and administrative workers. We do not observe a meaningful change in the wage rate paid to core workers in PE-acquired hospitals, but a substantial decline in the wage rate of administrative workers, by around 7% over the long run. This result reinforces the argument that PE acquirers trim spending related to administrative functions. In addition, we consider an alternative definition of core workers that only consists of nurses and pharmacists, while excluding physicians. This helps address the concern that physicians may be hired through part-time contracts and that the cost reports do not track their hours in the same way as other full-time employees. Furthermore, we control for state-year fixed effects to remove the confounding effects of changing local conditions. Finally, we test the sensitivity of our results to the sampling procedure in several ways. In one test, we change the hospital characteristics used in the matching process. In another, we exclude cases where the target hospital and its matched control are located nearby, so as to mitigate the spillover effects from local acquisitions. We also keep a balanced sample by requiring hospitals to have observations for multiple years before and after the event. Our results remain largely unchanged to these measures or sample refinements.

Taken together, our analysis reveals stark contrasts between changes in core medical workers and administrative workers at PE-acquired hospitals. These findings suggest that PE firms focus on reducing excess overhead costs while sustaining critical healthcare providers, likely because of their operational expertise and business skills.

Next, we explore the heterogeneity of our findings to shed light on mechanisms. We start by comparing target hospitals that operated as nonprofit to those operated as for-profit organizations prior to being acquired. To the extent that nonprofit hospitals face no investor scrutiny, they may operate less efficiently prior to PE acquisitions and undergo greater cost-cutting under PE ownership. Our evidence is consistent with this conjecture. At previously nonprofit hospitals, both total employment and wage bills decline more substantially. Administrative workers experience a 36% decline over the long-term event horizon, and there is barely any change in core workers. In contrast, target hospitals with previously for-profit status experience much smaller cost-cutting, especially through the reduction of administrative workers.

We next examine economies of scale as a source of operational change at PE-acquired hospitals. PE firms purchase a large number of units and organize them into larger systems through a "roll-up" process. Doing so might give them greater flexibility to reallocate and consolidate human capital within systems to achieve higher efficiency.[2] For example, hospitals in larger systems can more easily combine various administrative functions such as finance, accounting, and marketing. To test this conjecture, we compare cases where PE firms purchase a target hospital from a relatively small system and include it in a much larger system to cases where the target hospital experiences a smaller increase in system size. We find that the reductions in total employment, wage bills, and administrative employees are primarily concentrated in the former case.

Finally, we look into PE firms' expertise in the healthcare industry. We posit that PE firms with greater specialization in the healthcare industry can better target and improve inefficiencies. Our evidence supports this conjecture. PE firms more focused on the healthcare industry are associated with greater employment cuts, more signifi-

[2] See Cohn et al. (2022) for evidence on how PEs use roll-up acquisitions outside the hospital industry.

cant increases in core workers, and larger reductions in administrative workers at target hospitals, compared to PE acquirers with less expertise in the healthcare industry.

These findings reveal some key mechanisms through which PE firms help improve the operations of target hospitals. Specifically, PE firms make nonprofit hospitals accountable to investors, form large hospital systems to achieve economies of scale, and accumulate experience in the healthcare industry to become more specialized in their operations. Our findings also help answer important questions such as why cutting administrative burden requires the intervention from PE firms. We argue that, while it may be easy to detect administrative burden, it requires expertise and strategic decisions to restructure administrative functions smoothly without interrupting the normal course of business. Non-PE acquirers and pre-deal executives of target hospitals may not possess such expertise. They may also lack the high-powered incentives that PE firms have to trim employment and improve efficiency.

How do the changes in PE-acquired hospitals affect patients? The answer is not clear ex ante. On the one hand, the reduction in overall employment may result in worse patient experiences and outcomes. On the other hand, patient outcomes may not deteriorate, given that the trimmed employment largely consists of administrative workers and not core workers, especially in the long run. To see how patients fare at PE-acquired hospitals, we examine mortality rates and readmission rates related to heart attack, heart failure, and pneumonia at acquired hospitals. We do not find that patients at PE-acquired hospitals experience significant increases in mortality rates. Similarly, readmission rates do not increase for PE-acquired hospitals across any of the health conditions we examine. Overall, we do not find deterioration of patient outcomes at PE-acquired hospitals.

Lastly, we examine changes in hospital characteristics and patient composition around PE acquisitions. This analysis sheds light on the concern that PE-acquired hospitals may shift their focus to younger and wealthier patients and offer more profitable services. However, we do not find evidence in favor of such an operational shift. We show that PE targets stay largely unchanged in size, either measured by the number of beds or the number of patients treated. They also do not generate significantly greater revenue, hence the profitability boost we document likely originates from cost-cutting. As we examine patient composition in terms of the ratio of patients treated in the hospital as opposed to outside clinics as well as the percentage of Medicare and Medicaid patients, we find these metrics to stay roughly the same as their pre-acquisition levels. PE target hospitals increase their case-mix index which measures the number of resource intensive patients treated at a hospital, suggesting that target hospitals perform more clinically complex and resource intensive procedures after being acquired. Taken together, our evidence does not support the argument that hospitals drastically shift their operational focus to wealthier patients with better health profiles after PE acquisitions.

Our paper contributes to the growing literature on hospital mergers. The majority of the existing work in this literature focuses on the impact of mergers on hospital prices and costs (Dafny, 2009; Lewis and Pflum, 2017; Schmitt, 2017; Cooper et al., 2019; Dafny et al., 2019; Craig et al., 2021).[3] Prager and Schmitt (2021) investigate the implication of hospital mergers on the local labor market concentration for nurses and pharmacists. We extend this line of research by focusing on PE acquirers and examining their impact on operational efficiency and patient outcomes. Our results suggest that PEs' roles are not limited to aggressive cost-cutting across the board, but they implement selective changes concerning administrative functions. Our findings are also consistent with results in contemporaneous research including Andreyeva et al. (2022) and Duggan et al. (2022), which show that combining standalone hospitals into systems (i.e., "corporitization") and privatizing hospitals lead to efficiency gains. Our paper finds that hospitals are more likely to undergo these operational changes under PE ownership, and highlight the role of PEs' operational expertise in achieving such improvement.

Our paper is also related to the contemporaneous studies on the role of PE firms in the healthcare industry. Gandhi et al. (2020) document positive effects of PE firms on nursing homes in highly competitive markets. Gupta et al. (2021), on the other hand, find that PE owners reduce the quality of care at nursing homes. Our analysis com-

[3] Beaulieu et al. (2020) examine the quality of healthcare at acquired hospitals, but do not focus on PE acquirers. Bruch et al. (2020) use a smaller sample to examine the effect of PE acquisitions on hospitals' accounting performance and patient characteristics, but do not look at their effects on hospital employee profiles.

plements these studies by examining PE acquirers in the hospital industry, an industry accounting for a large fraction of employment in many local labor markets. Liu (2021) investigates the mechanisms through which PE firms increase healthcare prices and attributes a large portion of such price impact to PEs' superior bargaining power with private insurers. Different from this study, our paper primarily focuses on operational and employment outcomes at PE-acquired hospitals. We also document important differences in post-acquisition outcomes between PE-targets and non-PE targets, generating a more nuanced and comprehensive understanding of the role of PE firms in this industry.

We contribute to the rich literature examining the operational and employment effects of PE buyouts (see, among others, Kaplan 1989, Bernstein and Sheen 2016 Boucly et al. 2011, Davis et al. 2014, Olsson and Tåg 2017, and Antoni et al. 2019), as well as the burgeoning research documenting PEs' involvement in specific industries (Bernstein and Sheen, 2016; Spaenjers and Steiner, 2021; Fracassi et al., 2022; Ewens et al., 2022; Howell et al., 2022). We find that in the hospital industry, PE firms implement operational changes by reducing administrative employees while preserving employees critical in providing health care. These results are consistent with the operational engineering role of PE firms as elaborated in Kaplan and Stromberg (2009). They are also complementary to evidence in Bernstein and Sheen (2016), which document operational changes in restaurant chain following PE buyouts. Importantly, we highlight key mechanisms through which PEs improve hospital operations such as achieving economies of scale and creating investor accountability.

More generally, our paper is related to the emerging literature studying the intersection of healthcare and finance. Complementary to our focus on how PE firms affect survival, profitability, employment and patient outcomes in the hospital industry, recent contributions have examined the effect of financial and credit constraints on hospital outcomes (e.g., Adelino et al. 2015, Adelino et al. 2021, and Aghamolla et al. 2021).

2 Data and Sample

We collect data from several sources. We compile a list of hospital mergers and acquisitions (M&A) from 2001 to 2018 by manually cleaning and combining data from multiple sources, including SDC, Factset, and Becker's Hospital Review. Information regarding hospital characteristics and performance comes from the Centers for Medicare & Medicaid Services (CMS). We extract data on patient mortality and readmission rates from Hospital Compare Outcome Measures, published by the CMS and Hospital Quality Alliance (HQA).

2.1 Hospital M&As and the Classification of Acquirers

Data on hospital M&A activity come from multiple sources. We start from the merger roster during the period of 2001 through 2014 provided by Cooper et al. (2019), and then extend the sample to 2018 following their methodology.

We start from the AHA's Annual Survey of Hospitals and identify the changes in system identifiers of individual hospitals, which likely suggest changes in hospital ownership. We verify whether a change in system identifier is indeed associated with an acquisition by manually validating these events across several M&A databases, including SDC, Factset, and Becker's Hospital Review. In this process, we match the list of AHA system changes with acquisitions recorded in these databases based on the names and locations of target hospitals and acquirers, as well as the completion date of the deals. We also supplement the acquisition list based on information from these databases and record deals that are not correctly captured by changes in AHA system IDs. When the matching between Becker's and AHA is ambiguous, we search internet resources including local newspaper articles and American Hospital Directory (AHD) to verify the accuracy of the matches.

The above process yields a sample of 1,218 M&A deals that occurred during the period of 2001 through 2018. The deals involve 478 unique acquirers and 1,686 unique target hospitals. Among these deals, we focus on 281 acquisitions where the acquirer is a for-profit organization. These deals involve 610 unique target hospitals.

There are two types of hospital acquisitions where the acquirer is associated with a PE firm. First, a PE firm directly acquires a hospital or a system of hospitals. Second, PE-acquired hospitals conduct acquisitions themselves, commonly referred to as "roll-up acquisitions." We label acquirers in both types of deals as "PE acquirers." To identify PE acquirers, we obtain information from Preqin, CapitalIQ, and descriptions in Becker's, and manually verify this information. In the manual verification process, we supplement our data regarding the identities of hospital acquirers from news articles. We identify 117 deals where the acquirer is either a PE firm or a PE-owned hospital, with 419 unique target hospitals. We refer to acquirers that have had no PE ownership as *Non-PE Acquirers*. We have 164 deals by non-PE acquirers, involving 191 target hospitals.

Deals of PE acquirers involve a greater number of target hospitals belonging to a system, with a typical deal involving 3.58 target hospitals. The average deal conducted by non-PE acquirers, in comparison, involves only 1.16 target hospitals.

2.2 Hospital Characteristics Data

We obtain hospital characteristics data from the Healthcare Cost Report Information System (HCRIS) maintained by the CMS. Medicare-certified institutional providers are required to submit their annual cost report to a medicare administrative contractor. Such information is then compiled into the HCRIS. From these reports, we gather data regarding hospital characteristics, employment, and workforce composition.

Hospital characteristics include financial performance metrics such as gross margin, operating income over total assets (*OI/TA*), and returns on assets (*ROA*). It also includes other operational characteristics such as hospital size as measured by the log number of beds (*Log(Beds)*), the log gross (net) patient sales (*Log(Gross (Net) Patient Sales)*), and the log number of patients (*Log(Patients)*), the complexity of operations measured by case mix index (*CMI*), outpatient ratio given by the ratio of outpatient charges over total charges, as well as the percentage of patients that receive Medicare (*%Medicare*) and Medicaid insurance (*%Medicaid*).

We compile various measures of hospital employment, worker composition, and wages to study changes in the operational profile of target hospitals. To start, we construct a measure of total employment. The HCRIS provides data on paid work hours and wages for employees in various occupations. Paid work hours are then converted to full-time equivalent (FTE) employee counts based on the total number of work hours in a year. Specifically, annual employment is defined as the total paid work hours divided by 2,080 (40 hrs/week $\times$ 52 weeks), then converted to log terms (*Log(Employment)*). In addition, we look at the number of hospital workers in relation to the number of patients treated at the hospital by taking the ratio of the two, i.e., *Employment/Patients*. Following Schmitt (2017), the number of treated patients is defined as the number of inpatient discharges multiplied by $(1 + \frac{outpatient\ charges}{inpatient\ charges})$.[4]

For employee composition, we focus on core medical workers and administrative workers. Core medical workers include physicians (including contract physicians), nurses, and pharmacists, who are essential in providing quality health care.[5] Administrative employees are a subset of non-core workers, whose wages constitute an important component of hospital overhead costs (Shrank et al. 2019). Employees outside these categories include maintenance and repair staff, housekeeping, cafeteria employees, etc.

Based on HCRIS wage breakdown across employee categories, we construct various metrics of worker composition. First, we examine the log number of core workers (*Log(Core Workers)*) as well as the log number of administrative workers (*Log(Admin Workers)*). We also measure core and administrative workers scaled by the number of patients treated at the hospital, i.e., *Core Workers/Patients* and *Admin Workers/Patients*.

Finally, we measure the hourly wages paid to core workers and administrative workers, *Log(Core Wage Rate)* and *Log(Admin Wage Rate)*. Hourly wage rate is computed as the total wages paid divided by the total paid hours within each worker category.

[4] This adjustment is necessary for two reasons. First, information on outpatient discharges, i.e., the number of patients treated outside a hospital, is not available to us. Second, since outpatient treatment generally takes up less hospital resources and requires less time from nurses and physicians than inpatient treatment, the adjustment discounts the number of outpatients proportionately.

[5] See Appendix A for detailed job categories. In Appendix C, we show that our results are robust when we apply a more restrictive definition of core workers.

2.3 Patient-Level Outcomes

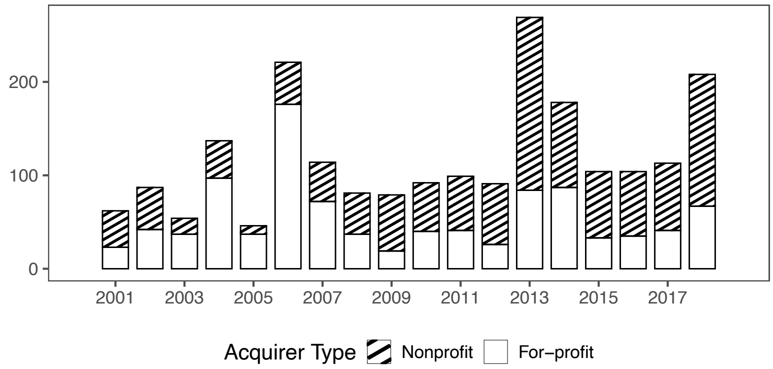
We obtain information on patient outcomes from Hospital Compare Outcome Measures, which is publicly disclosed by the CMS and the Hospital Quality Alliance (HQA). These databases provide rich information including details of medical treatment provided, patient recovery, complications during treatment, readmission rates, and mortality rates. We follow the prior literature and focus primarily on mortality and readmission rates as proxies for the quality of health care provision (e.g., Ho and Hamilton, 2000; Propper et al., 2004; Cooper et al., 2011; Gaynor and Town, 2011; Aghamolla et al., 2021). Mortality rate is the most commonly used indicator for the quality of care in hospitals. Readmission rate is also used as a measure of the effectiveness of treatment.

Our main measures of healthcare quality include 30-day mortality rates from heart attack (AMI), heart failure (HF), and pneumonia (PN), as well as 30-day readmission rates following treatment for the same conditions. Those measures have been adjusted for patient risk using statistical models. Patient risk includes clinical (e.g., types of treatments, severity of conditions), demographic (e.g., age and sex), and socioeconomic (e.g., race, income, ethnicity) factors.[6]

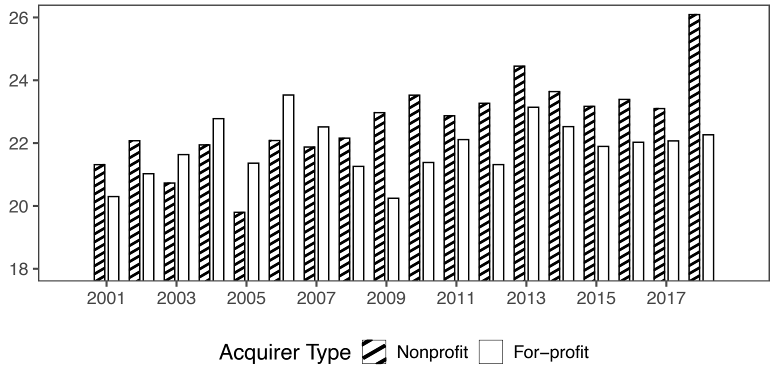
2.4 Initial Sample Construction

With data gathered from the above sources and procedures, we compile a hospital unit-year panel. Each standalone hospital and each hospital that belongs to a system has its own, separate observation. This allows us to follow and track an individual hospital after it is acquired. Following Cooper et al. (2019), we restrict our sample to general medical and surgical hospitals. Military and Veteran Health (VA) hospitals are excluded from the sample. For hospitals acquired more than once, we keep the first acquisition if those deals are over five years apart. We remove the hospitals that experience more than one acquisition within a five-year period. Target hospitals are required to have at least two years of observations before and after the acquisition year, so we can track the same hospital around the event.

[6]See more detailed explanation regarding risk adjustment in CMS MMS Blueprint.



(A) Number of Target Hospitals

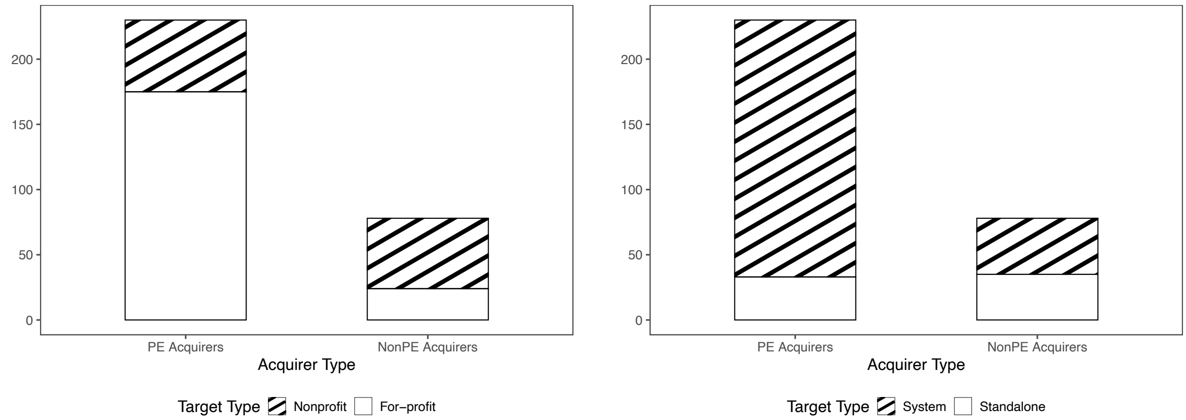


(B) Total Asset Value of Target Hospitals (log)

Figure 1. Hospital Mergers and Acquisitions Activity By Acquirer Type. This figure shows the time series patterns of hospital M&A activity in our sample. We classify acquired hospitals into two groups based on whether the acquirer is a for-profit or a nonprofit institution. Panel A reports the number of hospitals acquired by each acquirer type in a given year. Panel B reports the log of total asset values (in $) of target hospitals associated with each acquirer type.

2.5 Univariate Analysis

The hospital industry has experienced growing M&A activity over the past two decades. Figure 1 illustrates this time trend. Panel A reports the total number of U.S. hospitals acquired each year and Panel B reports the natural logarithm of total asset values of hospitals acquired each year. In both panels, white (patterned) columns represent deals conducted by for-profit (nonprofit) acquirers. Over our sample period, 46.5% of the target hospitals were acquired by for-profit organizations. Deal volume peaked in 2013, when nearly 240 hospitals were acquired, and again in 2018, when the total asset value of acquired hospitals reached $175 billion. Overall, hospitals acquired by for-profit organizations have a combined asset value of $79 billion, a substantial fraction of the total value across all acquisitions. These statistics suggest that for-profit acquirers play



(A) Nonprofit and For-profit Targets (B) System and Standalone Targets

Figure 2. Composition of Target Hospitals. This figure reports the breakdown of our sample of target hospitals by different types of for-profit acquirers. We first separate target hospitals based on whether the acquirer is PE or non-PE. In Panel A, we classify targets into two groups based on whether they operated as for-profit or nonprofit hospitals prior to being acquired. In Panel B, we group targets based on whether they belonged to a system of hospitals or were stand-alone prior to being acquired. The height of the each column represents the number of target hospitals within each classification.

an economically important role in the M&A landscape in the hospital industry.

Figure 2 reports the composition of deals involving different types of target hospitals acquired by for-profit organizations. In Panel A, we separate deals based on the for-profit status of targets prior to the acquisitions, while in Panel B, we classify target hospitals based on whether they belonged to a system of hospitals or not before being acquired. In each panel, we separately count the number of targets by PE and non-PE acquirers. We first note that PE acquirers account for the majority of the deals made by for-profit entities (74%). Across all acquirer categories, around 70% of target hospitals had for-profit status and around 80% of target hospitals belong to a system. The latter proportion is particularly high for hospitals acquired by PE firms.

In Table 1, we report and compare the characteristics of all target hospitals during the four years prior to their acquisition and hospitals that are never acquired during our sample period. Target hospitals have similar employment size, more core workers and fewer administrative workers compared to never-acquired hospitals. Once we scale these worker categories by the total number of patients, target hospitals have a smaller core worker-to-patient ratio as well as smaller administrative worker-to-patient ratio. In terms of real patient outcomes, target hospitals have lower mortality rates related to heart failure and pneumonia, but higher mortality related to heart attack. Finally, in terms of operating characteristics, target hospitals have more beds, higher case mix index, and a lower outpatient ratio (the ratio of outpatient charges over total charges). While target hospitals treat a greater proportion of Medicaid patients (those with limited financial resources to pay for health care), they have a smaller proportion of Medicare patients (65 years or older) than non-target hospitals.

TABLE 1 ABOUT HERE

3 Empirical Methodology

Given that target and non-target hospitals differ significantly in many important dimensions, we follow the existing work on hospital mergers such as Schmitt (2017) and Prager and Schmitt (2021) and conduct a matched sample analysis. In this analysis, we track each target hospital to a matched control hospital over a $[-4, +8]$ year event window around the year of the acquisition.

The matched control group is constructed as follows. We start with an initial pool of hospitals that includes all hospitals that have not been acquired in the corresponding event window. We also exclude from this pool of hospitals those that acquired other hospitals in our sample period. Hospitals also need to have at least two years of observations prior to the event year.

For each target hospital, we find one "nearest neighbor" hospital in the control pool based on a Mahalanobis matching method with replacement. The matched control hospital needs to locate in the same Census Region and have the same Metropolitan area status as the target hospital. More importantly, the matched unit needs to have the closest Mahalanobis distance to the target hospitals based on their average hospital characteristics during the four-year period prior to the acquisition, as well as the log number of core workers and administrative workers during the year prior to the deal. The hospital characteristics that we use in the matching process include *Log(Beds)*, *CMI*, *%Medicare*, *%Medicaid*, and outpatient ratio. Matching based on core and administrative workers at

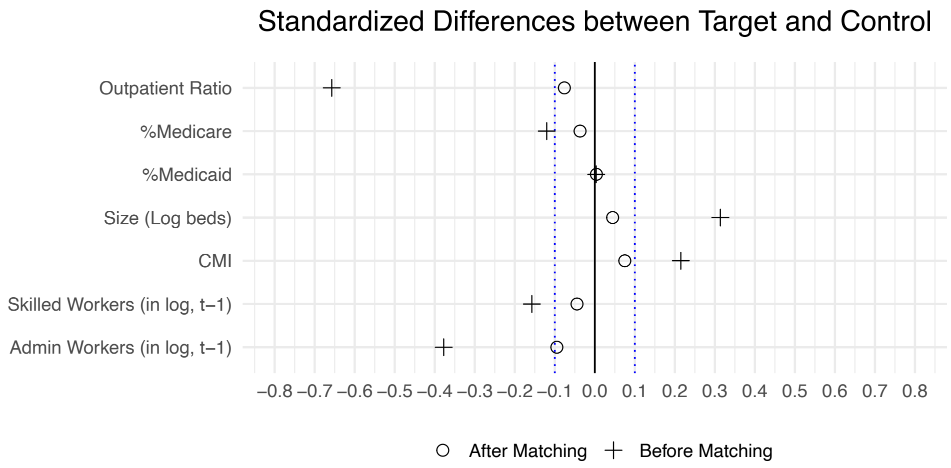


Figure 3. Covariate Balance. This figure shows the values of standardized differences between target and matched control hospitals. Standardized difference is computed as the average difference between the matched pairs (target − control) divided by the standard deviation computed over all observations. Detailed variable definitions are provided by Appendix A.

$t-1$ helps us control for pre-existing trends in the hospitals' labor force conditions prior to the acquisition.[7]

Figure 3 summarizes the covariate balance before and after matching. Similarity between target and control hospitals is measured by standardized difference, given by the average difference between the matched pairs (target − control) divided by the standard deviation computed over all observations. The literature indicates a threshold of 0.1 for the absolute standardized differences, under which the treated and control groups can be considered comparable (Austin 2009, 2011). After matching, we observe that the standardized differences between target and control hospitals fall below 0.1 across all dimensions.

Once each acquired hospital is matched with a control hospital, we track the pair over two event horizons. First, we examine the short-run effects of PE acquisitions, comparing the changes in target hospital characteristics from four years prior to the acquisition ($[-4,-1]$) to four years after ($[0,+4]$). This horizon is consistent with the literature examining short to medium term changes brought by PE firms (Kaplan 1989; Lerner et al. 2011). In addition, based on the evidence in Kaplan and Stromberg (2009) that holding

[7]The idea of matching on an outcome variable is also found in other matching methodologies such as entropy balancing or synthetic control methods, whereby the researcher identifies the control group by minimizing the difference in the sample moments of the outcome variable between the treatment and control groups (Abadie et al., 2010; Hainmueller, 2012). In Appendix C, we verify the robustness of our results when we match on the total number of workers before the acquisition. In Appendix D, we show that our results remain unchanged if we remove matched pairs where the control hospitals may be indirectly affected by the acquisitions of other local hospitals.

periods of PE firms have increased since the 1990s with only half of deals being exited within 72 months of the acquisition date, we also investigate the long term effects by comparing target hospital conditions during the pre-acquisition period ($[-4, -1]$) to the following four-year period after the acquisition (i.e., $[+5, +8]$).[8] Observations from $[0, +4]$ are excluded from this sample. This comparison reveals whether changes we observe in the short-run persist, disappear, or revert back in the longer horizon. Finally, we stack these observations associated with all matched pairs together. Our testing sample is thus an event-hospital unit-year panel, whereby an event refers to the acquisition of a hospital.

Table 2 reports the summary statistics related to key variables in our matched sample over the $[-4, +8]$ event window. The average hospital in this sample employs 883 people. Our sample hospitals have 184 beds and an outpatient discharge ratio of 0.42 on average. Among all job categories of which working hours are tractable in the HCRIS, 16% of aggregated working hours correspond to core workers and 24% to administrative workers in an average hospital. We note that these fractions rank among the highest across the 53 occupations provided in the HCRIS data.

TABLE 2 ABOUT HERE

We examine post-acquisition outcomes at target hospitals relative to their matched control hospitals in a difference-in-difference framework. Specifically, we estimate the following regression, both for the short-run and the long-run windows:

$$Y_{e,i,t} = \beta_1 PE\ Target_{e,i,t} + \beta_2 NonPE\ Target_{e,i,t} + \gamma \cdot X_{i,t} + \alpha_i + \mu_e + \tau_t + \epsilon_{e,i,t}, \quad (1)$$

where e indicates an acquisition event, i indicates a hospital, and t indicates a year around the event. $Y_{e,i,t}$ represents a variety of hospital outcomes that we examine, including operating performance, the log of employment, core and administrative workers, and the log of wage rates. *PE Target* indicates whether hospital i has been acquired by a PE acquirer in event e as of year t, and zero otherwise. *NonPE Target* is an indicator for

[8]In Appendix B, we verify that our results are not influenced by the attrition of hospital observations over the long-run horizon.

whether hospital i has been acquired by a non-PE acquirer in event e as of year t. Both indicators equal zero for years $[-4, -1]$ prior to the event.

We control for hospital fixed effects (α_i), event fixed effects (μ_e), and event-time fixed effects (τ_t). Hospital fixed effects allow us to trace the same hospital over the event horizon. Event fixed effects are separate indicators for each pair of matched target and control hospitals. Including these fixed effects help us compare within a pair of treated and control hospitals. Event-time fixed effects are a set of 9 indicators for each year in the event window. They absorb the common time-series changes across the matched pair around the event. We also include a multitude of hospital and county controls (X_{it}). Hospital controls include all variables in the matching process. County controls include population size, one-bedroom rent, and population demographics (e.g., the percentage of residents that are Asian and African American) in the county that the target hospital is located. Similar to existing studies (e.g., Schmitt, 2017; Gupta et al., 2021; Liu, 2021), we cluster standard errors by hospital.[9]

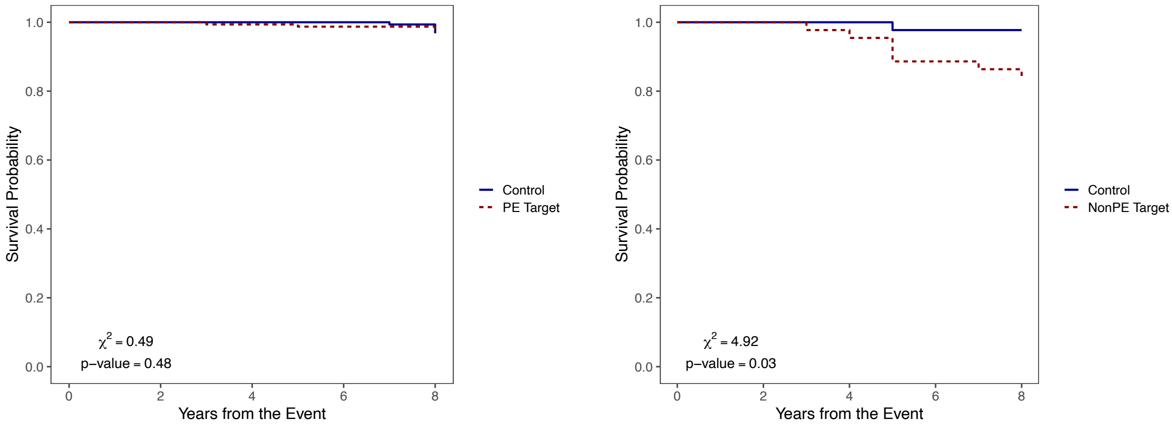
The coefficients of interest are β_1 and β_2, which measure how a target hospital changes subsequent to being acquired, compared to the concurrent changes in the conditions of its matched control hospital. We also report p-values from the Wald Chi-square test for $\beta_1 = \beta_2$, i.e., assessing whether the effects of PE and non-PE acquirers are statistically significantly different from each other.

4 Main Results

4.1 Hospital Survival and Profitability

There are concerns in the popular press that PE firms acquire hospitals, close them, and subsequently sell assets owned by those hospitals. To investigate the validity of such concerns using large scale data, we trace the survival likelihood of target and control hospitals in Figure 4. In Panel A (B), we compare the survival rates of PE (non-PE)-acquired hospitals and their matched control group. The lines indicate the survival rate of a hos-

[9]Our results are robust to several alternative clustering methods, including clustering by hospital-system, double clustering by hospital and system, and double clustering by hospital and acquirer.



(A) *Survival Analysis*, PE Targets (B) *Survival Analysis*, Non-PE Targets

Figure 4. Survival Analysis of PE and Non-PE Targets. This figure shows the survival rates of hospitals in each year after the acquisition. We compare survival likelihoods of hospitals acquired by PEs, hospitals acquired by non-PEs, and their respective matched control hospitals. The left-side panel represents the difference between PE targets (red dashed line) and their matched control hospitals (dark blue line), while the right panel reports the difference between non-PE targets (red dashed line) and their matched control hospitals (dark blue line).

pital from the acquisition event to the eighth year after the event. Higher values indicate that the hospital is more likely to remain open. The patterns suggest that PE-acquired hospitals are equally, if not more, likely to survive than their matched control group. In comparison, non-PE acquired hospitals are less likely to survive compared to their control group. This observation is at odds with the anecdotal claim that PE firms acquire hospitals with the purpose of closing them and profiting from the sale of their assets.

Next, we examine the profitability of acquired hospitals in Table 3. Profitability is measured by gross margins, operating income over total assets, and return on assets (ROA). We find that PE-acquired hospitals become significantly more profitable than their matched control hospitals shortly after the acquisition. Our estimates suggest that PE-acquired hospitals increase their gross margin by 2.5 percentage points, operating income by 5.4 percentage points and return on assets by 3.9 percentage points in the first four years after the acquisition. This profitability boost persists and further improves in the long run. Over the [5, 8]-year post event window, PE-acquired hospitals increase operating income by 7.4 percentage points, and ROA by 6.1 percentage points more than their control group. For context, we note that these magnitudes reflect the cumulative difference in hospital profitability between the pre-event years to the 8^{th} year after the event. The year-to-year average change in profitability is thus around one percent. In contrast, hospitals taken over by non-PE acquirers do not exhibit any improvement in profitability over either horizon.

TABLE 3 ABOUT HERE

Overall, our results from the survival and profitability analyses are inconsistent with the narrative that PE firms acquire hospitals simply to shut them down and sell the assets in possession. Instead, they are consistent with the argument that PE firms provide management expertise to the acquired hospitals, allowing them to survive and improve, in line with the recent findings in Cohn et al. (2021) outside the hospital industry. These results are also informative regarding whether PE firms are short-term and myopic investors. Our observation that profitability improvement persists in the long run helps alleviate such concerns.

4.2 Employment Outcomes

We next examine changes in the number and composition of employees at acquired hospitals, relative to those at matched control hospitals. To the extent that PE firms are efficiency-driven acquirers with expertise in shaving off excess costs, we expect employment and total wage costs to decline at hospitals after PE acquisitions. Yet, the effects of PE firms on employment may not be uniform across worker types. On the one hand, PE firms may reduce core medical workers more than other workers, as core workers require higher wages. On the other hand, PE firms could retain core medical workers to sustain the quality of health care delivery, but cut administrative workers given the documented evidence that hospitals suffer from administrative inefficiency.

4.2.1 Total Employees and Wage Expenditures

We examine the changes in the total number of employees as well as total wage expenditures at acquired hospitals following the specification in Equation 1. Table 4 reports the results. In columns (1) and (2), we examine the changes in the total number of employees, in columns (3) and (4), we examine the changes in the employee-to-patient ratio, and in columns (5) and (6), we look into total wage expenditures. Both total employment and total wages are in log terms, so the coefficients inform us of the percentage changes in these outcomes after acquisitions. For each outcome variable, we first present results over the short-term window ($[-4, -1]$ to $[0, +4]$-year windows), and then present effects from a longer horizon ($[-4, -1]$ to $[+5, +8]$-year windows).

Table 4 About Here

We find a large and significant decline in employment at PE-acquired hospitals. After being acquired, the average PE target hospital reduces its employment by over 7% over the next four years, and the decline becomes even larger over the next four year period following the first event window. Consistently, we also find a significant decline in the employment-patient ratio and the total wage bills of target hospitals. In the four years after acquisitions, hospitals' wage costs decline by 7%. Over the next four years, wage costs decline further, reaching 11% lower than their pre-acquisition level.

We also find a reduction in employment and wages at non-PE targets, but the effects are barely statistically significant and economically smaller compared to PE targets. Over the [5, 8]-year window after acquisitions, employment at hospitals acquired by non-PE buyers is not significantly lower than its pre-acquisition levels.

Overall, our results suggest that PE acquirers undertake substantial employment cuts and generate wage savings at target hospitals. These findings are consistent with the improved profitability and survival rates at target hospitals, as documented earlier.

An important question is whether by cutting employment, PE acquirers compromise the quality of healthcare and patient welfare at the hospitals they acquire. We attempt to answer this question in two ways. First, we look at changes in the composition of employees, including core and administrative employees. Later in our analysis, we examine whether changes in the employee composition at target hospitals are reflected in patient outcomes, including mortality and readmission rates.

4.2.2 Employee Composition

According to the HCRIS reporting convention, hospital employees are classified into 53 different occupations, reflecting the complexity and multidimensionality of the services hospitals provide. Among these occupations, we focus on two types of hospital employees: core medical workers that include physicians, nurses, and pharmacists, and administrative workers. Core medical workers are critical at providing quality health care. While administrative employees support key administrative functions of hospitals such as finance and accounting, U.S. hospitals are often criticized for having a bloated overhead structure, employing too many administrative workers and spending excessively on overhead costs (e.g., Shrank et al. 2019; Kocher 2013).

We track the changes in worker composition at acquired and control hospitals after the year of an acquisition. Table 5 reports the results. Similar to total employment measures, we first look at the log number of core and administrative employees (Panel A), and then scale the number of core and administrative workers by the number of patients to gauge the extent to which a hospital has core medical workers and overhead staff to service patient needs (Panel B). Within each panel, columns (1) and (2) contain results regarding core workers, and columns (3) and (4) provide results for administrative workers.

<ins>Table 5 About Here</ins>

We find that PE-acquired hospitals experience a temporary drop in the number of core workers by around 14% over the first event window. Notably, the effect dissipates over the second event window spanning from year 5 to year 8 following the acquisition. Comparing the core workers during this period to the pre-acquisition window, the difference is only 2% and is statistically insignificant from zero. The core worker-patient ratio at hospitals acquired by PE firms drops by about 6 basis points over the short term. This decline becomes statistically insignificant in the longer term as well.

In contrast, hospitals acquired by non-PE acquirers exhibit a stronger and more persistent decline in core workers across both measures we use. In the long-term, the number of core workers stays at a level that is around 25% lower than the pre-acquisition count.

The ratio of core worker over patients also continues to be 14 basis points below its pre-acquisition levels.

Different from what we observe with core workers, we find a significant and persistent decline in administrative workers at PE-acquired hospitals. Within the first four years after PE acquisitions, the number of administrative workers drops by around 18% at acquired hospitals. The reduction aggravates in the next four years, staying at around 22%. In contrast, we do not observe a decline in administrative workers at non-PE targets across either measure or horizon we use.

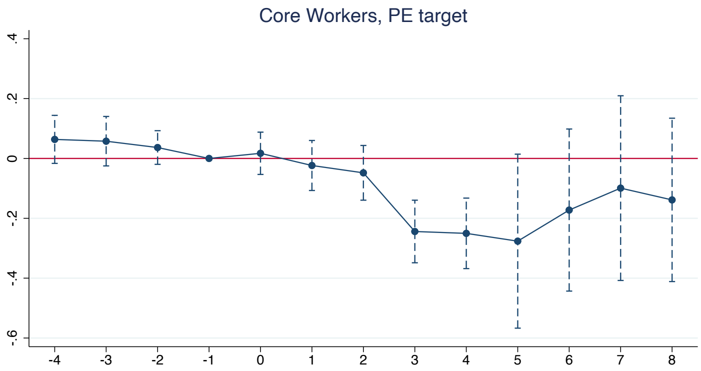
We next perform two refinements to our empirical design. First, we evaluate the concern that our results may be driven by changes in local conditions concurrent with PE acquisitions, such as the changes in local resident demographics, health conditions, income, or other preferences. These changes may drive hospital performance, employment, and even survival. We address this type of concerns by imposing state-year fixed effects in our baseline analysis. In Table 6, we find our results to be robust to including this stringent fixed effect structure.

TABLE 6 ABOUT HERE

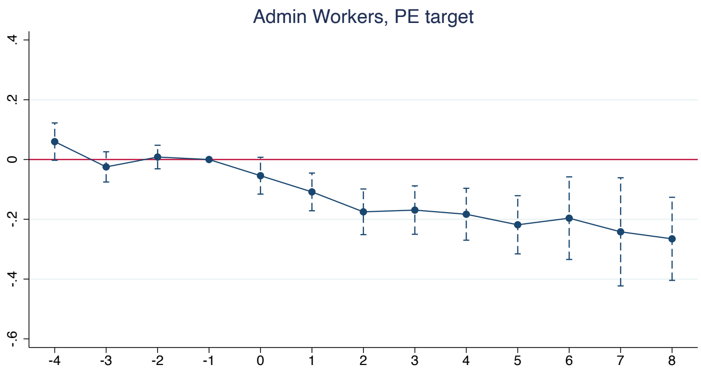
Second, we consider the possibility that our results may be biased by changes in sample composition over the event horizon. Note that this issue is alleviated by the inclusion of hospital fixed effects, which allow us to compare changes within the same hospital over time. We further address this concern by imposing more stringent sampling criteria, requiring each hospital to be in the sample for a minimum number of pre-event and post-event years. Appendix B shows that our results remain similar in these refined samples, despite the reduction in sample size.

4.3 Dynamic Effects of PE Acquisitions

We track how the number of core and administrative workers at PEs' target hospitals evolves over each year during our event horizon, compared to their matched control group. This examination allows us to infer when changes occur around the involvement of PE



(A) *Log(Core Workers)*



(B) *Log(Admin Workers)*

Figure 5. Dynamic Effect of PE Acquirers. This figure shows the changes in core workers and administrative workers for PE-acquired hospitals relative to their matched control hospitals over the [-4, +8]-year event window. The left panel represents outcomes for core workers, while the right panel reports outcomes for administrative workers. In each panel, the dots and intervals represent the coefficients and the associated 95-percentile confidence intervals, respectively. Year -1 is absorbed as the base year.

acquirers, and more importantly, evaluate whether PE buyers select hospitals based on their observable employment profiles. If they do, the changes in worker composition we document should start prior to the acquisition.

We track PE targets and their matched control hospitals over the [-4, +8] event horizon and require hospitals to have observations over the four years before and at least four years after the acquisition. With this sample, we extend the baseline regression model (Equation 1) by creating separate indicators of each year in the event horizon and interacting these indicators with *PE Target*. The estimation includes the same set of fixed effects and controls as in the baseline analysis, and further imposes state-by-year interactive fixed effects to help remove potential confounding effects from local conditions.

Figure 5 depicts the results. Panel A presents results for the log of total core workers, and Panel B presents results for the log of total administrative workers. In each panel, the dots represent point estimates, surrounded by 95% confidence intervals. The year prior to the event (i.e., year -1) is omitted as the benchmark year. We do not observe any significant pre-event changes in either outcome for PE targets. After PE acquisitions, target hospitals experience a temporary dip in core workers, but this effect dissipates in the longer term. Starting Year 5, PE target hospitals no longer have significantly fewer core workers compared to their own pre-event levels, or relative to the control hospitals. Importantly, we observe that PE-acquired hospitals experience a sizable decline in the number of administrative employees, and this effect persists in the long run.

These observations lend further support to our baseline findings. Importantly, the lack of pre-event trend suggests that our results are unlikely to be driven exclusively by PEs targeting hospitals that already exhibit signs of change along these dimensions. The significant post-event effects are consistent with PE acquirers increasing the operating efficiency of the hospitals they acquire without excessively reducing core workers.

4.4 Wage Rates

Finally, we examine whether core and administrative workers are paid lower wages at PE-acquired hospitals. This examination helps us address possibilities such as PE acquirers may lay off some administrative workers but offer higher wages to remaining ones, or that they suppress the wages of core medical workers.

We compute the hourly wage rates for core and administrative workers separately. Wage rates are transformed to log terms, i.e., *Log(Core Wage Rate)* and *Log(Admin Wage Rate)*. From Table 7, we find no change to core workers' wage rate at PE-acquired hospitals. In contrast, administrative workers' wage rate declines. In the long-run window, the wage rate of administrative workers declines significantly by around 7% compared to its pre-acquisition level. These results are consistent with PE acquirers reducing the costs associated with administrative functions. We do not find such an effect at target hospitals of non-PE buyers.

<u>Table 7 About Here</u>

5 Economic Mechanisms

In this section, we explore the heterogeneity of our results across deals to shed light on the economic mechanisms through which PE firms implement changes at target hospitals. We focus on three potential mechanisms. First, we examine the difference in post-acquisition outcomes based on whether a PE target hospital operated as a nonprofit or for-profit organization before being acquired. This investigation sheds light on whether PE firms create profit orientation and investor accountability in driving hospital performance. Second, we examine post-deal outcomes based on the extent to which a target hospital becomes a part of a larger system after being acquired. This helps us evaluate the role of scale economies in improving operating performance of a target hospital. Finally, we examine the role of PE firms' expertise in the healthcare industry based on their deal history in the healthcare industry.

5.1 For-Profit and Nonprofit Targets

Popular press has expressed concerns that PE firms acquire nonprofit hospitals and impose profit-orientation on their operations, and in some cases, they even downsize those hospitals significantly, compromising the provision of quality health care.[10] Meanwhile, others claim that PE firms improve the operating efficiency of nonprofit hospitals by creating accountability to investors. We formally evaluate these opposing views by comparing post-acquisition outcomes between targets that operated as for-profit and nonprofit organizations prior to being acquired. We focus on four outcomes, the log of total employment, total wage bill, core medical workers, and administrative workers. We regress each outcome on the interaction between the indicator for PE acquisition and targets' for-profit status prior to the deal. We also present statistics showing the difference between these interaction terms. We also include the indicator of non-PE acquisition, but suppress the coefficient for brevity.

Panel A of Table 8 reports results from this analysis. We note that both for-profit and nonprofit target hospitals experience significant declines in employment count and

[10] See, e.g., How private equity makes you sicker, *The American Prospect*, Oct. 2019.

total wage bills after being acquired, but the magnitudes of the declines are larger for nonprofit targets than for-profit ones over the first four-year window.

TABLE 8 ABOUT HERE

When we look into employee composition, we find that nonprofit targets exhibit no change in core worker counts, but a significant reduction in administrative workers both in the short-run and in the long-run. In contrast, the number of administrative workers only exhibits a short-term decline at for-profit targets, but recovers in the longer term. For example, estimates in column (8) suggest that administrative workers decline by 36% at nonprofit targets over the [5, 8]-year window after PE acquisitions. This sizable reduction likely reflects the substantial restructuring efforts at nonprofit hospitals which did not have investor accountability prior to being acquired. This magnitude stands in contrast to the 10%, statistically insignificant reduction at for-profit targets over the same horizon. This finding reveals a novel role for PE firms in transforming non-profit organizations into for-profit ones, and improving their efficiency.

5.2 Economies of Scale

We next investigate whether PE acquirers create value from economies of scale by acquiring a hospital and including it in a larger hospital system. Over our sample period, PE firms build up significantly larger systems than non-PE buyers. A potential benefit of operating large systems is that hospitals can share resources and reduce overhead costs, for example, by combining finance, accounting and marketing functions of individual hospitals. (Andreyeva et al., 2022). We thus expect that, if a PE acquisition transforms a target hospital from a standalone status or from belonging to a small system into a larger system, the target hospital may experience greater cost reduction, especially in its administrative departments.

To test this hypothesis, we first measure the increase in system size for target hospitals after PE acquisitions. We track down the parent systems to which the target belongs before and after the acquisition and count the number of hospitals belonging to each of those systems, and take the ratio between the two. Increases in system size is thus measured by:

$$\Delta System\ Size = \frac{\#Hospitals\ in\ System\ After\ Acquisition}{\#Hospitals\ in\ System\ Before\ Acquisition} - 1$$

We then define separate indicators *High* Δ*System Size* and *Low* Δ*System Size* for each deal, depending on whether Δ*System Size* exceeds the sample median. In Panel B of Table 8, we find that the effects of PE acquisitions are largely concentrated on targets that experience a large increase in system size. Those target hospitals exhibit significant reductions in employment, wage bills, and in particular, administrative workers. In the [5, 8]-year window after the events, these targets on average employ 30% fewer administrative workers than their pre-acquisition levels. While cost-cutting also occurs for deals where targets experience a smaller increase in system size, such changes are smaller in magnitude and often statistically insignificant.

5.3 Industry Expertise of PE Firms

In the last step of our cross-sectional analysis, we look into PE firms' expertise in the healthcare sector. Prior research documents that PE firms accumulate experience and develop expertise in an industry, which allows them to improve the operations of portfolio companies in that industry (Bernstein and Sheen, 2016). Hence, we expect PE buyers that are highly specialized in the healthcare industry to accumulate greater expertise and skills in restructuring hospitals. Such buyers should be associated with greater cost-cutting and larger reductions in administrative burden at target hospitals. We measure PE firms' industry expertise using the number of their past deals in healthcare over the total number of past deals they have completed.[11] This ratio is higher for PE firms that are highly focused on the healthcare industry.

Panel C of Table 8 provides evidence consistent with this conjecture. PE buyers with

[11] We utilize Capital IQ's MI Primary Industry classification and identify deals in the "Healthcare Facilities" industry. This industry includes hospitals, nursing homes, rehabilitation and retirement centers (https://www.spglobal.com/marketintelligence/en/documents/gics-mapbook-brochure.pdf).

greater specialization in the healthcare industry are associated with a greater reduction in total employment and wage bills as well as a substantial cut in administrative workers at target hospitals. In the long-run event horizon, administrative workers drop by 32% compared to the pre-event level at targets of specialized PE buyers, but only by around 18% for other PE buyers. Core workers at hospitals acquired by PE firms with greater healthcare specialization increase by the end of the longer event window, although not at a statistically significant level.Taken together, our findings highlight three mechanisms through which PE firms can improve the operations of portfolio hospitals: converting nonprofit hospitals into for-profit ones and hence, providing investor accountability, exploiting economies of scale, and leveraging on their industry expertise.

5.4 Additional Robustness Checks

In this section, we perform multiple additional analyses regarding the measures and sample we use to bolster our inferences.

First, we vary the definition of core medical workers. In our main analysis, core workers consist of physicians, nurses and pharmacists. As physicians may be hired through part-time contracts and be affiliated with multiple hospitals simultaneously, their hours are not tracked in the cost reports in the same way as other full-time employees. To mitigate potential errors in measurement, we restrict our definition of core workers to only nurses and pharmacists. Specifically, we examine changes in the log number of nurses and pharmacists (*Log(Nurses&Pharma)*) and the ratio of nurses and pharmacists to patients (*Nurses&Pharma/Patients*). Table 9 shows that our results are robust to this alternative definition of core workers. As before, we observe that the number of nurses and pharmacists at PE-acquired hospitals temporarily drops in the short run, but reverts back in the longer horizon. We document the same effect for the nurses and pharmacists-to-patient ratio. In contrast, at non-PE acquired hospitals, both the number and ratio of nurses and pharmacists experience a persistent and statistically significant decline subsequent to the acquisition. The initial decline does not reverse, but aggravates in the long run.

<u>TABLE 9 ABOUT HERE</u>

Our second test explores alternative matching approaches. Our main matching method uses the total core workers and administrative workers during the year prior to the acquisition as key matching variables. In Appendix C, we switch our employment matching variable to total worker counts. Our results remain unchanged.

Third, we address the concern that control hospitals may be influenced by the spillover effects from the acquisition of local, peer hospitals by PE firms. For example, as PE firms cut employment at target hospitals, those employees losing their jobs may switch to other hospitals in the same region, leading to an increase in employment in the control units. In Appendix D, we address this concern in several ways. First, we delete all matched target-control pairs where the control hospital is located in the same hospital referral regions (HRR) as the target (Panel A). Next, we remove from our sample matched pairs where the control hospital is located in an HRR where over 25 percent (Panel B) or 5 percent (Panel C) of the hospitals have experienced an acquisition by PE acquirers. Our inferences remain unchanged in these alternative samples.

6 Patient Outcomes

Does PE acquirers' cost-cutting motive affect patient interest and well-being? To explore this question, we track the changes in patient outcomes at acquired hospitals, including mortality rates and readmission rates of discharged patients. We also look into if patient composition and the number of treated patients change at target hospitals.

6.1 Patient Mortality and Readmission Rates

We consider two measures of patient outcomes, mortality and readmission rates. Mortality rate is an ultimate measure of patient welfare, and has been used frequently in prior studies as a metric of the effectiveness of healthcare quality (see Gaynor and Town (2011) for a review). The most widely used mortality metric is 30-day acute myocardial infarction (AMI) mortality rate, that is, the death rate of heart-attack patients during the 30-day period following hospitalization. We construct two supplementary mortality measures related to heart failure and pneumonia, defined analogously. Each aspect of mortality rate is based on the 30-day risk standardized rates, in percentage points.

Readmission rates after discharge are also an important indicator of the effectiveness of medical treatment (Ho and Hamilton, 2000). Similar to mortality rates, we also evaluate readmission rates using a 30-day window after discharge, and we focus on the same illnesses as before — heart attack, heart failure, and pneumonia.

In the CMS Hospital Compare database, mortality and readmission rates are reported with 3-year rolling windows. In other words, for year 2007, we observe the cumulative mortality/readmission rates calculated based on data from 2005–2007. We collect mortality rates reported over several time intervals, including a pre-event window $[t-3, t-1]$ and four post-event windows reported in year 3 through 6: $[t+1, t+3]$, $[t+2, t+4]$, $[t+3, t+5]$, and $[t+4, t+6]$. We exclude the windows that straddle the year of the acquisition because patient outcomes in those windows reflect partly pre-event conditions and partly treatment effects.

Because the pre-event window does not overlap with post-event ones, we adopt a first-difference approach to examine the effect of an acquisition on patient mortality and readmission rates. For each post-event window, we compute the change in mortality rate for a given hospital from the pre-event window to each of the four post-event windows. This gives us up to four observations for each hospital-acquisition event. The first-difference approach allows us to directly measure the changes in mortality/readmission rates following hospitalization from pre-acquisition years to post-acquisition years. We do not require observations for the [-4, +8] event windows as in our earlier analysis to avoid further sample attrition.

We regress the changes in mortality and readmission rates for PE and non-PE acquirers, with all control variables transformed in a first-difference approach. We also remove hospital fixed effects, which are absorbed by the first-difference approach. Our specification is as follows:

$$\Delta Y_{e,i,\tau} = \beta_1 PE\ Target_{e,i,\tau} + \beta_2 NonPE\ Target_{e,i,\tau} + \gamma \cdot \Delta X_{i,t} + \mu_e + \nu_{e,i,\tau}, \qquad (2)$$

where $\Delta Y_{e,i,\tau}$ represents the changes in mortality and readmission rates from the pre-event to a post-event window, indexed by τ. $\Delta X_{i,t}$ represents the first-difference in control variables, and μ_e stands for event fixed effects. In this specification, *PE Target* equals one for target hospitals of PE acquirers, and *NonPE Targets* indicates target hospitals of non-PE acquirers.

Table 10 reports the results from estimating Equation 2. We present coefficients from regressions with and without event fixed effects.

TABLE 10 ABOUT HERE

Panel A reports results regarding patient mortality. We do not find PE-acquired hospitals to exhibit significant increases in any of the three types of mortality rates we examine. There are mixed evidence regarding the effect of non-PE acquirers. While target hospitals of non-PE buyers have lower mortality rates regarding heart attacks compared to their control group, they also exhibit higher mortality rates related to heart failures. Panel B presents results regarding readmission rates. We again do not find PE acquirers to be associated with significant changes in any type of readmission rates. Non-PE acquirers are associated with a 0.9 percentage point decrease in readmission rates following pneumonia, but no changes in other readmission rates.

In untabulated analyses, we also look into other patient outcomes, including stroke, complications and infection during hospitalization. We do not find evidence that PE-acquired hospitals differ from the control group, or from non-PE acquired hospitals along these dimensions. Overall our evidence does not support the argument that PE acquirers reduce the quality of medical treatment at target hospitals compared to targets of non-PE acquirers as well as control hospitals. This finding complements the results from Liu (2021) that there is no significant change in the service quality of PE target hospitals.

6.2 Changes in Operational Characteristics

In the last step of our analysis, we discuss the possibility that changes in patient outcomes around PE acquisitions could be driven by acquired hospitals changing their patient composition or the type of medical procedures provided. Without data on individual patients and treatments, we follow Schmitt (2017) and directly examine the changes in observable hospital characteristics around acquisitions.

We first examine changes in the operating scale of hospitals along several dimensions. Since the number of beds represent a hospital's capacity to treat patients, the change in the number of beds in target hospitals is the first measure we analyze. We then turn to the log number of patients, inferred based on outpatient charges and outpatient ratio (Section 2.2). Next, we consider the amount of revenue generated by target hospitals, measured by the log of gross sales as well as net sales, which is gross sales after deducting rebate and discounts offered to patients. Panel A of Table 11 reports the results. We do not find target hospitals to exhibit meaningful changes in operating scale after PE acquisitions. Importantly, we note that PE target hospitals do not generate significantly higher revenue, which suggests that the increased profitability documented in Table 3 likely arises from reduced costs.

<u>Table 11 About Here</u>

We next investigate the changes in the type of hospital operations, by examining the changes in case mix index, outpatient ratio, and the percentage of patients enrolled in Medicare or Medicaid programs. The CMI represents the diversity, clinical complexity, and resource needs of all the patients treated in a hospital. Atr PE target hospitals, the CMI increases over the four years following the acquisitions, suggesting that PE-acquired hospitals treat a higher number of resource-intensive patients. Outpatient ratio, on the other hand, declines significantly. Because outpatient procedures are a more cost efficient source of revenue for hospitals, a declining ratio suggests that PE acquired hospitals do not shift their operations to outpatient services to generate revenue at lower costs compared to other hospitals.

Finally, we find that PE-acquired hospitals experience a small decline (by 1 percentage point) in the proportion of Medicare patients, but this effect dissipates over the longer horizon. We do not observe any changes in the percentage of Medicaid patients at either PE- or non-PE-acquired hospitals, alleviating the concern that target hospitals may cater to younger and wealthier patients after being acquired.

Overall, our investigation reveals little change in the operating scale or patient composition at PE targets. While we do not have information regarding individual patient characteristics, we provide several arguments alleviating the concern that our results on patient outcomes might be purely driven by changes in the patient composition at target hospitals. To start, we do not see PE acquirers decrease the percentage of Medicare and Medicaid patients, or to rely more on outpatient services. Second, our sample hospitals involve only acute-care hospitals providing a large array of basic services ranging from cardiology to neurology. This suggests limited scope for PE acquirers to shift their services to younger and wealthier patients and offer, for example, more profitable services such as cosmetic surgery.

7 Conclusion

Hospitals are an important sector of the economy. They not only provide essential healthcare, but are also key job providers in the U.S. As PE firms are increasingly involved in the hospital industry, in-depth research is needed to understand how such activity affects jobs, efficiency and patient outcomes at acquired hospitals.

We find that PE-acquired hospitals have better survival prospects and operating profitability compared to similar non-acquired hospitals as well as hospitals acquired by non-PE buyers. While we find that PE acquirers are associated with significant employment cuts, this cut largely involves administrative workers. In fact, there is no long term reduction in the number of core critical workers such as nurses and physicians as well as the number of core workers per patient once the initial high turnover period is over. On the other hand, there is a significant decline in administrative workers which persists over time. As a natural consequence, the wage bill paid to such employees goes down, providing the hospital an important source of savings.

Perhaps as a result of preserving core workers especially in the long run, we do not observe a deterioration in real patient outcomes such as mortality rates or readmission rates at PE-acquired hospitals. This result alleviates the concerns that PE firms improve efficiency and profitability at the expense of patients.

Overall, our evidence suggests that PE acquirers improve the operating efficiency of target hospitals without a compromise in healthcare quality. Targets of non-PE acquirers do not exhibit the same improvement in their operating efficiency. Thus, our analysis reveals a unique role of PE investors in shaping the hospital industry.

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Table 1
Summary Statistics For the Initial (Unmatched) Sample
This table reports the summary statistics for the main variables used in our study. The sample includes target hospital observations during the four years prior to their acquisition and all observations from non-target hospitals. Target − Non Target represents the difference between the two groups. Detailed variable definitions are provided by Appendix A. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Non Target	Target	Target−Non Target
Log(Employment)	6.37	6.38	0.02
Log(Core Workers)	3.72	3.84	0.13***
Core Workers/Patients (×100)	0.67	0.52	−0.16***
Log(Admin Workers)	4.28	4.11	−0.16***
Admin Workers/Patients (×100)	0.99	0.64	−0.34***
Log(Total Wages)	17.07	17.10	0.03
Core Wage Rate ($/hr)	48.00	40.70	−7.30***
Admin Wage Rate ($/hr)	25.62	25.52	−0.10
Mortality for Heart Attack (AMI)	15.00	15.54	0.54***
Mortality for Heart Failure	11.70	11.01	−0.69***
Mortality for Pneumonia	13.25	12.07	−1.18***
Readmission for Heart Attack (AMI)	17.98	19.47	1.48***
Readmission for Heart Failure	23.07	24.29	1.22***
Readmission for Pneumonia	17.39	18.36	0.97***
Beds	106.50	168.18	61.68***
CMI	1.31	1.36	0.05***
%Medicare	0.47	0.41	−0.06***
%Medicaid	0.13	0.14	0.01***
%Outpatient	0.58	0.41	−0.17***

Table 2
Summary Statistics for the Matched Sample
This table reports the summary statistics for the matched sample of targets and controls. Both target and control hospitals remain in the sample during the [-4, +8] event period. Detailed variable definitions are provided by Appendix A.

	Obs	Mean	Std	Median	P25	P75
Employment	4,332	882.86	659.89	706.23	445.30	1113.63
Log(Employment)	4,332	6.55	0.70	6.56	6.10	7.02
Core Workers	4,332	73.39	91.91	48.53	26.91	86.27
Log(Core Workers)	4,332	3.89	0.90	3.90	3.33	4.47
%Core Workers	4,332	0.16	0.08	0.15	0.11	0.20
Core Workers/Patients ($\times$100)	4,330	0.49	0.40	0.38	0.28	0.57
Admin Workers	4,332	90.96	69.69	72.20	45.42	113.49
Log(Admin Workers)	4,332	4.29	0.68	4.29	3.84	4.74
%Admin Workers	4,332	0.24	0.09	0.23	0.16	0.30
Admin Workers/Patients ($\times$100)	4,330	0.68	0.36	0.63	0.41	0.86
Total Wages (mil.$)	4,340	49.18	40.49	38.31	21.73	61.53
Log(Total Wages)	4,340	17.41	0.81	17.46	16.89	17.94
Core Wage Rate ($/hr)	4,251	43.83	11.69	41.86	35.94	48.87
Admin Wage Rate ($/hr)	4,203	26.97	7.27	26.11	21.54	31.75
Mortality for Heart Attack (AMI)	1,667	15.32	1.71	15.20	14.00	16.30
Mortality for Heart Failure	1,848	11.23	1.54	11.00	10.00	12.10
Mortality for Pneumonia	1,853	12.18	2.55	11.70	10.20	13.60
Readmission for Heart Attack (AMI)	1,275	18.78	1.73	19.00	17.60	20.00
Readmission for Heart Failure	1,577	23.91	2.09	24.00	22.40	25.40
Readmission for Pneumonia	1,580	17.98	1.58	18.00	16.90	19.00
Beds	4,358	184.22	120.50	160.00	101.00	235.00
CMI	4,323	1.38	0.21	1.37	1.25	1.52
%Medicare	4,358	0.38	0.13	0.38	0.28	0.46
%Medicaid	4,358	0.15	0.11	0.12	0.06	0.20
%Outpatient	4,357	0.42	0.14	0.40	0.31	0.53

Table 3
Profitability at Target Hospitals
This table examines changes in profitability at target hospitals around acquisitions. The dependent variable for columns (1) and (2) is *Gross Margin*, which is net income from service to patients (as given in HCRIS) over net patient revenues (as given in HCRIS). The dependent variable for columns (3) and (4) is *OI/TA*, which is net income from service to patients (as given in HCRIS) over total assets. The dependent variable for Column (5) and (6) is *ROA*, which is net income (total income−total other expenses, as given in HCRIS) over total assets. *PE Target* turns to one after a hospital is acquired by a PE acquirer. *Non-PE Target* turns to one after a hospital is acquired by a non-PE acquirer. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Hospital Controls include the log of total beds (*Log(Beds)*), case-mix index (*CMI*), percentage of patients covered by Medicare (*%Medicare*), percentage of patients with Medicaid (*%Medicaid*), and the percentage of patients that are outpatients (*%Outpatient*). County Controls include the percentage of Black residents (*%Black*), the percentage of Asian residents (*%Asian*), log of population (*Log(Pop)*), and the log of one bedroom rent in a county (*Log(FMR)*). See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*Gross Margin*		*OI/TA*		*ROA*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	0.0247**	0.0388	0.0542***	0.0739**	0.0386***	0.0605**
	(2.32)	(1.60)	(3.43)	(1.98)	(2.80)	(2.08)
NonPE Target	0.0365	0.0489	0.0238	0.0050	−0.0183	−0.0086
	(1.61)	(1.29)	(0.66)	(0.10)	(−0.75)	(−0.25)
Hospital Controls	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.61	0.81	0.42	0.23	0.03	0.10
Obs	4,296	2,575	4,288	2,569	4,288	2,569
Adj. R^2	0.62	0.62	0.61	0.57	0.58	0.50

Table 4
Employment and Wage Expenditures at Target Hospitals
This table examines changes in the employment and wages at target hospitals around acquisitions. The dependent variable in column (1) and (2) is the log of total employees (measured in full-time equivalent employees based on employed hours). The dependent variable in column (3) and (4) is the total employees per patient. The number of patients is estimated by adjusted discharges, defined as the number of discharged inpatients multiplied by (1+outpatient charges/inpatient charges). The dependent variable in column (5) and (6) is the log of total wages. *PE Target* turns to one after a hospital is acquired by a PE acquirer. *Non-PE Target* turns to one after a hospital is acquired by a non-PE acquirer. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Hospital Controls and County Controls are defined in the same way as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*Log(Employment)*		*Employment/Patients*		*Log(Total Wages)*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	−0.0697***	−0.0974**	−0.0040***	−0.0052	−0.0708***	−0.1073**
	(−4.68)	(−2.37)	(−3.56)	(−1.54)	(−3.85)	(−2.32)
NonPE Target	−0.0296	0.0085	−0.0006	−0.0030	−0.0639*	−0.0109
	(−0.92)	(0.13)	(−0.20)	(−0.70)	(−1.91)	(−0.16)
Hospital Controls	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.21	0.13	0.24	0.63	0.84	0.19
Obs	4,305	2,581	4,304	2,581	4,305	2,581
Adj. R^2	0.98	0.97	0.84	0.82	0.98	0.98

Table 5
Core and Administrative Workers at Target Hospitals

This table examines changes in core workers and administrative workers at target hospitals around acquisitions. Panel A reports the results for the number of core and administrative workers. The dependent variable for columns (1) and (2) is the log of total number of core workers, i.e., *Log(Core Workers)*. The dependent variable for columns (3) and (4) is the log of total number of administrative workers, i.e., *Log(Admin Workers)*. Panel B reports the results for the number of workers per patient. The number of patients is estimated by adjusted discharges, defined as the number of discharged inpatients multiplied by (1+outpatient charges/inpatient charges). *PE Target* turns to one after a hospital is acquired by a PE acquirer. *Non-PE Target* turns to one after a hospital is acquired by a non-PE acquirer. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Hospital Controls and County Controls are defined in the same way as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

(A) Log Number of Core and Administrative Workers

Dep. Var.:	*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	−0.1358***	−0.0200	−0.1792***	−0.2200***
	(−4.24)	(−0.24)	(−6.42)	(−3.68)
NonPE Target	−0.2041***	−0.2484	−0.0293	−0.0073
	(−2.74)	(−1.51)	(−0.51)	(−0.06)
Hospital Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.35	0.20	0.01	0.10
Obs	4,306	2,581	4,303	2,579
Adj. R^2	0.91	0.90	0.92	0.91

(B) Core and Administrative Workers per Patient

Dep. Var.:	*Core Workers/Patients*		*Admin Workers/Patients*	
	(1)	(2)	(3)	(4)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	−0.0006***	−0.0003	−0.0010***	−0.0015***
	(−3.63)	(−0.74)	(−4.89)	(−2.96)
NonPE Target	−0.0006*	−0.0014**	0.0000	−0.0005
	(−1.87)	(−2.29)	(0.12)	(−0.58)
Hospital Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.94	0.10	0.02	0.22
Obs	4,305	2,581	4,302	2,579
Adj. R^2	0.78	0.78	0.82	0.79

Table 6
Core and Administrative Workers, Controlling for Local Conditions
This table examines changes in core workers and administrative workers at target hospitals, while controlling for state-by-year interactive fixed effects. Panel A reports the results for the number of core and administrative workers. The dependent variable for columns (1) and (2) is the log of total number of core workers, i.e., *Log(Core Workers)*. The dependent variable for columns (3) and (4) is the log of total number of administrative workers, i.e., *Log(Admin Workers)*. Panel B reports the results for the number of workers per patient. The number of patients is estimated by adjusted discharges, defined as the number of discharged inpatients multiplied by (1+outpatient charges/inpatient charges). *PE Target* turns to one after a hospital is acquired by a PE acquirer. *Non-PE Target* turns to one after a hospital is acquired by a non-PE acquirer. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Hospital Controls and County Controls are defined in the same way as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

(A) Log Number of Core and Administrative Workers

Dep. Var.:	*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.1330^{***}	-0.0512	-0.1494^{***}	-0.2673^{***}
	(-3.85)	(-0.49)	(-4.54)	(-3.73)
NonPE Target	-0.1926^{***}	-0.1847	-0.0272	0.0698
	(-2.61)	(-1.14)	(-0.49)	(0.55)
Hospital Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes
State-by-Year FEs	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.44	0.44	0.03	0.01
Obs	4,263	2,530	4,260	2,528
Adj. R^2	0.91	0.90	0.93	0.92

(B) Core and Administrative Workers per Patient

Dep. Var.:	*Core Workers/Patients*		*Admin Workers/Patients*	
	(1)	(2)	(3)	(4)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.0005^{***}	0.0000	-0.0007^{***}	-0.0012^{**}
	(-2.72)	(0.02)	(-3.03)	(-2.10)
NonPE Target	-0.0006^{*}	-0.0016^{**}	-0.0001	-0.0004
	(-1.83)	(-2.34)	(-0.15)	(-0.50)
Hospital Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes
State-by-Year FEs	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.70	0.05	0.16	0.35
Obs	4,262	2,530	4,259	2,528
Adj. R^2	0.79	0.78	0.83	0.81

Table 7
Wage Rates for Core and Administrative Workers at Target Hospitals
This table examines changes in per hour salary paid to core workers and administrative workers at target hospitals around acquisitions. In columns (1) and (2), we present results related to *Log(Core Wage Rate)*, the log of hourly wage rate for core workers. In columns (3) and (4), we present results related to *Log(Admin Wage Rate)*, the log of hourly wage rate for administrative workers. *PE Target* turns to one after a hospital is acquired by a PE acquirer. *Non-PE Target* turns to one after a hospital is acquired by a non-PE acquirer. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Hospital Controls and County Controls are defined in the same way as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*Log(Core Wage Rate)*		*Log(Admin Wage Rate)*	
	(1)	(2)	(3)	(4)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	0.0097	−0.0358	−0.0000	−0.0731***
	(0.76)	(−1.08)	(−0.00)	(−2.60)
NonPE Target	−0.0376	0.0362	−0.0233	−0.0338
	(−1.47)	(0.72)	(−0.96)	(−0.75)
Hospital Controls	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.07	0.17	0.35	0.39
Obs	4,216	2,526	4,143	2,493
Adj. R^2	0.70	0.70	0.80	0.77

Table 8
Heterogeneity of Effects

This table presents heterogeneity of our results across target and acquirer characteristics. Panel A compares the results between for-profit and nonprofit targets, Panel B compares between PE acquirers with larger and smaller hospital systems, and Panel C examines the differential effects for PE acquires that are more or less specialized in the healthcare industry. In each panel, we report the results for the log of total employment (columns (1) and (2)), log of total wages (columns (3) and (4)), the log of core medical workers (columns (5) and (6)), and the log of administrative workers (columns (7) and (8)) at target hospitals around acquisitions. Target hospital for-profit status is characterized based on its status prior to the acquisition. Acquirer size is characterized based on the ratio of the number of hospitals in the system after the acquisition scaled by the number of hospitals in the target's previous system before the acquisition. *High (Low) Increase in System Size* equals one if after acquisition, this relative size ratio is higher (lower) than the sample median across all acquisitions. PE expertise is characterized based on the ratio of a PE firm's acquisitions in the healthcare facilities industry over total acquisitions up to the event year. *High (Low) Healthcare Expertise* equals one if a hospital is acquired by a private equity firm with above-median (below-median) expertise ratio in healthcare industry. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Hospital Controls and County Controls are defined in the same way as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

(A) For-profit vs. Nonprofit Targets

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	-0.1379^{***}	-0.1361^{**}	-0.1816^{***}	-0.1593^{**}	-0.0814	0.0444	-0.2070^{***}	-0.3627^{***}
× *Nonprofit Target*	(-5.06)	(-2.13)	(-5.81)	(-2.19)	(-1.25)	(0.35)	(-3.29)	(-5.32)
PE Target	-0.0504^{***}	-0.0651	-0.0393^{**}	-0.0639	-0.1513^{***}	-0.0739	-0.1714^{***}	-0.1037
× *For-profit Target*	(-3.38)	(-1.53)	(-2.12)	(-1.38)	(-4.67)	(-0.83)	(-6.29)	(-1.36)
Hospital Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: For-Profit=Nonprofit	0.00	0.30	0.00	0.21	0.28	0.40	0.57	0.00
Obs	4,305	2,581	4,305	2,581	4,306	2,581	4,303	2,579
Adj. R^2	0.98	0.97	0.98	0.98	0.91	0.90	0.92	0.91

(B) High vs. Low Increase in System Size

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	−0.0704***	−0.1573***	−0.0694***	−0.1758***	−0.1497***	−0.0769	−0.1889***	−0.3002***
× *High* Δ*System Size*	(−4.56)	(−2.89)	(−3.70)	(−2.88)	(−4.50)	(−0.65)	(−6.50)	(−4.32)
PE Target	−0.0436*	−0.0324	−0.0411	−0.0332	−0.0759	0.0411	−0.0873*	−0.1332
× *Low* Δ*System Size*	(−1.89)	(−0.65)	(−1.35)	(−0.58)	(−1.15)	(0.44)	(−1.88)	(−1.64)
Hospital Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: High=Low	0.22	0.06	0.32	0.05	0.27	0.38	0.03	0.08
Obs	4,281	2,557	4,281	2,557	4,282	2,557	4,279	2,555
Adj. R^2	0.98	0.97	0.98	0.98	0.91	0.90	0.92	0.91

(C) High vs. Low Healthcare Expertise

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	−0.0533***	−0.1949***	−0.0366*	−0.1925***	−0.1660***	0.3752	−0.2040***	−0.3181***
× *High Healthcare Expertise*	(−3.25)	(−3.92)	(−1.87)	(−4.04)	(−4.84)	(1.28)	(−7.39)	(−5.95)
PE Target	−0.0955***	−0.0558	−0.1245***	−0.0594	−0.0884*	−0.0221	−0.1399***	−0.1830***
× *Low Healthcare Expertise*	(−4.96)	(−1.35)	(−5.18)	(−1.27)	(−1.89)	(−0.28)	(−3.21)	(−2.89)
Hospital Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: High=Low	0.03	0.01	0.00	0.01	0.11	0.16	0.13	0.04
Obs	4,305	2,581	4,305	2,581	4,306	2,581	4,303	2,579
Adj. R^2	0.98	0.97	0.98	0.98	0.91	0.90	0.92	0.91

Table 9

Nurses and Pharmacists at Target Hospitals

This table examines changes in the proportion of nurses and pharmacists at target hospitals around acquisitions. In columns (1) and (2), we present results related to the log of total number of nurses and pharmacists, i.e., *Log(Nurses & Pharma)*. The dependent variable for columns (3) and (4) is *Nurses & Pharma/Patients*, the number of nurses and pharmacists scaled by the number of patients. The dependent variable for columns (5) and (6) is *Log(Nurses & Pharma Wage Rate)*, the log of hourly wage rate for nurses and pharmacists. *PE Target* turns to one after a hospital is acquired by a PE acquirer. *Non-PE Target* turns to one after a hospital is acquired by a non-PE acquirer. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Hospital Controls and County Controls are defined in the same way as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*Log(Nurses & Pharma)*		*Nurses & Pharma/Patients*		*Log(Nurse & Pharma Wage Rate)*	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	−0.1136***	0.0088	−0.0002***	−0.0000	−0.0005	0.0002
	(−3.62)	(0.12)	(−3.32)	(−0.10)	(−0.04)	(0.01)
NonPE Target	−0.1866***	−0.3955***	−0.0004***	−0.0009***	−0.0236	−0.0050
	(−3.30)	(−2.67)	(−2.72)	(−2.93)	(−1.40)	(−0.19)
Hospital Controls	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.22	0.01	0.22	0.01	0.17	0.86
Obs	4,292	2,571	4,291	2,571	4,153	2,495
Adj. R^2	0.91	0.93	0.78	0.78	0.76	0.79

Table 10
Mortality and Readmission Rates at Target Hospitals
This table examines the mortality and readmission rates at target hospitals around acquisitions. Panel A reports the results for mortality rates. The dependent variables are the 30-day risk-standardized mortality rate following heart attack hospitalization, heart failure hospitalization, and pneumonia hospitalization. Panel B reports the results for readmission rates. The dependent variables are the 30-day risk-standardized readmission rates for patients discharged from the hospital with a principal diagnosis of heart attack, heart failure, and pneumonia, respectively. Mortality rates and readmission rates are presented in percentage points. The regressions take a first-difference approach, with both the dependent variables and continuous control variables representing changes from the pre-acquisition window to a post-acquisition window. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Control variables are the same as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

(A) Changes in Mortality

Dep. Var.:	*Heart Attack (AMI)*		*Heart Failure*		*Pneumonia*	
	(1)	(2)	(3)	(4)	(5)	(6)
PE Target	−0.2759	0.1875	−0.0632	−0.1858	0.2189	0.3028
	(−0.73)	(0.52)	(−0.15)	(−0.52)	(0.39)	(0.65)
NonPE Target	−0.9067*	−1.7223***	0.6163	0.8328*	0.9204	0.6212
	(−1.70)	(−3.36)	(1.36)	(1.84)	(1.18)	(0.96)
Hospital Controls (differenced)	Yes	Yes	Yes	Yes	Yes	Yes
County Controls (differenced)	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs		Yes		Yes		Yes
H_0: PE=NonPE	0.32	0.01	0.23	0.08	0.39	0.68
Obs	202	201	252	251	253	253
Adj. R^2	0.14	0.50	0.06	0.36	0.09	0.46

(B) Changes in Readmission

Dep. Var.:	*Heart Attack (AMI)*		*Heart Failure*		*Pneumonia*	
	(1)	(2)	(3)	(4)	(5)	(6)
PE Target	0.3398	0.0409	0.2521	0.3993	0.0483	−0.0468
	(0.73)	(0.09)	(0.50)	(1.16)	(0.11)	(−0.11)
NonPE Target	0.3065	0.4724	0.0219	0.2061	−0.5590	−0.9205*
	(0.56)	(1.09)	(0.04)	(0.57)	(−1.11)	(−1.90)
Hospital Controls (differenced)	Yes	Yes	Yes	Yes	Yes	Yes
County Controls (differenced)	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs		Yes		Yes		Yes
H_0: PE=NonPE	0.95	0.47	0.68	0.67	0.21	0.16
Obs	144	142	199	198	200	200
Adj. R^2	0.13	0.61	0.18	0.67	0.08	0.38

Table 11

Size and Operating Characteristics at Target Hospitals

This table examines changes in scale and operating characteristics at target hospitals around acquisitions. Panel A examines hospitals operating scale, measured by the log number of beds, patients, and sales. Panel B examines the composition of hospitals patients or operations, measured by case mix index, outpatient ratio, and the percentage of Medicare and Medicaid patients. Rows with H_0's provide p-values from Wald Chi-square tests indicating whether two coefficients are statistically significantly different from each other. Control variables are the same as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

(A) Hospital Operating Scale

Dep. Var.:	*Log(Beds)*		*Log(Patients)*		*Log(Gross Patient Sales)*		*Log(Net Patient Sales)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	−0.0076	−0.0473	−0.0115	−0.0815	0.0173	−0.0245	−0.0287	−0.0167
	(−0.42)	(−0.88)	(−0.53)	(−1.02)	(0.74)	(−0.35)	(−1.14)	(−0.23)
NonPE Target	-0.0576^{**}	−0.0211	−0.0551	−0.0117	-0.0747^{*}	0.0078	-0.0841^{**}	0.0433
	(−2.40)	(−0.36)	(−1.24)	(−0.13)	(−1.96)	(0.08)	(−2.25)	(0.44)
Hospital Controls								
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.0316	0.6890	0.3087	0.4929	0.0165	0.7608	0.1287	0.5892
Obs	4,378	2,622	4,376	2,621	4,368	2,615	4,368	2,615
Adj. R^2	0.9657	0.9547	0.9528	0.9466	0.9792	0.9675	0.9668	0.9531

(B) Hospital Operation and Patient Composition

Dep. Var.:	*CMI*		*Outpatient Ratio*		*%Medicare*		*%Medicaid*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	0.0165^{**}	0.0301	-0.0198^{***}	-0.0251^{*}	-0.0120^{**}	0.0009	−0.0028	−0.0014
	(2.05)	(1.33)	(−3.63)	(−1.83)	(−1.97)	(0.06)	(−0.45)	(−0.08)
NonPE Target	−0.0176	−0.0056	−0.0047	−0.0114	0.0084	0.0171	−0.0062	−0.0287
	(−1.32)	(−0.21)	(−0.54)	(−0.83)	(1.03)	(0.98)	(−0.67)	(−1.59)
Hospital Controls								
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.0119	0.2379	0.0638	0.3173	0.0083	0.3751	0.7135	0.2063
Obs	4,330	2,594	4,377	2,621	4,378	2,622	4,378	2,622
Adj. R^2	0.9058	0.8896	0.9392	0.9299	0.8971	0.8772	0.7974	0.7515

Appendix A Variable Definitions

A Employment Variables

- *Log(Employment)*: The log of total employees (measured in full-time equivalent employees based on paid hours). The information is obtained from the HCRIS Worksheet S-3, Part II.
- *Log(Total Wages)*: The log of total wages. The information is obtained from the HCRIS Worksheet S-3, Part II.
- *%Core Workers*: The ratio of nurses, physicians (including contract physicians), and pharmacists relative to all employee trackable in HCRIS Worksheet S-3, Part II (measured in full-time equivalent employees based on paid hours). The information is obtained from the HCRIS Worksheet S-3, Part II. Core workers include Non-physician anesthethist Part A (Line Number 2), Non-physician anesthethist Part B (Line Number 3), Physician - Part A - Administrative (Line Number 4), Physician - Part A - Teaching (Line Number 4.01), Physician and Non Physician-Part B (Line Number 5), Interns & residents (in an approved program) (Line Number 7), Contracted interns & residents (in an approved program) (Line Number 7.01), Contract labor: Direct Patient Care (Line Number 11), Contract labor: Physician - Part A - Administrative (Line Number 13), Home office: Physician Part A - Administrative (Line Number 15), Home office & Contract Physician Part A - Teaching (Line Number 16), Nursing Administration (Line Number 38), and Pharmacy (Line Number 40).
- *Log(Core Workers)*: The log number of nurses, physicians, and pharmacists (measured in full-time equivalent employees based on paid hours). The information is obtained from the HCRIS Worksheet S-3, Part II. Core workers include Non-physician anesthethist Part A (Line Number 2), Non-physician anesthethist Part B (Line Number 3), Physician - Part A - Administrative (Line Number 4), Physician - Part A - Teaching (Line Number 4.01), Physician and Non Physician-Part B (Line Number 5), Interns & residents (in an approved program) (Line Number 7), Contracted interns & residents (in an approved program) (Line Number 7.01), Contract labor: Direct Patient Care (Line Number 11), Contract labor: Physician - Part A - Administrative (Line Number 13), Home office: Physician Part A - Administrative (Line Number 15), Home office & Contract Physician Part A - Teaching (Line Number 16), Nursing Administration (Line Number 38), and Pharmacy (Line Number 40).
- *Core Workers/Patients*: The ratio of nurses, physicians, and pharmacists, measured in full-time equivalent employees based on paid hours, relative to total discharges. The information is obtained from the HCRIS Worksheet S-3, Part II. Core workers include Non-physician anesthethist Part A (Line Number 2), Non-physician anesthethist Part B (Line Number 3), Physician - Part A - Administrative (Line Number 4), Physician - Part A - Teaching (Line Number 4.01), Physician and Non Physician-Part B (Line Number 5), Interns & residents (in an approved program) (Line Number 7), Contracted interns & residents (in an approved program) (Line Number 7.01), Contract labor: Direct Patient Care (Line Number 11), Contract labor: Physician - Part A - Administrative (Line Number 13), Home office: Physician Part A - Administrative (Line Number 15), Home office & Contract Physician Part A - Teaching (Line Number 16), Nursing Administration (Line Number 38), and Pharmacy (Line Number 40).
- *Log(Core Wage Rate)*: The log of hourly wages for nurses and physicians. The information is obtained from the HCRIS Worksheet S-3, Part II. Core workers include Non-physician anesthethist Part A (Line Number 2), Non-physician anesthethist Part B (Line Number 3), Physician - Part A - Administrative (Line Number 4), Physician - Part A - Teaching

(Line Number 4.01), Physician and Non Physician-Part B (Line Number 5), Interns & residents (in an approved program) (Line Number 7), Contracted interns & residents (in an approved program) (Line Number 7.01), Contract labor: Direct Patient Care (Line Number 11), Contract labor: Physician - Part A - Administrative (Line Number 13), Home office: Physician Part A - Administrative (Line Number 15), Home office & Contract Physician Part A - Teaching (Line Number 16), Nursing Administration (Line Number 38), and Pharmacy (Line Number 40).

- *%Admin Workers*: The ratio of administrative and general workers relative to all employee trackable in HCRIS Worksheet S-3, Part II (measured in full-time equivalent employees based on paid hours). The information is obtained from the HCRIS Worksheet S-3, Part II. Administrative and general workers include Administrative & General (Line Number 27) and Administrative & General under contract (Line Number 28).
- *Log(Admin Workers)*: The log number of administrative and general workers (measured in full-time equivalent employees based on paid hours). The information is obtained from the HCRIS Worksheet S-3, Part II. Administrative and general workers include Administrative & General (Line Number 27) and Administrative & General under contract (Line Number 28).
- *Admin Workers/Patients*: The ratio of administrative and general workers, measured in full-time equivalent employees based on paid hours, relative to total discharges. The information is obtained from the HCRIS Worksheet S-3, Part II. Administrative and general workers include Administrative & General (Line Number 27) and Administrative & General under contract (Line Number 28).Administrative and general workers include Administrative & General (Line Number 27) and Administrative & General under contract (Line Number 28).
- *Log(Admin Wage Rate)*: The log of hourly wages for administrative and general workers (including contract labor). The information is obtained from the HCRIS Worksheet S-3, Part II. Administrative and general workers include Administrative & General (Line Number 27) and Administrative & General under contract (Line Number 28).
- *Log(Nurses & Pharma)*: The log number of nurses and pharmacists (measured in full-time equivalent employees based on paid hours). The information is obtained from the HCRIS Worksheet S-3, Part II. Nurses and Pharmacists include Nursing Administration (Line Number 38), and Pharmacy (Line Number 40).
- *Nurses & Pharma/Patients*: The ratio of nurses and pharmacists, measured in full-time equivalent employees based on paid hours, relative to total discharges. The information is obtained from the HCRIS Worksheet S-3, Part II. Nurses and Pharmacists include Nursing Administration (Line Number 38), and Pharmacy (Line Number 40).
- *Log(Nurses & Pharma Wage Rate)*: The log of hourly wages for nurses and pharmacists. The information is obtained from the HCRIS Worksheet S-3, Part II. Nurses and Pharmacists include Nursing Administration (Line Number 38), and Pharmacy (Line Number 40).

B Patient Outcome Variables

- *Mortality for Heart Attack (AMI)*: 30-day risk-standardized mortality rate following heart attack hospitalization, in percentage points.
- *Mortality for Heart Failure*: 30-day risk-standardized mortality rate following heart failure hospitalization, in percentage points.
- *Mortality for Pneumonia*: 30-day risk-standardized mortality rate following pneumonia hospitalization, in percentage points.
- *Readmission for Heart Attack (AMI)*: 30-day risk-standardized readmission rates for pa-

tients discharged from the hospital with a principal diagnosis of heart attack, in percentage points.

- *Readmission for Heart Failure*: 30-day risk-standardized readmission rates for patients discharged from the hospital with a principal diagnosis of heart failure, in percentage points.
- *Readmission for Pneumonia*: 30-day risk-standardized readmission rates for patients discharged from the hospital with a principal diagnosis of pneumonia, in percentage points.

C Independent Variables

- *PE Target*: An indicator variable that turns to one for a target hospital after it is acquired by a PE firm or a PE-backed hospital.
- *NonPE Target*: An indicator variable that turns to one for a target hospital after it is acquired by a non-PE backed hospital.

D Control Variables

- *Log(Beds)*: The log of number of beds.
- *CMI*: The cost-mix index.
- *%Medicare*: The ratio of Medicare discharges relative to total discharges.
- *%Medicaid*: The ratio of Medicaid discharges relative to total discharges.
- *%Outpatient*: The ratio of outpatient charges relative to total charges.
- *%Black*: The fraction of Black in a given county at a given year.
- *%Asian*: The fraction of Asian in a given county at a given year.
- *Log(Pop)*: The log of population in a given county at a given year.
- *Log(FMR)*: The log of one bedroom rent price in a give county in a given year.

Appendix B Robustness Check: Balanced Panel Over Event Windows

Table B1
Requiring Hospital Observations Throughout Event Window.
This table reports results when we impose additional requirement for hospital observations during our event window. Panel A presents results when we require both target and control hospitals to have observations in each year of the $[-2, +2]$-year event window. In Panel B, we require both target and control hospitals to have observations in each year of the $[-2, +4]$-year event window. In Panel C, we require both target and control hospitals to have observations in each year of the $[-2, +6]$-year event window. In Panel D, we require both target and control hospitals to have observations in each year of the $[-2, +8]$-year event window. In Panel E, we require both target and control hospitals to have observations in each year of the $[-4, +6]$-year event window. In Panel F, we require both target and control hospitals to have observations in each year of the $[-4, +8]$-year event window. Controls are the same as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

(A) Requiring Observations Throughout $[-2, +2]$

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$
PE Target	−0.0711***	−0.1108***	−0.0733***	−0.1243***	−0.1426***	−0.0599	−0.1799***	−0.2193***
	(−4.73)	(−2.73)	(−3.95)	(−2.75)	(−4.46)	(−0.73)	(−6.40)	(−3.62)
NonPE Target	−0.0325	0.0049	−0.0663*	−0.0157	−0.1936***	−0.2528	−0.0317	−0.0103
	(−0.99)	(0.07)	(−1.95)	(−0.23)	(−2.60)	(−1.54)	(−0.54)	(−0.09)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.24	0.10	0.84	0.14	0.49	0.27	0.01	0.10
Obs	4,223	2,515	4,223	2,515	4,223	2,515	4,223	2,515
Adj. R^2	0.98	0.97	0.98	0.98	0.91	0.90	0.92	0.91

(B) Requiring Observations Throughout $[-2, +4]$

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.0684^{***}	-0.0984^{**}	-0.0705^{***}	-0.1144^{***}	-0.1471^{***}	-0.0738	-0.1804^{***}	-0.2246^{***}
	(-4.25)	(-2.58)	(-3.55)	(-2.74)	(-4.30)	(-0.96)	(-5.93)	(-3.68)
NonPE Target	-0.0496	0.0038	-0.0808^{**}	-0.0217	-0.2005^{**}	-0.2558	-0.0552	-0.0647
	(-1.54)	(0.06)	(-2.22)	(-0.31)	(-2.42)	(-1.50)	(-0.87)	(-0.54)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.56	0.14	0.78	0.20	0.51	0.31	0.05	0.21
Obs	3,748	2,213	3,748	2,213	3,748	2,213	3,748	2,213
Adj. R^2	0.98	0.98	0.98	0.98	0.91	0.90	0.92	0.91

(C) Requiring Observations Throughout $[-2, +6]$

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.0851^{***}	-0.0921^{**}	-0.0730^{**}	-0.1122^{**}	-0.1282^{**}	-0.0773	-0.1510^{***}	-0.2133^{***}
	(-2.97)	(-2.20)	(-2.34)	(-2.41)	(-2.26)	(-0.95)	(-3.40)	(-3.16)
NonPE Target	-0.0108	0.0290	-0.0272	-0.0013	-0.2021	-0.2715	-0.0553	-0.0397
	(-0.27)	(0.44)	(-0.64)	(-0.02)	(-1.60)	(-1.54)	(-0.77)	(-0.32)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.09	0.08	0.31	0.12	0.57	0.30	0.23	0.19
Obs	1,247	1,024	1,247	1,024	1,247	1,024	1,247	1,024
Adj. R^2	0.98	0.96	0.98	0.97	0.90	0.87	0.92	0.88

(D) Requiring Observations Throughout $[-2, +8]$

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$
PE Target	-0.0705^{**}	-0.1028^{**}	-0.0702^{*}	-0.1316^{**}	-0.0983	-0.0175	-0.1652^{***}	-0.2464^{***}
	(-2.19)	(-2.16)	(-1.93)	(-2.38)	(-1.49)	(-0.17)	(-2.83)	(-2.81)
NonPE Target	-0.0123	-0.0546	-0.0196	-0.0909	-0.1288	-0.3629	-0.1168	-0.2379
	(-0.21)	(-0.97)	(-0.36)	(-1.25)	(-0.59)	(-1.39)	(-1.08)	(-1.44)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.33	0.35	0.36	0.55	0.89	0.21	0.68	0.96
Obs	733	648	733	648	733	648	733	648
Adj. R^2	0.98	0.97	0.99	0.98	0.90	0.88	0.92	0.89

(E) Requiring Observations Throughout $[-4, +6]$

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$	$[0, 4]$	$[5, 8]$
PE Target	-0.1262^{***}	-0.1287^{**}	-0.1042^{**}	-0.1431^{**}	-0.1455^{**}	-0.1132	-0.1950^{***}	-0.2640^{***}
	(-3.19)	(-2.46)	(-2.52)	(-2.54)	(-1.99)	(-0.99)	(-3.58)	(-3.39)
NonPE Target	-0.0478	0.0159	-0.0812^{*}	-0.0561	-0.3355^{***}	-0.4207^{*}	-0.1001	-0.1223
	(-1.09)	(0.17)	(-1.75)	(-0.60)	(-2.66)	(-1.76)	(-1.12)	(-0.72)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.13	0.12	0.65	0.34	0.17	0.23	0.35	0.42
Obs	723	587	723	587	723	587	723	587
Adj. R^2	0.97	0.95	0.98	0.97	0.89	0.84	0.91	0.84

(F) Requiring Observations Throughout $[-4, +8]$

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.1118^{**}	-0.1418^{**}	-0.0915^{*}	-0.1594^{**}	-0.0824	-0.1338	-0.1930^{***}	-0.2326^{**}
	(-2.60)	(-2.36)	(-1.75)	(-2.24)	(-0.89)	(-1.00)	(-2.87)	(-2.16)
NonPE Target	-0.0771^{**}	-0.1425^{*}	-0.0859^{**}	-0.2493^{***}	-0.5012^{***}	-0.8851^{***}	-0.1608	-0.4318
	(-2.06)	(-1.89)	(-2.04)	(-3.57)	(-3.07)	(-2.83)	(-0.92)	(-1.46)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.42	0.99	0.88	0.09	0.03	0.03	0.87	0.52
Obs	371	326	371	326	371	326	371	326
Adj. R^2	0.98	0.97	0.99	0.98	0.91	0.89	0.90	0.83

Appendix C Alternative Matching

Table C1
Matching Based on Total Employment Count
This table shows the robustness of our main results when we match target hospitals to control hospitals based on pre-event *Log(Employment)*. Controls are the same as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]	[0, 4]	[5, 8]
PE Target	−0.0787***	−0.1209***	−0.0727***	−0.1266***	−0.1517***	−0.0666	−0.1401***	−0.1607**
	(−5.18)	(−3.15)	(−4.02)	(−3.06)	(−4.30)	(−0.75)	(−4.51)	(−2.52)
NonPE Target	−0.0521	−0.0485	−0.0795**	−0.0741	−0.2091***	−0.2423	−0.0088	0.0104
	(−1.62)	(−0.74)	(−2.41)	(−1.10)	(−2.78)	(−1.62)	(−0.15)	(0.09)
Hospital Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hospital FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Event Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.41	0.31	0.84	0.46	0.43	0.27	0.02	0.16
Obs	4,375	2,612	4,375	2,612	4,375	2,612	4,373	2,610
Adj. R^2	0.98	0.97	0.98	0.98	0.91	0.89	0.92	0.91

Appendix D Robustness Check: Spillover Effects of Local Mergers

Table D1
Alleviating Concerns Regarding the Spillover Effects of Local Mergers.
This table provides results from analysis to alleviate concerns regarding the spillover effects of local mergers. In Panel A, we drop matched pairs where the control hospital is located in the same HRR as the target hospital. In Panel B, we drop matched pairs where the control hospital is located in an HRR where over 25% of hospitals in that region are acquired by PEs over the $[-4,+8]$-year event period. In Panel C, we drop matched pairs where the control hospital is located in an HRR where 5% of hospitals are acquired by PEs over the $[-4,+8]$-year event period. Controls are the same as in Table 3. See Appendix A for variable definitions. t-statistics are reported in parentheses and standard errors are heteroskedasticity robust and clustered by hospital. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

(A) Dropping Pairs with Control Located in the Same HRR as Target

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.0662^{***}	-0.0810^{*}	-0.0677^{***}	-0.0961^{*}	-0.1378^{***}	-0.0493	-0.1860^{***}	-0.1783^{***}
	(-4.29)	(-1.77)	(-3.58)	(-1.88)	(-4.14)	(-0.54)	(-6.51)	(-2.84)
NonPE Target	-0.0525	-0.0729	-0.0878^{**}	-0.0935	-0.2380^{***}	-0.3698^{*}	-0.0467	-0.0953
	(-1.51)	(-1.47)	(-2.46)	(-1.55)	(-2.76)	(-1.86)	(-0.73)	(-0.75)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.69	0.89	0.57	0.97	0.24	0.13	0.03	0.54
Obs	3,832	2,263	3,832	2,263	3,833	2,263	3,830	2,261
Adj. R^2	0.98	0.98	0.98	0.98	0.91	0.91	0.93	0.92

(B) Dropping Pairs with Control Located in HRR over 25% of PE-Acquired Hospitals

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.0764^{***}	-0.0592	-0.0784^{***}	-0.0687	-0.1557^{***}	-0.0438	-0.1951^{***}	-0.2067^{***}
	(-4.75)	(-1.51)	(-3.94)	(-1.52)	(-4.55)	(-0.55)	(-6.86)	(-3.29)
NonPE Target	-0.0473	0.0127	-0.0761^{**}	-0.0085	-0.2338^{***}	-0.2687	-0.0444	-0.0124
	(-1.62)	(0.19)	(-2.33)	(-0.12)	(-3.13)	(-1.63)	(-0.77)	(-0.10)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.32	0.30	0.95	0.41	0.30	0.20	0.01	0.13
Obs	3,846	2,313	3,846	2,313	3,847	2,313	3,844	2,311
Adj. R^2	0.98	0.97	0.98	0.98	0.91	0.90	0.92	0.91

(C) Dropping Pairs with Control Located in HRR over 5% of PE-Acquired Hospitals

Dep. Var.:	*Log(Employment)*		*Log(Total Wages)*		*Log(Core Workers)*		*Log(Admin Workers)*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-Event Window	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$	$[0,4]$	$[5,8]$
PE Target	-0.0719^{***}	-0.0811	-0.0770^{***}	-0.1153^{**}	-0.1965^{***}	-0.0793	-0.2361^{***}	-0.1877^{**}
	(-3.92)	(-1.62)	(-3.24)	(-2.24)	(-4.17)	(-0.81)	(-7.52)	(-2.45)
NonPE Target	-0.0704^{**}	-0.0684	-0.1118^{***}	-0.0995	-0.3411^{***}	-0.2635^{**}	-0.0303	0.0143
	(-2.00)	(-1.17)	(-2.66)	(-1.31)	(-4.42)	(-1.98)	(-0.36)	(0.12)
Controls and Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
H_0: PE=NonPE	0.97	0.85	0.42	0.84	0.06	0.23	0.01	0.13
Obs	2,191	1,308	2,191	1,308	2,192	1,308	2,190	1,307
Adj. R^2	0.98	0.98	0.98	0.98	0.91	0.92	0.93	0.92

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RESEARCH ARTICLE

Private equity ownership and nursing home quality: an instrumental variables approach

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Abstract
Since the 2000s, private equity (PE) firms have been actively acquiring nursing homes (NH). This has sparked concerns that with stronger profit motive and aggressive use of debt financing, PE ownership may tradeoff quality for higher profits. To empirically address this policy concern, we construct a panel dataset of all for-profit NHs in Ohio from 2005 to 2010 and link it with detailed resident-level data. We compare the quality of care provided to long-stay residents at PE NHs and other for-profit (non-PE) NHs. To account for unobservable resident selection, we use differential distance to the nearest PE NH relative to the nearest non-PE NH in an instrumental variables approach with and without NH fixed effects. In contrast to concerns of the public regarding quality deterioration associated with PE ownership, we find that PE ownership does not lead to lower quality for long-stay NH residents, at least in the medium term.

Keywords Private equity · Acquisition · Nursing home · Quality · Instrumental variables · Organizational structures · Differential distance

JEL Classification G34 · I11 · L22

Introduction

Private equity (PE) firms play an active but often overlooked role in healthcare markets. They acquire, operate, and sell a variety of healthcare firms. PE firms often acquire companies that

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are undervalued, inefficiently operated, or are financially distressed. Through restructuring and financial reengineering, PE firms aim to add value, enhance productivity, and eventually sell the companies for a profit. Adding value is often achieved through replacing management and aligning managerial incentives, resulting in higher revenues and lower costs (Gompers et al. 2016). For many industries, such as retail or manufacturing, these strategies may theoretically make target companies more efficient and expedite the relocation of resources. However, in healthcare industries, enhancing efficiency, productivity, and profitability are often synonymous with reducing costs in manners that lead to lower quality and worse patient outcomes.

One health care industry where PE interest has been growing is nursing homes (NHs). PE firms have been actively acquiring NHs in the United States and between 1998 and 2008, it is estimated that just under 2000 facilities, about 18% of for-profit NHs, were involved in PE transactions (GAO 2010). In the NH industry, many residents have physical and cognitive impairments (e.g., dementia) and need around-the-clock care.[1] NH quality is often not directly observable at the time of admission except through a limited number of measures publicly reported on the Nursing Home Compare website and after admission cognitively impaired residents may have difficulty communicating quality concerns to family members. Hence, NHs with stronger profit motives may provide suboptimal quality to increase profits (Chou 2002). Although all for-profit NHs may have similar incentives not to provide socially optimal quality (Grabowski and Hirth 2003), PE firms are more likely to aggressively use debt financing, concentrate ownership interests to a few parties, and use high-powered performance-based compensation. These characteristics together with the relative short-termism among PE management all raise concerns that quality may deteriorate further and resident safety may be in jeopardy when NHs are acquired and operated by PE firms (Duhigg 2007).

The media and public often view PE ownership through a negative lens, emphasizing the profit-driven nature of PE firms could lead to lower quality in NHs (Kirchgaessner 2010; The Economist 2010). However, PE ownership could also be beneficial.[2] These benefits can theoretically come from PE firms paying a greater proportion of compensation through equity ownership that gives the management team strong incentives and control to implement change, such as instituting more standardized care practices and replacing underperforming NH administrators. In addition, PE firms may also provide much needed and low-cost capital to financially distressed NHs. If these changes directly enhance how care is provided, PE firms may improve quality while at the same time increase the operational efficiency and profitability of the NHs.

In this paper, we examine the relationship between PE ownership and quality for long-stay NH residents. Although a few studies examine what occurs to NHs after being acquired by PE firms, the literature generally does not find large declines in quality (GAO 2011; Stevenson and Grabowski 2008; Harrington et al. 2012; Pradhan et al. 2013). While the best studies utilize difference-in-differences approaches and attempt to causally identify the effect of PE ownership, they do not always account for the potential statistical biases that may occur because of which NH chains PE firms choose to acquire. Furthermore, these studies only examine facility-level outcomes and do not address statistical biases due to differential selection of residents into PE and non-PE NHs.

[1] Approximately, four in five NH residents have at least one limitation in activities of daily living and about two-thirds have moderate or severe cognitive impairment (CMS 2015).

[2] Jensen (1989) provides a detailed theoretical discussion for why PE ownership could be beneficial, arguing that PE ownership can enhance efficiency and productivity through mitigating conflicts of interest between owners and managers. Kaplan and Stromberg (2009) also provide a nice summary of the beneficial effects through financial, governance, and operational engineering.

This paper differs and contributes to the literature in a number of ways. First, we utilize causal estimation techniques that account for resident selection into NHs. We have resident assessment-level data from the Minimum Data Set (MDS) that allows us to estimate a type of instrumental variables approach, (i.e. two-stage residual inclusion (2SRI) regression) with a theoretically and empirically valid exclusion restriction. This exclusion restriction, which has been utilized by a number of studies to examine NH quality,[3] is the differential distance between each resident's zip code prior to entering the NH and the nearest for-profit NH that is owned and not owned by a PE firm. This 2SRI approach allows us to estimate the relationship between PE ownership and resident-level quality accounting for any biases that may arise from selection of residents into NHs.

Our analytic sample includes long-stay NH residents in for-profit NHs located in Ohio for the years of 2005 through 2010. Studying the state of Ohio has a few advantages. Ohio has a large number of for-profit NHs. This is important because PE firms operate as for-profit entities and mostly acquire NHs that are operated as for-profits. Moreover, we follow the approach of Stevenson and Grabowski (2008) and determine which NHs are owned by PE firms by using PE transactions of NH chains.[4] Therefore, we need a state that has many NH chains that were involved in PE transactions; a situation present in Ohio during our study period. This allows us to present results which utilize both a standard 2SRI approach and a more restrictive 2SRI plus NH-fixed effect model. In addition, by utilizing data from Ohio Medicaid Cost Reports, we have a data source that provides the tax-identification number, chain name, and owner name of each NH over time. Because the NH chain information is notoriously insufficient from any single public source (Grabowski et al. 2016), the combination of tax-identification, chain name, and owner name should increase the accuracy in identifying which NHs are owned by PE firms.

We examine a total of 17 quality measures for long-stay residents; 11 of which were reported on the Nursing Home Compare website during our study period and 6 of them were collected in MDS assessment but were not publicly reported between 2005 and 2010. Although the media and advocacy community are concerned that PE ownership would lead to lower quality, our results do not support this point of view. Comparing naïve ordinary least squares (OLS) and 2SRI results, we find that ignoring resident selection into NHs leads to a systematic underestimation of the long-stay quality of NHs owned by PE firms. Based on the results from the standard 2SRI approach, residents at PE NHs receive at least similar quality to long-stay residents at other for-profit NHs not owned by PE firms. When we use the more restrictive 2SRI plus NH-fixed effect model, the conclusions drawn are similar to the standard 2SRI approach. Taken together, PE ownership does not result in quality deterioration relative to other for-profit NHs, at least in the short and medium terms (i.e., 4 to 5 years).

Private equity and nursing homes

When firms mature and management becomes entrenched, some companies may be slow in reacting to market dynamics and may allocate capital and resources in an ineffective manner. This can create tension between management and shareholders, causing the company to become undervalued. PE firms such as the Carlyle Group or Kohlberg Kravis Roberts can

[3] For examples, please see Bowblis and McHone (2013), Bowblis et al. (2016), Grabowski et al. (2013), Huang and Bowblis (2018), and Rahman et al. (2016).

[4] We utilize this approach because the decision to purchase or sell an entire chain is not likely subject to the performance of any individual facility. There is also better information available on transactions involving whole chains than individual facilities.

play a role in enhancing the value of these companies by obtaining majority control or by taking the public company private through leveraged buyouts (Kaplan and Stromberg 2009). Through majority ownership, PE firms exercise control and help restructure the acquired company to increase efficiency and profitability. Generally, PE firms accomplish this by using debt financing, strengthening corporate governance, and through reorganization of operations.[5]

Debt is an important tool for PE firms because interest on debt is tax deductible, greater debt loads can lead to higher returns on equity (i.e. greater financial leverage), and debt disciplines managers to slash unnecessary spending and avoid suboptimal investments (Jensen 1986). PE firms can add value through their relationships with lenders, which allows PE firms to access capital that is unavailable to the target company and borrow greater sums of money at lower interest rates, often with less restrictive debt covenants (Demiroglu and James 2008; Ivashina and Kovner 2011).

In addition, PE firms can be a catalyst for changes in corporate governance. The governance of acquired companies are often changed through concentrating ownership to a few parties, increasing managerial ownership, replacing top management, and reducing the size of governing boards (Acharya et al. 2013).[6] These changes all grant PE firms greater control to expedite organization restructuring. Finally, PE firms can create value by restructuring the operations of the company with the help of access to industry talent and experts who have successful careers in the field (Acharya et al. 2013). Through company-wide assessments, PE firms determine the best course of action to cut waste, reduce costs, and increase revenues.[7]

The NH industry is an example of one industry in which PE firms have applied these strategies. Though the NH industry has long argued that Medicaid, the largest payer for NH services, reimburses at low rates that sometimes are below costs, NHs have a steady demand for their services and stable revenues from government payers.[8] PE firms believed that some NHs were operated inefficiently and through their expertise and knowledge, PE firms could enhance efficiency and operate NHs at a lower cost than existing management. This made PE involvement in the NH highly attractive throughout the 2000s, with over 18% of for-profit NHs being involved in transactions with PE firms (GAO 2010). While enhancing efficiency in the NH industry can take many forms, such as improving scheduling, reducing staff turnover, or hiring more effective managers, this wave of PE acquisition caused many to question the consequences of PE ownership (GAO 2010).

In the case of the NH industry, operational changes can also mean cuts to staffing and services that directly impact the quality of care provided to NH residents. It can also imply using care practices that are less costly, but may harm residents in terms of the quality of care and quality of life—such as using antipsychotics as a form of chemical restraint for residents with dementia. The use of debt can have negative consequences by increasing the amount of revenue that must be devoted to interest payments and paying down debt instead

[5] For examples, see Kaplan (1989), Smith (1990), Lichtenberg and Siegel (1990), Harris et al. (2005), Bergstrom et al. (2007); and Cumming et al. (2007).

[6] Studies show that chief executives and top management teams at the PE-owned companies have larger equity stacks (Kaplan 1989), and these companies have a smaller and more efficient governance board that meets more frequently (Cornelli and Karakas 2008).

[7] PE firms are likely to relocate capital and labor to improve productivity (Davis et al. 2014). For example, PE firms may reallocate resources from inefficient to more efficient factories. It is these operational changes that create a negative image of PE firms, as these restructuring decisions cause closure of plants and stores, leading to lay-offs (Wong 2007; SEIU 2009).

[8] There are over 1.4 million people living in NHs (CDC 2016), over 35% of Americans at the age of 65 are expected to use a NH at least once in their lifetimes (Houser 2007), and Medicaid is the largest payer for NH services, spending over $51 billion per year (MedPac 2016) and providing a steady source of cash flow.

of reinvesting in quality improvement efforts. In addition, some PE firms may split the NH into two companies—one that operates and provides NH care and another one that owns the real estate where care is provided. While this can "unlock" the value of assets for the PE firm, it also leaves the NH operating company with fewer resources and flexibility to invest in quality if financial situations become tough. Overall, there are sufficient concerns that PE ownership of NHs can lead to reductions in staffing, higher turnover of staff, an increase in the use of low-quality care practices, or a greater incentive of managers to focus on profits instead of quality.

In industries where quality is directly observable, the ability of managers to lower quality is limited because providing a lower quality product will reduce demand and revenue. However, the ability to observe and act on quality can be difficult for typical NH consumers. Foremost, NH quality is an experience good, which means multiple aspects of quality are not fully known to consumers prior to their admission. While some quality measures are available on public websites (e.g. Nursing Home Compare), many other aspects of NH care, such as quality of life, are not publicly reported. Moreover, many NH residents are cognitively impaired and therefore have difficulties in communicating quality issues to family members. Furthermore, many residents face high switching costs to move to another facility, even when they observe poor NH quality. Taken together, a NH is not fully rewarded by providing high quality, and at the same time, not fully penalized by providing lower quality. This creates a situation in which NHs can take advantage of residents by promising to provide high quality when first choosing a NH but underperform on promises, reduce services, and skimp on quality once admitted (Hirth 1999).

These special institutional details faced by NH residents creates strong incentives for profit-motived owners of NHs to provide lower quality than nonprofit operators that may be less motivated by profits (Grabowski et al. 2013). Since PE firms are not long-term operators, but instead have the goal of exiting investments within a specific timeframe,[9] PE-owned NHs may have stronger profit motivates and reduce quality more when compared to other for-profit entities. In particular, PE ownership of NHs could lead to significant quality deterioration as the acquired NHs needs to devote more resources towards financing debt and meet other financial goals required by PE investors.

A number of studies have examined the impact of PE ownership on NHs. In one paper, Stevenson and Grabowski (2008) used a difference-in-differences approach and found that after controlling for secular time trends there was no significant difference in quality between for-profit NHs and PE-owned NHs. Other studies on PE ownership in the NH industry also found no consistent difference in quality and other operating outcomes (Cadign et al. 2015; GAO 2011; Harrington et al. 2012; Pradhan et al. 2013). A limitation in the existing literature is that these studies are all based on facility-level quality measures and do not account for resident selection into NHs. If PE ownership systematically alters NHs' admissions practices, the results in previous studies could be biased. Utilizing the resident assessment-level data and quality measures, we attempt to address the resident selection problem and provide a more detailed empirical assessment of the relationship between PE ownership and NH quality for the long-stay residents.

[9] For example, 42% of PE investments are sold within 5 years and 72% are sold within 10 years, either through a sale to a strategic buyer, a sale to another PE firm, or through listing the company on a public stock exchange through an initial public offering (i.e., IPO) (Kaplan and Stromberg 2009).

Data and empirical strategy

Data and study sample

To examine the effect of PE ownership on NH long-stay quality, we merge data from multiple sources. We obtain the Ohio Medicaid Cost Reports for years 2005–2010 from the Ohio Department of Job and Family Services. The Ohio Medicaid Cost Reports are facility-level datasets collected annually for every NH that receives Medicaid reimbursement in the state of Ohio, including information on the entity that owns the facility and their tax-identification number.

Next, the Ohio Medicaid Cost Reports are merged with data from the Online Survey, Certification and Reporting (OSCAR) System. The OSCAR database is the most comprehensive source of facility-level information (e.g. operational characteristics, staffing, and quality) on NHs that is collected as part of the annual recertification process including all NHs receiving Medicare and/or Medicaid reimbursement. We also utilize the zip code of each NH to identify the urban/rural setting of the facility via rural-urban commuting area (RUCA) codes available from the WWAMI Rural Health Research Center. Community characteristics measured at the county-level are obtained from the Area Health Resource File (AHRF).

Finally, these datasets are merged with resident assessment-level information from the Minimum Dataset (MDS) version 2.0. MDS is an assessment of all residents which occur on admission, discharge, and at least quarterly between admission and discharge. MDS contains information of the resident's home zip code prior to entering the NH and the clinical condition of the resident. MDS also contains information to construct measures of quality. To identify long-stay residents, we restrict the sample to quarterly and annual assessments.

By definition, there is no PE ownership of nonprofit and government NHs. Therefore, we limit our sample to only for-profit NHs. Additionally, because the state of Ohio has very few hospital-based facilities and these tend to be operated by nonprofit entities, our analysis is restricted to free-standing NHs. Since we are focusing on Ohio NHs, we also require the resident to have lived in Ohio prior to admission to the NH. The unit of analysis is at the resident assessment-level, resulting in 752,240 assessments of long-stay residents in 691 for-profit NHs, though exact sample sizes vary with the quality measure analyzed.

Defining private equity ownership

While our sample includes residents in all for-profit NHs, we follow the strategy utilized by Stevenson and Grabowski (2008) and identify NH chains that were acquired or divested in entirety by PE firms from 2000 to 2010.[10] We use chain names to identify which individual NHs are owned by PE firms. This approach has two main empirical advantages. First, because the decision of purchase and sale of the entire chain is not likely subject to the performance of any particular facility, this approach mitigates endogeneity problems that may arise due to why some individual NHs participate in PE transactions. In spirit, this identification strategy is similar to the literature which focuses on large chain mergers (Dafny et al. 2012; Hastings and Gilbert 2005). Second, we also have better information on whole-chain acquisitions and are able to separate different types of PE transactions, which is overlooked in the literature.

[10] This period of time that is both before and during the study period. We use 2000–2004 as a look-back period to identify major PE transactions of NHs prior to our study period. This enables us to identify NHs owned by PE firms but the transaction occurred before the study period (2005–2010).

We rely on two sources to identify NH chains that were involved in transactions with PE firms. First, we construct a list of these chains from the literature (Cadign et al. 2015; Stevenson and Grabowski 2008; Pradhan et al. 2013). Second, we utilize Lexis–Nexis and conduct a search to identify NH chains that were acquired or divested by PE firms but were not mentioned in the literature. Next, we utilize the name of the entity which owned each NH and the tax-identification number of the owner to identify individual NH facilities that were operated or owned by these chains.[11] We then create a binary indicator variable, *PE*, indicating if a NH is owned by a PE firm any time during that calendar year. Because some NHs were owned and operated by PE firms during the entire study period of 2005 through 2010, whereas some NHs were acquired or divested by PE firms, our *PE* variable reflects if a nursing facility is contemporaneously owned by a PE firm in the year of the observation.

Overall, we are able to identify five regional or national chains involved in transactions with PE firms. All of these transactions occurred from February 2006 through December 2007. The NH chains involved in these transactions included *Harborside/Sun (2006)*, *HCR Manor Care (2007), Laurel Health Care (2006)*, *Tandem Health Care (2006)*, and *Trilogy Health Services (2007)*. These include some of the largest PE transactions involving NHs chains in the United States.[12] We classify these PE transactions into two categories: (1) transaction from a non-PE chain to a PE firm, or vice versa (HCR Manor, Harborside, and Laurel), (2) transaction from one PE firm to another PE firm (Tandem and Trilogy). This implies that NHs owned by Tandem and Trilogy are considered PE-owned for the entire study period. To the best of our knowledge, previous studies treat the acquisitions of Tandem and Trilogy as non-PE to PE transactions. This may lead to attenuation bias and underestimate the effect of PE ownership. We are unaware of other PE firms owning other NH chains in the state of Ohio for the entire study period.

Though we only observe the PE transactions associated with five NH chains, the number of NHs involved in these transactions is non-trivial and matches the general national trend (GAO 2010). Of the 691 for-profit NHs in the state of Ohio these five PE transactions affected 98 NHs (14.2% of the sample). Of these 98 NHs, 73 NHs involved switching from non-PE chain to PE-ownership or vice versa.

Defining nursing home long-stay quality

NHs provide care to short-stay, post-acute care patients and long-stay residents that need long-term care. Because these two populations are different in terms of how to measure their case-mixes and which aspects of quality are the most important, PE ownership could affect each population differently. Therefore, we focus on the quality of care provided to long-stay residents. Using MDS assessments, we construct a set of 17 binary resident-level quality measures that indicate whether long-stay NH residents had certain medical conditions or were treated with care practices that indicate poor quality. This implies that presence of a condition or care practice is associated with poor quality and we would expect the coefficient on PE ownership to be positive if PE firms provide worse quality.

We classify quality measures into two types: those publicly reported on the Nursing Home Compare (NHC) website and those that were not publicly reported during the study period of

[11] We also cross-check our data with a report on PE ownership in NHs from the Government Accountability Office (2010) to account for any chains that may operate various brand names.

[12] More details on these individual transactions are available in “Appendix A”.

2005 through 2010.[13] Because the NHC website may be used by resident in selecting NHs, we examine publicly and non-publicly reported quality separately because PE-owned NHs may have stronger incentives to maintain or improve publicly reported quality while allowing quality in non-reported dimensions to deteriorate (Lu 2016). For publicly reported quality, these measures are defined and constructed for each individual resident based on the technical instructions used for long-stay resident quality measures reported on the NHC website for MDS version 2.0 (Abt Associates 2004). The only difference between our measures and those reported on the website is our observations use quarterly and annual assessments and we do not aggregate the measures to the facility level.[14] Instead, we run regressions at the resident assessment level. For non-publicly reported measures, we define each quality measure using the items in MDS outlined in the instructions for how NHs fill out the OSCAR data (CMS Form-672).[15]

Quality measures reported on the NHC website during the study period include indicators for whether the resident had a decline in physical functioning, used a catheter, had moderate-severe pain, was mostly bed or chairfast, had incontinence issues, was physically restrained, had a urinary tract infection, had significant weight loss, had a pressure ulcer (low vs. high-risk resident), and had a fall with a major injury. Quality measures that were not reported on the NHC during the study period include indicators for whether the resident had a contracture, a rash, or was using one of four classes of psychotropic medications: antipsychotic, antianxiety, antidepressant, or hypnotic medication.

Empirical specification

Main equation

Our empirical model describes how a comprehensive set of quality measures are different based on whether a PE firm owns a NH. In our dataset, the unit of analysis is the resident assessment for long-stay residents (i.e. quarterly and annual assessments). Treating $Q_{i,j,t}$ as a binary measure of quality for resident i NH j in year t, we estimated the following linear probability model:

$$Q_{i,j,t} = PE_{j,t}\beta + Resident_{i,j,t}\delta + NH_{j,t}\gamma + M_{m,t}\theta + \tau_t + \delta_j + \epsilon_{i,j,t} \tag{1}$$

where $PE_{j,t}$ is the variable of interest and indicates whether NH j in year t is owned by an PE firm. $Resident_{i,j,t}$ are resident-level control variables, $NH_{i,t}$ is a vector of time-varying and exogenous NH-level variables, $M_{m,t}$ represents local market and demographic characteristics measured at the county-level, τ_t is a set of year indicator variables, δ_j is a NH fixed effect, and $\epsilon_{i,j,t}$ is an error term. Because residents can be assessed multiple times, standard errors are clustered by residents.

[13] Some quality measures related to medication use are currently reported on the NHC website but were not publicly reported during the study period.

[14] Assessments must also have non-missing data for control variables. Our measures are consistent with the aggregate measures reported on the NHC website.

[15] https://www.cms.gov/Medicare/CMS-Forms/CMS-Forms/Downloads/CMS672.pdf.

Identification in Eq. (1) comes from the fact that some for-profit NHs are owned by PE firm and that a number of PE-owned facilities were either acquired from or divested to non-PE entities. The NH-fixed effect accounts for unobservable and time-invariant facility-level characteristics. However, because those non-PE/PE ownership switches occurred in 2006 and 2007, we only have 3 or 4 years in data of the post-transaction periods to study the effects of PE ownership. This relatively short time frame may not be sufficient to observe the full PE effects on quality, and therefore our NH-fixed effects specification suits better to identify the effect of PE ownership in the short and medium terms. For completeness, we estimate and present results for Eq. (1) with and without NH-fixed effects, though our preferred specifications include NH-fixed effects.

Instrumental variable and first stage

One major concern in estimating Eq. (1) is the differential selection of residents between NHs that are owned and not owned by PE firms. On the demand side, selection may occur because PE transactions were publicly reported in the news, and sophisticated consumers who are often wealthier and better educated, may avoid PE NHs. Sophisticated consumers are also more likely to have better unobserved health status, leaving PE NHs with residents that have worse unobservable health conditions. Without controlling for this selection, Eq. (1) can be biased towards finding PE NHs have worse quality than they actually provide.

However, resident selection into NHs can also occur from the supply side. PE NHs may be more likely to avoid residents who have worse unobservable health status. These residents require more resources and without being offset by higher reimbursement, can lead to lower profitability. In this scenario, PE NHs are likely to have residents with better unobservable health. Conversely, NHs can exploit some reimbursement mechanisms to increase profits (Bowblis and Brunt 2014). If PE NHs are able to exploit reimbursement systems, then PE NHs may be more willing to admit residents with worse unobservable health in order to increase the utilization of ancillary services, leading to higher reimbursement and profitability. This implies on the supply side, the direction of the bias from resident selection depends on which mechanism is stronger, the incentive to admit or not admit residents with worse unobservable health conditions.

Taken together, there is sufficient rationale to be concerned that resident selection may bias the effect of PE ownership on quality, though the net direction of the bias is ambiguous. To address this selection problem, we use an instrumental variables approach, specifically, two-stage residual inclusion (2SRI) (Terza et al. 2008) that allows us to causally identify the effect of PE ownership on NH quality.[16] In 2SRI, we need at least one exclusion restriction that predicts whether a resident chooses a PE-owned NH but is uncorrelated with quality. Following the literature, we utilize the differential distance calculated in miles from the residents' home to the nearest for-profit NH owned by a PE firm and the nearest for-profit NH not owned by a PE firm in the year of the observation. This distance is calculated using the zip code of where the resident lived prior to being admitted to the NH and the zip code of the facility in the OSCAR data. Positive and larger values imply that a PE-owned NH is further away, otherwise negative and smaller values suggest PE NHs are the closest for-profit NHs.

Because the proximity to a resident's or family member's home is the most important factor in selecting NHs (Shugarman and Brown 2006; Gadbois et al. 2017), differential distance has

[16] There is an ongoing debate about whether 2SRI or two-staged least squares (2SLS) is more appropriate in various estimation contexts (Basu et al. 2018; Chapman and Brooks 2016). We compared our results using 2SRI with 2SLS and found similar results.

been found to be a theoretically valid exclusion restriction and is used by a number of papers to handle the endogeneity associated with resident selection into NHs (Bowblis and McHone 2013; Bowblis et al. 2016; Grabowski et al. 2013; Huang and Bowblis 2018; Rahman et al. 2016). We provide more discussion about the statistical validity of differential distance as an exclusion restriction in "Validity of the exclusion restriction" section. With differential distance, denoted $DD_{i,j,t}$, we estimate a first stage equation using the linear probability model as follows:

$$PE_{i,j,t} = DD_{i,j,t}\sigma + Resident_{i,j,t}\theta + NH_{j,t}\pi + M_{m,t}\theta + \tau_t + \mu_{i,j,t} \tag{2}$$

where all other variables have the same interpretation as Eq. (1). Following the 2SRI approach, we estimate Eq. (2) in order to obtain predicted residuals for each observation. These residuals are then included in Eq. (1) as a covariate.

Control variables

In all regression models, we also include control variables constructed from the MDS, OSCAR, RUCA, and AHRF. At the resident assessment level, we control for each resident's age, gender, white/non-white, cognitive status (MDS 2.0 item B4, Cognitive Skills for Daily Decision-Making), activities of daily living index score, and diagnoses of major health conditions. Facility-level controls include size (number of beds), chain affiliation, occupancy rate, staffing level, skill-mix of staffing, and the payer-mix among Medicaid, Medicare, and private payers. We also include an indicator for whether the facility has a dementia special care unit.

To control for differences in geographic settings, we differentiate between urban and rural settings by categorizing rural NHs into urban areas, micropolitan towns, small rural towns, and isolated small rural towns according to Categorization A provided by the WWAMI Rural Health Research Center.[17] To control demographic and economic differences among counties, we include population density, percentage of population are above 65 years old, per capital income, and poverty rate.

Results

Summary statistics

The summary statistics for the control variables are reported in Table 1 for the overall sample (N = 752,240), resident assessments at NHs not owned by PE firms (N = 691,630), and resident assessments at NHs owned by PE firms (N = 60,610). About 8.1% of long-stay assessments are in PE NHs. On average, most long-stay residents live further away from PE NHs, with the differential distance of 13.42 miles. The average differential distance of residents that live in non-PE NHs is 14.48 miles, and the average differential distance of the PE NH residents is 1.33 miles, suggesting residents choose NHs that are close to their home.

Among the resident-level characteristics, residents in both type of facilities are rather similar except that at PE NHs, there are fewer residents who are severely cognitively impaired (10.1 vs. 14.0%). For the facility and local market characteristics, weighted by the number of

[17] http://depts.washington.edu/uwruca/ruca-uses.php.

Table 1 Descriptive statistics: long-stay residents by private equity (PE) ownership

	All for-profit facilities		Not PE owned		PE owned	
	Mean	SD	Mean	SD	Mean	SD
Key Variables						
Differential distance of PE minus non-PE	13.423	14.206	14.483	14.240	1.325	5.606
Private equity (PE) owned	0.081	0.272				
Long-stay resident characteristics						
Age	82.931	8.213	82.896	8.224	83.327	8.078
Female	0.728	0.445	0.728	0.445	0.727	0.446
Non-white	0.131	0.337	0.131	0.337	0.127	0.333
Moderately independent cognitive status	0.218	0.413	0.216	0.412	0.243	0.429
Moderately impaired cognitive status	0.542	0.498	0.544	0.498	0.521	0.500
Severely impaired cognitive status	0.137	0.343	0.140	0.347	0.101	0.301
Activities of daily living index	12.244	4.649	12.207	4.663	12.663	4.456
Diabetes	0.344	0.475	0.343	0.475	0.350	0.477
Arteriosclerotic heart disease	0.191	0.393	0.190	0.392	0.196	0.397
Heart failure	0.277	0.448	0.277	0.447	0.287	0.453
Stroke	0.230	0.421	0.230	0.421	0.233	0.423
Hip fracture	0.027	0.163	0.028	0.164	0.023	0.150
COPD	0.235	0.424	0.235	0.424	0.237	0.425
Pneumonia	0.033	0.179	0.033	0.180	0.032	0.175
Facility characteristics						
Number of beds	116.250	45.587	116.479	45.626	113.640	45.054
Chain-owned facility	0.701	0.458	0.675	0.468	1.000	0.000
% Medicaid-paid	0.649	0.136	0.655	0.134	0.584	0.145
% Medicare-paid	0.132	0.076	0.129	0.075	0.165	0.078
Occupancy rate	0.877	0.103	0.877	0.103	0.868	0.094
Dementia special care unit	0.243	0.429	0.250	0.433	0.167	0.373
Registered nurse (HPRD)	0.300	0.160	0.295	0.157	0.347	0.183
Licensed practical nurse (HPRD)	0.890	0.288	0.896	0.289	0.829	0.264
Certified nurse aide (HPRD)	2.206	0.590	2.229	0.593	1.943	0.477
Urban-Rural						
Micropolitan	0.176	0.381	0.175	0.380	0.196	0.397
Small rural town	0.070	0.255	0.063	0.242	0.154	0.361
Isolated small rural town	0.020	0.138	0.019	0.136	0.027	0.162

Table 1 continued

	All for-profit facilities		Not PE owned		PE owned	
	Mean	SD	Mean	SD	Mean	SD
County characteristics of facility						
County population density	963.771	952.734	967.426	950.888	922.060	972.579
Number of facilities in county	33.274	32.209	33.464	32.216	31.109	32.051
% County aged 65 +	14.131	2.022	14.125	2.035	14.200	1.862
County per capita income	33,922.590	5547.132	33,933.945	5605.309	33,793.013	4832.077
County poverty rate	14.163	3.797	14.131	3.788	14.527	3.884
Sample size	752,240		691,630		60,610	

The unit of observation is a resident assessment. Differential distance is defined as distance in miles of nearest for-profit facility owned by a PE minus nearest for-profit facility not owned by a PE in the year of the observation. Larger values imply the closest PE owned facility is further away

HPRD hours per resident day

resident assessments, PE NHs are more likely to be part of a chain (100 vs. 67.5%),[18] have fewer Medicaid (58.4 vs. 65.5%) and more Medicare residents (16.5 vs. 12.9%), and are less likely to have dementia special care unit (16.7 vs. 25.0%). In terms of nursing staff, direct care staffing levels are measured in terms of hours per resident day (HPRD) for registered nurse (RN), licensed practical nurse (LPN), and certificated nurse aide (CNA) staffing. PE NHs have higher RN staffing levels (0.347 vs. 0.295 HPRD) and lower LPN and CNA staffing levels (0.829 vs. 0.896 HPRD; 1.943 vs. 2.229 HPRD). Both types of NHs are relatively similar in terms of size (number of beds) and occupancy rate. Residents in PE NHs are more likely to locate in rural settings (37.7 vs. 25.7%).[19] Both types of NHs are relatively similar in other county-level characteristics.

Table 2 reports the differences in the quality outcomes by PE ownership. Overall, there are no consistent differences between NHs that are owned and not owned by PE firms. Among publicly reported measures (Panel A), PE NHs do have slightly more residents with declines in physical functioning (13.4 vs. 12.1%), bowel or bladder incontinence (46.4 vs. 44.9%), and falls with a major injury (14.0 vs. 13.1%). In terms of non-publicly reported measures (Panel B), PE owned NHs have lower prevalence of contractures (48.3 vs. 52.9%) and prevalence of antipsychotic medication use (21.0% vs. 24.9%). Residents at PE NHs are also more likely to have rash (16.1 vs. 13.5%).

Validity of the exclusion restriction

In addition to the theoretical argument that differential distance is a satisfactory exclusion restriction, we empirically examine the validity of the exclusion restriction following work by Grabowski and Hirth (2003). Specifically, we divide the sample into observations above and below the median of differential distance and compare summary statistics of key covariates between these two groups. The purpose of this exercise is that if the exclusion restriction

18 Because our identification of PE ownership relies on chain transactions, all PE NHs in our sample by design are chain-affiliated.

19 PE NHs are more likely to locate at non-urban settings: 19.6% in micropolitan areas, 15.4% in small rural towns, and 2.7% in isolated small rural towns.

Table 2 Summary of long-stay quality measures by private equity (PE) ownership

		All For-Profit Facilities		Not PE Owned		PE Owned	
	Sample size	Mean	SD	Mean	SD	Mean	SD
Panel A: publicly reported quality measures							
Decline in physical functioning	658,955	0.122	0.328	0.121	0.326	0.134	0.341
Catheter use	733,712	0.065	0.247	0.065	0.247	0.068	0.252
Moderate-severe pain	733,197	0.090	0.286	0.090	0.286	0.090	0.286
Mostly bed or chairfast	752,021	0.036	0.186	0.036	0.187	0.033	0.179
Bowel/bladder incontinence	568,265	0.450	0.497	0.449	0.497	0.464	0.499
Physically restrained	751,460	0.056	0.230	0.057	0.231	0.051	0.219
Urinary tract infection	752,234	0.110	0.313	0.110	0.313	0.113	0.317
Weight loss	714,022	0.078	0.268	0.078	0.268	0.077	0.267
Pressure ulcers (low risk resident)	269,942	0.019	0.136	0.019	0.137	0.015	0.122
Pressure ulcers (high risk resident)	482,213	0.102	0.303	0.103	0.303	0.095	0.293
Falls with major injury	752,194	0.132	0.339	0.131	0.338	0.140	0.347
Panel B: non-publicly reported quality measures							
Contractures	752,148	0.525	0.499	0.529	0.499	0.483	0.500
Rash	752,220	0.137	0.344	0.135	0.342	0.161	0.368
Antipsychotic medication	708,707	0.246	0.430	0.249	0.432	0.210	0.407
Antianxiety medication	752,229	0.211	0.408	0.211	0.408	0.210	0.408
Antidepressant medication	752,229	0.583	0.493	0.583	0.493	0.585	0.493
Hypnotic medication	752,234	0.059	0.237	0.060	0.237	0.057	0.232

The unit of observation is a resident assessment. Sample sizes vary due to the inclusion criteria utilized for each quality measure is valid, then a higher (lower) proportion of residents will be admitted to PE NHs when the differential distance is below (above) the median. Furthermore, there should be little difference in summary statistics of observable covariates. If these observable covariates are balanced across the two groups, unobservable characteristics are also likely to be balanced and the exclusion restriction is less likely to be correlated with unobservable characteristics, such as unobservable health status, threatening the validity of the exclusion restriction.

As Table 3 shows, observations with the differential distance below the median (comparing to above the median) are more likely to be admitted to PE owned NHs (14.9% vs. 1.2%). More importantly, there is little variation in the summary statistics for resident covariates by having differential distance above or below the median, except that facilities with differential distance below the median have a greater proportion of non-white residents (16.8 vs. 9.4%). Also, facilities with differential distance below the median are larger, more likely to be chain-affiliated, less likely to have dementia special care unit, and less likely to be rural areas. While some of these differences become smaller by restricting the sample to only chain-based NHs, we provide a more detailed discussion of these issues in robustness checks ("Robustness checks" section). Overall, Table 3 suggests that the observable covariates are relatively balanced, mitigating the concerns that the exclusion restriction is correlated with unobservable characteristics.

At the bottom of the Table 4 we report the coefficient estimates for differential distance on the probability of a resident's choice of a PE NH. For one standard deviation increase in differential distance (14.21 miles), residents are less likely to be admitted to PE NHs by 5.68 percentage points. A F-test on the exclusion restriction in the first stage results in a F-statistics of 4406. This implies differential distance strongly predicts the use of PE owned NHs and our instrument passes weak instrument tests (Staiger and Stock 1997; Stock and Yogo 2005).

In addition to standard statistical tests for weak instruments, we conduct a falsification test that is consistent to the differential distance literature (Grabowski et al. 2013; Bowblis and McHone 2013). The intuition behind the falsification test is that differential distance should be more sensitive for a resident admitted to a NH closer to their previous residence, but should have less impact if the resident chose a NH that is further away. Therefore, the effect of differential distance on the probability of using a PE-owned NH should decrease when we restrict the sample to people who use NHs further from their prior residence. Based on our sample, which only includes residents who originally lived in Ohio, 50% of residents chose a NH that is within 8.6 miles and 75% of residents lived in a NH within 20 miles of their previous home. Therefore, we cannot use the standard 50, 100, or 200 miles travel distance as the cutoffs for the falsification test. However, we find that as the sample is restricted to those who travelled further, the predictive power of differential distance becomes weaker. For example, by restricting the sample to those who travelled at least 25 miles, the effect of one standard deviation increase in differential distance (14.21 miles) reduces the likelihood of choosing PE-owned NHs by only 0.89 percentage points. Therefore, our exclusion restriction passes the falsification test. Overall, differential distance satisfies both the exclusion restriction and relevance condition, and is a valid and strong instrumental variable in predicting the choice of PE NHs.

Table 3 Resident-level characteristics by differential distance

	Differential distance < median	Differential distance ≥ median
Private equity (PE) owned nursing homes	0.149	0.012
Long-stay resident characteristics		
Age	82.840	83.021
Female	0.724	0.732
Non-white	0.168	0.094
Moderately independent cognitive status	0.214	0.222
Moderately impaired cognitive status	0.551	0.533
Severely impaired cognitive status	0.136	0.137
Activities of daily living index	12.281	12.207
Diabetes	0.348	0.340
Arteriosclerotic heart disease	0.189	0.192
Heart failure	0.277	0.278
Stroke	0.236	0.224
Hip fracture	0.025	0.029
COPD	0.233	0.237
Pneumonia	0.033	0.034
Facility and county characteristics		
Number of beds	120.064	112.437
Chain-owned facility	0.745	0.658
% Medicaid-paid	0.643	0.655
% Medicare-paid	0.131	0.132
Occupancy rate	0.876	0.877
Dementia special care unit	0.222	0.264
Registered nurse (HPRD)	0.309	0.290
Licensed practical nurse (HPRD)	0.880	0.900
Certified nurse aide (HPRD)	2.159	2.252
Micropolitan	0.104	0.249
Small rural town	0.049	0.091
Isolated small rural town	0.010	0.029
County population density	1295.653	631.996
Number of facilities in county	43.881	22.671
% County aged 65+	14.072	14.189
County per capita income	35,674.519	32,171.228
County poverty rate	14.753	13.573
Sample size	376,059	376,181

The unit of observation is a resident assessment. Differential distance is defined as distance in miles of nearest for-profit facility owned by a PE minus nearest for-profit facility not owned by a PE in the year of the observation. Larger values imply the closest PE owned facility is further away. The median distance is 8.5 miles

Table 4 Effect of private equity ownership on long-stay quality

Long-stay quality measure	OLS (1)	OLS w/fixed effects (2)	2SRI (3)	2SRI w/fixed effects (4)
Publicly reported quality measures				
Decline in physical functioning	0.005***	0.001	0.110***	− 0.002
(N = 658,955)	(0.002)	(0.003)	(0.009)	(0.012)
Catheter use	− 0.003	− 0.004	− 0.041***	− 0.016
(N = 733,712)	(0.003)	(0.004)	(0.012)	(0.015)
Moderate-severe pain	− 0.003	− 0.004	− 0.023*	− 0.048***
(N = 733,197)	(0.002)	(0.004)	(0.013)	(0.016)
Mostly bed or chairfast	− 0.004***	0.002	− 0.005	− 0.006
(N = 752,021)	(0.002)	(0.002)	(0.009)	(0.011)
Bowel/bladder incontinence	− 0.008*	0.002	− 0.067***	− 0.029
(N = 568,265)	(0.005)	(0.006)	(0.023)	(0.028)
Physically restrained	0.002	0.004	0.017	0.023
(N = 751,460)	(0.002)	(0.003)	(0.012)	(0.014)
Urinary tract infection	− 0.003*	0.002	− 0.012	− 0.018
(N = 752,234)	(0.002)	(0.003)	(0.010)	(0.013)
Weight loss	− 0.003**	− 0.004*	− 0.026***	− 0.013
(N = 714,022)	(0.002)	(0.003)	(0.008)	(0.011)
Pressure ulcers (low risk resident)	− 0.003**	− 0.003	− 0.026***	− 0.022**
(N = 269,942)	(0.001)	(0.002)	(0.007)	(0.009)
Pressure ulcers (high risk resident)	− 0.008***	− 0.003	− 0.081***	− 0.029
(N = 482,213)	(0.003)	(0.004)	(0.014)	(0.017)
Falls with major injury	0.008***	0.008**	0.004	− 0.006
(N = 752,194)	(0.002)	(0.003)	(0.010)	(0.014)
Non-publicly reported quality measures				
Contractures	− 0.047***	− 0.041***	− 0.242***	− 0.173***
(N = 752,148)	(0.005)	(0.006)	(0.026)	(0.029)
Rash	0.017***	0.009**	− 0.045***	− 0.019
(N = 752,220)	(0.003)	(0.004)	(0.014)	(0.017)
Antipsychotic medication	− 0.007	− 0.001	0.046*	− 0.032
(N = 708,707)	(0.005)	(0.006)	(0.024)	(0.029)
Antianxiety medication	− 0.002	− 0.002	0.060***	− 0.083***
(N = 752,229)	(0.004)	(0.006)	(0.022)	(0.026)
Antidepressant medication	− 0.009	− 0.009	0.208***	− 0.092***
(N = 752,229)	(0.006)	(0.007)	(0.028)	(0.033)
Hypnotic medication	− 0.004*	− 0.003	− 0.006	0.001
(N = 752,234)	(0.002)	(0.003)	(0.011)	(0.015)

Table 4 continued

Long-stay quality measure	OLS (1)	OLS w/fixed effects (2)	2SRI (3)	2SRI w/fixed effects (4)
First stage results				
Differential distance			– 0.004***	
			(0.000)	
F-statistic on differential distance			4405.56	

Regressions include controls reported in Table 1, year fixed effects, and indicators for potentially erroneous staffing levels using a unit of observation of a resident assessment. For the catheter use and pain quality measures, additional controls are included based on CMS definitions of quality measures. Each column which reports with fixed effects include facility-fixed effects in the quality regression. Standard errors are clustered at the resident level

***$p<0.01$, **$p<0.05$, *$p<0.1$

Primary findings

The effects of PE ownership on NH quality are presented in Table 4. The rows of Table 4 report the difference between PE-owned and non-PE-owned NHs for various quality measures. The first and second columns reports results for Eq. (1) using the linear probability models that are estimated by OLS, with the second column including NH fixed effects. The third and fourth columns report the effects using 2SRI, with column 4 including NH fixed effects. In all cases, positive and larger numbers would indicate that PE-owned NHs provide worse quality.

Examining the OLS results (column 1), most of the coefficient estimates are negative, suggesting better quality at PE-owned NHs. Of the 17 quality measures, 8 measures are negative and statistically significant. In contrast, only three coefficients are positive and statistically significant. When the model is estimated using 2SRI, the results (column 3) are rather consistent with most quality measures having negative signs with larger magnitudes, indicating better quality in NHs owned by PE firms. Specifically, PE-owned NHs have 4.1 percentage points fewer residents using catheters, 2.3 percentage points fewer residents with moderate-severe pain, 6.7 percentage points fewer residents with incontinence, 2.6 percentage points fewer residents with significant weight loss, and 2.6 and 8.1 percentage points fewer low- and high-risk resident with pressure ulcers. In contrast, PE-owned NHs also have 11 percentage points more residents with a decline in physical functioning and 4.6 percentage points more residents using antipsychotic medications.

In the models that control for NH fixed effects and the potential selection of residents into PE-owned NHs (column 4), 15 out of 17 measures are negative, indicating better quality at PE-owned NHs. However, only 5 results are negative and statistically significant, with PE-owned NHs having fewer residents with moderate-severe pain, pressure ulcers among low-risk residents, contracture, use of antianxiety and antidepressant medication. The lack of statistical significance in some measures may be due to the loss of statistical power, as a smaller number of observations switch from PE to non-PE or vice versa. However, the fact that most coefficient estimates are negative in direction suggests that PE firms does not provide lower quality to long-stay residents than non-PE NHs.

Robustness checks

In addition to our main results that suggest PE firms provide similar or potentially better quality than non-PE-owned NHs, we complete a number of robustness checks. These robustness checks are reported in Table 5 with Columns 1 and 8 reporting the baseline specifications from Table 4.

The first robustness check is to ensure the differential distance variable mitigates resident selection into NHs at the time of admission. Specifically, a resident that lived in a retirement community that offers independent and assisted living may have consciously considered NH quality when choosing to live in that particular retirement community (Grabowski et al. 2013; Bowblis and McHone 2013). This would invalid the exclusion restriction. To determine if this is a concern, we exclude observations where residents lived in the same zip code as the NHs they receive care (Columns 2 and 9). Restricting the sample to residents who live at NHs located in different zip codes from their previous home yields stronger and more consistent results.

Also relevant to the validity of the exclusion restriction, is the potential effect of excluding nonprofits from the study sample. While by definition PE ownership should only apply to for-profit NHs, excluding nonprofit NHs from a consumer's choice set may weaken the first stage of the 2SRI regressions. To address this concern, we repeat the analyses and include nonprofit NHs in the analytic sample. Though not reported, the results are consistent to our baseline findings. This is also consistent with prior work that studies the relationship of for-profit ownership structures and quality that found both the OLS and instrumental variable results are not sensitive to the including and excluding nonprofits in a consumer's choice set (Huang and Bowblis 2018).

Another set of robustness checks determines whether our results are sensitive to the selection of the comparison group. In one robustness check, we exclude independently operated NHs from the sample because our identification of PE ownership relies on whole-chain PE transactions (Column 3 and 10). Another robustness check focuses on the fact that there are a smaller number of residents in rural areas and differences by rurality could be driving some of the results. In this case, we restrict the sample to only urban NHs (Column 4 and 11). In both set of robustness checks, our main findings still hold both qualitatively and quantitatively.

Next, we focus on staffing variables. Because direct care nursing staff is one of the most important inputs in providing high quality care to long-stay NH residents, we are concerned that differences in staffing levels by PE ownership found in our summary statistics could be confounding the results. Thus, we run a specification that excludes staffing control variables (Column 5 and 12). We find nearly identical results, suggesting that nursing staff differences is unlikely contributing to the difference in quality.

We are also careful about different types of PE transactions and the divestiture of individual NHs. We compare those NHs always owned by PE firms to NHs that were never owned by PE firms during the study period (e.g. 2005–2010).[20] This comparison enables us to study the effect of PE ownership for a longer horizon, which is important because it could take time for corporate restructuring to have sizable effects on quality (Column 6). For this subsample, PE ownership lasts more than 6 years. Additionally, PE firms may divest individual NHs or chains if these facilities underperform or are not aligned with PE firms' strategic plan. If these NHs have worse quality on average, failing to account for these divestitures could lead to a finding that PE firms provide better quality. Thus, we run model specifications which modifies the definition of PE ownership from being contemporaneously owned by a

[20] The NHs that were always part of a PE firm were part of PE to PE transactions (i.e. Tandem and Trilogy).

Table 5 Specification checks of effect of private equity on long-stay quality

Long-stay quality measure	Regressions using 2SRI							Regressions using 2SRI w/fixed effects				
	Baseline	Different zip code	Chains only	Urban facilities	Exclude staffing	Always/never PE	Ever owned by PE	Baseline	Different zip code	Chains only	Urban facilities	Exclude staffing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Publicly reported quality measures												
Decline in physical functioning	0.110***	0.180***	0.070***	0.232***	0.113***	0.290***	0.095***	− 0.002	− 0.019	0.001	0.018	− 0.002
Catheter use	− 0.041***	− 0.077***	− 0.022**	− 0.047**	− 0.037***	− 0.130***	− 0.035***	− 0.016	− 0.025	− 0.016	− 0.010	− 0.016
Moderate-severe pain	− 0.023*	− 0.024	− 0.034***	− 0.023	− 0.024**	− 0.055*	− 0.020*	− 0.048***	− 0.062**	− 0.055***	− 0.067**	− 0.046***
Mostly bed or chairfast	− 0.005	− 0.009	0.007	− 0.043**	− 0.007	0.003	− 0.004	− 0.006	0.002	− 0.009	− 0.004	− 0.005
Bowel/bladder incontinence	− 0.067***	− 0.094**	− 0.088***	− 0.213***	− 0.060***	− 0.075	− 0.058***	− 0.029	− 0.004	− 0.035	− 0.085*	− 0.026
Physically restrained	0.017	− 0.001	0.025***	0.022	0.020*	0.038	0.015	0.023	0.005	0.029**	0.036	0.022
Urinary tract infection	− 0.012	− 0.017	− 0.013	− 0.006	− 0.018**	− 0.037	− 0.011	− 0.018	− 0.024	− 0.023**	− 0.035	− 0.016
Weight loss	− 0.026***	− 0.011	− 0.022***	− 0.012	− 0.026***	− 0.046**	− 0.022***	− 0.013	0.008	− 0.019**	− 0.007	− 0.012
Pressure ulcers (low risk resident)	− 0.026***	− 0.049***	− 0.022***	− 0.046***	− 0.029***	− 0.066***	− 0.023***	− 0.022**	− 0.050***	− 0.017**	− 0.041**	− 0.021**
Pressure ulcers (high risk resident)	− 0.081***	− 0.097***	− 0.074***	− 0.120***	− 0.081***	− 0.223***	− 0.070***	− 0.029	− 0.044	− 0.023	− 0.040	− 0.026
Falls with major injury	0.004	0.013	0.024***	0.038**	0.007	0.010	0.004	− 0.006	− 0.025	− 0.005	− 0.016	− 0.006

Table 5 continued

Long-stay quality measure	Regressions using 2SRI							Regressions using 2SRI w/fixed effects				
	Baseline	Different zip code	Chains only	Urban facilities	Exclude staffing	Always/never PE	Ever owned by PE	Baseline	Different zip code	Chains only	Urban facilities	Exclude staffing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Non-publicly reported quality measures												
Contractures	−0.242***	−0.367***	−0.244***	−0.462***	−0.245***	−0.420***	−0.209***	−0.173***	−0.229***	−0.148***	−0.288***	−0.164***
Rash	−0.045***	−0.092***	−0.021*	−0.144***	−0.049***	−0.170***	−0.039***	−0.019	−0.058*	−0.023	−0.059*	−0.017
Antipsychotic medication	0.046*	0.048	0.004	0.127***	0.044*	0.125**	0.040*	−0.032	−0.105*	−0.020	−0.029	−0.029
Antianxiety medication	0.060***	0.131***	0.045**	0.202***	0.059***	0.188***	0.052***	−0.083***	−0.153***	−0.100***	−0.155***	−0.078***
Antidepressant medication	0.208***	0.370***	0.097***	0.360***	0.200***	0.536***	0.180***	−0.092***	−0.128**	−0.099***	−0.161***	−0.087***
Hypnotic medication	−0.006	0.023	0.007	0.025	−0.015	−0.012	−0.005	0.001	0.030	−0.009	0.020	0.000

Regressions are estimated using 2SRI or 2SRI with facility-fixed effects which include controls reported in Table 1, year fixed effects, and indicators for potentially erroneous staffing levels using a unit of obseration of a resident assessment. For the catheter use and pain quality measures, additional controls are included based on CMS definitions of quality measures. Baseline regression results come from Table 3

***$p<0.01$, **$p<0.05$, *$p<0.1$

PE firm to ever owned by a PE firm between 2005 and 2010 (Column 7). These last two robustness checks are only available for the 2SRI approach that does not include NH fixed effects because the PE variable has no variation over time. Both robustness checks provide results consistent with the main findings.

As a final robustness check, we are concerned that some NH residents may have received care at NHs before and after PE transactions. Because these residents received care prior to the transaction, any assessments of these residents immediately after PE transactions may reflect the quality of care provided prior to PE transactions. Thus, we exclude all assessments of residents that occur within a window around PE transactions. These windows include 30, 90, and 180-day before and after the close date of the PE transaction. While not shown, we found that results are consistent with the baseline specification.

In summary, all robustness checks have similar signs and statistically significances. More importantly, results from the robustness checks are quantitatively and qualitatively similar to the baseline result and reinforce that our main findings are not subject to a particular model specification.

Alternative hypothesis: did PE firms cherry-pick NHs?

Overall, our results suggest that PE ownership does not lead to deterioration in quality when compared to for-profit NHs that are not owned by PE firms. Yet, an alternative hypothesis is that PE firms cherry-pick NHs chains that had superior quality prior to their acquisitions. If this is the case, PE firms do not really enhance quality, but instead just select the outperformers that have better quality than other for-profit NHs. Similarly, if PE NHs had mediocre quality prior to being acquired, mean revision can lead to better quality. Because all non-PE to PE transactions in our study occurred between 2006 and 2007, we use 2005 as a pre-transaction year. We then examine, among for-profit NHs in 2005 that were not contemporaneously owned by a PE firm, whether there are systematic differences in quality for NHs that were eventually acquired by a PE firm in the study period. Because there is no change of PE ownership in the pre-acquisition period and NH-fixed effects are not feasible, we only run OLS and standard 2SRI regressions. The results of these regressions are reported in Table 6.

Overall, based on OLS results, we do not find consistent quality differences in the pre-acquisition period. Only 5 of 17 measures (three negative and two positive) are statistically significant and the remaining measures have mixed directions of the effect. When 2SRI is utilized, the coefficient estimates for those eventually acquired by a PE firm are negative for 13 of 17 quality measures and statistically significant at conventional levels for 6 measures. Of the four quality measures that are positive, three are statistically significant, with three of these measures being the use of various psychoactive medications. Synthesizing all the results, NH chains owned by PE firms had better or at least similar publicly reported quality as other for-profit NHs, and after acquisition, PE ownership does not lead to deterioration of quality.

Limitations

Our robustness checks find consistent results across a number of various specifications though we acknowledge a few limitations. Foremost, our statistical method addresses resident selection and finds that PE NHs have similar or potentially better long-stay quality than other for-profit NHs. While we find this result for clinical quality measures that can be calculated

Table 6 Quality differences for NHs acquired by PE firms in pre-acquisition period (2005)

Long-stay quality measure	OLS	2SRI
Publicly reported quality measures		
Decline in physical functioning	– 0.001	0.31***
(N = 79,754)	(0.005)	(0.065)
Catheter use	– 0.003	– 0.136*
(N = 88,206)	(0.005)	(0.076)
Moderate-severe pain	0.025***	– 0.124
(N = 88,171)	(0.007)	(0.089)
Mostly bed or chairfast	– 0.004	– 0.019
(N = 90.179)	(0.003)	(0.055)
Bowel/bladder incontinence	– 0.039***	– 0.239
(N = 67,426)	(0.009)	(0.138)
Physically restrained	– 0.003	– 0.103
(N = 90,200)	(0.004)	(0.074)
Urinary tract infection	– 0.010**	– 0.037
(N = 90,201)	(0.005)	(0.068)
Weight loss	0.005	– 0.142**
(N = 86,934)	(0.004)	(0.061)
Pressure ulcers (low risk resident)	0.007	– 0.107**
(N = 37,259)	(0.004)	(0.051)
Pressure ulcers (high risk resident)	– 0.003	– 0.381***
(N = 52,925)	(0.007)	(0.105)
Falls with major injury	0.007	– 0.073
(N = 90,197)	(0.005)	(0.072)
Non-publicly reported quality measures		
Contractures	– 0.046***	– 0.697***
(N = 90,200)	(0.010)	(0.142)
Rash	0.000	– 0.244***
(N = 90,200)	(0.006)	(0.087)
Antipsychotic medication	0.030***	0.211
(N = 85,675)	(0.009)	(0.137)
Antianxiety medication	0.011	0.622***
(N = 90,199)	(0.008)	(0.120)
Antidepressant medication	0.008	0.646***
(N = 90,200)	(0.011)	(0.153)
Hypnotic medication	0.005	– 0.070
(N = 90,200)	(0.005)	(0.069)

Regressions are restricted to non-PE owned for-profit NHs in 2005. The reported effects are for the NH was ever owned by a PE firm during the study period. Regressions include controls reported in Table 1, year fixed effects, and indicators for potentially erroneous staffing levels using an observation of a resident assessment. For the catheter use and pain quality measures, additional controls are included based on CMS definitions of quality measures. Standard errors are clustered at the resident level. Sample sizes vary with quality measure because of how CMS defines the inclusion criteria for each quality measure

***$p<0.01$, **$p<0.05$, *$p<0.1$

from the MDS, NH quality is multidimensional and these results may not hold for other measures of quality, such as resident and family satisfaction. Furthermore, our statistical method does not allow us to test the mechanisms that lead to any quality difference, therefore further research is needed to understand what drives these differences.

Our analysis is also limited by being restricted to the state of Ohio. Although we identify regional and national chains that were acquired by PE firms, it is still possible that PE firms own different types of NHs in other states. We use two PE-to-PE chain transactions to study the longer-term effect of PE ownership, but the majority of our statistical identification relies on non-PE-to-PE chain transactions and only have three to four years post-acquisition. Therefore, we are not able to assess the complete long-term PE effects, especially after their divestiture. It is estimated that about 23% of large public-to-private PE transactions across all industries in the 1980s went bankruptcy at some point (Andrade and Kaplan 1998) and this has come to fruition in the NH industry. In March of 2018, HCR ManorCare, one of the largest PE acquisition of NHs, filed for bankruptcy with $7.1 billion of debt on its balance sheet (Rucinski 2018). This recent bankruptcy highlights the need for longer-term evaluations and better understanding how PE-owned NHs weather the business and regulatory adversaries.

Conclusion

Prior studies have argued that for-profit NHs have the ability and incentive to take advantage of residents by skimping on quality in order to increase profits (Chou 2002). Given this incentive, the wave of high-profile PE acquisitions in 2000s have raised concerns from research and advocacy communities that NH residents may be exploited by PE firms. These acquisitions are often financed through large amounts of debt and PE firms expect to sell the target companies within a few years. Comparing to traditional for-profit NHs, PE ownership creates stronger incentives and pressures to skimp on quality for higher short-term profit. In addition, PE firms often acquire majority stakes of target companies and institute a smaller governing board. The concentrated ownership to fewer shareholders and smaller governing board create substantial control power to swiftly restructure the acquired companies. Combining stronger profit motives and more powerful corporate control, when incentives are not aligned with residents' interests, PE ownership theoretically can significantly lower NH quality and hurt vulnerable residents.

However, through rigorous statistical analysis, we find such concern is not consistent with the empirical evidence, at least in the short and medium timeframe. While we find suggestive evidence that PE firms acquire NH chains that had better quality, after adjusting for resident selection using 2SRI, we find that quality among long-stay residents in PE NHs is generally similar, and in some cases may be better than other for-profit NHs. These results together provide evidence that PE ownership does not deteriorate NH quality. Our findings are consistent even when we impose additional facility or market restrictions in the robustness analysis. Despite a growing and significant role in healthcare markets, our knowledge of PE ownership in healthcare firms is still very limited and many questions remain unanswered. For example, this paper focuses on long-stay residents who need chronic care. It is not clear if these results directly apply to post-acute care patients who focus on regaining functioning and returning home. Furthermore, if PE firms do not lower quality to enhance profitability, do they instead target premium consumers at the high-end markets? Or, do PE firms more aggressively engage in upcoding to increase reimbursements? In the era of tightening public funding, further examination of PE ownership on pricing and billing practice can help to more comprehensively evaluate the consequences of PE ownership in the healthcare sector.

Appendix A: information of PE acquisitions

HCR Manor Care

Through a $6.3 billion buyout ($5.5 billion borrowed), the private-equity firm, Carlyle group, acquired Manor Care in 2007 (The Deal 2007), and the transaction was completed in December 2007. At the time of transaction, Manor Care was a publicly traded company that employed more than 60,000 workers.

Harborside/Sun

In 1998, Harborside was purchased by a private-equity firm, Investcorp. Harborside was later sold to Sun, a public traded nursing home chain in 2006 for $625 million, which included $350 million in cash and $275 million in debt (The Deal 2006).

Laurel Health Care

In February 2006, Formation Capital Health Care bought Ohio-based Laurel Health Care for "nearly $200 million," as reported in July 2006 in The Senior Care Investor newsletter. Laurel Health Care had 26 facilities and 2736 beds. Four months later, Formation sold Laurel Health Care along with five other senior housing groups, totaling 186 facilities and 21,000 beds, to GE Healthcare Financial Services for $1.4 billion, according to a GE news release (GRBJ 2007).

Tandem Health Care

Behrman Capital, a private equity firm, sold Tandem Health Care to other private equity firms (JER Partners and Formation Capital) in July 2006. The deal was valued at $620 million.

http://www.behrmancap.com/behrman-capital-sells-tandem-health-care-to-jer-partners-and-formation-capital-in-620-million-transaction/.

Trilogy Health Services

A Swiss private-equity firm, Lydian Capital, paid $350 millions to purchase Trilogy Health Services in 2007 from a Chicago-based private-equity frim, Frontenac. At the time of the transaction, Trilogy employed more than 5100 workers at 44 long-term care facilities in Ohio, Kentucky, Michigan, and Indiana (Irish Independent 2007). In 2015, Lydian sold Trilogy to Griffin American Healthcare for $1.12 billion (Sunday Business Post 2015).

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RESEARCH LETTER

Private Equity Acquisitions of Physician Medical Groups Across Specialties, 2013-2016

Acquisition of physician practices by private equity firms has accelerated,[1,2] with unknown implications for care delivery and patient outcomes. However, available data are limited to single specialties or come from industry reports or opinion articles. A dearth of evidence and the use of nondisclosure agreements at early stages of negotiation have constrained the ability to evaluate this phenomenon empirically.[3] In this study, we describe physician group practices acquired in 2013-2016 across specialties.

Methods | We identified US physician group practice acquisitions by private equity firms using the Irving Levin Associates Health Care M&A data set,[4] which includes manually collected and verified transactional information on a broad set of health care mergers and acquisitions. We excluded practices bought by entities not classified as private equity firms at the time of acquisition. We verified practice names, locations, specialties, and group practice status via Google searches.

We linked acquisitions to the SK&A data set, a commercial data set of verified physician- and practice-level characteristics (eg, specialty, credentials, practice ownership, size, and locations) for US office-based practices. Transactions that spanned multiple sites and distinct practice names were considered separate acquisitions. Otherwise, we aggregated all practice sites observed in the SK&A data set and matched these to 1 observation from the M&A data set.

Linkages involved (1) fuzzy matching for nonexact records of a practice name in the SK&A data set with reported acquisitions in the M&A data set and (2) manual searches for nonmatches to identify name changes using publicly available records and practice websites. Within practices, we excluded physicians with primary administrative roles.

Match rates between practices in the SK&A data set and the M&A data set were 87% in 2013, 82% in 2014, 90% in 2015, and 87% in 2016. We benchmarked estimates against industry reports to ascertain data quality and integrity, and described numbers of practices, sites, and physicians in acquired practices across specialties.

Results | Of approximately 18 000 unique group medical practices, there were 355 physician practice acquisitions (1426 sites and 5714 physicians) by private equity firms from 2013 to 2016, increasing from 59 practices in 2013 to 136 practices in 2016 (**Table 1**). Acquired practices had a mean of 4.0 sites, 16.3 physicians in each practice, and 6.2 physicians affiliated with each site. Overall, 81.4% of these medical practices reported accepting new patients, 83.4% accepted Medicare, and 60.3% accepted Medicaid. The majority of acquired practices were in the South (43.9%).

Table 1. Characteristics of Physician Medical Groups Acquired by Private Equity Groups, 2013-2016 (N = 355)

Characteristic	Total	Year of Acquisition 2013	2014	2015	2016
Acquired by private equity group					
No. of practices	355	59	72	88	136
No. of sites	1426	216	308	386	516
No. of physicians[a]	5714	843	1413	1576	1882
Physicians per practice, mean (SD) [median]	16.3 (26.3) [7]				
Sites per practice, mean (SD) [median]	4.0 (7.8) [1]				
Physicians per site, mean (SD) [median]	6.2 (12.7) [2]				
Practice accepts new patients, No. (%)	289 (81.4)				
Practice accepts Medicare, No. (%)	296 (83.4)				
Practice accepts Medicaid, No. (%)	214 (60.3)				
Location of practice by US region, No. (%)[b]					
South	184 (43.9)				
Midwest	90 (21.5)				
Northeast	69 (16.5)				
West	76 (18.1)				

[a] Each physician was associated with only 1 practice but may have been affiliated with multiple sites within a practice.

[b] Some acquisitions spanned multiple US regions and are counted more than once; therefore, the total is 419 for this characteristic instead of 355.

Table 2. Specialties of Medical Groups and Physicians Among Those Acquired by Private Equity Firms, 2013-2016

	Specialty Practices					Specialty Physicians				
		Year of Acquisition[b]					Year of Acquisition[b]			
Specialty (Specialist Description)	Total, No. (%)[a]	2013	2014	2015	2016	Total, No. (%)[a]	2013	2014	2015	2016
Total	355 (100)	59	72	88	136	5714 (100)	843	1413	1576	1882
Anesthesiology (anesthesiologist)	69 (19.4)	10	20	15	24	1894 (33.1)	246	593	458	597
EM (emergency physician)	43 (12.1)	10	6	10	17	901 (15.8)	150	184	148	419
Family practice (family practitioner)	39 (11.0)	7	9	6	17	515 (9.0)	90	123	164	138
Dermatology (dermatologist)	35 (9.9)	1	5	11	18	334 (5.8)	11	26	86	211
Pediatrics (pediatrician)	20 (5.6)	4	8	5	3	166 (2.9)	9	61	57	39
Internal medicine (internist)	12 (3.4)	2	5	2	3	365 (6.4)	64	183	79	39
Ophthalmology (ophthalmologist)	11 (3.1)	0	2	2	7	134 (2.3)	6	35	68	25
Radiology (radiologist)	8 (2.3)	0	0	2	6	252 (4.4)	4	13	159	76
Urology (urologist)	8 (2.3)	5	1	1	1	92 (1.6)	13	37	37	5
Gastroenterology (gastroenterologist)	8 (2.3)	0	0	6	2	82 (1.4)	4	4	48	26
Cardiology (cardiologist)	8 (2.3)	1	0	1	6	106 (1.9)	32	14	28	33
Obstetrics/gynecology (obstetrician/gynecologist)	7 (2.0)	0	0	2	5	83 (1.5)	8	14	28	33
Hematology/oncology (hematologist/oncologist)	5 (1.4)	2	1	2	0	86 (1.5)	29	9	44	4
Orthopedic surgery (orthopedic surgeon)	5 (1.4)	0	0	2	3	130 (2.3)	0	13	43	74
Otolaryngology (otolaryngologist)	3 (0.8)	0	0	1	2	13 (0.2)	0	0	4	9
Nephrology (nephrologist)	2 (0.5)	0	0	0	2	19 (0.3)	0	7	2	10
Neurology (neurologist)	1 (0.3)	0	0	0	1	55 (1.0)	11	19	5	20
Psychiatry (psychiatrist)	1 (0.3)	0	0	1	0	22 (0.4)	5	2	2	13
Pulmonology (pulmonologist)	1 (0.3)	1	0	0	0	31 (0.5)	13	6	5	7
Pathology (pathologist)	1 (0.3)	1	0	0	0	23 (0.4)	15	2	3	3
Multispecialty	68 (19.4)	15	15	19	19					
Other types of specialty physicians										
Urgent care specialist						124 (2.2)	41	16	32	35
Neonatologist						79 (1.4)	44	25	10	0
Physical medicine/rehabilitation specialist						30 (0.5)	10	6	7	7
General surgeon						36 (0.6)	9	4	6	17
Radiation oncologist						26 (0.5)	2	2	10	12
Endocrinologist						17 (0.3)	1	2	10	4
Allergist/immunologist						10 (0.2)	1	5	3	1
Rheumatologist						15 (0.3)	2	3	3	7
Other specialist						74 (1.3)	23	7	34	10

Abbreviation: EM, emergency medicine.

[a] The percentages represent the proportion of total acquisitions across all years.

[b] Data are expressed as total numbers for each year.

The most commonly represented medical groups included anesthesiology (19.4%), multispecialty (19.4%), emergency medicine (12.1%), family practice (11.0%), and dermatology (9.9%) (**Table 2**). From 2015 to 2016, there was also an increase in the number of acquired cardiology, ophthalmology, radiology, and obstetrics/gynecology practices.

Within acquired practices, anesthesiologists represented 33.1% of all physicians; emergency medicine specialists, 15.8%; family practitioners, 9.0%; and dermatologists, 5.8%.

Discussion | Private equity acquisitions of physician practices increased across specialties from 2013 to 2016 but still constituted a small proportion of group physician practices in the United States. Industry reports suggest further growth in 2017-2018 private equity acquisitions, particularly in ophthalmology, dermatology, urology, orthopedics, and gastroenterology.[5] These data, which show acquired practices to have several sites and many physicians, match private equity firms' typical investment strategy of acquiring "platform" practices with large community footprints and then growing value by recruiting additional physicians, acquiring smaller groups, and expanding market reach.

Research is needed to understand the effect of these acquisitions and to mitigate unintended consequences. Private equity firms expect greater than 20% annual returns,[3] and these financial incentives may conflict with the need for longer-term investments in practice stability, physician recruitment, quality, and safety. There may be additional pressures to increase revenue streams (eg, elective procedures and ancillary services), direct more referrals internally, and rely on lower-cost clinicians.[6]

Key limitations include that the data are based on publicly announced transactions and therefore underestimate total acquisitions, particularly of smaller practices, and that available data lag behind the rapid pace of private equity acquisitions.

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COMMENT & RESPONSE

Unmeasured Confounding in Observational Studies of Management of Cerebellar Intracranial Hemorrhage

To the Editor In a propensity score–matched cohort of 578 patients from 4 observational cohort studies, Dr Kuramatsu and colleagues showed that evacuation of medium-sized intracerebellar hematomas (approximate volume, 20 cm^3) was not associated with better functional outcome.[1] Assessing treatment effectiveness in observational data is challenging because treatment decisions are based on patient characteristics that also are typically predictive of outcome, causing confounding by indication. Although the authors addressed this potential bias with propensity scores, we would like to emphasize the possibility of residual confounding.

In their study, surgically treated patients were younger, had worse Glasgow Coma Scale scores at presentation, had larger hematomas, and more often had intraventricular hemorrhage. In matching patients with the same risk of undergoing a surgical evacuation (the propensity), the authors suggested that treatment groups with similar prognosis were created. However, while measured confounding seems to have been properly addressed, unmeasured confounding may still be a problem. Many factors may influence decision-making in these patients, including frailty and preexisting conditions that could be contraindications for surgery. Contexts with strong measured confounding are also likely to show substantial unmeasured confounding. Propensity score matching is a statistically efficient alternative for regression-based covariate adjustment but still relies on the assumption that no unmeasured treatment preferences strongly relate to prognosis.[2,3]

A methodological study on comparable treatment considerations found that unmeasured confounding is not merely a theoretical problem.[3] In post hoc analyses of traumatic brain injury cohorts, analytical methods for surgery in traumatic intracranial hematomas and intracranial pressure–guided treatment were compared; propensity score matching was unable to account for unmeasured imbalances between treatment groups. A simulation study confirmed that propensity score matching resulted in an invalid estimate of the treatment effect in the case of unmeasured confounding,[3] which also was shown in other fields.[4]

Our view is that unmeasured confounding is an insurmountable problem in observational studies of acute neurosurgical decisions. A promising alternative for effect estimation is instrumental variable analysis. Although this method has its own difficulties, such as defining appropriate instruments and the necessity of large samples, it is not biased by unmeasured confounding.[3,5] Since the cohort in the study by Kuramatsu and colleagues came from 64 centers with likely differing practice culture among institutions, have the authors considered a regional comparison of treatment strategies?

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Original Investigation | Health Policy

Comparative Performance of Private Equity–Owned US Nursing Homes During the COVID-19 Pandemic

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Abstract

IMPORTANCE It is not known whether nursing homes with private equity (PE) ownership have performed better or worse than other nursing homes during the coronavirus disease 2019 (COVID-19) pandemic.

OBJECTIVE To evaluate the comparative performance of PE-owned nursing homes on COVID-19 outcomes.

DESIGN, SETTING, AND PARTICIPANTS This cross-sectional study of 11 470 US nursing homes used the Nursing Home COVID-19 Public File from May 17, 2020, to July 2, 2020, to compare outcomes of PE-owned nursing homes with for-profit, nonprofit, and government-owned homes, adjusting for facility characteristics.

EXPOSURE Nursing home ownership status.

MAIN OUTCOMES AND MEASURES Self-reported number of COVID-19 cases and deaths and deaths by any cause per 1000 residents; possessing 1-week supplies of personal protective equipment (PPE); staffing shortages.

RESULTS Of 11 470 nursing homes, 7793 (67.9%) were for-profit; 2523 (22.0%), nonprofit; 511 (5.3%), government-owned; and 543 (4.7%), PE-owned; with mean (SD) COVID-19 cases per 1000 residents of 88.3 [2.1], 67.0 [3.8], 39.8 [7.6] and 110.8 [8.1], respectively. Mean (SD) COVID-19 deaths per 1000 residents were 61.9 [1.6], 66.4 [3.0], 56.2 [7.3], and 78.9 [5.9], respectively; mean deaths by any cause per 1000 residents were 78.1 [1.3], 91.5 [2.2], 67.6 [4.5], and 87.9 [4.8], respectively. In adjusted analyses, government-owned homes had 35.5 (95% CI, −69.2 to −1.8; *P* = .03) fewer COVID-19 cases per 1000 residents than PE-owned nursing homes. Cases in PE-owned nursing homes were not statistically different compared with for-profit and nonprofit facilities; nor were there statistically significant differences in COVID-19 deaths or deaths by any cause between PE-owned nursing homes and for-profit, nonprofit, and government-owned facilities. For-profit, nonprofit, and government-owned nursing homes were 10.5% (9.1 percentage points; 95% CI, 1.8 to 16.3 percentage points; *P* = .006), 15.0% (13.0 percentage points; 95% CI, 5.5 to 20.6 percentage points; *P* < .001), and 17.0% (14.8 percentage points; 95% CI, 6.5 to 23.0 percentage points; *P* < .001), respectively, more likely to have at least a 1-week supply of N95 masks than PE-owned nursing homes. They were 24.3% (21.3 percentage points; 95% CI, 11.8 to 30.8 percentage points; *P* < .001), 30.7% (27.0 percentage points; 95% CI, 17.7 to 36.2 percentage points; *P* < .001), and 29.2% (25.7 percentage points; 95% CI, 16.1 to 35.3 percentage points; *P* < .001) more likely to have a 1-week supply of medical gowns than PE-owned nursing homes. Government nursing homes were more likely to have a shortage of nurses (6.9 percentage points; 95% CI, 0.0 to 13.9 percentage points; *P* = .049) than PE-owned nursing homes.

(continued)

Key Points

Question Compared with other nursing homes, are private equity (PE)–owned nursing homes associated with better or worse coronavirus disease 2019 (COVID-19) outcomes?

Findings In this cross-sectional study of 11 470 US nursing homes, there were no statistically significant differences in staffing levels, COVID-19 cases or deaths, or deaths from any cause between PE nursing homes and facilities with other ownership types. Compared with PE, all other ownership types were more likely to have at least a 1-week supply of N95 masks and medical gowns.

Meaning In this study, PE-owned nursing homes performed comparably with for-profit and nonprofit nursing homes based on COVID-19 cases and deaths and deaths by any cause but had less personal protective equipment than other nursing homes.

+ Supplemental content

Author affiliations and article information are listed at the end of this article.

Abstract (continued)

CONCLUSIONS AND RELEVANCE In this cross-sectional study, PE-owned nursing homes performed comparably on staffing levels, resident cases, and deaths with nursing homes with other types of ownership, although their shortages of PPE may warrant monitoring.

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Introduction

Nursing homes have been disproportionately affected by the coronavirus disease 2019 (COVID-19) pandemic. These facilities provide care to some of the nation's most at-risk patients, including individuals receiving postacute care and long-stay residents requiring 24-hour custodial care, most of whom are older adults. Although there are only 1.3 million patients in US nursing homes,[1] representing less than 0.4% of the country's population,[2] 43% of all COVID-19 deaths have been attributed to these facilities.[3]

For more than 2 decades, private equity (PE) firms have been acquiring nursing homes.[4] This trend has raised concerns among policy makers regarding the quality of care provided by facilities owned by these firms.[5] The disproportionate impact of the COVID-19 pandemic on nursing homes has amplified these concerns.[6]

PE firms typically make acquisitions with the expectation of high short-term returns on investment of 20% or more annually.[7] Opponents of PE ownership of nursing homes suggest that these firms will prioritize profit over patient care because of pressure to increase the returns of investors.[8] Unlike for-profit nursing homes that may have longer-term business plans, PE ownership may have little experience in nursing home care and may focus on selling acquired facilities within a short time, typically 3 to 5 years.[9] Furthermore, PE firms often use leveraged buyouts for acquisitions, and the nursing homes are responsible for payments on the loans that PE firms use to acquire them.[10] The targeting of short time frames before selling nursing homes for large returns on investment combined with large amounts of debt might lead PE-owned nursing homes to implement cost-cutting practices, such as reducing staffing levels. Staffing is the largest expenditure for nursing homes, accounting for approximately half the cost of providing care,[11] making reduced staffing levels an attractive strategy to increase profits. Lower staffing levels in nursing homes have been associated with poorer quality care.[12-14] PE-owned nursing homes might also attempt to attract more profitable patients, such as postacute patients covered by Medicare, which has high reimbursements compared with those for Medicaid patients, ie, for custodial care.[15]

Proponents of PE argue that these firms can bring management expertise that improves the quality and efficiency of nursing home care, eg, through better workforce management. Similarly, PE firms can bring capital to improve nursing homes' health information technology (IT) infrastructure, an area in which they have lagged behind other health care professionals, facilities, and systems.[16] Patients in nursing homes are often exposed to fragmented and poorly coordinated care,[17,18] which can potentially be improved by increasing health IT capabilities. Moreover, proponents note that the nursing home industry has a history of poor regulatory compliance[19] and that PE firms can use their management expertise, health IT, and legal resources to improve compliance.

Prior studies examining the association of PE ownership with nursing home quality and staffing have had mixed results.[3,9,15,20-23] To our knowledge, no study to date has analyzed the comparative performance of PE-owned nursing homes during the COVID-19 pandemic. We used a nationwide sample to compare COVID-19 cases and deaths among patients in PE nursing homes with those among patients in nonprofit, government-owned, and (non-PE) for-profit nursing homes. We also evaluated the likelihood of having personal protection equipment (PPE) and staffing shortages based on ownership.

Methods

The study was determined to be exempt by the institutional review board of Weill Cornell Medical College because it did not involve human participants. Therefore, informed consent was not required. This study followed the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) reporting guideline.

Study Data and Participants

We identified nursing home acquisitions by PE firms between 2010 and 2020 using the S&P Capital IQ, Irving Levin Associates Health Care M&A, and Centers for Medicare & Medicaid Services (CMS) Nursing Home Compare Ownership databases. The S&P and Irving Levin databases report acquisition details that include acquisition announcement date, name of the acquired nursing home, the platform nursing home that acquired the nursing home, and the PE firm that owns the nursing home. The Nursing Home Compare database provides a CMS Certification Number (CCN), facility name, facility address, owner name, and the date on which ownership began.

Acquisitions by PE firms were identified in 2 ways. First, we confirmed PE acquisitions in the S&P and Irving Levin databases by manually reviewing each acquirers' company profile using CB Insights, Bloomberg Businessweek, Pitchbook, and web-based searches to see whether they were a PE firm or PE-backed platform nursing home (eAppendix in the Supplement). Second, we used keyword searches in the Nursing Home Compare database to identify PE firms and PE-backed platform nursing homes that were not in the S&P and Irving Levin databases. The identified acquisitions were then manually matched to CMS Provider of Services data using nursing home name, address, and location to obtain the CCN. More details on the identification of PE acquisitions, in addition to the geographic distribution of acquisitions, are provided in the eAppendix and eTable 1 in the Supplement, respectively.

Using nursing home CCNs, we merged 2 additional data sources. First, we merged the acquisition database with the 2017 Long-term Care: Facts on Care in the US (LTCFocus) database to obtain nursing home patient and facility characteristics. Second, we merged the CMS Nursing Home COVID-19 Public File as of July 2, 2020, to obtain COVID-19–related measures. Nursing homes were required to begin reporting cumulative cases and deaths for the database beginning May 17, 2020, with weekly updates thereafter. We excluded hospital-based nursing homes, nursing homes with incomplete patient or facility information, and those that did not pass the US Centers for Disease Control and Prevention (CDC) quality assurance check.[24] Data quality assurance checks were performed by CMS on 8 data fields. If values in the data fields were implausible (eg, the number of deaths was implausibly high compared with the number of beds in the facility), they were flagged as not passing the quality assurance check. Unadjusted COVID-19 outcomes for nursing homes that did not pass the CDC quality assurance check appear in eTable 2 in the Supplement. The final sample included 11 470 nursing homes (eFigure in the Supplement).

Study Variables

Outcome Measures

From the CMS Nursing Home COVID-19 Public File, our outcomes included self-reported measures of COVID-19 cases and deaths and deaths by any cause; 5 measures of nursing home PPE supplies; and 4 measures of nursing home staffing. We measured total resident COVID-19 confirmed cases, total resident COVID-19 deaths, and deaths by any cause per 1000 residents. Total resident COVID-19 deaths were defined as residents with suspected or laboratory-positive COVID-19 tests who died in the nursing home or another location. Nursing homes that had a COVID-19 death must have reported at least 1 COVID-19 confirmed case. For PPE supplies, we created separate dichotomous measures for whether a nursing home reported at least a 1-week supply of N95 masks, eye protection, medical gowns, gloves, and hand sanitizer. For staffing, we created dichotomous measures for whether a nursing home reported having shortages of nursing, clinical, aid, or other personnel.

Covariates

From LTCFocus, the covariates used in our adjusted analyses included nursing home characteristics (ie, mean resident age, percentage women, occupancy rate, mean activities of daily living score, multifacility chain membership), including terciles of the distributions of the percentage of patients covered by Medicaid, percentage covered by Medicare, percentage of White residents, and the total number of beds. An indicator of rural location was derived from the US Department of Agriculture Rural-Urban Commuting Areas database.[25]

Statistical Analysis

We conducted 3 analyses. First, we made unadjusted comparisons of facility characteristics by category of ownership using 1-way analysis of variance for continuous measures and χ^2 tests for categorical measures (**Table 1**). Second, we made unadjusted comparisons for each outcome measure among other nursing home ownership categories compared with PE-owned nursing homes using *t* tests for continuous measures and proportional tests for dichotomous measures (**Table 2**). Third, we conducted the same comparisons while adjusting for the covariates described earlier using linear regression models. In all analyses, comparisons were made of nursing homes in the same Hospital Referral Region (HRRs) by including HRR fixed effects. Relative differences were derived by dividing estimates for continuous measures by the unadjusted mean of the outcome across all ownership types and by dividing estimates for dichotomous measures by the unadjusted proportions across all ownership types (eTable 3 in the Supplement). Standard errors were adjusted for clustering at the HRR level, and Bonferroni correction was used to adjust for multiple comparisons. All continuous outcome measures were winsorized at the top 1% of the distribution to exclude outliers.

Table 1. Characteristics of Nursing Homes by Ownership

Variable[a]	Nursing homes, No. (%)				
	For-profit, excluding private equity (n = 7793)	Private equity-owned (n = 543)	Nonprofit (n = 2523)	Government-owned (n = 611)	*P* value[b]
Nursing home characteristics					
Total beds, tercile					
Lowest	2260 (29.0)	161 (29.7)	1169 (46.3)	233 (38.1)	<.001
Middle	3095 (39.7)	228 (42.0)	722 (28.6)	162 (26.5)	
Highest	2438 (31.3)	154 (28.4)	632 (25.1)	216 (35.4)	
Occupancy rate, mean (SD), %	72.6 (15.3)	74.6 (15.1)	75.7 (14.3)	72.3 (15.7)	<.001
ADL score, mean (SD)[c]	16.6 (2.7)	17.1 (1.8)	17.0 (2.3)	16.0 (2.6)	<.001
Multifacility chain membership	4855 (62.3)	497 (91.5)	1190 (47.2)	214 (35.0)	<.001
Rural	390 (5.0)	26 (4.8)	255 (10.1)	110 (18.0)	<.001
Patient characteristics					
Patient age, mean (SD), y	78.1 (6.4)	78.1 (5.3)	83.9 (6.7)	80.1 (5.9)	<.001
Women residents, mean (SD), %	64.8 (11.6)	65.2 (10.0)	71.9 (10.9)	64.0 (16.9)	<.001
Percentage White residents, tercile					
Lowest	3131 (40.2)	197 (36.3)	397 (15.7)	139 (22.8)	<.001
Middle	2697 (34.6)	203 (37.4)	731 (29.0)	182 (29.8)	
Highest	1965 (25.2)	143 (26.3)	1395 (55.3)	290 (47.5)	
Percentage Medicare residents, tercile					
Lowest	2510 (32.2)	118 (21.7)	857 (34.0)	298 (48.8)	<.001
Middle	2621 (33.6)	202 (37.2)	823 (32.6)	208 (34.0)	
Highest	2662 (34.2)	223 (41.1)	843 (33.4)	105 (17.2)	
Percentage Medicaid residents, tercile					
Lowest	2087 (26.8)	134 (24.7)	1480 (58.7)	181 (29.6)	<.001
Middle	2736 (35.1)	198 (36.5)	657 (26.0)	207 (33.9)	
Highest	2970 (38.1)	211 (38.9)	386 (15.3)	223 (36.5)	

Abbreviation: ADL, activities of daily living.

[a] Facility and patient characteristics data are from the 2017 Long-term Care: Facts on Care in the US and the US Department of Agriculture Rural-Urban Commuting Areas database.

[b] Unadjusted comparisons of facility and patient characteristics by ownership category were made using 1-way analysis of variance for continuous measures and χ^2 tests for categorical measures.

[c] The ADL score ranges from 0 to 28, based on a score of 0 to 4 on 7 different ADLs, with 0 indicating completely independent and 28, completely dependent.

In sensitivity analyses, we addressed the skewness of our data by using Poisson regressions for continuous outcome measures and logistic regressions for dichotomous measures. Statistical analysis was conducted in Stata version 16.0 (StataCorp). Statistical significance was set at $P < .05$, and all tests were 2-tailed.

Results

Nursing Home Characteristics by Ownership

Table 1 presents unadjusted nursing home and patient characteristics by 4 categories of ownership: for-profit (non-PE), nonprofit, government-owned, and PE-owned. Of the 11 470 nursing homes in our sample, 7793 (67.9%) were for-profit, 2523 (22.0%) were nonprofit, 611 (5.3%) were government-owned, and 543 (4.7%) were PE-owned. Nursing home characteristics were generally similar across ownership categories. However, 497 PE-owned nursing homes (91.5%) were part of multifacility chains compared with 4855 for-profit nursing homes (62.3%), 1190 nonprofit nursing homes (47.2%), and 214 government-owned nursing homes (35.0%). Patient characteristics were similar across ownership types, although more PE-owned nursing homes were in the highest tercile of percentage of patients with Medicare: 223 PE-owned nursing homes (41.1%) vs 2662 for-profit nursing homes (34.2%), 843 nonprofit nursing homes (33.4%), and 105 government-owned nursing homes (17.2%).

Unadjusted Differences in Outcomes by Ownership

In unadjusted analyses (Table 2) for mean (SD) number of confirmed COVID-19 cases per 1000 residents, PE-owned nursing homes had the highest number (110.8 [8.1]) compared with for-profit (88.3 [2.1]; $P = .007$), nonprofit (67.0 [3.8]; $P < .001$), and government-owned (39.8 [7.6]; $P < .001$). For mean (SD) number of confirmed COVID-19 deaths per 1000 residents, PE-owned nursing homes also had the highest number (78.9 [5.9]) compared with for-profit (61.9 [1.6]; $P = .006$), nonprofit

Table 2. Unadjusted COVID-19 Outcomes by Ownership

Outcome[a]	% (SD) Private equity–owned (n = 543)	% (SD) For-profit, excluding private equity (n = 7793)	*P* value[b]	Nonprofit, % (SD), (n = 2523)	*P* value[b]	Government, % (SD), (n = 611)	*P* value[b]
Resident COVID-19 measures, mean (SD), per 1000 residents							
Confirmed COVID-19 cases	110.8 (8.1)	88.3 (2.1)	.007	67.0 (3.8)	<.001	39.8 (7.6)	<.001
COVID-19 deaths	78.9 (5.9)	61.9 (1.6)	.006	66.4 (3.0)	.06	56.2 (7.3)	.02
All deaths	87.9 (4.8)	78.1 (1.3)	.047	91.5 (2.2)	.50	67.6 (4.5)	.002
PPE supply measures							
1-wk supply of N95 masks	76.8 (1.5)	86.2 (0.4)	<.001	89.1 (0.7)	<.001	91.8 (1.4)	<.001
1-wk supply of surgical masks	94.1 (1.0)	93.0 (0.3)	.30	95.5 (0.5)	.23	96.2 (1.0)	.14
1-wk supply of eye protection	93.2 (1.0)	93.4 (0.3)	.87	95.8 (0.5)	.02	94.9 (1.0)	.22
1-wk supply of medical gowns	64.3 (1.4)	88.0 (0.4)	<.001	91.4 (0.6)	<.001	91.7 (1.3)	<.001
1-wk supply of gloves	94.3 (0.8)	95.7 (0.2)	.10	97.4 (0.3)	.001	97.9 (0.7)	.002
1-wk supply of hand sanitizer	93.9 (0.9)	94.8 (0.2)	.38	96.5 (0.4)	.01	96.7 (0.9)	.03
Staff shortage measures							
Shortage of nursing staff	10.9 (1.5)	15.5 (0.4)	.003	13.6 (0.7)	.11	20.3 (1.4)	<.001
Shortage of clinical staff	2.0 (0.7)	2.9 (0.1)	.25	2.6 (0.3)	.45	3.0 (0.6)	.34
Shortage of aides	13.3 (1.6)	18.1 (0.4)	.004	16.0 (0.8)	.13	21.6 (1.5)	<.001
Shortage of other staff	7.4 (1.2)	9.0 (0.3)	.20	8.5 (0.6)	.39	11.5 (1.2)	.02

Abbreviations: COVID-19, coronavirus disease 2019; PPE, personal protective equipment.

[a] Outcome measures are from the Centers for Medicare & Medicaid Services Nursing Home COVID-19 Public File as of July 2, 2020.

[b] Unadjusted comparisons of outcome measures by ownership category relative to private equity were made using *t* tests for resident COVID-19 measures and proportional tests for resident PPE supply and staff shortage measures.

(66.4 [3.0]; *P* = .06), and government-owned (56.2 [7.3]; *P* = .02) nursing homes. Nonprofit nursing homes had the highest mean (SD) number of deaths by any cause per 1000 residents (eg, PE vs nonprofit: 87.9 [4.8] vs 91.5 [2.2]; *P* = .50). Relative to PE nursing homes, for-profit nursing homes (78.1 [1.3]; *P* = .047) and government-owned nursing homes (67.6 [4.5]; *P* = .002) had fewer deaths from any cause per 1000 residents.

For N95 masks, PE-owned nursing homes reported having the lowest mean (SD) percentage of facilities with a 1-week supply of N95 masks (76.8% [1.5%]) and medical gowns (64.3% [1.4%]) compared with government-owned (masks: 91.8% [1.4%]; *P* < .001; gowns: 91.7% [1.3%]; *P* < .001), nonprofit (masks: 89.1% [0.7%]; *P* < .001; gowns: 91.4% (0.6%); *P* < .001), and for-profit nursing homes (masks: 86.2% (0.4%); *P* < .001; gowns: 88.0% [0.4%]; *P* < .001). Compared with PE-owned nursing homes, a higher percentage of nonprofit nursing homes reported having a 1-week supply of eye protection (93.2% [1.0%] vs 95.8% [0.5%]; *P* = .02), gloves (94.3% [0.8%] vs 97.4% [0.3%]; *P* < .001), and hand sanitizer (93.9% [0.9%] vs 96.5% [0.4%]; *P* = .01). Government-owned nursing homes were also more likely to have a 1-week supply of medical gloves (97.9% [0.7%]; *P* = .002) and hand sanitizer (96.7% [0.9%]; *P* = .03) compared with PE-owned nursing homes.

PE-owned nursing homes reported having the lowest percentage with a shortage of nurses (10.9% [1.5%]), clinical staff (2.0% [0.7%]), aides (13.3% [1.6%]), and other staff (7.4% [1.2%]) compared with for-profit (nursing staff: 15.5% [0.4%]; *P* < .003; clinical staff: 2.9% [0.1%]; *P* = .11; aids: 18.1% [0.4%]; *P* < .004; other staff: 9.0% [0.3%]; *P* = .20), government-owned (nursing staff: 20.3% [1.4%]; *P* < .001; clinical staff: 3.0% [0.6%]; *P* = .34; aids: 21.6% [1.5%]; *P* < .001; other staff: 11.5% [1.2%]; *P* = .02), and nonprofit (nursing staff: 13.6% [0.7%]; *P* = .25; clinical staff: 2.6% (0.3%); *P* = .45; aids: 16.0% [0.8%]; other staff: 8.5% [0.6%]; *P* = .39) nursing homes.

Adjusted Differences in Outcomes by Ownership

In multivariate analyses (**Table 3**), government-owned nursing homes reported 35.5 (95% CI, −69.2 to −1.8; *P* = .03) fewer COVID-19 confirmed cases per 1000 residents compared with PE nursing homes (eTable 4, eTable 5, and eTable 6 in the Supplement). However, COVID-19 deaths and deaths by any cause per 1000 residents did not differ significantly between PE-owned and nonprofit or

Table 3. Association Between COVID-19 Outcomes and Ownership

Outcome	For-profit, % (95% CI)[a]	*P* value[b]	Nonprofit, % (95% CI)[a]	*P* value[b]	Government-owned, % (95% CI)[a]	*P* value[b]
Resident COVID-19 measures, No. (95% CI), per 1000 residents						
Confirmed COVID-19 cases	−18.2 (−49.4 to 13.0)	.73	−25.6 (−57.4 to 6.2)	.20	−35.5 (−69.2 to −1.8)	.03
COVID-19 deaths	−5.3 (−27.4 to 16.9)	>.99	−8.9 (−32.0 to 14.1)	>.99	−6.7 (−35.0 to 21.7)	>.99
All deaths	9.0 (−22.4 to 4.3)	.44	−4.6 (−18.4 to 9.2)	>.99	−8.9 (−25.1 to 7.4)	.89
PPE supply measures						
1-wk supply of N95 masks	9.1 (1.8 to 16.3)	.006	13.0 (5.5 to 20.6)	<.001	14.8 (6.5 to 23.0)	<.001
1-wk supply of surgical masks	1.6 (−6.2 to 3.1)	>.99	2.5 (−2.3 to 7.3)	>.99	2.4 (−3.0 to 7.8)	>.99
1-wk supply of eye protection	−0.1 (−4.7 to 4.5)	>.99	3.7 (−1.2 to 8.6)	.29	2.3 (−3.2 to 7.8)	>.99
1-wk supply of gowns	21.3 (11.8 to 30.8)	<.001	27.0 (17.7 to 36.2)	<.001	25.7 (16.1 to 35.3)	<.001
1-wk supply of gloves	1.6 (−2.5 to 5.7)	>.99	3.0 (−1.3 to 7.2)	.39	3.3 (−1.0 to 7.7)	.25
1-wk supply of hand sanitizer	1.0 (−3.2 to 5.3)	>.99	2.7 (−1.8 to 7.3)	.65	1.8 (−3.4 to 6.9)	>.99
Staff shortage measures, %						
Shortage of nursing staff	3.2 (−1.4 to 7.7)	.41	1.0 (−4.0 to 6.0)	>.99	6.9 (0.0 to 13.9)	.049
Shortage of clinical staff	0.3 (−1.5 to 2.1)	>.99	0.0 (−2.0 to 2.1)	>.99	−0.2 (−3.0 to 2.7)	>.99
Shortage of aides	3.2 (−2.4 to 8.8)	.79	1.0 (−5.4 to 7.3)	>.99	4.7 (−2.9 to 12.2)	.60
Shortage of other staff	1.0 (−2.6 to 4.7)	>.99	0.3 (−3.6 to 4.1)	>.99	2.5 (−2.6 to 7.7)	>.99

Abbreviations: COVID-19, coronavirus disease 2019; PPE, personal protective equipment.

[a] Linear regressions were used for estimation. All models included the following covariates: mean age of residents; percentage women; occupancy rate; mean activities of daily living score; multifacility chain membership; rural status; terciles of the distributions of the percentage of patients covered by Medicaid, percentage of patients covered by Medicare, and percentage of White patients; and total number of beds. Private equity ownership is the comparison group for all models. Standard errors were adjusted for clustering at the level of the Hospital Referral Region.

[b] Bonferroni correction was used for multiple comparisons.

between PE-owned vs government-owned nursing homes. COVID-19 confirmed cases, COVID-19 deaths, and deaths by any cause per 1000 residents did not differ significantly between PE-owned and for-profit nursing homes.

Adjusted estimates for PPE measures indicated that for-profit, nonprofit, and government nursing homes were 10.5% (9.1 percentage points; 95% CI, 1.8 to 16.3 percentage points; $P = .006$), 15.0% (13.0 percentage points; 95% CI, 5.5 to 20.6 percentage points; $P < .001$), and 17.0% (14.8 percentage points; 95% CI, 6.5 to 23.0 percentage points; $P < .001$), respectively, more likely to report having at least a 1-week supply of N95 masks compared with PE-owned nursing homes. Additionally, for-profit, nonprofit, and government-owned nursing homes were 24.3% (21.3 percentage points; 95% CI, 11.8 to 30.8 percentage points; $P < .001$), 30.7% (27.0 percentage points; 95% CI, 17.7 to 36.2 percentage points; $P < .001$), and 29.2% (25.7 percentage points; 95% CI, 16.1 to 35.3 percentage points; $P < .001$), respectively, more likely to have at least a 1-week supply of medical gowns than PE-owned nursing homes. All other associations between PE-owned and for-profit, nonprofit, and government-owned nursing homes for PPE outcomes were not statistically different (Table 3).

Staffing shortages did not significantly differ between PE-owned and for-profit nursing homes or between PE-owned and nonprofit nursing homes. However, government-owned nursing homes had a 6.9 percentage point (95% CI, 0.0 to 13.9 percentage points; $P = .049$) higher probability of having a shortage of nursing personnel, representing a relative difference of 45.8%.

Estimates from sensitivity analyses using Poisson regressions for continuous outcomes and logistic regressions for dichotomous outcomes were largely consistent with those from our primary analyses. eTable 7 and eTable 8 in the Supplement present these analyses.

Discussion

In this national study of 11 470 nursing homes, PE-owned nursing homes did not have significantly higher self-reported rates of COVID-19 cases than nonprofit or for-profit nursing homes but had 35.5 more cases per 1000 residents than government-owned facilities. PE-owned homes did not have significantly higher rates of COVID-19 deaths or of deaths from any cause. It is possible that differences in rates of testing among facilities may have obscured differences in COVID-19 cases and deaths, but this would not have affected estimates of deaths by any cause.

By several measures, PE-owned facilities were less likely to have a 1-week or longer supply of PPE. PE-owned facilities were significantly less likely to have a 1-week supply of N95 masks and of gowns compared with all other types of facilities, less likely to have a supply of eye protection compared with nonprofit nursing homes, and less likely to have a supply of gloves compared with government-owned facilities. For example, PE-owned facilities were 14.8% less likely to have at least a 1-week supply of N95 masks compared with government facilities and 13.0% and 9.1% less likely to have them compared with nonprofit and for-profit nursing homes, respectively. There were no significant differences in staffing shortages between PE facilities and other types of facilities, except that government-owned facilities were 6.9% more likely to have a shortage of nursing staff.

It is not clear why PE-owned nursing homes had lower supplies of PPE. This may have been due to cost-cutting strategies undertaken by these facilities. If this was the case, it is not clear why staffing levels were not also lower for PE-owned nursing homes. It is possible that PE-owned homes were attempting to control costs by keeping the minimum level of supplies that they anticipated would be necessary.

Although no previous research that we are aware of has examined PE ownership and outcomes associated with COVID-19, 6 recent studies[4,26-30] compared outcomes of for-profit nursing homes with nursing homes with other types of ownership. The findings of these studies were inconsistent. Two studies[4,26] did not find an association between for-profit ownership and COVID-19 cases, 1 study[27] found that for-profit ownership was associated with higher COVID-19 mortality rates, 2 studies[28,29] did not find statistically significant associations between for-profit ownership and

COVID-19 cases or deaths, and 1 study[30] found both positive and negative associations of for-profit ownership with cases and deaths. These studies classified PE-owned nursing homes as for-profit entities and did not distinguish between the 2 types of ownership. Our study makes a new contribution by comparing PE with non-PE for-profit nursing homes in addition to other ownership categories, based on outcomes related to COVID-19. The distinction between PE and non-PE for-profit facilities is important because policy makers have expressed concerns that PE-owned nursing homes may have different incentives and provide lower quality care compared with for-profit as well as nonprofit homes.[5]

It is possible that nursing home characteristics, including past performance on quality measures, are not associated with whether COVID-19 makes its way into a facility, how rapidly it spreads once inside, or the related mortality rate after patients are exposed. For example, past scores on the CMS Five Star Rating system, used to measure nursing home quality, have not been associated with whether a facility has any COVID-19 cases or the number of cases among those with at least 1 case.[4] Similarly, performance on the quality measures used to construct the Five Star Ratings has not been associated with the number of cases in nursing homes.[31] Other factors, such as COVID-19 infection rates in the communities where staff reside[32] and differences in state policies implemented to prevent the spread of the virus,[33] may have stronger associations with COVID-19 morbidity and mortality in nursing homes.

Our study does not provide evidence regarding whether PE nursing homes perform better or worse than non-PE nursing homes on measures that more broadly reflect clinical quality. In studies conducted before the pandemic, 2 studies[9,23] found little evidence of an association between PE ownership and nursing home performance on a variety of clinical process and outcome measures. Both studies were based on comparisons of nursing homes more than a decade ago. One of these studies,[9] in addition to 3 others,[20-22] examined PE ownership and nursing home staffing levels; 3 studies[9,20,21] found little evidence of changes in staffing and 1 study[22] identified declines in staffing. Two recent working papers[3,15] also had mixed results. The first[15] found PE ownership to be associated with declines in staffing levels, increased rehospitalization rates, and worse CMS Five Star Ratings. Conversely, the second working paper[3] found that PE firms tended to increase staffing levels and improve the Five Star Ratings of nursing homes. The inconsistent results of the 2 studies may have been because of different lengths of study period or differences in their analytic approaches.

Limitations

Our study has several limitations. First, results are based on cross-sectional data, and the estimates may not reflect causal relationships because we cannot exclude the possibility of unmeasured or unobserved differences between nursing homes. Second, PE acquisitions that were identified through the Capital IQ and Irving Levin databases were captured through public announcements, which likely underestimate the total number of acquisitions. Additionally, acquisitions that are not publicly announced, such as smaller acquisitions and those in which PE firms had minority ownership, may not be identified in the databases. This may bias our estimates toward no effect owing to measurement error. However, we found a similar number of nursing home acquisitions as 2 other studies.[3,15] Third, 1232 nursing homes (8.6%) reported data that did not pass validity checks in the Nursing Home COVID-19 Public File. There may be differences between facilities that reported confirmed data and those that did not. Fourth, the Nursing Home COVID-19 Public File is being updated regularly as nursing homes report additional cases and deaths. Fifth, some nursing homes may not have tested for COVID-19 as much as others, which may have led to underreporting of COVID-19 cases and deaths by some facilities. Sixth, reporting requirements for COVID-19 deaths vary by state, and nursing home residents who were hospitalized prior to death may not have been counted toward facility totals in some cases. We compared nursing homes within geographic regions, but it is possible that the likelihood of hospitalization prior to death varied by type of ownership. Seventh, the data are self-reported; some nursing homes may have inaccurately reported PPE supplies and staffing levels. Despite these limitations, the Nursing Home COVID-19 Public File has a number of strengths for addressing this topic that are not found elsewhere, including a large national sample and detailed COVID-19 outcomes, which can be linked to other publicly available data sources to identify comprehensive information on nursing homes.

Conclusions

In this cross-sectional study, PE-owned nursing homes did not have more COVID-19 confirmed cases than nonprofit or for-profit nursing homes nor did they differ significantly in COVID-19 deaths or deaths by any cause. They reported having less PPE than other facilities and similar staffing shortages. Further study, including longitudinal studies, are needed to determine whether PE-owned nursing homes perform better or worse than non–PE-owned nursing homes on broader measures of clinical quality and whether they are associated with higher or lower health care spending.

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SUPPLEMENT.

eAppendix. Identification of Private Equity Nursing Home Acquisitions
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Have Private Equity Owned Nursing Homes Fared Worse Under COVID-19?*

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Abstract

Over 40% of U.S. COVID-19 deaths occurred in nursing homes. Given increasing private equity (PE) ownership in healthcare and long-standing concerns that PE investors focus on profits to the detriment of patients, it is important to understand the impact of PE ownership during the COVID-19 pandemic. This study evaluates how PE acquisitions impacted the readiness and outcomes of nursing facilities during the onset of the COVID-19 pandemic. We relate PE ownership to COVID-19 cases, deaths, and personal protective equipment (PPE) shortages, controlling for facility characteristics, resident composition, local characteristics, and the severity of COVID-19 outbreak near the facility. PE ownership was associated with a mean decrease in the probability of confirmed resident cases by 7.1 percentage points ("pp") ($p < 0.01$) and confirmed staff cases by 5.4 pp ($p = 0.01$). PE was also associated with decreased probability of PPE shortages—including N95s (6.4 pp; $p < 0.01$), surgical masks (7.6 pp; $p < 0.01$), eyewear (4.8 pp; $p < 0.01$), gowns (7.0 pp; $p < 0.01$), gloves (3.3 pp; $p = 0.02$), and hand sanitizer (2.3 pp; $p = 0.12$). Facilities previously (but not presently) owned by PE firms did not fare similarly well. Prior PE ownership was associated with increased PPE shortages and, if anything, higher probability of resident outbreaks. Our results indicate that—contrary to a common media narrative—PE-owned facilities have actually fared better under the COVID-19 pandemic. They also suggest that the long-run consequences of PE ownership warrant further research.

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1 Introduction

Private equity (PE) acquisitions in the healthcare industry have expanded in recent years, (Gondi and Song, 2019) reaching heights of \$67.5 billion and 951 deals in the U.S. in 2019.[1] While PE owners have been shown to improve the productivity (Lichtenberg and Siegel, 1990; Harris, Siegel and Wright, 2005; Davis et al., 2014) and financial performance (Bergström, Grubb and Jonsson, 2007; Boucly, Sraer and Thesmar, 2011) of their acquisitions, concerns abound that PE's focus on profits may adversely impact patients and providers. These concerns span the healthcare industry, including in physician groups (Zhu, Hua and Polsky, 2020), surgery (Joynt Maddox and Livingston, 2020), ophthalmology (Patel, Groth and Sternberg, 2019), dermatology (Resneck, 2018), otolaryngology (Miller, Rathi and Naunheim, 2020), behavioral health centers (Brown, O'Donnell and Casalino, 2020), and even emergency medical services (Chhabra et al., 2020). As PE's stake in the healthcare industry has grown, they have also lobbied to protect their financial interests, including by lobbying to prevent regulation of surprise billing (Batt and Appelbaum, 2019; Sanger-Katz, Creswell and Abelson, 2019; Brown, 2020; Lewis, 2020).

PE's healthcare buyouts have drawn scrutiny from regulators (U.S. Government Accountability Office, 2010), legislators (U.S. House of Representatives, 2007; U.S. Senate, 2008; Cumming, 2019), and the press (Duhigg, 2007; Whoriskey and Keating, 2018; Elk, 2019; Sanger-Katz, Creswell and Abelson, 2019; Kolhatkar, 2020). This has been particularly true for PE buyouts of nursing homes, due to the volume of PE acquisitions (12% of all facilities by 2010) (U.S. Government Accountability Office, 2010), the significant public expenditure on nursing home care through Medicare and Medicaid, and the vulnerability of nursing homes' elderly and infirm residents. In 2019, members of Congress sent letters to the executives of several PE firms with investments in the nursing home industry (including The Carlyle Group, Formation Capital, Fillmore Capital Partners, and Warburg Pincus LLC) to request additional information about the transactions (Brown, Pocan and Warren, 2019). Most recently, with over 40% of U.S. deaths from COVID-19 occurring in the long-term care setting (Kamp and Mathews, 2020; Conlen et al., 2020), PE owners have been accused of implementing cost-cutting measures that left their facilities exposed (Goldstein, Silver-Greenberg and Gebeloff, 2020; Gretchen and Saliba, 2020). Critics have argued that PE-owned nursing homes

[1] These values were computed from PitchBook and exclude deals classified as private investment in public equity.

cut staffing in the years leading up to the pandemic (Chaffin, 2020) and lacked adequate supplies to properly care for residents when crisis struck (Kingsley, 2020).

In this study, we evaluate such claims by comparing COVID-related outcomes at PE-owned and non-PE owned nursing homes, controlling for other observable facility characteristics and the magnitude of the local COVID-19 outbreak. Specifically, we examine the relationship between PE ownership and facilities' confirmed and suspected COVID-19 cases and deaths, as well as measures of facilities' supplies of personal protective equipment (PPE). This paper relates most closely to two literatures. The first is a growing literature examining PE acquisitions of nursing homes (Grabowski and Stevenson, 2008; Pradhan et al., 2013, 2015; Bos and Harrington, 2017; Gupta et al., 2020; Gandhi, Song and Upadrashta, 2020). The second is the very recent literature on factors characterizing nursing homes with COVID-19 outbreaks (Abrams et al., 2020; Gorges and Konetzka, 2020; He, Li and Fang, 2020; Konetzka, 2020). To the best of our knowledge, this is the first study to connect and build on both literatures by using data to evaluate how PE-owned nursing homes fared under COVID-19. Accordingly, the purpose of this study is to evaluate whether and how PE acquisitions may have impacted the readiness and response of nursing homes to the onset of the pandemic.

2 Methods

2.1 Study Design

On May 8, 2020, the Centers for Medicare & Medicaid Services (CMS) mandated that nursing homes report COVID-19 cases and deaths, as well as PPE shortages, to the Centers for Disease Control and Prevention (CDC) National Healthcare Safety Network (NHSN) system. Facilities were required to submit data on a weekly basis starting May 24, 2020. While they were asked to submit historical data as well, CMS did not require reporting of cases prior to May 8. In total, approximately 88 percent of nursing homes reported the required statistics. We use these data to estimate logistic regression models comparing COVID-19 outcomes at PE-owned and comparable non-PE facilities.

2.2 Data Sources

Facility-level NHSN survey data on COVID-19 outcomes for CMS-certified nursing homes come from the Data.CMS.gov website. These include data on case counts, deaths, and PPE shortages.

In addition, we collect facility characteristics from LTCFocus.org[2] (2017), Nursing Home Compare (December 2019), and the Provider of Services (POS) files (December 2019). These include facility size, occupancy rate, resident composition (by demographics, payer, and treatment intensity), CMS facility quality ratings, facility ownership, and facility geolocation data. We also obtain county-level COVID-19 incidence rates from the COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University (Dong, Du and Gardner, 2020).

We use several financial databases to identify historical PE investments, including PitchBook, Preqin, S&P Capital IQ, Irving Levin Associates, and SDC Platinum. In total, we identify 391 facilities owned by PE investors during the pandemic, and 1,219 facilities in which PE both invested and exited prior to 2020. For details on how we identified PE ownership, see Appendix B.

2.3 Measures

Our primary outcomes measure the prevalence of COVID-19 outbreak at nursing homes. For both resident and staff populations, we evaluate facility-level outbreak prevalence in three ways. The dependent variable "Confirmed" indicates if the facility reported ever having at least one laboratory positive individual prior to the survey date. Similarly, "Suspected" indicates if the facility reported ever having at least one suspected or laboratory positive individual prior to the survey date. Lastly, "Deaths" indicates whether the facility reported ever having at least one suspected or laboratory positive individual die prior to the survey date.

We are also interested in understanding how facility preparedness varies by ownership type. Our secondary outcomes measure facility-level shortages in PPE, including N95 masks, surgical masks, eyewear, gowns, gloves, and hand sanitizer. For each type of PPE, we construct a variable indicating whether the facility has at least a one-week supply.

For all outcome measures, we analyze the first week of data reported in the NHSN (typically

[2]LTCFocus.org is a product of the Shaping Long Term Care in America Project at Brown University, funded in part by the National Institute on Aging (1P01AG027296).

May 24) to best represent how facilities fared prior to the availability of national-level data to coordinate a response (e.g. additional resources may have been targeted to facilities based on initial reports).

The key independent variable in our study is an indicator for PE ownership during the pandemic ("PE"). Of secondary interest is a facility's previous PE ownership that concluded prior to the pandemic ("prior PE"). Appendix B describes the construction of these indicators in greater detail. We also report estimates for two additional ownership variables that we include as controls: indicators for facility for-profit status and membership in a multi-location chain.

2.4 Statistical Analysis

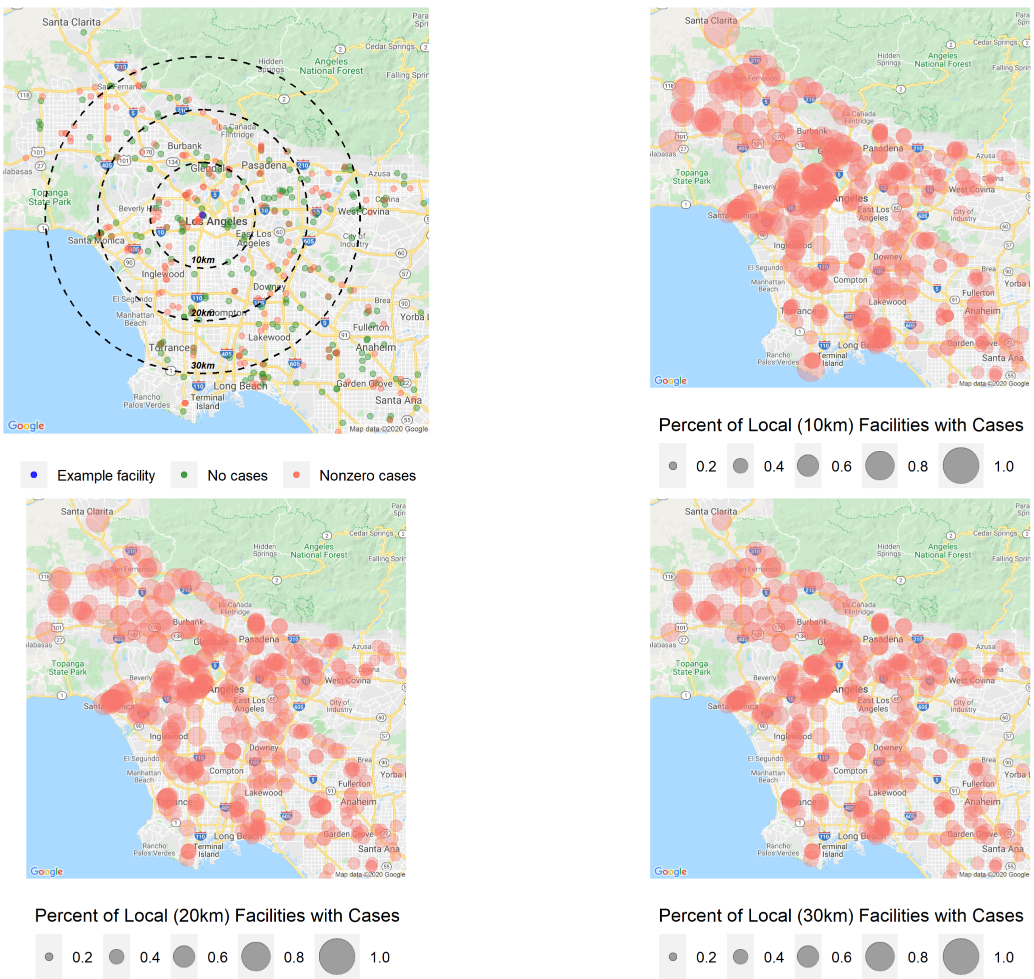
We perform two empirical tests, both at the facility level. First, we estimate a logistic regression model for multiple measures of outbreak prevalence on indicators for past and present PE ownership, with additional controls. This assesses the relationship between PE ownership and likelihood of a COVID-19 outbreak. Second, we analogously estimate a logistic regression model relating past and present PE ownership to the likelihood of PPE shortages.

In our empirical tests, we attempt to control for a variety of facility- and location-specific factors that may otherwise influence COVID-related outcomes. Along with state-level fixed effects, each regression includes four categories of additional control variables:

1. **Facility ownership:** indicators for membership in a chain and for-profit status.
2. **Facility characteristics:** a second-degree polynomial in the number of occupied beds, an indicator for access to on-site testing, and the date of first survey response.
3. **Composition of resident census:** the percentage of Black residents, the percentage of residents on Medicaid, the average resident case mix index (CMI), and the average resident Activities of Daily Living (ADL) score.
4. **Local characteristics:** local average COVID-19 measures (10-, 20-, and 30-km radius), Herfindahl index (HHI) measures to capture local facility competition (10-, 20-, and 30-km radius), and the natural logarithm of county population and COVID-19 cases per 100,000 county residents.

The inclusion of controls for facility ownership, characteristics, and resident composition helps ensure that we analyze facilities with similar features, contrasting PE-owned facilities with otherwise comparable non-PE facilities. This is especially important if susceptibility to COVID-19 varies with factors including age, race, and pre-existing health conditions. Similarly, location has been shown to strongly predict outbreaks, as connectedness to existing infections determines further spread. Consequently, we include not only broad geographic controls (state fixed effects, county COVID-19 incidence) but also local measures specific to a facility's immediate vicinity.

Figure 1: Example of Local Measures (Los Angeles)



Notes: The figure illustrates how facility-specific locally defined control measures are constructed. For an example facility located in Downtown Los Angeles, neighboring facilities are categorized according to distance from the example in kilometers. In Panel (a), points within the dashed inner boundary represent all facilities within 10km of the example selection; similarly, points within the intermediate boundary represent all facilities within 20km, and points within the outermost boundary represent all facilities within 30km. For an outcome variable of interest, we calculate the average value of the measure across all facilities inside the corresponding radius. Panels (b), (c), and (d) illustrate this for one of our primary COVID-19 prevalence measures (the presence of any confirmed COVID-19 cases), displaying the local outcome average for each facility at 10-, 20-, and 30-km radiuses.

In particular, we define controls specifically for each facility in our data, based on average outcome values for the unique set of geographically proximate facilities. Figure 1 illustrates the construction of this measure for the presence of confirmed resident cases. The first panel depicts 10-, 20-, and 30-kilometer radiuses around an arbitrary example facility in Downtown Los Angeles. The surrounding points represent nearby facilities with (red) and without (green) outbreaks, e.g. those within the innermost boundary are all facilities within 10km of the example. We construct controls for each dependent variable by averaging the corresponding values of the dependent variable for all other facilities within the concentric circle. In this example, the three controls are the percent of nearby facilities within 10-, 20-, and 30-kilometers with a confirmed case. The remaining panels of Figure 1 present the values of these controls computed for all facilities in the area. We repeat the analogous exercise for every facility in our data, for each outcome we study. An alternative approach to control for local shocks is to include county-level fixed effects instead of local average controls. We do this in Appendix C.1.

Finally, we include an indicator for the date on which the facility first responded to the survey to help control for confounding from response delays. To assess robustness of the model, we also estimate alternative specifications of the regression model for COVID-19 prevalence in Appendix C.2, including estimates of the intensive margin of COVID-19 prevalence (total counts of cases and deaths).

3 Results

Our analysis included a total of 13,398 nursing facilities from 49 states. Of these, 391 facilities were PE-owned during the pandemic, 1,219 facilities were previously PE-owned prior to the pandemic, and 11,788 facilities were never subject to PE ownership. In total, our sample comprised 1,043,007 occupied beds.

Table 1 presents unadjusted means for dependent and control variables for non-PE, prior PE and PE facilities. Facilities PE-owned during the pandemic had lower rates of outbreak as measured by all six of our primary measures; in contrast, prior PE facilities exhibited higher rates of outbreak than non-PE facilities.

Table 1: Descriptive statistics for sample of facilities, by PE ownership

	Non-PE		Prior PE			PE		
	Mean	SD	Mean	SD	t-stat	Mean	SD	t-stat
Any Confirmed Resident Cases	0.27	0.44	0.34	0.47	-5.20	0.18	0.38	3.93
Any Suspected Resident Cases	0.43	0.50	0.49	0.50	-3.84	0.30	0.46	5.30
Any Resident Deaths	0.20	0.40	0.26	0.44	-4.43	0.14	0.35	3.00
Any Confirmed Staff Cases	0.34	0.47	0.39	0.49	-3.17	0.23	0.42	4.40
Any Suspected Staff Cases	0.51	0.50	0.54	0.50	-1.62	0.39	0.49	4.91
Any Staff Deaths	0.02	0.13	0.02	0.14	-0.72	0.01	0.10	0.91
Lacking One-Week Supply of N95 Masks	0.16	0.37	0.26	0.44	-8.21	0.14	0.35	1.31
Lacking One-Week Supply of Surgical Masks	0.08	0.28	0.14	0.35	-6.66	0.04	0.20	2.77
Lacking One-Week Supply of Eye Protection	0.09	0.29	0.14	0.35	-5.29	0.07	0.26	1.32
Lacking One-Week Supply of Gowns	0.18	0.39	0.32	0.47	-11.65	0.15	0.36	1.45
Lacking One-Week Supply of Gloves	0.05	0.21	0.13	0.34	-12.31	0.04	0.19	1.07
Lacking One-Week Supply of Hand Sanitizer	0.06	0.24	0.14	0.35	-10.62	0.05	0.23	0.64
Occupied Beds	77.26	42.58	81.77	33.05	-3.58	83.72	32.97	-2.96
% Medicaid	59.01	23.49	66.77	17.05	-11.23	64.92	17.91	-4.92
% Black	8.97	18.10	12.64	20.05	-6.67	13.43	20.04	-4.76
Average Case Mix Index	1.17	0.22	1.18	0.14	-2.65	1.21	0.24	-4.23
Average ADL Score	16.46	3.25	16.50	2.26	-0.37	17.16	2.30	-4.21
Resident Access to Testing in Facility	0.95	0.21	0.98	0.15	-4.09	0.93	0.26	2.19
HHI (10km radius)	0.38	0.34	0.32	0.30	5.79	0.34	0.32	2.44
HHI (20km radius)	0.24	0.28	0.20	0.24	5.26	0.20	0.24	2.65
HHI (30km radius)	0.15	0.21	0.12	0.17	4.14	0.12	0.14	3.30
Total Occupied Beds in 10km Radius	660.06	1,126.79	628.76	817.98	0.94	620.33	715.10	0.69
Total Occupied Beds in 20km Radius	1,847.88	3,264.46	1,688.84	2,289.78	1.66	1,493.07	1,966.52	2.13
Total Occupied Beds in 30km Radius	3,237.58	5,545.99	2,991.21	4,085.52	1.51	2,496.80	3,717.96	2.62
Log(County COVID-19 Incidence)	5.38	1.42	5.55	1.23	-3.94	5.31	1.02	1.06
Log(County Population)	12.16	1.83	12.29	1.68	-2.49	12.34	1.64	-1.92
Facilities	11783		1219			389		

Notes: The table provides unadjusted mean and standard deviation values for key analysis variables, including the main outcome measures and several control variables. The first two columns provide statistics for the sample of facilities never acquired by a PE firm, either presently or previously. The middle three columns provide statistics for the sample of previously PE-owned facilities. The third of these columns includes the t-statistic corresponding to a test on the equality of means between the Non-PE and Prior PE samples. Similarly, the final three columns provide statistics for the sample of facilities that were PE-owned during the pandemic, and the last column reports the result of a test on the equality of means between the Non-PE and PE samples.

Notably, non-PE facilities were twice as likely to report confirmed cases or deaths among residents than PE-owned facilities. Relative to non-PE facilities, PE (prior PE) facilities were similarly less (more) likely to report shortages in all six categories of PPE. For instance, 26% of prior PE facilities reported insufficient supplies of N95 masks, compared to 14% of PE facilities.

Disparities in unadjusted rates of outbreak and preparedness could also be attributed to several other factors, including differences in facility location (with disease prevalence varying geographically) or resident composition. County-level disease incidence and population were similar across ownership types however, suggesting facility location may not fully explain the differences. Patient severity, as measured by CMI and ADL scores, was also similar across groups, though PE and prior PE facilities both had higher percentages of Black and Medicaid residents (Table 1).

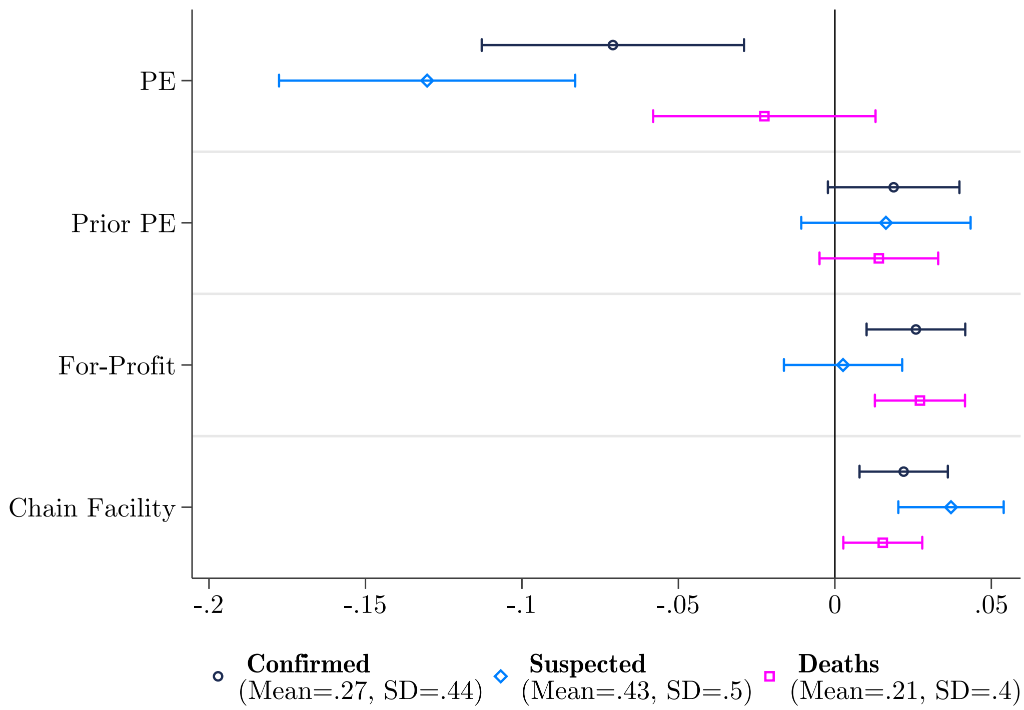
To account for location- and facility-level differences more explicitly, we investigate the role of PE ownership further using logistic regression models with controls. For ease of interpretation, we provide regression estimates as marginal effects rather than odds ratios (Norton, Dowd and Maciejewski, 2019). In each regression, the marginal effect captures the average change in predicted probability of an outbreak or PPE shortage associated with PE ownership.

After controlling for facility characteristics, resident composition, and local factors including nearby outbreak intensity, PE facilities remain less likely to experience a COVID-19 outbreak. PE ownership was associated with a mean decrease in probability of confirmed resident outbreak by 7.1 percentage points ("pp") [95% CI, -11.3 to -2.9 pp; $p < .001$], a decrease in suspected resident outbreak probability by 13.0 pp [95% CI, -17.8 to -8.3 pp; $p < .001$], and a (non-significant) decrease in the presence of COVID-19 resident deaths by 2.3 pp [95% CI, -5.8 to 1.3 pp; $p = .214$] (Figure 2).

On the other hand, outbreak prevalence was similar or greater for prior PE facilities than non-PE facilities. Most notably, prior PE ownership was associated with a 1.9 pp greater likelihood of confirmed resident cases [95% CI, -0.2 to 4.0 pp; $p = .080$]. Prior PE facilities also saw increases in resident deaths, but with statistical significance less robust to different specifications.

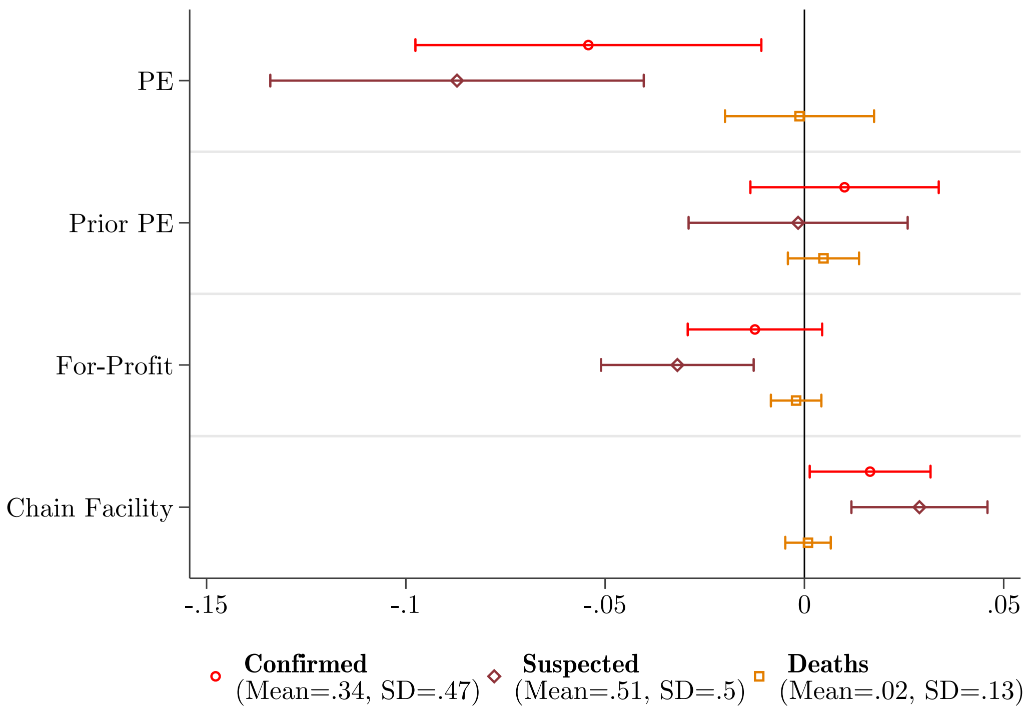
PE ownership was also associated with statistically significant decreases in staff outbreaks. This association was a reduction of 5.4 pp [95% CI, -9.8 to -1.1 pp; $p = .014$] for the likelihood of a confirmed staff outbreak and 8.7 pp [95% CI, -13.4 to -4.0 pp; $p < .001$] for the likelihood of a suspected staff outbreak (Figure 3).

Figure 2: PE ownership and presence of any reported resident cases



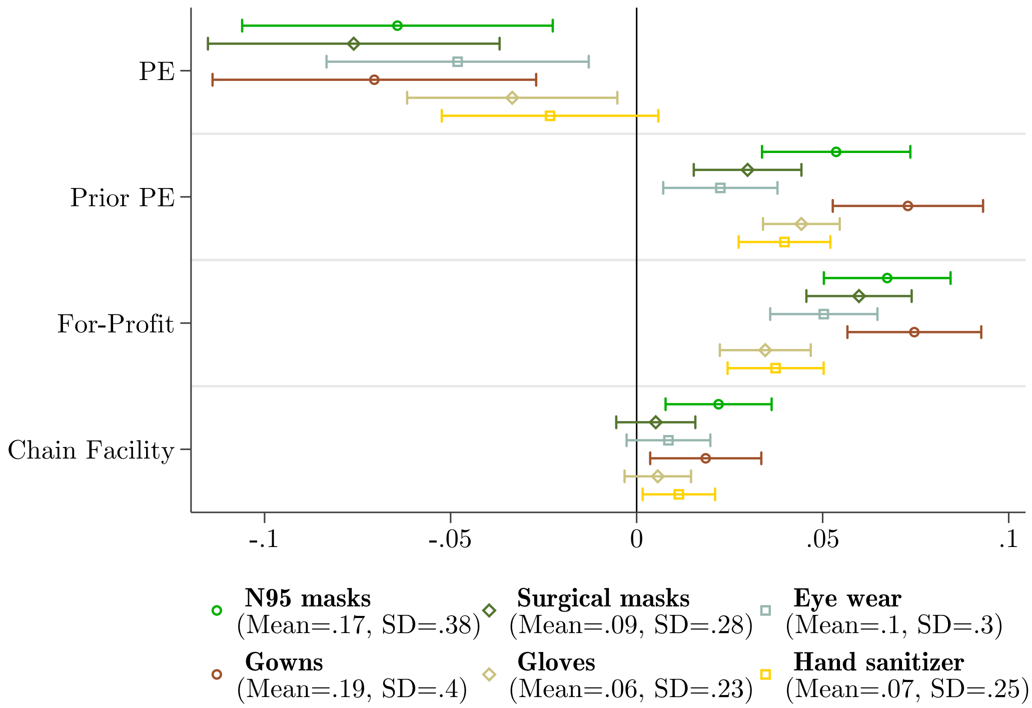
Notes: The figure presents estimates of a logistic regression model relating facility ownership and resident outcomes. Point estimates represent the incremental or marginal effect of a change in ownership status on the predicted probability that the facility reports presence of COVID-19 among residents. Error bars represent the corresponding 95% confidence interval. The outcome "Confirmed" indicates that the facility had at least one resident with a laboratory positive COVID-19 test, "Suspected" indicates that the facility had at least one resident with a suspected case (all confirmed cases are also considered suspected), and "Deaths" indicates that the facility had at least one resident death from COVID-19. Mean and standard deviation values of the outcomes in the sample population are provided in parenthesis. In addition to ownership variables, controls include facility characteristics, resident composition, and local characteristics as described in Section 2.4. We allow different intercepts for facilities missing data on a control variable.

Figure 3: PE ownership and presence of any reported staff cases



Notes: The figure presents estimates of a logistic regression model relating facility ownership and staff outcomes. Point estimates represent the incremental or marginal effect of a change in ownership status on the predicted probability that the facility reports presence of COVID-19 among staff. Error bars represent the corresponding 95% confidence interval. The outcome "Confirmed" indicates that the facility had at least one staff member with a laboratory positive COVID-19 test, "Suspected" indicates that the facility had at least one staff member with a suspected case (all confirmed cases are also considered suspected), and "Deaths" indicates that the facility had at least one staff death from COVID-19. Mean and standard deviation values of the outcomes in the sample population are provided in parenthesis. In addition to ownership variables, controls include facility characteristics, resident composition, and local characteristics as described in Section 2.4. We allow different intercepts for facilities missing data on a control variable.

Figure 4: PE ownership and personal protective equipment shortages



Notes: The figure presents estimates of a logistic regression model relating facility ownership and PPE shortages. Point estimates represent the incremental or marginal effect of a change in ownership status on the predicted probability that the facility reports insufficient PPE. Error bars represent the corresponding 95% confidence interval. The outcome "N95 Masks" indicates that the facility reported to lacking a sufficient supply of N95 masks to last for one week. Similarly, the outcome "Surgical Masks" indicates that the facility reported a shortage of surgical masks. Other dependent variables are defined analogously. Mean and standard deviation values of the outcomes in the sample population are provided in parenthesis. In addition to ownership variables, controls include facility characteristics, resident composition, and local characteristics as described in Section 2.4. We allow different intercepts for facilities missing data on a control variable.

Consistent and widespread PPE use is an important infection control protocol, and a likely predictor of how facilities fared during the pandemic. McGarry, Grabowski and Barnett (2020) find that PPE shortages are both prevalent (with more than 20% of facilities reporting severe shortages) and impactful, as nursing homes with COVID-19 outbreaks were more likely to report shortages. We therefore consider access to PPE a measure of facility capability and resources. Coinciding with decreased outbreak likelihood, PE-owned facilities also saw fewer PPE shortages. Relative to non-PE facilities, PE ownership was associated with mean decreases in probability of shortage for all six types of PPE, ranging from -2.3 pp for hand sanitizer to -7.6 pp for surgical masks. With the exception of hand sanitizer, all estimates are statistically significant at the 5% level. In contrast, previous PE ownership was associated with increased average probabilities of shortage for all six categories of PPE, ranging from 2.3 pp for eye wear to 7.3 pp for gowns [$p < .01$ for all] (Figure 4).

4 Discussion

As private equity acquisitions in healthcare have grown in recent decades, so too have concerns that PE's focus on profits will hurt patients and providers. PE acquisitions of nursing homes have been particularly controversial due to their volume, the particular vulnerability of nursing home residents, and the fact that nursing home care is largely financed by Medicare and Medicaid. Accordingly, these investments have received significant scrutiny from regulators, policymakers, and the public. Critics have recently suggested that PE owners have exacerbated the impact of the COVID-19 pandemic on nursing homes. Our analysis of the nationwide survey responses, however, suggests that these concerns may be misplaced.

We find that when controlling for facilities' characteristics, patient composition, and the size of local COVID-19 outbreaks, PE ownership is associated with lower likelihood of COVID-19 outbreaks among residents and staff, as well as with fewer shortages of critical PPE. In other words, our findings suggest that PE owners were more successful at preventing outbreaks at their facilities and ensuring stocks of necessary protective equipment. These results are consistent with prior research in non-healthcare settings that observe PE owners to improve product (Bernstein and Sheen, 2016) and workplace (Cohn, Nestoriak and Wardlaw, 2019) safety. Notably, the estimated effects of PE ownership are relative to other for-profit and chain facilities, suggesting that PE owners had unique managerial aptitude or resources not shared by other for-profit chains. Furthermore, insofar as PE owners are more profit-motivated than other for-profit facilities, the financial repercussions accompanying an outbreak may have only strengthened PE's incentives to take preventative measures.

In contrast, we find that prior PE ownership is associated with PPE shortages and, to a lesser extent, resident outbreaks. This suggests that while PE owners may have invested in protecting their current facilities from outbreaks, previously PE owned facilities may have lacked managerial or financial resources to do the same. One possibility is that the financial practices implemented by PE owners—such as selling the real estate of acquired facilities (Zuckerman, 2010; Genesis HealthCare, 2011)—may have had long-run financial impacts inhibiting facilities' preparedness.[3] While our findings do not provide specific mechanisms, they do suggest further study of the long-run impacts of PE ownership as a topic for future research.

Our study cautions against a presumption that PE ownership is necessarily bad for patients and providers. Rather, it suggests that policymakers, researchers, and the media should take an evidence-driven approach in assessing the impact of private equity in the healthcare industry. It is likely that PE will continue to acquire providers across the healthcare industry, and policymakers should exercise caution by ensuring that the necessary data is collected and analyzed to understand both the immediate and long-lasting effects of PE acquisitions in various healthcare settings.

4.1 Limitations

Our study has two key limitations stemming from the CMS-mandated survey data. The first is that the data are self-reported. As such, if PE-owned facilities systematically over- or under-report confirmed and suspected COVID-19 cases, our regression estimates will be biased. Our results must therefore be read carefully as relying on the assumption that any misreporting is not systematically different for PE-owned facilities. The second limitation is that the survey data are incomplete: facilities were not required to report cases prior to May 8, 2020, and 1,145 of the 15,423 providers in the survey data either failed to respond or submitted data that failed CMS' data quality checks.

[3]Sale-leasebacks create considerable cash flow (from the sale) at the expense of exposing facilities to the risk of lease price increases. Such financial decisions undertaken during PE management create liabilities likely to outlast PE ownership, and may adversely affect the long-run financial health of a facility or chain. Recent work (Begley and Weagley, 2020) suggests facilities' financial health may play a role in outcomes during the pandemic.

Our estimates will be biased in favor of PE-owned facilities if either PE-owned facilities experienced early outbreaks that concluded prior to May 1, or if PE-owned facilities with COVID-19 outbreaks systematically failed to submit survey responses that passed quality assurance checks.

In Appendix D, we perform several tests to investigate whether these data limitations are likely to significantly bias our results. We first assess the robustness of our result to self-reporting concerns. Notably, we confirm our main finding that PE-owned facilities had fewer outbreaks using data from state departments of health (Abrams et al., 2020). Moreover, we find no evidence that discrepancies between case reports in state and federal data differ according to PE ownership. We also consider whether survey incompleteness may bias our findings, but do not observe any systematic differences between PE and non-PE facilities.

5 Conclusions

We find PE ownership to be associated with reductions in the presence of reported COVID-19 cases among residents and nursing staff, as well as with increased availability of PPE. These differences are not explained by a facility's chain membership or for-profit status, suggesting a specific contribution of PE owners beyond introducing profit incentives or corporate management practices. The benefits also do not extend to previously PE-owned facilities, which were more likely to suffer PPE shortages and, if anything, were more likely to experience resident outbreaks. Our findings suggest that PE ownership positively affected patients and staff under COVID-19, and that additional careful study of the long-lasting effects of PE ownership is warranted.

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Appendices

A Tables Corresponding to Figures in Main Text

In this section, we provide the estimates corresponding to the figures included in the main text. Figures 2 and 3 correspond to Table A.1, and Figure 4 corresponds to Table A.2.

Table A.1: PE ownership and presence of any reported COVID-19 cases

	Residents			Staff		
	Confirmed	Suspected	Deaths	Confirmed	Suspected	Deaths
PE	-0.0709***	-0.130***	-0.0225	-0.0542**	-0.0872***	-0.00122
	(0.0214)	(0.0241)	(0.0181)	(0.0221)	(0.0239)	(0.00954)
$PriorPE$	0.0188*	0.0163	0.0141	0.0101	-0.00158	0.00478
	(0.0107)	(0.0138)	(0.00968)	(0.0121)	(0.0140)	(0.00456)
$For\text{-}Profit$	0.0259***	0.00263	0.0272***	-0.0124	-0.0319***	-0.00208
	(0.00805)	(0.00964)	(0.00735)	(0.00861)	(0.00977)	(0.00323)
$ChainFacility$	0.0220***	0.0371***	0.0153**	0.0165**	0.0289***	0.000906
	(0.00720)	(0.00859)	(0.00644)	(0.00773)	(0.00871)	(0.00291)
Facility Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Resident Composition	Yes	Yes	Yes	Yes	Yes	Yes
Local Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Local Outcome	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	State	State	State	State	State	State
Observations	13,396	13,396	13,396	13,396	13,396	13,396
Mean	0.27	0.43	0.21	0.34	0.51	0.02
Std. Dev.	0.44	0.50	0.40	0.47	0.50	0.13

Notes: The table presents estimates of a logistic regression model relating facility ownership and COVID-19 outcomes. The outcome "Confirmed" indicates that the facility had at least one individual with a laboratory positive COVID-19 test, while "Suspected" indicates that the facility had at least one individual with a suspected case. (All confirmed cases are also considered suspected.) "Deaths" indicates that the facility had at least one death of an individual suspected to have COVID-19. The first and last three columns correspond to resident and staff outcomes, respectively. In addition to ownership variables, controls include facility characteristics, resident composition, and local characteristics as described in Section 2.4. Measures of local COVID-19 prevalence are the analogous average of the dependent variable for all other local facilities within 10-, 20-, and 30-kilometer radiuses surrounding each facility. We allow different intercepts for facilities missing data on a control variable. Standard errors are provided in parenthesis. Significance at the 10%, 5%, 1% level are indicated using *, **, * respectively.

Table A.2: PE ownership and personal protective equipment shortages

	N95 Masks	Surgical Masks	Eye Wear	Gowns	Gloves	Sanitizer
PE	-0.0643***	-0.0760***	-0.0481***	-0.0705***	-0.0334**	-0.0233
	(0.0213)	(0.0200)	(0.0180)	(0.0222)	(0.0144)	(0.0148)
$PriorPE$	0.0536***	0.0298***	0.0225***	0.0729***	0.0443***	0.0397***
	(0.0102)	(0.00738)	(0.00784)	(0.0103)	(0.00526)	(0.00629)
$For\text{-}Profit$	0.0674***	0.0598***	0.0503***	0.0746***	0.0346***	0.0374***
	(0.00868)	(0.00722)	(0.00736)	(0.00917)	(0.00623)	(0.00659)
$ChainFacility$	0.0220***	0.00515	0.00856	0.0186**	0.00569	0.0113**
	(0.00727)	(0.00543)	(0.00574)	(0.00762)	(0.00456)	(0.00497)
Facility Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Resident Composition	Yes	Yes	Yes	Yes	Yes	Yes
Local Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Local Outcome	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	State	State	State	State	State	State
Observations	13,396	13,396	13,396	13,396	13,396	13,396
Mean	0.17	0.09	0.10	0.19	0.05	0.07
Std. Dev.	0.38	0.28	0.30	0.40	0.23	0.25

Notes: The table presents estimates of a logistic regression model relating facility ownership and availability of PPE supplies. The outcome "N95 Masks" indicates that the facility reported to lacking a sufficient supply of N95 masks to last for one week. Similarly, the outcome "Surgical Masks" indicates that the facility reported to lacking a sufficient supply of surgical masks for one week. Other dependent variables are defined analogously. Independent variables are defined in Table A.1 and described further in Section 2.4. Standard errors are provided in parenthesis. Significance at the 10%, 5%, 1% level are indicated using ***, **, * respectively.

B Identifying Facility Ownership

In this section, we describe our procedure to determine the ownership type of nursing facilities in our data. We classify each facility as either currently PE-owned, previously PE-owned, or never PE-owned. In addition, we identify whether facilities are organized as for-profit or non-profit, and whether they operate as standalone facilities or as part of a larger multi-facility chain (from LTCFocus.org).

To identify which nursing facilities are currently PE-owned, we use several databases of financial transactions, including PitchBook, Preqin, Irving Levin Associates, S&P Capital IQ, and SDC Platinum. From each of these databases, we obtain lists of mergers and acquisitions in the nursing home industry and determine whether the purchaser or investor was a private equity firm. This process results in a list of historical PE acquisitions of nursing home facilities and facility chains. Next, we connect PE deals to their associated facilities using facility names from LTCFocus.org and the names of parent organizations (chains) from the POS files. To determine which facilities are still PE-owned, and which have since transitioned ownership, we use information from the deal databases, supplemented by web searches. We explain this procedure further in greater detail below.

B.1 Identifying PE-Owned Facilities

We first use PitchBook, a data provider focusing on private capital markets, to identify the set of facilities currently PE-owned. First, we obtain a list of U.S. nursing home companies in the database by requiring the company's location to be listed as "United States," and filtering according to the following key words: "nursing home," "skilled nursing facility," "nursing facility," "skilled nursing home," "rehabilitation center," "senior care," "nursing services," "acute care," and "skilled nursing care." To identify the subset of firms with PE ownership, we then employ the following criteria:

1. The most recent financing status must be listed as "PE-Backed" (as of June 4, 2020).
2. The company must have been acquired through a transaction categorized as "Buyout," with a corresponding deal date prior to January 1, 2020.
3. The status of the associated deal establishing PE ownership must be listed as "Completed."
4. Investments by PE firms classified as "growth capital" are excluded, as PE investors typically obtain *minority* ownership (and hence, no control rights) in such deals.

If a firm experiences multiple PE acquisitions during its lifetime, we use the most recent transaction associated with that company. Applying these filters results in 305 companies. Next, we manually verify that the resulting list of companies includes only skilled nursing facilities and exclude other similar providers such as assisted living facilities. We subsequently connect the names of these companies with facilities in the federal COVID-19 database. To do so, we match nursing home company names obtained from PitchBook to the facility names and names of facility parent organizations (chains) provided in the December 2019 Provider of Services (POS) file.[1] When

[1]PE acquisitions of nursing home chains are sometimes classified in PitchBook as "add-on" deals. These deals specify firms who exhibit changes in ownership but are not directly acquired by (or do not directly receive investment from) a PE investor; instead, such firms may be acquired by another company that is already PE-owned. In such instances, the acquired firm is classified as the "target," and the acquiring company is classified as the "platform." If a PE deal associated with a nursing company is not listed as an add-on deal, we match facility names according to the name of the target. However, when an associated transaction is described as an add-on deal, we match according to

PitchBook's company description or deal description provides the location of acquired facilities, we validate our matches by confirming the addresses of facilities present in the POS file.

To capture any additional facilities acquired by PE investors but unclassified in PitchBook, we consider the duration of investments identified in the historical list of PE acquisitions, supplemented by transactions identified in Gandhi, Song and Upadrashta (2020), who study nursing home acquisitions between 1993 and 2016. For each deal in this earlier set, we attempt to identify whether the nursing company is still owned by the associated PE firm, or whether the firm has since exited the investment. This also helps us identify facilities that were previously—but are not currently—owned by PE investors. To collect information regarding PE firms' exits of their nursing home investments, we use three methods. For deals listed in the Preqin database, we utilize the "Investment Status" field, which notes whether a transaction is "Active" or "Realised" (as of June 2020). For deals in the PitchBook database, we consider whether the financing status of the nursing home company is described as "PE-Backed" or "Formerly PE-Backed." For all other deals, we conduct manual Internet searches to make assessments regarding ownership status.

When we can verify that a PE investor continues to maintain ownership and control of the nursing company, we include the facilities associated with such investments as PE-owned in our sample (i.e. the indicator "PE" equals 1). If instead, we verify that a transaction is inactive, we classify the associated facilities as previously PE-owned. In particular, we create an indicator variable ("PriorPE") that equals 1 if a nursing facility was previously acquired by a PE investor who subsequently exited their investment prior to January 1, 2020. In the instances in which we can neither identify the investment exit date or substantiate that the investment is still active, we assume a holding period of 7.5 years, the median level in our sample (for all transactions with identifiable exits). We categorize the corresponding facilities as previously PE-owned if the acquisition date is more than 7.5 years ago. For facilities that undergo multiple PE investments, we use the date of the last PE investment to determine exit behavior and ownership status. Overall, we successfully identify 391 facilities as presently PE-owned and 1,219 nursing facilities for which a PE investor both invested in and exited prior to January 1, 2020.

the names of both the target and platform companies. The latter criterion helps ensure that we also capture cases in which nursing home companies are indirectly PE controlled because the corresponding platform company is financed by a PE investor that obtains majority ownership.

C Additional Tables

C.1 County-Level Fixed Effects

In this section, we provide estimates corresponding to alternate specifications of our primary models. Each of these regressions include county-level fixed effects (rather than state-level fixed effects) and exclude the local averages of COVID-19 outcomes (defined analogously to the dependent variable) from the set of controls. Other local characteristics, including county-level population and COVID-19 incidence as well as locally defined HHI measures, are still included as controls. Due to computational challenges with estimating logistic regression models with high-dimensional fixed effects, we instead provide estimates of the corresponding linear probability models. Table C.1 presents the results corresponding to alternate specifications of Figures 2 and 3, and Table C.2 presents the results corresponding to an alternate specification of Figure 4. Importantly, we again find that PE ownership is associated with reduced likelihoods of cases among residents and staff, as well as a reduced likelihood of PPE shortages.

C.2 Total COVID-19 Prevalence

We next provide estimates for an additional specification measuring the *intensive* margin of COVID-19 prevalence (total counts of cases and deaths). As in the regressions underlying Figures 2 and 3, we include state-level fixed effects as well as local averages of COVID-19 outcomes in a facility's surrounding area (corresponding to 10-, 20-, and 30-km bandwidths around each facility). As the outcome variables are no longer binary however, we present estimates of the corresponding ordinary least squares (OLS) model. All counts of cases and deaths are transformed using the inverse hyperbolic sine function, where $\operatorname{arsinh} x = \ln\left(x + \sqrt{x^2+1}\right)$. Doing so approximates the natural logarithm transformation, but allows for retaining facilities with zero cases or deaths. See Burbidge, Magee and Robb (1988) and MacKinnon and Magee (1990) for further information.

As in Figures 2 and 3, we again find PE ownership to be associated with a lower prevalence of confirmed and suspected COVID-19 cases among both residents and nursing staff. In addition, previous PE ownership is associated with an increased likelihood of COVID-19 cases and deaths among residents. For both current and prior PE ownership, the results are both larger and more statistically robust for residents than for staff.

Table C.1: PE ownership and presence of any reported COVID-19 cases

	Residents			Staff		
	Confirmed	Suspected	Deaths	Confirmed	Suspected	Deaths
PE	-0.0731***	-0.128***	-0.0232	-0.0482*	-0.0880***	0.000534
	(0.0223)	(0.0270)	(0.0198)	(0.0252)	(0.0292)	(0.00617)
$PriorPE$	0.0239	0.0160	0.0222	0.0102	-0.00366	0.00334
	(0.0146)	(0.0163)	(0.0138)	(0.0152)	(0.0162)	(0.00537)
$For\text{-}Profit$	0.0317***	0.00744	0.0285***	-0.0158	-0.0410***	-0.00432
	(0.00984)	(0.0116)	(0.00910)	(0.0106)	(0.0117)	(0.00351)
$ChainFacility$	0.0256***	0.0427***	0.0165**	0.0268***	0.0419***	0.00120
	(0.00890)	(0.0102)	(0.00809)	(0.00945)	(0.0102)	(0.00290)
Facility Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Resident Composition	Yes	Yes	Yes	Yes	Yes	Yes
Local Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Local Outcome	No	No	No	No	No	No
Fixed Effects	County	County	County	County	County	County
Observations	13,396	13,396	13,396	13,396	13,396	13,396
Mean	0.27	0.43	0.21	0.34	0.51	0.02
Std. Dev.	0.44	0.50	0.40	0.47	0.50	0.13
R^2	0.42	0.37	0.42	0.43	0.37	0.17

Notes: The table presents linear probability model estimates from regressions of survey responses of COVID-19 outcomes for residents and staff on private equity ownership and other facility characteristics. For each facility, we retain only the first survey response, in order to capture a facility's condition before additional resources may have been targeted to facilities based on these initial responses. For residents, the outcome "Confirmed" indicates that the facility had at least one laboratory positive COVID-19 resident, "Suspected" indicates that the facility had at least one suspected or laboratory positive COVID-19 resident, and "Deaths" indicates that the facility had at least one death of an individual suspected to have had COVID-19. Outcome variables for nursing staff are defined analogously. Control variables include the facility characteristics, resident composition, and location-specific characteristics measures described in Section 2.4, but measures of local COVID-19 prevalence (averages of the dependent variable) in nearby facilities are excluded from the set of controls. In addition, each specification includes county-level fixed effects. Standard errors are provided in parenthesis. Significance at the 10%, 5%, 1% level are indicated using ***, **, * respectively.

Table C.2: PE ownership and insufficiency of personal protective equipment

	N95 Masks	Surgical Masks	Eye Wear	Gowns	Gloves	Sanitizer
PE	-0.0582**	-0.0490***	-0.0400**	-0.0501**	-0.0222	-0.0149
	(0.0242)	(0.0151)	(0.0186)	(0.0241)	(0.0144)	(0.0153)
$PriorPE$	0.0653***	0.0367***	0.0251**	0.0917***	0.0663***	0.0577***
	(0.0151)	(0.0118)	(0.0119)	(0.0162)	(0.0111)	(0.0118)
$For\text{-}Profit$	0.0545***	0.0482***	0.0404***	0.0649***	0.0230***	0.0250***
	(0.00908)	(0.00690)	(0.00718)	(0.0100)	(0.00539)	(0.00624)
$ChainFacility$	0.0199**	0.00281	0.00616	0.0200**	0.00479	0.0113**
	(0.00806)	(0.00603)	(0.00637)	(0.00879)	(0.00502)	(0.00557)
Facility Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Resident Composition	Yes	Yes	Yes	Yes	Yes	Yes
Local Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Local Outcome	No	No	No	No	No	No
Fixed Effects	County	County	County	County	County	County
Observations	13,396	13,396	13,396	13,396	13,396	13,396
Mean	0.17	0.09	0.10	0.19	0.05	0.07
Std. Dev.	0.38	0.28	0.30	0.40	0.23	0.25
R^2	0.26	0.26	0.27	0.25	0.24	0.23

Notes: The table presents linear probability model estimates from regressions of survey responses regarding PPE supplies on private equity ownership and other facility characteristics. For each facility, we retain only the first survey response, in order to capture a facility's condition before additional resources may have been targeted to facilities based on these initial responses. The dependent variable "N95 Masks" equals 1 if the nursing home reports lacking a sufficient supply of N95 masks to last at least one week. Similarly, the outcome "Surgical Masks" indicates whether the facility lacks a one-week supply of surgical masks. Other dependent variables are defined analogously. Control variables include the facility characteristics, resident composition, and location-specific characteristics measures described in Section 2.4, but measures of local PPE access (averages of the dependent variable) in nearby facilities are excluded from the set of controls. In addition, each specification includes county-level fixed effects. Standard errors are provided in parenthesis. Significance at the 10%, 5%, 1% level are indicated using ***, **, * respectively.

Table C.3: PE ownership and number of reported COVID-19 cases

	Residents			Staff		
	Confirmed	Suspected	Deaths	Confirmed	Suspected	Deaths
PE	-0.130**	-0.284***	-0.0130	-0.0839*	-0.150**	0.000704
	(0.0577)	(0.0690)	(0.0374)	(0.0479)	(0.0631)	(0.00474)
$PriorPE$	0.107**	0.0952**	0.0547*	0.0266	-0.0302	0.00347
	(0.0417)	(0.0470)	(0.0282)	(0.0336)	(0.0394)	(0.00375)
$For\text{-}Profit$	0.0954***	0.0503*	0.0459**	-0.0154	-0.126***	-0.00115
	(0.0260)	(0.0304)	(0.0180)	(0.0222)	(0.0283)	(0.00238)
$ChainFacility$	0.0709***	0.0885***	0.0353**	0.0416**	0.0372	0.000680
	(0.0243)	(0.0277)	(0.0164)	(0.0202)	(0.0251)	(0.00206)
Facility Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Resident Composition	Yes	Yes	Yes	Yes	Yes	Yes
Local Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Local Outcome	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	State	State	State	State	State	State
Observations	13,396	13,396	13,396	13,396	13,396	13,396
Mean	0.79	1.20	0.46	0.76	1.23	0.01
Std. Dev.	1.50	1.67	1.01	1.26	1.48	0.11
R^2	0.31	0.28	0.31	0.32	0.25	0.05

Notes: The table presents results of OLS regressions relating facility ownership and COVID-19 outcomes. The outcome "Confirmed" is the inverse hyperbolic sine of the number of laboratory positive COVID-19 individuals reported, while "Suspected" is the inverse hyperbolic sine of the number of suspected COVID-19 individuals (all confirmed individuals are also considered suspected). "Deaths" is the inverse hyperbolic sine of the number of reported deaths of individuals suspected to have had COVID-19. The first and last three columns correspond to resident and staff outcomes, respectively. In addition to ownership variables, controls include facility characteristics, resident composition, and local characteristics as described in Section 2.4. Standard errors are provided in parenthesis. Significance at the 10%, 5%, 1% level are indicated using ***, **, * respectively.

D Investigating Limitations in Data Quality

One of the key limitations of the CMS COVID-19 Nursing Home Dataset is that facilities were responsible for self-reporting statistics to the CDC National Healthcare Safety Network (NHSN) system. First, we investigate whether the decision to report *any* COVID-19 data to the NHSN system was driven by PE ownership. Among CMS-certified facilities included in the POS files and the LTCFocus.org database, we find an extremely small difference in probability of reporting statistics: PE-owned facilities were 0.18 percentage points less likely to report data to the NHSN system, a non-significant distinction [$p = 0.903$] (Table D.2).

However, if PE-owned facilities systematically over- or under-reported COVID-19 case (and death) counts to the NHSN system, our regression estimates will nonetheless be biased. We therefore replicate our analysis using facility-level outbreak data from 30 states' departments of health collected by Abrams et al. (2020). Though lacking national scope and neither as rich nor as standardized as the NHSN, the state data have two advantages. First, they provide coverage for a period before NHSN reporting was required. Second, state health departments may have had greater ability to monitor or verify reporting than CMS. Figure 2 and Table C.3 provide our estimates using these data. Importantly, we confirm our main result, observing again that PE-owned facilities are less likely (by 6 percentage points, on average) to report outbreaks among residents than non-PE facilities [$p = 0.003$] (Table D.1).

Furthermore, we investigate reporting discrepancies between the federal- and state-administered outbreak databases and compare the likelihood of discrepancy for PE and non-PE facilities. We construct analogous variables in the state data to measure (1) whether facilities had any cases, and (2) conditional on having cases, how many cases they had. We observe that PE-owned facilities are slightly less likely to self-report an outbreak when one is present in state data, but are more likely to self-report a greater number of cases than present in the state data (confirmed cases among residents). Neither result is statistically significant however [$p = 0.208$ and 0.278, respectively], suggesting no systematic biases resulting from self-reporting in the CMS-published federal statistics (Table D.1).

In addition, we perform two tests to assess whether survey incompleteness is likely to significantly bias our results (Table D.2). First, we compare the reported occupancy rates of PE-owned

Table D.1: PE ownership and discrepancies between state and federal data

	Replication of Result in State Data		Discrepancy in Datasets (State − Federal)	
	Any Confirmed	Total Confirmed	Any Confirmed	Total Confirmed
PE	-0.0598***	-1.937***	0.0312	-0.937
	(0.0204)	(0.642)	(0.0247)	(0.863)
$PriorPE$	0.0316**	0.890	-0.0119	-0.822
	(0.0157)	(0.728)	(0.0184)	(0.785)
$For\text{-}Profit$	0.0122	1.357***	-0.0241**	0.0852
	(0.00949)	(0.356)	(0.0114)	(0.452)
$ChainFacility$	0.00156	0.760**	-0.0157	0.171
	(0.00843)	(0.326)	(0.0101)	(0.406)
Facility Characteristics	Yes	Yes	Yes	Yes
Resident Composition	Yes	Yes	Yes	Yes
Local Characteristics	Yes	Yes	Yes	Yes
Local Outcome	No	No	No	No
Fixed Effects	State	State	State	State
Observations	8,774	8,774	8,774	8,774
Mean	0.26	6.28	-0.05	-0.68
Std. Dev.	0.44	17.61	0.46	18.56
R^2	0.37	0.35	0.16	0.13

Notes: This table presents results using state COVID-19 data from Abrams et al. (2020). The first two columns test for an association between PE ownership and confirmed COVID-19 cases among residents, replicating (via OLS) the underlying analysis for any confirmed cases in Figure 2 (column 1) and total confirmed cases in Table C.3 (column 2). The final two columns investigate whether discrepancies between case counts in the state and federal NHSN data vary according to PE ownership: column 3 compares discrepancies in whether any case is reported; column 4 compares discrepancies in the number of cases reported. In these columns, positive values suggest a facility's federal statistics are under-reported relative to the state data; negative values suggest federal statistics over-report relative to state data. In addition to ownership variables, controls include facility characteristics, resident composition, and local characteristics as described in Section 2.4. Standard errors are provided in parenthesis. Significance at the 10%, 5%, 1% level are indicated using ***, **, * respectively.

facilities to other non-PE facilities. Consider that if PE-owned facilities experienced disproportionate outbreaks that resolved prior to May 1, then their occupancy rates would likely be depressed due to deaths and re-hospitalizations from the prior outbreak. Counter to this hypothesis however, we find that PE-owned facilities are, on average, 0.72 percentage points *more* full [$p = 0.385$] during their first COVID-19 survey (statistically significant only when controlling for other ownership types). Second, we compare the likelihood of non- or low-quality survey responses for PE and non-PE facilities. We find that the probability of submitting a response passing the quality assurance check is only slightly (0.70 percentage points) lower for PE-owned facilities, and the difference is not statistically significant [$p = 0.583$].

Table D.2: PE ownership, reporting, and survey incompleteness

	Reporting Data to NHSN	Occupancy Percentage	Data Passed Quality Check
Panel A: *t*-tests			
PE	-0.00179	0.717	-0.00698
	(0.0147)	(0.825)	(0.0127)
Panel B: OLS regressions			
PE	-0.00880	2.334***	-0.00851
	(0.0148)	(0.841)	(0.0129)
$PriorPE$	0.000142	-0.795	-0.0164**
	(0.00862)	(0.525)	(0.00795)
$For\text{-}Profit$	0.00672	-4.574***	-0.0163***
	(0.00568)	(0.687)	(0.00438)
$ChainFacility$	0.0158***	-1.423***	0.0136***
	(0.00526)	(0.512)	(0.00433)
Controls	No	No	No
Observations	14,877	13,398	13,620
Mean	0.90	74.08	0.94
Std. Dev.	0.30	28.28	0.23

Notes: Panel A reports results from *t*-tests evaluating whether measures of survey incompleteness differ by PE ownership. Column 1 is a *t*-test assessing whether likelihood of reporting federal COVID-19 data to NHSN differs by PE ownership. Column 2 is a *t*-test assessing whether facility occupancy differs by PE ownership. Column 3 is a *t*-test assessing whether the likelihood of passing CMS quality assurance checks for reported data differs by PE ownership. Panel B reports results from OLS regressions relating measures of survey incompleteness and PE ownership. Column 1 assesses whether the likelihood of reporting federal COVID-19 data to NHSN differs by ownership type. Column 2 assesses whether facility occupancy differs by ownership type. Column 3 assesses whether the likelihood of passing CMS quality assurance checks for reported data differs by ownership type. Standard errors are provided in parenthesis. Significance at the 10%, 5%, 1% level are indicated using ***, **, * respectively.

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Original Investigation | Health Policy

Association Between Hospital Private Equity Acquisition and Outcomes of Acute Medical Conditions Among Medicare Beneficiaries

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Abstract

IMPORTANCE As private equity (PE) acquisitions of short-term acute care hospitals (ACHs) continue, their impact on the care of medically vulnerable older adults remains largely unexplored.

OBJECTIVE To investigate the association between PE acquisition of ACHs and access to care, patient outcomes, and spending among Medicare beneficiaries hospitalized with acute medical conditions.

DESIGN, SETTING, AND PARTICIPANTS This cross-sectional study used a generalized difference-in-differences approach to compare 21 091 222 patients admitted to PE-acquired vs non–PE-acquired short-term ACHs between January 1, 2001, and December 31, 2018, at least 3 years before to 3 years after PE acquisition. The analysis was conducted between December 28, 2020, and February 1, 2022. Differences were estimated using both facility and hospital service area fixed effects. To assess the robustness of findings, regressions were reestimated after including fixed effects of patient county of origin to account for geographic differences in underlying health risks. Two subset analyses were also conducted: (1) an analysis including only hospitals in hospital referral regions with at least 1 PE acquisition and (2) an analysis stratified by participation in the Hospital Corporation of America 2006 acquisition. The study included Medicare beneficiaries 66 years and older who were hospitalized with 1 of 5 acute medical conditions: acute myocardial infarction (AMI), acute stroke, chronic obstructive pulmonary disease exacerbation, congestive heart failure exacerbation, and pneumonia.

EXPOSURES Acquisition of hospitals by PE firms.

MAIN OUTCOMES AND MEASURES Comorbidity burden (measured by Elixhauser comorbidity score), hospital length of stay, in-hospital mortality, 30-day mortality, 30-day readmission, and 30-day episode payments.

RESULTS Among 21 091 222 total Medicare beneficiaries admitted to ACHs between 2001 and 2018, 20 431 486 patients received care at non–PE-acquired hospitals, and 659 736 received care at PE-acquired hospitals. Across all admissions, the mean (SD) age was 79.45 (7.95) years; 11 727 439 patients (55.6%) were male, and 4 550 012 patients (21.6%) had dual insurance; 2 996 560 (14.2%) patients were members of racial or ethnic minority groups, including 2 085 128 [9.9%] Black and 371 648 [1.8%] Hispanic; 18 094 662 patients (85.8%) were White. Overall, 3 083 760 patients (14.6%) were hospitalized with AMI, 2 835 777 (13.4%) with acute stroke, 3 674 477 (17.4%) with chronic obstructive pulmonary disease exacerbation, 5 868 034 (27.8%) with congestive heart failure exacerbation, and 5 629 174 (26.7%) with pneumonia. Comorbidity burden decreased slightly among patients admitted with acute stroke (difference, −0.04 SDs; 95% CI, −0.004 to −0.07 SDs) at acquired hospitals compared with nonacquired hospitals but was unchanged across the other 4 conditions. Among patients with AMI, a greater decrease in in-hospital mortality was observed in

(continued)

Key Points

Question What is the association between private equity (PE) acquisition of short-term acute care hospitals and measures of comorbidity, mortality, readmission, length of stay, and spending among Medicare beneficiaries admitted to the hospital with 1 of 5 acute medical conditions?

Findings In this cross-sectional study of more than 21 million Medicare beneficiaries with 5 different acute medical conditions who were hospitalized at short-term acute care hospitals, PE acquisition was associated with significantly lower inpatient mortality (−1.1 percentage points) and lower 30-day mortality (−1.4 percentage points) among patients admitted with acute myocardial infarction. However, PE acquisition was not associated with significant differences in other dimensions of quality and spending or with differences across other medical conditions.

Meaning The study's findings suggest that PE acquisition has mixed consequences for patient-level outcomes overall but is associated with moderate and consistent improvement in mortality among Medicare beneficiaries hospitalized with acute myocardial infarction.

+ Supplemental content

Author affiliations and article information are listed at the end of this article.

Abstract (continued)

PE-acquired hospitals compared with non–PE-acquired hospitals (difference, −1.14 percentage points, 95% CI, −1.86 to −0.42 percentage points). In addition, a greater decrease in 30-day mortality (difference, −1.41 percentage points; 95% CI, −2.26 to −0.56 percentage points) was found at acquired vs nonacquired hospitals. However, 30-day spending and readmission rates remained unchanged across all conditions. The extent and directionality of estimates were preserved across all robustness assessments and subset analyses.

CONCLUSIONS AND RELEVANCE In this cross-sectional study using a difference-in-differences approach, PE acquisition had no substantial association with the patient-level outcomes examined, although it was associated with a moderate improvement in mortality among Medicare beneficiaries hospitalized with AMI.

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Introduction

Private equity (PE) participation in the health care sector has increased over the past 20 years,[1-3] with approximately one-half of these transactions occurring in the care delivery sector (physicians, hospitals, and nursing homes).[4] Acute care hospitals (ACHs) are particularly attractive to PE firms; approximately 11% of all nongovernmental hospital discharges in 2017 were from a facility with a history of PE ownership.[5] Private equity firms' sustained interest in hospitals likely reflects several factors: (1) perceived inefficiencies that provide opportunities to improve operations,[2,6] (2) an aging population that will require more acute care services,[7,8] and (3) fragmented hospital markets that make horizontal consolidation a possible way to increase negotiating power over payers.[9-11] These for-profit incentives have raised concerns about their consequences for the provision of health care services and the patient-practitioner relationship. The American College of Physicians recently disseminated a position paper calling for greater regulatory transparency and "longitudinal research on the effect of private equity investment on physicians' clinical decision making, health care prices, access, and patient care, including the characteristics of models that may have adverse or positive effects on the quality and cost of care and the patient-physician relationship."[12]

Recent work by Gupta et al[13] estimated that PE ownership of nursing homes increased short-term mortality among Medicare patients by 10%, with concomitant reductions in other measures of patient well-being. In contrast, Braun et al[14] estimated that, despite no consistent impact for spending or procedural volume, prices paid to dermatology practices increased by 3% to 5% after PE acquisition. However, short-term ACHs differ markedly from both of these subsectors. Despite substantial interest from policy makers noted in the Medicare Payment Advisory Commission's June 2021 report,[15] few studies have analyzed the association of PE acquisitions of ACHs with spending and clinical outcomes. Bruch et al[11] identified modest but statistically significant improvements in risk-adjusted hospital-level quality measures for acute myocardial infarction (AMI) and pneumonia using data collected through the Centers for Medicare & Medicaid Services Hospital Compare program. The inconclusive findings of studies that have used aggregate quality measures and cost-to-charge ratios suggest the need for patient-level investigations to understand whether patient selection and/or differences in clinical practice bias are associated with changes in outcomes.

To address this knowledge gap, we examined the association between PE acquisition of short-term ACHs and outcomes among Medicare beneficiaries over an 18-year period using a difference-in-differences framework. We specifically sought to quantify the association of PE acquisitions using 6 important measures that encompass overall patient case mix and hospital clinical performance: comorbidity burden, inpatient mortality, 30-day mortality, 30-day readmission, inpatient length of stay (LOS), and 30-day episode spending.[16] We examined this association across 5 common medical conditions: AMI, acute stroke, chronic obstructive pulmonary disease (COPD), congestive heart failure (CHF), and pneumonia. These 5 conditions account for a substantial portion of nonelective admissions, both broadly and among Medicare beneficiaries in particular.[17,18]

Methods

Study Population

We used the 100% Centers for Medicare & Medicaid Services standard analytic files and enrollment database to identify Medicare fee-for-service beneficiaries hospitalized between January 1, 2001, and December 31, 2018. The analysis was conducted between December 28, 2020, and February 1, 2022. This study was approved by the institutional review board of Duke University Medical Center. Informed consent was waived due to the deidentified nature of the data. This study followed the Strengthening the Reporting of Observational Studies in Epidemiology (STROBE) reporting guideline for cross-sectional studies.[19]

We identified patients 66 years and older who were admitted via the emergency department with a principal diagnosis of 1 of the following 5 conditions: AMI, acute stroke, CHF exacerbation, COPD exacerbation, and pneumonia. Medical conditions were identified using diagnostic codes from the *International Classification of Diseases, Ninth Revision*, and the *International Statistical Classification of Diseases and Related Health Problems, Tenth Revision* (eTable 1 in the Supplement). These cohorts were considered independently, and previously published protocols to classify index admissions and readmissions were used.[20] Admissions classified as elective were excluded. To ensure that patients had at least 1 year of Medicare enrollment before hospital admission, those 65 years and younger were excluded.

Demographic information, including age, sex, race and ethnicity, and entitlement type, were abstracted from the beneficiary summary file. Comorbidities were identified using all admissions in the year before and up to the index admission and were summarized using Elixhauser comorbidity scores[21,22]; this approach was used because Centers for Medicare & Medicaid Services hierarchical conditions category risk scores were first implemented in 2004,[23] and our period of interest included the years before its implementation.[24]

Definition of Outcomes

Primary outcomes evaluated at the patient level included comorbidity burden (measured by Elixhauser comorbidity scores), in-hospital mortality, 30-day mortality (death within 30 days of admission), hospital LOS, 30-day all-cause readmission, and total inpatient spending per 30-day care episode. Hospital LOS and 30-day all-cause readmission were conditional on being discharged alive.

Hospital Characteristics

Hospitals acquired by PE firms via primary or add-on leveraged buyout between 2003 and 2015 were identified using previously described methods.[5] These hospitals were linked to financial data reported on Healthcare Cost Report Information System–Medicare Cost Reports and geographic location information (eg, physical address) contained in the Medicare provider of services files to determine patient county of origin, hospital service area (HSA), and hospital referral region. All non–federally owned hospitals with noncritical access that were not acquired by PE firms during this period were considered potential controls. Critical access hospitals were excluded because of their small inpatient bed count (<25 beds) and exemption from traditional reimbursement (ie, cost reimbursement rather than a prospective payment system) and fee structures. Hospital-level factors included size (<100 beds, 100-299 beds, or ≥300 beds), ownership type (for-profit, nonprofit, or government-run), teaching status (teaching vs nonteaching), medical school affiliation (affiliated vs unaffiliated), and core-based statistical area designation (metropolitan, micropolitan, or outside of a core-based statistical area designation).

Statistical Analysis

The association of PE acquisition with patient outcomes was estimated using a generalized difference-in-differences approach covering a minimum of 3 years in the preacquisition (baseline) period and a 3-year limitation in the postacquisition period, using the interaction term between an indicator for PE acquisition and the 3-year period after acquisition. Our postacquisition horizon of 3 years was chosen to match the exit strategies (eg, divestment or secondary buyouts by another PE firm) commonly used by PE firms, which are not readily disclosed or identifiable in the public domain.

The following patient-level covariates were included in our model: age, sex, race and ethnicity (Asian or Pacific Islander, Black, Hispanic, North American Native, White, or other or unknown race and/or ethnicity), dual eligibility, type of entitlement (age, disability, or end-stage kidney disease), admission type (emergency or urgent), hospitalization within the previous year, and Elixhauser comorbidity score. Hospital fixed effects were included to account for time-invariant hospital-specific unobserved confounders; fixed effects for HSA by year were included to control for region-specific time trends, which encompassed either changes in overall hospital quality, legislation, or treatment standards introduced by clinical guidelines. Binary outcomes were estimated using a linear probability model, and continuous outcomes (LOS and total payments) were log-transformed and right winsorized at the 99th percentile to mitigate skewness (ie, values >99th percentile were set to the value of the 99th percentile).

In this specification, the difference-in-differences estimator can be interpreted as the difference in outcomes among patients within hospitals after PE acquisition, after adjustment for patient-level factors. Standard errors were clustered at the hospital level. We controlled for false discovery rate in the primary analyses using the Benjamini and Hochberg method, and we reported corrected *P* values alongside uncorrected *P* values.[25] The full model specification is provided in eMethods in the Supplement, and results of the preparatory analyses using an event study framework to examine parallel trends are available in eFigures 1 to 6 in the Supplement. All analyses were conducted using Stata SE software, version 16.0 (StataCorp LLC), and a 2-tailed $P < .05$ was considered statistically significant.

Although our model specification allowed for a conservative estimate of the consequences of PE acquisition at the patient level, it may not have fully accounted for patient selection based on unobservable factors. In other words, differences detected within hospitals after PE acquisition may, in fact, have been associated with unobserved differences among patients admitted to those hospitals after acquisition. Therefore, we also considered an alternative specification that included fixed effects for a patient's county of origin to account for geographic variation in health care access, social factors associated with health, and demographic factors that might have been associated with any noted differences in outcomes.[26-28]

We further examined 2 alternative specifications of the study cohort to assess whether the directionality of our estimates was consistent. First, because PE acquisitions are concentrated in specific geographic regions (eg, southeastern US) based on published literature,[5] we repeated our analyses after restricting the sample to hospital referral regions in which at least 1 PE acquisition had occurred. Second, we stratified the sample of PE-acquired hospitals into 2 groups: members of the Hospital Corporation of America (HCA) health care system and all other hospitals. This decision was motivated by the fact that the 2006 HCA leveraged buyout by PE firms accounted for more than 50% of the hospitals in the sample, and stratification by HCA status was consistent with the approach used in recent PE scholarship.[11] We repeated each generalized difference-in-differences model and excluded, in turn, HCA hospitals and non-HCA hospitals from the treatment group.

Results

After accounting for observations in all years of our study period (2001-2018), a total of 21 091 222 care episodes were included across 3559 hospitals (257 of which were acquired by PE firms and had at least 3 years of data before and after acquisition; 11 hospitals closed within 3 years after acquisition). Overall, 20 431 486 episodes occurred at non–PE-acquired hospitals, and 659 736 occurred at PE-acquired hospitals. Across all admissions, the mean (SD) age was 79.45 (7.95) years; 11 727 439 patients (55.6%) were male, 9 363 783 (44.4%) were female, and 4 550 012 (21.6%) had dual insurance. Among the total of 21 091 222 patients, 2 996 560 (14.2%) were members of racial and ethnic minority groups (246 014 [1.2%] were Asian or Pacific Islander, 2 085 128 [9.9%] were Black, 371 648 [1.8%] were Hispanic, 73 348 [0.3%] were North American Native, and 220 422 [1.0%] were of unknown race and/or ethnicity), and 18 094 662 patients (85.8%) were White. A total of 3 083 760 patients (14.6%) were admitted with AMI, 2 835 777 (13.4%) with acute stroke, 5 868 034 (27.8%) with CHF exacerbation, 3 674 477 (17.4%) with COPD exacerbation, and 5 629 174 (26.7%) with pneumonia. Patient-level summary statistics across hospitals acquired and never acquired by PE firms are provided in the **Table**. Patient-level summary statistics across each of the 5 conditions (with data on age, sex, race and ethnicity, and Elixhauser comorbidity scores) at the beginning and end of our study period and annual rates for each outcome studied (in-hospital mortality, 30-day mortality, 30-day readmission, LOS, and spending) are provided in eTable 2 in the Supplement. Of note, unadjusted LOS and in-hospital mortality decreased across the study period for all 5 conditions. The proportion of patients receiving treatment across hospital types, including teaching vs nonteaching, for-profit vs nonprofit, and metropolitan vs micropolitan status are available in eTable 3 in the Supplement.

Table. Characteristics of Patients Treated in Hospitals Acquired and Not Acquired by PE Firms

Characteristic	No. (%) Non–PE-acquired hospitals	PE-acquired hospitals
Total patients, No.	20 431 486	659 736
Sex		
Female	9 074 055 (44.4)	289 728 (43.9)
Male	11 357 431 (55.6)	370 008 (56.1)
Age, mean (SD), y	80.0 (8.0)	79.0 (8.0)
Race and ethnicity[a]		
Racial and ethnic minority groups[b]	2 888 883 (14.2)	107 677 (16.3)
White	17 542 603 (85.8)	552 059 (83.7)
Elixhauser comorbidity score, mean (SD)	2.08 (2.95)	2.16 (3.00)
Condition		
AMI	2 990 957 (14.6)	92 803 (14.1)
Acute stroke	2 756 284 (13.5)	79 493 (12.0)
CHF exacerbation	5 674 250 (27.8)	193 784 (29.4)
COPD exacerbation	3 556 147 (17.4)	118 330 (17.9)
Pneumonia	5 453 848 (26.7)	175 326 (26.6)
Hospital size		
<100 beds	2 072 976 (10.1)	41 784 (6.3)
100-299 beds	7 827 556 (38.3)	342 686 (51.9)
≥300 beds	10 530 954 (51.5)	275 266 (41.7)
Hospital teaching status		
Teaching	9 739 995 (47.7)	222 070 (33.7)
Nonteaching	10 691 491 (52.3)	437 666 (66.3)
Hospital ownership		
For-profit	1 677 888 (8.2)	463 867 (70.3)
Nonprofit	15 955 329 (78.1)	157 882 (23.9)
Government-run	2 798 269 (13.7)	37 987 (5.8)
Core-based statistical area designation		
Metropolitan	17 358 393 (85.0)	596 186 (90.4)
Micropolitan	2 360 853 (11.6)	49 847 (7.6)
Outside of core-based statistical area designation	712 240 (3.5)	13 703 (2.1)

Abbreviations: AMI, acute myocardial infarction; CHF, congestive heart failure; COPD, chronic obstructive pulmonary disease; PE, private equity.

[a] Percentages for non–PE-acquired hospitals were calculated based on 20 431 486 total patients.

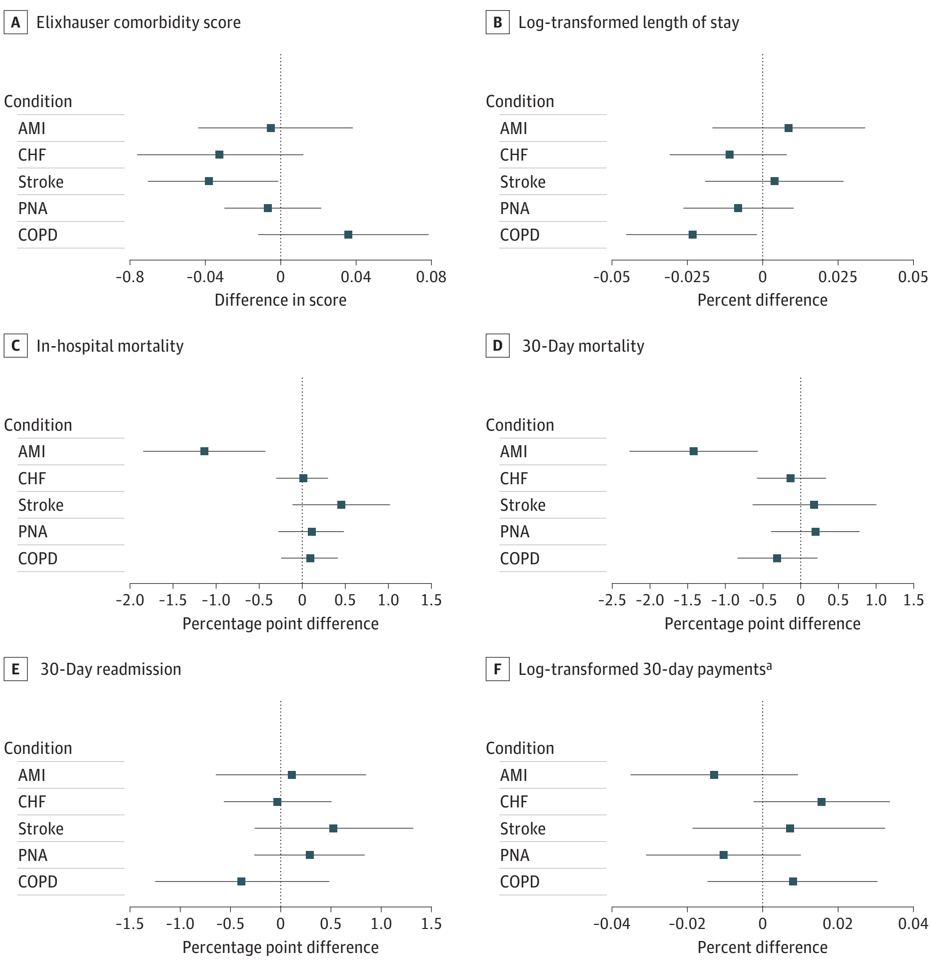
[b] Racial and ethnic minority group category includes 246 014 patients who identified as Asian or Pacific Islander, 2 085 128 who identified as Black, 371 648 who identified as Hispanic, 73 348 who identified as North American Native, and 220 422 who were of unknown race and/or ethnicity.

Patient Selection and Clinical Outcomes After PE Acquisition

The age at admission decreased slightly among patients hospitalized with pneumonia at PE-acquired hospitals compared with non–PE-acquired hospitals (difference, −0.20 SDs; 95% CI. −0.34 to −0.06 SDs; uncorrected *P* = .006; corrected *P* = .07) but was unchanged for the 4 other conditions examined. After PE acquisition, comorbidity burden decreased slightly among patients admitted with acute stroke (difference, −0.04 SDs; 95% CI, −0.004 to −0.07 SDs; uncorrected *P* = .03; corrected *P* = .24) at acquired hospitals compared with nonacquired hospitals but was unchanged across the other 4 conditions (**Figure 1**).

Among patients admitted with AMI, a greater decrease in in-hospital mortality was observed among PE-acquired hospitals compared with non–PE-acquired hospitals (difference, −1.14 percentage points; 95% CI, −1.86 to −0.42 percentage points; uncorrected *P* = .002; corrected *P* = .03). In addition, after PE acquisition, a −1.41 percentage point (95% CI, −2.26 to −0.56 percentage points; uncorrected *P* = .001; corrected *P* = .03) greater decrease in 30-day mortality was found at acquired hospitals compared with nonacquired hospitals. For the 4 other conditions examined, there were no differences in in-hospital mortality or 30-day mortality after PE acquisition. No differences in LOS were found among patients hospitalized with AMI, acute stroke, CHF, or pneumonia; patients admitted with COPD exacerbation had slightly shorter adjusted LOS after PE acquisition (difference, −2.34%; 95% CI, −4.52% to −0.15%; uncorrected *P* = .04; corrected *P* = .25), although this difference was not statistically significant after adjustment for false discovery rate. No

Figure 1. Difference-in-Differences Estimates for Primary Outcomes Across All 5 Medical Conditions



Whiskers represent 95% CIs. AMI indicates acute myocardial infarction; CHF, congestive heart failure; COPD, chronic obstructive pulmonary disease; and PNA, pneumonia.

[a] Total inpatient payments per 30-day care episode.

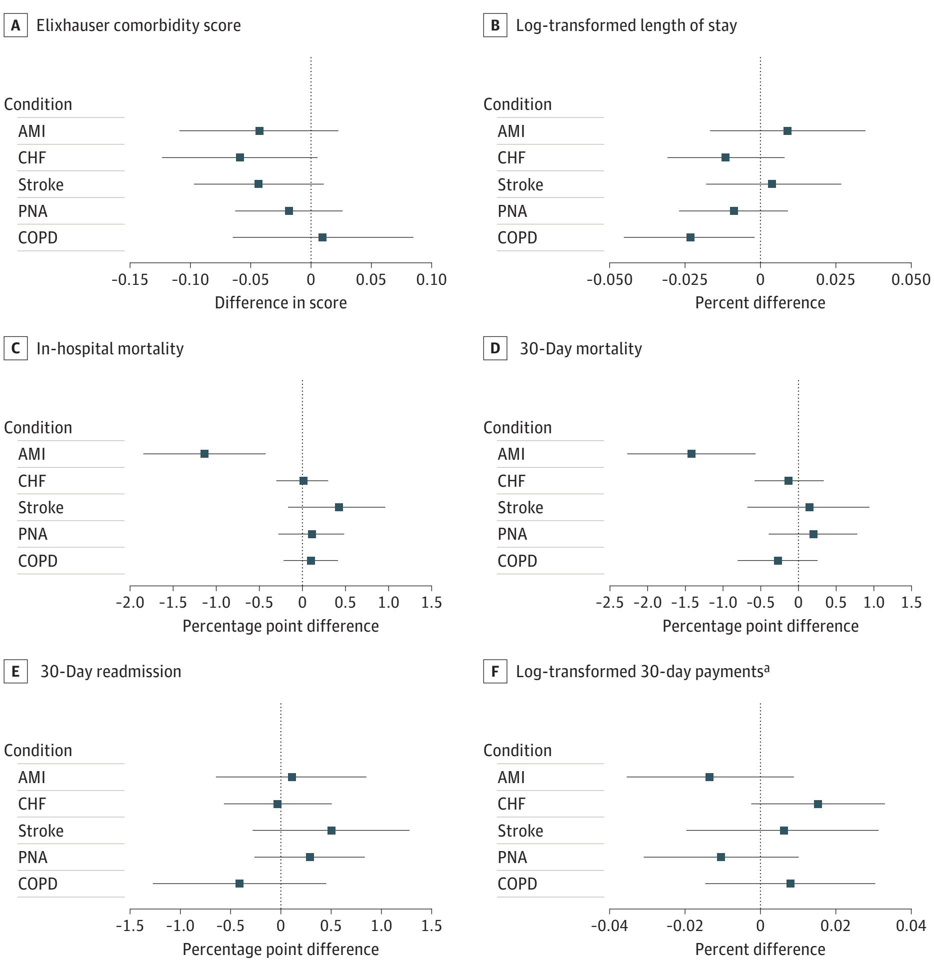
differences in 30-day readmission and 30-day episode spending across all 5 conditions were noted (Figure 1).

Robustness Assessments and Subset Analyses

When fixed effects for beneficiary county of residence were included in addition to hospital fixed effects and hospital HSA-year fixed effects, difference-in-differences estimates of changes in in-hospital mortality (difference, −1.14 percentage points; 95% CI, −1.86 to −0.42 percentage points) and 30-day mortality (difference, −1.41 percentage points; 95% CI, −2.26 to −0.55 percentage points) among patients with AMI were consistent with the analysis using only hospital fixed effects and hospital HSA-year fixed effects (**Figure 2**). Moreover, when year fixed effects were included in lieu of hospital HSA-year fixed effects, difference-in-differences estimates among patients admitted with AMI were consistent with those obtained using HSA-year fixed effects; specifically, decreases of 0.74 percentage points (95% CI, −1.19 to −0.29 percentage points) in in-hospital mortality and 0.94 percentage points (95% CI, −1.51 to −0.37 percentage points) in 30-day mortality were observed. Hospital LOS among patients admitted with COPD exacerbation was similarly shorter (difference, −1.88%; 95% CI, −3.40% to −0.33%), although the clinical importance of this was not clear.

After restricting the patient cohort to those who received treatment at hospitals in hospital referral regions with at least 1 PE acquisition, patients admitted with AMI had a greater decrease in in-hospital mortality (difference, −1.12 percentage points; 95% CI, −1.89 to −0.45 percentage points) and 30-day mortality (difference, −1.41 percentage points; 95% CI, −2.26 to −0.57 percentage points)

Figure 2. Difference-in-Differences Estimates After Including Fixed Effects of Patient County



Whiskers represent 95% CIs. AMI indicates acute myocardial infarction; CHF, congestive heart failure; COPD, chronic obstructive pulmonary disease; and PNA, pneumonia.

[a] Total inpatient payments per 30-day care episode.

after PE acquisition. In addition, LOS among patients admitted with COPD exacerbation similarly decreased (difference, −2.43%; 95% CI, −4.57% to −0.30%), although the extent of the estimate was small (eFigure 7 in the Supplement).

After partitioning hospitals based on whether they were part of the 2006 HCA acquisition, the directionality of significant findings remained consistent. Among HCA hospitals, patients with AMI experienced a greater decrease in in-hospital mortality (difference, −1.33 percentage points; 95% CI, −2.19 to −0.47 percentage points) and 30-day mortality (difference, −1.40 percentage points; 95% CI, −2.39 to −0.40 percentage points). Estimates for these 2 outcomes in non-HCA hospitals were similar in extent but not statistically significant (AMI in-hospital mortality: −0.59 percentage points [95% CI, −1.96 to 0.78 percentage points]; 30-day mortality: −1.10 percentage points [95% CI, −2.74 to 0.54 percentage points]). Among non-HCA hospitals, LOS among patients admitted with COPD exacerbation was lower after acquisition (difference, −4.29%; 95% CI, −8.26% to −0.32%), as was LOS among patients admitted with CHF (difference, −4.51%; 95% CI, −7.72% to −1.29%) (eFigure 8 in the Supplement).

Discussion

This cross-sectional study used a difference-in-differences framework to evaluate the quality of care at PE-owned ACHs relative to ACHs without a history of PE acquisition. The impact of PE acquisition for case selection and clinical quality at short-term ACHs remains relevant to ongoing policy discussions aimed at promoting greater value in health care spending. Among Medicare beneficiaries, PE acquisition was associated with consistent improvements in both in-hospital and 30-day mortality among patients with AMI, with comparable overall spending. We found a small but likely clinically nonmeaningful decrease in LOS among patients admitted with COPD exacerbation and no difference in 30-day spending or 30-day readmission for all 5 conditions studied. We did not find any evidence of systematic upcoding (ie, submission of diagnostic codes for services more expensive than those actually provided) or increased intensity in comorbidity coding (ie, more frequent comorbidity coding and/or submission of higher comorbidity scores) in the postacquisition period. These findings were largely corroborated by 2 separate robustness assessments, which included specifications accounting for any unobserved variation over time or between patients' socioeconomic status. These findings also persisted in subset analyses that restricted the study cohort to patients who received treatment in hospital referral regions that had any PE activity, and the directionality of our results persisted when hospitals were stratified by their participation in the 2006 HCA leveraged buyout.

This study's findings were inconsistent with the prevailing concerns surrounding PE acquisitions of health care systems, perhaps highlighting the need for nuanced investigations into the role of for-profit investments in health care. Regulators may determine that it is not sufficient that for-profit institutions do no harm; they may instead decide that for-profit owners produce improvements in value of care, either through better outcomes, lower costs, or both. Proponents of PE acquisition often assert that revenue generation from target hospitals via taxation is a societal boon, while the patients they serve may benefit from economies of scale, management expertise, and an incentive to implement cost-effective care.[5,7,29-31] Critics assert that PE firms, unlike other for-profit institutions, have an inherent incentive to favor short-term returns rather than long-term investments (eg, information technologies and care redesign) that would otherwise meaningfully improve population health.[29,32] In certain aspects of health care services, the latter view has been bolstered by important research[13,14,33,34] in nursing homes and outpatient clinical practices. Private equity–owned nursing facilities performed comparably, both in terms of equipment shortages and resident outbreaks, during the COVID-19 pandemic.[14,33] However, cost-cutting measures that led to unsafe staffing ratios resulted in worse patient outcomes,[13] and increased market power has made outpatient practices more costly for payers.[34] Concerns about similar impacts in hospitals are therefore justified. Rather than focus on PE acquisitions of hospitals as a distinct problem in the health care delivery sector, perhaps PE activity might be viewed as a proxy for market failures and payment loopholes that can be exploited (eg, surprise billing [unexpected charges from out-of-network hospitals or practitioners], horizontal consolidation, Medicare payment differentials for physician-administered drugs under Part B, and Medicare Advantage upcoding for benchmark payments [in which enrollees' potentially comorbid diagnoses are recorded to increase risk-adjusted payments]).[35]

Our findings with respect to the improvements in AMI care after PE acquisition were consistent with those of Bruch et al.[11] Acute MI may represent an ideal clinical condition for targeted quality improvement and care redesign efforts by PE ownership because of (1) clear guidelines from the American College of Cardiology/American Heart Association on the proper management of patients with AMI,[36] (2) a well-understood association between guideline adherence and improved outcomes,[37] and (3) the resource-intensive nature of AMI identification and treatment (eg, diagnostic imaging, cardiac catheterization, and percutaneous coronary intervention).[38]

There are several possible explanations for our findings. First, the variation in management practices and capital allocation across PE firms may result in substantively different operational changes after acquisition and may therefore have variable consequences for outcomes. Another explanation may be that all short-term ACHs, PE-acquired or not, are subject to the same regulatory oversight, accreditation, and quality reporting environment.[39] Although PE ownership has been associated with operational changes,[40] it is possible that extant regulatory tolerance for adverse outcomes prevents drastic cost-cutting measures that result in worse clinical care. At the same time, there may not be clear avenues for quality improvement in the 4 other medical conditions examined in the present study. Quality measures for those conditions have several benchmarking approaches that are not as universally accepted or as easily identifiable in Medicare claims (eg, oxygen assessment, antimicrobial timing, and appropriate initial antimicrobial selection for pneumonia).[41,42] Within the broader literature examining hospital for-profit conversions and their association with clinical outcomes, our results were also consistent with work by Joynt et al,[32] which found no difference in 30-day risk-adjusted mortality rates and process quality factors associated with AMI, CHF, and pneumonia across 237 hospitals that converted to for-profit status between 2003 and 2010.

Limitations

This study has several limitations. First, we consider all PE acquisitions to be the same type of exposure. Although PE firms structure their investments in different ways, we focused on leveraged buyouts in this study because they are the most common type, and this focus allowed us to avoid confounding in our analysis that could have occurred by including other deal structures. However, the amount of debt burden passed on to acquired hospitals by PE firms (ie, the financial obligation resulting from the deal that has implications for the extent of cost-cutting or revenue increases required to remain solvent) varies and is often not accessible in the public domain. Moreover, oversight by state regulators and overall investment practices of each firm may vary, which could result in nonuniform consequences for each acquisition.

Second, although we consider a broad set of medical conditions and quality measures, we are unable to capture all dimensions of care quality.[43] Third, a difference-in-differences analysis in which the treatment exposure is a nonrandom event (ie, PE acquisition) may be subject to selection bias.[43] Cross-sectional analyses of PE-owned hospitals have revealed that PE firms have lower staffing levels for a standard measure of patient burden.[44] We posit that the nature of nonelective admissions renders our findings less subject to patient selection or practitioner-related discrimination; future studies can further assess these differences in the likelihood of acquisition to estimate the impact of PE acquisition for changes in elective admissions.[45]

Fourth, our study was conducted entirely among Medicare beneficiaries; although subtle shifts in the total proportion of Medicare patients may be observable, profit-seeking via changes in patient access (ie, guiding Medicare or uninsured patients away from inpatient care after PE acquisition or reducing visits or readmissions among high-risk patients with complex conditions) and the consequences of vertical integration (as with the Medicare Advantage program) cannot be easily measured. Fifth, we examined a short period (3 years) after PE acquisition to avoid overstating or misassigning the consequences of PE ownership by incorporating data after the inevitable sale (exit) by the PE firm, which usually occurs at 5 to 7 years after acquisition. Therefore, our findings cannot be generalized to the longer-term consequences of PE acquisition.[46]

Conclusions

In this cross-sectional study, acquisition of short-term ACHs by PE firms was associated with modest improvements in measures of mortality for AMI, with no changes in mortality outcomes across 4 other medical conditions that account for a large proportion of nonelective admissions among Medicare beneficiaries. These findings challenge the narrative that PE investments in all subsectors of health care delivery organizations increase health spending and systematically worsen quality. We believe these findings may further motivate longitudinal research on the consequences of PE acquisition for physician decision-making, access to care, and health care prices.[12] Preference-sensitive elective admissions with clinical management and profitability that are more dependent on patient selection represent a rich clinical context for this inquiry.

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Financial Impacts And Operational Implications Of Private Equity Acquisition Of US Hospitals

ABSTRACT Although private equity acquisition of short-term acute care hospitals purportedly improves efficiency and cost-effectiveness, financial performance after acquisition remains unexamined. We compared changes in the financial performance of 176 hospitals acquired during 2005–14 versus changes in matched control hospitals. Acquisition was associated with a $432 decrease in cost per adjusted discharge and a 1.78-percentage-point increase in operating margin. The majority of acquisitions—134 members of the Hospital Corporation of America, acquired in 2006—were associated with a $559 decrease in cost per adjusted discharge but no change in operating margin. Conversely, non-HCA hospitals exhibited a 3.27-percentage-point increase in operating margin without a concomitant change in cost per adjusted discharge. When we examined markers of hospital capacity, operational efficiency, and costs, we found that private equity acquisition was associated with decreases in total beds, ratio of outpatient to inpatient charges, and staffing (total personnel and nursing full-time equivalents and total full-time equivalents per occupied bed). Therefore, financial performance improved after acquisition, whereas patient throughput and inpatient utilization increased and staffing metrics decreased. Future research is needed to identify any unintended trade-offs with safety and quality.

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Private equity firms have increased their participation in the health care sector in recent years (to nearly $79 billion in 2019), with the provider subsector (physicians, hospitals, and laboratory facilities) accounting for the majority of deals.[1] Plausible explanations include prevalent operational inefficiencies;[2,3] an anticipated increase in demand for health care due to an aging population;[4,5] and fragmented hospital markets, which create opportunities for horizontal consolidation and gains in market power.[6–10]

Private equity advocates often claim that acquisition provides management expertise to hospitals and funding for capital improvements that ultimately improve patient care.[5,11] In contrast, private equity firms have been criticized both because their acquisitions often burden hospitals with considerable debt and because of their overriding incentive to generate returns for investors within three to seven years.[7] This, in turn, might discourage long-term investments that are required for meaningful gains in population health.[12] It also is unclear whether private equity firms, compared with other types of hospital owners, prioritize metrics that drive financial outcomes (that is, payer mix, service volume, and unit prices) at the expense of service, educational, and academic mission obligations.[13]

For these reasons, the impact of private equity acquisition on hospital financial performance, health care spending, and patient care outcomes remains of interest to the broad stakeholder community (for example, researchers, policy makers, patients, and hospital administrators) in health care.[2,4] Despite the significant research characterizing private equity acquisitions in other sectors (health information technology, pharmaceuticals, and outpatient physician practices),[14–17] the changes in the organization, financing, and delivery of health care after private equity acquisition of hospitals remain relatively unexplored. Recent descriptive work has shown that hospitals with a history of private equity ownership are geographically concentrated yet account for 11 percent of all discharges in the US.[7] The relationship between these hospitals' overall financial outcomes and the scope and quality of their clinical practice is not yet fully understood, however. On the one hand, hospitals that underwent private equity acquisition saw significant increases in hospital net income, with concomitant increases in total daily inpatient charges, total charge-to-cost ratio, and case-mix index compared with control hospitals.[6] On the other hand, private equity acquisition has been associated with significant shifts toward more profitable service lines.[18] Whether these shifts—to higher charges, markups, and patient comorbidity and clinical complexity along with a marked pivot to services that generate more revenue relative to costs—also trigger changes to staffing or capital deployment remains unknown.

Our aim is to deepen understanding of the relationship between private equity hospital ownership and observable changes in hospital operational and financial performance. This is a critical considering the associated economic strain on health systems due to decreased revenues from elective admissions during the COVID-19 pandemic. We hypothesize that private equity acquisition will lead to operational changes in acquired hospitals that enhance their financial performance and lower the cost structure for care delivery.

Study Data And Methods

STUDY DESIGN The impact of private equity acquisition on hospital financial performance and patient care costs was evaluated using a matched difference-in-differences approach. Hospitals acquired by private equity between 2005 and 2014 were identified using a previously described search strategy.[7] These hospitals were then matched to control hospitals drawn from a pool of nonacquired hospitals. Three-year periods preceding and following private equity acquisition were used for pre-post comparisons; the year of acquisition was excluded to mitigate potential bias from carryover effects.

DATA SOURCES Hospitals and health systems that were acquired by private equity were collated from several business intelligence reporting platforms (Pitchbook, Zephyr, and CB Insights).[7] The date of ownership transfer was designated as "time zero" (that is, the index year) in our analytic framework. All transactions were independently confirmed in the lay press, corporate media releases, and governmental filings. All hospitals belonging to a private equity–acquired health system at time zero were confirmed using the SYSID variable of the American Hospital Association (AHA) Annual Survey for the relevant year. If a hospital was acquired more than once during the study period, only the first acquisition was considered for analysis.

Acute care hospitals (excluding specialty or surgery-specific hospitals) to be used as matched controls were identified in Centers for Medicare and Medicaid Services (CMS) Healthcare Cost Report Information System (HCRIS) cost reports, using the Medicare provider number taxonomy. Hospitals were excluded from the pool of potential controls if they were acquired by private equity between 2000 and 2017, if they were previously owned by management corporations that themselves had been private equity owned (for example, Community Health Systems), or if they lacked three years of cost data before and after a potential time zero.

OUTCOME MEASURES Data on hospital financial performance were abstracted from HCRIS forms 2552-96 and 2552-10 (as appropriate) for the relevant periods before and after private equity acquisition.[19,20] These data included cost per adjusted discharge, operating margin, capitalization ratio, and liquidity ratio and are defined formally in online appendix exhibit A-1.[21] Briefly, as hospitals vary in their total outpatient-to-inpatient mix, discharges are scaled by a factor reflecting the ratio of outpatient charges to inpatient charges. Operating margin reflects the percentage of total revenue that the hospital keeps after deducting the costs of patient care. The capitalization ratio, which reflects the extent to which a hospital is leveraged, was defined as the ratio of fund balances to total assets and is reported at the hospital level. Liquidity, a measure of a hospital's ability to meet cash obligations, was calculated as the ratio of current assets to total liabilities (and thus a higher number indicates better financial health).[19] These two measures were calculated using previously defined formulas to better contextualize the study's ultimate findings. To limit the influence of outliers, capitalization and liquidity were winsorized below and above the 2.5th and 97.5th per-

Our findings add nuances to the existing literature on for-profit transitions in hospital ownership structure.

centiles, respectively.[19] All dollar amounts were adjusted to 2017 US dollars, using the Medical Care Consumer Price Index from the Bureau of Labor Statistics.

HOSPITAL MATCHING A propensity score to match potential control hospitals to private equity–acquired hospitals in the index year (time zero) was constructed using the following features: teaching status, system membership (yes or no), total discharges, cost per adjusted discharge, operating margin, percentage Medicare discharges, percentage Medicaid discharges, ratio of outpatient to inpatient charges, transfer-adjusted case-mix index, bed size, wage index, census division, and urban or rural location. These features were obtained from HCRIS cost reports, the CMS impact file, and the AHA Annual Survey. These matching specifications are consistent with the empirical strategy described by Matt Schmitt in his examination of horizontal consolidation of hospitals and its impact on potential cost savings.[20] Hospitals are selectively identified by private equity firms for acquisition; these firms consider aspects such as size, location, system membership, and relative financial strength. Although including outcomes in the propensity score model has the potential to introduce bias, we purposefully matched only on features in the year of acquisition, which was accounted for by an indicator variable in the difference-in-differences estimation.

Stepwise selection was used to identify factors predicting private equity acquisition including year, census division, urban or rural location, total discharges, total number of acute inpatient beds, and ratio of outpatient to inpatient charges. In addition, because private equity firms have been known to target hospitals by their financial health and managerial structure, we also included membership in a hospital system, operating margins, and cost per adjusted discharge in the deal year in the propensity score model. Private equity–acquired and control hospitals were matched by the propensity of private equity acquisition, with an exact match on index year, census division, and urban or rural location. Balance in hospital characteristics before and after the propensity match was examined using absolute standardized mean differences, which allows for comparisons across variables of difference scales and units.[22]

STATISTICAL ANALYSIS The model specification of the difference-in-differences analysis is described in the appendix, along with the methodology for examining parallel pre trends.[21] Hospital fixed effects were included to account for unobservable, time-invariant features that differed across facilities. A state-specific effect was interacted with a linear time index to account for state-specific trends over time. If parallel pre trends could not be confirmed, we included an interaction term between the private equity–acquired hospital indicator and year[23,24] to allow us to account for different time trends in private equity–acquired and control hospitals. Last, we conducted subgroup analyses of hospitals included in the 2006 acquisition of the Healthcare Corporation of America (HCA), as this acquisition accounted for the majority of our analytic sample (134 of 176 hospitals).[6,7]

To further explore how gains in operational efficiency might account for specific results, we examined contemporaneous changes in patient mix and services.[20,25] This entailed separate difference-in-differences analyses for the following hospital characteristics: discharges (adjusted and total), bed size, ratio of outpatient to inpatient charges, total inpatient days, total personnel full-time equivalents (FTEs), and total nurse (that is, registered nurse [RN] and licensed practical nurse [LPN]) FTEs.[26] For each variable, the model residuals were examined by a normal quantile plot. Where appropriate, the variable was log-transformed, and regression estimates were reported as percentage changes in outcomes. Given the multiple difference-in-differences analyses performed in this study, we corrected the p values of difference-in-differences estimates to control the false discovery rate, using the linear step-up method described by Yoav Benjamini and Yosef Hochberg.[27] All analyses were performed using SAS Enterprise Guide, version 7.15. All statistical tests were two-sided, and p values <0.05 were considered statistically significant.

LIMITATIONS The study had several limitations. First, because of the nature of private equity–backed deals, information on them is rarely comprehensive. The data used in this study were derived from publicly available sources but might not fully capture the extent of these investments. Second, the motivation for each acquisition may differ, as do the specific features of the acquired hospital, and this motivation could not be readily assessed or accounted for in compiling our study sample (for example, whether or not hospital system membership is attractive to private equity firms). Although some of these hospitals may be seen as high-performing assets at the time of acquisition, others may be experiencing continual financial losses and are acquired in the course of corporate restructuring. Third, our analytic approach incorporated data aggregated at the hospital level and was susceptible to bias from unmeasured confounders. Finally, our study did not examine the long-term effects (if any) of private equity firms' eventual sale or liquidation of a hospital on the financial health of the hospital itself or its clinical outcomes.

Study Results

Overall, we identified 184 private equity–acquired hospitals acquired between 2005 and 2014, along with 2,757 HCRIS cost reports for potential matching. Of these hospitals, 176 were ultimately matched one to one to a control hospital (appendix exhibits A-2 and A-3).[21] Key hospital features and their absolute standardized differences in hospital features before and after matching are noted in exhibit 1 and appendix exhibit A-4.[21]

FINANCIAL PERFORMANCE AND HEALTH CARE COSTS Parallel pre trends (that is, in the three-year period preceding acquisition for both private equity–acquired and matched controls) held for models examining cost per adjusted discharge, operating margin, and liquidity ratio. Private equity acquisition was associated with a decrease in cost per adjusted discharge (−$432) and an increase in operating margin (+1.78 percentage points). We did not find a significant association between private equity acquisition and the change in liquidity ratio; similarly, no difference was found in the adjusted analysis for capitalization ratio (exhibit 2).

SENSITIVITY ANALYSES

▸ **STRATIFICATION BY HCA MEMBERSHIP:** To assess whether the above findings for our outcomes of interest (financial performance and health care costs) after private equity acquisition were disproportionately influenced by the 2006 acquisition of HCA hospitals, we conducted stratified analyses. Given the results of the test for parallel trends assumption in the initial pooled analysis, we included an interaction between the private equity–acquired hospital indicator and year for capitalization ratio. Overall, private equity acquisition of HCA hospitals was associated with a $559 decrease in cost per adjusted discharge, although no significant difference was noted for non-HCA hospitals. Conversely, operating margin increased after acquisition for non-HCA hospitals (+3.27 percentage points), but not for HCA hospitals. Finally, among non-HCA hospitals, the liquidity ratio increased 21.09 percentage points. Despite significant estimated increases in capitalization ratio for both HCA and non-HCA hospitals, these were nonsignificant after adjustment for nonparallel trends (exhibit 2).

EXHIBIT 1

Characteristics of private equity (PE)–acquired hospitals and unacquired controls, before and after propensity score matching, 2005–14

	Mean before match		Mean after match		ASD	
Characteristics	**PE hospitals**	**Controls**	**PE hospitals**	**Controls**	**Before match**	**After match**
Total discharges (no.)	10,489	10,643	10,416	9,835	0.016	0.079
Cost per adjusted discharge[a]	9.33	9.32	9.33	9.33	0.038	0.017
Operating margin (%)	0.05	−0.01	0.05	0.02	0.310	0.276
Transfer-adjusted case-mix index	1.41	1.40	1.41	1.41	0.049	0.007
Total beds	208.1	201.9	204.2	195.2	0.038	0.064
% Medicare discharges	38.42	38.12	38.18	38.15	0.024	0.003
% Medicaid discharges	13.32	14.29	13.00	13.51	0.091	0.052
Outpatient to inpatient charges[a]	−0.52	−0.04	−0.48	−0.40	0.805	0.154
Wage index	0.97	0.99	0.97	0.97	0.090	0.044
Medicare-affiliated teaching hospital (%)	27.7	35.5	27.8	31.3	0.167	0.075
Member of hospital system (%)	98.4	54.7	98.3	98.3	1.202	0.000

SOURCE Data from Healthcare Cost Report Information System Medicare cost reports and the American Hospital Association Annual Survey. **NOTES** All hospitals were short-term acute care facilities. Matching performed with exact match on index (acquisition) year, census division, and urban or rural location. Additional details are in the appendix (see note 21 in text). ASD is absolute standardized mean differences (difference in means in units of the pooled standard deviation). [a]These characteristics are reported after natural logarithmic transformation (logarithm in base *e*).

EXHIBIT 2

Impact of private equity acquisition on financial outcomes across all matched hospitals and by membership in Hospital Corporation of America (HCA) at acquisition, 2005–14

Financial outcomes and samples	Matched pairs (no.)[a]	Standard method		Standard method after adjustment for trends	
		Estimate	95% CI	Estimate	95% CI
Cost per adjusted discharge					
Entire sample	176	−431.6[b,****]	−622.2, −241.0	—[c]	—[c]
HCA	134	−559.3[b,****]	−796.8, −321.8	—[c]	—[c]
Non-HCA	42	−225.0[b]	−523.6, 73.6	—[c]	—[c]
Operating margin					
Entire sample	172	0.0178***	0.0063, 0.0294	—[c]	—[c]
HCA	131	0.0131*	−0.0002, 0.0263	—[c]	—[c]
Non-HCA	41	0.0327**	0.0064, 0.0590	—[c]	—[c]
Capitalization ratio					
Entire sample	173	0.1733****	0.1224, 0.2241	−0.0282	−0.1204, 0.0640
HCA	132	0.1944****	0.1290, 0.2597	−0.0530	−0.1675, 0.0615
Non-HCA	41	0.0871**	0.0200, 0.1541	0.0515	−0.0660, 0.1690
Liquidity ratio					
Entire sample	86	0.0626	−0.0398, 0.1650	—[c]	—[c]
HCA	51	−0.0155	−0.1370, 0.1060	—[c]	—[c]
Non-HCA	35	0.2109**	0.0224, 0.3993	—[c]	—[c]

SOURCE Difference-in-differences analysis conducted by authors using data from Healthcare Cost Report Information System (HCRIS) Medicare cost reports. **NOTES** All hospitals were short-term acute care. Asterisks reflect *p* values adjusted for multiple testing. [a]Hospitals were excluded from specific analyses in instances where the denominators were not reported on HCRIS Medicare cost reports. [b]In 2017 dollars. [c]Not applicable. $*p < 0.10$ $**p < 0.05$ $***p < 0.01$ $****p < 0.001$

▶ **OPERATIONAL CHANGES:** We conducted a similar difference-in-differences analysis of common measures of hospital capacity, patient volume, and operational expenditures to further contextualize the findings above. After private equity acquisition, hospital total bed count decreased by 2.79 percent, or approximately 4.43 beds; the ratio of outpatient to inpatient charges significantly decreased by 4.58 percent, or 2.91 percentage points. Measures of staff and labor costs as a function of FTEs also decreased. Total personnel FTEs decreased by 5.05 percent, which was equivalent to 36.97 FTEs, and total nursing FTEs decreased by 4.38 percent, which was equivalent to 10.52 FTEs, after private equity acquisition. No significant differences were observed in total discharges or total adjusted discharges (exhibit 3 and appendix exhibit A-5).[21] In addition, difference-in-differences estimates for total personnel FTEs per occupied bed decreased after private equity acquisition (−0.50 FTEs; appendix exhibit A-6).[21] To determine whether the decrease in costs per adjusted discharge was driven by decreases in FTEs, we examined costs per adjusted discharge after adjustment for total personnel FTEs, finding that the estimates were not significantly attenuated (exhibit 4).

Discussion

Private equity activity in the health care delivery sector, particularly in the acquisition of short-term acute care hospitals, is substantial.[7] Using a matched difference-in-differences approach, we found that private equity acquisition precipitated improvements in operating margin (+1.78 percentage points) and a concomitant cost savings of $432 per adjusted discharge. A key challenge in interpreting evaluations of financial performance is appreciating the relative contributions of gains in operational efficiency, patient selection, or service mix to higher-margin or higher-revenue activities, or cuts to the costs of providing care outlined above. With respect to each of these, we identified the following shifts: Private equity acquisition was associated with a small (2.79 percent) decrease in total beds at the hospital level, without any concomitant changes in total inpatient days. This may suggest slightly higher patient throughput, and its importance is underscored when considered in relation to changes in staffing metrics.

In addition, private equity acquisition was associated with a 4.58 percent decrease in the ratio of outpatient to inpatient charges. This runs contrary to the fact that in recent decades, US hospitals have undergone a shift toward greater use of outpatient settings, which are less resource-intensive relative to inpatient care.[28] A possible

EXHIBIT 3

Impact of private equity acquisition on hospital characteristics, 2005–14

Hospital characteristics[a]	Matched pairs (no.)[b]	Standard method		Standard method after adjustment for trends	
		Estimate	95% CI	Estimate	95% CI
Beds	174	−0.0279**	−0.0495, −0.0063	—[c]	—[c]
Adjusted discharges	176	−0.0352***	−0.0554, −0.0151	0.0043	−0.0324, 0.0411
Total discharges	176	−0.0127	−0.0348, 0.0093	0.0245	−0.0158, 0.0648
Outpatient to inpatient charges	176	−0.0458****	−0.0668, −0.0247	—[c]	—[c]
Total inpatient days	176	−0.0332*	−0.0653, −0.0010	—[c]	—[c]
Total personnel FTEs	169	−0.0505****	−0.0729, −0.0281	—[c]	—[c]
Total RN and LPN FTEs	169	−0.0438***	−0.0720 to −0.0156	—[c]	—[c]

SOURCE Difference-in-differences analysis conducted by authors using data from Healthcare Cost Report Information System (HCRIS) Medicare cost reports (beds, adjusted discharges, total discharges, outpatient to inpatient charges, and total inpatient days) and the American Hospital Association Annual Survey (total personnel full-time equivalents [FTEs] and registered nurse [RN] and licensed practical nurse [LPN] FTEs). **NOTES** All hospitals were short-term acute care. Asterisks reflect *p* values adjusted for multiple testing. [a]All values were estimated after natural logarithmic transformation (logarithm in base *e*) to determine percent changes due to acquisition. [b]In instances where total personnel FTEs, total beds, or total RN or LPN values were not reported or reported inaccurately, the institutions were not included in the specific analysis. [c]Not applicable. *$p < 0.10$ **$p < 0.05$ ***$p < 0.01$ ****$p < 0.001$

explanation is more aggressive price negotiation with commercial payers for inpatient services relative to outpatient services after private equity acquisition.[29] If this is true, the magnitude of cost savings identified by our model is conservative: Private equity–acquired hospitals may in fact have lower cost per discharge than what is estimated.[20]

Finally, and most saliently, we identified a 5.05 percent decrease in total personnel FTEs and a 4.38 percent decrease in nursing-specific FTEs, which points to a pattern of strategic reorganization of staffing. This decrease outpaced any reductions in clinical burden, as total personnel FTEs per occupied bed also declined after private equity acquisition (tantamount to one staff member for every two occupied beds). This is notable because health care is a distinctly labor-intensive industry, and labor accounts for the greatest share of hospital expenses (up to 50 percent by some estimates).[30] A strategic reduction in total personnel and nursing FTEs might indicate how private equity firms control hospital expenditures and enhance their financial position. However, we found that the decline in total costs per adjusted discharge was not attenuated after adjustment for total personnel FTEs, which suggests that hospitals cut costs in other dimensions—not only labor—after private equity acquisition. This echoes findings from the long-term care industry, as nursing homes acquired by private equity decreased staffing at multiple levels (with appreciable differences in outcomes).[31,32]

Our findings also add nuances to the existing literature on for-profit transitions in hospital ownership structure. For example, Karen Joynt and colleagues found that hospitals converting

EXHIBIT 4

Impact of private equity acquisition on cost per adjusted discharge, before and after adjustment for total employee full-time equivalents (FTEs), across all matched hospitals and by membership in Hospital Corporation of America (HCA) at acquisition, 2005–14

Hospital samples	Matched pairs (no.)	Unadjusted for total FTEs		Adjusted for total FTEs	
		Estimate	95% CI	Estimate	95% CI
Entire sample	169[a]	−520.3****	−708.5, −332.1	−510.9****	−699.5, −322.4
HCA	129	−700.9****	−934.0, −467.9	−691.3****	−924.4, −458.1
Non-HCA	40	−186.9	−488.9, 115.1	−184.1	−487.4, 119.1

SOURCE Difference-in-differences analysis conducted by authors using data from Healthcare Cost Report Information System Medicare cost reports and American Hospital Association Annual Survey (total personnel FTEs). **NOTE** Asterisks reflect *p* values adjusted for multiple testing. [a]Seven of 176 matched hospital pairs were excluded because of missing FTE information. ****$p < 0.001$

Private equity acquisition was noted to be associated with decreased costs per discharge and increased margins.

to for-profit status have increased margins, although without decreasing their share of Medicare discharges.[19] Jill Horwitz and colleagues have also shown that for-profit hospitals have been responsive to changes in service profitability and are more likely seek patient volumes for profitable services (relative to government-owned and not-for-profit hospitals).[33,34] With respect to private equity acquisitions specifically, Joseph Bruch and colleagues identified increases in net income and reported similar declines in Medicare discharges and increased price markups for hospital services after private equity acquisition.[6] Private equity–acquired hospitals are also more likely to pivot toward profitable service lines relative to nonacquired hospitals.[18] Our results call attention to the tenuous relationship between financial performance and clinical quality, where a boost in the former may result from structural changes that compromise the latter.[35,36]

Bruch and colleagues also reported improvements in the process quality measures for pneumonia and acute myocardial infarction after private equity acquisition.[6] However, these outcomes, along with the aggregate Medicare discharge declines and increased price markups, appeared to be largely driven by the HCA subsample.[4] In this vein, further research on private equity–acquired hospitals requires a caveat about the differential consequences on cost trends and financial performance after acquisition of this subsample (that is, whether or not a private equity–acquired hospital was an HCA hospital). In sensitivity analyses, the directionality of our findings persisted through HCA hospitals. However, we also identified a larger increase in operating margin among non-HCA hospitals, whereas HCA hospitals had a larger decrease in cost per adjusted discharge. The fact that non-HCA hospitals had a significant increase in liquidity ratio after acquisition (and HCA hospitals did not) may indicate different operational strategies and priorities among private equity firms. Ultimately, although the improved financial performance is noted across the board, our findings are not evidence that gains in efficiency are translated to improved patient outcomes or clinical experiences in either the short or the long term.

Conclusion

Private equity acquisition of short-term acute care hospitals was noted to be associated with decreased costs per discharge and increased margins. These portend increased profitability after private equity acquisition. Concurrent shifts in patient throughput and overall staffing were also identified and may have driven some of our primary findings. Given the devastating impact of COVID-19 on elective admissions in the short term and its impact on hospital financial viability in the medium and long term, our examination of private equity–acquired hospitals' financial performance has important policy implications. These findings point to notable areas for further evaluation, including whether this financial viability is achieved through a presumptive trade-off with safety and quality. ■

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Private Equity Acquisition And Responsiveness To Service-Line Profitability At Short-Term Acute Care Hospitals

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ABSTRACT As private equity firms continue to increase their ownership stake in various health care sectors in the US, questions arise about potential impacts on the organization and delivery of care. Using a difference-in-differences approach, we investigated changes in service-line provision in private equity–acquired hospitals. Relative to nonacquired hospitals, private equity acquisition was associated with a higher probability of adding specific profitable hospital-based services (interventional cardiac catheterization, hemodialysis, and labor and delivery), profitable technologies (robotic surgery and digital mammography), and freestanding or satellite emergency departments. Moreover, private equity acquisition was associated with an increased probability of providing services that were previously categorized as unprofitable but that have more recently become areas of financial opportunity (for example, mental health services). Finally, private equity–acquired hospitals were less likely to add or continue services that have unreliable revenue streams or that may face competition from nonprofit hospitals (for example, outpatient psychiatry), although fewer shifts were noted among unprofitable services. This may reflect a prevailing shift by acute care hospitals toward outpatient settings for appropriate procedures and synergies with existing holdings by private equity firms.

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Private equity acquisitions of physician practices, health care facilities, and hospitals have increased sharply during the past two decades. Combined with other investments in biomedical technology and associated industries, private equity acquisitions in health care have totaled more than $70 billion per year since 2017.[1–7] It has been estimated that more than 10 percent of all acute care admissions in 2017 were to hospitals that had been acquired by a private equity firm in the preceding fifteen years and that this activity occurred across 36 states and 106 hospital referral regions.[8]

Private equity firms acquire mature health care service providers (for example, acute care hospitals, nursing homes, and ambulatory surgery centers) via leveraged buyouts, using capital from limited partners.[9] Illustrative limited partners include sovereign wealth funds, pension funds, and people with high net worth. Private equity investments in health care have drawn some controversy because of concerns that limited partners' desire for high annualized returns on their investment and the abbreviated time horizon of private equity ownership (three to seven years) may drive a prioritization of profits over optimizing health care access, quality, and spending—that is, "profit over patients."[10] A recent study by Joseph Bruch and colleagues showed gains in net income, charge-to-cost ratios, case-mix index, and some aggregate process measures in hospitals after private equity acquisition.[11] In the wake of high-profile hospital closures[12] and the unprecedented financial challenges faced by hospitals as a result of COVID-19, some contend that private equity activity in health care delivery, and the scrutiny surrounding it, will intensify over the coming years.[10,13,14]

Despite the growing importance of private equity investment in health care, few empirical studies have assessed how the managerial practices and corporate restructurings performed by private equity firms enhance the value of target hospitals and allow them to achieve high investment returns for limited partners.[9] The strategic choices made by hospitals to maximize returns can manifest in decisions on pricing, the mix of products and services offered, and staffing levels. Because private equity firms are rarely long-term holders of corporate assets, their intent is to make acquisitions more attractive to potential buyers or public shareholders to complete a successful "exit." Furthermore, the structural changes and operational decisions of private equity firms after acquisition may affect health care spending, the immediate health of patients, and the long-term health of the broader community. Therefore, a better understanding of private equity ownership and its impact on hospital behavior will inform regulatory efforts at both the state and federal level.

Prior research has shown that for-profit hospitals are significantly more likely to offer certain services based on profitability.[15] Profitable service lines are expanded or pared down not only in accordance with a hospital's profit-maximizing strategy but also in relation to other high-margin services in that hospital's market.[16,17] Private equity acquisition, however, presents a unique situation: Although all hospitals might seek to maximize revenue over the long term, hospitals acquired by private equity firms have a heightened short-term focus. Private equity firms may target hospitals that have a greater potential for increased operating margins, but it remains unclear whether they systematically enact specific postacquisition changes in the types and range of services offered.

To explore this issue, we examined the relationship between private equity hospital acquisitions and changes in service lines. Using a predefined categorization of service-line profitability,[16,17] we used a generalized difference-in-differences framework to estimate the impact of private equity acquisition on the probability of a hospital providing profitable or unprofitable services. As changes in service lines may be associated with concomitant shifts in practice patterns (for example, a move toward an outpatient setting for certain procedures or accompanying changes in associated service lines), we further contextualize our results by examining changes in hospitals' contractual relationships or specialty-specific services.

Study Data And Methods

DATA SOURCES Private equity hospital acquisitions that took place between January 1, 2006, and December 31, 2015, were identified using a previously described search methodology that included the proprietary market intelligence reporting platforms Pitchbook, CB Insights, and Zephyr.[3,8] Transactions classified as either primary leveraged buyout or add-on acquisitions were included. The "index year" was determined as the calendar year in which deal closure (defined as the date when financial control transferred to the private equity firm) occurred. These data were cross-referenced with press releases, industry newsletters, and media focusing on hospitals and health systems.

Facility names and Medicare provider numbers were used to identify hospitals in the Centers for Medicare and Medicaid Services Healthcare Provider Cost Reporting Information System[18] and to compile data from the period 2004–18. This time horizon allowed for at least two years preacquisition and up to three years postacquisition (that is, private equity deal activity during 2006–15). This time horizon also reflects major private equity acquisitions (for example, the HCA acquisition by Bain/KKR) and the enactment of key health policy statutes (for example, the Affordable Care Act and the Health Information Technology for Economic and Clinical Health Act).[8] Facility-level data including physical address, bed size, and teaching status were extracted along with financial and operational data. Rural or urban classification was derived from Rural-Urban Continuum Codes set by the Department of Agriculture's Economic Research Service. Census data including area-level population estimates and per capita income at the county level were derived from the Area Health Resources Files. Year-specific and facility-level financial data were linked from the Healthcare Provider Cost Reporting Information System, using Medicare provider numbers; area-level data were linked using regional identifiers (for example, county Federal Information Processing Series code and hospital referral region) in the Area Health Resources Files. Hospital service provision at the facility or local system level was determined from the American Hospital Association annual survey (described in the online appendix).[19]

HOSPITAL SERVICES BY PROFITABILITY Previous research has characterized hospital services

The results show a relationship between private equity acquisition and systematic changes in the central activity of hospitals: providing care.

according to profitability: relatively profitable and relatively unprofitable.[15,17] These services ranged from the availability of specific technologies (for example, robotic surgery and specialized imaging) to types of treatment (for example, endovascular therapies, child psychiatric services, and HIV/AIDS) and specialty practices (for example, critical care, cardiac surgery, and inpatient psychiatry). Given our focus on hospital behavior in response to financial incentives, examining the provision of medical services that vary in profitability allowed us to identify patterns in management decision making.[17] Furthermore, the transition to or away from a particular service line requires concomitant operational changes—for example, in business development, physician relationships, organizational structure, and support functions such as human resources and information technology.[20]

PATTERNS IN SERVICE PROVISION AND MODEL ASSUMPTIONS We first explored the probability of a hospital offering a particular service using a nonparametric event study model with coefficients for each year relative to the acquisition year. This allowed us to visually examine patterns in outcomes relative to the event being studied (private equity acquisition). We used a dummy variable for each of the four years before and after acquisition to evaluate the leading and lagging difference-in-differences estimator. To focus on service decisions that are truly marginal, we excluded services provided by more than 90 percent of hospitals, such as emergency department services and outpatient surgery.

To estimate the impact of private equity acquisition on the probability of a hospital offering a particular service, we used a generalized difference-in-differences approach in a linear probability model. Comparator hospitals included all nonacquired hospitals that had at least seven years of continuous data reported. We adjusted for the following features: transition to critical access status (yes or no), bed size category (up to 100 beds, 101–400 beds, and more than 400 beds), for-profit status (versus government-owned or nonprofit), teaching status (yes or no), and market share (as a percentage of beds in the hospital referral region), including year and hospital-level fixed effects. Because private equity groups are likely to exit their investment between three and seven years postacquisition,[21] we excluded observations for acquired hospitals more than five years after acquisition.

The principal assumption that ensures internal validity of our difference-in-differences approach is that measured outcomes for never-acquired (that is, control) hospitals and acquired hospitals before acquisition have parallel trends. Absent private equity acquisition, the difference in the propensity to offer a particular service line between private equity–target hospitals and control hospitals would remain constant over time. We examined this assumption by jointly testing the equivalence of preacquisition event study coefficients. Only services that had parallel trends in the preacquisition period were examined using a difference-in-differences analysis. Standard errors were clustered at the hospital or health system level. Further details about the definition of services and model specifications are in the appendix.[19] This study was approved by the Institutional Review Board of the Duke University Health System.

LIMITATIONS Our study was subject to certain limitations stemming from both the data and the assumptions of the analytical framework. First, the identification of private equity acquisitions involving short-term acute care hospitals came from the reporting of individual transactions in the financial and lay press. Private transactions are unique in that the details of these deals, including their total value or debt restructuring agreements, are often opaque. However, our study design (a generalized difference-in-differences design) is suited for identifying discontinuities in the propensity to shift service lines irrespective of underlying financial motivations. Second, decisions to begin or terminate a service line (or offer a technology or procedure) often depend on regional factors, including the specialty, reputation, and market share of local competitors. However, our principal model specification included both year and hospital fixed effects, which allowed us to account for both secular trends and potentially unobserved variation within hospitals. Third, any analysis of private equity acquisition of short-term acute care hospitals includes the leveraged buyout of HCA, which accounts for more than half of all facilities acquired. To examine the impact of this deal on our findings, we conducted a sensitivity analysis excluding HCA hospitals. Finally, our study depended on data that, ultimately, were self-reported. However, the American Hospital Association annual survey remains the most comprehensive and widely used source of information on the breadth and diversity of hospital and health system services and has broad buy-in from administrators, policy makers, and researchers as reliable and consistent in its definitions.

Study Results

Our study sample consisted of 4,781 hospitals, including 228 that were acquired by private equity. Overall, private equity–acquired hospitals were more likely to be urban, in the medium bed-size category, nonteaching, and for-profit (appendix exhibits A-1 and A-2).[19] For each year, the proportion of hospitals that reported offering each service line was calculated, and this trend was examined over time. We calculated the variation in the provision of a particular service line as the percentage change in the proportion of hospitals that provided it between the first and last years in our study period (exhibits 1 and 2). The five service lines with the largest percentage increase were all in the profitable category: robotic surgery (+572 percent), digital mammography (+356 percent), freestanding or satellite emergency departments (+157 percent), adult cardiac surgery (+58 percent), and adult interventional cardiac catheterization (+52 percent) (exhibit 1). Two service lines decreased in their prevalence over this period: adult day care (–16 percent; exhibit 2) and birthing room or labor and delivery (–7 percent; exhibit 1).

PREACQUISITION TRENDS In the event study approach, we estimated the probability of each service line being offered in the years before and after acquisition for private equity–acquired hospitals relative to never-acquired hospitals. For both profitable and unprofitable services, there were no differences overall between private equity–acquired hospitals in the year before their acquisition and never-acquired hospitals (appendix exhibits A-3 and A-4).[19]

PROFITABLE AND UNPROFITABLE SERVICE PROVISION Private equity acquisition was associated with a significant increase in the probability of hospitals providing six of the eleven profitable services for which difference-in-differences estimators were calculated (exhibit 3). Specific prof-

EXHIBIT 1

Prevalence of profitable service lines in hospitals at the beginning and end of the study period, from largest to smallest percent change, 2004–18

	Hospitals providing service (%)			
Profitable service lines	**Beginning**	**End**	**Change[a]**	**Parallel pre-trends confirmed**
Robotic surgery[b]	5.4	35.9	571.7	Yes
Digital mammography[b]	17.0	77.6	356.0	Yes
Freestanding or satellite ED[b]	3.0	7.7	157.3	Yes
Adult cardiac surgery	26.2	43.2	57.7	No
Adult interventional cardiac catheterization	24.7	39.4	51.6	Yes
Urgent care center	32.6	48.4	50.2	No
Fertility center	12.7	17.4	37.1	Yes
Cardiac rehabilitation[b]	53.1	71.4	34.5	Yes
Extracorporeal shock wave lithotripsy	41.8	55.7	32.5	Yes
Neonatal intensive care unit	28.3	37.5	32.7	Yes
Women's health center	53.3	67.2	25.8	No
Fitness center	37.2	46.6	24.7	No
Inpatient orthopedic surgery	68.1	83.8	22.3	Yes
Hemodialysis	28.7	33.2	15.1	Yes
Cardiac intensive care unit	40.3	41.9	4.1	No
Outpatient surgery	89.6	92.6	3.4	—[c]
Birthing room or labor and delivery	66.8	62.1	–7.1	Yes

SOURCE Data extracted from American Hospital Association annual survey responses (2004–18). **NOTES** Data only available beginning in 2005. Parallel trends in the preacquisition period for private equity–acquired hospitals relative to control hospitals are noted in the rightmost column. ED is emergency department. [a]Values may vary from calculations based on what is shown in the "Beginning" and "End" columns because of rounding. [b]Data only available beginning in 2005. [c]Not applicable; services offered by more than 90 percent of hospitals were not examined in a difference-in-differences analysis.

EXHIBIT 2

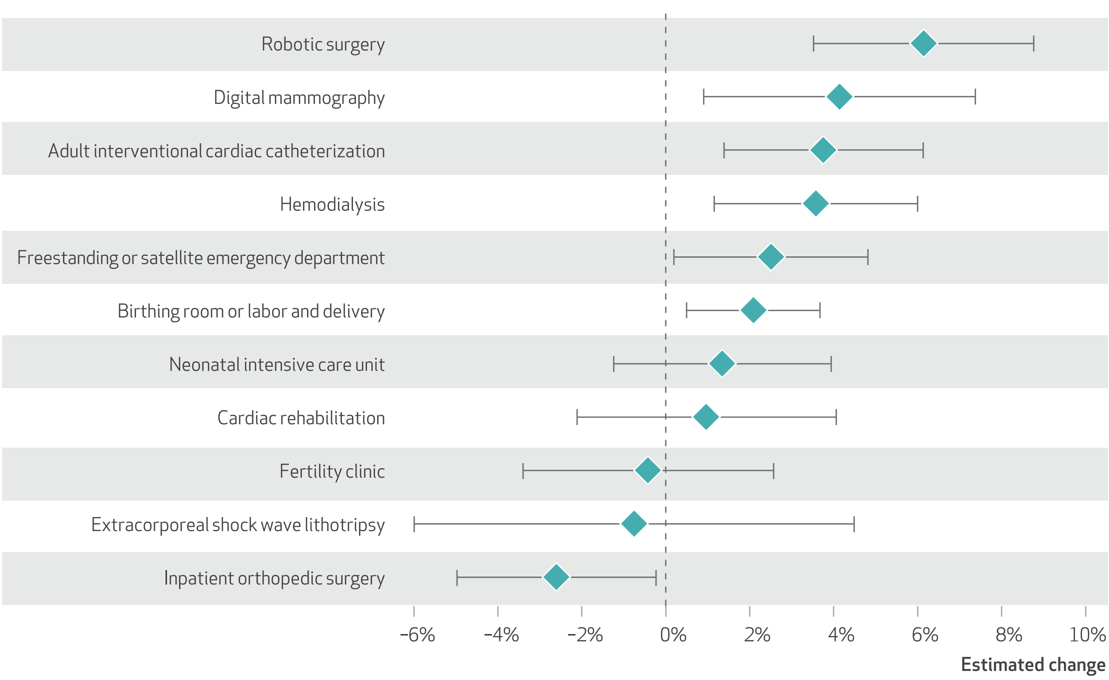
Prevalence of unprofitable service lines in hospitals at the beginning and end of the study period, from largest to smallest percent change, 2004–18

Unprofitable service lines	Hospitals providing service (%) Beginning	End	Change[a]	Parallel pre-trends confirmed
Trauma center	38.8	53.4	37.6	No
Outpatient psychiatric care	35.7	49.1	37.3	Yes
Hospice services	55.6	66.6	19.7	No
Inpatient psychiatric care	39.9	46.0	15.4	No
Inpatient detox program	18.2	20.7	13.7	Yes
Psychiatric emergency services	32.8	37.0	12.7	Yes
Burn treatment center	10.3	11.6	11.7	Yes
HIV-AIDS treatment	31.0	34.7	11.7	Yes
Volunteer services	76.3	82.7	8.4	No
Social work services	85.1	89.2	4.9	No
Emergency department	93.1	97.5	4.8	—[b]
Psychiatric partial hospital	25.8	26.4	2.6	Yes
Adult day care program	13.7	11.5	−15.9	Yes

SOURCE Data extracted from American Hospital Association annual survey responses (2004–18). **NOTES** Parallel trends in the preacquisition period for private equity–acquired hospitals relative to control hospitals are noted in the rightmost column. [a]Values may vary from calculations based on what is shown in the "Beginning" and "End" columns because of rounding. [b]Not applicable; services offered by more than 90 percent of hospitals were not examined in a difference-in-differences analysis.

EXHIBIT 3

Difference-in-differences estimates of the probability of hospitals offering specific profitable services after private equity acquisition



SOURCE Data extracted from American Hospital Association annual survey responses (2004–18). **NOTE** Error bars represent 95% confidence intervals.

itable services that were more likely to be offered after private equity acquisition included robotic surgery (+6.2 percent; $p < 0.001$), digital mammography (+4.1 percent; $p = 0.02$), and adult interventional cardiac catheterization (+3.8 percent; $p = 0.01$). In addition, private equity acquisition was associated with a greater increase in the probability of providing in-hospital hemodialysis (+3.6 percent; $p = 0.01$), of having a freestanding or satellite emergency department (+2.5; $p = 0.03$), and of having a birthing room or labor and delivery (+2.1 percent; $p = 0.01$). Of note, one profitable service (inpatient orthopedic surgery) had a lower probability of being provided after private equity acquisition (−2.6 percent; $p = 0.03$).

Among unprofitable services, private equity–target hospitals exhibited a lower probability of providing outpatient psychiatric care (−4.0 percent; $p = 0.001$) after acquisition. Conversely, acquisition resulted in a higher probability of offering psychiatric emergency services (+4.0 percent; $p = 0.01$) (exhibit 4).

SENSITIVITY ANALYSIS After excluding hospitals involved in the HCA leveraged buyout finalized in November 2006, we repeated the difference-in-differences analysis of profitable and unprofitable service provision. Among the eighty-four non-HCA private equity–acquired hospitals, the directionality of our main findings was preserved for all six of the services with statistically significant findings (robotic surgery, digital mammography, adult interventional cardiac catheterization, hemodialysis, freestanding or satellite emergency department, and birthing room or labor and delivery). Similarly, the directionality and significance of estimates for unprofitable services was preserved for the two services identified in the full analysis (psychiatric emergency services and outpatient psychiatric care; appendix exhibits A-5 and A-6).[19]

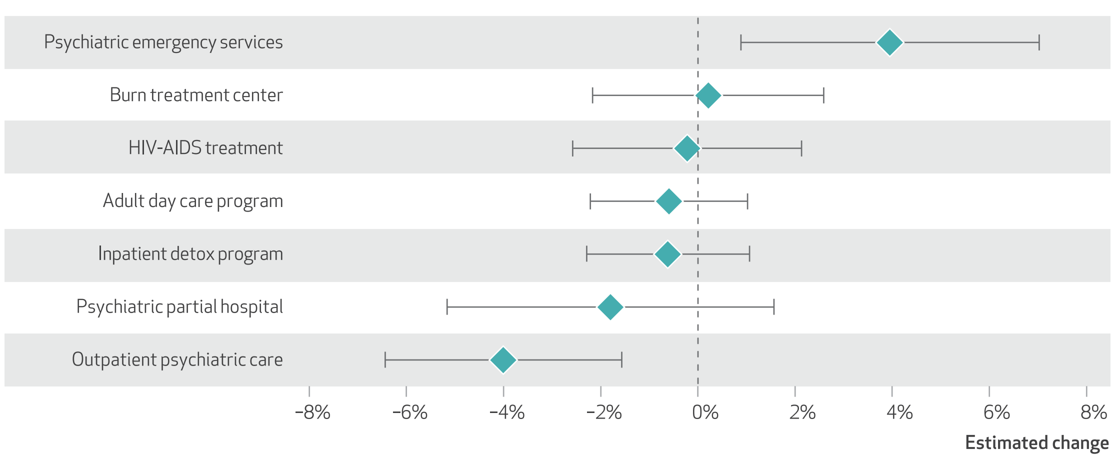
ASSOCIATED SERVICES AND CONTRACTUAL ARRANGEMENTS Finally, we examined potential trends in associated services and contractual arrangements that short-term acute care general hospitals may have adopted during this period, focusing on the services for which private equity acquisition resulted in significant shifts.

First, we examined the prevalence of reported contracts with limited-service hospitals and ambulatory surgical centers that may have accompanied shifts in inpatient services. These joint ventures have been touted as advantageous for private equity firms' short time horizons.[22] Over the course of our study period, the prevalence of ambulatory surgical center joint ventures increased 159 percent (from 7.0 percent of hospitals in 2005 to 18.1 percent in 2018; data not shown). Because we did not observe parallel pre-trends in joint venture adoption between private equity–acquired hospitals and never-acquired hospitals, a difference-in-differences analysis was not conducted.

We also examined hospital or health system provision of ambulance services, which had not been identified among the list of profitable or unprofitable services but may be associated with emergency care. During our study period, hospital or health system ambulance service provision increased 32 percent (from 39.7 percent to 52.5 percent; data not shown). However, after

EXHIBIT 4

Difference-in-differences estimates of the probability of hospitals offering specific unprofitable services after private equity acquisition



SOURCE Data extracted from American Hospital Association annual survey responses (2004–18). **NOTE** Error bars represent 95% confidence intervals.

These shifts may elucidate the mechanisms by which private equity–acquired hospitals generate financial returns for their owners.

ascertaining parallel pretrends between private equity–acquired hospitals and never-acquired hospitals (appendix exhibit A-4),[19] a difference-in-differences analysis found that private equity acquisition resulted in a 4.9 percent decrease in the probability of offering this service ($p < 0.001$).

Discussion

The considered entry of private equity into the health care provider market has affected the provision of both elective and emergent care. Since the $33 billion buyout of HCA by Bain/KKR in 2006,[23] private equity's effects on the unique functions of short-term, nonspecialized facilities remain relatively unstudied. Using a matched difference-in-differences framework, Bruch and colleagues showed that private equity acquisition was associated with a small increase in charge-to-cost ratio and net income,[11] although it is unclear whether this change is due to higher prices, lower overall expenditures, or both.[24,25] The results presented in this study show a relationship between private equity acquisition and systematic changes in the central activity of hospitals: providing care. Specifically, private equity acquisition was associated with an increased probability of hospitals providing six of the eleven profitable services studied. Conversely, among unprofitable services, private equity acquisition was associated with a decreased probability of offering one (of seven) services (outpatient psychiatric care).

These shifts must be considered within the broader context of how services and technologies provided by short-term acute care hospitals have changed. Many hospitals—not just private equity–acquired hospitals—have adopted profitable services such as robotic surgery, digital mammography, and freestanding or satellite emergency departments. However, this trend may belie an "acceleration" of service adoption by private equity–acquired hospitals ahead of profit-seeking changes in service lines exhibited by all hospitals, irrespective of the value these services may provide for patients. For example, the first results of a randomized controlled trial of digital mammography versus all-film mammography reported in 2005 showed no difference in diagnostic accuracy for asymptomatic screening and indicated that digital mammography was not cost-effective under Medicare reimbursement at that time.[26–28] Robotic surgery has been similarly heralded as a frontier technology in operative technique, despite documented variation in outcomes, a difficult learning curve for surgeons, high up-front costs for institutions, and overall higher charges for patients and payers.[29–31]

We also identified instances in which private equity acquisition was associated with effects opposite of those hypothesized. In particular, the period after private equity acquisition was associated with an increase in the probability that hospitals would provide psychiatric emergency services (an unprofitable service) and a decrease in inpatient orthopedic surgery (a profitable service). This may be explained by changes in the relative profitability of certain services over time. For instance, a 2014 study found that psychiatric emergency services, despite their relative paucity nationwide, were associated with positive net revenue and were buoyed by expansions in insurance coverage under the Affordable Care Act.[32] Moreover, a 2020 study found that more than half of recent acquisitions in the behavioral and mental health care sector were made by private equity firms.[33] Our finding that private equity acquisition was associated with an increased probability of offering emergency psychiatric services might be related to the trend toward private equity acquisition of behavioral and mental health services, as well as to the likelihood that these services are value driven.[34]

The provision of certain services cannot be viewed only as an inpatient phenomenon, given the growing movement toward outpatient and ambulatory care for procedures that have historically been relegated to inpatient settings.[35] Indeed, 66 percent of all surgical therapies were delivered in outpatient settings in 2014 (up from 57 percent in 1994);[36] moreover, the proportion of outpatient procedures performed in ambulatory surgical centers (as opposed to hospital inpatient settings) increased from 32 percent in 2005 to just over 50 percent in 2017.[37]

This trend may help clarify our finding that the period after private equity acquisition was associated with a decrease in the probability that acquired hospitals would provide inpatient orthopedic services. As procedures shift to an outpatient setting and outpatient services become concentrated in freestanding ambulatory surgical centers, this sector has become primed for consolidation.[38] In our study sample, joint ventures with ambulatory surgical centers or limited-service hospitals more than doubled during this period. Although we cannot draw a link between contractual participation with ambulatory surgical centers and surgical volume of private equity–acquired hospitals, we suspect that it may signal the bundling of certain investments in this clinical area.[39]

These shifts may elucidate the mechanisms by which private equity–acquired hospitals generate financial returns for their owners. Taken together, they underscore the fact that private equity acquisitions are hardly isolated, and operational changes in short-term acute care likely occur in tandem with other managerial decisions. These trends reinforce a prevailing shift away from acute, nonelective inpatient care, for which profitability is more volatile compared with elective care.[40] The shift away from emergency services that are integrated with hospitals and health systems may ultimately affect the extent of patients' access to care. This may also affect the provision of services at other hospitals in a particular service area. The cessation of crucial, but less profitable, services at one facility may force other facilities in the same service area to expand these service lines. Therefore, regulation that mitigates this "spillover" must address population-level metrics of health, not just outcomes specific to acquired hospitals.

Conclusion

Our findings suggest that private equity–acquired hospitals are different from their nonacquired counterparts. Private equity–acquired hospitals adopt technology in response to a profit incentive and pivot toward service lines and contractual arrangements that are rewarded by payers. Not only do private equity–acquired hospitals add profitable services faster, but also the changes they make to hospital operations may have broad implications for policy makers seeking to mitigate the potentially negative impacts of service-line disruptions and hospital market concentration. Policy makers may want to explore regulatory levers for ensuring equitable access and delivery of care in the face of private equity hospital acquisitions. ■

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Characteristics of Private Equity–Owned Hospitals in 2018

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Background

There are approximately 5200 community hospitals in the United States. About 1300 of them are investor-owned, for-profit hospitals whose owners are corporations, groups of physicians, or other private entities. Private equity firms use capital from individuals and institutions to invest in organizations. They are increasingly buying hospitals, which they typically plan to sell for a profit within 3 to 7 years after acquisition (1, 2). A recent study evaluated changes in hospital income, use, and quality associated with private equity acquisitions between 2005–2017 (2).

Objective

To compare acute care hospitals owned by private equity firms with similarly sized and located hospitals not owned by private equity firms.

Methods and Findings

We used merger and acquisition reports by Irving Levin Associates and public information to identify 130 hospitals under private equity control in 2018. We then identified all 2868 hospitals that had a full year of data in the 2018 Medicare Cost Report, extracted information on size (1 to 49 beds, 50 to 450 beds, and ≥451 beds), and assigned the hospitals to health care markets (hospital referral regions) using *The Dartmouth Atlas of Health Care*. We used health care markets and size groups to identify 688 hospitals that we could match to a private equity hospital. Within matched groups, there were 1 to 56 matched control hospitals per private equity hospital, with a median of 8 control hospitals and an interquartile range of 10. We assigned each private equity hospital a weight of 1 and assigned its matched hospitals weights that summed to 1. We used these weights in all analyses.

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Disclosures: Disclosures can be viewed at www.acponline.org/authors/icmje/ConflictOfInterestForms.do?msNum=M20-1361.

Reproducible Research Statement: *Study protocol:* Not available. *Statistical code:* Available from Mr. Bruch, jbruch@g.harvard.edu. *Data set:* Data on total acquisitions by private equity firm are available from Mr. Bruch (jbruch@g.harvard.edu).

We compared private equity hospitals with their matched hospitals on location, financial characteristics, and patient experience. Hospital characteristics were identified from the Medicare cost report after transforming (winsorizing) values outside the 95th and 5th percentiles to limit the effect of extreme outliers (3). We used the rurality score from the Rural-Urban Commuting Areas geographic taxonomy, version 3.10, to classify ZIP codes from 1 (metropolitan) to 10 (rural). We used 2017 median household income by ZIP code from *The Dartmouth Atlas of Health Care*. Scores were collected from the Hospital Consumer Assessment of Healthcare Providers and Systems survey, which contains patient perspectives of their hospital experience. It has often been called a patient satisfaction score and ranges from 0 (lowest) to 100 (highest) (4).

Mean values were compared between the groups, allowing for correlation in hospital characteristics, by using a linear regression model with private equity status as the key independent variable, conditional on hospital referral region. We controlled for total hospital beds in analyses involving cost report outcomes. We clustered SEs at the hospital referral region level. For outcomes expressed in ratios, we modeled the numerator as the outcome and adjusted for the denominator by including it as an independent variable, along with hospital beds and hospital referral region (5).

Results

Most hospitals in this study were in the South (Figure) and were medium sized (Table). When compared with their matched hospitals, private equity hospitals on average had a higher rurality score, were located in a ZIP code with a lower median household income, had a slightly lower patient experience score, had fewer patients discharged per year, and had fewer full-time equivalent employees per occupied bed (Table). Private equity hospitals did not differ from their matched hospitals on net income per patient discharged, total inpatient charge per inpatient day, total charge-to-cost ratio, or Medicare and Medicaid shares of patients discharged.

Discussion

In this comparison of characteristics between hospitals owned by private equity and those not owned by private equity in 2018, private equity hospitals were on average located in lower-income, more-rural areas and had fewer patients discharged and employees per bed, although several economic outcomes were similar. Some of these differences may be due to unobserved factors, such as private equity–owned hospitals being located in less metropolitan areas with different populations and socioeconomic conditions than more metropolitan areas, and it is unknown whether these differences are attributable to the private equity acquisition itself. A separate question is whether private equity–owned hospitals differ from other hospitals in ways that negatively affect patient care. Fewer full-time–equivalent employees per occupied bed and lower average patient experience scores among private equity–owned hospitals raise concern. These measures, however, do not fully capture quality of care, and the potential effect of private equity on quality and other outcomes was outside the scope of this cross-sectional analysis. Therefore, additional research is necessary to identify and characterize the mechanisms underlying these differences.

Acknowledgments

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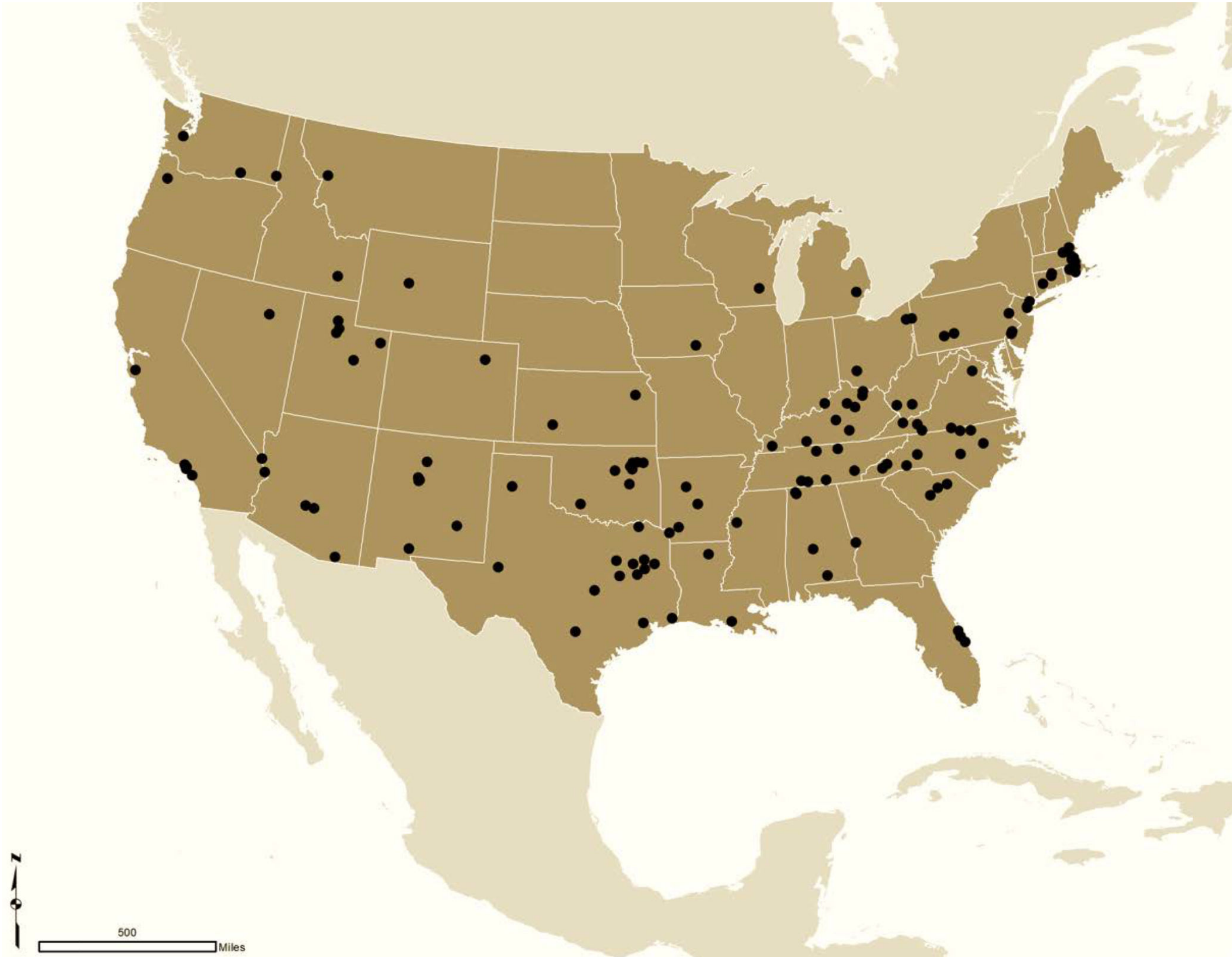


Figure.

Map of private equity–owned hospitals in 2018.

Using Medicare cost reports, the addresses for the 130 private equity–owned hospitals in 2018 were identified. There were no hospitals located in Hawaii or Alaska.

Table.

Characteristics of Private Equity–Owned Hospitals and Control Hospitals[*]

Characteristic	Private Equity–Owned Hospitals (n = 130)	Control Hospitals (n = 688)	Adjusted Difference (95% CI)	*P* Value[†]
Geographic region, *%*				
South	53	54	–	–
West	24	23	–	–
Northeast	17	17	–	–
Midwest	6	7	–	–
Hospital size, *%*				
Small (≤49 beds)	7	7	–	–
Medium (50–450 beds)	91	91	–	–
Large (≥451 beds)	2	2	–	–
Mean rurality score (SD)[‡]	2.70 (2.16)	2.28 (2.14)	0.42 (0.01 to 0.83)	0.043
Mean hospital ZIP code median household income (SD), *$*	45 389 (16 614)	50 112 (24 472)	−4723 (−9455 to 9)	0.050
Mean patient experience score (SD)[§]	85.47 (3.74)	88.19 (3.05)	−2.68 (−3.56 to −1.80)	<0.001
Mean total patients discharged (SD), *n*	5764 (4719)	8453 (6660)	−1839 (−2364 to −1313)	<0.001
Mean net income (SD), *million $*[‖]	5.1 (16.5)	15.1 (28.1)	−2.3 (−5.4 to 0.8)	0.149
Mean net income per patient discharged (SD), *$*	1030.3 (3453.2)	1038.2 (6499.0)	−7.9[¶]	–
Mean total inpatient charges (SD), *million $*[**]	307.7 (355.2)	423.2 (468.2)	22.3 (−16.6 to 61.2)	0.26
Mean total inpatient charges per day (SD), *$*	9194.7 (4110.9)	8825.6 (5498.3)	369.1[¶]	–
Mean total charges (SD), *million $*[††]	621.5 (562.8)	877.2 (802.8)	9.3 (−68.5 to 87.2)	0.81
Mean total charge-to-cost ratio (SD)	5.8 (2.0)	4.8 (2.4)	1.0[¶]	–
Mean Medicare patients discharged (SD), *n*[‡‡]	1958.5 (1603.0)	2709.8 (2150.1)	36.0 (−97.3 to 169.3)	0.59
Mean Medicare share of patients discharged (SD), *%*	36.1 (10.3)	34.4 (10.6)	1.7[¶]	–
Mean Medicaid patients discharged (SD), *n*[§§]	591.5 (756.4)	763.8 (890.5)	1.8 (−146.3 to 149.9)	0.98
Mean Medicaid share of patients discharged (SD), *%*	11.1 (9.9)	10.2 (11.1)	0.9[¶]	–
Mean full-time equivalent employees (SD), *n*[‖‖]	667.8 (487.5)	1077.9 (899.2)	−164.9 (−250.6 to −79.1)	<0.001

Characteristic	Private Equity–Owned Hospitals (*n* = 130)	Control Hospitals (*n* = 688)	Adjusted Difference (95% CI)	*P* Value[†]
Mean full-time equivalent employees per occupied bed (SD), *n*	12.6 (5.4)	14.5 (10.9)	−1.9[¶]	–

[*] Private equity–owned hospitals were matched to control hospitals on hospital referral region and size bin using exact matching in R MatchIt (R Foundation). We assigned control hospitals weights that summed to 1 (1/number of control hospitals). Hospital referral region was separated by geographic region for the purpose of this table.

[†] To compare acute care hospitals owned by private equity firms with similarly sized and located hospitals not owned by private equity firms and to accommodate correlation within hospital referral region, we used a linear regression model with the variable of interest as the dependent variable and private equity ownership status as the independent variable, conditional on hospital referral region. We clustered SEs at the hospital referral region level.

[‡] Rurality codes range from metropolitan (1) to highly rural (10). These codes come from the Rural-Urban Commuting Areas geographic taxonomy, version 3.10.

[§] The patient experience score refers to the Hospital Consumer Assessment of Healthcare Providers and Systems linear score.

[//] We modeled total net income as the outcome and adjusted for total discharges, total beds, and hospital referral region.

[¶] Value is a ratio difference.

[**] We modeled total inpatient charges as the outcome and adjusted for total inpatient days, total beds, and hospital referral region.

[††] We modeled total charges as the outcome and adjusted for total costs, total beds, and hospital referral region.

[‡‡] We modeled total Medicare discharges as the outcome and adjusted for total discharges, total beds, and hospital referral region.

[§§] We modeled total Medicaid discharges as the outcome and adjusted for total discharges, total beds, and hospital referral region.

[////] We modeled total full-time equivalent employees as the outcome and adjusted for occupied beds (total patient days/365), total beds, and hospital referral region.

Building Competition

How buy-and-build helps the American economy

FEBRUARY 2023

AMERICAN INVESTMENT COUNCIL

Data provided by PitchBook

Executive summary

Add-ons are a common transaction spearheaded by private equity (PE) sponsors. Investors can grow their portfolio companies both organically and "inorganically"—meaning add-ons—and typically use both strategies at the same time. Over the last decade or so, add-ons have become much more common in PE.

During those years, PE sponsors have employed something called the "buy-and-build" model, which investors use to acquire several smaller companies to create a new, more competitive business. "Buy-and-build" is a useful tool to help cost-intensive industries become more efficient and lower their costs, savings that are ultimately passed on to their customers.

Most add-ons are done in highly competitive, highly fragmented industries, where incumbents lack the market share to affect prices or create a monopoly. In fact, PitchBook data shows that more than 60% of today's add-ons are done by only a handful of fragmented industries in which PE is active. Buy-and-build is most common in industries such as insurance, where more than 400,000 brokers and agencies compete for customers in the United States.[1] It's also common in outpatient clinics, landscaping, construction, IT consulting, pest control and waste management services, among others. Not every "fragmented" industry is appropriate for buy-and-build, which is why PE sponsors look for predictability and reliable cash flows before investing. Sponsors look for companies and situations that can benefit from an institutional mindset, rather than simply buying a company and looking for cost reductions.

The buy-and-build model is often guided by operating partners, who are typically ex-CEOs or ex-COOs of big companies. Ex-CEOs tend to have extensive experience with acquisitions and integrating new products and services into a larger organization. They can also help PE sponsors look for operational shortcomings of unsponsored companies, given their backgrounds as former executives. While operating partners bring plenty of operational expertise, they also have significant industry expertise that can inform a PE sponsor's strategy and change the trajectory of a portfolio company. That's why LPs are looking to operating partners, rather than financial engineering, to drive value at private equity firms. In the same vein, it's also why PE sponsors use the buy-and-build approach: In the end, the sum is greater than all the parts put together, benefitting stakeholders, companies, employees and the communities that they serve.

1: "Insurance Brokers & Agencies in the US—Number of Businesses 2003-2028," IBISWorld, Updated June 23, 2022.

Why buy-and-build helps the American economy

What's an "add-on"?

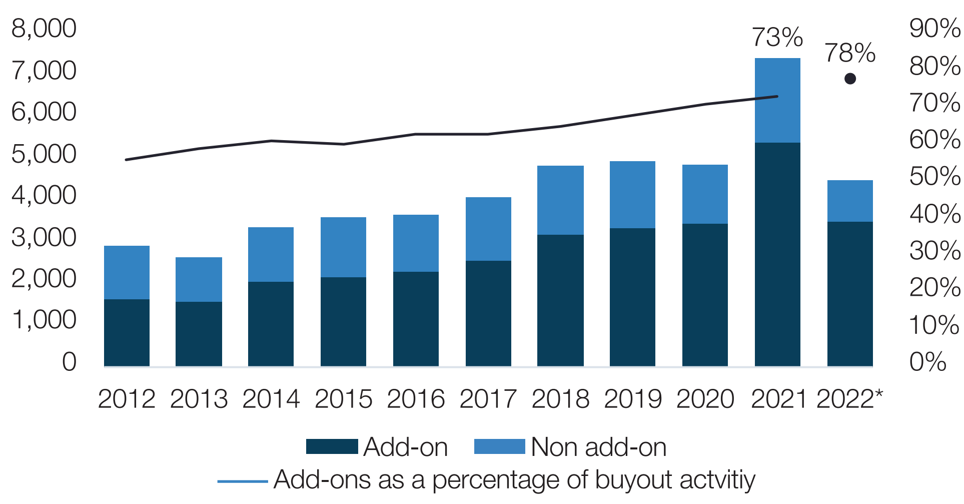
Add-ons refer to acquisitions made on behalf of PE-backed portfolio companies, and they're planned many months in advance by their financial sponsors. Sometimes they are done to bolster a portfolio company's product offerings. For example, a PE firm may invest in a dog food producer and identify a promising cat food producer to acquire; the combined company would be able to offer more products to their customers while reducing operating costs across the board. In the same vein, add-ons can be made on behalf of software companies so they can keep up with shifting customer demand more easily. In other instances, a brick-and-mortar company could "add on" a technology-based company operating in the same market.

The focus of this report is to highlight add-ons in fragmented markets, which are also common. In these cases, PE sponsors are looking to grow portfolio companies that operate in highly competitive markets, such as insurance or outpatient health care clinics, with reliable and sustainable cash flows and high operating costs. Fragmented markets have no dominant players and have virtually no risk of monopolization. In most cases, PE sponsors are looking to consolidate local companies into regional ones, creating more cost-efficient companies that can better serve their customers.

Today, about 78% of majority PE transactions are add-ons. That's up from 56% a decade ago. The Federal Trade Commission has expressed concern about this percentage, but the ratio requires context about the nature of the companies that are being acquired.

Using the same PitchBook methodology, 62% of add-ons are happening in 10% of the industry codes tracked by PitchBook. That indicates the 78% ratio is heavily influenced by a handful of markets. The nature of those markets is highly fragmented, which means there are tens of thousands, and in some cases hundreds of thousands, of similar companies operating in those industries. In every case, there is no

Add-ons as a percentage of PE buyouts



Source: PitchBook | Geography: US
*As of October 25, 2022

Why buy-and-build helps the American economy

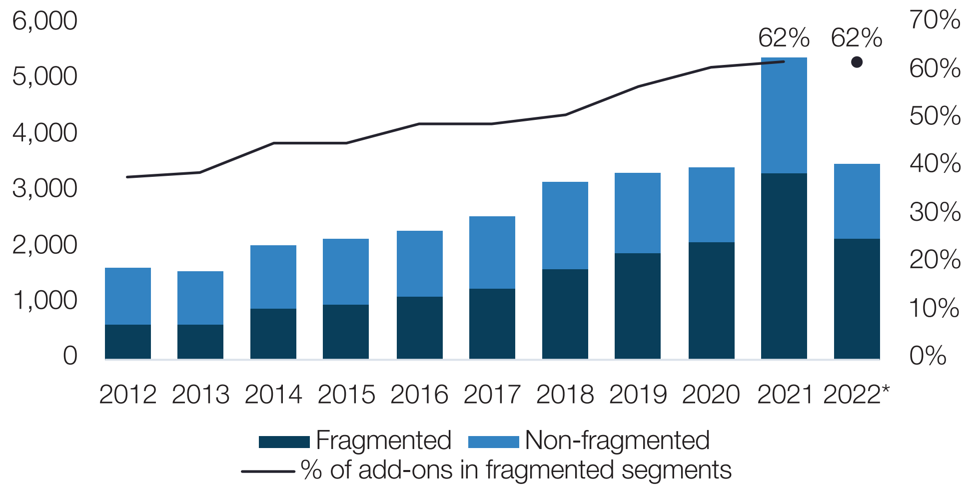
market participant that occupies a dominant position. To the contrary, 17 of these industries have at least 10,000[2] market participants, including eight with at least 100,000 market participants.[3]

In 2021, there were 5,392 PE-backed add-ons occurring in the US, which was a record. Of those, 3,321 were in fragmented markets favored by PE firms, and there were more than 7 million companies active in those industries around the country, according to IBISWorld.[4]

The buy-and-build model has to be geared toward fragmented industries to be used in the first place. PE sponsors take the buy-and-build approach seriously and only select companies that help the platform company grow its value. The results, if done well, are platform companies that are:

- More competitive and better organized for their employees
- Provide better goods and services at a lower price for their customers
- Create more channels of growth for the companies themselves
- Generate higher returns for long-term investors such as public pension plans.

Add-ons by industry fragmentation



Source: PitchBook | Geography: US
*As of October 25, 2022

Percentage of PE-backed platforms that added on in another state

88%

Among PE-sponsored companies that acquire other companies, almost 90% of them have acquired businesses in another state. While inter-state acquisitions are fairly common in M&A, they are usually deliberate for PE sponsors. Buy-and-build allows investors to transform portfolio companies into regional, more competitive businesses while producing economies of scale, which helps drive down costs. These acquisitions also mean new products and services are reaching new customers. For example, if a mental health provider is acquired in Ohio, that provider will receive the additional capabilities of the platform company, which could be a significantly bigger provider based in California. In this case, that means more patients have more access to more services than they did before the investment happened. For a detailed example, please see page 7.

2: Including insurance, landscaping, management consulting, IT consulting, manufacturing, construction & engineering, HVAC services, property management and wholesale distributors, advertising & marketing agencies, pest control, car washes, dry cleaners, elevator installation & services, business products and services software, and public relations firms
3: Including insurance, landscaping, management consulting, IT consulting, manufacturing, construction & engineering, HVAC services, property management, and wholesale distributors
4: 7 million figure compiled from the individual reports referenced in the graphic below. Individual reports can be searched at "Expert Industry Research You Can Trust," IBISWorld, Accessed October 28, 2022.

Common buy-and-build markets

Figures denote how many companies were in business in each market as of 2022

Construction **2,128,809**	Management consulting **1,121,512**	Manufacturing **718,796**	Distributors/wholesale **701,229**	Landscaping **632,895**
IT consulting **501,985**	Insurance brokerages **415,446**	Property management **307,621**	Engineering **141,898**	HVAC **122,638**
Advertising & marketing **87,712**	Car washes **67,163**	Public relations **54,581**	Outpatient health care clinics **34,752**	Pest control **29,535**
Dry cleaners **27,745**	Elevator services **20,469**	Software publishing **16,431**	Urgent care centers **9,616**	Waste management **8,352**

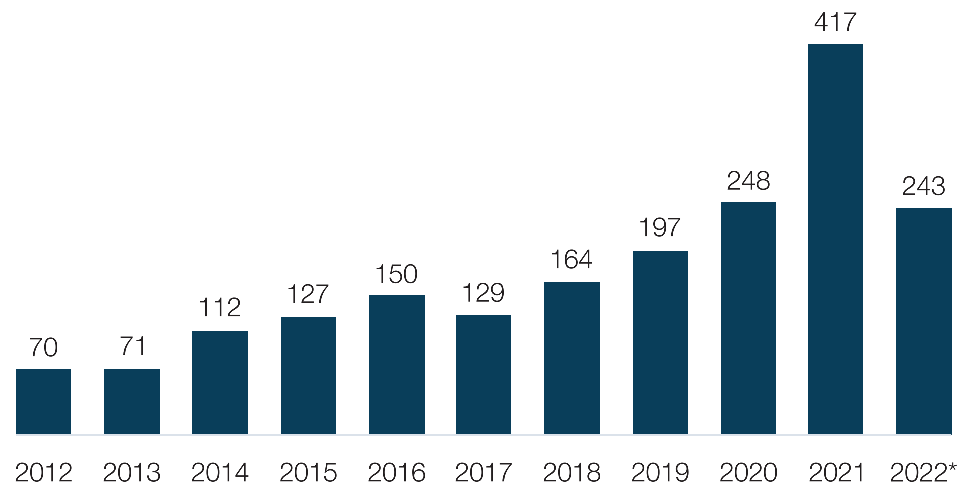
All datapoints provided by IBISWorld, accessed October 28, 2022. Individual reports can be searched at https://www.ibisworld.com

Creating new insurance markets

AMERICAN INVESTMENT COUNCIL

The insurance industry is among the most fragmented industries in the US. The largest brokerage in the US, State Farm, commands just 16% of market share, according to the National Association of Insurance Commissioners.[5] Over 400,00 other brokerages compete for the other 84%.[6] Insurance companies span the spectrum in size, from large, multi-billion-dollar platforms to much smaller regional brokerages that do business on a more personal basis. Regional consolidation helps build economies of scale and reduce operating costs—savings that can be passed on to retail and commercial customers.

PE-sponsored add-ons in insurance brokerages



Source: PitchBook | Geography: US
*As of October 25, 2022

Confie Seguros

Confie Seguros is one of the most underreported success stories in PE's history. It began with an unexpected insight from an insurance executive, Mordy Rothberg. While researching insurance trends in the US Hispanic community, Rothberg realized that the fastest-growing demographic in the country was wildly underserved as insurance customers. In 2008, he set about to change that by creating the country's first Hispanic-focused insurance company.[7]

Thanks to its financial resources and industrial know-how, Rothberg decided to partner with PE to achieve that goal. After acquiring a "platform company" in California alongside a PE sponsor, he renamed it to Confie Seguros, which means "trust insurance" in Spanish. With Confie Seguros in place, the platform began acquiring hundreds of insurance brokerages where Hispanic customers were underserved. Staff were retrained or hired to provide bi-lingual, personalized services to Hispanic customers, who preferred face-to-face meetings and translated insurance policies before purchasing insurance products.[8] More than a decade later, Confie Seguros grew into the first leading Hispanic insurance brokerage in American history while ranking first in every category in which it competed between 2016 and 2021, according to Insurance Journal.[9]

5: "Property and Casualty Insurance Industry 2021 Top 25 Groups and Companies by Countrywide Premium," National Association of Insurance Commissioners, 2022.
6: "Insurance Brokers & Agencies in the US—Number of Businesses 2003-2028," IBISWorld, Updated June 23, 2022.
7: "Storefront Marketer Grows by Acquisition: Confie Seguros Reaches $200 Million in Revenues Catering to the Hispanic Market," Rough Notes, Susan R.A. Honeyman, April 2012.
8: Ibid
9: "Closer Look: Personal Lines Leaders," Insurance Journal, September 6, 2021.

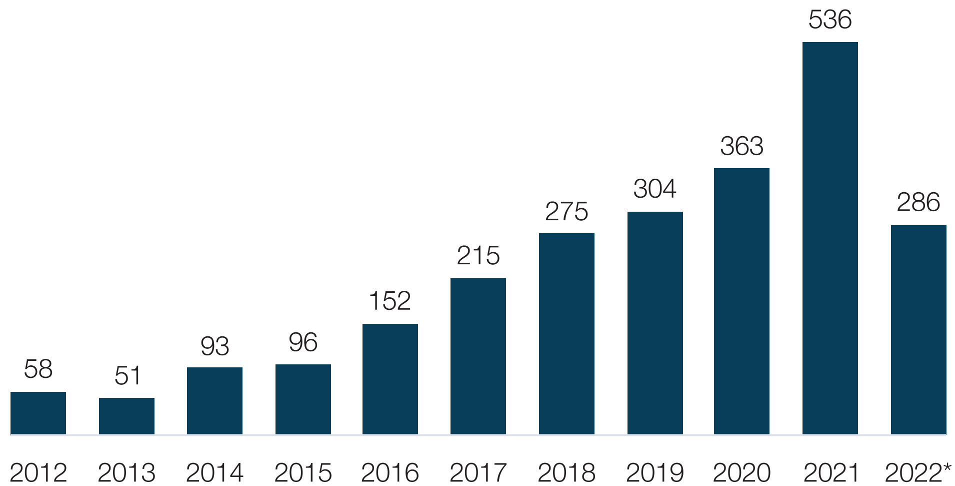
Streamlining health care costs

AMERICAN INVESTMENT COUNCIL

The buy-and-build model is used extensively in outpatient health care services. Specialties such as dermatology, cardiology, and oncology are often small, local providers that employ five or fewer doctors. In many cases, specialized clinics only have one doctor after whom the practice is named. Especially in the smallest cases, doctors in single-practice providers must perform their medical duties while also weighing in on administrative matters. Sometimes they even help hire new staff. But the country's physicians weren't trained in business management—they were trained to be doctors. In the real world, that means their most valuable skills are underused, their hours are longer, and they're performing business-related functions at which they might not be very good. It's also exhausting.

That's why PE investors are helping these industries—and ultimately helping patients while reducing their bills. When physicians can focus on their patients, and the organizations that employ them can become more efficient, health care providers can streamline costs and pass those savings into patients' pocketbooks.

PE-sponsored add-ons in outpatient healthcare clinics



Source: PitchBook | Geography: US
*As of October 25, 2022

Greater scale at the provider level leads to improved relationships with payors, further reducing costs at the patient level. It also creates a more welcoming environment for new physicians, who can join efficient regional providers without having to build a practice from scratch.

Representative markets

Veterinary services, dermatology, dentistry & orthodontics, radiology, oncology, cardiology, orthopedics, mental and behavioral health

PE at work: BayMark Health Services

BayMark HEALTH SERVICES

BayMark Health Services is based in Lewisville, TX, and specializes in treating opioid addiction. It was acquired by Webster Capital Management in 2015 and set about expanding into more pockets of the country. Webster's buy-and-build strategy wasn't limited to geographic expansion, as important as that was. The plan also included expanding into new service lines to better serve BayMark patients. In 2017, BayMark acquired a Louisiana-based provider called AppleGate, which offers office-based buprenorphine treatments. "With the state of opioid abuse in the country, BayMark is focusing on providing care through multiple approaches in as many areas as possible to address the epidemic," the firm said.[10]

10: "Webster Capital-Backed BayMark Health Services Makes Two Acquisitions," Citybizlist, February 6, 2017.

Bringing urgent care to rural communities

According to the Urgent Care Association, only 1% of urgent care centers operate in rural communities. Metropolitan areas, by contrast, account for 86.2% of all urgent care operations.[11] While urgent care facilities aren't the only health care providers around, they have become ubiquitous in wealthier communities across the country and help lighten the load for local hospitals and emergency rooms.

How PE is helping

Even with those barriers in place, PE is actively addressing the problem. Urgent care centers are natural candidates for growth capital and the buy-and-build model, since the model is scalable, replicable, and in high demand. PE firms start by investing in urgent care providers that have identified business models that work in their communities. Once partnered with PE, they can use their new capital to open new locations in underserved areas. Using a "buy-and-build" approach, PE-sponsored providers can increase the number of care facilities under their umbrella, reducing overhead costs and, ultimately, patients' bills. For many patients without urgent care options, relatively "local" hospitals are their only option, and emergency room bills can be stratospheric. A 2017 study from West Virginia University, which analyzed the impact of one urgent care chain—MedExpress—on the Appalachian area, found that "MedExpress's entry would seem to be freeing up valuable resources for more serious medical situations" and that urgent care centers "lead to a substitution to a lower cost option" for patients.[12]

Usually under the radar, PE firms have been leading the charge on rural health care expansion. According to The Journal of Urgent Care Medicine, there is "probably the potential for another 1,500 urgent care centers in rural/secondary markets" across the country,[13] and that pent-up demand would be welcomed with gratitude and loyalty by local towns. To meet that demand, PE firms are partnering with entrepreneurial physicians who have built sustainable business models and want to see their companies expand into underserved markets. As they often do, PE firms also bring experienced managerial help to urgent care portfolio companies. Sverica Capital, for example, acquired Med First in Jacksonville, NC, in 2016. As part of the partnership, two former CEOs of major hospitals joined the company's board. Sverica aimed to expand the company's model into other rural markets in the southeast and has almost doubled the number of Med First clinics over the span of five years.

PE at work: Fast Pace Urgent Care

FAST PACE HEALTH URGENT CARE

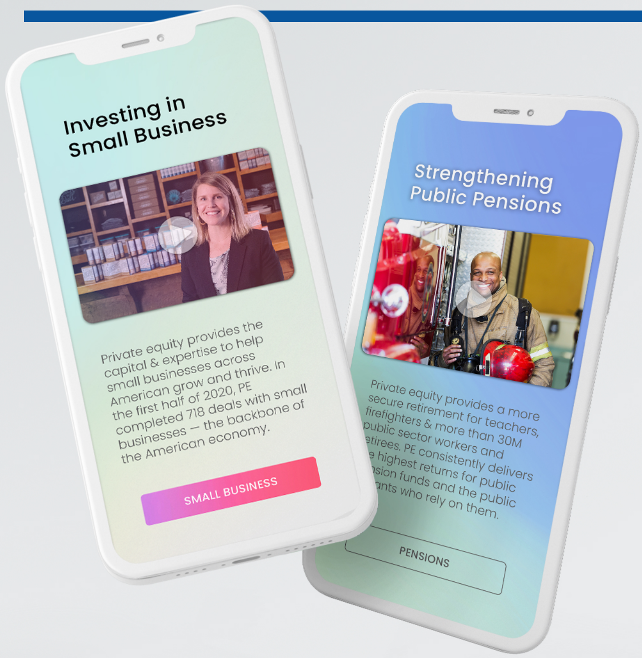
Fast Pace Urgent Care has been under PE sponsorship for almost ten years, first with Shore Capital and now with Revelstroke Capital Partners. Under Shore Capital, Fast Pace executed an accelerated growth plan, resulting in 29 new locations across rural Tennessee and Kentucky. It also increased foot traffic from 40,000 patients in 2012 to about 400,000 in 2016. That success allowed Fast Pace to increase employee headcount from 50 to 700 in the same time span.

11: "Benchmarking Report," Urgent Care Association, 2022.
12: "The Effect of Health Care Entrepreneurship on Local Health: The Case of MedExpress in Appalachia," JRAP, Joshua Hall and Amir B. Ferreira Neto, July 24, 2018.
13: "Rural and Tertiary Markets: The Next Urgent Care Frontier," JUCM, Alan A. Ayers, December 2, 2019.

PRIVATE EQUITY
INVESTING IN AMERICA

From urban to rural and everywhere in between, private equity is making a positive impact across America and investing in every community to:

- Back small businesses
- Support good-paying jobs
- Boost the American economy
- Strengthen public pensions



The American Investment Council (AIC) is an advocacy and resource organization established to develop and provide information about the private investment industry and its contributions to the long-term growth of the U.S. economy and retirement security of American workers. Member firms of the AIC consist of the country's leading private equity and growth capital firms united by their successful partnerships with limited partners and American businesses.

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Diamonds in the Rough

How PE breathes new life into unloved businesses

SEPTEMBER 2022

AMERICAN INVESTMENT COUNCIL

Data provided by PitchBook

Executive summary

Private equity's calling card is fixing companies. Sometimes they are household-name companies, and sometimes they are small, family-led companies known only to locals. Sometimes they aren't even companies at all, but PE sponsors see them becoming valuable contributors to the economy if given the chance.

This report covers "carveouts," a creative strategy employed by today's most innovative investors. These carveouts are for "noncore assets," which is a polite way of saying underloved, under-resourced, or misaligned business units nestled in much larger companies. Every year, PE firms identify hundreds of those businesses, buy them from their parent corporations, and make them independent companies—often with new names, logos, and management teams. PE can also help with antitrust situations: When regulators require divestitures in order for mergers to get approval, PE can step in and buy those assets and save a potentially transformational deal. Because they are fiduciaries for their own investors, PE firms need to make deliberate plans for how they will add value to those divested assets and make them attractive for future buyers.

This report, produced in conjunction with PitchBook, shows that thousands of new companies have been created over the past decade across every pocket of the US economy.

To take just one example, in 2018, PE firm Francisco Partners reached out to Discovery Communications, which runs the Discovery Channel, Animal Planet, and dozens of other media outlets. Francisco had an idea to build a new science education company and wanted to buy Discovery's Education platform as a starting point. A couple years later, Francisco acquired a promising startup called Mystery Science, which makes digital science and STEM curricula for grade schools, and merged it with its Discovery team. By the time it sold Discovery Education in early 2022, Francisco had created an innovative approach to education that gets grade schoolers more excited about science.

There are hundreds of other examples to mention. Once they stand on their own, these companies are motivated to prove themselves and do something new and beneficial for the economy.

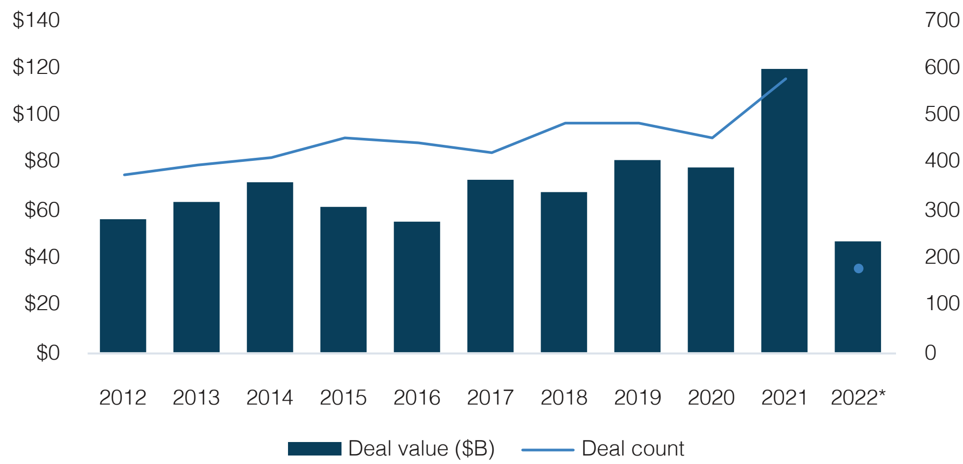
Rejuvenating businesses

PE carveouts are a popular and creative type of transaction that often go under the radar. But they happen all the time. According to PitchBook, there were more than 500 last year alone. Over the past decade, PE firms have carved out over 4,000 new, standalone companies, investing over $700 billion in the process.

Like normal buyouts, carveouts require vision and a clear roadmap to be successful. But instead of turning entire companies around, carveouts are about identifying teams and businesses that are lagging in their current situations and giving them a chance to prove themselves. Instead of being on the backburner of bigger corporations, these business units are given the resources they need to operate as their own companies.

Many of today's most successful companies started out as underloved divisions of other companies. Perhaps they didn't have adequate budgets to grow enough, or perhaps their old bosses were looking for ways to get rid of them. With PE sponsors in their corner, they no longer needed to worry about those things. With new names and logos and a new lease on life, those businesses could reach their full potential. There are countless success stories across dozens of sectors, including manufacturing, energy, healthcare services, pharmaceutical drug production, and retail, to name several. We highlight a few examples later in this report.

PE carveout deal activity



Source: PitchBook | Geography: US
*As of June 15, 2022

Step 1: "Noncore" business unit operates below the radar of a much larger company.

Step 2: Private equity recognizes potential of the business and pays the parent company to "carve it out."

Step 3: PE sponsor gives the company a new name, logo, and management team, then builds it into an independent success.

Step 4: PE sponsor exits the portfolio company by taking it public or selling it to a buyer that values it.

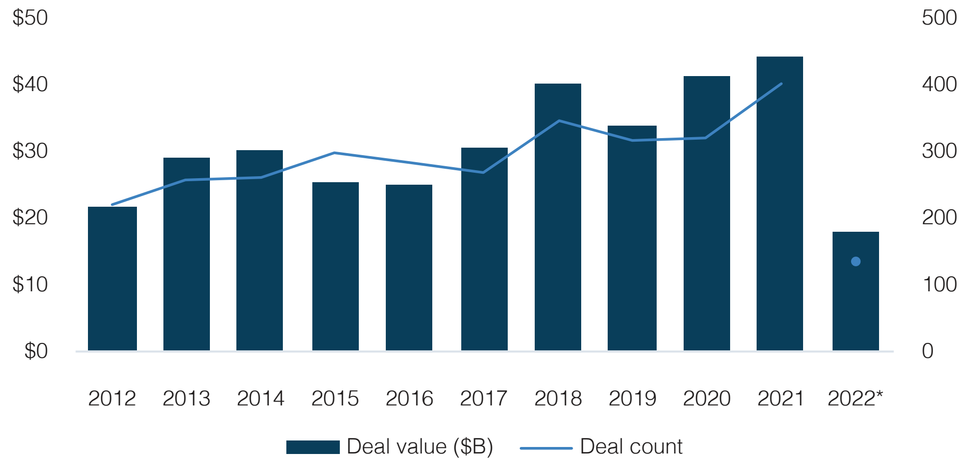
Rejuvenating businesses

Not every carveout turns into a new, independent company. Just as often, private equity sponsors will identify under-resourced businesses that can transform their existing portfolio companies. While those businesses don't always get the full, standalone treatment, they're appreciated just as much by their new owners.

Take GE Lighting, a former business unit of General Electric. AIC member KKR bought the business in summer 2020 and merged it with Savant Systems, which was already sponsored by KKR. Savant, a Boston-based company, provides smart home technology through a single interface, including climate, lighting, entertainment, security, and home energy settings. GE Lighting's technology was incorporated into Savant, which is investing in lamp space innovation and other advanced products. Under KKR, Savant's mission is to become the number one intelligent lighting company worldwide."[1] Acquiring talent and assets from a reputable company such as GE helps it get there.

Those types of transactions happen hundreds of times every year. PE-backed energy companies acquire unused properties from names such as BP and Shell; PE-backed food companies pick up noncore brands from companies such as Nestlé; the list goes on. But private equity has the playbook, financial resources, and creativity to initiate those transformative deals, thereby resulting in stronger portfolio companies and innovative solutions across every sector of the economy.

PE carveout deal activity (add-ons to existing companies)



Source: PitchBook | Geography: US
*As of June 15, 2022

Step 1: "Noncore" business unit operates below the radar of a much larger company.

Step 2: Private equity recognizes potential of the business and pays the parent company to "carve it out."

Step 3: PE sponsor blends the business into an existing portfolio company, where it can thrive.

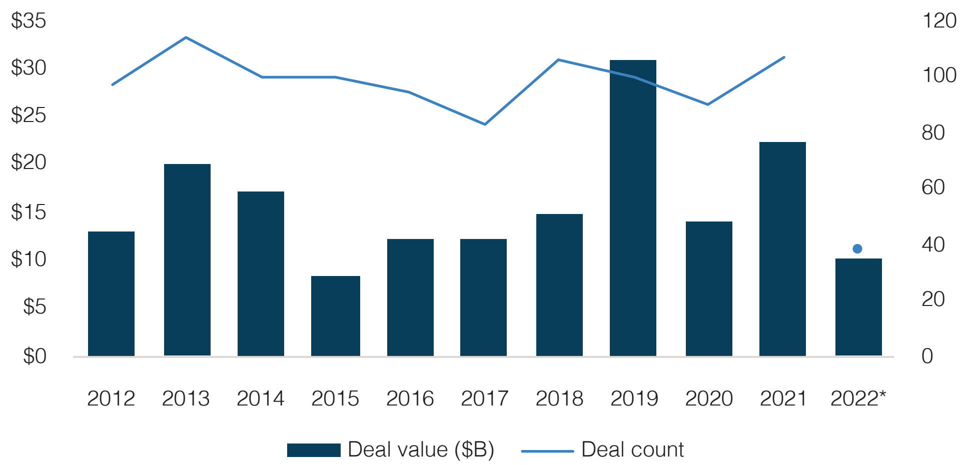
Step 4: PE sponsor exits the portfolio company by taking it public or selling it to a buyer that values it.

1: "Savant Systems, Inc. Completes Acquisition of GE Lighting," GE Lighting, July 1, 2020.

Manufacturing new companies

According to PitchBook, PE firms have created almost 1,000 new manufacturing companies over the past decade, including visionary deals in machinery, building products, consumer nondurables, food production, electrical equipment, and industrial parts suppliers. Many of those new businesses were carved out of household name, such as John Deere, Johnson & Johnson, General Electric, Pitney Bowes, and Dow Chemicals. With more resources behind them, those new manufacturing companies can hire more, without relying on approval from indifferent management teams.

Manufacturing PE carveout deal activity



Source: PitchBook | Geography: US
*As of June 15, 2022

Private equity at work

Core & Main used to be known as HD Supply Waterworks, a subsidiary of HD Supply. That changed in 2017, when AIC member Clayton, Dubilier & Rice (CD&R) made the company independent. Core & Main makes storm drainage, fire protection, and wastewater products for a range of industries, including contractors and municipalities. When CD&R bought the business, it employed about 2,900 people and operated 246 branches throughout the US. By the time it went public as an independent company in 2021, its headcount had increased to 3,700, and its footprint expanded to 285 locations. It also became much more valuable, almost tripling in value in about four years.

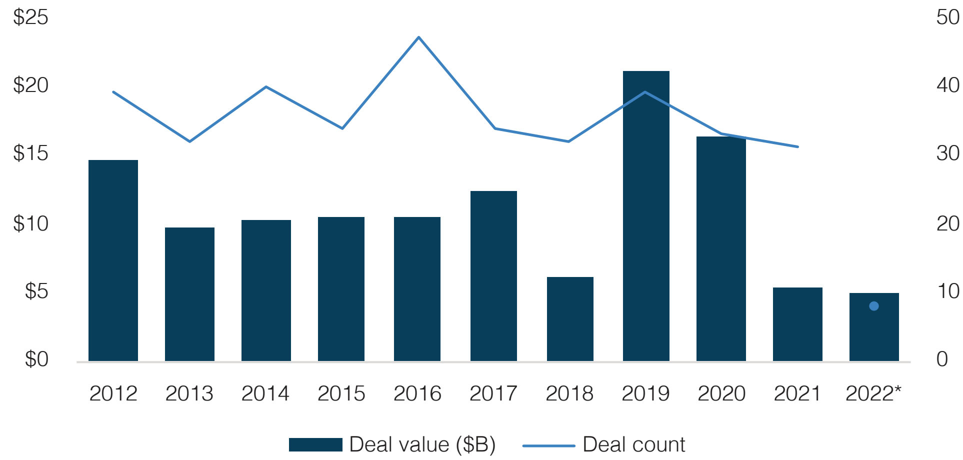
Beyond the numbers, Core & Main also transformed as a workplace under CD&R sponsorship. CD&R is behind the Lean Forward women's leadership summit, which promotes career growth at CD&R portfolio companies. Laura Schneider, Core & Main's chief human resources officer, said that the "customized training [its] team has received… has been a game changer for developing the next generation of leaders for Core & Main."[2]

2: "Clayton, Dubilier & Rice Summit Focuses on Women Leaders," Core & Main, August 1, 2019.

Creating new energy producers

Private equity takes a creative approach to the energy sector. That includes carveouts, which can come in the form of underutilized or undeveloped properties. PE firms are capable of buying those assets from sprawling producers such as Shell or BP and turn them into smaller but independent producers. In fact, PE investors can create energy producers from scratch—hiring management teams and oilfield workers within months—and provide those new producers with whatever resources they need to get off the ground.

Energy PE carveout deal activity



Source: PitchBook | Geography: US
*As of June 15, 2022

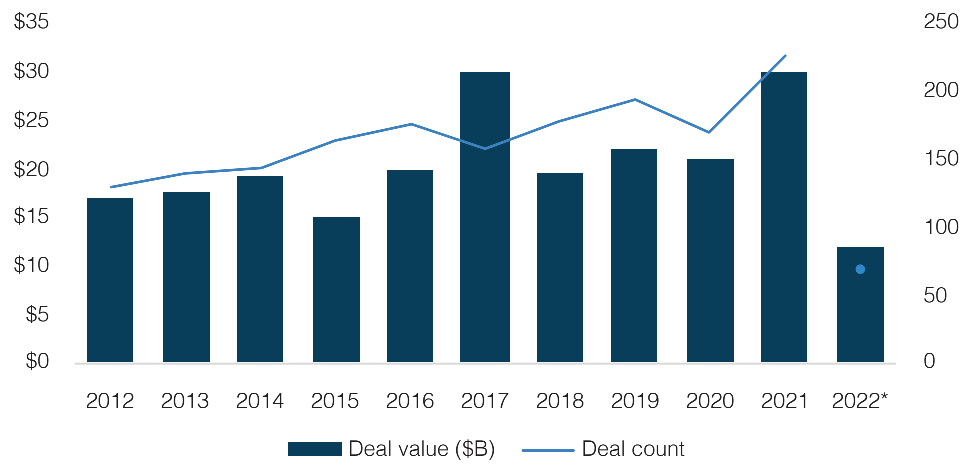
Private equity at work

In 2014, AEA Investors acquired the water technologies business of Siemens AG. Renamed Evoqua Water Technologies, the business provided water treatment solutions to a range of customers. In Evoqua, AEA recognized several improvements to be made and recruited new senior talent to implement them. Under AEA, Evoqua expanded its footprint in medical water, irrigation systems, industrial wastewater, and several other types of water purification systems. By the time Evoqua was taken public in 2017, the company's financial performance had improved thanks to organizational and process upgrades—transforming it into one of the best standalone water companies in the world.

Carving out business products & services

The business services sector is a valuable source for new companies. Among other companies, PE firms have rejuvenated business units from AIG, Thompson Reuters, PricewaterhouseCoopers, Aramark, and Waste Management. Thanks to creative carveouts, the US economy benefits from new companies in human capital, media, accounting, logistics, educational training, and environmental services.

Business products & services PE carveout deal activity



Source: PitchBook | Geography: US
*As of June 15, 2022

Private equity at work

In 2013, AIC member Platinum Equity took over the equipment rental business of Volvo for $1.1 billion. Volvo Rents had been in business since 2001, but Volvo leadership felt that "Volvo Rents' business [did] not have a sufficiently strong connection with [Volvo's] core operation to motivate continued ownership."[3] Its new owner, Platinum Equity, had a long history of transforming under-resourced businesses into new, standalone companies. Platinum Equity renamed the company BlueLine Rental and set it on an ambitious growth path, servicing industries such as construction, oil & gas, power, metals, and industrial manufacturing.

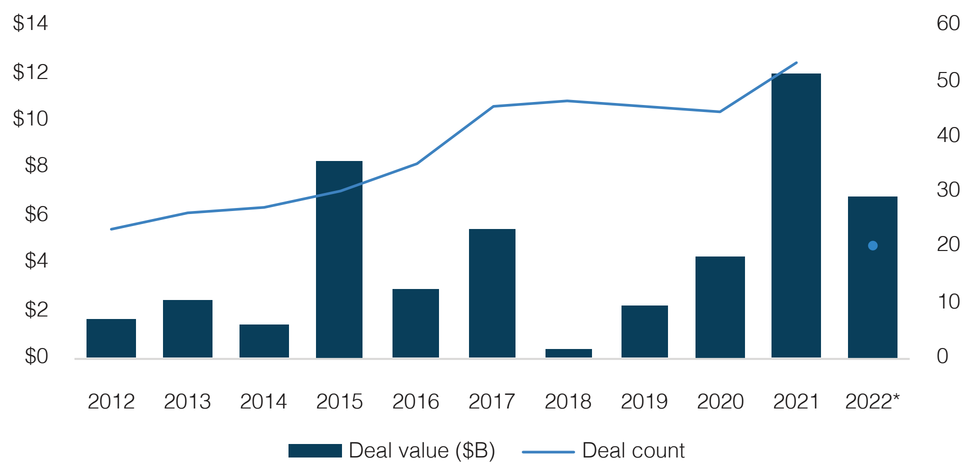
After five years under PE sponsorship and earnings improvements, BlueLine Rental was sold to United Rentals for $2.1 billion. BlueLine evolved into an industry leader and brought substantial assets to United Rentals, which felt it was going to market "with more talent, capacity and customer diversification than ever before."[4]

3: "AB Volvo To Sell Volvo Rents in North America," Volvo, December 10, 2013.
4: "United Rentals To Acquire BlueLine Rental for $2.1 Billion," United Rentals, September 10, 2018.

Downloading software units

The software sector is becoming an increasingly rich source of new, independent companies. Over the past decade, PE's expertise in software has strengthened, which makes it easier to identify promising business units and their growth trajectories. Innovative deals have been struck with Dell Technologies, Intuit, and Compuware, and software's rise as a market means more independent companies in operating systems, network management, financial and educational software, and productivity tools in the years ahead.

Software PE carveout deal activity



Source: PitchBook | Geography: US
*As of June 15, 2022

Private equity at work

Changepoint

In 2014, AIC member Marlin Equity Partners acquired three divisions of Compuware, with plans to create a new company. At the time, Compuware was under pressure to make changes from activist investors, and Marlin was able to come up with its acquisition plan in less than six weeks. The deal included two software units, as well as an IT services business unit. Marlin's plan was to merge all three into a new software company called Changepoint, which would offer professional services automation and project portfolio management software to professional service organizations. After seven years of growth under Marlin, Changepoint was acquired by Planview, a similar company backed by AIC members TPG and TA Associates. At the end of Changepoint's long journey came "an unmatched level of expertise, IP, and resources to ensure [Planview's customers'] more important outcomes are delivered with efficiency, urgency, and transparency."[5]

5: "Planview Announces Strategic Acquisitions of Clarizen and Changepoint to Accelerate Strategy to Delivery for Enterprises," Planview, January 12, 2021.

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INVESTING IN AMERICA

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- Support good-paying jobs
- Boost the American economy
- Strengthen public pensions

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Economic contribution of the US private equity sector in 2022

Prepared for the American Investment Council

April 2023

EY
Building a better working world

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Executive summary

This report estimates the current economic activity of, and related to, the US private equity sector – i.e., US private equity firms and private equity-backed companies – within the US economy in 2022.

Key findings include:

Economic activity of the US private equity sector

- **Employment and wage contribution.** The US private equity sector provides employment and earnings for millions of workers. Overall, in 2022, the US private equity sector directly employed 12 million workers earning $1 trillion in wages and benefits.[1] The average US private equity sector worker earned approximately $80,000 in wages and benefits in 2022. For a full-time worker this is approximately $41 per hour.[2] The median full-time US private equity sector worker earned approximately $50,000 in 2022.[3]

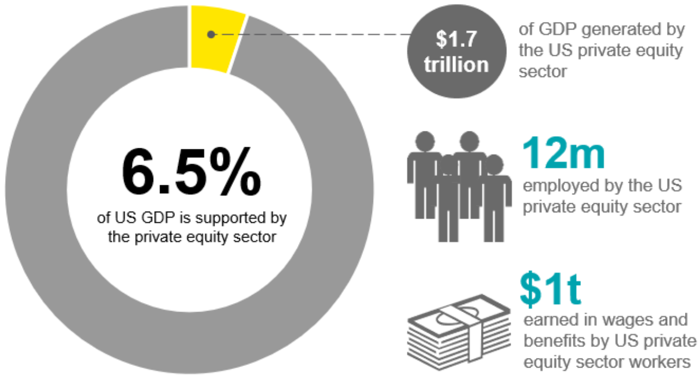
- **Share of US economic activity.** The US private equity sector directly generated $1.7 trillion of gross domestic product (GDP) in the United States in 2022. GDP measures a sector's or industry's contribution to the production of final goods and services produced in the United States. The US private equity sector comprised approximately 6.5% of US GDP in 2022.

- **PE-backed small businesses.** In 2022, the median PE-backed business employed 69 workers. Moreover, approximately 85% of PE-backed businesses were small businesses (i.e., had fewer than 500 employees). PE-backed small businesses directly employed a total of 1.4 million workers throughout the US economy in 2022. These workers earned $135 billion in wages and benefits and generated $240 billion of GDP.

- **Tax contribution.** The US private equity sector generates tax revenue through US private equity firms, private equity-backed companies, and its employees. In 2022, the US private equity sector paid $304 billion of federal, state, and local taxes. Approximately two-thirds of these were federal taxes ($208 billion) with the remaining taxes paid to state and local governments ($95 billion).

[1] All numbers are prorated to account for cases where private equity owns less than 100% of a company.
[2] This $80,000 is computed prior to rounding the wages and benefits and employment estimates. In particular, the $1 trillion of wages and benefits is approximately $961 billion and 12 million employees is approximately 11.957 million employees.
[3] By comparison, the comparable median wage for the US economy is approximately $50,000 and comparable average wage is approximately $73,000. See report for more detail.

Figure E-1. Economic contribution of the US private equity sector, 2022



Note: Figure only includes economic activity of the US private equity sector (i.e., the economic activity at US private equity firms and private equity-backed companies). Wages and benefits includes all labor income (i.e., employee cash compensation and benefits, as well as proprietors' income). Wages and benefits is a component of GDP.
Source: PitchBook; Dun & Bradstreet; US Bureau of Economic Analysis; EY analysis.

Economic activity related to the US private equity sector

- **Suppliers to the US private equity sector.** Suppliers to the US private equity sector employed an additional 7.8 million workers throughout the US economy earning $700 billion in wages and benefits and generating $1.1 trillion of US GDP in 2022. Suppliers to PE-backed small businesses (i.e., a subset of this) employed 1.3 million workers earning $110 billion of wages and benefits and generating $180 billion of US GDP. Wages and benefits is a component of GDP. This economic activity supported $207 billion of taxes – federal ($142 billion) and state and local ($65 billion).

- **Related consumer spending.** The consumer spending of workers of the US private equity sector and the sector's suppliers supported an additional 11.5 million workers throughout the US economy earning $700 billion in wages and benefits and generating $1.3 trillion of US GDP in 2022. Consumer spending related to PE-backed small businesses and their suppliers (i.e., a subset of this) supported 1.8 million workers earning $115 billion of wages and benefits and generating $195 billion of US GDP. Consumer spending related to the US private equity sector supported $235 billion of taxes – federal ($161 billion) and state and local ($74 billion).

Total economic activity of, and related to, the US private equity sector

In total, the US private equity sector, the sector's US suppliers, and the related US consumer spending supported an estimated 31.3 million workers earning $2.4 trillion in wages and benefits and generating $4.0 trillion in US GDP in 2022. PE-backed small businesses, their suppliers, and related consumer spending (i.e., a subset of this) together supported 4.4 million workers earning $360 billion in wages and benefits and generating $615 billion GDP. Additionally, the federal, state, and local taxes paid by, and related to, the US private equity sector totaled more than $700 billion in 2022.

Table E-1. Total economic activity of, and related to, the US private equity sector, 2022

Millions of jobs; trillions of dollars

	US private equity sector	Suppliers to US private equity	Related consumer spending	Total
Employment	12.0	7.8	11.5	31.3
Wages and benefits	$1.0	$0.7	$0.7	$2.4
GDP	$1.7	$1.1	$1.3	$4.0
Taxes paid	$0.3	$0.2	$0.2	$0.7

Note: Wages & benefits includes all labor income (i.e., employee cash compensation and benefits, as well as proprietors' income). Wages & benefits is a component of GDP. Figures may not sum due to rounding.
Source: EY analysis.

Contents

Economic contribution of the US private equity sector in 2022

I. Introduction

This report estimates the current economic activity of, and related to, the US private equity sector – US private equity firms and private equity-backed companies – within the US economy in 2022, a single point in time. As such, the estimates provide a snapshot of the economic footprint of the sector as measured by employment, wages & benefits, gross domestic product (GDP), and taxes. By providing information on the overall scope of the industry, measured and defined in several different ways, this report attempts to shed light on the reach of the private equity industry with the US economy.

Overall, the US private equity sector provides employment and earnings for millions of workers and contributes jobs and earnings to other sectors of the US economy that relate to private equity operations. In 2022, the US private equity sector directly employed 12 million workers throughout the US economy earning $1 trillion in wages and benefits and generating $1.7 trillion of GDP. Suppliers to the US private equity sector employed an additional 7.8 million workers throughout the US economy earning $700 billion in wages and benefits and generating $1.1 trillion of GDP. The consumer spending of workers of the US private equity sector and the sector's suppliers supported an additional 11.5 million workers throughout the US economy earning $700 billion in wages and benefits and generating $1.3 trillion of GDP.

Overview of private equity

Private equity firms partner with investors to form funds that invest in companies, primarily those in need of retooling or that are on the cusp of growth. The aim of the investment, which most often takes the form of a majority stake, is to help bolster the company through use of the private equity firm's access to capital and its strategic, financial, and operational expertise. Ultimately, transforming the target company's operations generates returns for the private equity fund, the private equity firm that manages the fund, and the fund's investors.

Private equity firms partner with a variety of investor types, including pension funds, university endowments, charitable foundations, and insurance companies. Private equity funds invest across a range of industries such as energy, healthcare, manufacturing, retail, and technology. In 2022, the US private equity sector included approximately 5,000 private equity firms and 18,000 PE-backed companies. Jobs at private equity firms are estimated to be less than 1% of US private equity sector employment.

Though some sector participants use the term private equity in different ways, for the purposes of this analysis private equity only includes private investment in growth capital or established companies aiming to improve the company. In contrast, venture capital – which is not included in the definition of private equity used for this analysis – consists of private investment in startup and early-stage companies.

The most common private equity fund types are: (1) buyout funds, and (2) growth equity funds. For the purposes of this study all deals in both subcategories will be referred to as private equity funds.

Private equity funds that invest in more mature businesses

An established company may, perhaps due to increased competition, the changing structure of an industry or its markets, or high overhead costs, perform below its potential. Identifying and addressing the root cause of underperformance, however, often requires expertise and potentially significant infusions of capital. A private equity fund can provide both.

Obtaining a controlling stake in such a company often requires significant investment. A private equity fund often finances acquisitions, in part through debt issuance, sometimes a cost-effective method for such transactions. In a typical case, the target company's future cash flows are the collateral. The private equity fund generates returns on its investment by maximizing profits net of interest expenses and payments of the debt principal. A successful private equity fund often has expertise in helping lift the performance of target companies, and both the private equity fund and target company can benefit from a buyout.

In addition, achieving higher performance may involve changes in higher-level management or refocusing of the target's business purposes by spinning off peripheral business components. Sweeping changes to a company are not always welcome by all stakeholders, particularly in the short term. The intervention of an outside actor can be not only beneficial, but critical to achieve change and realign a target company.

Growth equity funds

Growth equity funds are private equity funds that invest in companies to foster expansion. Growth equity target companies often have established business models, revenues, and operating profits, but are unable to raise sufficient capital to undertake a significant expansion. Such an expansion could include moving the company into new markets, facilitating new product development, or possibly a strategic acquisition.

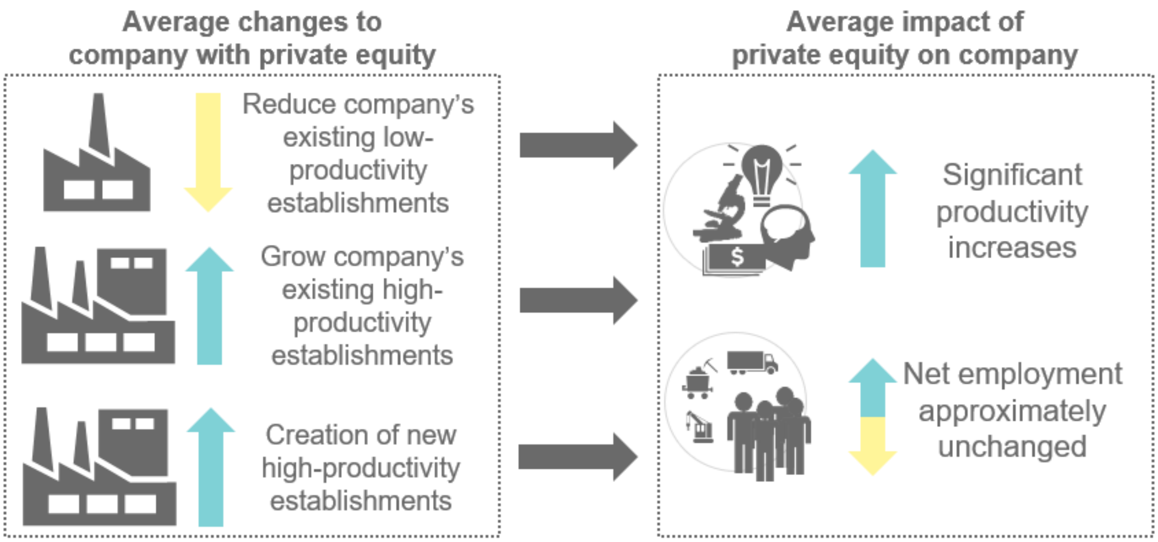
The target company for a growth equity fund, were it not for such investors, could be in a difficult spot. There are significant cost and regulatory hurdles to raising public capital. The target company might also not be able to rely on venture capital investment. Venture capital generally specializes in providing relatively smaller levels of financing and generating returns from very high revenue growth from very young companies that might be less likely from a more established company. Growth equity private equity funds, in effect, are a middle ground between venture capital that targets startup companies and private equity funds that focus on well-established companies in need of retooling.

Growth equity investors rely on the company's revenue growth to generate returns, which can be accomplished by providing additional capital, as well as through strategic and operational support from the private equity firm. Growth equity funds typically undertake a significant role in the target company's day-to-day operations.

Private equity-backed company performance

Notably, given the access to capital and expertise of private equity firms, private equity-backed companies often have better prospects for experiencing rapid growth and restructuring as compared to similar companies without a private equity investment. For example, as seen in Figure 1, a recent study analyzing the performance of 3,200 private equity-backed companies with more than 150,000 establishments from 1980 through 2005 estimates that, on average, two years after a private equity investment the productivity of a private equity-backed company increases significantly with a near-zero net employment change relative to a comparable company without private equity investment.[1] The study found that, relative to a comparable company, an average private equity-backed company was more likely to reduce a company's low-productivity establishments, grow a company's high-productivity establishments, and create new high-productivity establishments; that is, refocus the company on its higher-productivity activities.

Figure 1. Estimated average impact of private equity investment in a company after two years



Note: Figure shows the average changes to a company with a private equity investment relative to a comparable company without it. That is, the changes displayed are estimates of what would have happened to a private equity-backed company, on average, if not for a private equity investment. Results are from an analysis of 3,200 PE-backed companies with more than 150,000 establishments over the 1980-2005 period for two years after the LBO.
Source: Davis et al. (2014) and Davis et al. (2019).

II. Economic activity of the US private equity sector

The US private equity sector, comprised of US private equity firms and private equity-backed companies, provides employment and income for millions of workers and contributes to jobs in other sectors of the economy that are connected to private equity operations.

The economic activity described in this report includes the following indicators:

- **Employment.** Employment is measured as the total headcount of workers. For example, a company with three full-time workers and a company with two full-time workers and one part-time worker would both be measured as having three workers.
- **Wages and benefits.** Wages and benefits includes employee cash compensation and benefits as well as proprietor income.[2] Wages and benefits is a component of GDP.
- **GDP.** GDP measures a sector's contribution to the production of all final goods and services produced in the United States.

As displayed in Table 1, the US private equity sector supported 12 million jobs in 2022. The table also displays the type of economic activity of these jobs.[3] The largest share of US private equity sector employment was estimated to be in business services. Business services accounted for 4.3 million jobs, or 36% of US private equity sector employment in 2022. These services include finance and insurance, real estate and rental and leasing, professional, scientific, and technical services, management of companies and enterprises, administrative and support services, and waste management and remediation services. Included in business services are US private equity firms. However, the vast majority of US private equity sector workers were estimated to be employed at private equity-backed companies, as opposed to private equity firms. Jobs at private equity firms are estimated to comprise less than 1% of US private equity sector employment.[4]

Personal services employed the second largest share of US private equity sector workers with 3.1 million jobs, or 26% of US private equity sector employment. Personal services includes healthcare, accommodation, food services, recreation, and other personal services. US private equity sector employment in manufacturing is the third largest segment of the sector's employment. In particular, 1.5 million workers were estimated to be employed in manufacturing in 2022. This is 13% of total US private equity sector employment. These three segments of the US private equity sector – personal services, business services, and manufacturing – comprise approximately three-quarters of the sector's total employment. Other significant segments of the sector include retail trade (0.8 million jobs; 7% of total), information (0.8 million jobs; 6% of total), wholesale trade (0.4 million jobs; 4% of total), transportation and warehousing (0.4 million jobs; 3% of total), and construction (0.3 million jobs; 3% of total).

Table 1. US private equity sector employment by type of economic activity, 2022

Millions of jobs

	Jobs	% of total
Business services	4.3	36%
Personal services	3.1	26%
Manufacturing	1.5	13%
Retail trade	0.8	7%
Information	0.8	6%
Wholesale trade	0.4	4%
Transportation and warehousing	0.4	3%
Construction	0.3	3%
Utilities	0.2	1%
Mining, quarrying, and oil and gas extraction	0.1	1%
Agriculture, forestry, fishing, and hunting	*	*
Total employment	**12.0**	**100%**

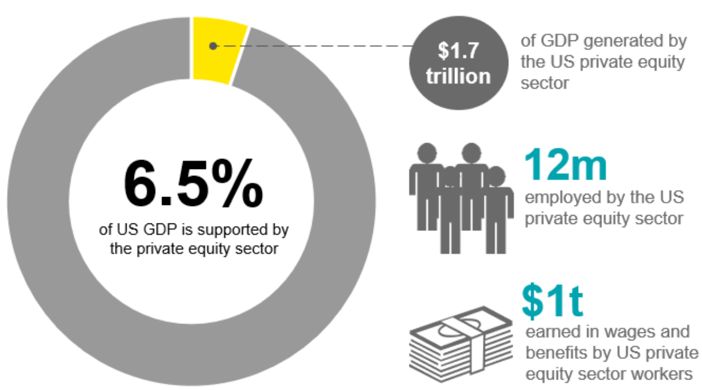
*Less than 0.05 million jobs or 0.5%

Note: Companies industry classifications use the North American Industry Classification System (NAICS), which is commonly used in government statistics. Company NAICS classifications were generally identified using Dun & Bradstreet. Figures may not sum due to rounding.

Source: PitchBook; Dun & Bradstreet; and EY analysis.

Overall, in 2022, the US private equity sector employed 12 million workers throughout the US economy who earned $1 trillion in wages and benefits and generated $1.7 trillion of GDP.[5] Wages and benefits is a component of GDP. This amounts to an average worker in the US private equity sector earning approximately $80,000 in wages and benefits.[6] The comparable average wages and benefits for the US economy is approximately $73,000.[7] For a full-time worker this is approximately $41 per hour.[8] The median full-time US private equity sector worker earned approximately $50,000 in 2022.[9] The comparable median for the US economy is also approximately $50,000.[10] Additionally, as summarized in Figure 2, the $1.7 trillion of GDP of the US private equity sector in 2022 was approximately 6.5% of US GDP ($25.5 trillion in 2022).[11]

Figure 2. Economic contribution of the US private equity sector, 2022



Note: Figure only includes economic activity of the US private equity sector (i.e., the economic activity at US private equity firms and private equity-backed companies). Wages and benefits includes all labor income (i.e., employee cash compensation and benefits, as well as proprietors' income). Wages and benefits is a component of GDP.

Source: PitchBook; Dun & Bradstreet; US Bureau of Economic Analysis; EY analysis.

III. Economic activity related to the US private equity sector

In addition to the economic activity of the US private equity sector, this report also estimates the related economic activity of: (1) suppliers to the US private equity sector, and (2) related consumer spending:

- **Suppliers to the US private equity sector.** The US private equity sector purchases goods and services from other businesses, which support jobs, wages and benefits, and GDP at these supplier businesses. For example, the US private equity sector's expenditures on utilities, telecommunications, raw materials, and security, among other goods and services, support sales at suppliers. Moreover, demand for these goods and services leads to additional rounds of economic activity as suppliers to the US private equity sector purchase operating inputs from their own suppliers. Goods and services imported from abroad are not included in this report's estimates of US economic activity.

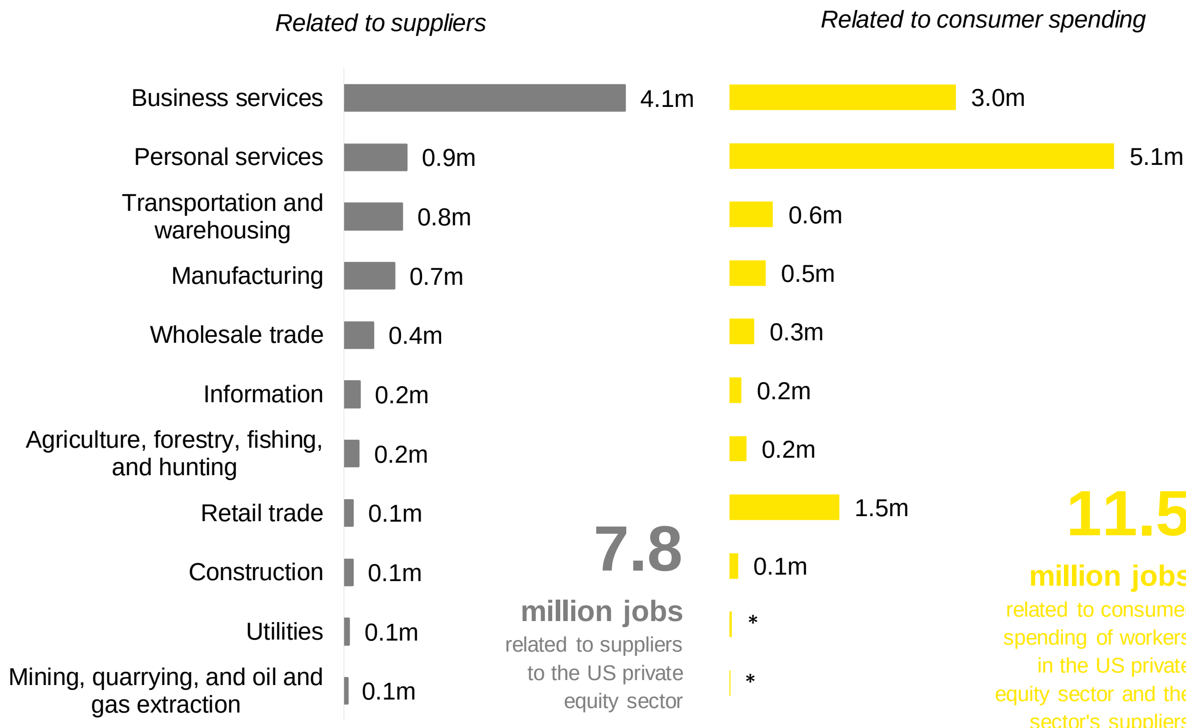
- **Related consumer spending.** Related consumer spending refers to the consumer spending supported by workers in the US private equity sector and their suppliers. When these workers spend their earnings at US businesses (e.g., grocery stores, retailers, movie theaters), they support economic activity in those sectors. The earnings that these workers spend on food at a restaurant, for example, creates jobs at the restaurant and at farms, transportation companies, and other industries that are involved in the restaurant's supply chain.

The magnitude of the economic activity related to the US private equity sector is estimated with the 2021 Impacts for Planning (IMPLAN) input-output model of the United States. Unlike other economic models, IMPLAN includes the interaction of more than 500 industries, thus identifying the interaction of specific industries that are related to the US private equity sector. See the Appendix for further details.

As displayed in Figure 3, suppliers to the US private equity sector were estimated to support 7.8 million jobs throughout the US economy in 2022. The largest segments of suppliers to the US private equity sector were estimated to be business services (4.1 million jobs; 52% of total), personal services (0.9 million jobs; 12% of total), and transportation and warehousing (0.8 million jobs; 11% of total). These three supplier industries comprise three-quarters of the total employment related to suppliers to the US private equity sector. The remaining related supplier employment includes manufacturing (0.7 million jobs; 9% of total), wholesale trade (0.4 million jobs; 5% of total), information (0.2 million jobs; 3% of total), agriculture, forestry, fishing, and hunting (0.2 million jobs; 3% of total), retail trade (0.1 million jobs; 2% of total), construction (0.1 million jobs; 2% of total), utilities (0.1 million jobs; 1% of total), and mining, quarrying, and oil and gas extraction (0.1 million jobs; 1% of total).

Consumer spending of workers in the US private equity sector and the sector's suppliers was estimated to support 11.5 million jobs throughout the US economy in 2022. The largest segments of employment related to the consumer spending of workers in the US private equity sector and the sector's suppliers were estimated to be personal services (5.1 million jobs; 44% of total), business services (3.0 million jobs; 26% of total), and retail trade (1.5 million jobs; 13% of total). These three industries comprise more than three-quarters of the related economic activity. The remaining employment related to the consumer spending of workers in the US private equity sector and the sector's suppliers includes transportation and warehousing (0.6 million jobs; 5% of total), manufacturing (0.5 million jobs; 4% of total), wholesale trade (0.3 million jobs; 3% of total), agriculture, forestry, fishing, and hunting (0.2 million jobs; 2% of total), information (0.2 million jobs; 1% of total), and construction (0.1 million jobs; 1% of total).

Figure 3. Economic activity related to the US private equity sector, 2022



*Less than 0.05m
Note: Industry definitions are based on the North American Industry Classification System (NAICS). Figures may not sum due to rounding.
Source: EY analysis.

Table 2 summarizes the estimated economic activity of, and related to, the US private equity sector in the 2022 US economy. The private equity sector directly employed a total of 12 million workers throughout the US economy who earned $1 trillion in wages and benefits and generated $1.7 trillion of GDP. Wages and benefits is a component of GDP. Suppliers to the US private equity sector employed an additional 7.8 million workers throughout the US economy who earned $700 billion in wages and benefits and generated $1.1 trillion of GDP. In addition, the consumer spending of workers in the US private equity sector and its suppliers employed 11.5 million workers throughout the US economy who earned $700 billion in wages and benefits and generated $1.3 trillion of GDP.

Table 2. Total economic activity of, and related to, the US private equity sector, 2022

Millions of jobs; trillions of dollars

	US private equity sector	Suppliers to US private equity	Related consumer spending	Total
Employment	12.0	7.8	11.5	31.3
Wages and benefits	\$1.0	\$0.7	\$0.7	\$2.4
GDP	\$1.7	\$1.1	\$1.3	\$4.0

Note: Wages & benefits include all labor income (i.e., employee cash compensation and benefits, as well as proprietors' income). Wages and benefits is a component of GDP. Figures may not sum due to rounding.
Source: EY analysis.

IV. Economic contribution of US PE-backed small businesses

As previously noted, in 2022, the private equity sector included approximately 18,000 PE-backed companies. As seen in Table 3, approximately 85% were businesses with fewer than 500 employees. Moreover, 14% had fewer than 10 employees, 40% had fewer than 50 employees, and more than 60% had fewer than 100 employees. Throughout this report, small businesses refers to PE-backed companies with fewer than 500 employees. The median PE-backed business employed 69 employees.

Table 3. PE-backed companies and jobs, by company size
Thousands of companies; thousands of jobs

Number of employees	Companies	Share of companies	Jobs	Share of jobs
<10	2.6	14%	14	*
11 to 50	4.8	26%	127	1%
51 to 100	3.9	21%	275	2%
101 to 500	4.4	24%	1,024	9%
501 to 1,000	1.0	6%	771	7%
1,001 to 5,000	1.3	7%	2,839	24%
5,001+	0.4	2%	6,816	57%
Total	**18.4**	**100%**	**11,867**	**100%**

*Less than 0.5%
Note: Figures are rounded. The numbers are reflecting PE-backed companies only, without PE firms themselves.
Source: PitchBook; Dun & Bradstreet; and EY analysis.

As displayed in Table 4, PE-backed small businesses supported 1.4 million jobs in 2022. The largest share of PE-backed small business employment was in business services, which included 476,000 jobs, or 35% of PE-backed small business employment in 2022. The second largest sector was manufacturing, which included 265,000 jobs, or 19% of PE-backed small business employment. The third largest sector was personal services, which included 207,000 jobs, or 15% PE-backed small business employment. These three sectors (business services, manufacturing, and personal services) comprised approximately 70% of total PE-backed small business employment in 2022.

Table 4. PE-backed small businesses by type of economic activity, 2022
Thousands of jobs

	Jobs	% of total
Business services	476	35%
Manufacturing	265	19%
Personal services	207	15%
Information	118	9%
Wholesale trade	109	8%
Retail trade	61	4%
Construction	52	4%
Transportation and warehousing	34	2%
Mining, quarrying, and oil and gas extraction	24	2%
Utilities	19	1%

Agriculture, forestry, fishing, and hunting	6	*
Total employment	**1,371**	**100%**

*Less than 0.5%
Note: Companies industry classifications use the North American Industry Classification System (NAICS), which is commonly used in government statistics. Company NAICS classifications were generally identified using Dun & Bradstreet. Figures may not sum due to rounding.
Source: PitchBook; Dun & Bradstreet; and EY analysis.

Table 5 summarizes the estimated economic activity of, and related to, PE-backed small businesses in 2022. PE-backed small businesses directly employed a total of 1.4 million workers throughout the US economy. These workers earned $135 billion in wages and benefits and generated $240 billion of GDP. Wages and benefits is a component of GDP.

Suppliers to PE-backed small businesses employed an additional 1.3 million workers throughout the US economy. These workers earned $110 billion in wages and benefits and generated $180 billion of GDP. In addition, the consumer spending of workers at PE-backed small businesses and their suppliers employed 1.8 million workers throughout the US economy. These workers earned $115 billion in wages and benefits and generated $195 billion of GDP.

In total, the economic activity of, and related to, PE-backed small businesses supported 4.4 million jobs, $360 billion of wages and benefits, and $615 billion of GDP.

Table 5. Total economic activity of, and related to, the US PE-backed small businesses, 2022

Millions of jobs; billions of dollars

	US private equity sector	**Suppliers to US private equity**	**Related consumer spending**	**Total**
Employment	1.4	1.3	1.8	4.4
Wages & benefits	$135	$110	$115	$360
GDP	$240	$180	$195	$615

Note: Wages & benefits include all labor income (i.e., employee cash compensation and benefits, as well as proprietors' income). Wages and benefits is a component of GDP. Figures may not sum due to rounding.
Source: EY analysis.

IV. State distribution of economic activity of, and related to, the US private equity sector

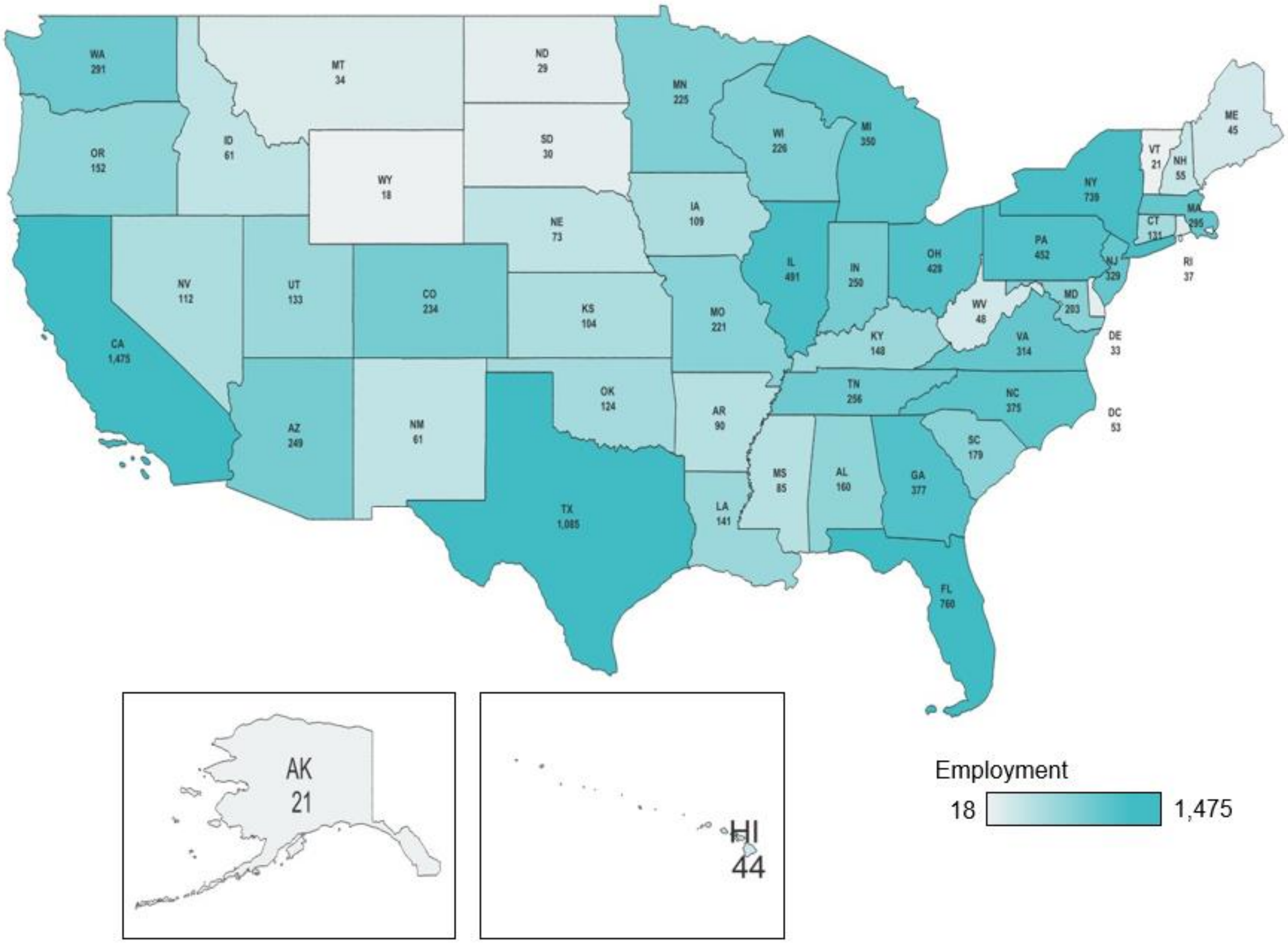
The distribution of jobs, wages and benefits, and GDP by state (plus the District of Columbia) of the economic activity of the US private equity sector is displayed in Table 6 and Figure 4. The states estimated to have the most US private equity sector employment are: (1) California (1.5 million jobs), (2) Texas (1.1 million jobs), (3) Florida (760,000 jobs), (4) New York (739,000 jobs), and (5) Illinois (491,000 jobs).

Table 6. Direct economic activity of the US private equity sector by state, 2022
Thousands of jobs; billions of dollars

	Jobs	Wages & benefits	GDP		Jobs	Wages & benefits	GDP
Alabama	160	13	22	Montana	34	3	4
Alaska	21	2	3	Nebraska	73	6	11
Arizona	249	19	33	Nevada	112	8	12
Arkansas	90	7	12	New Hampshire	55	4	7
California	1,475	123	211	New Jersey	329	26	45
Colorado	234	20	35	New Mexico	61	5	8
Connecticut	131	10	18	New York	739	60	102
Delaware	33	2	4	North Carolina	375	29	50
District of Columbia	53	5	7	North Dakota	29	3	5
Florida	760	56	95	Ohio	428	34	59
Georgia	377	30	51	Oklahoma	124	10	18
Hawaii	44	3	5	Oregon	152	12	21
Idaho	61	5	8	Pennsylvania	452	36	62
Illinois	491	39	65	Rhode Island	37	3	5
Indiana	250	19	34	South Carolina	179	13	23
Iowa	109	9	15	South Dakota	30	2	4
Kansas	104	8	15	Tennessee	256	20	34
Kentucky	148	12	20	Texas	1,085	93	161
Louisiana	141	12	20	Utah	133	11	19
Maine	45	3	6	Vermont	21	2	3
Maryland	203	16	26	Virginia	314	25	41
Massachusetts	295	26	44	Washington	291	26	48
Michigan	350	28	47	West Virginia	48	4	7
Minnesota	225	19	32	Wisconsin	226	18	31
Mississippi	85	6	11	Wyoming	18	2	3
Missouri	221	18	30	**United States**	**11,957**	**$961**	**$1,652**

Note: Table only includes employment at US private equity firms and private equity-backed companies. Wages and benefits includes all labor income (i.e., employee compensation and proprietor income). Wages and benefits is a component of GDP. Figures may not sum due to rounding.
Source: EY analysis.

Figure 4. Economic activity of the US private equity sector by state, 2022
Thousands of jobs



Note: Figure only includes employment at US private equity firms and private equity-backed companies. Figures may not sum due to rounding.
Source: EY analysis.

The distribution of jobs, wages and benefits, and GDP by state (plus the District of Columbia) of economic activity of, and related to, the US private equity sector is displayed in Table 7 and Figure 5. The states estimated to have the most employment in or related to the US private equity sector are: (1) California (3.9 million jobs), (2) Texas (2.8 million jobs), (3) Florida (2.0 million jobs), (4) New York (1.9 million jobs), and (5) Illinois (1.3 million jobs).

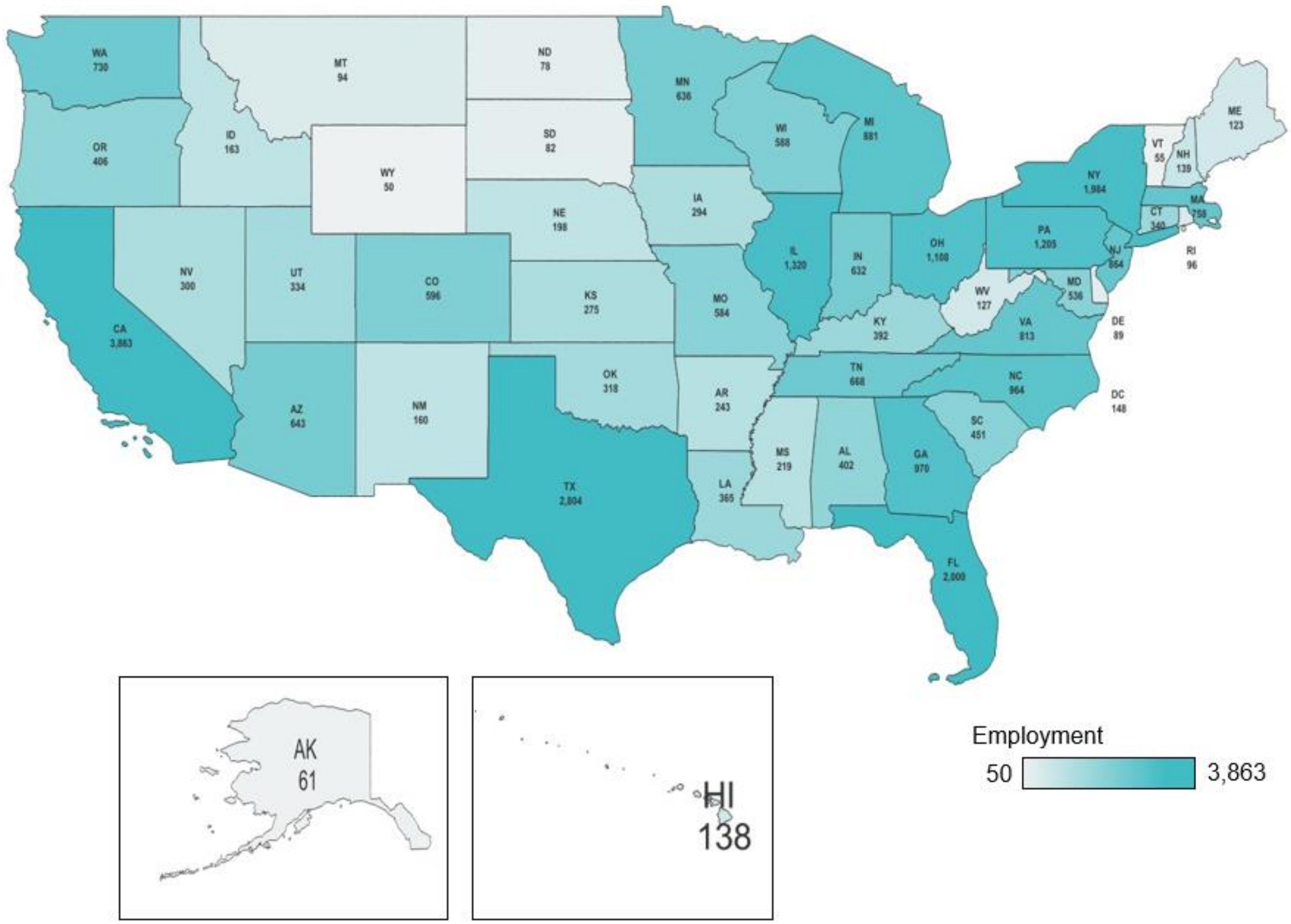
Table 7. Total economic activity of, and related to, the US private equity sector by state, 2022

Thousands of jobs; billions of dollars

	Jobs	Wages & benefits	GDP		Jobs	Wages & benefits	GDP
Alabama	402	30	51	Montana	94	7	11
Alaska	61	5	8	Nebraska	198	15	25
Arizona	643	47	80	Nevada	300	20	33
Arkansas	243	18	30	New Hampshire	139	10	17
California	3,863	295	509	New Jersey	864	65	108
Colorado	596	46	82	New Mexico	160	12	20
Connecticut	340	25	43	New York	1,984	149	254
Delaware	89	6	10	North Carolina	964	71	121
District of Columbia	148	12	19	North Dakota	78	6	11
Florida	2,000	142	245	Ohio	1,108	84	141
Georgia	970	72	123	Oklahoma	318	24	42
Hawaii	138	10	15	Oregon	406	30	51
Idaho	163	11	19	Pennsylvania	1,205	91	150
Illinois	1,320	100	174	Rhode Island	96	7	11
Indiana	632	46	79	South Carolina	451	32	56
Iowa	294	22	36	South Dakota	82	6	10
Kansas	275	21	35	Tennessee	668	49	82
Kentucky	392	28	47	Texas	2,804	222	393
Louisiana	365	28	49	Utah	334	25	43
Maine	123	9	15	Vermont	58	4	7
Maryland	536	40	65	Virginia	813	62	99
Massachusetts	758	60	99	Washington	730	57	104
Michigan	881	67	113	West Virginia	127	10	17
Minnesota	636	51	92	Wisconsin	588	44	74
Mississippi	219	15	26	Wyoming	50	4	7
Missouri	584	44	74	**United States**	**31,289**	**$2,356**	**$4,021**

Note: Table includes employment at US private equity firms and private equity-backed companies as well as the related supplier and consumer spending employment. Wages and benefits includes all labor income (i.e., employee compensation and proprietor income). Wages and benefits is a component of GDP. Figures may not sum due to rounding.
Source: EY analysis.

Figure 5. Total economic activity of, and related to, the US private equity sector by state, 2022
Thousands of jobs



Note: Figure includes employment at US private equity firms and private equity-backed companies as well as the related supplier and consumer spending employment. Figures may not sum due to rounding.
Source: EY analysis.

V. Taxes paid by, and related to, the US private equity sector

The US private equity sector generates tax revenue through US private equity firms, private equity-backed companies, and their employees. Additionally, taxes are paid by the suppliers of the US private equity sector and paid on worker-related consumer spending. Table 8 summarizes the federal, state, and local taxes paid by, and related to, the US private equity sector in 2022. The estimates of taxes paid include all major federal, state, and local taxes (e.g. corporate and individual income taxes, sales and excise taxes, property taxes), where applicable. Taxes paid by the US private equity sector are displayed separately for those paid by businesses and those paid by its employees.

As seen in Table 8, the US private equity sector generated $304 billion of federal, state, and local taxes in 2022. Approximately two-thirds of these were federal taxes ($208 billion) with the remaining taxes being paid to state and local governments ($95 billion). About two-thirds of the $208 billion of federal taxes paid were employee taxes ($141 billion). These were primarily individual income taxes ($102 billion) and payroll taxes ($33 billion). State and local taxes were more evenly split between major tax types: property taxes ($30 billion), sales taxes ($23 billion), individual income taxes ($22 billion), excise, license, and other taxes ($17 billion), and corporate income taxes ($3 billion).

Table 8 also summarizes the federal, state, and local taxes related to the US private equity sector. Suppliers to the US private equity sector paid $207 billion of federal ($142 billion) and state and local ($65 billion) taxes. Additionally, consumer spending related to the US private equity sector supported $235 billion of federal ($161 billion) and state and local ($74 billion) taxes. Overall, the federal, state, and local taxes paid by, and related to, the US private equity sector totaled nearly $750 billion in 2022.

Table 8. Federal, state, and local taxes paid by, and related to, the US private equity sector, 2022
Billions of dollars

	US private equity sector			Suppliers of US private equity	Related consumer spending	Total
	Business taxes	Employee taxes	Total			
Federal taxes	**$67**	**$141**	**$208**	**$142**	**$161**	**$511**
Individual income taxes	14	102	116	79	90	284
Payroll taxes	33	33	66	45	51	161
Corporate income taxes	19	0	19	13	14	46
Excise taxes	1	3	4	3	3	10
Customs duties and fees	1	4	4	3	3	11
State and local taxes	**$38**	**$57**	**$95**	**$65**	**$74**	**$234**
Property taxes	16	13	30	20	23	73
Sales taxes	10	13	23	15	18	56
Individual income taxes	0	22	22	15	17	55
Excise, license, and other taxes	9	8	17	12	13	43
Corporate income taxes	3	0	3	2	3	8
Total taxes	**$106**	**$198**	**$304**	**$207**	**$235**	**$746**

Note: Figures may not sum due to rounding.
Source: EY analysis.

The distribution of state and local taxes paid by state (plus the District of Columbia) by the US private equity sector is displayed in Table 9. The states estimated to have the most taxes paid by the US private equity sector are: (1) California ($13.8 billion), (2) New York ($8.4 billion), (3) Texas ($8.2 billion), (4) Illinois ($4.2 billion), and (5) Florida ($4.1 billion).

Table 9. State and local taxes paid directly by the US private equity sector by state, 2022

Billions of dollars

Alabama	1.0	Montana	0.2
Alaska	0.1	Nebraska	0.6
Arizona	1.7	Nevada	0.7
Arkansas	0.7	New Hampshire	0.4
California	13.8	New Jersey	2.9
Colorado	1.8	New Mexico	0.6
Connecticut	1.2	New York	8.4
Delaware	0.3	North Carolina	2.6
District of Columbia	0.7	North Dakota	0.4
Florida	4.1	Ohio	3.3
Georgia	2.5	Oklahoma	0.9
Hawaii	0.4	Oregon	1.2
Idaho	0.4	Pennsylvania	3.6
Illinois	4.2	Rhode Island	0.3
Indiana	1.9	South Carolina	1.1
Iowa	0.9	South Dakota	0.2
Kansas	0.8	Tennessee	1.5
Kentucky	1.1	Texas	8.2
Louisiana	1.1	Utah	1.1
Maine	0.4	Vermont	0.2
Maryland	1.7	Virginia	2.3
Massachusetts	2.5	Washington	2.4
Michigan	2.5	West Virginia	0.4
Minnesota	2.1	Wisconsin	1.8
Mississippi	0.6	Wyoming	0.1
Missouri	1.5	United States	**$95.4**

Note: Table reports state and local tax contribution of the private equity sector. This table does not include the state and local taxes paid by suppliers of the private equity sector or state and local taxes supported by consumer spending related to the private equity sector. Figures may not sum due to rounding.
Source: EY analysis.

The distribution of state and local taxes paid by state (plus the District of Columbia) by the US private equity sector and related economic activity is displayed in Table 10. The states estimated to have the most taxes paid by, and related to, the US private equity sector are: (1) California ($33.3 billion), (2) New York ($21.0 billion), (3) Texas ($19.5 billion), (4) Illinois ($11.0 billion), and (5) Florida ($10.4 billion).

Table 10. Total state and local taxes paid by, and related to, the US private equity sector by state, 2022
Billions of dollars

Alabama	2.5	Montana	0.6
Alaska	0.4	Nebraska	1.5
Arizona	4.1	Nevada	1.8
Arkansas	1.7	New Hampshire	0.8
California	33.3	New Jersey	7.2
Colorado	4.2	New Mexico	1.4
Connecticut	2.9	New York	21.0
Delaware	0.7	North Carolina	6.3
District of Columbia	1.8	North Dakota	0.9
Florida	10.4	Ohio	8.0
Georgia	6.0	Oklahoma	2.1
Hawaii	1.3	Oregon	3.1
Idaho	1.0	Pennsylvania	9.1
Illinois	11.0	Rhode Island	0.7
Indiana	4.5	South Carolina	2.7
Iowa	2.3	South Dakota	0.5
Kansas	2.1	Tennessee	3.6
Kentucky	2.7	Texas	19.5
Louisiana	2.6	Utah	2.5
Maine	1.1	Vermont	0.5
Maryland	4.2	Virginia	5.7
Massachusetts	5.9	Washington	5.3
Michigan	6.0	West Virginia	1.0
Minnesota	5.8	Wisconsin	4.3
Mississippi	1.6	Wyoming	0.3
Missouri	3.7	**United States**	**$234.2**

Note: Table reports state and local taxes paid by, and related to, the private equity sector. This table includes the state and local taxes paid by suppliers of the private equity sector and the state and local taxes supported by consumer spending related to the private equity sector. Figures may not sum due to rounding.
Source: EY analysis.

VI. Caveats and limitations

The estimates of the economic contribution of the US private equity sector presented in this report are based on an input-output model of the US economy and the data and assumptions described elsewhere in the report. Readers should be aware of the following limitations of the modeling approach and limitations specific to this analysis.

- **The results show a snapshot of current economic contributions.** The input-output modeling approach used in this analysis shows the 2022 economic contribution of the US private equity sector based on its relationships with other industries and households in the US economy. The analysis is at a single point in time (i.e., 2022). The results do not reflect or attempt to estimate an expansion, contraction, or any other changes, or related impacts, of the sector.

- **Estimates are limited by available public information.** The analysis relies on information reported by federal government agencies (primarily the US Bureau of Economic Analysis, US Bureau of Labor Statistics, US Census Bureau, and Congressional Budget Office), and other publicly available sources (i.e., PitchBook, Dun & Bradstreet, and IMPLAN model). The analysis did not attempt to verify or validate this information using sources other than those described in the report.

- **Modeling the economic contribution of the US private equity sector relies on government industry classifications.** This report relates the activities of US private equity sector companies to the operating profiles of various industries as defined by the North American Industry Classification System (NAICS) to most effectively estimate the economic contribution of the US private equity sector. Workers in the US private equity sector are assumed to receive the average wages and benefits of workers in their respective industry and to require the level of operating input purchases characteristic of the industries into which they have been categorized. This analysis relies on estimates of the domestically purchased inputs from the IMPLAN economic model, which are estimated using aggregate trade flow data and may vary by industry.

- **Modeling the average and median wage of the US private equity sector relies on industry averages.** This report relates the activities of US private equity sector companies to the operating profiles of various industries as defined by the NAICS industry classification system to most effectively estimate the average and median wage of the US private equity sector. Workers in the US private equity sector are assumed to receive the average wages and benefits of workers in their respective industry and to require the level of operating input purchases characteristic of the industries into which they have been categorized.

- **Estimates do not reflect the economic impact of the PE industry.** This analysis does not attempt to estimate or indicate the effect or impact of the PE industry or sector on the US economy. Rather, the analysis presents estimates of the economic contribution or footprint of the PE sector. An economic impact analysis might instead analyze the impact on the US economy of a change to or in an industry or sector, perhaps due to a policy change, natural disaster, or some other exogenous factor. An economic impact analysis might attempt to account for the economic dynamics that occur in response to such a change and show the impact net of shifts of economic activity across different parts of the economy (e.g., industries, sectors) as impacts ripple through the economy.[12]

- **Input-output modeling can include double counting.** Input-output modeling can include double counting in its indirect and induced estimates. For example, a PE-backed company's suppliers or suppliers of suppliers could be a PE-backed company and consumer re-spending of income supported by private equity could be at PE-backed businesses or businesses with PE-backed suppliers. This limitation is due to the use of industry averages in estimating indirect and induced economic contributions in input-output modeling. This analysis attempts to remove double counting by assuming the private equity sector is included in the indirect and induced contributions, by industry, proportional to its direct employment share in each industry.

- **State-level results are high-level estimates.** The state-level results are an allocation of the national results to the 50 states (plus the District of Columbia) with a high-level estimate based on the industries in which the private equity sector operates. An allocation approach is necessary because sufficiently detailed data on the US private equity sector are not available by state from publicly available sources. For example, for a given private equity-backed company only a total employment number is available, not a state-by-state number.

- **Taxes paid by, and related to, the US private equity sector based on historical averages.** In general, estimates of federal, state, and local taxes paid are based on the historical relationship between federal, state, and local tax collections (by tax type) to economic activity.

- **Results are not sensitive to including or excluding employment and labor income of private equity firms.** Employment and labor income at private equity firms contribute less than 1% of the total for the private equity sector. Results included throughout this report are not sensitive to including or excluding private equity. That is, the economic and tax contribution estimates are primarily a result of PE-backed companies.

Appendix. Modeling approach

Technical details: IMPLAN model of the US economy

This analysis uses an input-output model to estimate the economic contribution of the US private equity sector in 2022. The economic multipliers in this report were estimated using the 2021 Impacts for Planning (IMPLAN) input-output model of the United States. IMPLAN is used by more than 500 universities and government agencies. Unlike other economic models, IMPLAN includes the interaction of more than 500 industries, thus identifying the interaction of specific industries that are related to the US private equity sector.

The multipliers in the IMPLAN model are based on the Leontief production function, which estimates the total economic requirements for every unit of direct output in a given industry based on detailed inter-industry relationships documented in the input-output model. The input-output framework connects commodity supply from one industry to commodity demand by another. The multipliers estimated using this approach capture all of the upstream economic activity (or backward linkages) related to an industry's production by attaching technical coefficients to expenditures. These output coefficients (dollars of demand) are then translated into dollars of GDP and wages and benefits and number of employees based on industry averages.

The multipliers presented in this report include the US private equity sector, suppliers to US private equity, and related consumer spending. Economic activity at suppliers to the US private equity sector is attributable to operating input purchases from US suppliers. Economic activity related to consumer spending is attributable to spending by US private equity sector and supplier employees based on household spending patterns. The US private equity sector is estimated to have an employment multiplier of 2.6, a wages and benefits multiplier of 2.5, and a GDP multiplier of 2.4.

In general, estimates of federal, state, and local taxes paid are based on the historical relationship between federal, state, and local tax collections (by tax type) to economic activity (measured as personal income). This ratio estimates the effective tax rates for each tax type as a share of total personal income.

Endnotes

[1] In particular, the analysis' estimates of the net differential in the employment growth rate of the private equity-backed companies relative to the comparable companies not backed by private equity after two years range from a 0.26 percentage-point increase to a 0.88 percentage-point decrease. This reflects the net effect of (1) higher rates of job destruction at a company's shrinking and exiting establishments, (2) greater job creation at a company's expanding establishments, and (3) greater job creation at new company establishments. See Steven J. Davis, John Haltiwanger, Kyle Handley, Ron Jarmin, Josh Lerner, and Javier Miranda, (2014), "Private equity, jobs, and productivity," *American Economic Review* 104(12): pp. 3956-3990. An analysis using a similar methodology for approximately 5,100 private equity buyouts between 1980 and 2011 found similar results (i.e., a statistically insignificant impact on employment and significant productivity increases), but highlighted heterogeneity within the data. In particular, on average, public-to-private buyouts resulted in significant employment declines and private-to-private buyouts and secondary deals resulted in significant employment increases. See Steven J. Davis, John Haltiwanger, Kyle Handley, Ben Lipsius, Josh Lerner, and Javier Miranda, (2019), "The Social Impact of Private Equity Over the Economic Cycle."

[2] Proprietor income includes the payments received by self-employed individuals and unincorporated business owners.

[3] Companies are classified based on the North American Industry Classification System (NAICS), which is commonly used for industry classification in government statistics. Company NAICS classifications were generally identified using Dun & Bradstreet. All numbers are prorated to account for cases where private equity owns less than 100% of a company.

[4] The only available employment data from PitchBook on US private equity firms was for the number of investment professionals. This report estimated the total number of employees at US private equity firms based on the ratio of non-investment professionals to investment professionals for the securities, commodity contracts, and other financial investments and related activities industry with data from the US Bureau of Labor Statistics.

[5] EY was provided data on 2022 private equity-backed companies by PitchBook. Private equity-backed companies only included those headquartered in the United States with an ownership status of privately held (backing), in IPO registration, or publicly held. Companies backed by venture capital were not included in these data. In order to classify each company based on the NAICS hierarchy, as well as supplement PitchBook's data with additional employment data for entities lacking this information through PitchBook, the PitchBook company list was matched to data from Dun & Bradstreet (D&B). The resulting dataset was reviewed and cleaned as per the following steps: verifying NAICS code matching for a subset of companies, as well as additional review of the top 250 companies (based on PitchBook employment data, or D&B employment data when PitchBook data was missing).

The next step of the data cleaning procedure was to verify NAICS code matching for a subset of companies, given that certain issues arise in the process of matching PitchBook data to D&B data. For instance, certain companies were classified under NAICS codes associated with holding companies as opposed to the NAICS code associated with the company's primary activity. In addition, companies may have been matched incorrectly, resulting in an incorrect NAICS code classification, or a NAICS code may simply not be provided. To address these issues, EY manually reviewed the NAICS codes for the top 250 companies (based on PitchBook employment data, or D&B employment data when PitchBook data was missing), and when errors were identified, manually updated the NAICS codes based on PitchBook descriptions of the company's primary function.

For the top 250 companies, EY verified whether there are active investors in the business according to PitchBook, and manually reviewed and excluded transactions involving portfolio acquisitions.

The wages and benefits paid to employees of private equity-backed companies and GDP generated by private equity-backed companies were estimated from industry averages with the IMPLAN model, which is discussed in the appendix to this report.

[6] This $80,000 is computed prior to rounding the wages and benefits and employment estimates. In particular, the $1 trillion of wages and benefits is approximately $961 billion and 12 million employees is approximately 11.957 million employees.

[7] The $80,000 average wage estimated in this analysis is based on industry-level labor income data from the IMPLAN model. Accordingly, the comparable average wage is the average wage for the overall US economy in the IMPLAN model, which is approximately $73,000. The main IMPLAN economic data sources are Census of Employment and Wages (Bureau of Labor Statistics), Regional Economic Accounts (Bureau of Economic Analysis), County Business Patterns (Census Bureau), and National Income and Product Accounts (Bureau of Economic Analysis).

[8] In particular, the average wages and benefits per worker number was converted to a full-time equivalent average wages and benefits per worker number and then divided by the number of hours a full-time worker works. This estimate assumes that a full-time worker works 40 hours per week 52 weeks a year (i.e., 2,080 hours).

[9] In order to calculate the median wage of employees in the private equity sector, this analysis used the American Community Survey (ACS) 2021 micro-level data of individual wage earners. The ACS is a survey conducted by the US Census Bureau that is representative of the overall United States. In particular, this analysis assumes that the wage distribution of employees in the private equity sector, by industry, follows that of the overall United States. This

assumption then facilitates the calculation of a median wage for the private equity sector. This calculation excludes proprietor income.

[10] As discussed in endnote 9, to calculate the median wage of employees in the private equity sector, this analysis used the American Community Survey (ACS) 2021 micro-level data of individual wage earners. The comparable calculation of median earnings for the overall US economy is approximately $50,000 (full-time, year-round workers 16 years and over with earnings).

[11] This is 2022 US GDP as reported by the US Bureau of Economic Analysis.

[12] A key point is that an economic impact analysis typically attempts to estimate impacts that net out shifts in economy activity across industries and sectors as the economy moves from its initial equilibrium to its new equilibrium. In contrast, an economic contribution analysis shows the gross amount of economic activity tied to an industry or sector directly, and through its suppliers and related consumer spending. The EY Quantitative Economics and Statistics (QUEST) practice has other modeling frameworks it uses to account for the shifts in economic activity and estimate net impacts.

The US Antitrust Agencies' NPRM re

Additional Information Requirements for HSR Filings

REPORT OF PROFESSOR S.P. KOTHARI

September 26, 2023

Table of Contents

I. EXECUTIVE SUMMARY

1. On June 27, 2023, the FTC and DOJ announced a Notice for Proposed Rulemaking (NPRM) regarding changes to the Hart-Scott-Rodino (HSR) form.[1] The NPRM proposes to expand radically the information that merging parties have to submit at the time of HSR filing. The vast majority of mergers that are notified to the Antitrust Agencies do not involve competitive concerns and are allowed to proceed without even a preliminary investigation. Yet the proposed rule, if enacted, would require every single HSR notification to be accompanied with the additional information, to be separately submitted by each merging party. The direct monetary cost of the additional burdens on merging parties could reach $1 to $2 billion or more. The Agencies also do not consider indirect costs, such as the potential negative impact of the additional monetary burden, potential delays, and uncertainty on the level of value-creating M&A activity.

2. The FTC purports to provide an estimate of the additional costs to parties for providing this information. The FTC's estimates are based on outdated and biased or unsupported figures and grossly underestimate the likely actual cost of complying. A survey of antitrust practitioners and company counsel indicates that the actual cost is likely to be between four and five times the FTC's estimate. This would be in addition to the non-pecuniary costs of delay that will be created by having to gather and provide the information as well as to engage with the Agencies pre-HSR to ensure that the filing will not be deemed deficient. The proposal would have an especially disproportionate effect on small transactions which typically involve companies that do not have the resources to comply with the proposed information burden.

3. Remarkably, the Agencies offer no evidence that these types of additional information would enable them to identify competitively problematic transactions that they might somehow have missed in the past. The Agencies also do not have the

[1] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178, available at https://www.federalregister.gov/documents/2023/06/29/2023-13511/premerger-notification-reporting-and-waiting-period-requirements.

manpower to review this additional information within the 30-day statutory time limit for deciding whether a merger warrants a Second Request. The rule, if enacted, would institutionalize the gathering of a vast amount of information at the time of HSR filing with little purpose. Nevertheless, the new information would require dozens of new Agency staff just to read the submissions.

4. Mergers and acquisitions lead to the allocation of economic resources to their most efficient use and thus serve as an important engine of economic growth. The FTC's proposal would have the effect of deterring or significantly raising the cost of merger activity. The proposed rule would thus have the ultimate effect of acting as a clog on economic growth without serving any beneficial purpose for merger enforcement.

II. QUALIFICATIONS AS AN EXPERT

5. I specialize in the areas of accounting, economics, and finance as they relate to business analysis, valuation, financial disclosures, and compensation, among other areas. I have senior executive experience in government, academia, and industry, with expertise in strategic and policy issues, securities regulation, auditing, and corporate governance. I have been on the faculty of the Massachusetts Institute of Technology ("MIT") Sloan School of Management since 1999. I currently hold the Gordon Y. Billard Professorship of Accounting and Finance. In addition to my faculty duties, I have also held the positions of Deputy Dean, Faculty Director of the MIT-India Program, and Head of the Department of Economics, Finance, and Accounting at MIT. From 2018 to 2019, while at MIT, I co-chaired the Board of Governors of Asia School of Business, Kuala Lumpur.

6. My most recent experience outside academia was at the U.S. Securities and Exchange Commission as the Chief Economist and Director of the Division of Economic and Risk Analysis. In this role, I led 160 economists and data scientists focused on U.S. securities regulation, domestic and international prudential regulation, and data analytics. During 2008 and 2009, I was the global head of equity research for Barclays Global Investors (acquired by BlackRock) and spearheaded the firm's active equity quant research for a $100 billion portfolio and a team of 50 PhDs globally.

III. THE PROPOSED RULE WILL HAVE THE EFFECT OF EXPANDING AND FRONT-LOADING INFORMATION SOUGHT FROM THE PARTIES WHICH WILL LIKELY DELAY AND/OR DISCOURAGE HSR FILINGS

A. The Motivation for the Proposed Rule Is Not Connected to the Evidence

7. The primary motivation offered by the Agencies for proposing changes to the HSR notification form is that there has been significant growth in sectors of the economy that rely on "technology and digital platforms" to conduct business.[2] The Agencies state that in these sectors, relationships between the merging parties are sometimes neither horizontal, nor vertical, as they operate in adjacent spaces. Such mergers can allegedly lead to a lessening of "potential competition" that could have stemmed from the likelihood that one party could have entered the space of the other in the future but for the merger.

8. To the extent that the Agencies seek to use the additional information to identify problematic mergers that they feel they may have missed, the Agencies do not report having undertaken any kind of retrospective studies that identified how many, or which, mergers slipped through the cracks because of the alleged deficiencies of the HSR form. To our knowledge, in recent years the Agencies have challenged several mergers after they were consummated. However, we are not aware of any additional mergers that would have been blocked by the Agencies before consummation had the Proposed Rule been part of the HSR requirement. Some of the mergers that the Agencies ultimately challenged were non-reportable and the Agencies learned about them only after they were consummated.[3] The extent of information involved in an HSR filing is moot for these mergers. (Filing thresholds are set by Congress each year.)

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[2] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42179.

[3] Examples include FTC and State of Idaho v. St. Luke's Health Sys., Ltd., 1:12-cv-00560-BLW-REB (D. Id. filed March 13, 2013), available at https://www.ftc.gov/opa/2013/03/stluke.shtm; U.S. v. Bazaarvoice, Inc., C13-0133 (N.D. Cal., filed Jan. 10, 2013), available at https://www.justice.gov/atr/public/press_releases/2013/291185.htm; U.S. and State of New York v. Twin America LLC, 12 CV 8989 (S.D.N.Y., filed Dec. 11, 2012), available at https://www.justice.gov/atr/public/press_releases/2012/290136.htm; In the Matter of Polypore International, Inc., Docket No. 9327 (Complaint issued Sept. 10, 2008), available at https://www.ftc.gov/enforcement/cases-andproceedings/cases/2013/12/polypore-international-inc-corporation-matter.

9. The second type of mergers that the Agencies are seeking to unwind after consummation is the group of mergers that were notified to the Agencies, received lengthy investigations including the issuance of Second Requests, and initially unchallenged.[4] For these second category of mergers, the proposed additional information sought at the time of filing was requested and reviewed by the Agencies during the course of the investigations. In other words, the second category of mergers did not fall through the cracks but were identified as raising potential concerns and thoroughly investigated anyway, regardless of what information may not have been available at the time of HSR filing.

10. Similarly, to the extent that the Agencies may believe that certain industries have gotten "over-concentrated" as a result of mergers and acquisition activity, the Agencies have not reported the nature of such industries or explained which mergers have caused them to get over-concentrated. (The merger guidelines identify market concentration using the Herfindahl-Hirschman Index.)

11. While the scope of additional information listed in the NPRM would be burdensome for any merger that triggers an HSR filing, they are particularly burdensome for two types of transactions. The first type are transactions involving private equity or financial firms. The additional information pertinent to such transactions that are proposed to be sought includes information such as limited partnerships, roll-up of prior acquisitions, and identities of members of boards of directors, past and present. The second type are acquisitions involving large technology firms that rely on acquisitions of smaller innovative firms to add features to their product/service offerings to consumers. For such acquisitions, the burden is associated with information that will allow the Agencies to review an acquisition in the broader context of all prior acquisitions made by the buyer.

[4] A prominent example is the acquisition of Instagram by Facebook (Meta), and the acquisition of WhatsApp by Facebook (Meta). See First Amended Complaint for Injunctive and Other Equitable Relief, Federal Trade Commission, plaintiff v. Facebook, Defendant (available at https://www.ftc.gov/system/files/documents/cases/ecf_75-1_ftc_v_facebook_public_redacted_fac.pdf).

B. The Additional Information Requirements

12. The Proposed Rule seeks several additional types of new information beyond the existing HSR filing. These include the following, although there are many other new information requirements.

- Expansion of required regular course of business documents like Strategic and Marketing Plans.[5] Notably, this includes draft versions of these and other transaction-related documents.[6]

- List of minority shareholders of both the buyer and target firms.[7] This includes information on investment funds' limited partners with more than 5% and less than 50% interest in the fund or acquiring entity (whereas previously limited partners were not required to be disclosed).[8]

- All prior acquisitions of both the buyer and target firms for the past 10 years without the prior reporting threshold of $10 million annual net sales or net assets.[9]

- All director, officer, or board observer positions over the two years prior to filing of any individual who is a director, officer or board observer in the acquiring entities, acquired entities, or is expected to be a director, officer or board observer of the post-merger firm.[10]

- The Agencies propose adding a section to the filing requiring a competitive analysis of each party.[11] This would include separate narrative descriptions of any horizontal overlaps, vertical supply relationships, and labor markets

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[5] See e.g., proposed "Periodic Plans and Reports" section. Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42195.

[6] See e.g., proposed "Periodic Plans and Reports" section. Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42194.

[7] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42188.

[8] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42188.

[9] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42203.

[10] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42189-42190.

[11] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42196-198.

information such as skill types of employees, where they live, and other occupational safety information with respect to both current and planned future products of both the buyer and target.

- Notably, each of the additional information requirements would now be imposed on both the buyer and target firms.

C. Costs, Burden, and Uncertainty of Narrative Information

13. The horizontal and vertical narratives requirement frontloads analysis and information that is typically provided by the Parties after the issuance of a Second Request. [12] The practice of requesting the information at a later stage of an investigation reflects decades of practical experience of the Agencies relating to the timeliness and necessity of information, which makes the investigative process efficient for both the Agencies and the Parties. Parties have a great incentive to provide Agencies with sufficient information promptly to avoid regulatory delay and the issuance of a Second Request. The requirement that such information be provided at the time of filing HSR upends a time-tested process.

14. Under the current merger review process, once an HSR is filed, the Agencies take what is referred to as a "quick look" to assess whether the merger warrants the opening of a preliminary investigation. As is described later, over 90 percent of HSR filings do not lead to a preliminary investigation. If an Agency does open a preliminary investigation, it sometimes reaches out to inform the Parties. A few things take place during the initial 30-day waiting period. In some cases, the Agency issues a voluntary access letter ("VAL") which asks for some additional information from the Parties, as for example, most recent strategic plans, list of top 20 customers during the last 3 years, and win/loss data if customers of the Parties make their purchase decisions through competitive bidding. The Parties, at their own discretion, sometimes make a presentation that walks the Agencies through the transaction rationale and broad arguments as to why there is no risk of anti-competitive effects from the proposed transaction. Sometimes, if there is not enough time to undertake

[12] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42196-198.

these activities, the Parties voluntarily "pull and refile" the HSR to give an extra 30 days to the Agency to complete its preliminary investigation.

15. If the Agency, after completing its preliminary investigation, believes that the merger poses competitive risks, then it issues a Second Request. As explained later, this occurs in just 3 percent of HSR filings. The Second Request asks for, among other things, each Party's organization chart, detailed data on sales, costs, and any competitive intelligence maintained by the Parties in the regular course of their business. While the Parties comply with the Second Request, or shortly afterwards, the Parties engage the Agencies with economic analysis as to why any horizontal overlap or vertical relationships should not create anti-competitive effects. This involves rigorous market definition, identification of market participants, and rigorous analysis of why the merger might create efficiencies that are otherwise not attainable.

16. The horizontal overlap and vertical supply relationship narratives that the FTC proposes be filed at the time of the HSR filing would require all of the post Second Request effort to be undertaken at the outset – for all mergers, most of which do not raise concern sufficient during the "quick look" for even a preliminary investigation and, thus, not even lead to the issuance of a Second Request. These analyses involve significant amounts of time of Parties' business executives who provide the necessary information, and that of the Parties' outside counsel and economists. The additional information requirements will not only create costs that are borne by all Parties even in cases where no further investigation would have occurred, but they also delay the filing of HSRs. The additional information requirements also will create the risk that the Parties' narratives – prepared in haste and without knowing where the Agencies will ultimately focus their investigative efforts – might inadvertently provide information that is of no value to the Agencies.

17. Taken together, the proposed changes requiring additional information create costs, burdens, and uncertainty that will likely delay or discourage transactions that would have been made under the current system.

IV. THE VAST MAJORITY OF REPORTED TRANSACTIONS DO NOT RAISE COMPETITIVE CONCERNS THAT WARRANT SUBMISSION OF THE ADDITIONAL INFORMATION

A. Most Transactions Do Not Raise Sufficient Concern to Request Additional Information Beyond the Current HSR Filing

18. Each year, the agencies receive over two thousand HSR filings for reportable transactions, yet only a very small number of these filings raise questions sufficient for the agencies to issue a Second Request for further information. During the 21 years 2001 to 2021, roughly 35,000 transactions were reported averaging roughly 1,700 per year.[13] That number has risen somewhat in more recent years and has averaged over 2,200 during 2017 to 2021.

19. Yet, few of these HSR filings have raised sufficient concern to warrant the issuance of a Second Request. In fact, such concerns are rare. Across the tens of thousands of filings 2001 through 2021, the agencies have issued a Second Request in just over 1,000 transactions, or about 3 percent of the HSR filings; the remainder did not raise any competitive concerns to warrant even a rudimentary scrutiny. As shown in Figure 1, the portion of HSR filings that received a Second Request has varied over time. Yet they show no systemic change from remaining highly infrequent and rare each year. The average for the past decade roughly matches that of the entire two-decade period.

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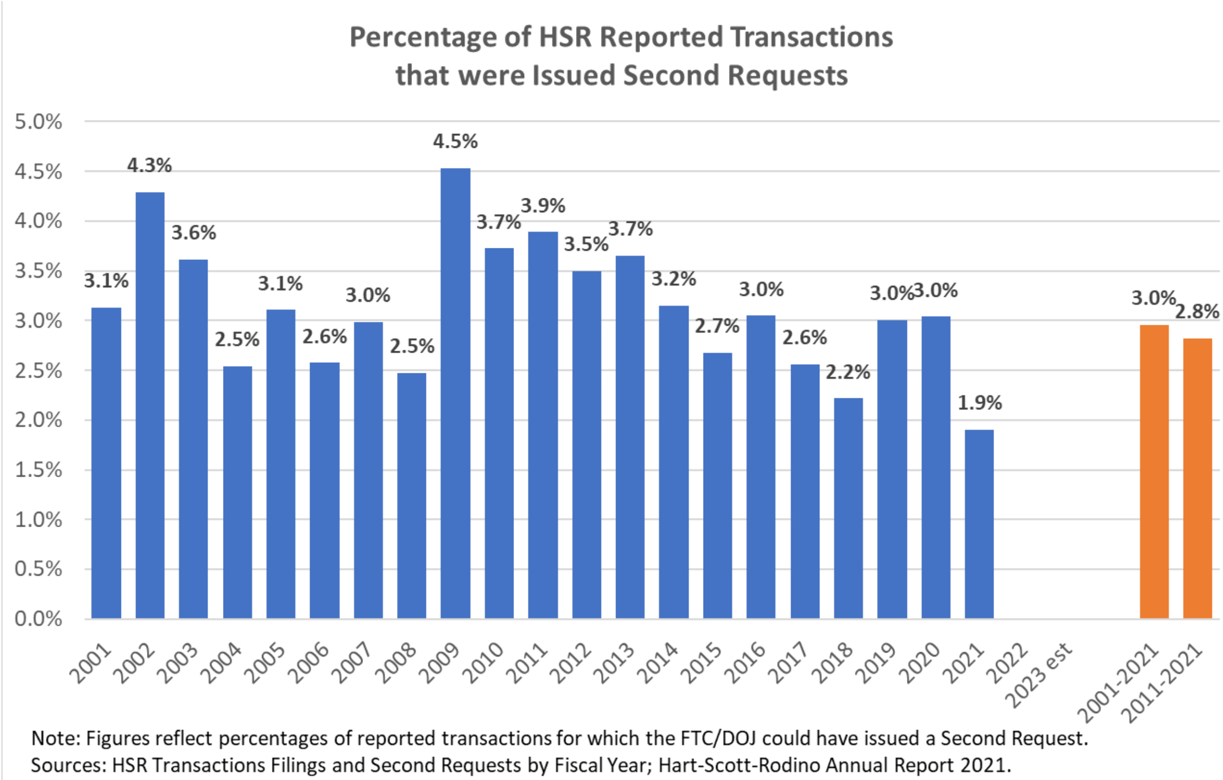
[13] These figures reflect the 97 percent of reported transactions for which the FTC or DOJ could have issued a Second Request. Some other transactions might be incomplete, abandoned, or otherwise not satisfy the criteria where a Second Request could be issued. See HSR Transactions Filings and Second Requests by Fiscal Year, available at https://www.ftc.gov/policy-notices/open-government/data-sets; and Hart-Scott-Rodino Annual Report 2021, https://www.ftc.gov/reports/hart-scott-rodino-annual-report-fiscal-year-2021.

Figure 1



B. Most Transactions Do Not Raise Questions Sufficient For Even A Preliminary Investigation

20. When HSR filings are received, the antitrust agencies use an internal process called "clearance" to determine whether the FTC or DOJ will conduct a preliminary investigation to review the submitted information. An agency seeks clearance for a specific transaction when staff raises questions sufficiently serious to warrant a preliminary investigation.

21. Clearance is rare. During fiscal 2017 to 2021, clearance was sought in less than 8 percent of transactions.[14] In most cases, the agencies showed no interest in even a preliminary investigation of the information submitted in the HSR filing.

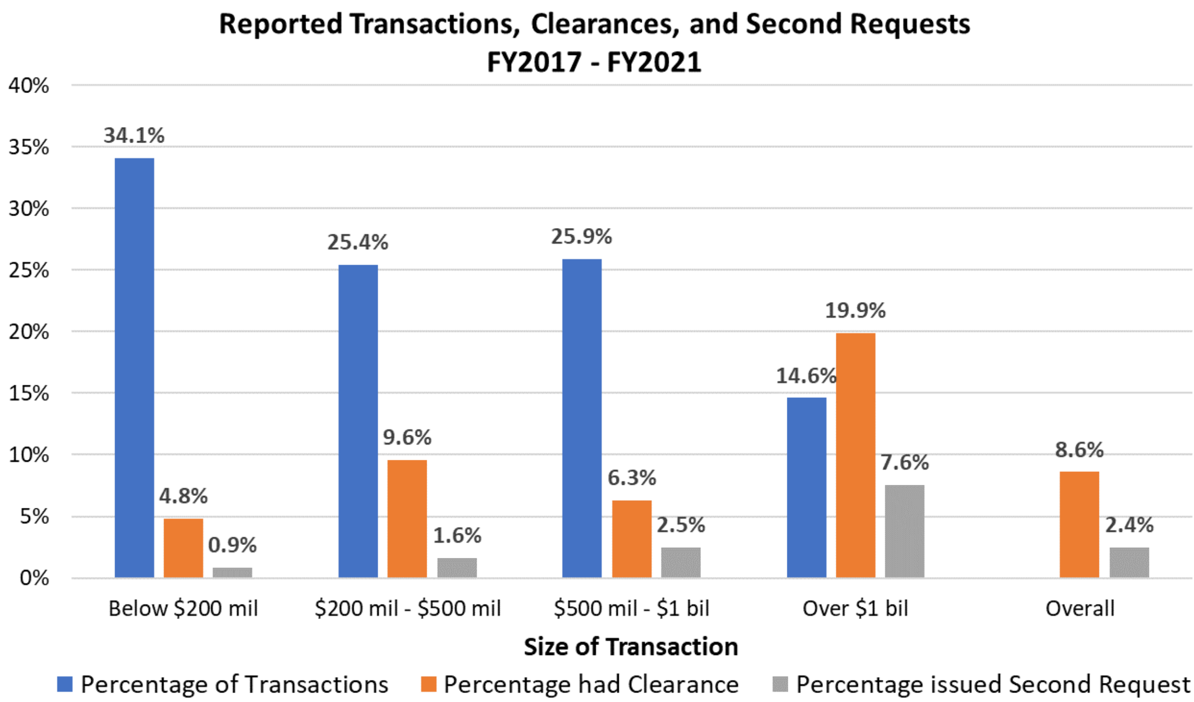
[14] Clearance was sought in 950 of the total 11,043 transactions. Hart-Scott-Rodino Annual Reports Fiscal Years 2017 to 2021.

C. Small Transactions Are Numerous and Have Low Likelihood of Raising Competitive Concerns

22. While knowing that just 3 percent of transactions receive a Second Request already seems low, this figure still overstates the likelihood of competitive concerns for the majority of transactions. This is because the likelihood of a competitive concern is not the same for all transactions. Such concerns are very uncommon for small and even midsize transactions. Yet, these transactions account for the majority of all reported transactions. These transactions are less likely to raise concerns sufficient for clearance for a preliminary investigation and also are less likely to still raise competitive concerns even after such investigation to be issued a Second Request.

23. As shown in Figure 2, between 2017 and 2021, over one third of reported transactions were valued under $200 million. Less than 5 percent of these transactions sparked concerns for clearance and only 0.9 percent of these smaller transactions were issued a Second Request.

Figure 2



Note: Figures are based on the 11,043 transactions reported for FY2017 through FY2021.
Sources: Hart-Scott-Rodino Annual Reports Fiscal Years 2017 to 2021.

24. Even midsized transactions are less likely to raise competitive concerns. Midsize transactions valued between $200 million and $1 billion accounted for over half of all reported transactions yet received clearance for preliminary investigation in less than 8 percent of transactions and were issued a Second Request in less than 2.5 percent of cases. Only large transactions valued above $1 billion were more likely to be issued a Second Request.

25. Given that the vast majority of transactions have not raised concerns to warrant a preliminary investigation let alone a Second Request, most of the costs associated with the additional burdens of the Proposed Rule are potentially little more than wasted activity.

D. Agencies Are Unlikely to Review the Additional Information Under the Proposed Rule

26. Currently, the Agencies do not conduct even a preliminary investigation of the information already provided in the vast majority of HSR filings.[15] They do not have the resources to conduct preliminary investigations on all of the average 2,200 HSR filings received each year 2017 to 2021.[16] This will not improve with additional information.

27. The agencies have limited staff to review transaction filings and many of the staff are already tasked with duties other than review of filings. The FTC has roughly 380 attorneys, economists, and support staff in its Bureau of Competition and Bureau of Economics.[17] The DOJ Antitrust Division has 412 attorneys.[18] Many of these are support staff, research and policy staff, or senior management so that far less than the 792 total staff are available for review of initial filings. If this limited staff is increasingly dedicated to the review of extensive new information, the agencies will necessarily reduce support for investigation of the 3 percent of transactions that warrant such investigation.

28. Instead of leading to further deeper initial review of HSR filings, the additional information burden for all HSR filings will likely end up in the warehousing of information by the Agencies that are neither necessary for the Agencies to undertake their enforcement duty, nor likely to ever be reviewed. Thus, the Agencies' mission of enforcing the competition laws is unlikely to be better served by seeking this additional information at the time of HSR filings.

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[15] As noted before, the agencies sought clearance for a preliminary investigation in less than 8 percent of transactions during fiscal years 2017 to 2021. See Hart-Scott-Rodino Annual Reports Fiscal Years 2017 to 2021.

[16] Fiscal year 2021 had over 3,400 filings. To the extent this may indicate an upward trend in filings, the agencies are not even prepared to review the extensive new information requested by the Proposed Rule imposed on all transactions.

[17] This includes 300 lawyers and support staff at the Bureau of Competition and 80 PhD-holding economists at the Bureau of Economics. See Bureau of Competition, https://www.ftc.gov/about-ftc/bureaus-offices/bureau-competition; and Careers in the FTC Bureau of Economics, https://www.ftc.gov/about-ftc/bureaus-offices/bureau-economics/careers-ftc-bureau-economics.

[18] This reflects the FY2022 budgeted positions. See FY 2023 Budget Summary, https://www.justice.gov/file/1489426/download.

V. THE PROPOSAL TO SEEK ADDITIONAL INFORMATION FROM BOTH PARTIES MAY BE UNDULY BURDENSOME, ESPECIALLY FOR SMALLER TARGET FIRMS

29. Small target firms are likely to be most greatly burdened by the proposed changes. They tend to have fewer available resources to assemble information, yet the same new information requirements as far larger firms. The additional compliance burden could be very significant relative to the value of many small target firms or to investment funds involved in smaller transactions.

30. Being able to sell to a Buyer that can commercialize the product or services developed by a start-up is an important source of entrepreneurial motivation. In fact, selling can move a business toward its long-term goals and allows a smooth transition to a new phase after current ownership leaves, whether this involves re-imagining business direction or leadership or pivoting to meet new challenges. Often entrepreneurs and innovators develop the company itself as the product to be sold and becomes their time and investment exit strategy. It is estimated that as few as one in thirty companies are developed for IPO rather than for acquisition.[19]

31. A significant proportion of HSR filings involve the acquisition of small firms (say, tech start-ups) that lack the resources necessary to comply with the additional information proposed to be sought by the Agencies. As shown in Figure 2, over one third of HSR filings are for transactions valued below $200 million. These firms could be an order of magnitude smaller than the large transactions. Yet, the burden under the Proposed Rule is likely not an order of magnitude smaller.

32. The burdensome proposed filing requirements will ultimately discourage innovative activity that is undertaken by small firms and consequently reduce the pace of economic growth in the United States.

[19] Why Founders Are Afraid to Talk About Exit Strategies, Harvard Business Review, August 18, 2022, https://hbr.org/2022/08/why-founders-are-afraid-to-talk-about-exit-strategies.

VI. THE AGENCIES SIGNIFICANTLY UNDERESTIMATE THE ADDITIONAL MONETARY COST TO PARTIES

33. The NPRM presents the Agencies' estimates for time and cost burdens for filers that are substantially understated and exclude many areas of costs. In particular, the Agencies have only provided incomplete estimates of costs associated with the Parties' information burden. The Agencies have not included estimates of the additional costs to the Agencies in their own work and decision-making from receiving and reviewing information that has a low likelihood of being useful. Moreover, the Agencies have not accounted for the opportunity costs that the extensive review of the additional information would place on the Agencies' potentially justified review of transactions that are more likely to result in a Second Request.

34. The Proposed Rule will likely impose billions of dollars of additional monetary costs each year. In addition to those costs, the Proposed Rule will likely discourage pro-competitive entrepreneurial and innovative activity. These costs are not accounted for in the Agencies' analysis of the Proposed Rule.

A. The Agencies' Estimate

35. The Agencies' estimate of the monetary burden on the Parties is based on a simple calculation of (a) estimated additional hours of preparation time multiplied by (b) the assumed hourly cost of personnel that would undertake the collection and production of such information. Based on canvassing Agency staff that have previously prepared an HSR filing in private practice, the Agencies estimate that the current filings require approximately 37 hours to complete including internal personnel and outside counsel.[20] The Agencies then estimate that the proposed changes to initial filings would increase the requirement by an average 107 hours for a total of 144 hours.

36. The additional hours estimate was prepared by the FTC Premerger Notification Office (PNO).[21] The NPRM describes that the PNO canvassed current Agency staff

[20] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42208.

[21] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42208.

who had previously worked in private practice collecting data for initial filings. The NPRM does not describe a formal survey process or scientific approach for these estimates.[22] There is no assurance that respondent Agency attorneys had prior practice experience that is representative of the entire population of transactions that will be affected by the Proposed Rule. There are many notable deficiencies not limited to the following.[23] There is no assurance of sample size validity and indeed that sample size is not disclosed. There is no assurance that respondents' private practice experience is either recent or relevant. By definition, respondents' prior experience does not include assembly of the new types of information anticipated by the Proposed Rule. Finally, the NPRM does not account for the potential bias of respondents, who now wish to see this information but are not the ones responsible for providing it.

37. The additional hours estimate relies on arbitrary and speculative assumptions that cannot be called "conservative." Notably, as the NPRM describes, HSR filings can range in complexity from relatively simple transactions currently requiring few documents in the filing to more complex ones involving large transactions, many products, or other complex interactions.[24] The NPO canvass found that the Proposed Rule could add between 12 additional hours for so-called simple transactions to 222 hours for a more complex one.[25] Given this wide range, the NPRM uses an

[22] While the current NPRM does not disclose how the PNO arrived at its estimate of the additional hours the Proposed Rule would require, the estimate that current filings require approximately 37 hours to complete is likely from a very limited sample. Twelve years ago in July 2011, the FTC reported in a prior NPRM that the PNO "canvassed eight practitioners from the private bar" to arrive at an estimate of 37 hours to complete an HSR filing. See Premerger Notification; Reporting and Waiting Period Requirements, 76 FR 4 at 42479

[23] The PNO's sampling method is known as "convenience sampling" (i.e., sample selected based on being readily available, rather than being representative of the relevant population). The Reference Manual on Scientific Evidence cautions that "special precautions" are required to reduce the likelihood of bias in convenience samples that quantitative values from such samples should be viewed as "rough indicators" rather than precise quantitative estimates. Shari Seidman Diamon, "Reference Guide on Survey Research," *Reference Manual on Scientific Evidence, Third Edition*, The National Academies Press, 2011, 361-423.

[24] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42207-208.

[25] It is not clear whether the 12 hour and 222 hour estimates are each statistical averages, or represent the range within the responses obtained by the NPO's canvass. For a convenience sample such the NPO's canvass, the Reference Guide on Survey Research cautions that a wide interval in sample data "may be a useful indication of the limited value of the estimate." Shari Seidman Diamon, "Reference Guide on Survey Research," *Reference Manual on Scientific Evidence, Third Edition*, The National Academies Press, 2011, 361-423 at 383.

assumption based on filings made under the current rules. The NPRM assumes that, because 45 percent of current filings have no reported overlaps, 45 percent of filings under the Proposed Rule would have only the lower 12-hour additional hours requirement. The remaining 55 percent or filings are assumed to be more complex and require 222 hours of additional filing burden. Taken together, the range of additional hours and the proportions of transactions assumed simple or more complex result in an average calculated additional number of hours per filing of 107 hours.[26]

38. There is no empirical basis for these assumptions. Among other things, it simply assumes that what makes a transaction "complex" under the current rules would apply to the Proposed Rule. Yet, the proportion of transactions that are moderately to highly complex would likely rise given the many new types of information that must be gathered and analyzed for an initial filing.

39. The Agencies forecast that the 107 hours of additional time burden from the Proposed Rule will result in 759,000 total additional hours devoted by filers in Fiscal 2023, assuming an expected 7,096 relevant filings that year.[27] Since the Proposed Rule has filing requirements from both the acquiring and acquired entities, the expected 7,096 filings is effectively double the expected number of transactions. Notwithstanding the methodological deficiencies that lead to substantial understatement, the Agencies' estimates reflect nearly 1 million more hours of filing burdens each year on U.S. transactions.

40. The Agencies calculate the monetary cost of the additional filing hours by assuming a \$460 hourly cost of attorney time.[28] This figure lacks empirical or other basis and is far from a reasonable estimate of the actual costs that would be incurred. The FTC first assumed the \$460 rate in its 2013 proposed rule, without any supporting research

[26] This reflects the weighted average where 45 percent of transactions are assumed simple and require 12 additional hours, while the remaining 55 percent of transactions are more complex and require 222 hours. Specifically, 107 hours equals 45% times 12 hours plus 55% times 222 hours.

[27] The Agencies expect the Proposed Rule to affect non-index filings, of which they expect 7,096 in Fiscal 2023. Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42208.

[28] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42208.

or data.[29] For the 2023 Proposed Rule, the Agencies did not update the rate for inflation or other factors that would cause the rate to change. Based on adjusting for inflation alone, the $460 per hour cost should be adjusted upward by over 30 percent.[30] There is reason to believe that the relevant legal costs would have grown faster than inflation, so a 30% increase in the hourly rate due to price inflation alone is likely to significantly underestimate the relevant increase in hourly rates.[31] Further, to the extent some of the additional information requires expertise or qualifications not currently needed for existing disclosures (e.g., additional time by senior executives or consultants and economists), there is further reason to assume the relevant rate is significantly higher than $460 per hour.

41. Even assuming the Agencies' hourly rate and additional filing time, the additional hours devoted by filers in Fiscal 2023 would cost them almost $350 million.[32] Even taking the Agencies' figures, the Proposed Rule would result in hundreds of millions of dollars in additional costs. Since these costs are borne across all transactions, they are predominantly borne by the vast majority of transactions that had little to no likelihood of raising competitive concerns and not the few transactions that would subsequently be issued a Second Request even under the current rules.

B. Estimated Additional Monetary Costs

42. The Proposed Rule requires far more information and many new types of information not previously requested from either party. Gathering and providing the additional information is likely to involve several types of professionals including the Parties' attorneys, company executives, and outside vendors such as data search and

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[29] See Premerger Notification; Reporting and Waiting Period Requirements, 78 FR 68705 at 68712.

[30] The Consumer Price Index, a measure of the buying power of past and present dollars, increased 30.6 percent from 2013 to the second quarter of 2023. See Consumer Price Index, 1913-, Minneapolis Federal Reserve, https://www.minneapolisfed.org/about-us/monetary-policy/inflation-calculator/consumer-price-index-1913-.

[31] Survey data from legal recruiting firm Major, Lindsey & Africa estimate average law firm partner compensation grew over 56% from 2013 to 2021 (based on 2014 and 2022 surveys). Additional data on "big law" associated compensation shows first-year associate compensation grew 38% from 2013 to 2022 while eighth-year associate compensation grew 56%. Sources: Major, Lindsey & Africa LLC Partner Compensate Surveys 2014 and 2022; https://www.biglawinvestor.com/biglaw-salary-scale/.

[32] This reflects $460 per hour times 107 additional hours per filing times 7,096 expected relevant filings in Fiscal 2023, which totals $349,265,120.

production companies. The Agencies' estimates do not account for all of these professionals needing to contribute.

43. During mid-August to early-September 2023, the United States Chamber of Commerce conducted a survey to provide a more comprehensive and reliable estimate of the additional costs associated with the Proposed Rule beyond the costs of the current rules.[33] Survey respondents were in-house and external counsel that have typically worked on dozens of proposed transactions over their careers.[34] Over one-third of respondents previously worked at the DOJ or FTC in a capacity involved with merger review. The survey asked respondents the amounts of time and costs to prepare information to submit as part of an initial transaction filing under the current rules and the Proposed Rule.

44. The survey identified several sources of additional costs associated with filings under the proposed Rule. These include outside counsel, internal personnel, and other external costs. The survey additionally asked for estimates of the additional costs borne by the Agencies for the time required to review the newly expanded filings.

1. **Costs of Outside Counsel**

45. The time and cost of outside counsel is included in the Agencies' estimates but are substantially understated. Survey respondents estimate that outside counsel currently spends an average of 54.3 hours per transaction preparing and submitting information for an initial filing. Filing under the Proposed Rule is expected to add 140.3 hours so that the average time to outside counsel time would rise to 194.6 hours. The additional hours are needed not only because the Proposed Rule asks for a greater volume of information, but it also asks for narrative descriptions of the parties and products.

46. Outside counsel can be costly especially as the filings become more expansive. Respondents estimate that the average billable hour for outside counsel is $936 per

[33] See U.S. Chamber HSR/Merger Guides Practitioner Survey, September 19, 2023, available at https://www.uschamber.com/finance/antitrust/antitrust-experts-reject-ftc-doj-changes-to-merger-process.

[34] The average respondent has worked on over 80 proposed transactions during work in one of the agencies or working outside.

hour. This value may be on the low side given the time commitment of some senior counsel on the back-and-forth and drafting of narrative parts of the new information requirements. Moreover, higher hourly rates for outside counsel will be particularly the case for acquirers and targets who lack relevant in-house legal staff. Thus, the survey's results suggest that the additional outside counsel cost of filing under the Proposed Rule would average roughly $131,342. [35]

2. Costs of Internal Personnel

47. Internal personnel are ultimately the source of the information in the filing. Typically, executives, senior managers, in-house counsel, and sometimes company founders take on the roles of point persons for assembling the required information. They must take time from the duties that operate the business to attend interviews with counsel, assemble information, and iterate on how the filing is prepared. Survey respondents estimate that the current filing requires 30.4 hours of time from internal personnel. This would rise by 101.6 hours to 131.9 hours under the proposed Rule.

48. It is difficult to estimate the value of the lost time used in assembling the transaction filing. The cost of lost time is not simply a wage rate. The company incurs an opportunity cost of lost time from its executives as business decisions are not being made. To be conservative, if internal personnel time were valued at the same rate as outside counsel, the additional cost of filing under the Proposed Rule would average approximately $95,055.[36]

3. Other External Costs

49. Transaction filings often incur other external costs beyond the outside counsel. In addition to outside counsel, these external costs could include economic consultants, investment bankers, and data vendors.[37] The additional information under the proposed Rule includes many areas requiring specialized consultants and executives. These contributors to the company's filing may cost much more than those assisting

[35] This reflects the 140.3 additional hours times $936 per hour.

[36] This reflects the 101.6 additional hours times $936 per hour.

[37] Survey respondents were not asked to break out the additional external costs by category.

in the current, simpler filing requirements. Survey respondents estimate that these external costs currently can total $79,569 on an average filing. After excluding the outside counsel costs that are separately provided by the survey, the other external costs for a filing average $28,744.[38]

50. Respondents estimate that the additional information requirements of the Proposed Rule would raise external costs by $234,259 to $313,828 on an average filing. Excluding outside counsel costs, the additional external costs per filing would be $102,917.[39]

4. Total Additional Monetary Costs and Filing Burden

51. Each of the types of filing costs identified in the survey exceed the estimates provided the Agencies and the total is over six times their estimates. Figure 3 summarizes the estimates of filing costs provided by the Agencies and the survey respondents. The Agencies estimate the total filing cost under the Proposed Rule would be $66,240. The survey results show that the actual average cost would be $437,314, nearly seven times the Agencies' estimate. Both the Agencies and the survey find that the proposed Rule will increase filing costs to roughly four times their current levels.

[38] Based on the survey results, the average current outside counsel cost per filing is $50,825 reflecting 54.3 hours at an average $936 per hour. The other external costs of $28,744 are the $79,569 total external cost minus the $50,825 for outside counsel.

[39] The calculation is similar to that for the current external cost of filing. Based on the survey results, the average additional outside counsel cost per filing is $132,292 reflecting 141.3 additional hours at an average $936 per hour. The additional other external costs of $88,843 are the $221,136 additional total external costs minus the $132,292 additional cost for outside counsel.

Figure 3

Monetary Costs Associated with Transaction Filing

		Current Filings [a]	Additional Burden [b]	Proposed Rule [c]=[a]+[b]	Ratio Proposed to Current [d]=[c]/[a]
Agencies' Estimate per Filing	[1]	$ 17,020	$ 49,220	$ 66,240	3.9
Survey Results per Filing					
Internal Personnel	[2]	$ 28,431	$ 95,055	$ 123,486	4.3
External Outside Counsel	[3]	$ 50,825	$ 131,342	$ 182,167	3.6
External Other Costs (e.g., consultants)	[4]	$ 28,744	$ 102,917	$ 131,661	4.6
Total Costs per Filing	[5]=[2]+[3]+[4]	$ 108,001	$ 329,314	$ 437,314	4.0
Ratio of Survey Results to Agencies' Est.	[6]=[5]/[1]	6.3	6.7	6.6	
Expected Total Relevant Filings in FY2023	[7]	7,096			
Estimated Total Costs for FY2023					
Agencies' Estimate	[8]=[1]x[7]	$ 121 million	$ 349 million	$ 470 million	
Conservative Estimate	[9]=[5]x[7]/2	$ 383 million	$ 1,168 million	$ 1,552 million	
Primary Estimate	[10]=[5]x[7]	$ 766 million	$ 2,337 million	$ 3,103 million	

Sources: NPRM, 88 FR 42178 at 42208; and U.S. Chamber of Commerce survey.

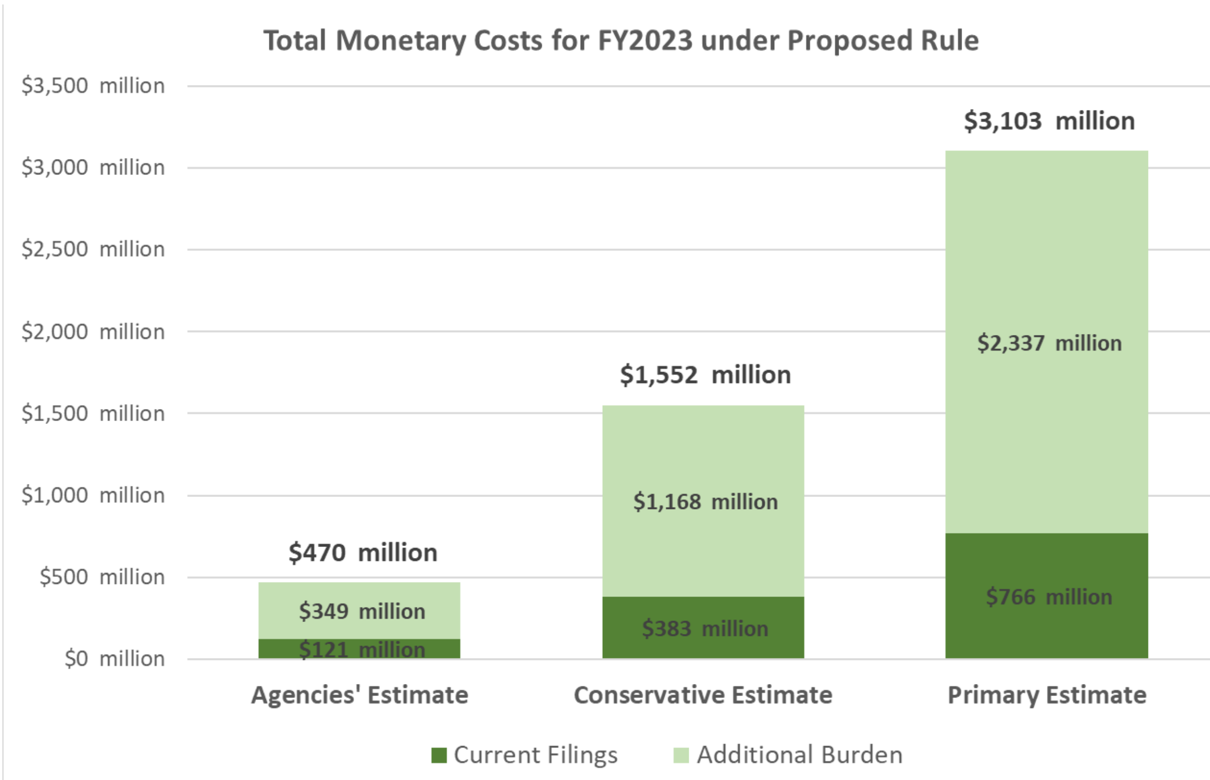
52. These estimates reflect the costs to just one average filing. As noted earlier, the Agencies estimate there will be 7,096 relevant filings in Fiscal 2023. This includes filings from each of the acquirer and acquired entity so that the 7,096 filings reflect 3,548 transactions. If each of the 7,096 filings results in the estimated $329,314 additional cost, the additional costs associated with the Proposed Rule would cost filers in Fiscal 2023 over $2.3 billion.[40] It is possible the acquired entity filing may entail a lower average cost, for example, due to sharing of information gathering between the acquiring and acquired entities. Even if one were to be overly conservative by ignoring the burden on the acquired entity, the 3,548 transactions would still result in the Proposed Rule costing filers in Fiscal 2023 almost $1.2 billion.[41] Figure 4 provides a comparison of the Agencies' estimates of the total monetary costs and the estimates based on the U.S. Chamber's survey of practitioners. The total monetary costs expected by practitioners far exceed the

[40] This reflects $329,314 in additional cost per filing, as shown in Figure 3, times 7,096 expected relevant filings in Fiscal 2023, which totals $2,336,810,381.

[41] This reflects $329,314 in additional cost per filing, as shown in Figure 3, times 3,548 expected transactions in Fiscal 2023, which totals $1,168,405,190.

additional costs estimated by the Agencies, even before consideration of indirect costs discussed elsewhere in this report. The vast majority of these costs would be borne by transactions that had little to no likelihood of raising competitive concerns and not the few transactions that would subsequently be issued a Second Request even under the current rules.

Figure 4



5. **Agency Costs to Review Additional Information**

53. The Proposed Rule does not include estimates of the costs to the Agencies themselves to review the new information. Survey respondents with prior Agency experience involved with merger review estimate that the Proposed Rule would result in an additional 24.9 hours of Agency staff effort to review each filing. Given the expected 7,096 relevant filings in Fiscal 2023, the additional time would result in over 177,000

additional hours for staff. This is equivalent to roughly 100 full-time attorneys working on nothing but initial filings.[42]

54. The Agencies are unlikely to have 100 idle attorneys available for dedicated review of initial filings. The DOJ and FTC have fewer than 800 attorney and economist staff in total. The Proposed Rule would require one-eighth of all activity at the Agencies to be devoted to reviewing the additional information. Of course, as noted earlier, the Agencies may be unlikely to actually review the additional information in the majority of cases. If so, the costs on the Agencies would be lower but the cost burden on filing parties would be no lower for preparation of expanded filings the Agencies may not intend to review (i.e., there would be substantial cost to filers with no possible benefit to the Agencies if they do not even review the information).

C. Costs to the Economy Exceed the Direct Monetary Costs

55. The analysis of the costs and benefits of the Proposed Rule has focused on the monetary burden. However, one must also consider the broader costs on innovation and entrepreneurial activity. The expectation that any transaction would entail greater cost and uncertainty can lead business to rethink potential transactions. When once a transaction would have been beneficial, it would now be fraught with risk and costs. The filing cost analysis ignores that the Proposed Rule's additional burdens to the economy dissuade potential transactions from occurring.

56. The additional information burden of the Proposed Rule will result in longer times preparing the more complex filings ultimately delaying transactions. The monetary cost analysis ignores the cost of added regulatory approval delay to the firms in the transaction. For example, the parties must delay the realization of business synergies and improvements. The delay is not only from added filing preparation but also the evaluation period after filing. Currently, there is a 30-day statutory requirement, but the additional data burdens may lead to more extensions beyond the initial 30 days

[42] This assumes the average Agency attorney reviews filings for roughly 1,800 hours per year.

than there otherwise would be. Delays could kill deals and lead parties to abandon transactions.[43]

57. The Proposed Rule will result in increased uncertainty in several ways. An advisory committee to the DOJ has identified that regulatory delays such as additional filing requirements create uncertainty because "delay breeds uncertainty in product, labor, and capital markets, enabling competitors to raid customers and staff."[44] Moreover, there is additional uncertainty for potential filers arising from the Agencies turning away from the decades of practice under the current rules. Regulatory uncertainty arising from new burdens imposed by the Proposed Rule can have substantial impact on the level of merger and acquisitions activity. For example, a 2018 paper published in the Journal of Financial Economics found that a one standard deviation increase in regulatory policy uncertainty is associated with a 6.6 percent decrease in aggregate M&A deal value and a 3.9 percent decrease in the number of transactions during the next 12 months.[45] Other academic papers have found similar results.[46] Of course, the effects may be greater if they are longer lasting. The Proposed Rule is not just a temporary increase in uncertainty.

58. Mergers and acquisitions have been shown to improve efficiency and contribute significantly to economic output. For example, using the plant-level data, a 2013 paper published in the Journal of Finance shows that acquired plants gain in productivity more than the non-acquired plants.[47] The gain in productivity is higher

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[43] For example, a DOJ-created advisory committee reported that "Mergers are almost always time sensitive; delays may prove fatal to a transaction…" See International Competition Policy Advisory Committee, Final Report to the Attorney General and Assistant Attorney General for Antitrust, 2000, Chapter 3, https://www.justice.gov/atr/final-report

[44] See International Competition Policy Advisory Committee, Final Report to the Attorney General and Assistant Attorney General for Antitrust, 2000, Chapter 3, https://www.justice.gov/atr/final-report

[45] Bonaime, A, Gulen, H., and Ion, M. Does policy uncertainty affect mergers and acquisitions? Journal of Financial Economics, 129(3), September 2018, 531-558.

[46] For example, a 2016 paper in the Review of Financial Studies found a strong negative link between various measures of uncertainty and M&A deal activity. One key source of this uncertainty arises from market changes occurring during delays in consummating the transaction. V. Bhagwat et al., The real effects of uncertainty on merger activity, Review of Financial Studies, 29(11), 2016, 3000-3034.

[47] Maksimovic, V., Phillips, G., & Yang, L. "Private and public merger waves." *The Journal of Finance*, 2013, 68(5), 2177-2217.

when there are more frequent M&A transactions and when the buyer's valuation is high. Similarly, research analyzing changing ownership of U.S. power plants found that acquisitions reallocate assets to more productive uses (high productivity firms buy under-performing assets from low productivity firms and then make the acquired assets more productive after acquisition).[48] Another 2021 paper published in the Review of Economic Studies shows that the mergers and acquisitions contribute 14% to the overall output of the economy and 4% to the overall consumption in the economy.[49] This contribution is driven by the reallocation of resources and new entrepreneurship.

59. Mergers and acquisitions have also been shown to incentivize R&D spending and innovation. For example, a 2013 paper published in the Review of Financial Studies shows that successful innovation makes smaller firms attractive acquisition targets.[50] Thus, potential M&A activity provides incentives to small firms to invest in R&D and innovate more when they know they can later sell out to larger firms.

60. Another study published by the US Chamber of Commerce and NERA Economic Consulting found no evidence that merger activity leads to reduced innovative activity.[51] In fact, the study found a strong positive and statistically significant relationship where mergers cause, to a great extent, subsequent increased R&D expenditure and patent applications.

61. Given the importance of M&A activity to the economy, potential delay and discouragement of acquisition transactions caused by the combination of increased monetary compliance as well as indirect costs (such as opportunity costs of

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[48] Mert Demirer, Ömer Karaduman, "Do Mergers and Acquisitions Improve Efficiency: Evidence from Power Plants." Working paper, January 13, 2022.

[49] David, J. M. "The aggregate implications of mergers and acquisitions." *The Review of Economic Studies*, 2021, 88(4), 1796-1830.

[50] Phillips, G. M., & Zhdanov, A. "R&D and the incentives from merger and acquisition activity." *The Review of Financial Studies*, 2013, 26(1), 34-78.

[51] Kulick, R, & Card, A. "Mergers, Industries, and Innovation: Evidence from R&D Expenditure and Patent Applications." NERA Economic Consulting and U.S. Chamber of Commerce, February 2023, available at https://www.uschamber.com/finance/antitrust/mergers-industries-and-innovation-evidence-from-r-d-expenditure-and-patent-applications.

executives' time, potential transaction delay, and additional regulatory uncertainty) would, to the extent M&A activity were curtailed at the margin, adversely affect innovation, entrepreneurship, and the economy in general. Event studies of acquisition announcements have shown that acquisitions result in statistically significant increases in the combined market value of the acquirer and target.[52] Thus, any discouragement of transactions would lead to a significant loss of value creation.

62. Even where M&A activity is not curtailed, potential delay may have significant economic costs. Given that many transactions are pursued for the purposes of realization of efficiencies or productivity improvements, any delays would lead to some lost post-acquisition gains. For example, if an acquisition were expected to result in post-closing cost savings of $12 million per year, a one-month delay would result in the loss of $1 million in costs that could have been avoided.

63. The Agencies do not present any analysis of countervailing benefits to competition from the Proposed Rule. As noted elsewhere, the Agencies do not present evidence that the current filing fails to screen transactions where competitive concerns should be raised. They thus have not provided a systematic rationale for why a more extensive information burden should be imposed on all transactions to result in the same challenges otherwise captured by the current VAL and Second Request procedure.

1. Burdens to Private Equity Investors

64. The US private equity (PE) sector provides economic benefits to the US economy both directly and indirectly through backing businesses. In 2022, the US PE sector directly generated $1.7 trillion of US Gross Domestic Product ("GDP"), which is about 6.5% of the total US GDP. The sector directly employed 12 million workers paying them $1 trillion in wages and benefits in 2022. The US PE sector provided indirect benefits to the US economy through backing mostly small businesses. In 2022, PE-backed small businesses employed a total of 1.4 million workers in the US.

[52] Robert F. Bruner, "Does M&A Pay? A Survey of Evidence for the Decision-Maker," *Journal of Applied Finance*, Spring/Summer 2002, 48-68.

These workers earned $135 billion in wages and benefits and generated a total of $240 billion of US GDP in 2022.[53]

65. PE investment helps in increasing competition in the marketplace and improve consumer welfare. For example, PE often acquires carveouts (non-core assets of a company which are under-utilized and/or must be divested in order for other mergers to be approved by regulators). PE investments in carveouts often create independent companies with new management, and steps in to fund and grow the carveout.[54] Funding the carveouts has resulted in creation of over 4,000 new companies and an investment of over $700 billion over the past decade.[55]

66. Another example is add-on acquisitions which can create significant efficiency gains in cost-intensive industries. Evidence suggests that such strategies are concentrated in more fragmented and competitive industries such as insurance where more than 400,000 brokers and agencies compete. As a result, it is less likely that consolidation will lead to anticompetitive effects in those industries. In those add-on acquisitions, the PE investments lower costs and improves the operations of the portfolio company which benefits all stakeholders including consumers.[56] The add-on acquisitions, in particular, will suffer from informational burden under the additional information requests given the volume of smaller transactions (many of which might previously have been below the reporting threshold) for such a PE strategy.

67. The reallocation of resources via PE investment has been shown to improve innovation. One study using the data for 19 industries in 52 countries shows that PE investment improves productivity, employment, and capital expenditures of competing public firms in the same industry.[57] Another study using data from PE

[53] Ernst and Young, Economic contribution of the US private equity sector in 2022, Prepared for the American Investment Council, April 2023.

[54] AIC and Pitchbook "Diamonds in the Rough. How PE breathes new life into unloved businesses," September 2022.

[55] AIC and Pitchbook "Diamonds in the Rough. How PE breathes new life into unloved businesses," September 2022.

[56] AIC and Pitchbook, "Building Competition. How buy-and-build helps the American economy," February 2023.

[57] Aldatmaz, S., & Brown, G. W. (2020). Private equity in the global economy: Evidence on industry spillovers. Journal of Corporate Finance, 60, 101524.

backed leveraged buyouts in UK shows that PE investment increases innovative output (measured by patents) after the deal.[58]

68. Venture capital (VC) is a form of PE. Unlike PE in general, VC tends to invest in smaller companies and entrepreneurs at an earlier stage in development. Such companies are often not yet profitable or have established sales. VC investors can help small companies minimize risk and avoid the mistakes of many startups. The VC investors often actively lend experience and help these small companies find opportunities.

69. Certain of the additional information requirements in the proposed rulemaking will be particularly burdensome to PE and VC firms, as described below. The additional burdens may discourage some PE and VC activity. This may be particularly the case for funds with larger portfolios of smaller targets, general partners managing multiple investment funds, or newly emerging funds with limited back-office infrastructure to track and manage the additional information disclosure requirements. To the extent that smaller, emerging investment managers are disproportionately burdened by the expanded disclosure requirements, competition within the fund management industry may be negatively impacted. If smaller fund managers are more likely to forego transactions, larger fund managers will have less competition for its investment choices and would be able to attract capital from investors more easily with any diminished capability for smaller or emerging managers to compete.

2. Officers, Directors, and Board Observers

70. The NPRM proposes that all proposed officers, directors, and board observers would be required to disclose all other entities for which each individual had served as an officer, director, or board observer within two years of filing.[59] The Agencies justify this request for purposes of knowing existing, prior, or potential interlocking directorates. Such information is likely to be a significant burden to PE and VC

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[58] Amess, K., Stiebale, J., & Wright, M. (2016). The impact of private equity on firms′ patenting activity. European Economic Review, 86, 147-160.

[59] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42189.

managers, whose members often serve as directors on many firms within the investment firm's portfolio, and whose board memberships may change rapidly with changes in a fund's portfolio holdings.

3. **Disclosure of Limited Partners**

71. The NPRM proposes to require disclosure of limited partners with between 5% and 50% interest in funds making reportable acquisitions.[60] The Agencies' justification for this requirement is that "after more than a decade, the Commission now believes it is inappropriate to make generalizations regarding the role of investors in limited partnerships structures" (where, previously, it was understood that limited partners had no control over operations of a fund or its portfolio companies). Further, the Agencies argue limited partner information "can provide valuable information about co-investors and lead to identification of potentially problematic overlapping investments resulting from the transaction that could violate Section 7."

72. Notably, the Agencies do not argue that limited partners exercise any control over operations of the fund or its portfolio companies, and arguably by definition, limited partners are precluded from exercising any operational control. This information requirement will be burdensome to funds which may have significant confidentiality agreements in place with investors, and where the potential of such disclosures may discourage certain investors from making investments that would lead to exceeding the 5% reporting threshold, thus making fundraising more difficult. Furthermore, the Agencies have not demonstrated that there has been any failure to identify "potentially problematic overlapping investments" through Second Requests or other means of obtaining information after an initial filing.

VII. CONCLUSION

73. The Proposed Rule will lead to substantial additional direct monetary costs for HSR filers that could total over $2 billion. These additional costs will be borne by all filers, not just the very small fraction the Agencies identify each year for further investigation. The Proposed Rule will also lead to further costs to the economy

[60] Premerger Notification; Reporting and Waiting Period Requirements, 88 FR 42178 at 42188.

beyond the direct monetary costs, including burdens on innovation and entrepreneurial activity as well as additional costs on the Agencies themselves to review the new information. The benefits to consumers are likely to be limited due to the small percentage of filings that progress to a preliminary investigation, let alone those that ultimately result in an enforcement action. The Agencies have not demonstrated there will be benefits to consumers in excess of the additional direct monetary and other economic burdens imposed.

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S.P. Kothari

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Private Equity and Financial Fragility during the Crisis
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ABSTRACT

Do private equity firms contribute to financial fragility during economic crises? We find that during the 2008 financial crisis, PE-backed companies increased investments relative to their peers, while also experiencing greater equity and debt inflows. The effects are stronger among financially constrained companies and those whose private equity investors had more resources at the onset of the crisis. PE-backed companies consequentially experienced higher asset growth and increased market share during the crisis.

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1 Introduction

The recent global financial crisis increased the attention paid by policy makers, regulators, and academics to financial stability. While much attention has been devoted to deficiencies in the banking system in this and earlier crises (Bernanke and Gertler, 1990; Reinhart and Rogoff, 2009; and Fahlenbrach, Prilmeier, and Stulz, 2012), high levels of corporate debt have also triggered concerns. Highly leveraged firms may enter financial distress during a crisis, exacerbating cutbacks in investment and employment and contributing to the persistence of the downturn (Bernanke, et al., 1988; and Bernanke, 1983).

The practices of the private equity (PE) industry in particular have raised concerns. In the three years leading up to the crisis (between 2006 and 2008), global PE groups raised almost $2 trillion in equity[1], with each dollar typically leveraged with more than two dollars of debt (Kaplan and Stromberg, 2009). This phenomenon was not confined to the recent crisis. Private equity markets are prone to distortions introduced by credit cycles. As documented by Axelson, et al. (2013), periods characterized by booming financial markets also experienced greater private equity fundraising, higher transaction valuations, and critically more leverage.

The impact of PE investment patterns on the economy during periods of financial turmoil, however, remains poorly understood. On the one hand, the cyclicality of private equity activity, combined with the leveraging of their portfolio companies, may exacerbate the negative effects of shocks to the financial sector, aggravating the boom and bust dynamic of the economy. In line with this idea, the Bank of England suggests that buyouts should be monitored for macro-prudential reasons, because "the increased indebtedness of such companies poses risk to the

[1] http://www.preqin.com

stability of the financial system."[2] Moreover, the pressure to complete deals during boom times may lead to the financing of lower-quality firms (Kaplan and Stein, 1993), leaving PE-backed companies more exposed to changes in underlying economic conditions. Finally, the increased fundraising and investment during boom periods may reduce the ability of private equity groups to effectively monitor and fund their portfolio companies once economic conditions deteriorate. These concerns have led to efforts to cap the amount of leverage used in PE transactions by the U.S. Federal Reserve Bank and the European Central Bank.

Alternatively, PE-backed companies may be resilient to downturns, and therefore play a stabilizing role during bad times. In particular, these companies may be better positioned to obtain external funding when financial markets are dysfunctional. First, PE groups have strong ties with the banking industry (Ivashina and Kovner, 2011) and may be able to use these relationships to access credit for their firms during periods of crisis. Second, because PE groups raise funds that are drawn down and invested over multiple years—commitments that are very rarely abrogated—they may have "deep pockets" during downturns. These capital commitments may allow them to make equity investments in their firms when accessing other sources of equity is challenging.

Motivated by these alternative hypotheses, this paper seeks to understand whether private equity contributed to the fragility of the economy in the United Kingdom (UK) during the recent financial crisis. The UK is a perfect environment to study this question. First, the UK had the largest private equity market as a share of GDP before the crisis (Blundell-Wignall, 2007)— with PE assets at about 11% of GDP[3] — and one of the largest in absolute value. In line with these numbers, the Bank of England estimated that before the crisis, PE-backed companies had issued

[2] *Bank of England Quarterly Bulletin*, 2013Q1

[3] This number is obtained by dividing the total fundraising between 2004 and 2008, as estimated by the European Venture Capital Association and PEREP Analytics, by GDP in 2008 (as reported by the World Bank).

more than 10% of all non-financial corporate debt in the UK.[4] Second, the UK provides detailed income statement and balance sheet information for almost every active company, whether public or private (Brav, 2009; and Michaely and Roberts, 2012). Similar financial data are not available in the United States. Finally, the UK experienced a severe credit market freeze during the 2008 crisis, with a dramatic decline in private sector investment and lending (Riley, Rosazza, Bondibene, and Young, 2014). As illustrated in Figure 1, aggregate investment declined by more than 20% during 2008 in the UK, which simultaneously experienced a sharp credit contraction (Figure 2).

To address the above questions, we study the relative evolution of PE-backed and non-PE companies in the wake of the financial crisis. We focus on the financial decisions and performance of these firms during this period, in an attempt to understand whether private equity exacerbates or dampens the sensitivity of the economy to credit cycles.

Our main analysis focuses on a final sample of almost five hundred companies that were backed by PE prior to the financial crisis. Using a difference-in-difference approach, we study how the financial decisions and performance of the PE-backed companies were affected by the onset of the crisis relative to a control group. The control group employs companies that were operating in the same industry as the PE-backed companies and had similar size, leverage, and profitability in 2007, following the methodology of Boucly, Sraer, and Thesmar (2011).[5] The matching firms had similar trends in the years leading to the crisis along dimensions such as investment, revenue, returns on assets, equity contributions, and debt issuances. Therefore, this approach allows us to

[4] Bank of England Quarterly Bulletin, 2013Q1

[5] As we discuss in the paper, the main results are confirmed also when using a similar matching procedure but excluding leverage as a matching variable.

explore differences that stem from organization structure, rather than their balance sheet or investment characteristics.

We start by comparing the PE-backed companies and their peers' behavior during the financial crisis. We find that PE-backed companies decreased investments less than non-PE-backed companies did during the financial crisis, with between five and six percent greater spending, an effect that is strongly statistically significant. Looking at the timing of the effects, the two groups did not differ significantly in the pre-crisis period, but the investment rate of the PE group substantially diverged from the control group beginning in 2008. In fact, the divergence of the PE group occurs exactly when aggregate investments and credit growth in the UK started to decline sharply.

We then show that the higher investments by PE-backed companies reflect the fact that these firms appear to have been less bound by financial constraints. We find that debt issuance over assets was four percentage points higher for PE-backed companies during the crisis, and similarly, equity issuances over assets increased by two percentage points relative to their peers. At the same time, the cost of debt, measured by interest expense over total debt, was relatively lower for PE-backed companies during the crisis. As before, these effects appeared first in 2008 and continued through the remainder of the period (with varying levels of statistical significance).

The idea that private equity firms can help relax the financial constraints of portfolio companies is also consistent with two additional findings. First, the positive effect on investment is particularly large among companies that were ex-ante more likely to be financially constrained during the crisis. We find this result using various proxies of financial constraints, such as size, industry dependence on external finance (Rajan and Zingales, 1998), and pre-crisis leverage.

Second, the increase in investment is larger when the private equity sponsor had more resources available at the onset of the crisis to help the portfolio company. To explore this dimension, we exploit the heterogeneity across private equity firms. First, we focus on the amount of dry powder—capital raised but not yet invested – that is available to investors at the time of the financial crisis. Second, we look at to whether their most recent fund was at an earlier stage at the time of the financial crisis. These tests are based on the underlying assumption that PE firms that are at the early years of their fund and with more dry powder have more resources—both financial and operational—to invest in their portfolio companies during the crisis. Our results confirm this hypothesis.

Taken together, these results illustrate that PE-backed companies do *not* appear to be more sensitive to the onset of the financial crisis. Rather, during a period in which capital formation dropped dramatically, PE-backed companies invested more aggressively than peer companies did. This ability to maintain a high level of investment appears related to the superior access of PE-backed companies to financing, in terms of both equity and debt issuances, and the lower cost of debt.

The results are robust to a battery of checks. Throughout the analysis, we control for firm fixed effects, and thus remove time-invariant characteristics of the control and treatment firms. We also show that the results are not driven by non-parallel trends in the pre-crisis period and they are not affected by the addition of company controls. Second, our main results generally do not change when we exclude companies whose private equity deals were management buyouts (MBOs), a class of transactions in which the engagement of private equity firms is traditionally lower. Third, it does not appear that the results simply reflect differences in attrition between PE and non-PE companies. Fourth, the results remain unchanged if we control for time-varying industry shocks around the crisis. Lastly, we also confirm that the results are robust to alternative matching estimators. In particular, we find that removing leverage from the variables used to match companies does not affect our results.

In the final part of the paper, we examine the performance of PE-backed companies during the financial crisis. We find that PE-backed companies experienced a greater growth in their stock of assets in the years after the crisis, consistent with the greater investment seen above. Similarly, we find that PE-backed companies increased their market share in the industry during the crisis. At the same time, PE-backed companies did not underperform their peers: that is, they did not become relatively less profitable, whether measured by the ratios of EBITDA to revenue or net income to assets. These findings are contrary to what would be expected if companies were pursuing value-destroying investments during this period.

We also examine the exit patterns of PE-backed companies relative to the control group during the crisis period. We find that PE-backed companies were not more likely to go bankrupt, but they were more likely to be sold through non-distressed M&A transactions. Overall, these results regarding exits—while by their nature limited—again seem to be inconsistent with the hypothesis that PE financing increases the financial fragility of the portfolio firms.

This paper relates to an extensive body of work examining the behavior of financial institutions during the financial crisis and their consequences for operating firms (e.g., Ivashina and Scharfstein, 2010; and Ben-David and Franzoni, 2012). The conclusions, though, are more benign here than many of the studies examining other financial institutions, including banks, rating agencies, and hedge funds. The role of private equity groups seems more akin to those of the Japanese banks during the 1980s documented by Hoshi, Kashyap, and Scharfstein (1990, 1991), where better information and aligned incentives allowed bank-affiliated firms to overcome the capital constraints that limited investments of their peers without such relationships. During the financial crisis, the greater alignment and relatively longer time horizons of private equity investors may have allowed firms to more successfully respond to the economic dislocations.[6] The paper is also related to an extensive literature that explores the effects of private equity ownership on firm outcomes (e.g., Bernstein, et al., 2016; Bernstein and Sheen, 2016; Boucly, Sraer, and Thesmar, 2011; Cohn and Towery, 2013; Davis, et al., 2013; John, Lang, and Netter, 1992; Kaplan, 1989; and Lichtenberg and Siegel, 1990).

The paper is organized as follows. In Section 2, we present the data used in this study. Section 3 then describes the empirical approach employed in the paper. Sections 4 and 5 present the main results on investment and performance, discussing the possible mechanisms behind our results and presenting a battery of robustness tests. Finally, Section 6 summarizes our results and conclusions.

2 Data

2.1 Sample Construction

We start our data construction by extracting from Capital IQ all UK companies backed by private equity before the financial crisis. We identified private equity deals in Capital IQ by searching for events such as "going private," "leveraged buyout," "management buyout," and "platform." In so doing, we excluded "growth buyouts," "venture capital" and "expansion capital" investments, where investors generally buy a stake in the company using little or no leverage. Since we are interested in studying the behavior of UK PE-backed companies around

[6] The high-degree of diversification among different types of limited partners (LP) of buyout funds may explain why they were able to support their portfolio companies during the crisis. In contrast, Illig (2012) argues that that venture capital funds had difficulties in raising capital and had to defer capital calls during the crisis because their LPs base was less diversified and more concentrated among endowments.

the financial crisis, we selected only firms that (i) were headquartered in United Kingdom at the time of the deal; (ii) had received a PE investment by the end of 2007 and (iii) did not experience an exit by the PE group by the end of 2008.[7]

We then further filter our data, keeping only those firms that had balance sheet and income statement information in Amadeus, a Bureau Van Dijk (BvD) data set of European companies. Amadeus collects data from the "Companies House," the United Kingdom official national registrar office. As already pointed out by other authors (Brav, 2009; Michaely and Roberts, 2012), the United Kingdom is a perfect setting for studies of private companies. According to current regulations, every registered limited company is required to provide financial and income information annually to the public register.

The extent of the requirement to disclose financial information in the UK, however, varies with the size of the company. Small (and some medium-sized) companies are allowed to file abbreviated accounts.[8] Since the amount of information small firms disclose to Companies House (and hence in the Amadeus dataset) is quite limited, we excluded this group from our analysis. The reliability of the source and its coverage of the remaining private firms is a key strength of our study. Most of the companies in our sample consist of middle-sized private enterprises, for which similar financial data are not available in the United States.[9]

We supplement Amadeus data with Orbis, another data product from BvD. While both Amadeus and Orbis collect information from the Companies House, Amadeus generally removes firms from the sample after a few years of inactivity. This is not the case for Orbis. Since the post-

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[7] During 2008, there were 28 exits of PE firms. The results remain unchanged if we include them in the sample.
[8] Since 2008, a small company is defined as one meeting at least two of the following criteria: total assets less than £3.26 million, annual turnover less than £6.5 million, and an average number of employees fewer than 50. This group usually reports only assets, revenue, and profits.
[9] One limitation of this data set is that balance sheet items are always reported at the book value.

financial crisis period was characterized by an increase in firm exit, using only Amadeus would have generated selection concerns that could undermine the reliability of our results.[10]

Therefore, we further restrict the sample to firms meeting the following criteria: (i) matched to Amadeus; (ii) not a small firm, as defined by the Companies House; and (iii) not operating in the financial (SICs 600-699), public (SICs 900-999), or utility sectors (SICs 489-493).[11] This led to an initial sample of 987 unique firms. Once we exclude firms that did not meet minimum data requirements for the matching process described below, the sample includes 722 firms.[12]

2.3 Other Data

We supplement the data from Amadeus/Orbis to identify potential acquisitions and bankruptcies during the crisis. We start by constructing two different variables that identify whether a firm went out of business. In particular, we generate a dummy "Out of Business," which is equal to one if the firms' information is missing in Amadeus/Orbis by 2011, suggesting that the firm no longer exists.[13] On its own, the interpretation of this variable is unclear, since a firm can exit from the company registry for many different reasons, such as bankruptcy or acquisition.

We thus further refine this measure by generating a dummy— "Bad Exit"— that identifies companies that went out of business unambiguously because of distress. We generate this variable using the firm status history, available through Orbis. The data provider collects information from

[10] Orbis and Amadeus are essentially the same data product. The main two differences are the deletion of exited firms, as discussed above, and the interface used to distribute the data.
[11] This industry sample selection is common to the private equity literature. Similarly, Michaely and Roberts (2012) apply similar filter with the same data set.
[12] We require that companies have data on industry, return on assets, capital expenditures, asset, and leverage in 2007.
[13] In particular, we look at the total assets variable to identify company exits. Information on total assets is always required by UK reporting rules, and therefore when this field is missing, the company no longer exists.

the Companies House and assigns to each firm a status, such as active, dissolved, dormant, or in liquidation, which may change over time. We define a company status as a “bad exit” if (a) the firm was not active by 2011 and (b) before disappearing from the data, its status implied that the firm was in liquidation or in insolvency proceedings.

Similarly, we use Capital IQ to identify potential profitable exits by looking at firms involved in M&A transactions from 2008 onwards. Since M&A transactions may also arise because of distress, we provide an alternative measure by excluding companies that were involved in M&A but were also identified in the same period as in distress, as discussed above.

Lastly, we also collect information on the history of the PE investors for each portfolio company, in order to identify when the PE investors raised their last fund before the crisis. The younger the last fund at the onset of the crisis, the more likely that the private equity firm will have more financial and operational resources available, since the PE firm had less time to deploy capital and commit time to existing portfolio companies. In order to compile this information, we manually search the private equity firms in ThomsonOne and Capital IQ and collect information about their fundraising history.[14]

We also construct a measure of PE firm “dry powder,” a proxy for the dollar amount of financial resources that the PE investors have available by the time of the crisis. To generate this measure, we collect in ThomsonOne the fundraising and investment history for the PE investors during the 2001-2007 period, and calculate aggregate fundraising and investments.[15] The dry

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[14] If a portfolio company has more than one PE firm, we conservatively use the most recent fundraising year among all investors.

[15] In order to measure capital investments of PE investors, we sum the total equity investment made over the specified period. The estimated equity investment in each case is estimated in the following manner. If available, we use the estimated equity investment as reported by ThomsonONE. When this is not available, we estimate the equity investment by the fund as the total amount of equity invested in the firm divided by the total number of funds investing in the round. When ThomsonONE does not report the total equity invested, we use the value of the deal minus the debt (we assume debt to be zero when missing). Since the fund used for investment is not always reported, we use the total investment made by the PE group over the period.

powder measure is the difference between the PE firm fundraising and investment. Using this variable, we construct a dummy "high dry powder" that is equal to one if the company's PE investors are in the top quartile for dry powder. If a company has more than one PE, we use the shareholder with most resources.

3 Empirical Strategy

To understand how the crisis affected the financial and investment policies of PE-backed companies, we develop a difference-in-difference design where we compare PE-backed companies to a control group of non-PE backed companies around the financial crisis. We first describe how we construct the sample of matched firms and then discuss the empirical specification.

3.1 Constructing a Matched Control Group

Private equity-backed companies are clearly not a random sample of the population: for instance, they are likely to be larger and more leveraged than the average firm. Therefore, the first step in the analysis is to identify a proper control group for the set of PE-backed companies.

Following Boucly, Sraer, and Thesmar (2011), we identify a suitable control group through a matching procedure for each PE-backed company in our sample. We identify a set of control firms that operate in the same industry and had a similar size, leverage, and profitability in 2007. This procedure involved two steps. First, for all private equity-backed companies in our data, we selected every company in the Amadeus/Orbis sample that (a) belonged to the same two-digit SIC; (b) had a return on assets (ROA), defined as net income over total assets, within a 30% bracket around our PE firm; (c) had assets within a 30% bracket around our PE firm; and (d) had leverage within a 30% bracket around our PE firm. Second, if this first step identified more than five firms, we selected the closest five, based on quadratic distance computed based on the variables.

Overall, this procedure is a more conservative version of Boucly, Sraer, and Thesmar (2011), since we add an additional variable to the matching—leverage—and use a narrower matching bandwidth.[16] Using this methodology, we were able to match 434 of the 722 firms, generating a total sample of 1,984 firms. In the robustness section, we present an alternative matching procedure that is closer to Boucly, Sraer and Thesmar (2011), by eliminating leverage in the matching procedure.[17]

For every firm in the final sample, we extract from Amadeus/Orbis the full set of income and financial information available for the period from 2004 to 2011. Using these data, and following Brav (2009) and Michaely and Roberts (2012), we construct several measures of firm activity. In particular, we calculate capital investments as the change in assets plus the reported depreciation. Furthermore, we identify equity injections in the company by measuring the change in equity minus profit. Similarly, debt issuance is computed as the change in total liabilities in the year. All these variables are normalized by total assets. In addition, we measure firm leverage as total liabilities over total assets, and cost of debt as the ratio of total interest expenses to total debt. In order to limit the influence of outliers, we winsorize all ratios at 1%. The Data Appendix provides more information about the variables and the sample.

Panel A of Table 1 shows the industry distribution of PE-backed companies in the sample. We compare it with the universe of the UK firms, after we eliminate small businesses and

[16] The other difference is that we measure size in terms of assets and not employment. The reason for this choice is that employment variable in Amadeus is significantly less populated than assets. However, in a robustness test, we added employment as a fourth variable in our matching procedure and show that this does not affect the results.
[17] In a previous version of the paper, we have used wider matching boundaries (50% instead of 30%), which is in line with Boucly, Sraer, and Thesmar (2011). In general, these changes increase the size of our final sample and marginally reduced the quality of the matches, but did not affect the main results.

companies in the financial or regulated sectors. The majority of the sample firms are in either the services (38%) or manufacturing (32%) industries. Other important industries include wholesale trade, construction, and retail. The sample industry distribution is relatively close to the universe of companies: the major difference is that PE-backed companies tend to be more concentrated in manufacturing, and less represented in the construction industry and services. Both treatment and control samples have the same industry distribution due to the matching procedure.

In Panel B of Table 1, we compare the characteristics of firms in the treatment and the matched control group in 2007. The average firm in the sample is a mid-sized firm with around \$80 million in revenue. Across the two groups, firms have very similar ROA, investment, leverage, and equity and debt issuances. These differences are insignificant, with small economic magnitude. The only exception is that PE-backed companies are slightly larger than the control group in terms of revenue. Overall, this matching procedure suggests that differences in funding patterns across the treated and control groups mostly disappear when we compare firms with similar sizes, leverage ratios, and profitability within the same industry.[18]

Since this paper relies on a difference-in-difference analysis, it is important to explore the assumption of pre-crisis parallel trends. We explore whether this assumption holds in the observables in Panel C. In particular, we compare one and two-year growth rates ending in 2007 for the main firm characteristics considered so far. We find that the differences in the growth rates between the two groups are not significantly different from zero across all observables. Similar patterns can be seen graphically in Figures 3, 4, and 5, in which both treatment and control firms follow similar trends in the years leading to the crisis.

[18] One residual concern regards the presence of listed firms in the control group. In principle, listed firms may have been differentially affected by the financial crisis and this feature may have partially affected the result. However, we have only 19 control firms (<2%) that are public.

Overall, these analyses suggest that PE-backed companies were similar in 2007 to the control group. In principle, this is not a necessary condition for our identification—which instead hinges on the presence of parallel trends between the two groups—but it allows us to exclude the possibility that differences in behavior around the crisis were due to differences in other observable characteristics. Later in the paper, we further show that our results are also stable when we augment our model with a set of controls for firm characteristics in 2007, which should absorb any residual differences in observables across the two groups.

Moreover, the two groups present similar growth paths before the crisis, which alleviate concerns that PE-backed companies were outperforming the control group before the crisis. As we discuss below, our estimates are consistent with the assumption of parallel trends between treated and control groups during the pre-crisis period leading to the crisis, the main identification assumption in our difference-in-difference design. A more formal and direct test of the parallel trend assumption will be discussed in Section 4.

3.2 Identification Strategy

We estimate this model using a panel data set from 2004 to 2011, a symmetric window around the 2008 shock.[19] The choice of 2008 as the first year of the crisis is in line with a large body of empirical evidence on the crisis (e.g., Duchin, Ozabas, and Sensoy, 2010; Kahle and Stulz, 2013), as well as official statistics on the UK provided by the Bank of England. As we show in Figure 1, aggregate investment in the UK declined by more than 20% between the beginning of

[19] For consistency, both PE-backed companies and each corresponding control group enter in the sample at the same time, which is 2004 or the year of the PE deal if after 2004.

2008 and mid-2009. At the same time, credit availability experienced a sharp contraction starting in the first quarter of 2008 (Figure 2).[20] We estimate the following equation:

$$y_{it} = \alpha_t + \alpha_i + \beta_1(PE\ firm_i * Crisis) + \theta X_{it} + \varepsilon_{it} \qquad (1)$$

where y_{it} is an outcome variable measured for company i at time t, (α_i,α_t) are a set of company and year fixed effects, $PE\ firm_i$ is a dummy for the companies that are backed by PE investors, and *Crisis* is a dummy for the period from 2008 to 2011. Furthermore, we augment our specification with a set of firm covariates X_{it}. Lastly, we cluster standard errors at the firm level (Bertrand, Duflo, and Mullainathan, 2004).

The inclusion of firm fixed effects removes time-invariant differences between treatment and control firms. However, the causal interpretation of the results crucially depends on the parallel trend assumption. In particular, we need to assume that PE-backed companies would have experienced the same change in behavior as non PE-backed companies in the absence of the financial crisis.[21] The parallel trend assumption is intrinsically untestable, since we cannot observe the true counterfactual in the absence of the shock. However, we can strengthen the interpretation of our analysis by providing evidence consistent with this assumption, exploring pre-shock trends.

First, it is important to recognize that our treatment and control groups are similar, at least in terms of observable characteristics. By construction, both groups have the same industry distribution, and as we discussed before, profitability, investment, and leverage are similar across these groups. Even more importantly for the parallel trend assumption, the PE and non-PE companies have similar growth rates in the years leading up to the crisis, as we illustrate below.

[20] Statistics are taken from the Bank of England "Trends in Lending - April 2009" (2009).
[21] For instance, it would be problematic if treated firms differed from untreated firms along some characteristics that would be affected by the financial shock independently from their status as a PE-backed company.

Pushing this argument one step further, we can formally examine the time-varying behavior of the treatment effects for the main outcomes in our analysis by estimating:

$$y_{it} = \alpha_t + \alpha_i + \sum \beta_k (PE\ firm_i) + \theta X_{it} + \varepsilon_{it} \qquad (2)$$

where we estimate a different β_k for every year between 2004 and 2011, using the last year before the crisis, 2007, as the reference year. If our parameter β_k in the standard equation is correctly capturing the causal effect of the crisis on private equity firms—rather than a differential trend between the two groups—then we expect the effect of private equity to appear only at the onset of the crisis. In the next section, we will show evidence consistent with this argument.

In the paper, we take two additional steps to strengthen the analysis further. First, we augment our specifications with controls that capture the heterogeneity across firms in important characteristics before the crisis. In particular, we control for firm size (log of revenue), growth of revenue, normalized cash flow (cash flow over assets), profitability (ROA), and leverage. To avoid concerns regarding the endogeneity of controls (Angrist and Pischke, 2008; and Gormley and Matsa, 2014), these variables are measured in the pre-crisis period (2007) and then interacted with the crisis dummy to allow them to have a differential impact around the shock. These controls further alleviate concerns regarding the presence of some unbalanced observable characteristics across treatment and control groups before 2008.

Second, as a robustness test for our main results, we also add a full set of time-varying industry fixed effects, which can account for changes in industry demand and other industry considerations around the financial crisis. In particular, we interact two-digit industry fixed effects with the post dummy. We discuss this, as well as additional robustness tests, in Section 4.2.

4 Investment and Funding

4.1 Main Results

We start by examining whether companies backed by PE investors were more or less affected by the financial crisis. While overall investments dropped significantly in the UK during the crisis period, it is important to understand whether PE-backed companies experienced even a more severe decline during the crisis.

We start our analysis by studying the change in investment policies in PE- and non PE-backed companies. In column (1) of Table 2, we find that PE-backed companies decreased investments less than non-PE backed companies around the financial crisis. This effect is not only statistically significant, but also large in economic magnitude. Normalized by assets, the PE firms saw their investments increase almost 6% relative to the non-PE companies in the post-crisis period. In column (2), we find that the results are unchanged—in terms of both size and statistical significance—when we add the standard set of firm-level controls.[22]

In Figure 3, we plot the year effects estimates around the crisis—and the corresponding standard errors—separately for the PE-backed companies and matched companies. As illustrated in the figure, both treated and control firms followed similar paths before the crisis: the estimates are not statistically different from one another. Hence, the estimates seem to satisfy the parallel trends assumption. Once the crisis ensued, both the PE-backed companies and the matched companies decreased investments dramatically during 2008 and 2009. However, the PE-backed companies decreased their investments significantly less during the crisis years, consistent with the evidence in Table 1. This higher level of investment persisted in the years after the crisis.

[22] Since we focus on the PE treatment effects around the crisis and therefore after the PE investments, our results do not account for the potentially positive impact of the initial private equity investment on operation and financing. Since the effects of the initial investment has been generally found to be positive (e.g. Kaplan, 1989), our estimates may under-estimate the overall effect of PE on the portfolio companies.

Similar conclusions arise from Column (1) of Table 3, where we estimate equation (2) to capture year-by-year PE effects (we add company controls in column (2)). This analysis formally estimates the significance of the differences between the two groups, confirming the lack of statistically significant patterns before the crisis. In contrast, note that investments by PE-backed companies substantially diverged from the control group at the same time as the sharp decline in aggregate investments and credit in the UK, as illustrated in Figures 1 and 2. This positive difference persists in the next few years. We plot the estimates in column (2) graphically in Panel A of Figure A.1 in the Appendix.

Overall, the results so far suggest that companies backed by private equity firms were more resilient in the face of the financial crisis than a similar set of non-PE backed companies, therefore contradicting the claim that PE firms increased financial fragility. Next, we move to explore the mechanism behind this finding. One hypothesis is that private equity firms help their portfolio companies to maintain high investment levels by relaxing their financial constraints, particularly during periods of financial upheaval. This can happen in two ways. First, private equity firms have fund commitments that are rarely abrogated and may therefore be in a better position to inject equity into the companies if access to financial markets is barred. Second, private equity firms have strong ties with banks (Ivashina and Kovner, 2011) and should therefore find it easier to access credit markets during periods of turmoil. We find evidence that is generally consistent with both these channels in Table 2.

We find that net equity contributions increased more for PE-backed companies than for the control group around the crisis (Table 2, Columns 3 and 4).[23] Normalized by assets, equity

[23] Notice that we define equity contribution by looking at the changes in equity that are not explained by profit (see Data Appendix). Therefore, we cannot distinguish whether positive effects are due to raising more capital or paying out fewer dividends.

contributions during the financial crisis were 2% higher for PE-backed companies relative to non-PE firms. As illustrated in Figure 4, equity contributions for both classes of firms dropped significantly during the crisis. However, the decline was smaller for PE-backed companies. This suggests that PE funds were willing to support the operations of their portfolio companies by injecting equity into the firms. As illustrated in Columns (3) and (4) of Table 3, there are no divergent trends before the crisis. This divergence in financial policy appeared only in 2008. We plot these estimates in the Appendix, in Panel B of Figure A.1.

At the same time, Column (5) of Table 2 illustrates that PE-backed companies also experienced a relative increase in debt issuance.[24] While on average debt issuance over assets declined during the financial crisis, this decline was 4% smaller for PE-backed companies. The result is similar when adding controls, as illustrated in Column (6). These patterns can be observed in Figure 5. In the years leading to the crisis, PE-backed companies and matched companies followed similar, parallel trends. Both treated and control companies experienced a significant decline in debt issuances during the crisis. PE-backed companies, however, experienced a relative increase in debt issuance in 2008, exactly when lending conditions and aggregate investment started to decline in the UK. We find similar results when estimating equity issuances on a yearly basis in Columns (5) and (6) of Table 3. Again, we plot these estimates graphically in the Appendix, in Panel C of Figure A.1.

While overall debt issuance was greater, PE companies did not materially increase their leverage, as is evident from columns 7 and 8 in Table 2. The PE coefficient in this regression is positive, but it is non-significant and small in magnitude. This null result reflects the joint increase in equity and debt. However, in columns (9) and (10) of Table 2, we find that the relative cost of

[24] As discussed in the data section and in the Appendix, this is measured as the change in total debt, scaled by assets.

debt, measured by the ratio of interest expense of total debt, declined for the PE-backed companies. This is also illustrated over time in Columns (7) and (8) of Table 3, confirming that the relative decline in cost of debt appears first in 2008, the onset of the financial crisis.

One concern regarding the interpretation of the results is that by matching on leverage (in addition to other variables), we may have captured firms that are somewhat unrepresentative due to their high leverage. For this reason, we repeat the main analyses using an alternative matching that does not rely on leverage, but only on size, ROA, and industry. This matching estimator allows the two groups to have different leverage ratios in the pre-crisis period. This approach has two main advantages. First, this matching is closer to the approach of Boucly, Sraer, and Thesmar (2011). Second, using fewer matching variables allow us to match a larger number of observations.

In Table 4, we repeat the analysis with the alternative matching methodology. We find that all results remain unchanged. In Columns (1) and (2), we find as well that PE-backed companies experience a smaller decline in investment during the crisis and the effect is still highly statistically significant. Similarly, we find similar results with respect to equity contribution (Columns 3 and 4) and debt issuances (Columns 5 and 6). The only difference with our main results is a positive increase in the relative leverage ratio for PE-backed companies (Columns 7 and 8), but the effect is small in magnitude and only of borderline significance. Moreover, as is the case in the main results, we still find a decline in interest expense during the crisis for PE-backed companies.

Overall, these analyses suggest that private equity firms alleviated financing constraints of portfolio companies during the financial crisis, allowing them to invest more when credit markets were frozen and economic uncertainty high. In particular, private equity firms appear to have taken advantage of their fund structures and bank relationships to provide both equity and debt financing to their portfolio companies, with the latter at a lower cost.

4.2 Robustness

In this section, we examine a set of robustness analyses. First, we drop management buyouts from the main sample. At least historically in the UK, MBOs were characterized by lower engagement of private equity firms. If their inclusion completely drove the results, the interpretation and generalization of the analysis might be subtler. To explore whether this is the case, we eliminate MBOs from the sample and repeat the main analysis. As we show in Table A.1 in the Appendix, we find similar. In columns (1) and (2), we find that the effect on investment when MBOs are dropped is even larger than the effect in our baseline model. Similarly, we confirm the expansion in equity contribution and debt issuance, the relative stability in the leverage ratio, and the decline in interest expense. Therefore, the exclusion of MBOs does not affect the results.

Second, we explore whether the main results can be driven by attrition. As usual with panel data, the endogenous exit of firms from the data may bias results. Exit may be particularly problematic if PE-backed companies are more likely to enter into distress or be targeted in M&A transactions. To start, note first that as illustrated in Table 3, the shift in investment and financing policies occurred in 2008, immediately at the onset the financial crisis and arguably concerns about attrition may take place only later. We can also illustrate this pattern more directly by estimating our standard model using data from 2007 and 2008 only (Table A.2), in which we find similar results. In other words, much of the shift in corporate policy happened soon after the onset of the crisis.

Another alternative robustness test to revisit attrition bias concerns is to focus only on firms that did not exit the sample. In Table A.3, we take this conservative approach and drop every firm that exited the database before 2011. This approach leads to approximately 15% fewer observations in the sample.[25] Even with this reduced sample, the main results remain unchanged. PE-backed companies appear to experience a lower decline in investment and a relative increase in equity contributions and debt issuance. At the same time, the leverage ratio stays constant, and interest expense declines.

Third, we show that our results are robust to changes in industry dynamics. One concern is that PE-backed companies may be more or less sensitive than the control group to changes in demand that are contemporaneous to the shock. In principle, this should not be a problem, because the treatment and control groups are matched across industries. Nonetheless, we augment our analysis with a full set of (two-digit) industry fixed effects interacted with the crisis dummy. This set of fixed effects can control non-parametrically for changes in demand and other time-varying industry characteristics. As we show in Table A.4 in the Appendix, despite the large number of fixed effects that the model introduces, the main results remain unchanged. The estimates are still close in magnitude and statistical power to the one presented before.

4.3 The Heterogeneity of PE-Backed Companies

The results so far are consistent with the idea that private equity can play an important role during financial turmoil by relaxing the financial constraints faced by their portfolio companies. In this section, we provide more evidence consistent with this hypothesis by focusing on financially constrained firms.

We use several measures as proxies for financing constraints. First, we study how the effect of PE backing on investment differs between large and small firms (Table 5, Panel A). Consistent with the idea that small companies are more likely to be financially constrained, small businesses

[25] For every PE-backed company, there are up to five matched control firms. There are 310 companies that exit before 2011, which corresponds to 245 groups of companies that are dropped.

have been shown to be more sensitive to credit market shocks (Petersen and Rajan, 1994; Chodorow-Reich, 2014; and Bottero, Lenzu, and Mezzanotti, 2015). In our sample, we identify large firms by looking at the top quartile of employment at 2007, the last year in our pre-shock period, and classify remaining firms as small. Using this measure, we show in Columns (1) and (2) that the positive effect on investment is stronger for small companies.

Second, we find similar results when we look across companies that operate in industries that are more likely to depend on external finance, identified using the standard Rajan-Zingales (RZ) index (Rajan and Zingales, 1998). In particular, we define more financially dependent firms as companies operating in two-digit SIC industries characterized with an above-median share of capital expenditure that is externally financed.[26] In principle, firms that were more dependent on external finance should have been more affected by the financial crisis, given the dramatic decline in credit availability. Therefore, if private equity provides some relief to financial stresses, companies in industries characterized by larger RZ indices should benefit more from PE. Consistent with this idea, in Columns (3) and (4) of Panel A of Table 5, we find that the positive effect of being backed by private equity is larger for firms in more financially dependent industries.

Third, we find similar results when comparing firms that were more leveraged entering into the crisis. In general, firms with higher leverage are characterized by lower financial flexibility and higher interest payment burdens. Therefore, they face more risks when credit markets dry up. Comparing companies based on their 2007 leverage levels, we define high-leverage firms if they are at the top quartile of the leverage distribution at the onset of the crisis. We find that companies with high pre-crisis leverage experienced lower investment post-crisis. But high leverage

[26] In line with the literature, this measure is computed using data from US corporations between 1980 and 2008, available through Compustat. In particular, for each two-digit SIC industry, we measure the RZ index as the median of CAPEX minus cash flows from operation, scaled by CAPEX.

companies backed by PE investors increased investments significantly more than their non-PE counterparts (Table 5, Panel A, Columns 5 and 6). The presence of a PE investor counter-balanced the negative effect of high leverage on investments.[27]

Similarly, we find that the effect of PE on debt issuance seem to be stronger among financially constrained companies (Table 5, Panel B). This is true when looking across size (Columns 1 and 2) and dependence on external finance (Columns 3 and 4). When sorting by leverage (Columns 5 and 6), however, the result is positive but not statistically significant. In Panel C of Table 5, we explore the case of equity issuances. While the coefficients are generally in the expected direction, they are not statistically significant. This suggest that financially constrained firms particularly benefited from debt issuances to alleviate financing constraints. By way of contrast, equity contributions benefited all PE-backed companies similarly.

Overall, these results suggest that the positive effect of private equity on investments was stronger among firms that are more likely to be financially constrained. Differences in funding strategies in response to the financial crisis—particularly with regard to debt—seem to explain this result.

4.4 The Heterogeneity of PE Firms

To further explore the underlying channel of the findings, we exploit heterogeneity across the private equity groups themselves. We focus on the differences in the firms in their financial and operational resources that were available by 2007, at the onset of the financial crisis. We look at this hypothesis in two ways.

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[27] Clearly, leverage at 2007 is endogenous to many firm characteristics, in particular debt capacity. If anything, firms that expect to respond more successfully to a negative credit shock should ex-ante employ more debt. Therefore, it is reasonable to think that the results are actually characterized by a downward bias.

First, we compare PE groups based on the amount of "dry powder" that they had available. As we discuss in the data section, we have collected information from ThomsonOne about amount of capital that PE firms had raised, but not invested, in the pre-crisis period. Firms with more capital available may be better positioned to provide liquidity to their portfolio companies and may be able to commit more time and attention to portfolio companies since they deployed less capital. As discussed in the data section, we used Capital IQ and ThomsonOne to identify the PE investors in each PE-backed company. We divided the PE-backed companies in two groups, depending on whether they had PE investors that were on the top quartile of dry powder in 2007.

In Table 6, we present the results. We naturally restrict the sample to PE-backed companies only, since the variation is at investor level is therefore available only for PE-backed companies. In Columns (1) and (2), we find that firms whose PE investors had considerable amount of dry powder at the beginning of the crisis increased their investment level relatively more. The result is both statistically and economically significant: a high dry powder firm experienced a 10% increase in investment (normalized again by assets) relative to the control group. Consistent with this result, we find that this group of PE firms were also more active in financing their portfolio companies. Companies financed by high dry powder groups had 5% greater debt issuances (Columns 5 and 6) and, importantly, 7% larger equity injections (Columns 9 and 10), consistent with their greater availability of capital.

Second, we test this hypothesis using an alternative measure, which is whether the PE group's most recent fund was of a relatively recent vintage in 2008. This analysis is based on the underlying assumption that PE firms with younger funds have more resources available—both financial and operational—to provide to their portfolio companies. Over the course of the first three to five years of the fund, PE firms deploy capital and commit their time and attention to portfolio companies. Therefore, PE funds that are younger at the onset of the crisis could direct more financial and operational resources to portfolio firms. We identify the year in which these investors raised their last fund before the financial crisis. We use a dummy that equals to one if the fund was formed in the years between 2002 and 2007.[28]

In Table 6, we also find a larger increase in investment when PE investors had raised a fund more recently (Columns 3 and 4), suggesting the importance of the availability of resources for the PE group. We find similarly strong patterns with respect to debt issuances, which have increased much more for companies where the investors had raised a fund more recently (Table 6, Columns 7 and 8). The effects are both economically and statistically significant. The results for equity contributions are similarly positive and statistically significant in Column 11. When adding firm controls in Column 12, however, while coefficients remain similar, they are no longer statistically significant.

Overall, the results in this section are consistent with the hypothesis that portfolio companies with PE investors that had more resources at the onset of the crisis, financial and operational, managed to increase investments in portfolio companies during the crisis.

5 Performance and Company Outcomes Analysis

5.1 Company Performance

In this section, we attempt to understand to what extent greater investment increased the long-term prospects of PE-backed companies. We examine this question by looking at various measures of company performance. Were the investment by PE-backed companies imprudent or

[28] The result is also robust when using a continuous measure of fundraising time, the year of the last fund raised before 2007.

wasteful, we would expect that these decisions would have had detrimental effects on their performance.

In Columns (1) and (2) of Panel A in Table 7, we find that PE-backed group assets grow faster than the matched firms'. This is consistent with prior findings that illustrate that PE-backed companies increased their relative investment during the crisis. Next, we turn to explore accounting measures of firm performance around the crisis period. In Columns (3) and (4), we explore EBITDA scaled by revenue, and in Columns (5) and (6), ROA, defined as net income over assets. In both cases, we do not find that PE-backed companies experienced worse performance relative to the matched firms during the crisis.

This analysis suggests that the average performance of PE-backed companies was not differentially affected by the financial shock: the increase in investment of PE-backed companies did not lead to a low quality or excessively risky projects. However, given the long-term nature of the returns in many corporate capital expenditures, these accounting measures of performance may fail to fully capture the underlying changes in asset quality and company value around the crisis. Therefore, next sections will explore two alternative dimensions of firms' performance. First, to capture the potential benefits of an increased investment, we explore how firms' market shares evolve around the crisis. Second, we examine exit patterns—both positive (M&A acquisition) and negative (bankruptcy)— in the post-crisis period.

5.2 Market Share

The increase in investment may yield long-term profitability (and valuation) benefits if it allows the firm to capture a larger share of its market. We explore this hypothesis in Panel B of Table 7. For each firm in our sample, we measure its market share as the firm's sales relative to the total operating revenue in its industry (using the three-digit SIC codes).[29] Then, using a log-specification, we explore the change in market share of PE-backed companies relative to their peers during the crisis.

In Columns (1) and (2), we explore this question focusing on market share outcomes in the first two years in the crisis (2008 and 2009), where we found the largest divergence in investment and funding policies. In other words, using market share as a dependent variable, we repeat the standard difference-in-difference model using the 2004-2009 period. We find that in the crisis period, PE-backed companies experienced an 8% increase in market share relative to the control group. In Columns (3) and (4), we show that results are also similar when we use the full sample period (2004-2011), albeit smaller and less precisely estimated.[30]

In order to explore what may drive this slight difference in the result, in Columns (5) and (6), we employ the fully interacted model where we examine the effect of being a PE-backed company on every year in our panel. Consistent with our previous results, we find that PE-backed companies experience a larger increase in market share in 2008 and 2009, but this effect becomes smaller and statistically non-significant in 2010 and 2011. This is consistent with the fact that the change in investment and funding policy were mostly concentrated in the 2008 and 2009, a period during which the financial turmoil and credit market freeze were most severe.

Overall, our results suggest that PE-backed companies may have channeled their investment towards an increase in market share rather than increasing their short-term profitability.

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[29] The total operating revenue of the industry is constructed using only medium and large firms in the Orbis/Amedeus data, as previously discussed. Results are also similar using the SIC two-digit industry classification.

[30] In Appendix Table (A.5), we show that these results are robust to an alternative specification. In particular, rather than using the full panel, we employ only cross-sectional variation. In particular, in Columns (1) and (2), we show that PE-backed companies were more likely to have larger market share in 2009 relative to 2007 (conditional logit model). Similarly, in Columns (5) and (6), we look at the growth in market share over the same period and we find that this growth in market share was 6% higher.

In the next section, we look at exit patterns as another relevant dimension to understand firms' performance.

5.2 Exit Analysis

In this section, we examine exit patterns in the post-crisis period. In particular, we compare the relative likelihood that PE-backed companies entered distress, went bankrupt, or were successfully acquired. This will provide an additional perspective on the performance of PE-backed companies during the crisis, potentially capturing dimensions not easily captured by accounting measures or market share.

As we discussed in the Section 2, we define "bad exits" when firm exit the sample after a status of financial distress or bankruptcy. We identify "potentially profitable exits" as company acquisitions without prior company distress. Thus, we examine how post-crisis exit patterns differed across PE-backed and non-PE companies. The variation under study is only cross-sectional, as we explore the status of the sample firms in 2011. Therefore, the difference-in-difference design is not suitable for these tests. To make PE and non-PE-related companies comparable in the cross-section, we control for industry fixed effects and firm characteristics. Even with these adjustments, a causal interpretation of these results requires much stronger assumptions than the previous set of results.

In particular, we estimate the following equation:

$$Exit_i = \alpha_{ind(i)} + \beta(PE\ firm_i) + \gamma {X_i}^{PRE} + \varepsilon_{it}$$

where $Exit_i$ is a firm-level dummy that identifies the type of exit activity ("bad" or "potentially profitable"), $\alpha_{ind(i)}$ are industry-level fixed effects at two digit SIC level, $PE\ firm_i$ is a dummy variable identifying PE-backed companies, and ${X_i}^{PRE}$ are the firm-level characteristics measured before the crisis (2007). Since the outcome here is only cross-sectional and discrete, we estimate this model using a conditional logit model. To facilitate interpretation, all the results are presented as marginal effects at the mean.

In Panel C of Table 7, we show the main results. We find that PE-backed companies were more likely to experience a potentially profitable exit (Columns 1 and 4). The results are similar with and without controls, but the magnitude is a bit smaller with controls. The most conservative estimates suggest that PE-backed companies were about 30% more likely to be acquired in the post-crisis period. At the same time, PE-backed companies were not more likely to go out of business or enter into distress in the post-crisis period (columns 5-8). The results are not only insignificant, but also small in magnitude.

Overall, in the post-crisis period, PE-backed companies were more likely to be targeted in a potentially profitable M&A transaction, and also were not more likely to go out of business. Despite the limitations of the cross-sectional analysis, the results are inconsistent with the hypothesis that PE financing increased the financial fragility of the PE-backed companies.

6 Conclusion

In this paper, we have studied how PE-backed companies responded to the turmoil caused by the 2008 financial crisis by exploring their investments, financing, and performance. One of the main objectives of this analysis is to explore whether PE-backed companies increased the fragility of the economy during the financial crisis, as recently articulated by the Bank of England and others. Furthermore, this analysis can improve our understanding about the relationship between economic cycles and financial intermediaries more generally.

We find that PE-backed companies decreased investments relatively less than the control group during the financial crisis. This result can be explained by the ability of PE-backed companies to utilize the resources and relationships of their private equity sponsors to raise equity and debt funding in this difficult period, and to lower their cost of capital, as captured by the interest expense. Furthermore, we find that the positive investment effects of private equity were particularly large in companies more likely to be financially constrained at the time of the crisis and more likely to occur when PE firms have more resources. The increase in investment during the crisis led to increased asset growth, market shares, and ultimately a higher probability to be acquired. Altogether, these results are inconsistent with the hypothesis that private equity contributed to the fragility of the economy during the recent financial crisis.

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Figure 1: Investment in United Kingdom around the financial crisis

This figure shows the quarterly business investment volume in the United Kingdom (seasonally adjusted). Currency values are at 2013. The measure does not include expenditure on dwellings, land and existing buildings and costs of ownership transfer of non-produced assets. The data is available at the "Office of National Statistics" in the UK.(https://www.ons.gov.uk/economy/grossdomesticproductgdp/bulletins/businessinvestment/quarter3julytosept2016revisedresults).

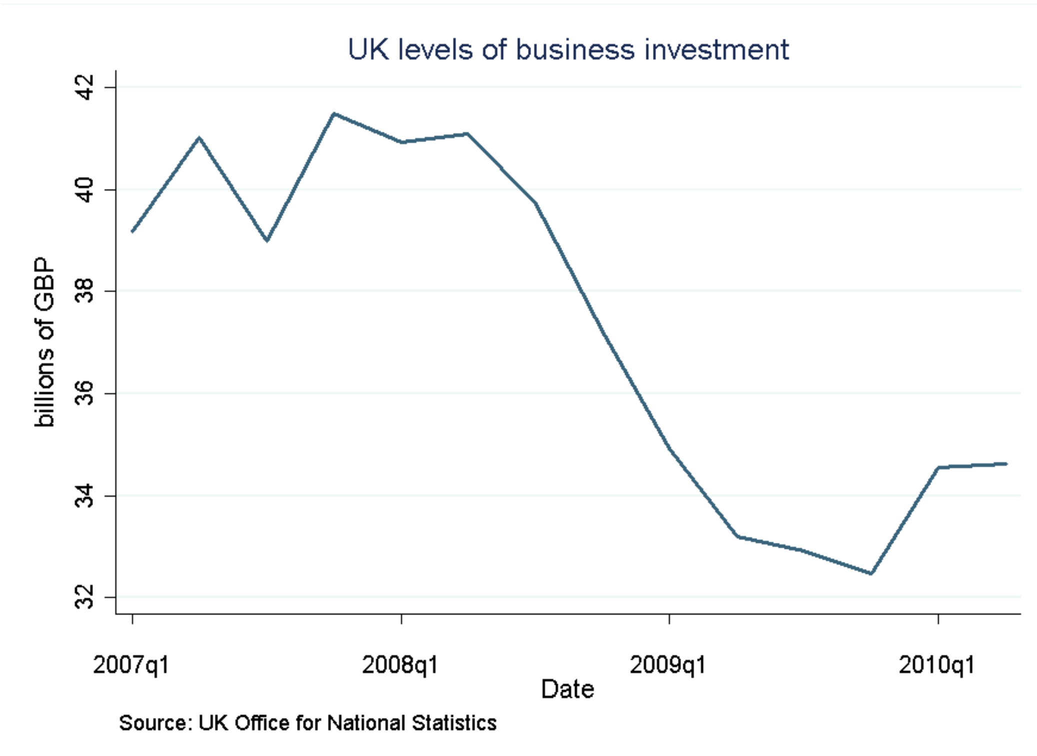


Figure 2: Lending growth in UK around the financial crisis

This figure shows the growth rate in the stock of lending by UK monetary financial institutions to private non-financial corporations (PNCF) or non-financial businesses. The stock of lending is the total amount of outstanding net lending. Series included are PNFC M4Lx (seasonally adjusted), sterling loans to PNFCs (seasonally adjusted), all currency loans to PNFCs (seasonally adjusted), all currency loans to non-financial businesses (non-seasonally adjusted). PNFC M4Lx is the lending to PNFCs, which includes loans, securities, reverse repos, overdrafts, and commercial paper. The other three measures each includes loans, reverse repos and overdrafts. The data is available at the official statistics of the Bank of England and they are reported in the report "Trends in Lending (2014)." http://www.bankofengland.co.uk/publications/Pages/other/monetary/trendsinlending2014.aspx .

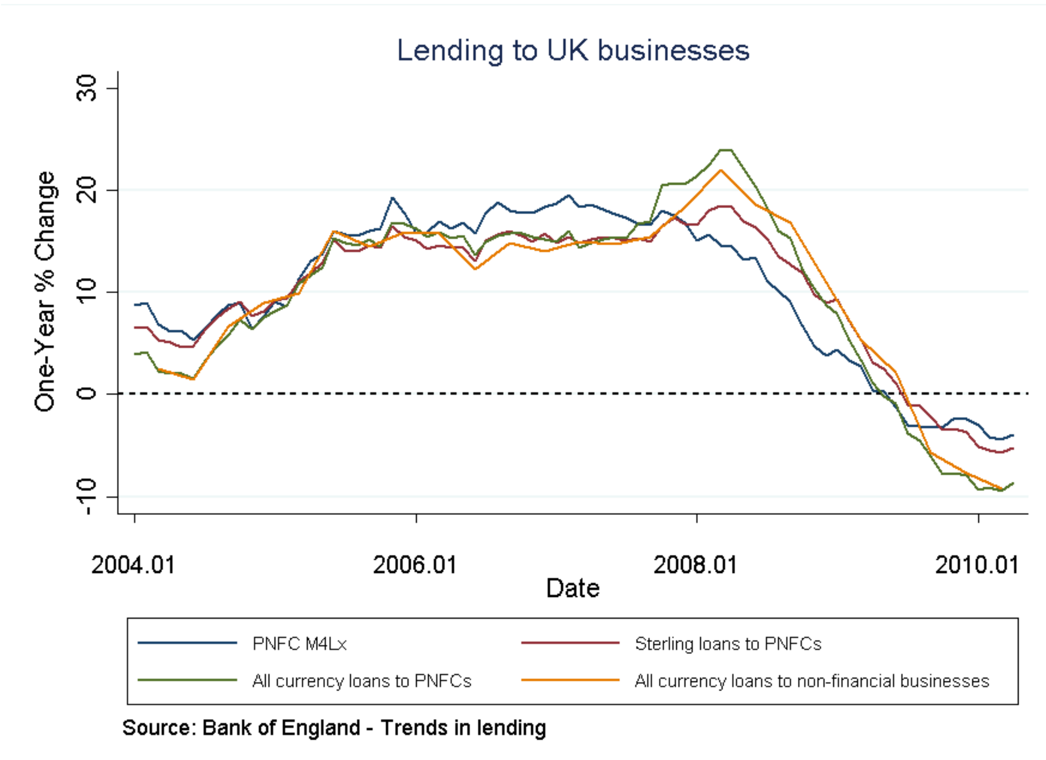


Figure 3: Effect of PE-backed companies on investment over time

This figure illustrates the change in investment separately for both PE and non-PE companies in our sample. Specifically, the figure reports α_t of the following equation: $y_{it} = \alpha_t + \alpha_i + \varepsilon_{it}$, estimated separately for PE and non-PE companies, where α_t capture year fixed effects, and α_i firm fixed effects. The year 2007 is used as base period and therefore the corresponding coefficient is normalized to zero. The estimates are plotted with standard errors above and below the point estimates. Standard errors are clustered at firm-level.

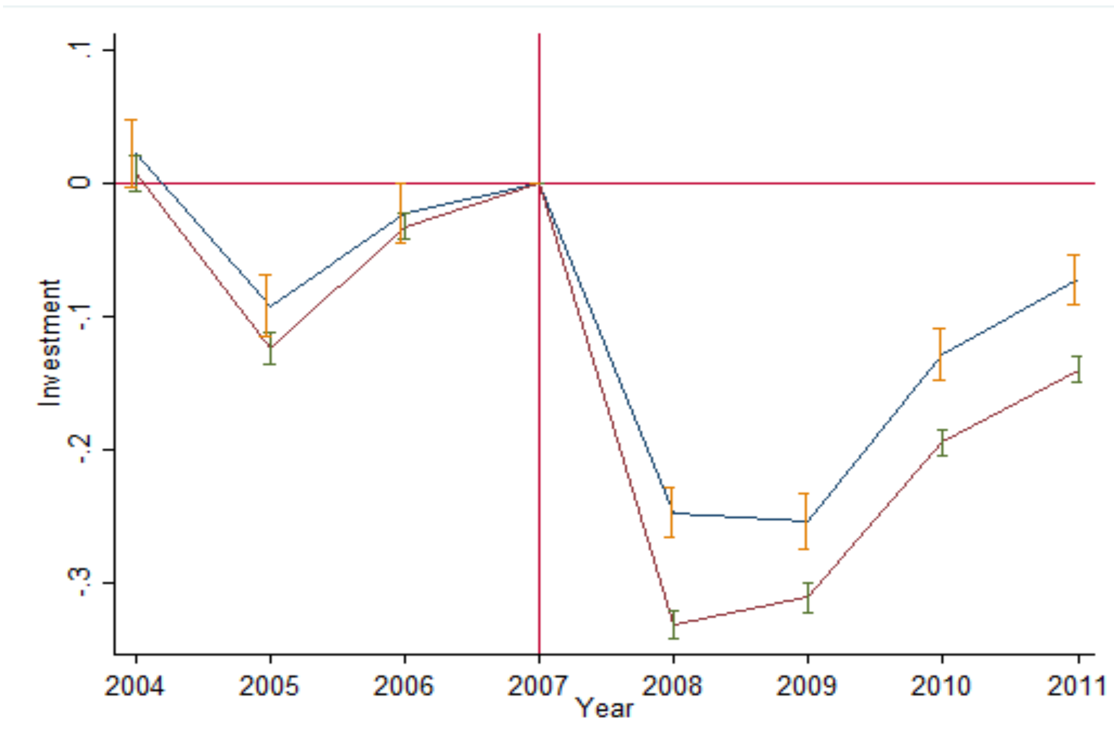


Figure 4: Effect of PE-backed companies on equity contributions over time

This figure illustrates the change in equity contributions separately for both PE and non-PE companies in our sample. Specifically, the figure reports α_t of the following equation: $y_{it} = \alpha_t + \alpha_i + \varepsilon_{it}$, estimated separately for PE and non-PE companies, where α_t capture year fixed effects, and α_i firm fixed effects. The year 2007 is used as base period and therefore the corresponding coefficient is normalized to zero. The estimates are plotted with standard errors above and below the point estimates. Standard errors are clustered at firm-level.

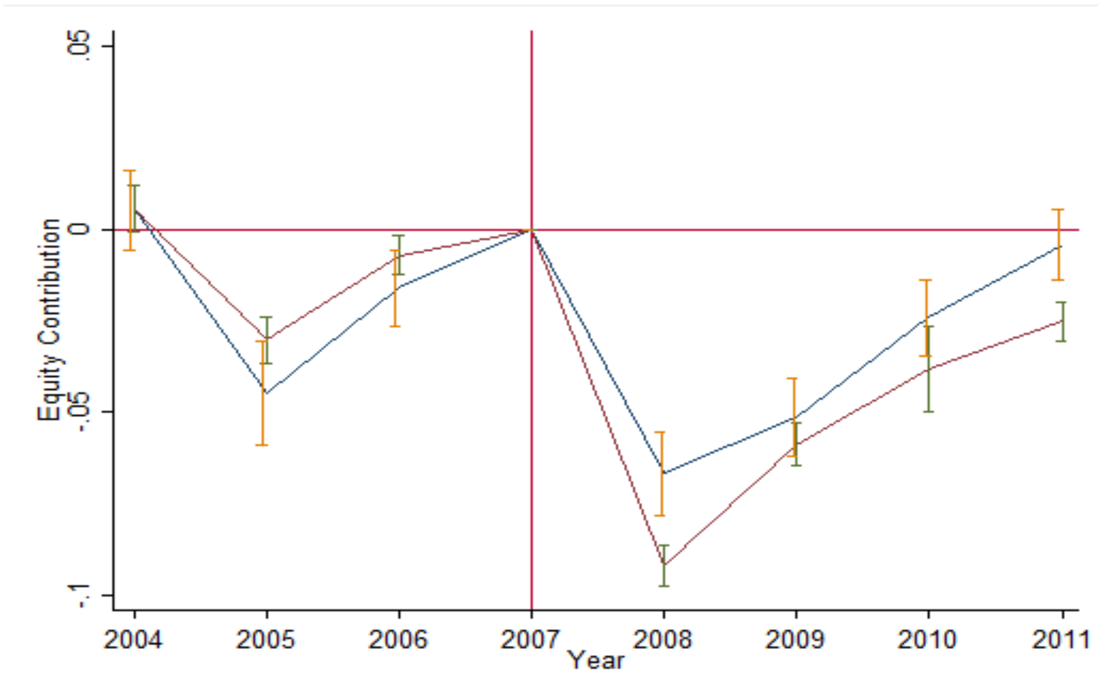


Figure 5: Effect of PE-backed companies on debt issuances over time

This figure illustrates the change in debt issuances separately for both PE and non-PE companies in our sample. Specifically, the figure reports α_t of the following equation: $y_{it} = \alpha_t + \alpha_i + \varepsilon_{it}$, estimated separately for PE and non-PE companies, where α_t capture year fixed effects, and α_i firm fixed effects. The year 2007 is used as base period and therefore the corresponding coefficient is normalized to zero. The estimates are plotted with standard errors above and below the point estimates. Standard errors are clustered at firm-level.

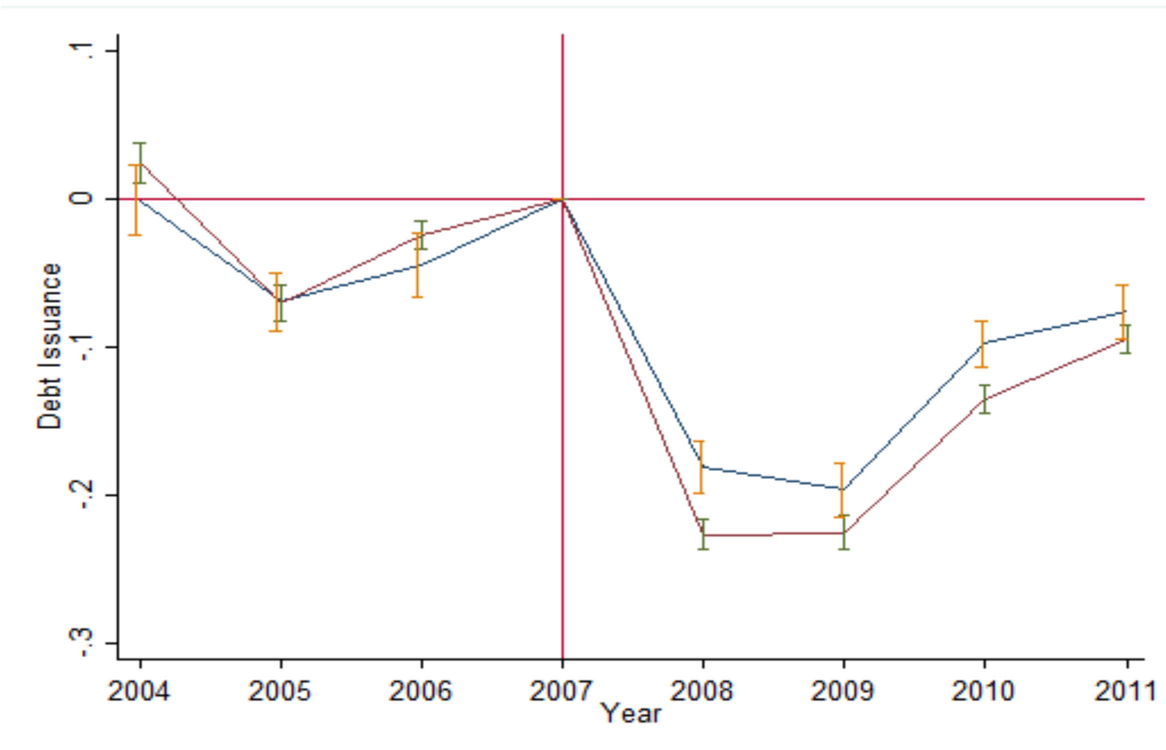


Table 1: Summary Statistics

Panel A reports the industry distribution at the macro industry level (1-digit SIC) for the PE sample and the whole universe of medium and large UK firms, but excluding financial, insurance, regulated or public administration. In Panel B reports the summary statistics of sample firms in 2007 across treated (PE-backed companies) and non-treated firms (non-PE companies). The last column reports the mean difference across the two groups. Level variables are in millions of dollars. Panel C reports the one-year and two-year growth in the characteristics in 2007. The last column reports the mean difference across the two groups. More information on variable definition is available in the Appendix. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Panel A: Industry distribution

Industry Distribution	PE Sample	Full Sample
Mining	1%	2%
Construction	6%	15%
Manufacturing	32%	17%
Wholesale Trade	12%	11%
Retail Trade	7%	6%
Transportation	4%	6%
Services	38%	44%

Panel B: Firms' characteristics in 2007

	PE Sample				Matched Sample				
	N	Mean	Median	SD	N	Mean	Median	SD	Mean Diff.
Revenue (M$)	432	98.05	35.30	240.81	1527	77.64	29.86	184.49	20.41*
ROA	434	0.09	0.09	0.23	1550	0.09	0.09	0.22	0.01
Investment/Asset	434	0.19	0.20	0.18	1550	0.20	0.20	0.18	-0.01
Equity Contr/Asset	415	-0.02	0.01	0.13	1513	-0.01	0.01	0.13	-0.01
Net Debt Iss. /Asset	415	0.09	0.10	0.23	1513	0.11	0.08	0.24	-0.01
Debt/Asset	434	0.71	0.70	0.39	1550	0.69	0.67	0.39	0.02

Panel C: Firms' trends in 2007

	PE Sample				Matched Sample				
	N	Mean	Median	SD	N	Mean	Median	SD	Mean Diff.
One Year Growth									
Revenue	423	0.37	0.18	1.34	1456	0.35	0.17	1.17	0.02
ROA	427	0.71	-0.03	5.21	1483	0.79	0.07	4.48	-0.07
Investment/Asset	386	1.54	0.10	5.86	1434	1.37	0.05	5.20	0.17
Equity Contr/Asset	372	-0.59	0.39	15.96	1376	-0.93	0.09	13.73	0.34
Net Debt Iss. /Asset	376	2.95	0.32	15.09	1428	2.25	0.20	12.86	0.70
Debt/Asset	418	0.02	-0.03	0.34	1516	0.02	-0.02	0.31	0.01
Two Year Growth									
Revenue	393	0.56	0.33	2.08	1362	0.71	0.34	2.33	-0.15
ROA	400	1.10	0.05	8.33	1388	1.40	0.11	6.97	-0.29
Investment/Asset	339	1.85	0.61	6.22	1333	2.39	0.94	6.06	-0.54
Equity Contr/Asset	330	0.43	1.09	23.44	1274	0.70	1.05	18.95	-0.28
Net Debt Iss. /Asset	343	3.45	0.65	18.73	1359	2.94	0.76	13.99	0.51
Debt/Asset	382	0.01	-0.04	0.46	1442	0.04	-0.04	0.60	-0.03

Table 2: Investment and funding policies

This table reports the estimates of a difference-in-difference fixed effect model on the investment and funding variables. All specifications include firm and year fixed effects. The main parameter of interest is the interaction between the crisis dummy and PE-backed company dummy variable. Odd columns contain the baseline regression and even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables include firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. In Columns (1) and (2) the outcome is investment scaled by assets; in Columns (3) and (4) is net equity contribution over assets; in Columns (5) and (6) is the net debt issuance scaled by assets; in Columns (7) and (8) is the total leverage; in Columns (9) and (10) on average interest rate. More information on the variables are available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Investment/Assets		Net Equity Contr./Assets		Net Debt Iss./Assets		Leverage		Interest Rate	
PE firm x Crisis	0.059***	0.056***	0.022***	0.021***	0.042***	0.039***	0.013	0.012	-0.003**	-0.003**
	(0.013)	(0.013)	(0.007)	(0.007)	(0.011)	(0.011)	(0.015)	(0.014)	(0.001)	(0.001)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	12456	11910	12469	12003	12903	12274	13205	12553	10222	9831
Clusters	1984	1878	1981	1876	1982	1876	1984	1878	1841	1743
R-squared	0.160	0.161	0.040	0.059	0.090	0.104	0.011	0.029	0.016	0.022

Table 3: Investment and funding policies over time

This table reports the estimates from a time-varying fixed effects model. All specifications include firm and year fixed effects. Specifically, the table reports β_t of the following equation: $y_{it} = \alpha_t + \alpha_i + \beta_t(PE\ firm_i) + \varepsilon_{it}$ where α_t capture year fixed effects, and α_i firm fixed effects. E columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. In Columns (1) and (2) the outcome is investment scaled by assets; in Columns (3) and (4) the outcome is net equity contribution over assets; in Columns (5) and (6) is the net debt issuance over assets; in Columns (7) and (8) is average interest rate. More information on the variables is available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Investment/Assets		Net Equity Contr./Assets		Net Debt Iss./Assets		Interest Rate	
PE firm x y2004	0.015	0.018	-0.000	0.002	-0.026	-0.028	0.001	-0.000
	(0.029)	(0.029)	(0.013)	(0.013)	(0.027)	(0.028)	(0.003)	(0.003)
PE firm x y2005	0.032	0.032	-0.015	-0.013	0.001	0.000	-0.002	-0.002
	(0.026)	(0.026)	(0.015)	(0.016)	(0.023)	(0.023)	(0.002)	(0.002)
PE firm x y2006	0.010	0.009	-0.009	-0.010	-0.020	-0.024	0.002	0.002
	(0.024)	(0.025)	(0.012)	(0.012)	(0.024)	(0.024)	(0.002)	(0.002)
PE firm x y2008	0.084***	0.087***	0.025**	0.025**	0.046**	0.043**	-0.003**	-0.003**
	(0.022)	(0.022)	(0.013)	(0.012)	(0.021)	(0.020)	(0.001)	(0.001)
PE firm x y2009	0.057**	0.050**	0.008	0.006	0.029	0.022	-0.002	-0.002
	(0.024)	(0.024)	(0.012)	(0.012)	(0.021)	(0.021)	(0.002)	(0.002)
PE firm x y2010	0.067***	0.064***	0.014	0.013	0.037**	0.032*	-0.003*	-0.004**
	(0.021)	(0.021)	(0.012)	(0.012)	(0.018)	(0.018)	(0.002)	(0.002)
PE firm x y2011	0.068***	0.064***	0.021*	0.018*	0.019	0.019	-0.002	-0.003
	(0.021)	(0.021)	(0.011)	(0.011)	(0.020)	(0.020)	(0.002)	(0.002)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes
Observations	12456	11910	12469	12003	12903	12274	10222	9831
Clusters	1984	1878	1981	1876	1982	1876	1841	1743
R-squared	0.160	0.161	0.040	0.059	0.090	0.104	0.016	0.021

Table 4: Main results with alternative matching sample

This table reports the estimates of a difference-in-difference fixed effect model on the investment and funding variables using an alternative matching estimator based only on ROA, industry and size. All specifications include firm and year fixed effects. The main parameter of interest is the interaction between the crisis dummy and PE-backed company dummy variable. Odd columns contain the baseline regression and even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables include firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. In Columns (1) and (2) the outcome is investment scaled by assets; in Columns (3) and (4) is net equity contribution over assets; in Columns (5) and (6) is the net debt issuance scaled by assets; in Columns (7) and (8) is the total leverage; in Columns (9) and (10) on average interest rate. More information on the variables are available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Investment/Assets		Net Equity Contr./Assets		Net Debt Iss./Assets		Leverage		Interest Rate	
PE firm x Crisis	0.057***	0.053***	0.025***	0.022***	0.040***	0.037***	0.026*	0.024*	-0.003***	-0.004***
	(0.012)	(0.012)	(0.007)	(0.007)	(0.011)	(0.011)	(0.015)	(0.015)	(0.001)	(0.001)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16318	15672	16347	15790	16872	16150	17259	16512	12808	12369
Clusters	2598	2477	2596	2475	2596	2475	2598	2477	2356	2251
R-squared	0.153	0.153	0.040	0.076	0.075	0.103	0.009	0.026	0.011	0.015

Table 5: Heterogeneity across firms' financial constraints

These tables estimate standard difference-in-difference fixed effect model and repeat the specification of Table 2 while exploring various proxies of financing constraints in 2007. All specifications include firm and year fixed effects. In each table, the interaction term in Columns 1 and 2 is based on firm size, and equal one if the firm is at the top quartile of firm employment versus the rest of the sample. The interaction in Columns 3 and 4 is based on dependency on external finance, measured by RZ index (Rajan and Zingales, 1998). The interaction equals one if dependence on external finance is above the median, and zero otherwise. In Columns 5 and 6, the interaction is based on firm leverage. The interaction equals one if firm leverage is at the top quartile within the sample. Panel A reports the results using investment as an outcome, Panel B uses instead debt issuance over assets as dependent variable and lastly Panel C reports the results with net equity contributions over assets. Even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. More information on the variables are available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Panel A - Investment / Assets

	(1)	(2)	(3)	(4)	(5)	(6)
PE firm x Crisis	0.014	0.011	0.030**	0.023	0.023	0.023
	(0.020)	(0.020)	(0.014)	(0.014)	(0.017)	(0.018)
Interaction x Crisis	-0.025*	-0.016	-0.047***	-0.041***	-0.055***	-0.038***
	(0.013)	(0.015)	(0.012)	(0.012)	(0.012)	(0.014)
Interaction x Crisis x PE	0.053**	0.051**	0.067**	0.077***	0.072***	0.064***
	(0.026)	(0.026)	(0.027)	(0.026)	(0.025)	(0.025)
Interaction Variable	Small		External Dependence		High Leverage	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	No	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes
Observations	11539	11105	12456	11910	12456	11910
Clusters	1824	1742	1984	1878	1984	1878
R-squared	0.160	0.162	0.161	0.162	0.162	0.162

Panel B - New Debt Issuances / Assets

	(1)	(2)	(3)	(4)	(5)	(6)
PE firm x Crisis	-0.004	0.003	0.014	0.012	0.036***	0.033**
	(0.019)	(0.019)	(0.013)	(0.012)	(0.013)	(0.013)
Interaction x Crisis	-0.015	-0.030**	-0.049***	-0.035***	-0.152***	-0.096***
	(0.014)	(0.014)	(0.012)	(0.011)	(0.011)	(0.013)
Interaction x Crisis x PE	0.055**	0.046*	0.062***	0.062***	0.028	0.023
	(0.024)	(0.024)	(0.024)	(0.024)	(0.021)	(0.021)
Interaction Variable	Small		External Dependence		High Leverage	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	No	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes
Observations	11891	11400	12903	12274	12903	12274
Clusters	1823	1741	1982	1876	1982	1876
R-squared	0.089	0.101	0.091	0.105	0.105	0.109

Panel C - Net Equity Contr./Assets

	(1)	(2)	(3)	(4)	(5)	(6)
PE firm x Crisis	0.035***	0.026**	0.016*	0.013	0.011	0.013
	(0.012)	(0.012)	(0.009)	(0.009)	(0.012)	(0.012)
Interaction x Crisis	-0.006	0.014	0.002	-0.002	0.067***	0.044***
	(0.007)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Interaction x Crisis x PE	-0.016	-0.008	0.015	0.016	0.011	0.009
	(0.016)	(0.015)	(0.015)	(0.014)	(0.015)	(0.014)
Interaction Variable	Small		External Dependence		High Leverage	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	No	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes
Observations	11564	11183	12469	11989	12469	11989
Clusters	1823	1739	1981	1873	1981	1873
R-squared	0.045	0.063	0.040	0.059	0.053	0.064

Table 6: Heterogeneity across funds

This table reports the estimates from a difference-in-difference fixed effect model, while exploring heterogeneity across resource availability of PE firms backing the company. The analysis is a cross-section estimated using only the set of PE-backed companies. High Dry Powder is a dummy variable equals to one if PE investors are at the top quartile for amount of dry powder at 2007, defined based on the amount of capital raised but not invested. The variable 1(Fund 02-07) is a dummy variable equals to one if the PE firm raised its latest fund between 2002 and 2007. All specifications contain firm and year fixed effects. Even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. More information on the variables are available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Investment/Assets				Net Debt Iss./Assets				Net Equity Contr./Assets			
Post*High Dry Powder	0.105**	0.086**			0.053*	0.062**			0.070***	0.055**		
	(0.048)	(0.041)			(0.031)	(0.03)			(0.025)	(0.022)		
Post*1(Fund 02-07)			0.075*	0.090**			0.064**	0.073***			0.036*	0.030
			(0.039)	(0.036)			(0.026)	(0.026)			(0.021)	(0.021)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1582	1539	1582	1539	1589	1546	1589	1546	1565	1527	1565	1527
Adjusted R-squared	0.108	0.117	0.106	0.117	0.064	0.068	0.064	0.068	0.028	0.048	0.023	0.044

Table 7: Performance Analysis

Panel A reports a standard difference-in-difference fixed effect model exploring various performance measures. All specifications include firm and year fixed effects. In Columns (1) and (2) the outcome is one year assets growth; in Columns (3) and (4) is total EBITDA scaled by revenue; in Columns (5) and (6) is ROA. Standard errors are clustered at firm level. In Panel B, the dependent variable is firm market share, measured as the log of share of firms' revenue scaled by total revenue at the level of three-digit SIC industry. Columns (1) and (2) estimate the standard model, but using only data from 2004-2009. Columns (3) and (4) instead uses the full sample period of 2004-2011. Lastly, Columns (5) and (6) report the coefficient from the time-varying regression. Standard errors are clustered at firm-level. In Panel C, we report the marginal value (at the mean) of a conditional logit model where we study the effect of being a PE-backed company on various exit outcomes. Even columns have firm level controls at 2007. In Columns (1) and (2) the outcome is a dummy equal to one if the company was the target of an M&A activity in the post-crisis period; in Columns (3) and (4) the outcome is instead a dummy equal to one if the company was a target of an M&A activity and the company does not exit from the data in the same time frame; in Columns (5) and (6) the outcome is the dummy equal to one if the company exit the data set in the post period; lastly in Columns (7) and (8) the outcome is a dummy if the company exit the data and it reported some financial difficulties before the exit. In all three panels, even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. See the Appendix and the paper for more info on the variables. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

Panel A- Accounting Performance

	(1)	(2)	(3)	(4)	(5)	(6)
	Assets Growth		EBITDA/REV		ROA	
PE firm x Crisis	0.148***	0.124***	-0.009	-0.010	-0.003	-0.004
	(0.040)	(0.038)	(0.013)	(0.014)	(0.009)	(0.008)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes
Observations	13180	12528	12507	12137	12865	12364
Clusters	1984	1878	1960	1878	1984	1878
R-squared	0.026	0.042	0.001	0.015	0.005	0.041

Panel B: Market Share

	(1)	(2)	(3)	(4)	(5)	(6)
PE firm x Crisis	0.081**	0.079**	0.050	0.055*		
	(0.035)	(0.034)	(0.034)	(0.033)		
PE firm x y2004					0.039	0.048
					(0.057)	(0.059)
PE firm x y2005					0.035	0.047
					(0.050)	(0.049)
PE firm x y2006					-0.035	-0.020
					(0.036)	(0.035)
PE firm x y2008					0.094***	0.106***
					(0.031)	(0.034)
PE firm x y2009					0.072**	0.088***
					(0.031)	(0.033)
PE firm x y2010					0.039	0.052
					(0.037)	(0.039)
PE firm x y2011					-0.007	0.005
					(0.053)	(0.055)
Sample	2004-2009		Whole Sample		Whole Sample	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes
Observations	9090	8847	12697	12326	12697	12326
Clusters	1960	1878	1960	1878	1960	1878
R-squared	0.035	0.087	0.021	0.064	0.021	0.064

Panel C - Exit Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Marginal Eff.	1{M&A}		1{M&A, No Distress}		1{Exit}		1{Bankruptcy}	
PE Firm	0.351***	0.325***	0.351***	0.318***	0.058	0.039	0.092	0.099
	(0.023)	(0.101)	(0.024)	(0.100)	(0.085)	(0.087)	(0.103)	(0.106)
Industry (2-digit) F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Firm Controls 2007		Y		Y		Y		Y
Observations	1635	1635	1635	1635	1368	1368	1360	1360

Appendix

A.1 Data and variable construction

Aside from Capital IQ, all the data in the paper come from the Amadeus/Orbis database, produced by Bureau Van Dijk. To minimize the chances that data errors could drive our results, we winsorize at 1% every ratio and growth rate used in the analysis. The winsorization was undertaken over the full sample of Amadeus/Orbis, companies. All variables in levels are in millions of dollars.

The main variables we used in the analysis are the following: (a) Investment/Assets, where investment is constructed as the change in assets over the past year, plus depreciation; (b) (Net) Equity Contribution/Assets, where the equity contribution is measured as the difference in total equity (shareholder value) over the past year, minus the profit; (c) (Net) Debt Issuance/Assets, where the debt issuance is measured as the overall change in debt; (d) Leverage, which is simply total debt (short- and long-term) divided by assets; (e) ROA, which is net income over assets; (f) EBITDA/Assets, where EBITDA is the earnings before interest, taxes, depreciation, and amortization; (g) the Logarithm of Market Share, where market share is the ratio of the firm revenue in a specific year and the total revenue of all medium and large firms in the same SIC two-digit industry.

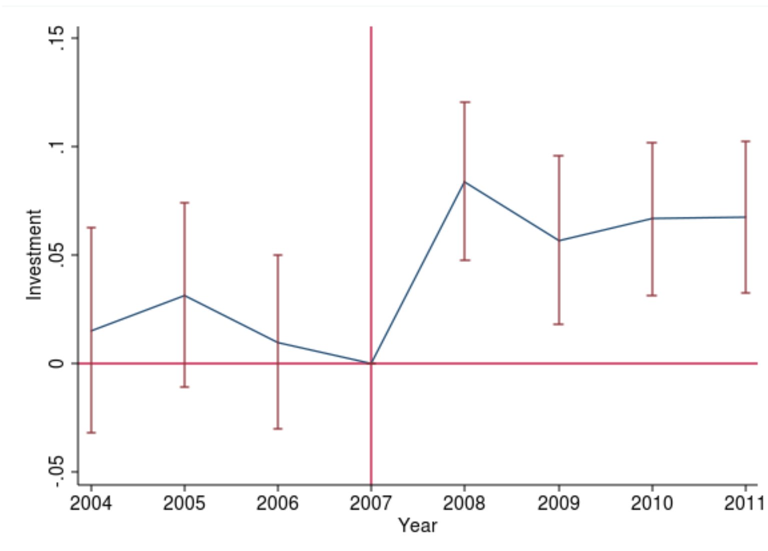
In the paper, we use different methodologies to determine companies that are more or less likely to be financial constrained at the time of the financial crisis. First, we use size by looking at the top quartile of revenue in 2007, across the sample. Second, we identify firms that are in the top quartile of leverage in 2007. Third, we identify companies operating in industries that are more dependent on external finance. The index is constructed using all firms in Compustat between

1980 and 2007: we construct a score for every two digit SIC code, which is the median of CAPEX minus operating cash flow, scaled by CAPEX.

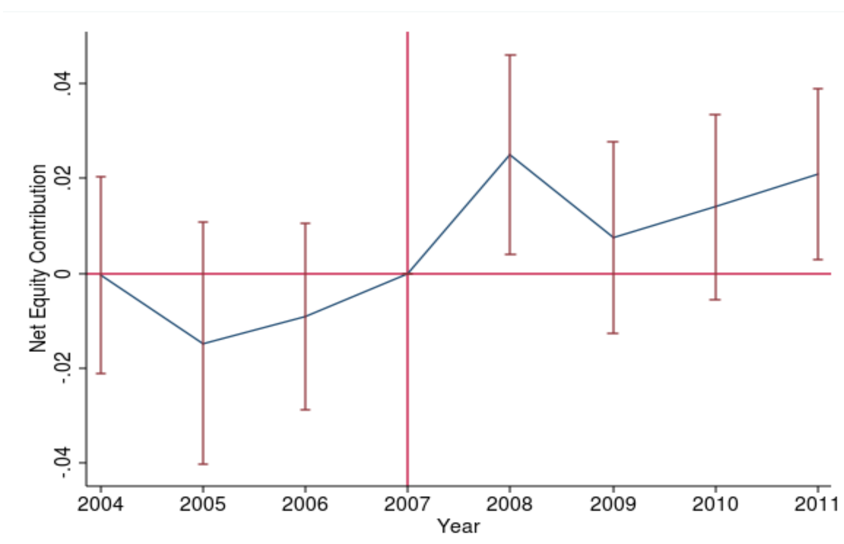
Figure A.1 : Effect of PE-backed companies over time

This figure reports the time-varying effect of being a PE-backed company on the main outcomes. Panel A reports the effect on investment, Panel B on debt issuance and Panel C on equity contribution. Specifically, this Figure reports the β_t of the following equation: $y_{it} = \alpha_t + \alpha_i + \beta_t(PE\ firm_i) + \varepsilon_{it}$. As explained in the paper, the year 2007 is used as base period and therefore the corresponding coefficient is normalized to zero. The central dot reports the point estimate while the straight vertical lines report the 90% confidence interval. The confidence interval is constructed using standard errors clustered at firm level. More info on this measure is available in the paper and in the Appendix.

Panel A - Investment



Panel B – Equity Contributions



Panel C – Debt Issuance.

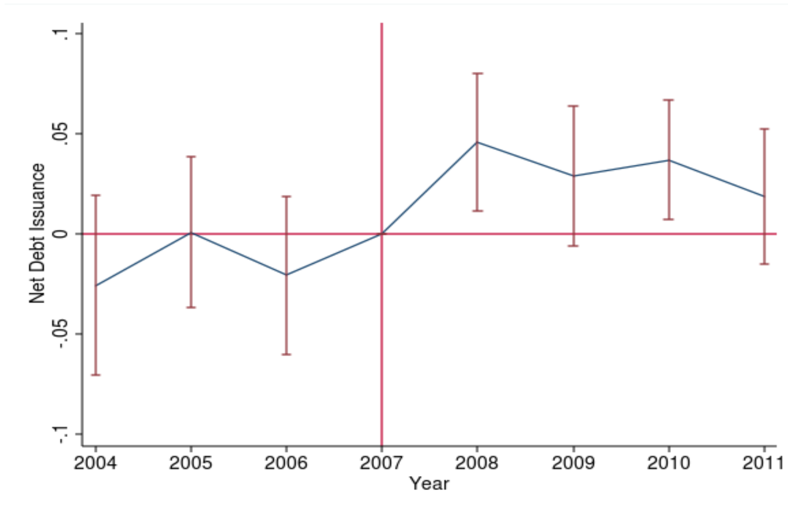


Table A.1: Robustness excluding MBO

This Table reports a robustness test, where we estimate the standard difference-in-difference fixed effect model on the main outcome variables dropping the PE-backed companies whose deal is identified as a management buyout (MBO) and the corresponding matched companies. Every specification contains a set of firm and year fixed effects. The main parameter of interest is the interaction between the crisis dummy and a dummy identifying PE-backed companies. Odd columns contain the baseline regression where instead even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. In Columns (1) and (2) the outcome is investment scaled by assets; in Columns (3) and (4) is net equity contribution; in Columns (5) and (6) is the net debt issuance; in Columns (7) and (8) is leverage; in Columns (9) and (10) is ROA. More information on the variables is available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Investment/Assets		Net Equity Contr./Assets		Net Debt Iss./Assets		Leverage		Interest Rate	
PE firm x Crisis	0.068***	0.072***	0.022**	0.018**	0.047***	0.053***	0.022	0.022	-0.003	-0.003*
	(0.017)	(0.017)	(0.010)	(0.009)	(0.015)	(0.015)	(0.020)	(0.020)	(0.002)	(0.002)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	8295	7965	8316	8028	8557	8181	8764	8376	6711	6483
Adjusted R-squared	0.157	0.159	0.039	0.064	0.085	0.103	0.009	0.029	0.016	0.021

Table A.2: Robustness using only 2007-2008

This table reports a robustness test, where we estimate the standard difference-in-difference fixed effect model on various outcomes using only data from 2007 and 2008. This corresponds to the last year before the crisis and the first one in the crisis. Every specification contains a set of firm and year fixed effects. The main parameter of interest is the interaction between the post dummy and a dummy identifying PE-backed companies. Odd columns contain the baseline regression where instead even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. In Columns (1) and (2) the outcome is investment scaled by assets; in Columns (3) and (4) is net equity contribution; in Columns (5) and (6) is the net debt issuance; in Columns (7) and (8) is leverage; abd in Columns (9) and (10) is ROA. More information on the variables is available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Investment/Assets		Net Equity Contr./Assets		Net Debt Iss./Assets		Leverage		Interest Rate	
PE firm x Crisis	0.083***	0.085***	0.027**	0.029**	0.047**	0.039*	0.004	0.001	-0.003**	-0.003**
	(0.022)	(0.022)	(0.013)	(0.012)	(0.021)	(0.021)	(0.010)	(0.010)	(0.001)	(0.001)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3924	3715	3860	3672	3892	3696	3948	3737	3183	3019
Adjusted R-squared	0.385	0.399	0.139	0.262	0.234	0.296	0.002	0.020	0.009	0.020

Table A.3: Robustness using only companies not experiencing exit

This table reports a robustness test, where we estimate the standard difference-in-difference fixed effect model on various outcomes using only data only for groups of matched firms where no company is identified as leaving the data by 2011 (survivorship bias free). Every specification contains a set of firm and year fixed effects. The main parameter of interest is the interaction between the post dummy and a dummy identifying PE-backed companies. Odd columns contain the baseline regression where instead even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. In Columns (1) and (2) the outcome is Investment scaled by asset; in Columns (3) and (4) is net Equity Contribution; in Columns (5) and (6) is the net Debt Issuance; in Columns (7) and (8) is leverage; in Columns (9) and (10) is ROA. More information on the variables is available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Investment/Assets		Net Equity Contr./Assets		Net Debt Iss./Assets		Leverage		Interest Rate	
PE firm x Crisis	0.044***	0.040***	0.017**	0.016**	0.030**	0.025**	0.003	0.000	-0.002	-0.003*
	(0.014)	(0.014)	(0.008)	(0.008)	(0.013)	(0.012)	(0.014)	(0.014)	(0.001)	(0.001)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	9658	9271	9700	9367	10020	9567	10242	9776	7963	7683
Adjusted R-squared	0.160	0.160	0.046	0.061	0.090	0.102	0.029	0.040	0.020	0.026

Table A.4: Robustness adding time-varying industry fixed effects

This table reports a robustness test, where we estimate the standard difference-in-difference fixed effect model on various outcomes adding set of fixed effects generated as the product of industry (two digit SIC) and the post dummy. Every specification contains a set of firm and year fixed effects. The main parameter of interest is the interaction between the post dummy and a dummy identifying PE-backed companies. Odd columns contain the baseline regression where instead even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. In Columns (1) and (2) the outcome is investment scaled by asset; in Columns (3) and (4) is net equity contribution; in Columns (5) and (6) is the net debt issuance; in Columns (7) and (8) is leverage; and in Columns (9) and (10) is ROA. More information on the variables is available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Investment/Assets		Net Equity Contr./Assets		Net Debt Iss./Assets		Leverage		Interest Rate	
PE firm x Crisis	0.055***	0.053***	0.022***	0.021***	0.039***	0.037***	0.012	0.013	-0.002*	-0.003**
	(0.013)	(0.013)	(0.007)	(0.007)	(0.011)	(0.011)	(0.015)	(0.014)	(0.001)	(0.001)
Industry X Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Control	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	12456	11910	12469	12003	12903	12274	13205	12553	10222	9831
R-squared	0.163	0.163	0.042	0.060	0.093	0.105	0.019	0.040	0.021	0.026

Table A.5: Robustness of Market Share Results

This table reports a robustness test on the market share results. All regressions are cross-sectional regressions, where we compare firms across PE and non-PE backed companies. In Columns (1)–(4), we estimate a conditional logit model, where the outcome is a dummy equal to one if the market share of the firm increased over 2009 and 2007 (Columns 1 and 2) or over 2011 and 2007 (Columns 3 and 4). The reported beta are marginal effect at the average and the model is estimated with SIC two-digit fixed effects. In Columns (5)-(8), we estimate an OLS model where the outcome is the growth rate in market share between 2009 and 2007 (Columns 5 and 6) or 2011 and 2007 (Columns 7 and 8). The market share growth is winsorized at 1% to reduce the influence of outliers. Odd columns contain the baseline regression where instead even columns augment the baseline model with a set of firm level controls measured before the crisis and interacted with the post dummy. These variables are firm size (log of revenue), growth in revenue, cash flow over assets, ROA, and leverage. More information on the variables is available in the Appendix. Standard errors are clustered at firm level. *** denotes significance at the 1% level, ** at the 5%, and * at the 10%.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Market Share Increase - Dummy				Market Share Growth			
	2007-2009		2007-2011		2007-2009		2007-2011	
PE Firm	0.054**	0.032*	0.042	0.017	0.046*	0.063**	0.027	0.054
	(0.024)	(0.018)	(0.027)	(0.012)	(0.027)	(0.031)	(0.049)	(0.050)
Industry (2-digit) F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Firm Controls 2007		Y		Y		Y		Y
Observations	1639	1639	1564	1564	1655	1655	1565	1565

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Abstract

We provide the first evidence on the performance of private operating firms as acquirers. Private bidders experience greater post-acquisition operating performance improvements compared to public bidders. This effect is not due to differences in target types, merger accounting, financing constraints, private equity ownership or subsequent listing of some private bidders, and is robust to instrumentation. Further analysis of governance arrangements at least partially attributes the private bidder effect to lower agency costs in private firms. Not only do private firms pay lower prices for target firm assets, they also operate them more efficiently by containing overhead costs and capital expenditures.

Keywords: private firms, mergers and acquisitions, operating performance improvements, agency conflicts

JEL Classifications: G34

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Abstract

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1 Introduction

Corporate takeovers are among the largest forms of corporate investment that a firm may undertake. For instance, corporations have spent US$5 trillion on deals worldwide in the year 2015 alone, amounting to 6.8% of world GDP.[1] Given the size and importance of this market, the performance of acquiring firms has received considerable attention in the academic literature. The extant empirical evidence shows that shareholders of acquiring firms earn, on average, close-to-zero and often negative abnormal returns around the time of takeover announcement, and that operating performance improvements often fail to materialize.[2] However, virtually all of the existing evidence on acquirer performance is based on public acquiring firms. There is no evidence on the success of acquisitions made by private operating firms (not to be confused with private equity buyouts), which represent a large portion of the real economy and a sizeable fraction of the mergers and acquisitions (M&A) market. Such undersampling has the potential to skew our understanding of takeovers (Netter, Stegemoller, and Wintoki (2011)).

In this paper we provide the first evidence on acquisition-related performance of private operating firms and compare it to that of public acquirers. Because private firms exhibit less separation of ownership and control, classic agency theory would predict that efficiency gains as a motive for acquisitions should be more prevalent – and empire-building less prevalent – in private firms as compared to public companies. However, it is also possible that higher agency costs in public firms are offset by benefits such as easier access to capital, monitoring by analysts and the market for corporate control, learning from stock prices, attracting better managerial talent, and optimal diversification of shareholders' portfolios. Whether private

[1]Source: Thomson Reuters SDC and International Monetary Fund.

[2]Many recent papers provide abnormal return estimates for takeover announcements, including Fuller, Netter, and Stegemoller (2002), Moeller, Schlingemann, and Stulz (2004, 2005), Masulis, Wang, and Xie (2007), and Golubov, Yawson, and Zhang (2015). Operating performance improvements are studied in Healy, Palepu, and Ruback (1992), Harford (1999), Ghosh (2001), Heron and Lie (2002). See also a review by Betton, Eckbo, and Thorburn (2008).

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or public firms generate greater efficiencies from their acquisitions is thus an open empirical question.

We bring this question to the data on both public and large private firms in the U.S. While data on private firms are generally unavailable, we take advantage of the fact that certain private firms are required to disclose their financials to the U.S. Securities and Exchange Commission (SEC) because of the size of their assets or because they have publicly traded debt. Although not representative of a typical private firm, these private firms are observably comparable to public firms in terms of size and information availability through 10-K filings.

Our analysis is based on a sample of 8,803 acquisition deals over the period 1997-2014 drawn from Capital IQ, of which roughly 15% were undertaken by private operating firms and the remainder by public bidders. Because the firm's listing status is likely endogenous, our tests are designed to address the associated identification challenges. For the majority of our analysis we rely on state-of-the-art matching techniques and compare private bidders to public bidders with the closest propensity to be private based on observable characteristics, disregarding public bidders that are too dissimilar. We also instrument listing status with venture capital availability in the firm's headquarter state during its early years, as in Asker, Farre-Mensa, and Ljungqvist (2015).

We find that, on average, private bidders exhibit positive operating performance improvements around acquisition deals, whereas operating performance changes for public bidders are mostly negative. Specifically, private bidders increase their return on assets (ROA) by 3-8% in the three years following the completion of the deal, while public bidders see a modest decline in their ROA of between zero and 2%. Industry- or control-firm adjustment of the performance metrics makes little difference to these magnitudes. Asset utilization rates, as measured by asset turnover (ATO), follow similar patterns.

Consistent with our agency-based prediction, differences in operating performance changes between private and public bidders are positive and statistically significant. Further regres-

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sion adjustment of our estimates confirms that the private bidder effect survives controls for acquiring firm's size, prior performance and acquisition experience, growth opportunities (age), *target* firm type (public versus private), relative deal size, industry relatedness, hostility, and cross-border status. That is, differences in operating performance changes are *not* picking up observable differences in bidder, target, or deal types.

Next we test whether the private bidder effect can indeed be attributed to differences in agency costs using firm-level data on governance arrangements of public and private firms in our sample. We take advantage of Capital IQ's coverage of antitakeover defences[3] and complement these data with hand-collected information on CEO ownership and ownership concentration by outside shareholders for both public and private firms. As anticipated, private bidders employ significantly fewer provisions limiting shareholder control and exhibit greater levels of CEO ownership and ownership concentration by the largest shareholders. We find that the private bidder effect is driven by private bidders with higher CEO ownership, higher ownership concentration by outside shareholders, and fewer takeover defences. Thus, the evidence is consistent with the agency cost/incentive alignment channel behind the private bidder effect. We also explore the sources of superior operating performance changes in private bidders and find that they come from better containing overhead costs and capital expenditures.

Finally, we rule out several alternative explanations for the private bidder effect. First, it is possible that private bidders simply go after targets with higher levels of ROA/ATO than target firms acquired by public firms, resulting in greater combined firm profitability. This does not appear to be the case. In the subsample of deals where the target firms' financials are available, we show that targets of private bidders are *not* more profitable than those of public bidders.[4] A second potential explanation has to do with merger accounting. If

[3]Note that most of our private bidders have more than 500 shareholders, rendering takeover defences relevant even for private firms. In addition, these provisions capture limitations to shareholder control more broadly, beyond takeover situations.

[4]In addition, if targets of private bidders were more profitable, this would be reflected in higher prices

public bidders pay higher prices for target firm assets (as shown by Bargeron, Schlingemann, Stulz, and Zutter (2008) for public targets), then more accounting goodwill is created in acquisitions by public firms, resulting in higher book value of assets of the combined firm. Holding cashflows constant, a larger denominator in ROA and ATO ratios leads to lower post-deal ROA and ATO of the combined firm, potentially underestimating performance improvements of public bidders. We examine transaction multiples (EV/Book, EV/Sales, EV/EBITDA) paid by private versus public bidders, and find that private bidders, indeed, pay lower prices for target firm assets. However, we show a similar private bidder effect on post-takeover performance when using changes in return on sales (Δ ROS) - a measure of performance improvement that is free from merger accounting effects. A third possibility we consider is that private firms are financially constrained and can only finance their best acquisition, whereas public firms can finance more marginal deals, resulting in lower average gains in profitability for public firms. However, we are able to rule this explanation out by showing that the private bidder effect is driven by firms that are characterized as *less* financially constrained.

While our matching-based and IV-based tests partly assuage concerns regarding endogeneity of a firm's listing status, we acknowledge a potential sample selection issue that remains. As noted at the outset, private firms in Capital IQ are not representative of a typical private firm in the economy. Therefore, our results are not immediately generalizable to the overall population of privately-owned companies. However, to the extent that lower agency conflicts is the channel behind the private bidder effect (as we have shown), a typical private firm exhibits even less separation of ownership and control than the private firms we study. We also note that data limitations preclude us from distinguishing between different types of private firm ownership (e.g. family-owned versus venture-capital-owned), meaning that our set of private firms likely exhibits considerable heterogeneity in terms of corporate

paid for those assets (holding risk constant). In fact, we find the opposite.

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governance arrangements, some of which may not be superior to those in public firms.

This paper contributes to the M&A literature by providing the first evidence on the performance of acquisitions made by private operating firms. Our results thus complement prior research that was limited to public acquirers.[5] Moreover, our findings help interpret some of the prior results in this literature. In particular, Bargeron, Schlingemann, Stulz, and Zutter (2008) show that private firms pay lower premia relative to public bidders – a result we confirm in a broader sample of deals using transaction multiples. There are two possibilities: either private firms are more disciplined due to better incentive alignment, or they simply enter deals with lower synergy gains that would naturally warrant lower prices. Our results on greater operating performance improvements suggest it is the former case, and further demonstrate that, not only do private bidders pay lower prices for target firm assets, they also operate those assets more efficiently. Finally, our paper contributes to the nascent literature that studies the characteristics of private firms (Brav (2009), Saunders and Steffen (2011), Michaely and Roberts (2012), Gao, Harford, and Li (2013), Asker, Farre-Mensa, and Ljungqvist (2015), Bernstein (2015), Sacchetto and Xiong (2018)). We expand this set of studies by providing new evidence on the effect of private ownership on post-acquisition performance, and, by extension, on the quality of private firms' investment decisions more broadly.

The rest of the paper proceeds as follows. Section 2 develops our hypotheses in light of related studies. Section 3 describes our sample. Our empirical analysis is presented in Sections 4 and 5. We consider alternative explanations in Section 6. Section 7 concludes the paper.

[5]The only exception is a study by Maksimovic, Phillips, and Yang (2013) who use plant-level data for U.S. manufacturing firms to study public and private firm participation in merger waves. They show, among others, that productivity gains (measured by total factor productivity) following plant acquisitions are greater when the buyer is public. Our results are not necessarily in conflict, because i) our sample is not limited to manufacturing firms, and ii) we measure efficiency gains as changes in overall operating profitability at the firm level, which takes account of various expenses not captured in total factor productivity.

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2 Hypotheses development and related studies

2.1 Hypotheses development

A large literature examines takeover gains to acquiring firms, though virtually all papers are limited to studying public acquirers and use abnormal stock returns to measure takeover gains (see Betton, Eckbo, and Thorburn (2008) and Mulherin, Netter, and Poulsen (2017) for summaries of this literature). In general, evidence on the ability of acquiring firms to generate value through takeovers has been mixed. Fuller, Netter, and Stegemoller (2002) study abnormal returns for public firms that acquired five or more targets within a three-year period, showing that public acquirers gain when buying a private or subsidiary firm, but lose or break-even when buying a public firm. In a sample of acquisitions by public firms from 1980 to 2001, Moeller, Schlingemann, and Stulz (2005) show that acquiring-firm shareholders lose $25.2 million on average upon announcement.

One of the main hypotheses put forward to explain lacklustre acquirer performance is agency-driven empire-building and overpayment. As public firms are subject to considerable separation of ownership and control, they suffer from agency costs of outside equity (Jensen and Meckling 1976), manifesting in poor acquisition decisions (Jensen 1986). For instance, Moeller, Schlingemann, and Stulz (2004) find that large public bidders generate lower announcement returns than smaller ones, which they attribute to greater agency costs at larger firms. Along similar lines, Masulis, Wang, and Xie (2007) show that poorly governed public bidders – as measured by their use of antitakeover provisions – exhibit lower returns than better governed bidders. Further, Harford (1999) shows that cash-rich public bidders are more likely to undertake value-destroying acquisitions.

In contrast, private firms exhibit higher levels of ownership by managers and higher levels of ownership concentration, aligning the interests of managers and shareholders and encouraging owners to more closely monitor management (Ang, Cole, and Lin 2000). For instance,

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Gao, Harford, and Li (2017), show that an average public firm in a sample similar to ours exhibits CEO ownership of 4.05% and ownership concentration by top 5 outside shareholders of 18.09%; for private firms these statistics are 10.74% and 49.32%, respectively. Sacchetto and Xiong (2018) quantify agency frictions for private and public firms using a structural estimation approach and find that large private firms face fewer agency problems than their public counterparts. If agency conflicts are one of the reasons behind poor performance of public acquirers, and if private firms face fewer such conflicts, we could expect private firms to generate greater efficiency gains from their acquisition activity. This leads to our main hypothesis.

H1: Private bidders generate greater acquisition-related efficiency gains than public bidders, ceteris paribus.

While the agency-based prediction is well-motivated theoretically, whether it holds true in the data remains an empirical question. This is because agency costs faced by public firms may be offset – or even outweighed – by benefits that are not available to private firms. Such benefits include easier access to capital, monitoring by analysts and the market for corporate control, learning from stock prices, and attracting better managerial talent. In addition, if concentrated shareholdings in private firms come at the expense of portfolio diversification, private firm managers may forgo profitable investment projects with high idiosyncratic risk. All of these circumstances may improve the investment opportunity set and decision-making at public firms vis-a-vis private companies.[6]

In light of this tension, we develop a secondary prediction designed to zero-in on the agency-based foundations of our main hypothesis. Since our premise is that a private firm

[6]Also, to the extent that private firms are not subject to the same capital market pressures emphasizing short-term profitability as public firms are, private firms are more likely to undertake deals that result in long-term value creation at the expense of immediate effects on earnings. At the same time, public firms may be coerced into deals that result in near-term improvements in profitability. If this is the case, our analysis focusing on the first three years following the deal could fail to detect greater operating performance changes for private bidders.

is a (crude) proxy for a better-governed firm, we further conjecture that the private bidder effect should be concentrated among private bidders that exhibit governance characteristics traditionally associated with good corporate governance – and not present for private bidders whose governance arrangements look like those of public firms. Insider ownership and ownership concentration are variables that have been traditionally linked to the extent of agency problems. This is because insider ownership aligns the interests of managers with those of outside shareholders (Jensen and Meckling 1976), and because concentrated holdings make monitoring efforts worthwhile (Shleifer and Vishny 1986). More recent studies also suggest that provisions limiting shareholder power – such as antitakeover defences – can further entrench managers and result in agency costs (Gompers, Ishii, and Metrick 2003, Bebchuk, Cohen, and Ferrell 2008, Cremers and Ferrell 2014). We therefore put forward the following secondary hypothesis.

H2: The private bidder effect (if any) is driven by firms with strong internal governance characteristics (e.g., insider ownership is high, ownership concentration is high, use of takeover defences is low).

2.2 Related studies

Our paper joins a small but growing literature that studies private companies. Sheen (2019) and Asker, Farre-Mensa, and Ljungqvist (2015) find that private firms invest more and are more responsive to investment opportunities. On the other hand, Gilje and Taillard (2016) examine a unique dataset of U.S. natural gas producers and show that investment by private firms reacts less to changes in natural gas prices. Brav (2009) and Saunders and Steffen (2011) investigate the financial policies of private and public firms in the U.K. and find that private firms face higher costs of external finance. Michaely and Roberts (2012) study dividend policies of public and private firms in the U.K. and find that private firms smooth dividends significantly less than public firms. Gao, Harford, and Li (2013) shows that private firms hold, on average, about half as much cash as public firms do.

In the voluminous M&A literature, only two papers have touched upon private acquirers. Bargeron, Schlingemann, Stulz, and Zutter (2008) investigate premiums paid in all-cash takeovers of U.S. public targets by private and public bidders from 1990 to 2005. They find that private equity bidders pay 63% lower premiums relative to public bidders, and that private operating companies (the focus of our paper) pay 14% lower premiums relative to public firms. Maksimovic, Phillips, and Yang (2013) study a sample of acquisitions by U.S. manufacturing firms using plant-level data from the Census Bureau. They find that gains in total factor productivity are greater when the buyer is a public firm.

3 Sample selection and basic results

3.1 The sample

Our primary data source is the Capital IQ database. Starting from the late-1990s, Capital IQ provides data on U.S. firms' M&A activity and financial information with a similar level of detail as provided by SDC Mergers and Acquisitions Database and Compustat for public firms. We start with U.S. public firms traded on the NYSE, Nasdaq, or Amex. A private firm is required not to have shares traded on any major stock exchange or OTC market. In the U.S., firms have to file financial reports with the Securities and Exchange Commission (SEC), if they have \$10 million or more in total assets and 500 or more shareholders (2,000 shareholders since April 2012), or if they list their securities with the SEC, such as public debt. Capital IQ collects private firms' financial data from the SEC through forms 10-K or S-1. In our final sample, data for most private firms (96%) come from 10-K reports, and the remainder (4%) comes from S-1 filings. Most private firms in the sample are large or have access to public debt. Although they are not representative of a typical private firm,

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this makes them comparable to public firms in terms of size, disclosure requirements, and information availability.

We collect a sample of U.S. mergers and acquisitions from Capital IQ. M&A data from Capital IQ, and in particular data on leveraged buyouts, have been used in a recent study by Axelson, Jenkinson, Stromberg, and Wesibach (2013). Following the literature, we collect all completed transactions for the period 1997 to 2014 (to allow for 3 years worth of post-acquisition performance data) in which the acquirer owns 100% of the shares of the target after the deal. We exclude all deals with non-operating targets, with missing deal values, and where the bidder is a group of investors. We further remove all regulated or financial bidders with SIC codes between 4900 and 4999 or between 6000 and 6999. Since our main variable of interest requires the operating performance before the deal to be available, we require all acquirers to have financial data in the year prior to the deal. Because a private bidder does not have publicly traded equity to offer, it is not surprising that most acquisitions by private bidders are cash deals. In the initial sample, more than 90% of acquisitions by private bidders are all-cash deals. In contrast, about 40% of public bidders use all-stock payment or mixed offers. To obtain a sample where deals are most comparable between public and private acquirers, we exclude all non-cash deals. Excluding non-cash deals results in a final sample of 8,803 deals where 7,458 deals involve a public bidder and 1,345 deals a private bidder, although the sample size varies across tests due to the availability of the relevant outcome and control variables.[7]

Table 1 reports the distribution of the number and the aggregate value of the transactions measured in 2009 purchasing power through time. In total, public firms participate more than private firms as buyers of assets in mergers and acquisitions. Among all deals, 85% of

[7]We have compared Capital IQ M&A data coverage with that of Thomson Reuters SDC. Applying the same sample selection criteria to both databases, we find that Capital IQ and SDC coverage of acquisitions by public bidders is very similar, but coverage of acquisitions by private bidders is significantly more comprehensive in Capital IQ.

the deals involve a public bidder, with 15% deals involving a private bidder.[8] In contrast, most target firms are private.

3.2 Descriptive statistics

We collect all financial performance measures and deal characteristics from Capital IQ. We focus on bidder and deal characteristics that both empirical and theoretical literature has found to be important. Panel A of Table 2 reports firm and deal characteristics for private acquirers and Panel B for public acquirers.[9] Variable definitions are given in Table A.1 in the Appendix. The first two variables are total assets and operating income measured in CPI-adjusted 2009 dollars. It is not surprising that private bidders are smaller than their public counterparts in total assets and operating income. We find that private acquirers have higher leverage than public acquirers. Consistent with Gao, Harford, and Li (2013), we also find that public bidders hold, on average, more cash than private bidders do. Private bidders tend to be younger firms and have fewer industry segments than public bidders. In addition, private bidders have, on average, more tangible assets, invest less in R&D, and exhibit higher sales growth. Mean dollar value of deals measured in CPI-adjusted 2009 dollars is around $240 million for both public and private bidders, and the median is around $30 million. Given that deal values are comparable across public and private bidders, but public bidders

[8]The share of private bidders declines significantly over the early sample years, which we believe has to do with our sampling procedure. Since private bidders conduct almost exclusively cash-based acquisitions (they have no publicly-traded equity to offer), our sample is restricted to all-cash deals. The period of 1998-2000 was a period of rising equity valuations (sometimes referred to as the dot-com bubble), and public companies were increasingly using their stock to make acquisitions (e.g., Fig. 1 in Golubov, Petmezas, and Travlos 2016). In addition, pooling of interests merger accounting method was eliminated in 2001. Pooling of interests was popular among bidders as it resulted in no goodwill creation, and one of the conditions for the use pooling-of-interests accounting was that the deal is a stock-for-stock transaction. Hence, rich equity valuations and the availability of pooling-of-interests accounting contributed to a large portion of public firm M&A deals being stock-financed. As stock-financed deals are excluded from our sample, this results in a greater fraction of private bidders in the early sample years as compared to later ones.

[9]It is interesting to also compare the characteristics of target firms. However, financial information for target firms is limited, because most targets are relatively small private firms that are not required to disclose to the SEC. Nevertheless, below we investigate target firm profitability in a subsample of deals.

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tend to be larger, relative deal size is greater for private bidders. The fraction of non-US targets is higher for public bidders, while the fraction of solicited deals is higher for private bidders. The fraction of targets from a two-digit SIC code other than that of the bidder is also somewhat higher for private bidders.

Finally, we compare our sample bidders to the full population of firms in Capital IQ (public and private, respectively). Table A.2 in the Appendix shows that, for both public and private companies, almost every firm characteristic is significantly different between bidders and the average firm. Typically, a bidder tends to be larger, older, has more industry segments, higher asset tangibility, and higher CAPEX than the average firm.

3.3 Basic univariate comparisons across bidder types

In this section, we examine post-acquisition operating performance changes for public and private bidders at the univariate level in the full sample. Our main measure of operating performance is return on assets (ROA): operating income before depreciation divided by total assets. Operating income captures the cashflows of the underlying business and is not affected by differences in capital structure, taxes, and depreciation policy. Scaling by total assets partially controls for divestitures and differences in growth and size. Broadly speaking, ROA can be interpreted as measuring the efficiency with which the acquiring firms use a given amount of assets, and changes in ROA can be interpreted as improvements in this efficiency. As an additional measure of efficiency, we look at asset turnover (ATO), defined as sales divided by total assets. This ratio captures the efficiency with which the firm is using its assets to generate revenue, and post-takeover changes measure improvements in productive asset utilization. We will also examine return on sales (ROS) in our later analysis.

Following Kaplan (1989) and Maksimovic, Phillips, and Yang (2013), we examine operating performance during the first three years after the deal. Specifically, we measure the change in the performance metric from the last year prior to deal completion (year -1) to

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years one, two, and three following the consummation of the deal. We scale this change by the absolute value of pre-deal performance to facilitate interpretation and to make economic magnitude of the results readily apparent. This is consistent with the literature on operating performance improvements following leveraged buyouts (e.g., Kaplan 1989 and Guo, Hotchkiss, and Song 2011).[10] We exclude year 0 (the year of completion) as those figures are difficult to interpret as pre- or post-deal performance. Furthermore, accounting measures in year 0 may be abnormal due to deal-related fees and asset write-ups. In all subsequent tests we trim the sample by removing the 5th and 95th percentiles of the dependent variable to reduce the influence of outliers.

The first panel of Table 3 reports raw (unadjusted), industry-adjusted, and control-firm-adjusted mean percentage changes in ROA and ATO for private bidders. Industry-adjusted and control-firm-adjusted measures attempt to provide a measure of abnormal performance changes. Industry-adjusted performance changes are net of the median performance change of the bidder's 2-digit SIC industry over the same period (bidding firms are purged from the computation of industry medians). Control-firm-adjusted performance changes are net of the contemporaneous performance change of a control firm chosen in year −1. The control firm is of the same listing status, comes from the same 2-digit SIC industry, and has the level of ROA in year −1 closest to that of the bidder (this is prior-performance-matching as recommended by Barber and Lyon (1996)). During the first three years, ROAs of private bidders improve by 7.96%, 7.44%, and 6.92%, all significantly different from zero. Turning to ATO, the improvements are 3.48%, 4.39%, and 5.14% in years one, two, and three, respectively. Using industry-adjusted and control-firm adjusted performance improvements, we continue to find that private bidders experience positive changes in ROA and ATO and the magnitudes are similar to the unadjusted values.

[10]Our conclusions are the same when using percentage point (unscaled) changes. See Table A.5 in the Appendix.

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The second panel of Table 3 reports the same outcomes for public bidders. On average, public bidders experience negative changes in ROA of −0.77%, −1.31%, and −2.07% in years +1, +2, +3 on an unadjusted basis, respectively. The same pattern is observed for ATO, where mean percentage changes are −1.61%, −2.31%, and −2.29%, in years one, two, and three, respectively. All of the changes are also significantly different from zero. Once again, industry-adjusting or control-firm adjusting performance improvements does not change the picture in most cases: on average, public bidders experience zero-to-negative changes in ROA and ATO following mergers. The only exception is the control-firm-adjusted change in ATO, which becomes positive in years two and three. The bottom panel of Table 3 reports differences between public and private firm changes in ROA and ATO. These differences are statistically significant across all years and performance measures. Overall, private bidders exhibit incremental 3–9% changes in ROA and ATO.[11]

We also investigate whether private firms exhibit higher changes in ROA and ATO *in general* – regardless of acquisition activity. However, we do not find this. These results are reported in the Appendix. For this analysis, we focus on the entire population of private firms in Capital IQ and use both the full sample and a matched sample of public firms. Following the literature such as Gao, Harford, and Li (2013) and Asker, Farre-Mensa, and Ljungqvist (2015), we match private and public firms with replacement based on size and industry. For each private firm, we select a matched public firm closest in size (total assets) from the same 2-digit SIC industry and year. If no match is found, we discard the observation from the sample. We then compare changes in operating performance between private and public firms one, two, and three years in the future. Table A.3 presents these results. With the exception of a negative difference in the change in ROA in year +3 relative to the overall population of public firms, private firms generally exhibit the same evolution of ROA and

[11]We also perform our tests (full sample comparisons and the matching estimator) using median changes in ROA and ATO. Our conclusions are unchanged. Please see Table A.6 in the Appendix.

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ATO. Hence, our results on superior operating performance changes for private bidders are likely attributable to their acquisitions.

While the initial evidence is consistent with our hypothesis *H1* that private bidders undertake better acquisitions, this full sample comparison is naive because it ignores the fact that being public or private is, likely, an endogenous decision. The listing status can be correlated with a variety of characteristics, thus affecting the evolution of firms' operating performance. Of particular concern is a variable that is positively correlated with the propensity to stay private and, at the same time, positively affects post-takeover operating performance changes.[12] In the next section we describe our approach to dealing with this identification concern and present our main results.

4 Main results

4.1 Empirical setup

We rely on state-of-the-art matching techniques as our main research design. For robustness we also consider an instrumental-variable (IV) approach.[13] The matching technique we use is a variable ratio (k:1) nearest neighbor matching (with replacement), whereby the nearest neighbors are identified based on a propensity score. Specifically, for each deal in the private bidder sample, we select up to 5 deals from the public bidder sample that are in the same industry, same year, and closest in the propensity of the bidder in question to be private. We then compare the outcomes for each private bidder deal to the outcomes of its matched

[12]Note that if the omitted variable correlated with the propensity to stay private negatively affects post-takeover performance, then this would bias our results downward, working against our finding of a positive private bidder effect. The typical narrative, whereby high quality firms/assets select into public status, fits this description - to the extent that asset quality is positively related to performance changes following takeovers, public firms would be expected to do better than private firms.

[13]Another potential (imperfect) solution could be to use within-firm variation in public/private status. Unfortunately, there is not enough firms in our sample that change listing status and conduct acquisitions both before and after the change.

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public bidder deal(s) only. We perform this comparison on both univariate and multivariate basis. The latter is known as further regression adjustment (or "double robustness") in the matching literature (see a review by Stuart 2010).[14]

We start with a probit regression where the private bidder indicator is the dependent variable and the explanatory variables are bidder characteristics as of year -1 relative to the deal. Specifically, we use the natural logarithm of revenue as a measure of size, the level of ROA and the change in ROA between year -1 and -2 as measures of prior performance, natural logarithm of firm age as a proxy for life cycle, as well as cash holdings, leverage, capital expenditures, asset tangibility, sales growth, number of segments, and R&D intensity. These variables are included because all of them exhibit statistically significant differences between public and private bidders as shown in Table 2. Industry (2-digit SIC) and year fixed effects are also included, because we select nearest neighbours conditional on the same industry and year and want the propensity score to be a function of residual differences in the covariates. We use the estimates from this probit regression to calculate bidding firms' propensity scores (i.e., the probability that the bidder is private, conditional on the covariates) and then match each private bidder transaction to up to 5 public bidder deals from the same industry and year by minimizing the absolute value of the differences in their propensity scores. The goal is to compare private bidders to public bidders from the same industry and year that were just as likely to be private given their observable characteristics.

Table 4 reports the results of our matching procedure. First, Panel A reports the propensity score estimation results. Most variables in the propensity score model are statistically significant predictors of a bidder's listing status. Smaller, better performing, and younger

[14] In terms of implementation, this estimator is obtained by regressing the outcome variable on the private bidder indicator (with or without controls) and a fixed effect that uniquely identifies each private bidder deal and its matched public bidder deal(s). Given that we use variable ratio matching (there can be between 1 and 5 public bidder control deals depending on availability), the estimation is weighted such that each private bidder deal receives the weight of one, and each public bidder deal receives the weight of $1/n$, where n is the number of public bidder control deals for a given private bidder deal.

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bidders are more likely to be private. Private bidders also hold less cash, are more levered, have fewer tangible assets and fewer segments. This mirrors the univariate differences observed in Table 2 (with the exception of tangibility, where the univariate difference was of the opposite sign). The pseudo-R^2 of the propensity score model is reasonably high at 34.4%. Panel A further reports diagnostics from our matching procedure, namely, mean differences in characteristics entering the propensity score estimation between private bidders and their propensity-score matched public counterparts. Only one covariate difference (prior profitability, ROA(-1)) is significantly different from zero, indicating that our matching procedure successfully eliminates virtually all observable differences that exist prior to matching.[15]

Panel B of Table 4 reports the distribution of the number of matches we obtain for each private bidder deal. In the 81.87% of cases we obtain 5 matches. Overall, our post-PSM sample contains 899 deals by private bidders and 4,080 deals by public bidders.

4.2 Baseline matching estimates

Table 5 reports the results of our main tests of hypothesis *H1*. Panel A reports the univariate difference in Δ ROA and Δ ATO around the acquisition between private bidders and their matched public bidders. We find that private bidders improve their ROA and ATO significantly more than their matched public bidders. The differences in operating performance changes between public and private bidders are all positive and statitically significant at the 1% level. Private bidders experience incremental Δ ROA of 7-11% and incremental Δ ATO of 5-6%. These magnitudes are comparable to those in the full sample analysis. It appears that selection on observable characteristics does not bias our results significantly.

[15]Note that testing for statistically significant differences as a matching diagnostic is too high a bar in large samples, because economically small differences can be precisely estimated when the number of observations is large. The matching diagnostic prescribed in the matching literature is the standardized mean difference (standardized by the standard deviation of the covariate in the treated population (private bidders)), which should be no greater than 0.25 (Stuart (2010)). This is the case for all of our covariates, including ROA(-1).

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Panel B of Table 5 performs further regression adjustment of these estimates by controlling for prior performance (the level of and the change in ROA prior to the deal), size ($Log(revenue)$), as well as additional bidder and deal characteristics found important by prior literature, namely a dummy for private targets, relative size of the deal (deal value to total assets) and its square, age of the bidder (in logs) and its acquisition experience (deal order), and dummies for hostile deals, solicited deals, diversifying deals, and cross-border deals. Stack fixed effects ensure that each private bidder is compared only to its own set of matched public bidders (as opposed to all public bidders). The coefficient on $PrivateBidder$ is of interest.

The estimation results confirm that on average private acquiring firms experience greater changes in profitability than public acquiring firms in terms of ROA. The coefficient on $PrivateBidder$, the indicator for whether the bidder is private, is positive and significant at the 1% level for all three post-takeover years. Private acquirers realize an incremental 7.3% increase in ROA during the year after the acquisition, 9.5% two years after the acquisition, and 6.7% three years after the acquisition compared to public acquirers. We also find that the coefficients on ROA(-1) and Δ ROA($-2, -1$) are negative and significant in all columns, implying a negative association between the bidder's pre-deal operating performance and subsequent changes.

Regression estimates for Δ ATO are similar. The specification is the same except that controls for prior performance measure prior level and growth in ATO instead of ROA. Again, we find that private acquirers realize greater improvements in ATO than public acquirers. The coefficients on $PrivateBidder$ are positive and statistically significant at the 1% level for all years. The incremental improvements in ATO are on the order of 5.2–6.3%. The coefficients on ATO(-1) are negative and significant in all specifications, consistent with the regression estimates using ROA as the performance measure. Across both ROA and ATO regressions, the coefficient on relative size is negative, suggesting that large deals are

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associated with lower changes in profitability, while the coefficient on deal order (experience) is generally positive and significant.

Overall, there is strong evidence that acquiring firm listing status is associated with post-takeover performance. This result holds after controlling for numerous potential confounding effects, such as differences in acquirer size, prior performance, growth opportunities (age) and acquisition experience, relative deal size, target type (private vs public target), and various deal types. So far our results are consistent with the notion that private bidders make better acquisition decisions, as predicted by hypothesis *H1*.

4.3 Instrumental variable approach

It is possible that, despite the matching process, there remains an *unobserved* characteristic that is positively correlated with both private firm status and operating performance changes following takeovers. To address this concern, we employ an instrumental variable (IV) approach as an alternative to our matching design. Here we borrow from Asker, Farre-Mensa, and Ljungqvist (2015), who compare investment behavior of public and private firms and instrument listing status with venture capital (VC) availability in the firm's headquarter state 2 years after foundation.[16] Specifically, the variable $VCsupply$ is the number of firms receiving first-round VC funding in the firm's headquarter state two years after the firm was founded, scaled by the number of firms in the state that were less than three years old at that time (VC data is from VentureExpert, and the number of firms less than three years old is from the Longitudinal Business Database of the U.S. Census Bureau). The instrument varies by state-year, and the intuition behind its relevance is straightforward: firms are more likely to have gone public at some point if they have received VC backing in their early years. This is because VC investors need an exit event to realize the value of their

[16]We thank John Asker, Joan Farre-Mensa, and Alexander Ljungqvist for making their instrument available to us.

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investment. Therefore, VC availability in the firm's geography two years after its foundation (typical firm age in first-round VC deals) should be positively associated with the likelihood that the firm has early VC investors, which, in turn, increases the probability of an eventual IPO. The exclusion criterion (the instrument must not affect the outcome variable of interest other than through its effect on the endogenous variable) should be satisfied by the virtue of time separation. That is, even if firms or VC investors were attracted to the particular geography by favorable economic conditions, many years have passed from that time until the measurement of our outcome variables, rendering any such correlation less relevant. The median age of our private firms at the time of the deal is 20 years, and the median for public firms is 30 years. Nevertheless, to the extent that the economic factors driving regional VC intensity are persistent, causing our instrument to be correlated with current economic conditions, the exclusion restriction will be violated.[17]

Given that our main endogenous variable is binary, we use a three-step approach described in Angrist and Pischke (2009) and used, for example, in Adams, Almeida, and Ferreira (2009). In the first step we estimate a probit model of a firm's listing status as a function of the instrument and other covariates. We then use the predicted probability from this probit regression as an instrument for the firm's listing status in the usual (linear) 2SLS model. The benefit of this approach is that it avoids the "forbidden regression" problem while allowing for a non-linear functional form in the association between early years VC availability and listing status (for a potential gain in efficiency).

Table 6 presents the results of our IV analysis (only the coefficients of interest are shown;

[17] To assess the severity of this concern, we examine the persistence of VC intensity over time. In particular, we sort states into quintiles based on their VC intensity in a given year, and then track the fraction of states that are still in the same quintile many years later. The results indicate that persistence in VC intensity at the state level is not particularly strong and decays substantially over time. For instance, while 65.25% of the states falling into the bottom quintile of VC intensity are still in the bottom quintile the following year, this fraction drops to 47% at t+10, to 41.6% at t+20, and 31.46% at t+30. Twenty and thirty year marks are of interest given that this is the average age of our private and public bidders, respectively. Persistence is somewhat stronger at the top of the distribution, with the one year out fraction of 65% falling to 55.9%, 52.35%, and 47.14% in years 10, 20, and 30, respectively.

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other covariates are identical to those used in our main regressions above). Panel A reports the first step probit model estimation. The relevance of venture capital availability at founding for a firm's listing status is evident: the $VCsupply$ variable is a strong negative predictor of a firm's private status many years later. The coefficient is statistically significant at the 1% level in all but the last specification (where it is significant at the 5% level). We obtain the predicted probability from this regression and use it as an instrument in a 2SLS model. Panel B reports the first stage, showing that the predicted probability of being private is a strong positive predictor for being a private firm. The F-test for the excluded instrument is significantly above 10, which is the recommended cut-off value for the case of one endogenous variable and one instrument (Staiger and Stock 1997). Panel C reports the second-stage estimation results. We find that the instrumented private bidder indicator continues to be positive and significant across all specifications. The fact that the effect is robust to instrumentation suggests that, subject to the exclusion restriction being satisfied, the private bidder effect is not picking up unobserved characteristics that are not a direct outcome of being a public versus a private firm. The magnitudes of the private bidder effect we obtain in this alternative identification approach are greater than those in our baseline matching approach. To remain conservative, we will use the matching approach in the remainder of our tests.

In the following sections we examine the hypothesized agency channel as well as the mechanism behind the private bidder effect. In other words, we ask *why* private bidders perform better than public bidders, and *how* they achieve that. In addition, we attempt to rule out possible alternative or mechanical explanations.

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5 The agency cost channel

Our results suggest that operating performance changes around acquisition deals are greater when the bidder is private than when the bidder is public. What is the reason for this outperformance? We have argued above that public ownership comes with greater agency conflicts relative to private ownership. We now investigate directly whether agency costs are behind the private bidder effect. In particular, we test hypothesis *H2*, which predicts that the private bidder effect is driven by private bidders with strong internal governance characteristics, such as high insider ownership, high ownership concentration, and few limits to shareholder power.

While firm-level data on governance arrangements in private firms are scarce, we are able to obtain three such variables, namely, CEO ownership, ownership concentration by top 1 outside shareholder, and a takeover defence score.[18] The latter variable comes from Capital IQ, while data on CEO ownership and ownership concentration come from Gao and Li (2015) and Gao, Harford, and Li (2017), which we further hand-collect for the most recent sample years.[19]

We begin by summarizing the four governance variables for public and private firms. For the sake of exposition these statistics are presented in Table A.4 of the Appendix. As expected, private firms exhibit significantly higher levels of CEO ownership (mean of 0.092 vs. 0.043), and ownership concentration by top 1 outside shareholder (mean of 0.462 vs. 0.112). In addition, the average takeover defence score for private firms is significantly lower than for public firms (0.237 vs. 0.320), indicating that private firms use fewer provisions

[18] Capital IQ covers 24 unique antitakeover and corporate governance provisions, from which it constructs a takeover defence score. In addition to standard antitakeover provisions such as poison pills and classified boards, this index captures such limitations/enhancements of shareholder rights as cumulative voting for board seats, causes for director removal, and limits to amend the corporate charter and bylaws, among others. The score is a number between 0 and 1, where a higher score indicates greater limitations to shareholder control. This takeover defence score is similar to corporate governance indices computed in Gompers, Ishii, and Metrick (2003) and Masulis, Wang, and Xie (2007).

[19] We would like to thank Huasheng Gao for kindly sharing these variables with us.

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limiting shareholder rights. Overall, these statistics are consistent with private firms having better incentive alignment between managers and shareholders, as well as monitoring by shareholders. *H2* predicts that the private bidder effect is most pronounced for private bidders characterized by stronger governance arrangements. To test this hypothesis, we split our private bidders into three subsets according to the level (high, medium, and low using tercile points of the distribution) of CEO ownership, ownership concentration by top 1 and top 5 outside shareholders, and takeover defense score. We then run subsample regressions, whereby we estimate the private bidder effect separately for each subset of private bidders.[20] Table 7 presents the results. Only the coefficient of interest is shown; regression specifications are the same as those in Table 5.

Panel A uses CEO Ownership as our first governance proxy. As predicted by the agency channel, the private bidder effect is concentrated in firms with high and medium CEO ownership. There is no positive private bidder effect when comparing public bidders to low CEO ownership private bidders. Panel B uses the concentration of ownership by the top 1 outside shareholder as our second governance characteristic. Once again, we find that the private bidder effect is driven by firms in the top tercile of ownership concentration by outside shareholders. This is despite a significant reduction in sample size in this panel (ownership concentration is available only after 2003). In Panel C we use the takeover defence score as our final internal governance proxy. The private bidder effect is driven by private firms with the lowest and medium level of takeover defence use. Overall, our results support *H2*: the private bidder effect is driven by private bidders that exhibit characteristics traditionally associated with low agency costs.

A question that still remains, though, is how exactly do private bidders achieve superior operating performance around acquisition events. In other words, while agency costs is the

[20]Note that we do not split the control group of public bidders associated with each private bidder. Our goal is to examine how different types of private bidders compare to their public bidder matches.

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channel behind the private bidder effect, what is the *mechanism* behind it? While we are limited in terms of data availability, we perform three tests designed to shed light on this question.

First, we consider whether private bidders are more likely to generate production cost efficiencies, as captured by changes in the ratio of cost of goods sold (COGS) to total assets. Second, we assess whether private bidders are more likely to find overhead savings, as proxied by changes in the ratio of selling, general, and administrative expenses (SG&A) to total assets. Third, we test whether private bidders are more likely to identify investment efficiencies, as proxied by the change in the ratio of capital expenditures (CAPEX) to total assets.

The results of this analysis are reported in Table 8. Private bidders experience greater reductions in SG&A expenses, but no significant differences in changes in COGS. Private bidders also experience greater reductions in CAPEX. Thus, it appears that the mechanism behind superior operating performance improvements by private bidders is better containment of overhead costs and greater investment efficiency. Overall, these mechanisms tie well with the agency cost channel that we document. We now consider whether alternative explanations can account for the private bidder effect.

6 Alternative explanations

6.1 Do private bidders buy more profitable targets?

So far we find higher changes in ROA and ATO for private bidders around takeover events. One possible explanation is that private acquirers simply pick targets with higher levels of, or growth rates in, operating performance. Note that we compare pre-deal operating results of the bidder with the post-deal operating results of the combined firm assets. To investigate this concern, we examine target firms' pre-deal performance. However, this analysis is limited

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to a subsample of target firms with financial information available from Capital IQ, because most target firms are private and small. As the overlap between the post-PSM sample and the sample for which target firm financials are available is too small to conduct meaningful tests, the analysis in this subsection is performed on the full sample.

We measure the level as well as the percentage change of the target firm's ROA and ATO in the last fiscal year prior to deal completion (relative to two years prior in the case of changes). Table 9 reports target's pre-deal performance. There are no discernible differences in levels of ROA and ATO (Panel A) and growth rates in ROA and ATO (Panel B) of the targets of public and private bidders.

Another way to assess whether targets of private bidders are more profitable is to examine prices paid for those assets. If targets acquired by private bidders are more profitable, one would expect higher prices paid for those assets (holding risk constant). Panel C examines mean and median transaction multiples paid by public and private bidders. We use deal value to total assets, deal value to sales, and deal value to operating income before depreciation. These multiples approximate price-to-book, EV/Sales and EV/EBITDA valuation multiples. We find that private bidders consistently pay lower prices for their targets: all transaction multiples are significantly lower for targets acquired by private firms. Panel D repeats this analysis in a regression framework with industry and year fixed effects to control for differences in the composition of deals in terms of industry and timing. Once again, we find that private bidders are paying lower transaction multiples. This result confirms the findings of Bargeron, Schlingemann, Stulz, and Zutter (2008) who find that private bidders pay lower bid premiums for comparable public targets. Overall, there is no evidence that targets of private bidders are more profitable, ruling this out as a possible explanation for better post-takeover performance of private firms.

Finally, Panel E conducts regression analysis of operating performance changes similar to that reported in Table 5 on a subsample of deals with target firm financials available.

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The difference is that we use the weighted-average performance of the bidder and the target in year $t-1$ in the computation of the dependent variable.[21] Only the coefficient of interest is reported. The sample size declines significantly to just over 1,000 observations (with only about 100 acquisitions by private firms), suggesting that power may be an issue. Nevertheless, we continue to find a positive and significant private bidder effect in 5 out of 6 specifications.

6.2 Merger accounting

Second potential explanation that we address has to do with merger accounting. Under U.S. Generally Accepted Accounting Principles (GAAP), the bidder has to account for the entire purchase price on its balance sheet. Any value in excess of the (stepped up) value of identifiable assets is recognized as goodwill.[22] If public bidders pay higher prices (as we have shown above), then more accounting goodwill is created, resulting in a higher accounting asset base for the combined firm. Since we measure ROA as the ratio of operating income to total assets, this can potentially explain why public acquirers have smaller post-deal ROA and the associated changes from before to after the deal. To mitigate this measurement concern, we use return on sales (ROS), as in the Custodio (2014) study of the diversification discount. Similar to ROA, we measure the annual percentage changes in ROS in the first three years following deal completion (years +1, +2, +3) relative to the most recent fiscal year prior to the deal. Panel A of Table 10 reports univariate comparisons between private bidders and matched public bidders, and Panel B reports the results of further regression adjustment. Our results continue to hold. Univariate differences in ROS changes between private and public bidders are all positive statistically significant. Similarly, the coefficients on the *PrivateBidder* indicator in Panel B are positive and significant at the 5% level for

[21]Given that we use the full (pre-PSM) sample in this analysis, stack fixed effects are replaced with industry and year fixed effects.

[22]This is also the case under International Financial Reporting Standards (IFRS).

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all windows. The magnitude of the effect is comparable to that in prior analysis using ROA and ATO, with 3-4% greater changes in profit margins for private bidders. Therefore, merger accounting effects cannot be the explanation behind better ROA and ATO changes for private bidders.

6.3 Access to capital

Another reason for better observed performance of private bidders could be the fact that they are more financially constrained. Specifically, if private bidders can finance only their best acquisition opportunity, whereas less constrained public bidders are able to finance more marginal deals, this would bring down the average post-takeover performance changes of public firms. Note that this would still imply that private firms make acquisitions with greater efficiency gains, but agency conflicts we allude to are not the reason behind it. Preliminary investigation of the data suggests that this is a valid concern: private bidders in our sample conduct an average of three acquisitions, while public firms conduct an average of five deals.

To formally test this explanation, we proxy for financing constraints with three different variables. First, we employ the SA index from Hadlock and Pierce (2010), who show that it performs better than the Kaplan-Zingales index (Lamont, Polk, and Saa-Requejo 2001) and the Whited-Wu index (Whited and Wu 2006).[23] The SA index is based on firm characteristics that predict actual qualitative assessments by management of their firms' ability to access capital. Hadlock and Pierce (2010) show that firm size, size-squared, age, leverage, and free cash flow are consistently associated with financing constraints. While leverage and free cash flow do incrementally predict the level of financing constraints (positively and negatively, respectively), Hadlock and Pierce (2010) choose to avoid these arguably more endogenous

[23]Besides, the computation of the Kaplan-Zingales and Whited-Wu indices require numerous financial variables that are often missing for private firms.

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variables in the construction of their index. We therefore use leverage and free cash flow separately as additional indicators of financing constraints. According to Hadlock and Pierce (2010), high levels of SA index, high leverage, and low free cash flow are symptomatic of high levels of financing constraints. If limited access to capital is the reason why private firms do better deals, we should find that the private bidder effect is driven by financially constrained private bidders.

Table 11 presents the results of our subsample analysis, whereby private bidders are split into low, medium, and high financing constraints based on tercile points of the distribution. Once again, we report only the coefficient of interest; all control variables are included. Panel A uses the SA index of Hadlock and Pierce (2010) as our first proxy for financing constraints. Interestingly, the private bidder effect is concentrated in private bidders with medium and low levels of SA index - opposite to what the access to capital explanation predicts. Panels B and C use free cash flow and leverage, respectively, as two additional proxies for financing constraints. Once again, we find results inconsistent with access to capital explanation of the private bidder effect: it is driven by private bidders with medium and high free cash flow, and with medium and low leverage (less constrained private bidders). Overall, it appears that more selective deal making as a result of greater challenges in accessing capital cannot explain the private bidder effect.

6.4 Subsequent listing and organizational form

Finally, successful acquirers may change their listing status after the acquisition. For example, private acquirers may choose to go public after their acquisitions. If so, greater performance improvements of private acquirers may be due to the IPO and the infusion of capital to fund growth and not from their acquisitions. In the sample, only 214 (15.9%) private acquirers go public within 3 years after the deal, and only 36 (0.4%) public acquirers go private within 3 years after the deal. We eliminate these bidders from the sample

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and rerun the regression adjustment tests. The results are shown in Panel A of Table 12. The coefficients on $PrivateBidder$ remain positive, with magnitudes and significance levels similar to those in prior analysis.

We further examine the organizational form of private bidders in our sample. First, we distinguish between independent private firms and those whose ultimate parent is a listed firm. We find that 23.8% of private bidders in our sample have public firms as their ultimate parents. We then examine whether these bidders perform any differently to independent private firms (one prediction could be that private firms whose ultimate parents are public may suffer from similar agency conflicts as their parents). Panel B of Table 12 reports the subsample analysis. With the exception of the change in ATO in years +1 and +2, the private bidder effect is observed only for independent private firms and not for private firms whose ultimate parent is public.

Finally, we also investigate whether the private bidder effect is driven by the private equity ownership model. Capital IQ provides information on whether the firm has received private equity sponsorship at any point in time. Similar to all of our tests above, we split private bidders into those that have never received private equity investment and those that did and perform subsample tests. Panel C of Table 12 reports the estimation results. While the subsample of non-PE backed private firms is small, we find that the private bidder effect is generally present in both subsamples, suggesting that the effect is common to the private ownership model more broadly.

7 Conclusion

Using a dataset covering both public and large private U.S. firms, we examine the effect of public versus private ownership on post-merger operating performance improvements. In particular, we test the hypothesis that acquisitions by private firms generate greater

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efficiency gains due to lower agency costs in private firms. Besides, private acquirers are of great interest in their own right, since virtually all existing evidence on acquirer performance is limited to public bidders.

We find that, on average, private acquirers experience greater operating performance changes following takeovers. Consistent with the agency cost channel, the effect is driven by private bidders with high CEO ownership and ownership concentration, and fewer limits to shareholder rights. We further examine the sources of superior operating performance changes in acquisitions by private bidders and find that they stem from better containing overhead costs and capital expenditures.

Overall, our evidence supports the view that private firms face fewer agency problems and make better investment decisions as a result. One limitation of our analysis is that we are not able to differentiate between various types of private firm ownership (e.g. family-owned versus venture-capital-controlled). The heterogeneity in governance arrangements across different types of private firms is an interesting topic for future research.

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Table 1: **Sample distribution by bidder type**

The table presents sample distribution by year and bidder type. The sample includes 8,803 completed cash-only mergers and acquisitions resulting in 100% ownership by the bidder announced between 1997 and 2014. Deal value is in CPI-adjusted 2009 millions of dollars. Data source: Capital IQ.

	All deals		Public bidders		Private bidders		Fraction of deals	
Year	n	Deal value ($m)	n	Deal value ($m)	n	Deal value ($m)	Private bidders	Private targets
1997	48	10,979	23	5,528	25	5,451	0.52	0.73
1998	153	53,868	110	39,244	43	14,624	0.28	0.75
1999	202	93,173	138	74,019	64	19,154	0.32	0.70
2000	304	140,923	239	107,839	65	33,084	0.21	0.85
2001	351	121,313	278	103,947	73	17,366	0.21	0.86
2002	345	52,141	284	49,334	61	2,807	0.18	0.91
2003	427	62,918	344	51,056	83	11,862	0.19	0.91
2004	520	136,207	443	68,687	77	67,520	0.15	0.93
2005	642	137,150	522	115,597	120	21,553	0.19	0.92
2006	701	187,830	587	166,579	114	21,251	0.16	0.92
2007	773	194,880	650	186,446	123	8,434	0.16	0.90
2008	711	127,481	636	113,017	75	14,464	0.11	0.92
2009	461	100,834	404	69,581	57	31,253	0.12	0.95
2010	748	124,948	663	119,439	85	5,509	0.11	0.92
2011	636	153,935	544	143,129	92	10,806	0.14	0.93
2012	622	129,409	554	117,654	68	11,755	0.11	0.93
2013	578	113,396	508	107,548	70	5,848	0.12	0.93
2014	581	137,591	531	132,173	50	5,418	0.09	0.95
Total	8,803	2,078,976	7,458	1,770,817	1,345	308,159	0.15	0.91

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Table 2: **Summary statistics on bidder and deal characteristics**

The table presents descriptive statistics for bidder and deal characteristics for a sample of 1,345 deals undertaken by private bidders and 7,458 deals undertaken by public bidders. Panel A reports private bidder characteristics and Panel B public bidder characteristics. Symbols ***, **, and * next to the means and medians indicate statistically significant differences between private and public bidders at the 1%, 5% and 10% levels, respectively. All variables are defined in the Appendix.

	Mean	Median	Std	p5	p25	p75	p95
			Panel A: Private bidders				
Total assets ($m)	4,306.490***	661.632***	12,394.703	14.826	211.290	1,879.355	22,912.148
Operating income	431.900***	70.189***	1,278.120	-2.130	24.656	177.849	2,122.430
Return on assets	0.204***	0.106***	0.396	-0.050	0.065	0.170	1.114
Δ ROA (−2, −1)	0.138*	0.027	0.726	-0.721	-0.214	0.252	1.623
Asset turnover	0.948***	0.751***	0.823	0.107	0.369	1.193	2.639
Δ ATO (−2, −1)	0.026	0.013***	0.341	-0.491	-0.131	0.115	0.671
Return on sales (ROS)	0.222***	0.166	0.314	-0.131	0.078	0.336	1.000
Leverage	0.425***	0.444***	0.331	0.000	0.141	0.610	1.046
Cash	0.097***	0.04***	0.148	0.000	0.012	0.104	0.461
Age	33.69***	20.000***	35.776	2.000	8.000	45.000	119.000
Segment	1.635***	1.000***	1.355	1.000	1.000	1.000	5.000
Tangibility	0.266***	0.189***	0.243	0.011	0.070	0.401	0.805
Capital expenditure	0.052*	0.026***	0.076	0.000	0.007	0.061	0.207
R&D	0.014***	0.000***	0.040	0.000	0.000	0.002	0.106
Sales growth	0.372***	0.129**	0.813	-0.153	0.030	0.379	1.821
Deal value	239.249	27.380***	1,895.975	0.995	7.819	103.211	760.234
Relative size	0.264***	0.057***	0.562	0.001	0.013	0.206	1.520
Private target	0.921	1.000	0.270	0.000	1.000	1.000	1.000
Non-US target	0.103***	0.000***	0.305	0.000	0.000	0.000	1.000
Hostile	0.001	0.000	0.027	0.000	0.0000	0.000	0.000
Solicited	0.097***	0.000***	0.297	0.000	0.000	0.000	1.000
Diversifying	0.303**	0.000**	0.460	0.000	0.000	1.000	1.000
Deal order	2.700***	1.000***	3.763	1.000	1.000	3.000	9.000
			Panel B: Public bidders				
Total assets ($m)	7,209.538	1,383.207	16,147.945	70.245	457.076	4,943.567	39,885.849
Operating income	1,241.960	177.179	3,234.455	3.697	50.923	649.235	7,674.837
Return on assets	0.150	0.136	0.089	0.027	0.090	0.196	0.324
Δ ROA (−2, −1)	0.102	0.017	0.528	-0.525	-0.125	0.178	1.054
Asset turnover	1.003	0.838	0.679	0.232	0.534	1.249	2.479
Δ ATO (−2, −1)	-0.001	-0.001	0.201	-0.362	-0.093	0.085	0.347
Return on sales (ROS)	0.207	0.164	0.159	0.020	0.093	0.294	0.532
Leverage	0.214	0.190	0.184	0.000	0.047	0.325	0.574
Cash	0.163	0.097	0.171	0.005	0.031	0.240	0.553
Age	47.585	32.000	39.851	6.000	17.000	73.000	130.000
Segment	3.318	3.000	1.701	1.000	3.000	4.000	6.000
Tangibility	0.224	0.145	0.216	0.022	0.069	0.302	0.746
Capital expenditure	0.045	0.030	0.047	0.005	0.016	0.055	0.146
R&D	0.031	0.003	0.046	0.000	0.000	0.049	0.135
Sales growth	0.164	0.114	0.272	-0.201	0.025	0.247	0.712
Deal value	237.426	36.255	916.374	1.636	10.634	137.346	911.990
Relative size	0.092	0.029	0.164	0.001	0.009	0.091	0.456
Private target	0.908	1.000	0.289	0.000	1.000	1.000	1.000
Non-US target	0.181	0.000	0.385	0.000	0.000	0.000	1.000
Hostile	0.001	0.000	0.031	0.000	0.000	0.000	0.000
Solicited	0.071	0.000	0.256	0.000	0.000	0.000	1.000
Diversifying	0.272	0.000	0.445	0.000	0.000	1.000	1.000
Deal order	5.360	3.000	5.485	1.000	2.000	7.000	17.000

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Table 3: **Operating performance changes around takeovers: full sample**

This table reports full sample comparisons of mean operating performance changes, Δ ROA(-1,$+j$) and Δ ATO(-1,$+j$) ($j = 1, 2, 3$), around acquisition deals undertaken by public and private bidders. Year -1 is the last fiscal year prior to deal completion. Year $+i$ is the ith fiscal year after deal completion. Industry-adjusted performance changes are net of the contemporaneous median performance change of all firms in the bidder's 2-digit SIC industry. Bidders are purged from the computation of industry medians. Control-firm-adjusted performance changes are net of the contemporaneous performance change of a control firm. Control firms with the level of pre-deal ROA closest to that of the bidder are selected from the same 2-digit SIC industry, year, and private/public type. Symbols ***, **, and * denote statistics that are significantly different from zero at the 1%, 5% and 10% levels, respectively. All variables are defined in the Appendix.

	From year i to year j		
	−1 to +1	−1 to +2	−1 to +3
		Private bidders	
Δ Return on assets (ROA)	0.0796***	0.0744***	0.0692***
Δ Asset turnover (ATO)	0.0348***	0.0439***	0.0514***
Industry-adjusted Δ ROA	0.0489***	0.0611***	0.0317***
Industry-adjusted Δ ATO	0.0231***	0.0298***	0.0407***
Control-firm-adjusted Δ ROA	0.0605***	0.0326***	0.0301***
Control-firm-adjusted Δ ATO	0.0343***	0.0529***	0.0767***
		Public bidders	
Δ Return on assets (ROA)	−0.0077**	−0.0131***	−0.0207***
Δ Return on assets (ATO)	−0.0161***	−0.0231***	−0.0229***
Industry-adjusted Δ ROA	−0.0138***	−0.0011	−0.0175***
Industry-adjusted Δ ATO	−0.0265***	−0.0242***	−0.0149***
Control-firm-adjusted Δ ROA	−0.0075***	−0.0101***	−0.0123***
Control-firm-adjusted Δ ATO	−0.0046*	0.0075**	0.0133***
		Private bidders − Public bidders	
Δ Return on assets (ROA)	0.0873***	0.0875***	0.0899***
Δ Asset turnover (ATO)	0.0509***	0.067***	0.0743***
Industry-adjusted Δ ROA	0.0627***	0.0622***	0.0492***
Industry-adjusted Δ ATO	0.0496***	0.0540***	0.0556***
Control-firm-adjusted Δ ROA	0.0680***	0.0427***	0.0424***
Control-firm-adjusted Δ ATO	0.0389***	0.0454***	0.0634***

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Table 4: **Matching private bidders to public bidders**

The table reports the details of propensity score matching private bidders to public bidders. The type of matching performed is variable ratio (k:1) nearest neighbor matching with replacement (k=1, ..., 5). Panel A reports the estimation results of the propensity score probit model. Industry (2-digit SIC) and year fixed effects are included. Panel A also reports post-matching covariate balance (mean differences and the associated t-statistics) for private bidders and their matched public bidders. Panel B reports the number of successfully matched private bidders and the distribution of the number of matches. Symbols *, **, *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. All variables are defined in the Appendix.

Panel A: Propensity score estimation and diagnostics

Dependent variable: *PrivateBidder* (probit)		Differences after PSM	t-stat
Log(revenue)	−0.031**	−0.0335	−1.61
	(0.016)		
ROA(-1)	0.571***	0.0303***	2.57
	(0.103)		
Δ ROA(-2,-1)	0.010	−0.0282	0.87
	(0.029)		
Log(age)	−0.081***	−0.0588	−1.48
	(0.025)		
Cash	−1.212***	−0.0049	−1.19
	(0.212)		
Leverage	1.855***	0.0088	0.36
	(0.110)		
Capital expenditure	0.390	−0.0023	−0.60
	(0.422)		
Tangibility	−0.560***	−0.0182	−1.19
	(0.167)		
Sales growth	0.010	−0.0738	−0.59
	(0.010)		
Segment	−0.388***	−0.0927	−1.31
	(0.025)		
R&D	0.482	−0.0006	−0.21
	(0.399)		
Industry FEs	Yes		
Year FEs	Yes		
Observations	8,214		
Pseudo R^2	0.344		

Panel B: Number of matches

Private bidder with	Number	Fraction
One public bidder match	52	5.78
Two public bidder matches	30	3.34
Three public bidder matches	36	4.00
Four public bidder matches	45	5.01
Five public bidder matches	736	81.87
Total	899	100.00

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Table 5: **Operating performance changes around takeovers: matching estimator**

The table reports comparisons of operating performance changes, Δ ROA(-1,$+j$) and Δ ATO(-1,$+j$) ($j = 1, 2, 3$), around acquisition deals between private and public bidders using a matching estimator. Panel A reports univariate differences. Panel B reports the results of further regression adjustment. The matching estimator is implemented by regressing the outcome variable on the private bidder indicator (with or without controls) and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. All variables are defined in the Appendix.

	Δ ROA			Δ ATO		
	(−1,+1)	(−1,+2)	(−1,+3)	(−1,+1)	(−1,+2)	(−1,+3)
			Panel A: Univariate comparison			
Private−public	0.0812***	0.1069***	0.0750***	0.0539***	0.0643***	0.0569***
			Panel B: Regression adjustment			
Private bidder	0.073***	0.095***	0.067***	0.053***	0.063***	0.052***
	(0.017)	(0.022)	(0.025)	(0.011)	(0.014)	(0.016)
ROA/ATO(−1)	−0.598***	−0.485***	−0.733***	−0.074***	−0.096***	−0.111***
	(0.109)	(0.181)	(0.247)	(0.007)	(0.012)	(0.011)
Δ ROA/ATO(−2,−1)	−2.090***	−1.624***	−1.516***	−0.023	−0.022	0.038**
	(0.302)	(0.286)	(0.245)	(0.019)	(0.022)	(0.018)
Log(revenue)	0.000	−0.002	0.012	−0.010***	0.000	−0.008
	(0.006)	(0.007)	(0.007)	(0.004)	(0.004)	(0.005)
Private target	−0.046*	−0.006	−0.013	−0.016	0.040**	−0.039*
	(0.024)	(0.036)	(0.032)	(0.017)	(0.017)	(0.020)
Non-US target	0.004	0.042*	0.016	0.014	0.002	0.005
	(0.021)	(0.024)	(0.024)	(0.014)	(0.018)	(0.017)
Relative size	−0.133**	−0.188**	−0.249***	−0.245***	−0.223***	−0.281***
	(0.058)	(0.073)	(0.074)	(0.045)	(0.046)	(0.050)
Squared relative size	−0.002***	−0.002***	−0.002***	−0.000***	−0.000	−0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(age)	−0.004	−0.013	−0.025**	0.001	−0.003	−0.001
	(0.009)	(0.011)	(0.011)	(0.006)	(0.007)	(0.008)
Hostile	0.287***	0.159**	0.076	0.123***	0.048	0.100
	(0.033)	(0.078)	(0.114)	(0.028)	(0.044)	(0.083)
Solicited	0.004	0.003	0.004	−0.018	0.026	0.027
	(0.032)	(0.033)	(0.031)	(0.020)	(0.024)	(0.020)
Diversifying	−0.023	0.011	0.001	0.008	−0.004	−0.001
	(0.016)	(0.021)	(0.022)	(0.011)	(0.014)	(0.015)
Deal order	0.002**	0.001	0.003**	0.002**	0.004***	0.003*
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)
Stack FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,276	4,226	4,165	4,336	4,252	4,230
Adj. R-squared	0.474	0.403	0.425	0.337	0.338	0.386

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Table 6: **Instrumenting listing status with VC availability at founding**

The table reports the results of instrumental variable (IV) analysis of operating performance changes, Δ ROA(-1,$+j$) or Δ ATO(-1,$+j$) ($j = 1, 2, 3$), around acquisitions deals for public and private bidders. Panel A reports estimation results of the first step probit regression of the *PrivateBidder* indicator on the instrument (*VCsupply at founding*) and all other covariates. Panel B reports estimation results of the first-stage regression of the 2SLS model, where the *PrivateBidder* indicator is regressed on the predicted probability from the probit model reported in Panel A and all other covariates. The F-test for the significance of the excluded instrument is also reported. Panel C reports estimation results of the second-stage regression of the main outcome variables on the instrumented private bidder indicator. Only the coefficients of interests are shown; other covariates in all three models are the same as those in Panel B of Table 5. Industry (2-digit SIC) and year fixed effects are included in all models. Standard errors clustered at the firm level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. All variables are defined in the Appendix.

Panel A: First step probit regression						
VCsupply at founding	−0.820***	−0.754***	−0.819***	−0.658***	−0.646***	−0.545**
	(0.249)	(0.251)	(0.263)	(0.238)	(0.240)	(0.232)
Pseudo R^2	0.219	0.217	0.215	0.211	0.209	0.212
Observations	4,357	4,333	4,247	4,221	4,120	4,076
Panel B: 2SLS first-stage regression						
Prob(Private bidder)	0.744***	0.788***	0.833***	0.804***	0.834***	0.996***
	(0.110)	(0.111)	(0.114)	(0.117)	(0.119)	(0.125)
F−stat	45.14	49.74	53.22	46.79	48.45	63.08
Observations	4,357	4,333	4,247	4,221	4,120	4,076
Panel C: 2SLS second-stage regression						
	Δ ROA			Δ ATO		
	(−1,+1)	(−1,+2)	(−1,+3)	(−1,+1)	(−1,+2)	(−1,+3)
Private bidder	0.537***	0.676***	0.418**	0.281***	0.257**	0.330**
	(0.185)	(0.222)	(0.191)	(0.099)	(0.117)	(0.135)
Observations	4,357	4,333	4,247	4,221	4,120	4,076
R-squared	0.143	0.064	0.172	0.082	0.136	0.116

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Table 7: **The Agency cost channel**

The table reports comparisons of operating performance changes, Δ ROA(-1,+j) and Δ ATO(-1,+j) ($j = 1, 2, 3$), around acquisition deals between private and public bidders using a matching estimator with further regression adjustment, conditional on the type of private bidder. The sample is subset into low, medium, and high according the value of the governance characteristic of the private bidder using terciles of the distribution. In Panel A the sub-setting variable is CEO ownership. In Panel B the sub-setting variable is ownership by top 1 outside shareholder. In Panel C the sub-setting variable is the takeover defence score. The matching estimator is implemented by regressing the outcome variable on the private bidder indicator, control variables, and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. Only the coefficients of interests are shown. The specifications are otherwise identical to those in Table 5. All variables are defined in the Appendix.

	Δ ROA			Δ ATO		
	(-1,+1)	(-1,+2)	(-1,+3)	(-1,+1)	(-1,+2)	(-1,+3)
Panel A: CEO ownership						
Low: Private bidder	-0.048	-0.057	-0.052	-0.053**	-0.059**	-0.076***
	(0.035)	(0.042)	(0.046)	(0.023)	(0.024)	(0.029)
Observations	1,125	1,122	1,109	1,106	1,103	1,084
Medium: Private bidder	0.100***	0.083**	0.064*	0.092***	0.101***	0.077**
	(0.031)	(0.035)	(0.033)	(0.021)	(0.024)	(0.031)
Observations	1,103	1,093	1,058	1,084	1,083	1,067
High: Private bidder	0.169***	0.136**	0.039	0.211***	0.339***	0.365***
	(0.044)	(0.059)	(0.064)	(0.029)	(0.040)	(0.047)
Observations	1,027	1,002	983	1,019	987	978
Panel B: Outside top1 ownership						
Low: Private bidder	0.063	0.031	0.004	0.033	0.016	0.025
	(0.047)	(0.071)	(0.069)	(0.037)	(0.035)	(0.044)
Observations	658	651	637	601	581	583
Medium: Private bidder	0.011	0.035	0.030	0.056**	0.049	0.093***
	(0.037)	(0.051)	(0.051)	(0.025)	(0.037)	(0.035)
Observations	617	609	616	582	586	595
High: Private bidder	0.107***	0.157***	0.208***	0.073***	0.132***	0.149***
	(0.038)	(0.040)	(0.068)	(0.028)	(0.033)	(0.047)
Observations	551	533	520	599	600	570
Panel C: Takeover defence score						
Low: Private bidder	0.055**	0.056*	0.054	0.056***	0.101***	0.030
	(0.025)	(0.031)	(0.039)	(0.020)	(0.025)	(0.027)
Observations	1,429	1,408	1,368	1,431	1,427	1,386
Medium: Private bidder	0.073**	0.129***	0.030	0.046**	0.041	0.069**
	(0.033)	(0.042)	(0.037)	(0.021)	(0.026)	(0.031)
Observations	1,325	1,294	1,293	1,304	1,275	1,262
High: Private bidder	0.023	-0.081**	-0.049	0.022	0.013	0.007
	(0.032)	(0.037)	(0.043)	(0.020)	(0.020)	(0.023)
Observations	1,211	1,205	1,193	1,178	1,181	1,181

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Table 8: **Sources of private bidder advantage**

The table reports full sample comparisons of, as well as the matching estimator of the difference in, percentage changes in SG&A, COGS, CAPEX (as a ratio of total assets) between public and private bidders. Year −1 is the last fiscal year prior to deal completion. Year $+i$ is the ith fiscal year after deal completion. The matching estimator is implemented by regressing the variable of interest on the private bidder indicator and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. All variables are defined in the Appendix.

	From year i to year j		
	−1 to +1	−1 to +2	−1 to +3
		Private bidder	
SG&A	−0.0105***	−0.0118***	−0.0142***
COGS	−0.0153***	−0.0189***	−0.0129***
CAPEX	−0.0035***	−0.0071***	−0.0110***
		Public bidder	
SG&A	−0.0058***	−0.0069***	−0.0068***
COGS	−0.0137***	−0.0213***	−0.0256***
CAPEX	−0.0011***	−0.0023***	−0.0032***
		Private bidder − public bidder	
SG&A	−0.0048***	−0.0050**	−0.0073***
COGS	−0.0016	0.0025	0.0127*
CAPEX	−0.0025***	−0.0048***	−0.0078***
		Private bidder − matched public bidder	
SG&A	−0.0037***	−0.0037***	−0.0059***
COGS	0.0077	0.0101	0.0005
CAPEX	−0.0019***	−0.0029***	−0.0041***

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Table 9: **Do private bidders buy more profitable targets?**

The table reports full sample comparisons of target profitability, transaction multiples, and operating performance changes, Δ ROA(-1,$+j$) and Δ ATO(-1,$+j$) ($j = 1, 2, 3$), around acquisition deals between public and private bidders. Panel A reports the mean target firm ROA and ATO one year prior to the deal. Panel B reports the mean percentage change in target firm ROA and ATO one year prior to the deal relative to the year before. Panel C reports mean and median transaction multiples (Deal value/Assets, Deal Value/Sales, and Deal value/Operating Income). Tests for differences are also shown. Panel D reports the coefficient of interest from regressions of transaction multiples on the *PrivateBidder* indicator and industry (2-digit SIC) and year fixed effects. Panel E reports the coefficient of interest from regressions of Δ ROA(-1,$+j$) and Δ ATO(-1,$+j$) ($j = 1, 2, 3$) on the *PrivateBidder* indicator, control variables (see Panel B of Table 5), and industry (2-digit SIC) and year fixed effects, except that the dependent variable is computed using the weighted-average performance of the bidder and the target in year $t-1$ (with total assets as weights). Standard errors clustered at the firm level are reported in parentheses. Symbols ***, **, and * denote the significant differences at the 1%, 5% and 10% levels, respectively.

Target's characteristics	Acquired by Private firms	Acquired by Public firms	Test of differences
	Panel A: Level		
Return on asset (ROA)	0.059	0.066	−0.007
Asset turnover (ATO)	1.568	1.409	0.159
	Panel B: Growth		
Δ Return on asset (ROA)	0.031	0.056	−0.025
Δ Asset turnover (ATO)	0.099	0.084	0.015
	Panel C: Prices paid		
Deal value/Assets			
Mean	1.896	2.701	−0.805***
Median	1.621	2.075	−0.454***
Deal value/Sales			
Mean	1.883	2.848	−0.965***
Median	1.477	1.898	−0.421***
Deal value/Operating Income			
Mean	9.973	13.218	−3.245**
Median	8.942	12.907	−3.965**

Panel D: Prices paid - regressions

	Deal value/Assets	Deal value/Sales	Deal value/OI
Private bidder	−0.503***	−0.759***	−4.408**
	(0.155)	(0.217)	(1.979)
Observations	1,212	1,214	1,216

Panel E: Changes in combined firm performance

	Δ ROA			Δ ATO		
	(−1,+1)	(−1,+2)	(−1,+3)	(−1,+1)	(−1,+2)	(−1,+3)
Private bidder	0.161**	0.065	0.193**	0.094***	0.106**	0.166***
	(0.068)	(0.067)	(0.081)	(0.033)	(0.044)	(0.047)
Observations	1,190	1,161	1,160	1,119	1,105	1,101

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Table 10: **Merger accounting? Changes in return on sales (ROS)**

The table reports comparisons of changes in return on sales, Δ ROS(-1,$+j$) ($j = 1, 2, 3$), around acquisition deals between private and public bidders in the full sample (Panel A) as well as using a matching estimator with and without further regression adjustment (Panels B and C). Year -1 is the last fiscal year prior to deal completion. Year $+i$ is the ith fiscal year after deal completion. Industry-adjusted performance changes are net of the contemporaneous median performance change of all firms in the bidder's 2-digit SIC industry. Bidders are purged from the computation of industry medians. Control-firm-adjusted performance changes are net of the contemporaneous performance change of a control firm. Control firms with the level of pre-deal ROA closest to that of the bidder are selected from the same 2-digit SIC industry, year, and private/public type. The matching estimator is implemented by regressing the outcome variable on the private bidder indicator (with or without controls) and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. Only the coefficients of interests are shown. The specifications are otherwise identical to those in Table 5. All variables are defined in the Appendix.

Panel A: Full sample comparisons

	From year i to year j		
	−1 to +1	−1 to +2	−1 to +3
		Private bidder	
Δ Return on sales (ROS)	0.0789***	0.0611***	0.0301**
Industry-adjusted Δ ROS	0.0541***	0.0432***	0.0191***
Control-firm-adjusted Δ ROS	0.0754***	0.0686***	0.0524***
		Public bidder	
Δ Return on sales (ROS)	0.0107***	−0.0110*	−0.0038
Industry-adjusted Δ ROS	−0.0141***	−0.0183***	−0.0293***
Control-firm-adjusted Δ ROS	0.0169**	0.0020	−0.0138**
		Private bidder − Public bidder	
Δ Return on sales (ROS)	0.0682***	0.0721***	0.0339***
Industry-adjusted Δ ROS	0.0682***	0.0615***	0.0484***
Control-firm-adjusted Δ ROS	0.0585***	0.0666***	0.0662***

Panel B: Matching estimator: univariate

	Δ ROS(−1,+1)	Δ ROS(−1,+2)	Δ ROS(−1,+3)
Private−public	0.0519***	0.0576***	0.0477***

Panel C: Matching estimator: regression adjustment

	Δ ROS(−1,+1)	Δ ROS(−1,+2)	Δ ROS(−1,+3)
Private bidder	0.034**	0.036**	0.035**
	(0.015)	(0.015)	(0.016)
Observations	4,356	4,252	4,249

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Table 11: **Access to capital**

The table reports comparisons of operating performance changes, Δ ROA($-1,+j$) and Δ ATO($-1,+j$) ($j = 1, 2, 3$), around acquisition deals between private and public bidders using a matching estimator with further regression adjustment, conditional on the type of private bidder. The sample is subset into low, medium, and high according the value of proxies for financing constraints of the private bidder using terciles of the distribution. In Panel A the sub-setting variable is the SA Index of Hadlock and Pierce (2010). In Panel B the sub-setting variable is free cash flow (FCF). In Panel C the sub-setting variable is leverage. The matching estimator is implemented by regressing the outcome variable on the private bidder indicator, control variables, and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. Only the coefficients of interests are shown. The specifications are otherwise identical to those in Table 5. All variables are defined in the Appendix.

	Δ ROA			Δ ATO		
	(−1,+1)	(−1,+2)	(−1,+3)	(−1,+1)	(−1,+2)	(−1,+3)
Panel A: SA Index						
Low SA index: Private bidder	0.071**	0.059	0.081*	0.059***	0.095***	0.109***
	(0.029)	(0.038)	(0.044)	(0.018)	(0.024)	(0.024)
Observations	1,458	1,437	1,426	1,461	1,442	1,439
Medium SA index: Private bidder	0.067**	0.127***	0.018	0.063***	0.078***	0.086***
	(0.029)	(0.038)	(0.040)	(0.019)	(0.024)	(0.031)
Observations	1,404	1,399	1,358	1,425	1,400	1,392
High SA index: Private bidder	0.040	0.000	-0.017	0.011	-0.005	-0.061*
	(0.038)	(0.043)	(0.045)	(0.024)	(0.030)	(0.035)
Observations	1,414	1,390	1,381	1,450	1,410	1,399
Panel B: Free Cash Flows (FCF)						
Low FCF: Private bidder	0.071**	0.021	0.044	0.013	-0.024	-0.040
	(0.028)	(0.037)	(0.046)	(0.022)	(0.026)	(0.032)
Observations	1,418	1,401	1,387	1,415	1,391	1,390
Medium FCF: Private bidder	0.068**	0.067*	0.002	0.075***	0.108***	0.102***
	(0.029)	(0.035)	(0.033)	(0.019)	(0.024)	(0.030)
Observations	1,414	1,401	1,372	1,472	1,422	1,406
High FCF: Private bidder	0.067*	0.148***	0.088*	0.067***	0.077***	0.077***
	(0.040)	(0.053)	(0.049)	(0.022)	(0.027)	(0.024)
Observations	1,444	1,424	1,406	1,449	1,439	1,434
Panel C: Leverage						
Low leverage: Private bidder	0.122***	0.093**	0.085*	0.095***	0.105***	0.126***
	(0.035)	(0.040)	(0.045)	(0.020)	(0.024)	(0.031)
Observations	1,435	1,439	1,408	1,418	1,415	1,390
Medium leverage: Private bidder	0.073***	0.135***	0.056	0.063***	0.079***	0.086***
	(0.027)	(0.039)	(0.038)	(0.019)	(0.023)	(0.025)
Observations	1,439	1,412	1,403	1,465	1,442	1,446
High leverage: Private bidder	0.056**	0.040	0.033	-0.013	-0.013	-0.059**
	(0.028)	(0.029)	(0.037)	(0.021)	(0.025)	(0.025)
Observations	1,402	1,375	1,354	1,453	1,395	1,394

Table 12: **Subsequent listing and organizational form**

The table reports comparisons of operating performance changes, Δ ROA$(-1,+j)$ and Δ ATO$(-1,+j)$ $(j = 1, 2, 3)$, around acquisition deals between private and public bidders using a matching estimator with further regression adjustment. In Panel A the sample excludes all bidders changing their listing status in the 3 years following the deal. In Panel B the sample is split according to whether the ultimate parent of the private bidder is public. In Panel C the sample is split according to whether the private bidder is currently or previously backed by a PE group. The matching estimator is implemented by regressing the outcome variable on the private bidder indicator, control variables, and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. Only the coefficients of interests are shown. The specifications are otherwise identical to those in Table 5. All variables are defined in the Appendix.

	Δ ROA			Δ ATO		
	(−1,+1)	(−1,+2)	(−1,+3)	(−1,+1)	(−1,+2)	(−1,+3)
Panel A: Firms not changing listing status following takeovers						
Private bidder	0.081***	0.101***	0.073***	0.071***	0.094***	0.091***
	(0.019)	(0.024)	(0.027)	(0.013)	(0.015)	(0.019)
Observations	3,600	3,560	3,501	3,655	3,575	3,551
Panel B: Public parent ownership of private bidders						
Public parent: Private bidder	0.046	0.068	0.019	0.056**	0.056*	-0.008
	(0.042)	(0.055)	(0.057)	(0.027)	(0.033)	(0.036)
Observations	1,080	1,061	1,022	1,081	1,058	1,046
No public parent: Private bidder	0.093***	0.090***	0.070***	0.052***	0.061***	0.061***
	(0.019)	(0.024)	(0.027)	(0.013)	(0.015)	(0.017)
Observations	3,196	3,165	3,143	3,255	3,194	3,184
Panel C: Private equity ownership of private bidders						
PE backed: Private bidder	0.084***	0.085***	0.066**	0.049***	0.063***	0.043**
	(0.020)	(0.025)	(0.028)	(0.013)	(0.015)	(0.017)
Observations	3,591	3,538	3,478	3,604	3,537	3,513
Non-PE backed: Private bidder	0.058	0.135*	0.045	0.070***	0.057	0.110**
	(0.053)	(0.081)	(0.065)	(0.024)	(0.037)	(0.045)
Observations	685	688	687	732	715	717

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A Appendix

Table A.1: **Variable definitions**

All variables are from Capital IQ unless otherwise noted.

Variable	Definition
Key dependent variables	
Δ ROA$(-1, +j)$	Percentage change in ROA margin, defined as ROA(+j) minus ROA(-1), scaled by the absolute value of ROA(-1), where year +j is the j'th year following the deal
Δ ATO$(-1, +j)$	Percentage change in ATO margin, defined as ATO(+j) minus ATO(-1), scaled by the absolute value of ATO(-1), where year +j is the j'th year following the deal
Δ ROS$(-1, +j)$	Percentage change in ROS margin, defined as ROS(+j) minus ROS(-1), scaled by the absolute value of ROS(-1), where year +j is the j'th year following the deal
Firm and deal characteristics	
Total assets	Total Assets from Capital IQ, reported in CPI-adjusted 2009 millions of dollars
Total revenue	Total revenue from Capital IQ, reported in CPI-adjusted 2009 millions of dollars
Operating income	Total Revenue less Cost of Goods Sold and Selling General & Admin Exp, reported in CPI-adjusted 2009 millions of dollars
Return on assets (ROA)	Operating income scaled by total assets
Asset turnover (ATO)	Total revenue scaled by total assets
Return on sales (ROS)	Operating income scaled by total revenue
Leverage	Long term debt scaled by total assets
Cash	Total Cash & shot-term investments scaled by total assets
Age	Firm's age since the year founded
Segment	Number of business segments
Tangibility	Net property, plant & equipment scaled by total assets
Capital expenditure	Capital expenditure scaled by total assets
R&D	R&D expenditure scaled by total assets
Sales growth	Annual increase in total revenue scaled by beginning-of-year total revenue
Deal value	Total transaction value, reported in CPI-adjusted 2009 millions of dollars
Relative size	Deal value scaled by Total Assets of the bidder
Private target	Indicator variable taking the value of one if the target firm is private, and zero otherwise
Non-US target	Indicator variable taking the value of one if the target firm is non-US, and zero otherwise

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Table A.1: **Variable definitions (continued)**

Variable	Definition
Hostile	Indicator variable taking the value of one if the deal is reported as hostile, and zero otherwise
Solicited	Indicator variable taking the value of one if the the deal is reported as solicited, and zero otherwise
Diversifying	Indicator variable taking the value of one if the bidder and the target do not share the same two-digit SIC code, and zero otherwise
Deal order	The number of deals conducted by the bidder up to that point
Financing constraints proxies	
SA Index	$(-0.737 \times Size) + (0.043 \times Size^2) - (0.040 \times Age)$, where Size is the log of book assets, and Age is the number of years from foundation. Size is capped at the log of \$4.5 billion, and age is capped at 37 years following Hadlock and Pierce (2010). Note: in Hadlock and Pierce (2010) age is measured as the number of years with non-missing stock price in Compustat; we replace this with the year of foundation since private firms do not have a stock listing.
Free cash flow (FCF)	Operating income minus interest minus tax minus dividends paid, scaled by total assets
Leverage	Book value of long term debt scaled by book value of total assets
Governance proxies	
Takeover Defence Score	Index of 24 corporate governance provisions, scaled to range from zero to one, with higher values indicating greater limits to shareholder rights
CEO Ownership	Fraction of company shares owned by the CEO (available from year 2000). Data from Huasheng Gao (NTU).
Outside Top1 Ownership	Fraction of company shares owned by the top 1 outside shareholder (available from year 2000). Data from Huasheng Gao (NTU).
Instrument for private status	
VCsupply at founding	Number of firms receiving first-round VC funding in the firm's headquarter state two years after the firm was founded, scaled by the number of firms in the state that were less than three years old at that time. VC data is from VenureExpert, and the number of firms less than three years old is from the Longitudinal Business Database of the U.S. Census Bureau. We obtain this variable directly from the authors of the Asker, Farre-Mensa, and Ljungqvist (2015) study.

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Table A.2: **Summary statistics on bidders vs. all firms in Capital IQ**

The table presents comparisons of firm characteristics between bidders and the universe of firms in Capital IQ. Panel A reports the statistics for private firms and Panel B for public firms. Symbols ***, **, and * next to the means and medians indicate statistically significant differences between private (public) bidders and the universe of private (public) firms at the 1%, 5% and 10% levels, respectively. All variables are defined in the Appendix.

	Bidders in Capital IQ					All Capital IQ firms				
	Mean	Median	Std	p25	p75	Mean	Median	Std	p25	p75
Panel A: Private firms										
Total assets ($m)	4306.490***	661.632***	12394.703	211.290	1879.355	1704.181	327.586	3721.354	51.318	1252.963
Operating income ($m)	431.900***	70.189***	1278.12	24.656	177.849	97.277	17.320	235.021	0.415	70.142
Return on assets	0.204***	0.106***	0.396	0.065	0.170	0.066	0.062	0.141	0.013	0.114
Asset turnover	0.948***	0.751***	0.823	0.369	1.193	1.279	0.986	1.070	0.534	1.645
Return on sales	0.222***	0.166***	0.314	0.078	0.336	0.075	0.066	0.150	0.015	0.138
Leverage	0.425***	0.444	0.331	0.141	0.610	0.471	0.436	0.379	0.163	0.680
Cash	0.097	0.040	0.148	0.012	0.104	0.102	0.041	0.148	0.013	0.117
Age	33.689*	20.000***	35.776	8.000	45.000	32.616	16.000	38.087	6.000	45.000
Segment	1.635***	1.000***	1.355	1.000	1.000	1.187	1.000	0.784	1.000	1.000
Tangibility	0.266***	0.189***	0.243	0.070	0.401	0.313	0.232	0.266	0.089	0.488
Capital expenditure	0.052**	0.026***	0.076	0.007	0.061	0.059	0.036	0.068	0.017	0.072
R&D	0.014***	0.000***	0.040	0.000	0.002	0.022	0.000	0.065	0.000	0.000
Sales growth	0.372***	0.129***	0.813	0.030	0.379	0.248	0.077	0.584	−0.008	0.266
Panel B: Public firms										
Total assets ($m)	7209.538***	1383.207***	16147.945	457.076	4943.567	3201.868	573.534	7383.611	148.173	2204.28
Operating income ($m)	1241.960***	177.179***	3234.455	50.923	649.235	327.296	45.943	833.882	5.152	207.000
Return on assets	0.150***	0.136***	0.089	0.090	0.196	0.083	0.086	0.099	0.037	0.138
Asset turnover	1.003***	0.838***	0.679	0.534	1.249	1.185	1.018	0.745	0.649	1.518
Return on sales	0.207***	0.164***	0.159	0.093	0.294	0.088	0.082	0.121	0.032	0.146
Leverage	0.214***	0.190***	0.184	0.047	0.325	0.201	0.170	0.188	0.019	0.316
Cash	0.163	0.097	0.171	0.031	0.240	0.165	0.097	0.176	0.029	0.242
Age	47.585	32.000***	39.851	17.000	73.000	47.300	33.000	37.471	19.000	67.000
Segment	3.318***	3.000***	1.701	3.000	4.000	2.963	3.000	1.969	1.000	4.000
Tangibility	0.224***	0.145***	0.216	0.069	0.302	0.255	0.188	0.217	0.086	0.362
Capital expenditure	0.045***	0.030***	0.047	0.016	0.055	0.053	0.035	0.053	0.018	0.066
R&D	0.031***	0.003***	0.046	0.000	0.049	0.027	0.000	0.051	0.000	0.034
Sales growth	0.164***	0.114***	0.272	0.025	0.247	0.127	0.083	0.255	−0.005	0.203

Table A.3: **Operating performance changes in the population of Capital IQ firms**

This table reports mean differences in operating performance changes (Δ ROA and Δ ATO) between private and public firms in the universe of Capital IQ firms, using both full sample and matched firm comparisons. Matched firms are closest in size (total assets) and come from the same industry (2-digit SIC code) and year. Year 0 represents current fiscal year and year $+i$ the ith year after. Symbols ***, **, and * denote statistical significance at the 1%, 5% and 10% levels, respectively.

	From year i to year j		
Percentage changes	0 to +1	0 to +2	0 to +3
Panel A: Private firms − Public firms			
Δ Return on assets (ROA)	0.036	−0.088	−0.148**
Δ Asset turnover (ATO)	0.013	−0.009	0.006
Panel B: Private firms − Matched public firms			
Δ Return on assets (ROA)	−0.004	−0.124	−0.161
Δ Asset turnover (ATO)	−0.007	0.018	0.024

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Table A.4: **Governance characteristics across private and public bidders**

The table presents descriptive statistics for firm-level governance characteristics of public and private bidders. CEO ownership is the fraction of company shares owned by the CEO (available from year 2000). Outside Top 1 Ownership is the fraction of company shares owned by the top 1 outside shareholder (available from year 2004). Takeover Defence Score is an index of 24 corporate governance provisions from Capital IQ, scaled to range from 0 to 1, with higher values indicating stronger limits to shareholder rights. Symbols *, **, and *** denote statistically significant differences between public and private bidders at the 10%, 5%, and 1% level, respectively.

	Private Bidders			Public Bidders		
	Mean	Median	Obs.	Mean	Median	Obs.
CEO Ownership	0.092	0.040	765	0.043***	0.006***	6,835
Outside Top1 Ownership	0.462	0.438	422	0.112***	0.084***	5,264
Takeover defence score	0.237	0.210	1,176	0.320***	0.310***	7,125

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Table A.5: **Analysis using percentage point changes**

The table reports comparisons of unscaled operating performance changes around acquisition deals between private and public bidders in the full sample (Panel A) as well as using a matching estimator with and without further regression adjustment (Panels B and C). Year -1 is the last fiscal year prior to deal completion. Year $+i$ is the ith fiscal year after deal completion. Industry-adjusted performance changes are net of the contemporaneous median performance change of all firms in the bidder's 2-digit SIC industry. Bidders are purged from the computation of industry medians. Control-firm-adjusted performance changes are net of the contemporaneous performance change of a control firm. Control firms with the level of pre-deal ROA closest to that of the bidder are selected from the same 2-digit SIC industry, year, and private/public type. The matching estimator is implemented by regressing the outcome variable on the private bidder indicator (with or without controls) and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. Only the coefficients of interests are shown. The specifications are otherwise identical to those in Table 5. All variables are defined in the Appendix.

Panel A: Full sample comparisons

	From year i to year j		
	-1 to $+1$	-1 to $+2$	-1 to $+3$
		Private bidder	
Δ Return on assets (ROA)	0.0079***	0.0054*	0.0027
Δ Asset turnover (ATO)	0.0099***	0.0049***	0.0032***
Industry-adjusted Δ ROA	0.0049***	0.0055***	-0.0023**
Industry-adjusted Δ ATO	0.0099***	0.0074***	-0.0014*
Control-firm-adjusted Δ ROA	0.0049	0.0082**	0.0091*
Control-firm-adjusted Δ ATO	0.0216*	0.0181*	0.0368**
		Public bidder	
Δ Return on assets (ROA)	-0.0116***	-0.0149***	-0.0189***
Δ Asset turnover (ATO)	-0.0107***	-0.0239***	-0.0327***
Industry-adjusted Δ ROA	-0.0123***	-0.0146***	-0.0171***
Industry-adjusted Δ ATO	-0.0219***	-0.0275***	-0.0221***
Control-firm-adjusted Δ ROA	-0.0107***	-0.0065***	-0.0029**
Control-firm-adjusted Δ ATO	-0.0177***	-0.0083**	0.0030
		Private bidder $-$ Public bidder	
Δ Return on assets (ROA)	0.0195***	0.0203***	0.0216***
Δ Asset turnover (ATO)	0.0206***	0.0288***	0.0359***
Industry-adjusted Δ ROA	0.0172***	0.0201***	0.0148***
Industry-adjusted Δ ATO	0.0318***	0.0349***	0.0207***
Control-firm-adjusted Δ ROA	0.0156***	0.0147***	0.0120*
Control-firm-adjusted Δ ATO	0.0393***	0.0264**	0.0338**

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Table A.5: **continued**

	Δ ROA			Δ ATO		
	(−1,+1)	(−1,+2)	(−1,+3)	(−1,+1)	(−1,+2)	(−1,+3)
Panel B: Matching estimator: univariate						
Private−public	0.0085***	0.0123***	0.0152***	0.0302***	0.0427***	0.0405***
Panel C: Matching estimator: regression adjustment						
Private bidder	0.005**	0.007**	0.003	0.028***	0.036***	0.029**
	(0.003)	(0.003)	(0.003)	(0.008)	(0.009)	(0.011)
Observations	4,298	4,282	4,176	4,341	4,296	4,238

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Table A.6: **Analysis using median percentage changes**

The table reports comparisons of median operating performance changes around acquisition deals between private and public bidders in the full sample (Panel A) as well as using a matching estimator with and without further regression adjustment (Panels B and C). Year −1 is the last fiscal year prior to deal completion. Year $+i$ is the ith fiscal year after deal completion. Industry-adjusted performance changes are net of the contemporaneous median performance change of all firms in the bidder's 2-digit SIC industry. Bidders are purged from the computation of industry medians. Control-firm-adjusted performance changes are net of the contemporaneous performance change of a control firm. Control firms with the level of pre-deal ROA closest to that of the bidder are selected from the same 2-digit SIC industry, year, and private/public type. The matching estimator is implemented by regressing (using quantile regressions estimated at the median) the outcome variable on the private bidder indicator (with or without controls) and a fixed effect that uniquely identifies each private bidder and its matched public bidder(s) (stack fixed effects). Each stack is weighted equally (private bidders receive the weight of 1, public bidders receive the weight of $1/n$, where n is the number of control public bidders in the stack). Standard errors clustered at the stack level are reported in parentheses. Symbols *, **, or *** denote statistical significance at the 10%, 5%, or 1% level, respectively. Only the coefficients of interests are shown. The specifications are otherwise identical to those in Table 5. All variables are defined in the Appendix.

Panel A: Full sample comparisons

	From year i to year j		
	−1 to +1	−1 to +2	−1 to +3
		Private bidder	
Δ Return on assets (ROA)	0.0106**	0.0092	−0.0121
Δ Asset turnover (ATO)	0.0099*	0.0144***	0.0081**
Industry-adjusted Δ ROA	0.0222*	0.0196*	0.0044**
Industry-adjusted Δ ATO	0.0086	0.0126**	0.0185***
Control-firm-adjusted Δ ROA	0.0193	0.0184	0.0173
Control-firm-adjusted Δ ATO	0.0227*	0.0195*	0.0581**
		Public bidder	
Δ Return on assets (ROA)	−0.0215***	−0.0382***	−0.0324***
Δ Asset turnover (ATO)	−0.0232***	−0.0224***	−0.0341***
Industry-adjusted Δ ROA	−0.0234***	−0.0161***	−0.0117***
Industry-adjusted Δ ATO	−0.0229***	−0.0187***	−0.0121**
Control-firm-adjusted Δ ROA	−0.0369***	−0.0112**	−0.0279
Control-firm-adjusted Δ ATO	−0.0076**	0.0049**	0.0075***
		Private bidder − Public bidder	
Δ Return on assets (ROA)	0.0321**	0.0474**	0.0203**
Δ Asset turnover (ATO)	0.0331***	0.0368***	0.0422**
Industry-adjusted Δ ROA	0.0456*	0.0357*	0.0161**
Industry-adjusted Δ ATO	0.0315***	0.0313***	0.0306***
Control-firm-adjusted Δ ROA	0.0562**	0.0296*	0.0452*
Control-firm-adjusted Δ ATO	0.0303*	0.0146*	0.0506**

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Table A.6: **continued**

	Δ ROA			Δ ATO		
	(−1,+1)	(−1,+2)	(−1,+3)	(−1,+1)	(−1,+2)	(−1,+3)
Panel B: Matching estimator: univariate						
Private−public	0.0315**	0.0811***	0.0353*	0.0318***	0.0530***	0.0412**
Panel C: Matching estimator: regression adjustment						
Private bidder	0.053**	0.060**	0.008	0.036***	0.033**	0.017
	(0.027)	(0.024)	(0.036)	(0.009)	(0.013)	(0.015)
Observations	4,276	4,226	4,165	4,336	4,252	4,230

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Value Creation in Private Equity * † ♠

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Value Creation in Private Equity

Abstract

We disentangle the effects of treatment, selection, and financial engineering on investor returns in private equity deals using a narrative approach for identification. Exploiting confidential textual data contained in pre-deal investment memos and value creation plans, we show that PE firms create value for investors by selecting companies that are about to outperform (akin to stock picking) and by helping portfolio companies improve production through CAPEX and acquisitions, but not by financial engineering. Tracking the post-investment implementation of value creation plans using confidential monitoring reports, we show that successful execution increases investor returns over and above the return-boosting effects of successful stock picking.

JEL Classification: G11, G24, G30, G32, L26.

Keywords: Private equity, value creation, treatment vs. selection, investor returns.

Private equity (PE) has grown into a sizeable asset class, with USD 7.1 trillion in worldwide assets under management in 2022.[1] Broadly speaking, prior research finds that investors have historically done well out of PE, earning returns after fees that exceed those available in the public equity markets.[2] In principle, a PE firm can create financial value for its investors in one of three ways. It can improve the operations and performance of its portfolio companies in ways that increase their value when eventually sold; we will call this treatment. It can invest in companies whose operations and performance are about to improve regardless of PE ownership; we will call this selection. Or it can use financial leverage to boost returns; we will call this financial engineering. Our aim in this paper is to disentangle the effects of treatment, selection, and financial engineering on investor returns in private equity deals.

While prior research finds that companies receiving PE investment undergo operational changes (e.g., in terms of employment, productivity, and product pricing), causally attributing operational changes to PE investment, and investor returns to operational changes, is methodologically challenging given that PE firms are unlikely to select their targets randomly. The current state of the art in the literature is to create potential counterfactuals for how portfolio companies would have performed absent treatment using matched firms without PE investment. This approach is subject to the limitation that PE firms likely select targets based on factors unobservable to the econometrician. Whether PE firms create value for their investors by improving the operations of their portfolio companies is thus an open question.

Important work by Acharya et al. (2013) typifies the state of the art in the literature on PE value creation. Using a sample of 395 deals by 48 European PE funds between 1991 and 2008, Acharya et al. show that portfolio companies grow sales and profitability faster than the median listed firm in the same sector during the holding period and that deal-level returns (measured four different ways) are higher the more sales and profitability have increased, even after stripping out the effects of financial engineering.

[1] McKinsey Global Private Markets Review 2023, exhibit 4, summing buyout, growth equity, and venture capital.
[2] See Ljungqvist and Richardson (2003), Kaplan and Schoar (2005), Kaplan and Strömberg (2009), Phalippou and Gottschalg (2009), Higson and Stucke (2013), Phalippou (2014), and Harris, Jenkinson, and Kaplan (2014), though whether PE returns beat public-market benchmarks when controlling for differences in risk, liquidity, and leverage remains an open question (Lerner and Schoar 2004, Cochrane 2005, Metrick and Yasuda 2007, Korteweg and Sørensen 2010, Franzoni, Nowak, and Phalippou 2012, Kleymenova, Talmor, and Vasvari 2012, Driessen, Lin, and Phalippou 2012, Axelson, Sørensen, and Strömberg 2013, Axelson et al. 2013, Ewens, Jones, and Rhodes-Kropf 2013, Sørensen, Wang, and Yang 2014, Jegadeesh, Kräussl, and Pollet 2015, Korteweg and Nagel 2016, Robinson and Sensoy 2016, Ang et al. 2018, Gredil, Sørensen, and Waller 2019, Gupta and van Nieuwerburgh 2021, Stafford 2022, and Boyer et al. 2023).

Figure 1 connects our data to this literature.[3] Using a sample of 1,580 deals made by 171 European PE funds between 1992 and 2017, Figure 1 shows that portfolio companies experience a range of improvements during the holding period: compared to propensity-score-matched control firms, portfolio companies invest more, increase capital intensity, grow net assets, make more acquisitions, increase labor productivity and employment, become more productive and more profitable, increase sales, sales growth, and market share, ramp up leverage, and streamline working capital. Figure 1 further shows that deal-level returns (measured the same four ways as in Acharya et al.) are higher the more sales, profitability, employment, and net assets have increased and the more acquisitive the portfolio company has been.

The big unanswered question for any study of PE value creation is whether the effects shown in Figure 1 reflect treatment (i.e., whether they are caused by a PE firm's actions during the holding period), or whether they instead reflect selection (i.e., that PE firms select targets whose performance was going to improve regardless). Our main innovation is to disentangle treatment and selection by taking a "narrative approach" to identification, following in the footsteps of Romer and Romer (1989).

We exploit unique confidential textual data on the value creation plans (or "playbooks") PE firms develop for their portfolio companies before they agree to invest, which we combine with detailed textual information on the subsequent execution of these value creation plans, open-source data on company-level operational changes over the investment period, and confidential data on realized deal-level investor returns. Because value creation plans are formulated *before* investment, we can investigate whether PE firms create value for investors through treatment or selection (or indeed both).

The key to our narrative identification strategy is that PE firms draft detailed pre-deal plans that focus on increasing a portfolio company's value in ways that would not have happened had the PE firm not invested in the company. For each deal, the playbook catalogues what changes are already in motion at a prospective portfolio company (which constitute part of the PE firm's unobserved selection criteria and may contribute to performance regardless of PE ownership) and what additional actions the PE firm plans to implement during the ownership period in order to increase the portfolio company's value further. In

[3] Full details can be found in Section IA.A in the Internet Appendix.

other words, given management's existing plan, each playbook encapsulates the PE firm's intended treatment effects in the form of an action plan. Our proprietary data allow us to observe these action plans, to track their implementation during the holding period, and to link them to observed outcomes.

If PE investment has a causal treatment effect on portfolio company performance, we expect improvements in outcomes to align with the specific action items included in the PE firm's playbook. To illustrate, suppose the PE firm plans to create value by pursuing add-on acquisitions while noting that the company has already identified a suitably qualified candidate to become its new CFO. If the company subsequently did grow through acquisitions over the holding period, that would be consistent with a PE treatment effect as plan and outcome would align. If the company subsequently did hire a new CFO, that would be consistent with a PE selection effect as the PE firm knew of management's plan to hire a CFO but had no plan of its own to focus on management changes. Our empirical specification exploits such variation in action plans across deals by relating a given outcome (say, "number of completed acquisitions" or "hire a CFO") to the presence or absence in the portfolio company's playbook of a relevant action item (say, "pursue add-on acquisitions" or "strengthen management").

Using an otherwise standard control-firm design to remove common trends at the country-by-industry level, our narrative approach shows that the observed company-level changes in Figure 1 (which uses the literature's workhorse model) reflect a mixture of selection and treatment effects. On average, companies whose playbooks involve *no* plans to reduce costs or to boost top-line growth nonetheless experience significant improvements over the holding period in labor productivity, employment, total factor productivity, and EBITDA on the one hand, and in sales, sales growth, and market share on the other (net of contemporaneous improvements at similar-sized control firms operating in the same country and industry). We interpret these findings as selection effects: our PE firms appear to target companies with efforts already under way to reduce costs and grow sales. Interestingly, PE firms *with* plans to focus on cost reductions and the top line achieve no better outcomes than PE firms *without* such plans. This suggests that PE firms struggle to add additional value over and above management's pre-existing plans in these dimensions. In summary, we find no evidence of treatment effects on the cost or growth sides, in contrast to prior studies that rely for identification on matching firms on observable characteristics.

Where we do find evidence of treatment effects is in a portfolio company's CAPEX and the reshuffling of its assets. Portfolio companies whose playbooks involve plans to improve operations by buying, upgrading, or selling assets or pursuing add-on acquisitions experience significant increases in net investment and engage in significantly more acquisitions and divestments than portfolio companies without such plans (net of contemporaneous changes at matched control firms). We interpret these findings as treatment effects: PE firms planning to engage in operational engineering of this sort on average successfully implement such plans. Taken together, our findings suggest that PE firms, in our sample, are good at reshaping portfolio companies through CAPEX and M&A transactions and less good at making portfolio companies more operationally efficient or helping them grow their sales.

Financial engineering, finally, has both a selection and a treatment component. Over the holding period, leverage and net debt to EBITDA increase significantly at portfolio companies whose playbooks involve no plans to improve tax efficiency, consistent with selection, and they increase significantly more at portfolio companies whose playbooks do involve such plans, consistent with treatment.

To what extent do PE firms' selection skills, their treatment activities, and their financial engineering contribute to investor returns? We approach this question in two ways. Armed with our classification of which of the observed portfolio-company-level changes reflect selection effects and which reflect treatment effects, we can take another look at Figure 1. In view of our finding that increases in EBITDA, employment, sales, and sales growth over the holding period reflect selection rather than treatment, we interpret the significantly positive relations in Figure 1 between deal-level returns and these changes in operations and performance as evidence of stock-picking skills: PE firms create financial value for their investors by selecting companies that are about to outperform their observably similar matched peers thanks to management plans to improve operations and performance that are already under way. In view of our finding that changes in CAPEX and the rate of acquisitions over the holding period reflect treatment rather than selection, we interpret the significantly positive relations in Figure 1 between returns and these activities as evidence of successful treatment: PE firms create financial value for their investors by helping their portfolio companies invest in their production facilities. Finally, in view of our finding that financial engineering has no effect on returns, we conclude that PE firms do not create financial value for their investors by financial engineering.

Our second approach takes a top-down approach. Our confidential textual data allow us to track the implementation of each playbook by observing which action items are achieved during the holding period. If PE firms' treatment activities causally increase returns over and above the return-boosting effects of selection, we expect the return on a deal to be higher the more of its plan the PE firm has managed to implement during the holding period, all else equal. If instead returns are driven mainly by selection, we expect deal-level returns to be largely unrelated to plan implementation. The main empirical challenge with this test is holding all else equal. The chief concern is that benign market environments facilitate execution while simultaneously boosting returns for reasons unrelated to a PE firm's treatment activities. While we find no evidence that plan achievement is driven by anything other than factors that are idiosyncratic to the deal and the PE firm, we nonetheless control for a deal's market environment. Holding constant growth in GDP, the return on the local stock market index, and growth in the portfolio company's industry, we find support for the prediction that deal-level returns are higher the more fully a PE firm has managed to implement its value creation plan. In other words, successful execution drives returns over and above the return-boosting effects of successful stock picking.

Our contribution to the literature on PE value creation is threefold. First, we contribute to the extensive and growing literature on the changes companies undergo while owned by a PE firm.[4] As

[4] A consistent finding from matched-control-firm studies is that companies increase sales, profitability, and leverage and reduce employment while under PE ownership (see Acharya et al. 2013 for pan-European evidence, Bharat et al. 2014, Kaplan 1989a, 1989b, Guo, Hotchkiss, and Song 2011, and Davis et al. 2014 for the U.S., Antoni, Maug, and Obernberger 2019 for Germany, and Boucly, Sraer, and Thesmar 2011 for France.). PE firms improve portfolio companies' productivity by increasing CAPEX (Boucly, Sraer, and Thesmar) and reallocating resources to more productive plants amid net job destruction (Davis et al. 2014, though Bharath, Dittmar, and Sivadasan 2014 disagree). Studies of individual industries go beyond accounting data to focus on detailed operational metrics. Fracassi, Previtero, and Sheen (2022) find that consumer-goods companies acquired by PE firms raise prices only marginally on existing products, introduce new products, and increase variety. Results on the effects of PE investment on quality indicators are mixed. On the bright side, Cohn, Nestoriak, and Wardlaw (2021) report that PE investment improves workplace safety, Bernstein and Sheen (2016) report that health inspection records improve at restaurant chains that receive PE investment, and Gao, Sevilir, and Kim (2021) report that PE acquirers improve hospitals' operational efficiency without compromising healthcare quality. On the dark side, Eaton, Howell, and Yannelis (2020) find that PE investment reduces graduation rates and future earnings among graduates of higher education institutions acquired by PE firms, Gupta et al. (2021) find that PE investment is associated with higher mortality rates among Medicare patients in nursing homes, and Ewens, Gupta, and Howell (2022) find that PE firms' acquisitions of local newspapers reduce voter turnout in local elections.

noted, the key empirical challenge in this literature stems from the fact that PE firms select targets based on factors that are likely unobservable to the econometrician and that may independently affect companies during the holding period. Whether the many changes previously attributed to PE ownership are caused by PE ownership or whether they instead would have happened anyway is an open question. Using Romer and Romer's (1989) narrative approach to disentangle selection and treatment effects on companies' operations and performance, we show that many of the changes previously attributed to PE ownership—notably growth in sales, cost reductions, and increases in profitability—would have happened anyway. They thus more plausibly reflect selection than treatment, at least in our sample.

Second, we contribute to the literature on the drivers of deal-level investor returns in PE (Cochrane 2005, Kaplan and Schoar 2005, Braun, Jenkinson, and Stoff 2017, among others). We believe we are the first to show that PE firms causally create financial value for their investors through both stock picking (selecting companies that are at inflection points in their performance relative to observably similar companies) and their involvement in their portfolio companies (treatment), but not through financial engineering. Specifically, investor returns are higher when companies selected on their plans to improve profitability through a focus on employment and sales deliver on their plans and when their PE backers succeed in helping them invest in their production facilities through CAPEX and acquisitions.

Beyond speaking to the age-old question of selection vs. treatment, we identify a new driver of deal-level returns: the successful execution of value creation plans. Thanks to our access to both pre-investment plans and post-investment reporting on the achievement of these plans, we show that investor returns increase in the extent to which PE firms manage to implement their playbooks. This new finding has a potentially important implication for investors in PE funds: in addition to screening on past performance, fund strategy, and the reputation of a fund's principal dealmakers, investors should consider the PE firm's track record of successful execution of their value creation plans in past funds.

Third, we contribute to a small but important literature on value creation plans. Kaplan and Strömberg (2009) are the first to note the existence of such plans. Because plans are confidential, there is no systematic evidence on what they look like or whether they help improve performance or investor returns. Existing studies rely instead on surveys. A survey of 79 PE firms by Gompers, Kaplan, and Mukharlyamov (2016) finds that PE firms focus their value creation activities on increasing growth rather than reducing costs. Our analysis of 1,136 value creation plans suggests instead that operational engineering is as popular in our sample as top-line growth activities. A survey of institutional investors by Gompers et al. (2020) reports that while deal sourcing, deal selection, and post-investment value-added are all believed to contribute to returns, investors believe that selection is the most important driver. Our empirical finding that treatment and selection both contribute to returns agrees with the former belief, while our finding that successful execution boosts returns provides nuance to the latter.

1. Identification strategy, sample, and data

1.1 Narrative identification

We take a "narrative approach" (Romer and Romer 1989) to identifying the effects of PE ownership on portfolio companies and investor returns. The narrative approach seeks to establish a causal effect of a policy or intervention on an outcome of interest. As Romer and Romer (2023) explain, the narrative approach is "an empirical technique where one gathers systematic evidence from contemporaneous qualitative sources (such as newspapers, government reports, and policy meeting transcripts), and incorporates it into statistical analysis."[5] The original motivation for the technique is to deal with omitted variables that may affect policy and outcomes of interest at the same time.[6] In the PE setting, the main identification concern is the likely non-random selection of portfolio companies based on largely unobserved criteria that may correlate with outcomes of interest independently of PE ownership. Our implementation of the narrative approach reveals these otherwise unobserved criteria, allowing us to focus on PE firms' value-adding treatment activities.

To illustrate the identification challenge, suppose PE firms tend to invest in companies that are about to experience high growth as a result of changes already initiated by the existing managers. (Table IA.A2

[5] See Romer and Romer (2023) for examples of using the narrative approach to achieve identification in macroeconomics. In finance, Campello, Gao, and Xu (2023) use the narrative approach to establish how state-level reductions in personal income taxes causally drive firms to increase their skill requirements when hiring workers.

[6] Romer and Romer (1989) use Federal Reserve records to identify a subset of monetary policy actions that were not motivated by output or other factors affecting output. Such actions constitute policy "shocks" that "should give relatively unbiased estimates of the causal impact of monetary policy." Campello, Gao, and Xu (2023) collect local narratives from politicians, journalists, and policy analysts to infer the underlying motives for changes in state personal income taxes.

in the Internet Appendix reports evidence consistent with such behavior.) Suppose further that PE firms add no value during the ownership period. A finding that portfolio companies experience high growth under PE ownership, even when compared to matched control firms as in prior literature, then reflects a pure selection effect rather than a PE value-adding treatment effect.

Our narrative approach relies on detailed textual records in the form of PE firms' confidential pre-investment playbooks to disentangle PE treatment effects from PE selection effects. Our approach exploits the fact that PE firms routinely spell out, in their internal documents, what changes are already in motion at a prospective portfolio company (which constitute part of the selection criteria that are unobservable to outsiders and which may contribute to performance regardless of PE ownership) and what changes the PE firm intends to catalyze during the ownership period in order to increase the portfolio company's value further.

The following example illustrates changes that are already in motion and so predate PE ownership:

> *"Management is currently concentrating on increasing prices as well as rationalizing procurement to boost profitability. Additionally they are putting in place open-ended contracts to be able to increase prices in line with material increases in the cost structure. Management's key target for [Year] is to return to business plan, through the price increases implemented in [Year], improving the margins through procurement of raw materials and changes in the product mix, while rationalizing customer contacts and optimizing internal stocks."*

Presumably, management's pre-existing plans made the target company attractive to the PE firm in the first place and so form part of the unobserved selection process. Were management's pre-existing plans to bear fruit, they would affect the portfolio company's performance through a PE selection effect rather than through a PE treatment effect. Any potential PE treatment effect would instead result from the successful implementation of actions that the PE firm plans to take during the ownership period and that would not have happened otherwise. The following example illustrates a typical PE action plan:

> *"[The PE firm] will enable [PortCo] to take the step from being a largely management-owned one-country business to a leading regional player in its market. [The PE firm]'s main value added is expected from new motivation tools for management, financial strategy, financing, and know-how in add-on acquisitions. Key managers will be offered a co-investment opportunity and/or bonus structure based on both [PortCo]'s results and success of the buy & build strategy. [The PE firm] will act as an active owner in the following ways: 1. work at the supervisory board level of [PortCo] (two seats or more); 2. actively support management in add-on acquisition activities (evaluating targets, negotiating with owners, using contact networks in [Country X] and [Country Y]; 3. arrange the most appropriate financing for add-on acquisitions; 4. develop and see through a 100-day plan, including …"*

The key to our narrative identification strategy is that PE firms will focus their ownership-period efforts on increasing the portfolio company's value in ways that would not have happened had the PE firm not invested in the company. After all, there is no point wasting time and effort on performance improvements that are already underway (and that drive selection). PE firms thus draft playbooks that focus on actions they think will create additional value beyond the value management are already creating on their own. Our proprietary data allow us to observe these action plans, to track their implementation, and to link them to observed outcomes.

If PE ownership truly has beneficial effects on portfolio company performance, we expect to observe improvements in outcomes that align with the specific action items included in PE firms' playbooks. Crucially, to be attributable to PE firms' value-adding treatment activities, such improvements need to exceed any improvements observed at portfolio companies whose playbooks do not target the outcome in question (such as improvements driven by pre-existing management plans).

1.2 Implementation

Our implementation of the narrative approach satisfies Romer and Romer's (2023) four requirements for rigorous narrative analysis. The first requirement is a "reliable narrative source" that is contemporaneous, consistent over time, and accurate. For each deal, our textual sources consist of pre-investment playbooks and post-investment quarterly reports. Playbooks are contained in internal pre-deal investment memos and investment-committee presentations. Importantly, playbooks are written before an investment is made. This ensures that subsequent outcomes cannot affect how the PE firm describes its value creation plan for the deal. Post-investment reports are standardized, which helps us track how playbooks are implemented or revised over time in a consistent and objective manner.

The second requirement is a "clear sense of what one is looking for in the source." Romer and Romer (2023) recommend searching for information that can aid in establishing causation. In our context, this involves capturing the action items the PE firm plans to implement to affect a change in an outcome over the holding period, and equally importantly, the action items that are not included in the PE firm's playbook.

The third requirement is to "approach the narrative source dispassionately and consistently" to "resist temptation to see what you want to see in the narrative source." Romer and Romer (2023) suggest that each document is read by multiple readers from beginning to end. To this end, we have two of us independently read each playbook and each post-investment report, recording each individual action item. Areas of disagreement between the two readers are reconciled based on a third reading of the source material. This process takes on average three hours per deal or 3,400 man-hours in total.[7]

The fourth requirement is to "document the narrative evidence carefully," to ensure transparency and replicability. We implement this requirement by recording quotes from the source documents to support how we code each action item.[8]

1.3 Sample and data

The unique advantage of our data is that we have access to detailed confidential value creation plans for a large set of deals through an anchor investor that has invested in 171 emerging-market PE funds raised between 1992 and 2017 that in turn have invested in 1,580 portfolio companies. In addition to textual data gathered from the value creation plans, we have access to confidential quarterly post-investment "monitoring reports" that allow us to observe the implementation of, or changes to, each playbook over time. We combine these rich textual data with open-source company-level accounting data and confidential deal-level investor returns, allowing us to study both company-level operational changes and deal-level performance.

Our data cover a 26-year period in 20 emerging markets. The data come from the European Bank for Reconstruction and Development (EBRD). The EBRD is among the largest investors in PE funds that operate in emerging markets. As part of its mandate, the EBRD seeks to contribute to the development of the PE industry in its region, which spans Central, Eastern, and Southern Europe, the Baltics, the Commonwealth of Independent States, and the Middle East and North Africa. Since it started operations

[7] Our sample is sufficiently small so that we can use human readers rather than relying on necessarily noisier natural language programming and machine learning tools to convert text into data.

[8] To illustrate, suppose the playbook reads: "*[We plan] to grow revenues in three ways: price management, international expansion, and revenue from additional services. Prices can be increased by an additional 15% or more per year in the coming several years in [Country].*" We then code the playbook as including action items "change pricing strategy," "pursue international expansion," and "change product/services mix." Other action items are coded as 0. We then track the implementation of each action item during the ownership phase. If a monitoring report states that "*In January 2008, [PortCo] increased prices significantly, almost doubling the basic service fee from schools*," we code action item "change pricing strategy" as achieved.

in 1991, the EBRD has committed USD 5.165 billion to PE funds targeting its region (as of December 2017). Given the coverage and the obligatory reporting demanded by the EBRD, our data do not suffer a survivor bias resulting from, say, only the best or only the largest fund managers contributing data.

Our dataset extends the sample used in Cornelli, Kominek, and Ljungqvist (2013). Our 171 sample funds raised an average (median) of USD 168.0 million (USD 93.6 million) which they invested in an average (median) of 9.2 (9) deals per fund. Table 1, Panel A provides an overview of our sample by country and time period. The top five countries are (in descending order) Russia, Poland, the Czech Republic, Romania, and Turkey, which together account for two-thirds of the sample by number of deals. Deal activity has varied over time, with the busiest five-year periods being 1997-2001 (501 deals) and 2012-2017 (401 deals).

Based on our reading of PE firms' pre-deal investment memos, we group deals into five types: early-stage, growth, buyout, secondaries, and turnarounds. Early-stage deals can be thought of as traditional venture capital deals, involving startups, pre-revenue companies, and pre-profit companies. Growth deals typically involve external financing (but not outright acquisition) of companies with growing sales and profits. Buyouts usually involve acquisition (or at least majority control) of mature companies with relatively stable cash flows, such as a division of a larger firm or a stock market listed company. In a secondary deal, one PE firm acquires the portfolio company of another PE firm. Secondaries are more common in growth equity and buyouts than in early-stage companies. Turnarounds focus on underperforming or struggling companies.

As Table 1, Panel B shows, growth deals account for more than half of our sample (940 deals, 59%), followed by early-stage investments (303 deals, 19%), buyouts (206 deals, 13%), secondaries (99 deals, 6%), and turnarounds (32 deals, 2%). Average and median deal size, reported in Table 1, Panel C, are USD 12.4 million and USD 5.0 million, respectively. These relatively small deal sizes reflect the sample's tilt towards growth-equity and early-stage investments.

As of December 2020, 1,230 of the 1,580 sample deals have been "exited." Of these, 857 were sold (mostly to a strategic buyer, management, or another PE firm), 107 were exited through an initial public offering on a stock market, 21 repurchased the fund's securities, and 245 returned less than invested capital or were written off completely. 350 deals are not yet exited, including 63 that have been "partly realized" (typically via a stock market listing in which the PE firm remains an investor post-listing).[9]

To estimate deal-level returns to investors, we use precisely dated cash flows between portfolio company and fund (i.e., initial and subsequent investments, dividends, and exit-related proceeds, if any). Cash flows are gross of the fund's management fees and carried interest and thus reflect a portfolio company's underlying performance. All cash flows are converted into USD using market exchange rates. Table 1, Panel D reports summary statistics for the loss ratio (the fraction of portfolio companies fully written off) along with the four return measures used by Acharya et al. (2013): the multiple on invested capital (MOIC), Kaplan and Schoar's (2005) public market equivalent (PME), unlevered return, and abnormal performance (alpha).[10]

The average fully exited portfolio company returns 2.28 times invested capital, outperforms the public-market benchmark with a PME of 1.83, and yields an unlevered annualized return of 12% and an abnormal performance of 2% p.a.; 13% of portfolio companies are written off. The relatively high level of performance reflects the sample skew towards early-stage and growth-equity deals, which tend to be characterized by a "home run" return pattern (a few very large wins, many strike-outs). Such deals also tend to be smaller. Returns are accordingly lower in the larger sample deals: when weighted by investment cost, the average exited portfolio company returns 1.88 times invested capital, outperforms the public-market benchmark with a PME of 1.36, and yields an unlevered annual return of 11%. The weighted average abnormal performance of 3% p.a. is a little higher and the loss ratio of 10% a little lower than on an unweighted basis.

2. What do PE playbooks look like?

Value creation plans play a central role in PE investments. They embody the PE firm's investment thesis for making the deal in the first place and spell out the PE firm's post-investment action plan, given

[9] We use the terms exited and realized interchangeably.

[10] Full definitions of all variables and details of their construction can be found in the Appendix. Briefly, following Acharya (2013), MOIC is returned capital (plus fair market value, if not fully exited) over invested capital; PME adjusts for the return on a public-market index but not for leverage; the unlevered return is the de-levered IRR on the deal; and abnormal performance is the difference between the unlevered return on the deal and the unlevered return on the public-market country index.

the PE firm's knowledge of management's pre-existing strategy and plans to increase value and the PE firm's assumptions about how it can help management to increase value further once it has invested in the company. Action plans tend to be both specific and detailed; they include measurable milestones and deliverables. For example, if the PE firm's market analysis suggests that customers are willing to pay a premium for a standardized service, the PE firm may formulate a plan to fund and facilitate a series of roll-up acquisitions of independent operators and help integrate them under the portfolio company's brand. Specific action items might then be to "negotiate a line of credit to fund acquisitions," "acquire operators A, D, and H," and "standardize service protocols and IT systems."

Once it has invested, the PE firm implements the planned action items over time to the extent that its investment thesis turns out to be correct, or makes changes to the playbook if its investment thesis turns out to be incorrect ("contrary to expectations, management lacks the skills to execute the business plan; we have therefore initiated a replacement of the management team"). Because we have access to each fund's quarterly reports to its investors and to the EBRD's internal half-yearly monitoring reports of each fund's activities, we can track both plan implementation and plan changes over time.

We have detailed information about PE firms' action plans for 1,136 of the 1,580 deals in our sample. Table 2, Panel A provides an overview of the playbooks in our sample. Each playbook consists of one or more action items. We track 23 distinct action items, which we group into five strategies: operational improvements, top-line growth, governance engineering, financial engineering, and cash management.[11] The two most popular strategies in our sample are operational improvements and top-line growth, which feature in 84% and 74% of playbooks, respectively. Governance engineering and financial engineering feature in roughly half (48%) and a third (35%) of playbooks, respectively. Improvements in cash management feature less often (14%).

It is common for action plans to span multiple strategies. In our sample, 929 playbooks (or 82%) do so. As Figure 2(a) shows, most plans span two or three strategies; the average is 2.5. With five strategies

[11] In their surveys, Kaplan and Strömberg (2009) and Gompers et al. (2016) focus on three of these strategies: financial engineering, operational improvements, and governance engineering. We add top-line growth and cash management based on our reading of the playbooks in our sample.

to choose from, there are 32 ($= 2^5$) possible combinations (four of which are never chosen). In practice, sample PE firms choose from a highly concentrated set of combinations. Figure 2(b) illustrates this high degree of concentration by plotting the cumulative distribution function of observed combinations in descending order of popularity against a uniform cdf.[12]

PE firms follow a rich variety of plans to add value to their portfolio companies. As Figure 2(c) shows, sample PE firms typically set out to implement two to five action items; the average is 4.5. The three most popular planned action items are buying new or upgrading existing physical assets (66%), changing the mix of products or services (37%), and pursuing add-on acquisitions (33%). The least frequent action item is improving inventory management (4%). With 23 action items to choose from, there is a very large number (2^{23}) of possible combinations. In practice, we observe a total of 776 unique combinations of action items. A closer look at sample PE firms' choices reveals evidence of both commonality in plans and a great amount of heterogeneity across deals. Figure 2(d) plots the frequency with which each of the 776 combinations is chosen against its popularity rank. Visual inspection of the figure clearly rejects the null hypothesis that combinations are distributed uniformly. The 10 most popular combinations of action items feature in 11.6% of sample playbooks, a vastly greater fraction than if combinations were distributed uniformly.[13] At the same time as we see evidence of commonality in plans, it is also true that 88.4% of sample playbooks pursue 766 other combinations of action items, suggesting that PE firms are quite heterogeneous in their plans. A plausible interpretation of such heterogeneity is that PE firms tailor each plan to each portfolio company's specific needs and circumstances.

We characterize the playbooks in greater detail in Section IA.D in the Internet Appendix. The main takeaways are that operational improvements are uniformly popular regardless of time period or deal

[12] PE firms clearly have favorite combinations. As Table IA.D1 in the Internet Appendix shows, the 10 most popular account for 80% of sample playbooks (twice as many as in a uniform distribution). Eight of the top 10 involve operational improvements or top-line growth or both. Governance engineering features in six of the top 10, with financial engineering and cash management in three and two of the top 10 combinations, respectively. The three most popular combinations involve both operational improvements and top-line growth, either with no other strategy (18%) or in combination with governance engineering (15%), or with governance and financial engineering (11%).

[13] Table IA.D2 in the Internet Appendix provides a breakdown of the 10 most popular combinations. All 10 include planned purchases/upgrades of physical assets. The most popular combination features in 3.5% of deals. It includes two action items: in addition to asset purchases/upgrades, the plan is to optimize the portfolio company's capital structure.

type; that financial engineering, governance engineering, and growth strategies have become more popular over the sample period; that the popularity of top-line growth and governance engineering strategies increases as the maturity of deals increases from early-stage to buyout; that financial engineering is much more common in buyout and turnaround deals; and that playbooks are more detailed in buyouts than in early-stage and growth deals. It is common for playbooks to be revised over time (77.3% add one or more action items after the holding period's first year), but revisions tend to be minor. The most common newly added action item is cost reduction, which 31% of deals add at some point during the holding period, perhaps because the deal has underperformed relative to expectations or has experienced an external shock (such as a recession).

3. The effect of PE ownership on portfolio-company performance

In this section, we link company-level changes in key outcome variables during the PE ownership period to PE firms' value creation plans in an effort to disentangle the treatment effects of PE investment from the selection effects that arise as PE firms target companies that are about to undergo major changes regardless of whether or not a PE firm invests in them.

3.1 Sample, data, and measures

To estimate the effect of PE ownership on portfolio companies during the holding period, we combine our narrative data with data obtained from Orbis, a global provider of harmonized financial data for public and private companies. We are able to link 1,373 of our 1,580 portfolio companies to Orbis by name (including historical ones where names have changed).[14] Using the Orbis data, we construct 17 measures related to four of the five playbook strategies.[15] To track PE firms' operational activities, we measure changes in a company's net investment, capital intensity, net assets, acquisitions and divestments, labor productivity, employment, total factor productivity (TFP), and EBITDA. To track top-line growth activities, we measure changes in sales, sales growth, market share, and price-cost markups. To track financial engineering activities, we measure changes in leverage and net debt to EBITDA.

[14] Table IA.A1 in the Internet Appendix confirms that the Orbis sample is representative of our full sample of 1,580 deals in terms of investor returns. Data gaps in Orbis thus appear random at least in this sense.
[15] The missing strategy is governance engineering. Unfortunately, Orbis does not retain historic data on such governance measures as the number of shareholders, ownership concentration, or board composition.

Finally, to track cash management activities, we measure changes in working capital and collection period. Further details and summary statistics can be found in Section IA.A in the Internet Appendix.

3.2 Empirical model

Our narrative approach builds on a standard control-firm design to remove common trends for similar-sized companies operating in the same country and industry. The approach exploits variation in action plans across deals by relating a given outcome (say, "number of completed acquisitions") to the presence or absence in the playbook for the portfolio company of an action item targeting that particular outcome (say, "pursue add-on acquisitions"). We run one such regression for each outcome/action-item pair in our dataset.

Our regression specification takes the following triple-diff form:

$$y_{it}^{o} = \beta_0 + \beta_1 PE_i * post_{it} + \beta_2 PE_i * post_{it} * plan_i^{o} + \gamma_{ct} + \varepsilon_{it} \quad (1)$$

where y_{it}^{o} is company i's outcome o in year t, $plan_i^{o}$ is an indicator capturing the presence of an action item related to outcome o in company i's playbook, and PE_i is an indicator set equal to 1 for portfolio companies and 0 for their matched controls.[16] We track each portfolio company and its matched controls (which together constitute a "cell" c) from (up to) five years before the PE investment to (up to) five years after the exit (or the end of our sample period if unexited).[17] For each portfolio company and its matched controls, $post_{it}$ equals 1 from the year the PE firm invests in the portfolio company and 0 before, while $plan_i^{o}$ takes the same value in a given cell. We estimate regression (1) with a full set of cell-by-year fixed effects, γ_{ct}, to remove common trends at the size-country-industry-year level and cluster standard errors at the company level, as disturbances to a company's operations and performance are potentially correlated over time.

The two coefficients of interest, β_1 and β_2, can be interpreted as follows. Coefficient β_1 captures the average change in outcome o at PE portfolio companies whose playbooks contain no action items related

[16] For each portfolio company, we include a group of up to five control companies that have never experienced PE ownership during our sample period. Specifically, we follow prior literature and select control companies based on an exact match on country, industry, and investment year, and a propensity-score match on observable financials (total assets in investment year t, revenue in years $t-1$ and t, and employment in year t). Control companies can be listed or unlisted.

[17] Our results are qualitatively unchanged if we drop post-exit years from the estimation. The inclusion of post-exit years means that we identify a long-term PE ownership effect.

to the outcome, net of the contemporaneous change at observably similar non-PE companies. Outcome o can change in the absence of a relevant action item as a result of pre-existing plans by management to improve the outcome. If so, β_1 can be thought of as capturing, in part, a PE selection effect (to the extent that such pre-existing plans made the company an attractive target to begin with). Coefficient β_2, on the other hand, captures a PE treatment effect: it equals the average *additional* change in outcome o at PE portfolio companies whose playbooks contain an action item related to the outcome relative to those whose playbooks do not contain such an action item (again, net of the contemporaneous change at observably similar non-PE companies). The average *total* change in outcome o for portfolio companies with a relevant action item is $\beta_1 + \beta_2$, that is, the sum of the selection effect and the treatment effect.

3.3 Results

Table 3 summarizes the regression estimates of company-level changes in our 17 outcome variables during the PE holding period as a function of the presence or absence of an action item related to the outcome variable. Figure 3 illustrates these estimates by plotting standardized effects on each of the 17 outcome variables for portfolio companies without an associated action item (i.e., the selection effect β_1 in equation 1), the difference between the effects in the presence and absence of an associated action item (i.e., the treatment effect β_2), and the combined effect (i.e., $\beta_1 + \beta_2$).

We find plenty of selection effects (i.e., significant company-level changes that occur in the absence of a plan by the PE firm to affect those particular outcome variables) and only a few treatment effects (i.e., significant company-level changes that relate to one or more of the PE firm's action items). The selection effects relate to all four strategies. Portfolio companies whose playbooks involve *no* plans by the PE firm to reduce costs or to boost top-line growth nonetheless experience significant improvements over the holding period in labor productivity (p<0.001), employment (p<0.001), total factor productivity (p<0.001), and EBITDA (p=0.017) on the one hand, and in sales (p<0.001), sales growth (p=0.035), and market share (p<0.001) on the other (net of contemporaneous improvements at similar-sized control firms operating in the same country and industry). They also significantly improve their cash management by reducing their use of working capital (p=0.008) and fine-tune their capital structure by increasing leverage (p<0.001) and net debt to EBITDA (p=0.059). Absent any plans by the PE firm to create value on these margins, we view these changes as the outcomes of management plans that would have been implemented regardless of the PE firm's investment.

We find no evidence of treatment effects on the cost or growth sides: portfolio companies in which the PE investor plans to focus on cost reductions and the top line achieve no better outcomes than portfolio companies in which the PE investor has no such plans. Interestingly, TFP falls significantly among portfolio companies whose PE firms plan to improve it. Overall, these patterns suggest that despite their pre-deal value creation plans, PE firms struggle to add additional value to a portfolio company's income statement, over and above management's pre-existing plans in these dimensions.

Instead, the positive treatment effects are concentrated on the assets and liabilities sides of a portfolio company's balance sheet. Portfolio companies whose playbooks involve plans to improve operations by buying, upgrading, or selling assets or pursuing add-on acquisitions and those whose playbooks involve plans to improve tax efficiency experience significant increases in net investment, engage in more acquisitions and divestments, and increase both leverage and net debt to EBITDA by significantly more than portfolio companies without such plans. In other words, PE firms planning to engage in these types of operational or financial engineering activities on average successfully implement their action plans. Economically, these treatment effects are large. For example, portfolio companies whose PE firm planned to optimize capital structure increase leverage by 5.2 percentage points more on average than the 6.8 percentage-point increase seen among portfolio companies without such a plan.[18]

The combined effects of selection and treatment are extensive. Over the course of the PE ownership period, portfolio companies invest more than their matched controls (p<0.001), increase capital intensity (p=0.008) and net assets (p=0.018),[19] complete more acquisitions (p<0.001) and divestments (p=0.038), increase employment (p<0.001), sales (p<0.001), sales growth (p=0.021), and market share (p=0.060),

[18] Tables IA.E1 through IA.E3 in the Internet Appendix report results for the subsamples of early-stage, growth, and buyout deals. In early-stage deals, the significant treatment effects are on net investment, markups, leverage, and net debt to EBITDA, while both labor productivity and TFP fall significantly despite the PE firm's plan to improve productivity. In growth deals, the significant treatment effects are on net investment, the number of acquisitions, and net debt to EBITDA. In buyout deals, the sole significant treatment effect is on the number of divestments; sales increase significantly, but by less than among the portfolio companies of PE firms with no action plan to boost sales.

[19] For capital intensity and net assets, β_1 and β_2 are noisily estimated, preventing us from disentangling treatment and selection.

along with leverage (p<0.001), and net debt to EBITDA (p<0.001), while streamlining their cash management (p=0.006). As Figure 3 shows, by far the largest effect, economically speaking, is on leverage, followed by sales, employment, acquisitions, and market share.

4. The effect of PE ownership on deal-level returns

To what extent do PE firms' treatment activities during the ownership period contribute to investor returns? We approach this question in two ways. First, we follow the literature and study the association between company-level changes during the ownership period and deal-level returns. The twist is that our narrative approach allows us to categorize each company-level change as reflecting either a selection effect or a treatment effect (or potentially both). In other words, we can isolate the ways in which PE ownership causally affects returns in a treatment sense. Second, we test whether returns increase in the extent to which the PE firm's value creation plan is successfully implemented.

4.1 Approach 1: Company-level changes and deal-level returns

4.1.1 Empirical model

Figure 1 shown in the introduction relates Acharya et al.'s (2013) four measures of deal-level returns to the 17 company-level changes in outcomes investigated in Section 3. The underlying estimates come from cross-sectional company-level regressions specified, following Acharya et al., as follows:

$$y_i^r = \alpha_0 + \alpha_1\left[x_{i,exit} - x_{i,entry-1}\right] + \alpha_2 X_i + \xi_t^{entry} + \xi_t^{exit} + \varepsilon_{it} \tag{2}$$

where y_i^r is the return on deal i (measured using one of MOIC, PME, unlevered return, and abnormal performance), $\left[x_{i,exit} - x_{i,entry-1}\right]$ is a vector of company-level changes in our 17 outcome variables measured over the PE holding period,[20] X_i is a vector of controls such as log deal size and log deal duration, and ξ_t^{entry} and ξ_t^{exit} are time dummies for the years of PE investment and PE exit, which we include to capture macro factors that affect all investments and all exits at the same time, respectively (including the impact of market timing via "multiple expansion," i.e., the change in average market-wide valuation multiples between investment and exit). The sample includes all exited deals plus unexited

[20] To guard against fluctuations in company-level changes driven by entry and exit years (and to increase the number of observations), we average entry and exit values over a three-year period. That is, $x_{i,entry}$ is measured as the mean of $x_{i,t-1}$, $x_{i,t}$, and $x_{i,t+1}$, where t is the investment year, and analogously for $x_{i,exit}$. Results are qualitatively unaffected if we use only outcomes observed in entry and exit years.

deals that have been held in a PE fund's portfolio for at least four years.[21]

4.1.2 Results

Table 4 reports results separately for each of the four return measures, considering each of the 17 company-level outcome variable one at a time. The coefficients are standardized so that they can easily be compared in terms of their economic significance. The number of observations included in each regression varies depending on data availability in Orbis.

Four company-level changes correlate significantly with all four return measures: the more a portfolio company manages to increase its net assets, sales, and employment, and the more acquisitions it completes during the holding period, the higher are investors' return on the deal. Taken at face value, these correlations echo the core result in Acharya et al. (2013). In addition, we find that increases in net investment are associated with higher abnormal performance, increases in capital intensity are associated with higher MOICs and PMEs and increases in EBITDA are associated with higher MOICS, PMEs, and abnormal performance. Interestingly, changes in outcomes associated with financial engineering or cash management never correlate significantly with realized investor returns in the cross-section.[22]

Which of these correlations plausibly reflect treatment effects and which are instead more likely to reflect selection? Treatment results in greater returns when planned action items affect both company-level performance and deal-level returns (i.e., when $\beta_2 \neq 0$ in equation (1) and $\alpha_1 \neq 0$ in equation (2)). Selection results in greater returns when portfolio companies undergo changes during the holding period that did not feature in the PE firm's pre-investment value creation plan but that significantly correlate with deal-level returns (i.e., when $\beta_1 \neq 0$ in equation (1) and $\alpha_1 \neq 0$ in equation (2)). Table 5 summarizes our results along these lines. In view of our finding in Table 3 that increases in employment, profitability, and sales over the holding period reflect selection rather than treatment, we interpret the significantly positive relations in Table 4 between deal-level returns and employment, profitability, and sales as not being causal in a treatment sense; instead, they are likely driven by changes that result from

[21] Excluding unexited deals does not change our results qualitatively, but it reduces our sample size.
[22] Table IA.E4 in the Internet Appendix reports the results of specifications that interact each of the 17 company-level changes with deal-type indicators. Broadly speaking, the results in Table 4 reflect the behavior of early-stage, growth, and buyouts deals much more than the behavior of secondaries or turnarounds. For both secondaries and turnarounds, price cuts and improvements in working capital are associated with higher returns.

PE firms selecting companies whose management already had plans in place to increase their company's employment, profitability, and sales. The significantly positive relation in Table 4 between deal-level returns and the number of acquisitions completed during the holding period, on the other hand, plausibly reflects treatment, in view of our finding in Table 3 that the portfolio companies of PE firms with plans to pursue add-on acquisitions complete significantly more acquisitions than the portfolio companies of PE firms without such plans.

Interestingly, even though we find both a PE selection effect and a PE treatment effect on capital structure in Table 3, changes in capital structure do not affect any of the return measures in Table 4. In other words, financial engineering does not create value on average in our sample.

4.2 Approach 2: Plan achievement and deal-level returns

If PE firms' value creation plans increase returns to investors causally (in a treatment sense), we expect the return on a deal to be higher the more of its playbook the PE firm manages to implement during the ownership period, all else equal. If instead returns to investors are driven mainly by selection effects, we expect deal-level returns to be unrelated to plan execution.

The main empirical challenge with this test is holding all else equal. The main potential confound is that benign market environments may facilitate the execution of a playbook while simultaneously boosting the return on the deal for reasons unrelated to the PE firm's treatment activities. Whether successful execution is driven by macroeconomic rather than idiosyncratic factors is an empirical question. While our return regressions will control for a deal's market environment, we first investigate directly whether, and if so how, a deal's market environment affects plan achievement.

4.2.1 Determinants of plan achievement

Our textual data allow us to track the implementation and achievement (or otherwise) of each action item in each deal's playbook over time. We code an action item as achieved based on the quarterly fund reports funds are required to provide to their investors.[23] Generally speaking, PE firms manage to

[23] We take seriously the possibility that PE firms' reports to their investors are strategic: perhaps PE firms only *claim* to have successfully executed on their plans in their ex post successful deals, in order to *appear* to be adding value, and blame failure in ex post unsuccessful deals on external events that interfered with their ability to put plans into action even when plans were in fact implemented (just not with the hoped-for results). There are two (in our mind good) reasons to doubt that strategic

implement most of their individual action items and strategies. We illustrate this by plotting the number of planned and achieved strategies in Figure 4(a) and action items in Figure 4(b) using bubble diagrams. The size of each bubble reflects the number of deals. For both strategies and action items, we see that most deals lie on the 45-degree line, meaning that most planned strategies and action items are achieved. For example, of the 349 deals intending to pursue a combination of two strategies, 287 achieve both, 50 achieve one, and only 12 achieve neither. Table 6 tabulates achievement rates at the individual strategy and action item levels. Achievement rates are generally high across all strategies and action items. The lowest achievement rates are associated with plans to "target market share" (62%), "improve corporate governance" (71%), and "pursue international expansion" (73%).

Do macro conditions determine whether a PE firm successfully implements an action item, or are achievement rates largely driven by idiosyncratic factors and the PE firm's skills? To answer these questions, we relate plan achievement to a set of three macroeconomic controls (local GDP growth, the return on the country's stock market index, and revenue growth in the portfolio company's industry, each measured over the holding period), and a proxy for idiosyncratic factors in the form of Cornelli, Kominek, and Ljungqvist's (2013) "bad luck" indicator, which captures whether, in the opinion of the portfolio company's board of directors, poor performance was caused by factors beyond management's control.[24] In addition, we control for the characteristics of the playbook, the deal, and the fund.

Table 7 reports the results of four specifications.[25] The first specification is a linear-probability model in which the dataset is constructed at the action-item-by-deal level such that the unit of observation is an action item. The dependent variable is set equal to one if an action item is achieved during the holding

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reporting of this kind can account for our findings. First, strategic reporting is difficult to square with the timing of our data: funds report each portfolio company's progress on a quarterly basis rather than providing a (possibly strategic) narrative after write-off or successful exit. It is thus impossible for a fund to pretend not to have been able to execute its plan in an ex post unsuccessful deal, though we recognize that a fund *might* begin to report strategically once it deems the chances a deal will succeed to have worsened. Second, Cornelli, Kominek, and Ljungqvist (2013) discuss the EBRD's monitoring processes of the quarterly fund reports from which we extract implementation information, concluding that "reports are unbiased" and that "it is highly unlikely that a fund manager would deliberately withhold or distort information," for both legal and reputation reasons.

[24] Examples of "bad luck" include fires affecting a company's main production facility or a key supplier going bankrupt. Cornelli, Kominek, and Ljungqvist's (2013) data end in 2010. Due to reporting changes, we can only update their bad luck indicator through calendar year 2017, limiting the estimation sample accordingly.

[25] In this exercise, we focus on exited deals, to allow for a definitive assessment of whether or not the PE firm achieved the action item or strategy in question.

period, and zero otherwise. The second specification uses a strategy-by-deal-level dataset, with the dependent variable being the share of planned action items that are successfully implemented in a given strategy and deal. The third and fourth specifications are estimated at the deal level, with the dependent variables being the shares of action items and the share of strategies that are successfully implemented in a deal, respectively.

Controlling for deal year, deal type, and fund fixed effects, Table 7 shows that macro conditions have essentially no effect on plan achievement.[26] Idiosyncratic factors outside management's control, on the other hand, have a large negative effect at the action item and strategy levels: in deals experiencing "bad luck" events, an action item is 4.6 percentage points less likely to be achieved (p=0.003 in column 1) and the share of action items that are achieved per strategy is 4.6 percentage points lower (p=0.004 in column 2).[27] At the deal level, on the other hand, adverse idiosyncratic shocks play no role, suggesting that idiosyncratic shocks are diversifiable at the deal level.

4.2.2 Empirical model

If PE firms' treatment activities causally drive investors returns, we expect the return on a deal to increase in the extent to which the PE firm successfully implements its value creation plan. If instead returns are driven by selection, we expect deal-level returns to be unrelated to plan implementation. Given that our data allow us to observe which action items are achieved during the ownership period, we are in a unique position to test this prediction. To this end, we estimate the following regression in the cross-section of fully realized deals:

$$y_i^r = \delta_0 + \delta_1 share\ achieved_i + \eta_1 X_i + \tau_t^{entry} + \tau_t^{exit} + \omega_f + \epsilon_i \quad (3)$$

where y_i^r is the return on deal i (as before, measured using Acharya et al.'s 2013 four return metrics), and $share\ achieved_i$ is the share of action items that the PE firm successfully implements over the life of the

[26] All results are similar if instead of PE fund fixed effects we include PE firm fixed effects. See Table IA.E5 in the Internet Appendix.

[27] We briefly summarize the results for the control variables, using column 1 to illustrate. The more action item a strategy includes, the less likely each action item is to be achieved (p=0.024), suggesting that action items are substitutes rather than complements. The more time a deal spends in a PE firm's portfolio, the more likely an action item is achieved (p<0.001). Deal size has a positive effect (p=0.007), perhaps because funds focus their limited attention on those deals that, due to their larger size, can generate larger dollar returns. Majority ownership increases the chance that an action item is achieved by an economically large 3.9 percentage points (p=0.016), while a focus on inorganic growth reduces it by an economically large 5.4 percentage points (p=0.004). Action items are less likely to be achieved in deals made later in a fund's life (p=0.049).

deal. Following Acharya et al., X_i includes controls for deal duration (since a longer holding period provides more opportunities to achieve the plan, all else equal) and deal size (since we expect PE firms to focus their resources on their larger deals). We also control for the number of action items in the playbook (since more detailed plans may be harder to achieve in their entirety). As in equation (2), we include entry and exit year fixed effects to control for macro factors that affect all investments and all exits at the same time, respectively (including the impact of market timing via multiple expansion). We cluster standard errors at the PE fund level to allow for returns to be correlated within fund.

Under the null hypothesis that returns reflect selection, $\delta_1 = 0$. Under the alternative hypothesis that returns are positively affected by PE firms' treatment activities, $\delta_1 > 0$. The main challenge to interpreting δ_1 as the treatment effect of PE investment on returns comes from potential omitted variables. As noted, the chief concern is that benign market environments make it easier to implement value creation plans while simultaneously boosting returns for reasons unrelated to a PE firm's treatment activities. While we found no evidence, in the previous section, that plan achievement is driven by anything other than factors that are idiosyncratic to the deal, we nonetheless include in X_i the three macro variables from Table 7 to control for a deal's market environment. To capture idiosyncratic deal-level factors beyond a PE firm's control, which Table 7 shows have a first-order effect on the likelihood that an action item is successfully implemented, we include Cornelli, Kominek, and Ljungqvist's (2013) "bad luck" indicator. To capture idiosyncratic fund-level factors, we include a set of fund fixed effects, ω_f; these ensure that identification comes from exploiting variation across deals within the same PE fund and so help address concerns that some funds may possess superior skill or luck in both executing playbooks and achieving high returns or that some funds operate in geographic areas or industries in which returns tend to be higher.

4.2.3 Results

Table 8 reports the results. Not surprisingly, bad luck is uniformly bad for deal-level returns, while high stock market returns and high revenue growth in the portfolio company's industry improves returns. Controlling for these influences, we find that achieving a greater share of planned action items is strongly associated with higher returns in the cross-section. Economically, a one-standard-deviation increase in the share of action items that are achieved is associated with a 13.4% increase in MOIC (p=0.060, column 1), a 9.8% increase in PME (p=0.053, column 3), a 3.5% increase in unlevered returns (p=0.053, column 5), and a 3.1% increase in abnormal performance (p=0.076, column 7), relative to the sample mean. In other words, the further from the 45-degree line a deal ends up in Figure 4, the worse is its performance. This result suggests that execution contributes to achieving high returns for investors.

The specifications in the four even-numbered columns show that the importance of execution varies across deal types. Successful execution is associated with significantly higher returns only in growth, buyout, and secondary deals. In early-stage deals, achievement of planned action items does not correlate significantly with returns, suggesting that risk factors in early-stage ventures are more idiosyncratic and hence more difficult for a PE firm to influence: will customers like the product? will the team be able to execute the business plan? how will incumbents respond? Turnarounds often involve tricky negotiations with lenders over covenant waivers, loan refinancing, asset sales, and collateral impairment, activities that may be central to returns but are not explicitly mentioned in the playbooks.

5. Conclusion

We seek to disentangle the effects of treatment, selection, and financial engineering on investor returns in private equity deals by combining Romer and Romer's (1989) narrative approach for identification with an otherwise standard control-firm design. We exploit confidential textual data contained in pre-deal investment memos, value creation plans, and post-investment monitoring reports for a large sample of deals by European PE firms spanning nearly three decades. Value creation plans are highly differentiated, suggesting that they are tailored to the needs and circumstances of each individual portfolio company. We use these tailored plans to differentiate between PE firms' intended treatment effects at their portfolio companies while under their ownership and any changes that would have been realized under management's existing plans, which more plausibly reflect selection effects.

In summary, our evidence shows that PE firms create value for their investors both through selection and through treatment, but not through financial engineering. PE firms select companies that are about to outperform their matched peers in terms of both the top-line and the bottom-line (akin to stock picking in public equity markets), and they help their portfolio companies improve their production facilities and processes by supporting CAPEX and acquisitions. These company-level changes in turn increase the valuations at which the portfolio companies can later be sold, resulting in higher deal-level returns to investors. Tracking the post-investment implementation of value creation plans using confidential reports to investors, we show that successful execution increases investor returns over and above the return-boosting effects of successful stock picking, especially in growth, buyout, and secondary deals. In early-stage deals, execution appears to matter less, perhaps because risk factors are more idiosyncratic and hence more difficult for a PE firm to plan for: optimizing production processes may not matter much if customers turn out not to like a startup's minimum viable product or the management team is unable to work together effectively.

Prior work on PE value creation deals with the main identification concern—that targets are selected non-randomly—by matching on observables. Our results highlight the limitations of the matched-control approach in the PE setting. We find substantial selection effects in most of the company-level outcomes that are typically studied in the literature on value creation in PE. For example, a consistent finding in the literature is that portfolio companies increase their sales while under PE ownership. We do not disagree with this finding; in fact, we find the same in yet another sample. But we caution that the finding may not reflect treatment: portfolio companies increase their sales while under PE ownership even if the PE firm has no plans to take any actions designed to increase sales; moreover, when the PE firm does plan to increase sales, the increase in sales is no greater, on average, than when the PE firm has no such plan. With the caveat that our findings may not generalize to other samples, we conclude that the widely observed increase in sales among PE portfolio companies reflects not a treatment effect but a selection effect: PE firms target companies whose sales will increase regardless of PE ownership.

Ours is the first comprehensive study of PE playbooks. As such, we can open up the black box of value creation in two main ways. First, our narrative approach and tracking of plan execution help us differentiate between deal selection and post-investment actions as value drivers. Second, our findings are based on quantifying textual data reflecting the actual strategies and actions that PE firms plan to undertake in each deal and their subsequent execution. This enables us to sidestep issues related to survey methodology, in particular the worry that PE firms may want to cast themselves in a positive light or respond selectively. Along the way, we identify two value creation strategies—top-line growth and cash management—that are increasingly popular among PE firms but have not featured in academic surveys.

Our focus in this paper is on value creation conditional on a PE fund having selected a company for investment. Surveys of PE firms and their investors (Gompers, Kaplan, and Mukharlyamov 2016, Gompers et al. 2020) highlight the importance of deal sourcing and selection, especially in the context of startups. How PE funds source and select their investments is a promising avenue for future research.

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Appendix. Variable definitions.

<u>Value creation plans: Action items</u>

Operational improvements

Buy/upgrade assets refers to plans to buy or upgrade fixed the portfolio company's assets.

Sell existing assets refers to plans to sell some of the portfolio company's fixed assets.

Divest/spin off companies refers to plans to sell or spin off parts of the portfolio company's business.

Reduce costs refers to plans to reduce the portfolio company's cost of goods sold (e.g., direct labor, materials, and overhead) and/or operational expenses (e.g., selling, general, and administrative costs).

Improve IT systems refers to plans to improve the portfolio company's information technology (IT) systems (e.g., its management information system).

Improve distribution or logistics refers to plans to improve the movement of raw materials into the portfolio company and/or the movement of finished goods out of the portfolio company to the end-customer.

Improve organizational structure refers to plans to reorganize the portfolio company's business functions and/or business units.

Top-line growth

Target market share refers to plans to increase the portfolio company's market share or reach a certain scale.

Pursue add-on acquisitions refers to plans to merge with or acquire another business.

Change product/services mix refers to plans to introduce, upgrade, or eliminate products and/or services from a company's offering.

Pursue international expansion refers to plans to enter new geographies or leave existing geographies.

Change pricing strategy refers to plans to increase or reduce the portfolio company's prices.

Improve marketing/promotion refers to plans to improve the portfolio company's marketing, promotion, or communication strategy.

Improve quality refers to plans to improve the quality of the portfolio company's products and/or services.

Governance engineering

Change CEO refers to plans to replace the portfolio company's chief executive officer (CEO).

Change CFO refers to plans to replace the portfolio company's chief financial officer (CFO).

Change other management refers to plans to change members of the portfolio company's senior management team other than the CEO or CFO (e.g., the chief operating officer or chief information officer) and/or middle management (e.g., heads of departments).

Improve corporate governance refers to plans to improve the system of rules, practices, and processes by which the portfolio company is directed and controlled (e.g., internal controls, disclosure, and transparency).

Change board/shareholder structure refers to plans to change the size and/or composition of the portfolio company's board of directors or its ownership structure and/or to resolve shareholder conflicts.

Financial engineering

Optimize capital structure refers to plans to borrow additional debt to finance projects, to refinance existing debt, or to increase the portfolio company's leverage for tax reasons.

Improve incentive systems refers to plans to introduce performance-based incentive systems for the portfolio company's management and/or employees (e.g., through equity ownership or bonus programs).

Cash management

Improve receivables/payables refers to plans to reduce the portfolio company's payment terms to customers and/or to extend suppliers' payment terms.

Improve inventory management refers to plans to improve the portfolio company's process of ordering, storing, and using a company's inventory.

Value creation plans: Strategy-level variables

 # action items per strategy is defined as the number of action items a fund plans to pursue in a value creation strategy.

Share of action items in strategy achieved is defined as the number of action items per strategy achieved divided by the number of planned action items per strategy.

Share of deals following strategy is defined as the number of deals in which a fund plans to pursue a specific value creation strategy divided by the total number of deals in the fund's portfolio.

Deal-level variables

Deal characteristics

Deal size is defined as the total cost of investment in a portfolio company by a fund. If there are multiple funds investing in a portfolio company, we sum each fund's investment cost.

Deal duration is defined as the number of years that a deal spends in a fund's portfolio, rounded up to the nearest integer.

Majority ownership is an indicator variable set equal to one if a fund's largest equity ownership stake in the portfolio company is equal to or greater than 50% over the deal's holding period, and zero otherwise.

Inorganic growth is an indicator variable set equal to one if a fund plans to pursue add-on acquisitions as an action item, and zero otherwise.

 # action items per deal is defined as the number of action items a fund plans to pursue in a deal.

Share of action items achieved in a deal is defined as the number of planned action items a fund achieves during the deal's holding period divided by the number of action items the fund planned to pursue in the deal.

 # strategies per deal is defined as the number of strategies a fund plans to pursue in a deal.

Share of strategies achieved in a deal is defined as the number of planned strategies a fund achieves during the deal's holding period divided by the number of strategies the fund planned to pursue in the deal.

Deal sequence number is the deal's chronological sequence number in a fund's portfolio when deals are sorted chronologically by investment date, with 1 corresponding to the earliest invested deal.

GDP growth is the average year-on-year real GDP growth rate of the country in which a portfolio company is headquartered, measured between a deal's investment and exit dates. Source: World Development Indicators, World Bank.

MSCI index growth is the average year-on-year growth rate of the MSCI Country Total Return Index of the country in which a portfolio company is headquartered, measured between a deal's investment and exit dates.

Industry revenue growth is the average year-on-year growth rate in revenues of public companies operating in the same 2-digit industry as the portfolio company and listed in the country in which a portfolio company is headquartered, measured between the deal's investment and exit dates and weighted by prior-year revenues. Source: Orbis.

Bad luck is an indicator variable set equal to one if, in the opinion of the portfolio company's board of directors, a deal's poor performance in a given year was caused by factors beyond management's control. The coding follows the methodology of Cornelli, Kominek, and Ljungqvist (2013). We update their data through the end of our sample period.

Performance and deal-level investor return measures

Fully realized is defined as a deal that has been fully exited by a fund either through an initial public offering (IPO), a trade sale, or a secondary sale, or has been written off.

Unrealized is defined as a deal that has not been fully exited as of the end of our sample period (December 2020).

Loss ratio is the fraction of a fund's portfolio companies (either by number or by investment cost) that have been written off.

Multiple on invested capital (MOIC) is defined as the sum of investment proceeds received and current fair value divided by total investment cost based on gross-of-fees cash flows between a fund and a portfolio company, in USD.

Public market equivalent (PME) is defined as the present value of gross-of-fees cash flows between a fund and a portfolio company relative to the present value of cash flows from a hypothetical contemporaneous investment in a public market index, in USD. The computation follows Kaplan and Schoar (2005) and uses the MSCI Emerging Markets Total Return Index as a public-market benchmark. PMEs greater than one indicate PE investments that yield higher gross-of-fees returns than contemporaneous public-market investments, not holding risk, liquidity, or leverage constant.

Unlevered return is defined as the internal rate of return (IRR) de-levered using a portfolio company's average deal debt/equity ratio between the investment and exit dates, where IRR captures all cash inflows and cash outflows of the PE fund (before fees) during its ownership, in USD. The computation follows Acharya et al. (2013):

$$R_{U,i} = \frac{R_{L,i} + R_{D,i}(1-t)(D/E_i)}{(1 + D/E_i)}$$

where $R_{L,i}$ is deal-level IRR, $R_{D,i}$ is the average cost of debt, t is the average corporate tax rate during the holding period for the country in which the portfolio company is headquartered, and D/E_i is the average debt/equity ratio employed in the deal. Since funds do not report the average cost of debt, we use the portfolio company's average implicit interest rate measured between the deal's investment and exit dates.

Abnormal performance is defined as the difference between the deal's unlevered return and the unlevered country return rate, in USD. The country return rate is measured using the MSCI Country Total Return Index and unlevered in the same way as the deal return, but using publicly listed firms to calculate the average cost of debt and the debt/equity ratio, measured between the deal's investment and exit dates.

Company-level variables related to strategy "operational improvements"

Employment is defined as the natural logarithm of the total number of full-time employees. Source: Orbis.

Labor productivity is defined as the natural logarithm of revenue per employee. Source: Orbis.

Net investment in fixed assets is the annual change in fixed assets net of depreciation and scaled by beginning-of-year nominal total assets, in USD. Source: Orbis.

Net assets is defined as the natural logarithm of the difference between total assets and total liabilities, in USD. Source: Orbis.

Capital intensity is defined as the natural logarithm of the ratio of fixed assets in USD to employment. Source: Orbis.

Acquisitions is defined as the number of companies bought as part of a merger or acquisition completed in that calendar year. Source: Orbis.

Divestments is defined as the number of companies sold in that calendar year. Source: Orbis.

Total factor productivity (TFP) captures the efficiency with which all inputs into production (labor, materials, and capital) are used. For details of its construction, see Section IA.B in the Internet Appendix.

EBITDA is defined as the natural logarithm of a company's earnings before interest, taxes, depreciation, and amortization (EBITDA, in USD) if EBITDA is positive, and minus the natural logarithm of minus EBITDA if EBITDA is negative. We replace EBITDA with EBIT whenever the former is missing. Source: Orbis.

Company-level variables related to strategy "top-line growth"

Sales is defined as the natural logarithm of annual operating revenue measured in USD. Source: Orbis.

Sales growth is defined as the annual change in the natural logarithm of sales. Source: Orbis.

Markup is defined as the natural logarithm of the estimated ratio of price to marginal cost. For details of its construction, see Section IA.C in the Internet Appendix.

Market share is defined as the ratio of annual company sales to the total of annual sales by all companies in the same four-digit NACE industry and country. Source: Orbis.

Company-level variables related to strategy "financial engineering"

Leverage is defined as the ratio of short-term bank loans plus long-term debt (= total debt) to total assets. Source: Orbis.

Net debt to EBITDA is defined as the ratio of total debt minus cash to EBITDA. Source: Orbis.

Company-level variables related to strategy "cash management"

Working capital is defined as the ratio of working capital to the sum of working capital and fixed assets. Source: Orbis.

Collection period is defined as the ratio of debtors' accounts to operating revenue, multiplied by 360. Source: Orbis.

Figure 1. Company-level changes during the PE holding period and deal-level returns.
The figure graphs estimates of company-level changes in 17 outcome variables during the PE holding period (in the first column) and the effect of company-level changes in each of the 17 outcome variables on deal-level investor returns as measured by PME, MOIC, unlevered return, and abnormal performance (in the remaining four columns). Each plotted coefficient corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized deals as well as control firms matched on size, country, industry, and investment year. Both outcomes and investor returns are standardized to have a mean of 0 and a standard deviation of 1, ensuring that effect sizes can be compared within and across specifications. Error bands indicate 95% confidence intervals (clustered at the company level). For full details of the estimation of the first column, see Section IA.A in the Internet Appendix. For full details of the estimation of the remaining four columns, see Section 4.1. For variable definitions and details of their construction see the Appendix.

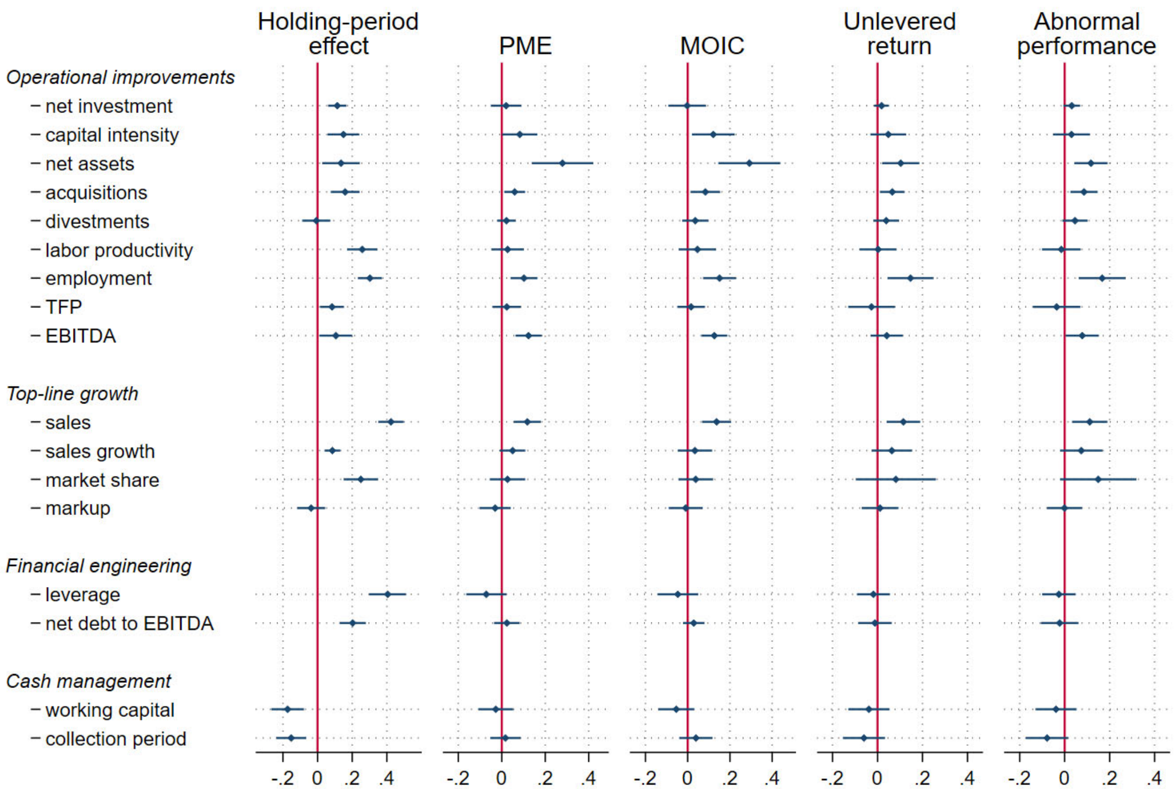
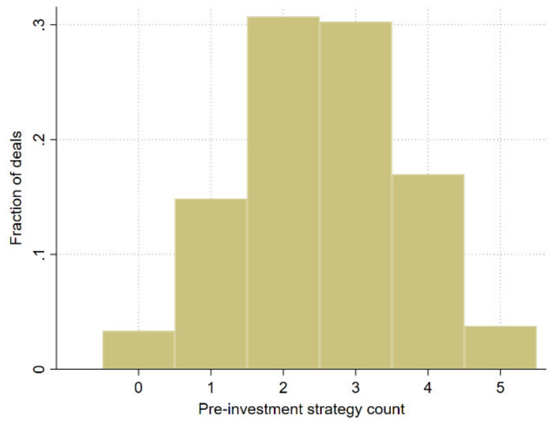
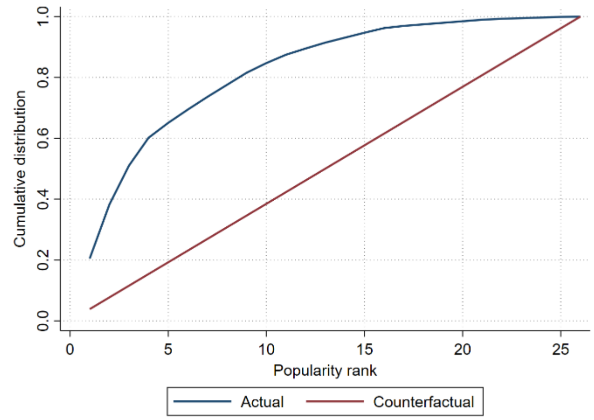


Figure 2. Value creation plans: Strategies and action items.

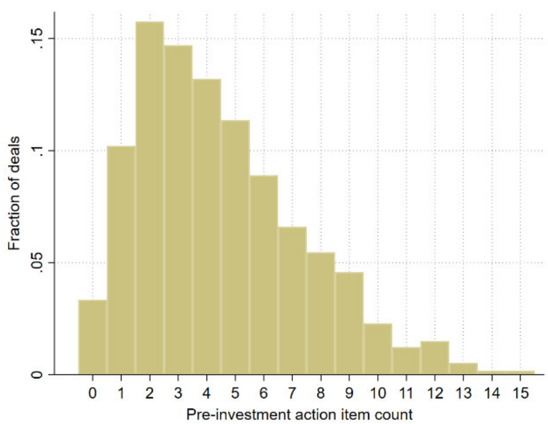
The two graphs on the left show the distribution of the total number of initial strategies (a) and action items (c) per playbook. The two graphs on the right show the distribution of unique combinations of strategies (b) and action items (d) against their popularity rank, conditional on a playbook including at least two strategies or two action items, respectively. "Actual" in (b) and (d) shows the observed empirical distribution of combinations, while "counterfactual" shows the hypothetical distribution that would obtain if each combination were observed equally often.



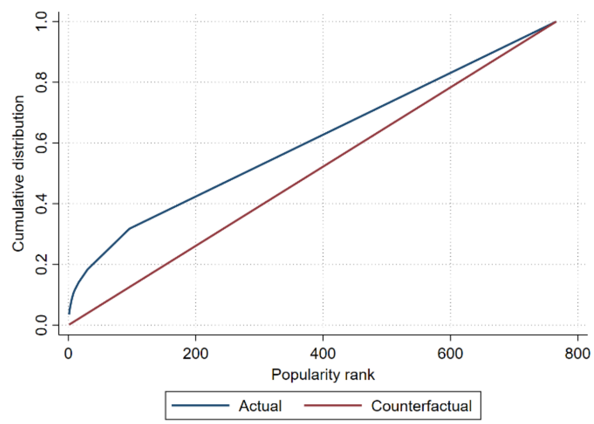
(a) Distribution of strategies



(b) Distribution of strategy combinations



(c) Distribution of action items



(d) Distribution of action item combinations

Figure 3. The effect of PE ownership on portfolio-company performance.

The figure graphs estimates of company-level changes in 17 outcome variables during the PE holding period as a function of the presence or absence of an action item related to the outcome variable in question for four of the five strategies in our playbooks. (We lack outcome variables related to the fifth strategy, governance engineering.) Each plotted coefficient corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized deals as well as control firms matched on size, country, industry, and investment year. For each outcome/action-item pair, we estimate regression (1): $y_{it}^{o} = \beta_0 + \beta_1 PE_i * post_{it} + \beta_2 PE_i * post_{it} * plan_i^{o} + \omega_{ct} + \varepsilon_{it}$, where y_{it}^{o} is company i's outcome o in year t, $plan_i^{o}$ is an indicator capturing the presence of an action item related to outcome o in company i's playbook, and PE_i is an indicator set equal to 1 for portfolio companies and 0 for their matched controls. We track each portfolio company and its matched controls, which constitute a "cell" c, from (up to) five years before the PE investment to (up to) five years after the exit or the fifth anniversary if unexited as of the end of our sample period. For each portfolio company and its matched controls, $post_{it}$ equals 1 from the year the PE firm invests in the portfolio company and 0 before, while $plan_i^{o}$ takes the same value in a given cell. Each regression includes a full set of cell-by-year fixed effects, ω_{ct}. Coefficient estimates are standardized to have mean 0 and standard deviation 1 to make them comparable across the 17 outcome variables. Error bands indicate 95% confidence intervals (clustered at the company level). Full regression results are summarized in Table 3. For variable definitions and details of their construction see the Appendix.

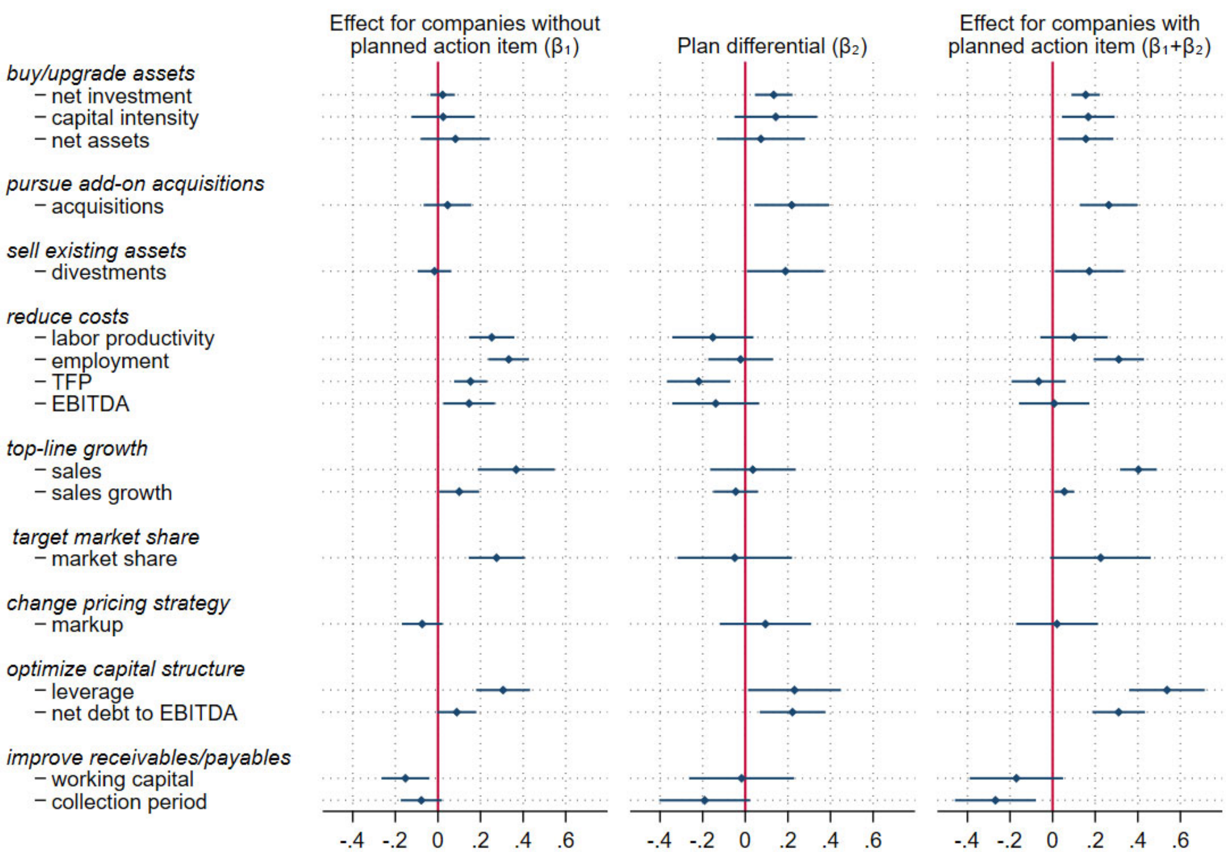
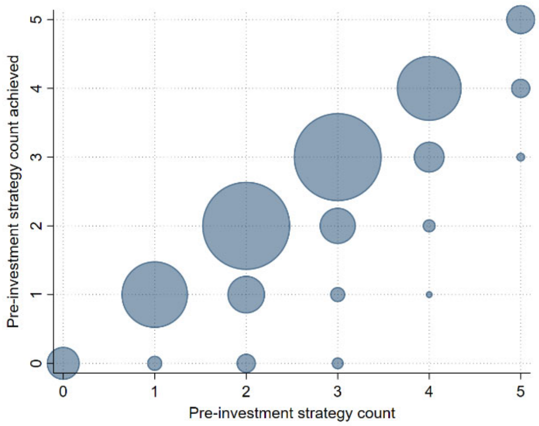
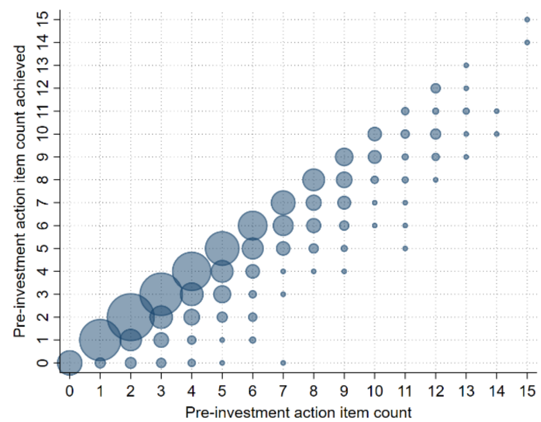


Figure 4. Achievement of value creation plans during the holding period.
The figure shows a scatterplot of how many of the value creation strategies (a) and action items (b) are eventually achieved in a given deal. We code the composition of playbooks using information available at the time of investment in a portfolio company and use all subsequent information to track achievement. The sample size is 1,136 deals. Bubble size represents the number of deals.



(a) Achievement of strategies



(b) Achievement of action items

Table 1. Sample overview.

The sample consists of 1,580 deals by 171 private equity funds investing in Central, Eastern, and Southern Europe, the Baltics, the Commonwealth of Independent States, and the Middle East and North Africa. The PE funds were raised between 1992 and 2017 and made investments between 1992 and 2017. We track each investment through the earlier of the final outcome or December 2020 and record whether it has been "fully realized" (through an IPO or a trade sale, or are written off) or "unrealized" as of December 2020. Tracking each deal over time gives us an unbalanced panel.

Country	1992-1996	1997-2001	2002-2006	2007-2011	2012-2017	Fully realized	Unreal-ized	All deals
Panel A. Number of deals								
All countries	163	501	232	283	401	1,230	350	1,580
Bosnia & Herzegovina		4	5	1	2	10	2	12
Bulgaria	1	13	20	25	7	58	8	66
Croatia		11	6	4	6	22	5	27
Czech Republic	16	39	19	26	13	106	7	113
Estonia	2	24	17	6	15	52	12	64
North Macedonia		12	3		4	16	3	19
Greece			2	3	12	8	9	17
Hungary	12	39	14	11	5	79	2	81
Kazakhstan		12	2	5	16	23	12	35
Latvia		6	11	4	6	22	6	27
Lithuania	1	21	5	5	4	33	2	36
Morocco					18	6	12	18
Poland	54	113	40	57	56	279	41	320
Romania	6	40	16	27	22	87	24	111
Russia	38	121	52	74	96	277	104	381
Serbia			2	4	12	6	12	18
Slovak Republic		20	5	3	5	29	4	33
Slovenia	7	15	1	2	4	25	4	29
Turkey			3	12	76	31	60	91
Ukraine	26	11	9	14	22	61	21	82
Panel B. Deal type								
Early Stage	69	117	27	22	68	240	63	303
Growth	71	330	129	155	255	722	218	940
Buyout	1	9	52	84	60	158	48	206
Secondaries	17	33	22	14	13	84	15	99
Turnaround	5	12	2	8	5	26	6	32
Panel C. Deal size (USD millions)								
Mean	3.4	4.0	13.1	24.3	17.7	9.9	21.1	12.4
Median	2.0	2.3	5.9	13.2	9.6	4.0	10.8	5.0
Panel D. Performance								
Loss ratio								
mean	0.14	0.12	0.10	0.20	0.09	0.13	-	0.13
weighted mean	0.11	0.08	0.08	0.14	0.04	0.10	-	0.10
MOIC								
mean	1.67	2.69	2.78	1.45	1.88	2.28	-	2.28
weighted mean	2.10	2.59	2.53	1.24	1.76	1.88	-	1.88
PME								
mean	1.75	2.34	1.35	1.24	1.67	1.83	-	1.83
weighted mean	2.26	1.53	1.37	1.07	1.56	1.36	-	1.36
Unlevered return								
mean	0.13	0.10	0.19	0.04	0.07	0.12	-	0.12
weighted mean	0.17	0.15	0.16	0.06	0.00	0.11	-	0.11
Abnormal performance								
mean	-0.02	0.00	0.10	0.00	0.01	0.02	-	0.02
weighted mean	0.03	0.04	0.10	0.01	-0.07	0.03	-	0.03

Table 2. Overview of value creation plans.

The table provides an overview of the prevalence of strategies and individual action items in the 1,136 value creation plans in our sample. We code a strategy equal to 1 if at least one action item belonging to that strategy is pursued, and 0 otherwise. Fractions are reported with respect to the total deal count of 1,136 reported in the top row. For variable definitions and details of their construction see the Appendix.

	Value creation plans	
	Deal count	Fraction
Total deal count	1,136	
Operational improvements	951	0.84
…buy/upgrade assets	749	0.66
…sell existing assets	78	0.07
…divest/spin off companies	70	0.06
…reduce costs	293	0.26
…improve IT systems	188	0.17
…improve distribution or logistics	173	0.15
…improve organizational structure	124	0.11
Top-line growth	838	0.74
…target market share	159	0.14
…pursue add-on acquisitions	376	0.33
…change product/services mix	420	0.37
…pursue international expansion	244	0.21
…change pricing strategy	158	0.14
…improve marketing/promotion	356	0.31
…improve quality	114	0.10
Governance engineering	548	0.48
…change CEO	222	0.20
…change CFO	223	0.20
…change other management	298	0.26
…improve corporate governance	52	0.05
…change board/shareholder structure	157	0.14
Financial engineering	395	0.35
…optimize capital structure	346	0.30
…improve incentive systems	93	0.08
Cash management	154	0.14
…improve receivables/payables	126	0.11
…improve inventory management	50	0.04

Table 3. The effect of PE ownership on portfolio-company performance.

The table summarizes estimates of company-level changes in 17 outcome variables during the PE holding period as a function of the presence or absence of an action item related to the outcome variable in question for four of the five strategies in our playbooks. (We lack outcome variables related to the fifth strategy, governance engineering.) Each row corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized deals as well as control firms matched on size, country, industry, and investment year. The number of observations varies depending on the availability of individual data items in Orbis. For each outcome/action-item pair, we estimate regression (1): $y_{it}^{o} = \beta_0 + \beta_1 PE_i * post_{it} + \beta_2 PE_i * post_{it} * plan_i^{o} + \omega_{ct} + \varepsilon_{it}$, where y_{it}^{o} is company i's outcome o in year t, $plan_i^{o}$ is an indicator capturing the presence of an action item related to outcome o in company i's playbook, and PE_i is an indicator set equal to 1 for portfolio companies and 0 for their matched controls. We track each portfolio company and its matched controls, which constitute a "cell" c, from (up to) five years before the PE investment to (up to) five years after the exit or the fifth anniversary if unexited as of the end of our sample period. For each portfolio company and its matched controls, $post_{it}$ equals 1 from the year the PE firm invests in the portfolio company and 0 before, while $plan_i^{o}$ takes the same value in a given cell. Each regression includes a full set of cell-by-year fixed effects, ω_{ct}. For variable definitions and details of their construction see the Appendix. Heteroskedasticity-consistent standard errors clustered at the company level are shown in italics next to the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

		Effect for companies without planned action item: β_1: $PE * post$		Plan differential: β_2: $PE * post * plan$		Effect for companies with planned action item: $\beta_1 + \beta_2$			
Action item	Outcome variable	coeff. (1)	s.e. (2)	coeff. (3)	s.e. (4)	coeff. (5)	s.e. (6)	R-sq. (7)	N (8)
Operational improvements									
buy/upgrade assets	net investment	0.003	*0.004*	0.017***	*0.006*	0.020***	*0.004*	0.389	17,176
	capital intensity	0.042	*0.133*	0.250	*0.172*	0.292***	*0.110*	0.527	16,329
	net assets	0.406	*0.407*	0.363	*0.521*	0.769**	*0.325*	0.396	15,951
pursue add-on acquisitions	acquisitions	0.015	*0.018*	0.070**	*0.029*	0.085***	*0.022*	0.289	19,988
sell existing assets	divestments	-0.003	*0.008*	0.037**	*0.018*	0.034**	*0.017*	0.298	19,988
reduce costs	labor productivity	0.327***	*0.070*	-0.197	*0.126*	0.130	*0.105*	0.561	16,851
	employment	0.546***	*0.080*	-0.036	*0.127*	0.510***	*0.100*	0.657	17,358
	TFP	0.129***	*0.033*	-0.184***	*0.064*	-0.055	*0.055*	0.728	15,993
	EBITDA	0.789**	*0.332*	-0.748	*0.563*	0.041	*0.455*	0.338	18,528
Top-line growth									
top-line growth	sales	0.838***	*0.211*	0.081	*0.233*	0.919***	*0.100*	0.685	19,153
	sales growth	0.051**	*0.024*	-0.023	*0.027*	0.028**	*0.012*	0.393	17,292
target market share	market share	0.044***	*0.011*	-0.008	*0.022*	0.036*	*0.019*	0.495	19,157
change pricing strategy	markup	-0.062	*0.041*	0.080	*0.092*	0.018	*0.083*	0.636	15,993
Financial engineering									
optimize capital structure	leverage	0.068***	*0.014*	0.052**	*0.025*	0.120***	*0.020*	0.349	15,599
	net debt to EBITDA	0.517*	*0.274*	1.293***	*0.459*	1.810***	*0.368*	0.257	15,038
Cash management									
improve receivables/	working capital	-0.054***	*0.020*	-0.006	*0.044*	-0.060	*0.039*	0.357	16,989
payables	collection period	-4.727	*2.982*	-11.551*	*6.589*	-16.279***	*5.875*	0.397	15,109

Table 4. Company-level changes and the cross-section of deal-level investor returns.

The table summarizes estimates of the effects of company-level changes in 17 outcome variables during the PE holding period on four measures of deal-level investor returns: MOIC, PME, unlevered returns, or abnormal performance. Each row represents four regressions, using MOIC, PME, unlevered returns, and abnormal performance as the dependent variable, respectively. The estimation sample in each regression includes all exited deals plus unexited deals that have been held in a PE fund's portfolio for at least four years. The number of observations varies depending on the availability of individual data items in Orbis. For each outcome variable, we estimate regression (2): $y_i^r = \alpha_0 + \alpha_1[x_{i,exit} - x_{i,entry-1}] + \alpha_2 X_i + \xi_t^{entry} + \xi_t^{exit} + \varepsilon_{it}$, where y_i^r is the return on deal i, $[x_{i,exit} - x_{i,entry-1}]$ is the change in the outcome level in question measured over the PE holding period, X_i is a vector of controls (log deal size and log deal duration), and ξ_t^{entry} and ξ_t^{exit} are time dummies for the years of PE investment and PE exit. To conserve space, the table reports only the coefficient of interest, α_1. Both returns and outcomes are winsorized at the 1% level and standardized to have mean 0 and standard deviation 1. For variable definitions and details of their construction see the Appendix. Heteroskedasticity-consistent standard errors are shown in italics next to the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	MOIC			PME			No.	Unlevered return			Abnormal performance			No.
	coeff.	s.e.	R-sq.	coeff.	s.e.	R-sq.	obs.	coeff.	s.e.	R-sq.	coeff.	s.e.	R-sq.	obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Op. improvements														
net investment	0.020	*0.036*	0.114	-0.002	*0.046*	0.157	545	0.018	*0.017*	0.255	0.032^{*}	*0.019*	0.210	357
capital intensity	0.083^{**}	*0.041*	0.142	0.121^{**}	*0.052*	0.191	468	0.048	*0.041*	0.301	0.031	*0.042*	0.262	307
net assets	0.280^{***}	*0.072*	0.222	0.292^{***}	*0.075*	0.236	321	0.103^{**}	*0.042*	0.277	0.117^{***}	*0.038*	0.226	305
acquisitions	0.059^{**}	*0.025*	0.111	0.084^{**}	*0.036*	0.155	758	0.066^{**}	*0.028*	0.244	0.086^{***}	*0.031*	0.217	489
divestments	0.021	*0.022*	0.107	0.036	*0.032*	0.148	758	0.039	*0.029*	0.241	0.046	*0.029*	0.212	489
labor productivity	0.027	*0.038*	0.135	0.046	*0.045*	0.172	473	0.002	*0.042*	0.271	-0.015	*0.044*	0.232	309
employment	0.102^{***}	*0.032*	0.144	0.151^{***}	*0.040*	0.184	523	0.147^{***}	*0.052*	0.308	0.167^{***}	*0.053*	0.284	340
TFP	0.023	*0.033*	0.113	0.016	*0.033*	0.160	483	-0.027	*0.053*	0.199	-0.035	*0.054*	0.160	332
EBITDA	0.123^{***}	*0.031*	0.130	0.126^{***}	*0.031*	0.168	595	0.041	*0.037*	0.225	0.078^{**}	*0.038*	0.190	395
Top-line growth														
sales	0.118^{***}	*0.032*	0.132	0.137^{***}	*0.035*	0.173	603	0.115^{***}	*0.038*	0.263	0.112^{***}	*0.040*	0.222	396
sales growth	0.050^{*}	*0.030*	0.117	0.034	*0.042*	0.161	528	0.064	*0.046*	0.252	0.074	*0.048*	0.209	342
market share	0.026	*0.042*	0.116	0.038	*0.042*	0.154	599	0.081	*0.091*	0.249	0.149^{*}	*0.087*	0.211	396
markup	-0.030	*0.037*	0.113	-0.009	*0.041*	0.159	483	0.012	*0.042*	0.198	-0.001	*0.040*	0.159	332
Financial engineering														
leverage	-0.071	*0.047*	0.123	-0.047	*0.049*	0.171	522	-0.018	*0.037*	0.243	-0.026	*0.038*	0.183	340
net debt to EBITDA	0.023	*0.030*	0.118	0.029	*0.026*	0.169	515	-0.012	*0.038*	0.250	-0.022	*0.043*	0.185	335
Cash management														
working capital	-0.028	*0.041*	0.128	-0.054	*0.043*	0.164	552	-0.038	*0.047*	0.208	-0.038	*0.047*	0.162	367
collection period	0.017	*0.036*	0.162	0.039	*0.040*	0.199	309	-0.060	*0.048*	0.258	-0.078	*0.048*	0.219	294

Table 5. The effects of treatment and selection on deal-level investor returns.
The table links treatment and selection to deal-level returns based on Tables 3 and 4. We categorize the changes in our 17 company-level outcome measures during the PE holding period as reflecting either treatment or selection or both based on the narrative approach in Table 3. We then tabulate their effect on deal-level returns based on the results reported in Table 4. Treatment results in greater returns when planned action items affect both company-level performance and deal-level returns (i.e., when $\beta_2 \neq 0$ in equation (1) and $\alpha_1 \neq 0$ in equation (2)). Selection results in greater returns when portfolio companies undergo changes during the holding period that did not feature in the PE firm's pre-investment value creation plan but that significantly correlate with deal-level returns (i.e., when $\beta_1 \neq 0$ in equation (1) and $\alpha_1 \neq 0$ in equation (2)).

	Effect on company-level performance (Table 3)		Effect on deal-level returns ($\alpha_1 \neq 0$ in Table 4)				Treatment results in greater returns? ($\beta_2 \neq 0, \alpha_1 \neq 0$)				Selection results in greater returns? ($\beta_1 \neq 0, \alpha_1 \neq 0$)			
	Treatment ($\beta_2 \neq 0$)	Selection ($\beta_1 \neq 0$)	MOIC	PME	Un-levered deal return	Abn. perf.	MOIC	PME	Un-levered deal return	Abn. perf.	MOIC	PME	Un-levered deal return	Abn. perf.
Op. improvements														
net investment	yes					yes	no	no	no	yes				
capital intensity			yes	yes										
net assets			yes	yes	yes	yes								
acquisitions	yes		yes	yes	yes	yes	yes	yes	yes	yes				
divestments	yes						no	no	no	no				
labor productivity		yes									no	no	no	no
employment		yes	yes	yes	yes	yes					yes	yes	yes	yes
TFP		yes									no	no	no	no
EBITDA		yes	yes	yes		yes					yes	yes	no	yes
Top-line growth														
sales		yes	yes	yes	yes	yes					yes	yes	yes	yes
sales growth		yes	yes	yes							yes	yes	no	no
market share		yes				yes					no	no	no	yes
markup														
Financial engineering														
leverage	yes	yes					no	no	no	no	no	no	no	no
net debt to EBITDA	yes	yes					no	no	no	no	no	no	no	no
Cash management														
working capital		yes									no	no	no	no
collection period	yes						no	no	no	no				

Table 6. Implementation of value creation plans over the life of a deal.

The table reports the number and fraction of deals in which an initial strategy or action item is achieved. Share achieved in column 4 is the fraction of deals including a particular action item in its initial playbook from Table 2 that successfully implement it. We code a strategy as having been achieved if at least one action item belonging to that strategy is achieved, and 0 otherwise. For variable definitions and details of their construction see the Appendix.

	Number of deals planning action	Fund implements initial playbook		
		Number of deals	Fraction of sample	Share achieved
	(1)	(2)	(3)	(4)
Operational improvements	951	904	0.80	0.95
…buy/upgrade assets	749	698	0.61	0.93
…sell existing assets	78	75	0.07	0.96
…divest/spin off companies	70	56	0.05	0.80
…reduce costs	293	275	0.24	0.94
…improve IT systems	188	154	0.14	0.82
…improve distribution or logistics	173	147	0.13	0.85
…improve organizational structure	124	106	0.09	0.85
Top-line growth	838	762	0.67	0.91
…target market share	159	98	0.09	0.62
…pursue add-on acquisitions	376	280	0.25	0.74
…change product/services mix	420	376	0.33	0.90
…pursue international expansion	244	178	0.16	0.73
…change pricing strategy	158	138	0.12	0.87
…improve marketing/promotion	356	310	0.27	0.87
…improve quality	114	89	0.08	0.78
Governance engineering	548	519	0.46	0.95
…change CEO	222	207	0.18	0.93
…change CFO	223	211	0.19	0.95
…change other management	298	287	0.25	0.96
…improve corporate governance	52	37	0.03	0.71
…change board/shareholder structure	157	139	0.12	0.89
Financial engineering	395	349	0.31	0.88
…optimize capital structure	346	303	0.27	0.88
…improve incentive systems	93	84	0.07	0.90
Cash management	154	137	0.12	0.89
…improve receivables/payables	126	113	0.10	0.90
…improve inventory management	50	41	0.04	0.82

Table 7. Determinants of plan achievement.

The table reports regression results of playbook achievement at the deal-by-action-item level (column 1), at the deal-by-strategy level (column 2), and at the deal level (columns 3 and 4). The unit of analysis in column 1 is action item j pursued in deal i. The unit of analysis in column 2 is strategy k pursued in deal i. The unit of analysis in columns 3 and 4 is deal i. Column 1 is estimated as a linear probability model. Columns 2 through 4 are estimated as fractional logits given that the dependent variable is a fraction bounded on the interval [0,1]. In these columns, we report marginal effects instead of coefficients. For variable definitions and details of their construction see the Appendix. The estimation sample includes only fully realized deals. (In columns 3 and 4, we lack cash flow data and hence deal size for 13 of the 959 fully realized deals.) Standard errors, shown in italics below the coefficient estimates, are clustered at the fund level. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Planned action item achieved? (1)	Share of action items achieved per strategy (2)	Share of action items achieved in a deal (3)	Share of strategies achieved in a deal (4)
Market environment				
GDP growth	0.000	0.002	-0.000	0.002
	0.006	*0.005*	*0.005*	*0.004*
MSCI index growth	0.068	0.066	-0.007	0.013
	0.041	*0.071*	*0.022*	*0.015*
Industry revenue growth	0.008	0.023	0.013	0.000
	0.037	*0.028*	*0.035*	*0.031*
Idiosyncratic factors				
Bad luck	-0.046***	-0.046***	-0.017	-0.015
	0.015	*0.016*	*0.018*	*0.018*
Playbook characteristics				
# action items per strategy	-0.040**	-0.030*		
	0.017	*0.017*		
# action items per deal			0.069***	
			0.011	
# strategies per deal				0.206***
				0.016
… squared	0.004	0.003	-0.005***	-0.035***
	0.003	*0.003*	*0.001*	*0.004*
Deal characteristics				
Log deal duration	0.085***	0.071***	0.087***	0.071***
	0.023	*0.019*	*0.020*	*0.017*
Log deal size	0.025***	0.018*	0.027**	0.014*
	0.009	*0.009*	*0.012*	*0.008*
Majority ownership	0.039**	0.036**	0.030	0.027
	0.016	*0.016*	*0.019*	*0.017*
Inorganic growth	-0.054***	-0.050***	-0.054**	-0.020
	0.018	*0.019*	*0.024*	*0.018*
Deal sequence number	-0.005**	-0.005*	-0.011***	-0.007**
	0.003	*0.003*	*0.003*	*0.003*
Fixed effects				
Deal year FE	Yes	Yes	Yes	Yes
Deal type FE	Yes	Yes	Yes	Yes
Fund FE	Yes	Yes	Yes	Yes
Unit of analysis	Deal by action item	Deal by strategy	Deal	Deal
R-squared/pseudo R-squared	0.126	0.170	0.208	0.386
Number of obs.	4,088	2,326	946	946

Table 8. Playbook plan achievement and the cross-section of deal-level returns.

The table reports estimates of the effect of playbook plan achievement on four alternative measures of deal-level investor returns. The estimation sample includes only fully realized deals, for a sample size of 946 for MOIC and PME (as we lack cash flow data for 13 of the 959 fully realized deals) and 740 for unlevered return and abnormal performance (as we lack cash flow and leverage data for 219 of the 959 fully realized deals). For each return measure, we estimate regression (3): $y_i^r = \delta_0 + \delta_1 share\ achieved_i + \eta_1 X_i + \tau_t^{entry} + \tau_t^{exit} + \omega_f + \epsilon_i$, where y_i^r is the return on deal i and $share\ achieved_i$ is the share of action items that the PE firm successfully implements over the life of deal i. X_i is a matrix of control variables. τ_t^{entry} and τ_t^{exit} are time dummies for the years of PE investment and PE exit. The dependent variable is winsorized at 1%. For variable definitions and details of their construction see the Appendix. Heteroskedasticity-consistent standard errors clustered at the fund level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	MOIC		PME		Unlevered return		Abn. performance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share of action items achieved	0.483^{*}		0.354^{*}		0.126^{*}		0.113^{*}	
	0.255		*0.181*		*0.064*		*0.063*	
x early stage		-0.196		-0.240		0.024		0.008
		0.339		*0.240*		*0.086*		*0.082*
x growth		0.737^{***}		0.573^{***}		0.143^{**}		0.130^{**}
		0.254		*0.192*		*0.065*		*0.064*
x buyout		1.375^{***}		0.929^{***}		0.216^{***}		0.200^{**}
		0.321		*0.226*		*0.077*		*0.078*
x secondaries		0.953^{**}		0.883^{***}		0.234^{**}		0.244^{**}
		0.380		*0.313*		*0.097*		*0.100*
x turnaround		-0.036		0.055		0.003		-0.021
		0.859		*0.664*		*0.155*		*0.156*
Number of planned action items	0.011	-0.001	0.005	-0.006	0.002	0.001	0.000	0.000
	0.038	*0.039*	*0.025*	*0.026*	*0.006*	*0.006*	*0.006*	*0.006*
GDP growth	0.048	0.039	0.040	0.031	0.002	0.001	0.004	0.002
	0.050	*0.050*	*0.038*	*0.039*	*0.010*	*0.010*	*0.010*	*0.009*
MSCI index growth	0.696^{***}	0.676^{**}	0.401^{*}	0.388^{*}	0.107	0.112	0.056	0.063
	0.266	*0.269*	*0.22*	*0.227*	*0.093*	*0.097*	*0.055*	*0.060*
Industry revenue growth	0.706^{*}	0.681^{*}	0.404^{*}	0.382^{*}	0.112^{*}	0.107^{*}	0.099	0.094
	0.376	*0.360*	*0.234*	*0.225*	*0.060*	*0.057*	*0.061*	*0.058*
Bad luck	-1.003^{***}	-0.993^{***}	-0.744^{***}	-0.730^{***}	-0.072^{***}	-0.068^{***}	-0.070^{***}	-0.066^{***}
	0.189	*0.182*	*0.146*	*0.139*	*0.020*	*0.020*	*0.020*	*0.020*
Deal size and duration	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Entry and exit year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fund FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.322	0.341	0.284	0.309	0.477	0.493	0.461	0.480
Number of obs.	946	946	946	946	740	740	740	740

INTERNET APPENDIX
for
Value Creation in Private Equity [†] [♠]

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IA.A Changes during the holding period and returns

To connect our sample to prior literature, this section replicates two canonical findings, namely that portfolio companies undergo significant changes during the PE holding period and that holding-period changes correlate with deal-level returns. Our data allow us to consider a broader range of holding-period changes than have previously been considered in the literature. Specifically, we analyze changes in operational metrics, top-line growth, financial engineering, and cash management, each measured before, during, and after the holding period.

The state of the art in the literature is to use control firms as a benchmark against which to judge whether observed changes are plausibly associated with PE investment or would have happened anyway. In this section, we follow prior literature and compare sample portfolio companies to a set of control companies matched on country, industry, year of investment, and observable financials. The control companies come from narrowly defined "cells" in which firms are likely to experience the same macro and industry shocks or expectations about future profitability as our portfolio companies.[28] Constructing such tight control groups based on observables is similar to the strategy followed by Davis et al. (2014) and Bharath, Dittmar, and Sivadasan (2014) to tackle concerns of unobservable company attributes that may correlate with performance regardless of PE investment.

IA.A1 Sample, data, and measures

We manually search for sample companies in Orbis, a global provider of harmonized financial data for public and private companies. We are able to link 1,373 of the 1,580 sample companies to Orbis by name (including historical ones where names have changed). Table IA.A1 confirms that the Orbis sample is representative of our full sample of 1,580 deals in terms of investor returns. Data gaps in Orbis thus appear to be random, at least in this sense.

[28] Specifically, we estimate the propensity of receiving PE investment by estimating a probit regression on total assets in the investment year t, sales in years $t-1$ and t, employment in year t, and fixed effects for two-digit NACE industry and investment year separately for each country in the sample. We then select up to five companies from the same country-industry-year cell as the portfolio company, chosen to have the closest propensity to receive PE investment as the corresponding portfolio company. We also ensure that control companies have not received any PE investment before and that they are not treated while serving as controls.

Using Orbis data, we construct 17 measures related to four of our five playbook strategies.[29] To track PE firms' operational activities, we measure changes in a company's employment, labor productivity, net investment, capital intensity, net assets, acquisitions and divestments, EBITDA, and total factor productivity (TFP).[30] To track top-line growth activities, we measure changes in sales, sales growth, market share, and price-cost markups.[31] To track financial engineering activities, we measure changes in leverage and net debt to EBITDA. To track cash management activities, we measure changes in working capital and collection period. See the Appendix in the paper for detailed definitions of these variables.

Table IA.A2 reports summary statistics in the form of pre-investment levels and pre-investment trends for each of our 17 outcome variables, separately for portfolio companies and their matched controls, along with t-tests of differences in means. The two groups are generally well balanced on pre-investment observables, especially in terms of the pre-investment levels of net assets, employment, and sales (which we match on) as well as profitability and productivity. The two groups differ in the pre-investment growth rates of key variables such as net assets, employment, and sales. These differences suggest that PE firms target faster growing companies.

IA.A2 Empirical model

To estimate how portfolio companies change during the PE holding period, we estimate regressions of the following form:

$$y_{it}^{o} = \gamma_0 + \gamma_1 PE_i * postPE_{it} + \gamma_2 postPE_{it} + \gamma_3 PE_i * postPE_{it} * exit_{it} + \gamma_4 postPE_{it} * exit_{it} + \varphi_{ct} + \varepsilon_{it} \qquad \text{(IA.A1)}$$

where y_{it}^{o} is company i's outcome o in year t and PE_i is an indicator set equal to 1 for PE portfolio companies and 0 for their matched controls. For portfolio companies, $postPE_{it}$ equals 1 in the years

[29] We are unable to find data with which to track governance activities as Orbis does not retain historic data on such governance measures as the number of shareholders, ownership concentration, or board composition.

[30] TFP captures the efficiency with which labor, materials, and capital are used. We follow the production-function approach to TFP estimation pioneered by Olley and Pakes (1996), Levinsohn and Petrin (2003), and Ackerberg et al. (2006). This approach deals with the challenge that input choices are likely correlated with the error term, given that companies choose inputs based on unobserved future productivity. For further details of the approach, see Section IA.B in the Internet Appendix.

[31] We follow De Loecker and Warzynski (2012) in deriving company-level markups from a production-function framework. Details of the approach can be found in Section IA.C in the Internet Appendix.

following the first PE funding round and 0 before. For control companies, $postPE_{it}$ equals 1 in the years after their matched target first received PE funding and 0 before.

Our main coefficient of interest is γ_1, which is identified from the interaction of PE_i and $postPE_{it}$. We estimate model (IA.A1) with a full set of cell-by-year (φ_{ct}) fixed effects, where each cell contains a portfolio company and its matched controls. The inclusion of these fixed effects ensures that the coefficients are identified off changes for a portfolio company relative to its matched controls. Given this demanding specification, $postPE_{it}$ and $postPE_{it} * exit_{it}$ drop out of the estimation. We cluster standard errors at the company level, as disturbances to a company's operations and performance are potentially correlated over time.

We track portfolio companies that are fully realized for up to five years post-exit and portfolio companies that are not fully realized until the end of our sample period. This allows us to isolate changes that manifest themselves during the PE holding period and test whether these changes persist or abate post-exit. To this end, equation (IA.A1) includes the interaction term $PE_i * postPE_{it} * exit_{it}$, where $exit_{it}$ equals 1 post-exit and 0 otherwise.[32] The γ_3 coefficient on this additional interaction term captures any incremental post-exit effects, over and above the average impact of PE investment captured by the γ_1 coefficient (and relative to control companies). If the sign of γ_3 disagrees with the sign of γ_1, the effect realized during the PE holding does not persist and reverts toward the pre-PE investment level. To estimate the long-term effect of PE investment, we report the linear combination $\gamma_1 + \gamma_3$, which compares the sum of the holding-period effect and the post-exit effect to the pre-investment level of the outcome variable in question.

Model (IA.A1) generalizes the approach of Acharya et al. (2013) by considering a broader range of 17 outcome variables and by using a regression framework that distinguishes between the pre-period, the holding period, and the post-exit period.[33]

[32] Our data identify the buyers when deals are exited. We code as exits only strategic sales, IPOs, or write-offs. In secondaries involving PE buyers, we define $postPE_{it}$ to equal 0 only after the last PE fund has exited the company.
[33] Specifically, Acharya et al. (2013) report sample averages for changes in three deal-level variables (sales, margins, and multiples) between the pre-period and the holding period.

IA.A3 How do portfolio companies change during the PE holding period?

The results of estimating equation (IA.A1) for each of our 17 outcome variables are summarized in Table IA.A3 and visualized in Figure IA.A1. Table IA.A3 reports one regression per row, focusing on the coefficients of interest γ_1 (capturing average changes during the PE holding period), γ_3 (capturing persistence effects), and the linear combination $\gamma_1 + \gamma_3$ (capturing long-run effects).

Consistent with the hypothesis that PE firms effect changes in their portfolio companies during the holding period, Table IA.A3, columns 2 and 3 show that a majority of the 17 outcome variables we consider change significantly, over and above contemporaneous changes at control companies. On the operational side, portfolio companies significantly increase employment, labor productivity, and investments (as measured by either net investment, capital intensity, net assets, or acquisitions). These operational improvements translate into higher TFP and profitability (as measured by EBITDA) during the holding period. On the top-line side, portfolio companies significantly increase the level of sales, sales growth, and market share while leaving markups on their products unchanged on average. On the financial engineering side, portfolio companies significantly increase their leverage and net debt to EBITDA ratios. On the cash management side, portfolio companies significantly reduce their working capital needs and collection periods. Plotting standardized coefficients, Figure IA.A1 shows that the largest changes are the increases in sales, leverage, employment, labor productivity, and market share.

A key caveat is that a causal interpretation of the results in Table IA.A3 requires parallel trends, i.e., that treated and controls are only randomly different from each other. This assumption looks to be violated in our setting, in light of the significant differences in some pre-investment growth rates (see Table IA.A2). A violation of the parallel-trends assumption is to be expected in a setting in which selection on unobservables is likely. The well-known difficulty in establishing a plausible counterfactual by way of matched-control companies is one reason why our study adopts a narrative approach instead.

Figure IA.A2 complements the estimates of average PE holding effects shown in Table IA.A3 and Figure IA.A1 with estimates for the cross-section of selected outcome variables obtained from quantile regressions. This reveals that the scope for holding-period changes varies in the cross-section with each company's starting position in ways that sit well with economic intuition. While all companies increase employment significantly, employment increases are larger at smaller companies. Labor productivity too increases significantly and uniformly across companies. Increases in net investment are not statistically significant for portfolio companies at the bottom of the distribution and they are largest (and statistically significant) for companies towards the top end of the distribution. Increases in sales, uniformly statistically significant throughout the distribution, are largest among the largest companies, leading to outsized gains in market share for companies with already high market shares. The increase in leverage during the holding period is slightly larger for companies with higher levels of initial leverage. Finally, while all companies improve their use of working capital significantly (and most also improve their collection period), it is the most working-capital intensive companies (and companies with the longest collection periods initially) that see the largest improvements.

Most of the changes in average outcomes in Table IA.A3 persist beyond the PE firm's exit from the company (in the sense that $\gamma_3 = 0$ for most outcome variables; see columns 4 and 5). There is some evidence of a reduction in labor productivity, sales, and net debt to EBITDA following the PE firm's exit. The estimated reduction in labor productivity is around a third (= –0.134/ 0.335) of the holding-period effect; for sales it is around a quarter (= –0.247/0.968), and for net debt to EBITDA it is around 80% (= –0.956/1.185).

The long-run effects $\gamma_1 + \gamma_3$ shown in column 6 confirm that a stint in a PE firm's portfolio is associated with fundamental changes: compared to the period before they received PE investment (and relative to matched controls), in the five years post-exit, portfolio companies employ significantly more people and operate with greater labor productivity and higher net investment. They also sell more, both in absolute terms and relative to their competitors, with the increase in sales and market share accompanied by no changes in price-cost markups. Leverage remains elevated (averaging nearly eight percentage points of total assets more than before), although average levels of net debt to EBITDA, while positive, are not statistically higher in the long run than before. Reductions in working capital needs, on the other hand, prove more persistent.

Figure IA.A1. Company-level changes during and after the PE holding period.

The figure graphs estimates of company-level changes in 17 outcome variables during and after the PE holding period, as specified in equation (IA.A1). Each row of plotted coefficients corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized deals as well as control firms matched on size, country, industry, and investment year. Coefficient estimates are standardized to have mean 0 and standard deviation 1 to make them comparable across the 17 outcome variables. Error bands indicate 95% confidence intervals (clustered at the company level). Full regression results are summarized in Table IA.A3. For variable definitions and details of their construction see the Appendix in the paper.

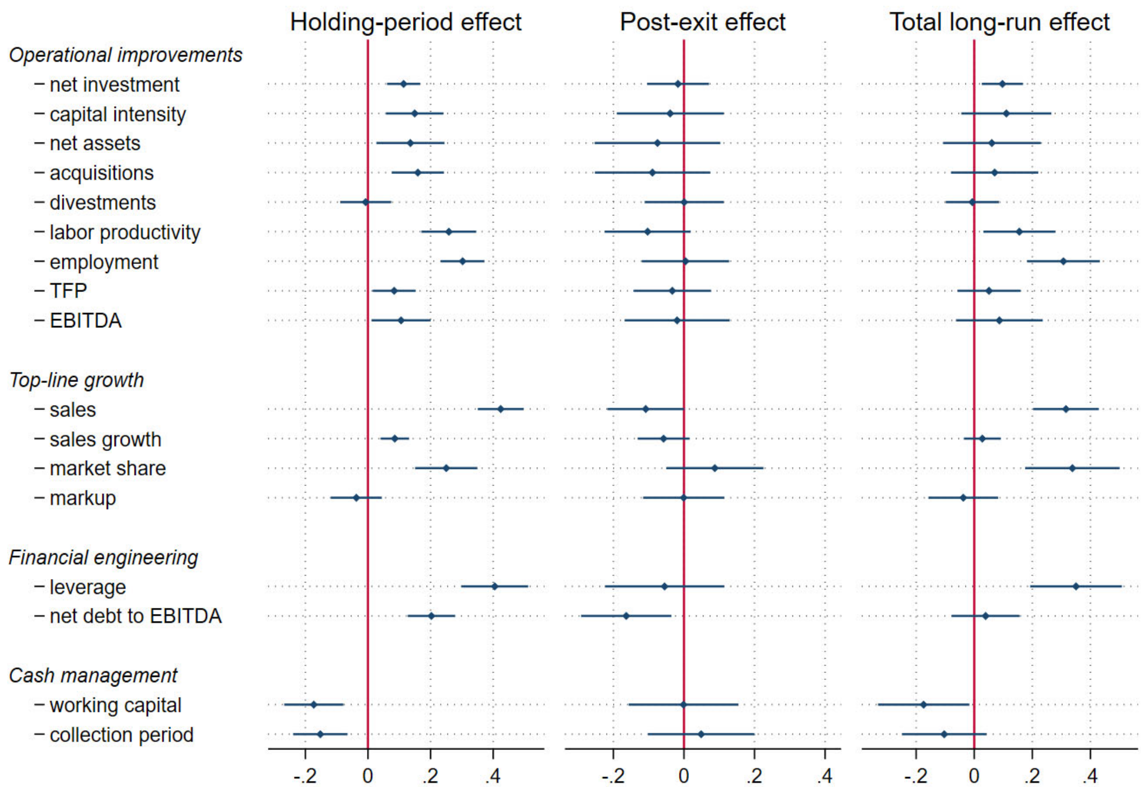
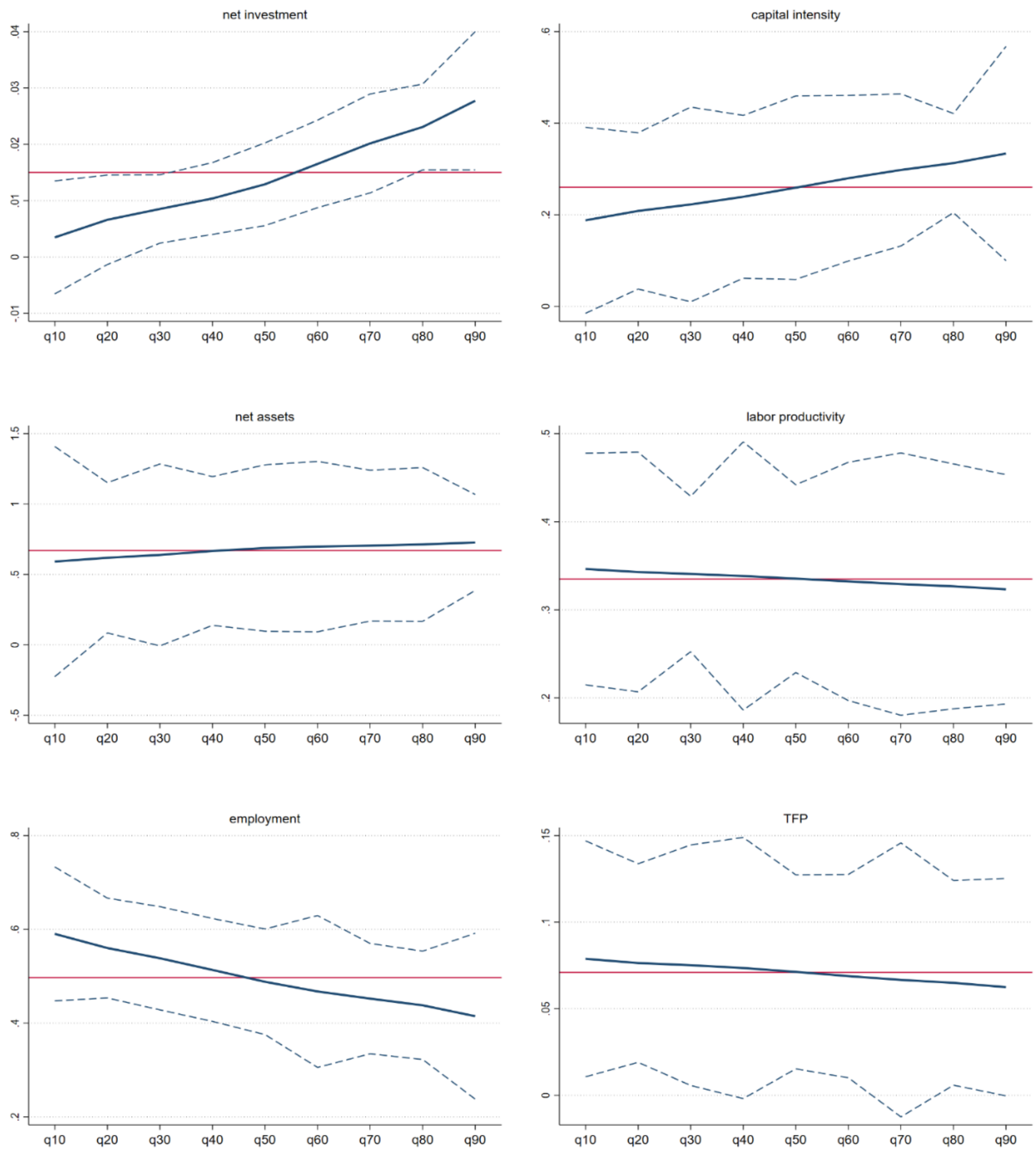
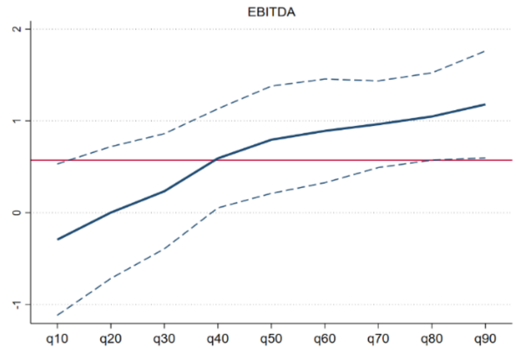


Figure IA.A2. Quantile regression estimates for the PE holding-period effect.

The figure graphs quantile-regression estimates of the PE holding-period effect for selected variables. We estimate decile-level quantile regressions of equation (IA.A1) and report the PE ownership effect γ_1. The horizontal red line indicates the corresponding OLS estimate from Table IA.A3. The dashed lines indicate 95% bootstrapped confidence intervals.

Operational improvements




EBITDA
2
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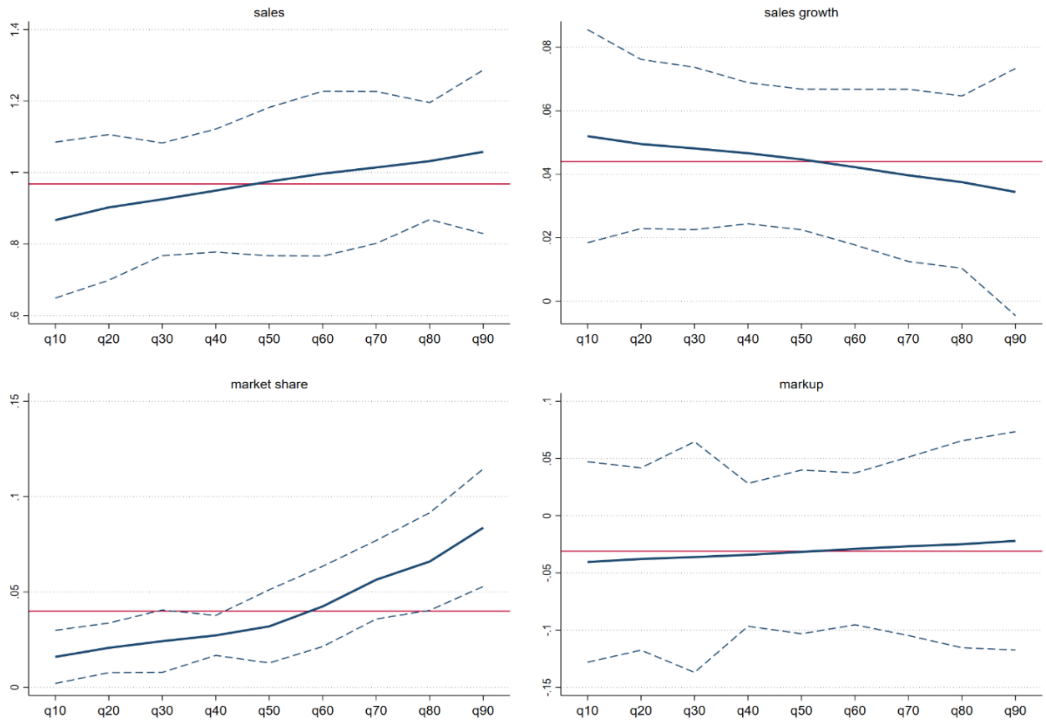
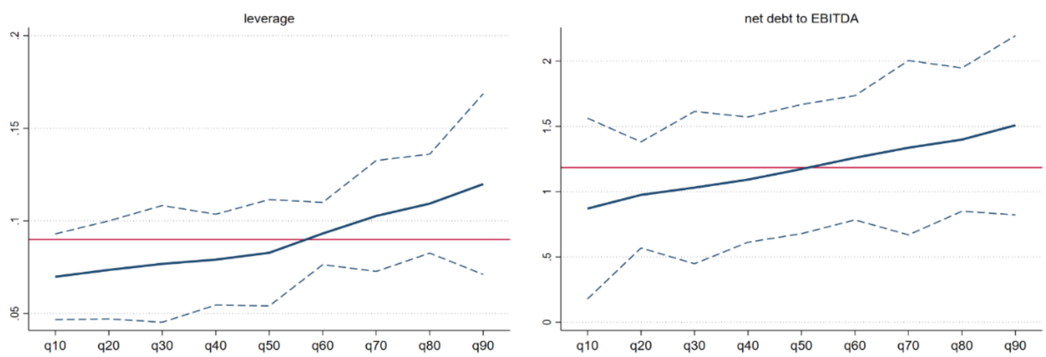
q70
q80
q90

Top-line growth



Financial engineering



Cash management

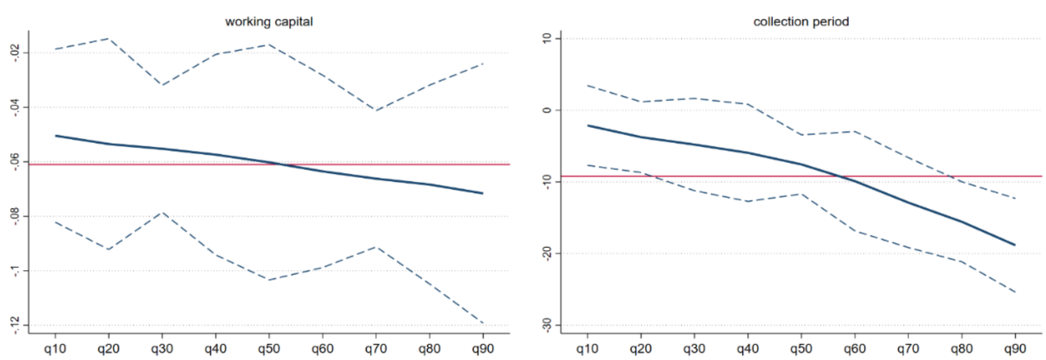


Table IA.A1. Deal-level returns across the samples used in the analysis.

The table reports summary statistics on deal-level investor returns for the full sample, the playbook sample, and the Orbis sample of companies. Weighted means report returns weighted by investment cost. For variable definitions and details of their construction see the Appendix in the paper. We lack cash flow data for 18 of the 1,230 sample deals in Panel A, for 13 of the 1,035 sample deals in Panel B, and for 15 of the 1,076 sample deals in Panel C.

		MOIC		PME		Unlevered return		Abnormal performance	
	N	Mean	Weighted mean	Mean	Weighted mean	Mean	Weighted mean	Mean	Weighted mean
A. Full sample									
Fully realized	1,212	2.28	1.88	1.83	1.36	0.12	0.11	0.02	0.03
B. Playbook sample									
Fully realized	1,022	2.39	1.89	1.91	1.34	0.12	0.11	0.02	0.03
C. Orbis sample									
Fully realized	1,061	2.40	1.90	1.91	1.36	0.13	0.11	0.03	0.04
D. Two-sample *t*-tests of equality of means (full sample less playbook sample)									
t-stat.		-0.23		-0.12		0.00		0.00	
p-value		0.82		0.91		1.00		1.00	
E. Two-sample *t*-tests of equality of means (full sample less Orbis sample)									
t-stat.		-0.25		-0.13		-0.18		-0.21	
p-value		0.80		0.90		0.86		0.83	

Table IA.A2. Pre-investment characteristics of portfolio and control companies.
The table reports summary statistics for 17 company-level outcome variables grouped by strategy, separately for portfolio companies and their matched control companies. For pre-investment levels, each variable is averaged over the three years preceding the first year of PE funding. For pre-investment trends, we calculate the change from the previous year to the first year of PE funding for each variable. All dollar amounts are reported in thousands. The *p*-values for the differences in means are derived from heteroskedasticity-consistent standard errors.

	Pre-investment levels					Pre-investment trends				
	Portfolio	Control	Difference in means			Portfolio	Control	Difference in means		
	companies	companies	Diff.	*t*-stat	*p*-value	companies	companies	Diff.	*t*-stat	*p*-value
Operational improvements										
net investment	0.07	0.05	0.03	4.20	0.00	0.00	-0.02	0.02	1.28	0.20
capital intensity	87.6	102.6	-15.0	-1.79	0.07	11.3	1.4	9.9	1.68	0.09
net assets	10,678.4	11,747.6	-1,069.3	-0.99	0.32	3,189.0	197.6	2,991.4	3.93	0.00
acquisitions	0.05	0.02	0.03	2.64	0.01	0.02	0.01	0.01	0.80	0.42
divestments	0.01	0.01	0.00	0.03	0.98	0.02	0.00	0.02	0.94	0.35
labor productivity	151.6	138.2	13.4	1.40	0.16	3.6	2.3	1.3	0.21	0.84
employment	294.3	270.2	24.1	1.18	0.24	31.0	8.5	22.5	2.74	0.01
TFP	1.69	1.60	0.09	1.89	0.06	-0.05	-0.01	-0.04	-2.07	0.04
EBITDA	2,901.8	2,501.3	400.6	1.53	0.13	-84.6	30.1	-114.7	-0.60	0.55
Top-line growth										
sales	32,199.8	30,375.1	1,824.7	0.68	0.50	3,807.7	1,117.4	2,690.3	2.07	0.04
sales growth	0.38	0.24	0.14	5.16	0.00	-0.10	-0.11	0.01	0.15	0.88
market share	0.12	0.08	0.04	3.88	0.00	0.00	0.00	0.00	0.80	0.42
markup	2.27	2.19	0.08	0.51	0.61	-0.02	0.06	-0.08	-1.17	0.24
Financial engineering										
leverage	0.22	0.17	0.06	5.07	0.00	0.00	0.00	0.00	-0.25	0.80
net debt to EBITDA	1.04	0.56	0.49	2.10	0.04	-0.55	0.09	-0.63	-1.65	0.10
Cash management										
working capital	0.34	0.36	-0.02	-1.11	0.27	0.01	0.00	0.01	0.49	0.63
collection period	59.3	68.8	-9.53	-3.39	0.00	2.8	3.6	-0.79	-0.27	0.78

Table IA.A3. Company-level changes during and after the PE holding period.

The table summarizes estimates of company-level changes in 17 outcome variables during and after the PE holding period. Each row corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized deals as well as control firms matched on size, country, industry, and investment year. The number of observations varies depending on the availability of individual data items in Orbis. For each outcome variable, we estimate regression (IA.A1): $y_{it}^{o} = \gamma_0 + \gamma_1 PE_i * postPE_{it} + \gamma_2 postPE_{it} + \gamma_3 PE_i * postPE_{it} * exit_{it} + \gamma_4 postPE_{it} * exit_{it} + \varphi_{ct} + \varepsilon_{it}$, where y_{it}^{o} is company i's outcome o in year t and PE_i is set equal to 1 for companies receiving a PE investment and 0 for companies in the control group. We track each portfolio company and its matched controls, which constitute a "cell" c, from (up to) five years before the PE investment to (up to) five years after the exit or the fifth anniversary if unexited as of the end of our sample period. For portfolio companies, $postPE_{it}$ equals 1 in the years following the first PE funding round and 0 before. Each regression includes a full set of cell-by-year fixed effects, φ_{ct}. For variable definitions and details of their construction see the Appendix in the paper. Heteroskedasticity-consistent standard errors clustered at the company level are shown in italics next to the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	pre-PE mean	Holding-period effect: γ_1: $PE_i * postPE_{it}$		Post-exit effect: γ_3: $PE_i * postPE_{it} * exit_{it}$		Total long-run effect: $\gamma_1 + \gamma_3$		R-sq.	N
		coeff.	s.e.	coeff.	s.e.	coeff.	s.e.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Operational improvements									
net investment	0.07	0.015***	*0.003*	-0.002	*0.006*	0.012***	*0.005*	0.381	21,855
capital intensity	87.6	0.260***	*0.082*	-0.068	*0.135*	0.192	*0.138*	0.500	20,781
net assets	10,679	0.670**	*0.273*	-0.371	*0.449*	0.299	*0.423*	0.392	19,239
acquisitions	0.05	0.051***	*0.014*	-0.029	*0.027*	0.023	*0.025*	0.282	25,511
divestments	0.01	-0.001	*0.008*	0.000	*0.011*	-0.001	*0.009*	0.284	25,511
labor productivity	151.6	0.335***	*0.058*	-0.134*	*0.081*	0.201**	*0.082*	0.569	21,569
employment	294.3	0.497***	*0.058*	0.007	*0.104*	0.505***	*0.105*	0.652	22,171
TFP	1.69	0.071**	*0.030*	-0.028	*0.047*	0.043	*0.047*	0.721	20,502
EBITDA	2,901.8	0.572**	*0.259*	-0.105	*0.410*	0.467	*0.411*	0.344	23,223
Top-line growth									
sales	32,200	0.968***	*0.085*	-0.247*	*0.126*	0.720***	*0.132*	0.693	24,498
sales growth	0.38	0.044***	*0.012*	-0.029	*0.019*	0.014	*0.017*	0.387	22,078
market share	0.12	0.040***	*0.008*	0.014	*0.011*	0.055***	*0.013*	0.495	24,501
markup	2.27	-0.031	*0.035*	-0.001	*0.050*	-0.032	*0.052*	0.639	20,502
Financial engineering									
leverage	0.22	0.090***	*0.012*	-0.012	*0.019*	0.078***	*0.018*	0.354	19,579
net debt to EBITDA	1.04	1.185***	*0.226*	-0.956**	*0.382*	0.229	*0.349*	0.259	18,896
Cash management									
working capital	0.34	-0.061***	*0.017*	0.000	*0.028*	-0.061**	*0.028*	0.355	21,250
collection period	59.3	-9.217***	*2.689*	2.953	*4.672*	-6.263	*4.503*	0.405	18,116

IA.B Estimating productivity

Assume output is given by $Y = L^{\beta_l} K^{\beta_k} M^{\beta_m} * \Omega$, where Ω is an unobserved technology parameter and L, K, and M are labor, capital, and materials, respectively. TFP is typically calculated as the residual in a Cobb-Douglas production function in logs:

$$y_{it} = \beta_l l_{it} + \beta_k k_{it} + \beta_m m_{it} + \omega_{it} \tag{IA.B1}$$

where y_{it} denotes output, l_{it} denotes labor inputs, k_{it} denotes the capital stock, m_{it} denotes material inputs, and ω_{it} denotes unobserved productivity for company i at time t. The residual from a regression of output on the three inputs should therefore give us TFP. However, as Marschak and Andrews (1944) were first to point out, such a regression suffers from endogeneity: input choices are correlated with the error term because companies are likely to choose their inputs based on their productivity, which is observed to the company but not to the econometrician. OLS estimates of the coefficients in equation (IA.B1) and the error term are then biased.

To address this endogeneity, researchers either follow the dynamic panel literature (as in Bharat, Dittmar, and Sivadasan 2014) or use the structural methods pioneered by Olley and Pakes (1996) and Levinsohn and Petrin (2003).[34] The latter use observed input decisions to control for unobserved productivity shocks. The two methods essentially differ in their assumptions about how unobserved productivity evolves to identify the coefficients in equation (IA.B1). In structural models, unobserved productivity follows an arbitrary first-order Markov process,

$$\omega_{i,t+1} = g(\omega_{it}) + \xi_{i,t+1}, \tag{IA.B2}$$

where $g(.)$ is any non-parametric function and $\xi_{i,t+1}$ is a shock to productivity. In contrast, dynamic panel models have to make the more restrictive assumption that the Markov process is parametric and linear.

Given their ability to accommodate arbitrary productivity processes, we estimate TFP using

[34] See Ackerberg et al. (2006) for a detailed discussion of problems encountered in the identification of production functions and how structural methods differ from the use of dynamic panel estimators.

structural methods. We implement the methodology with a Cobb-Douglas production function as in equation (IA.B1), subject to the productivity process in equation (IA.B2). As companies may differ across countries or industries in the intensity with which they use each input, we estimate the production function separately for each country and industry pair.[35] This allows for differences in technology across industry-country pairs. We measure capital stock as the reported book value of fixed assets and labor inputs as total staffing costs.[36] We deflate all values by the appropriate country and industry level deflator, which transforms them into real values, stripped of the effect of price changes.[37]

We closely follow Ackerberg et al. (2006) and De Loecker and Warzynski (2012) in obtaining estimates of the production function. Estimation proceeds in two stages. In the first stage, we obtain predicted output by estimating equation (IA.B1) via OLS in the universe of companies available in the Orbis database. In the second stage, we compute the company's unobserved productivity ω_{it} using predicted output and regress it on a third-order polynomial approximation of past productivity (i.e., we approximate function $g(.)$ in equation (IA.B2) non-parametrically) to recover the productivity shocks $\xi_{i,t+1}$. The production-function coefficients are then identified by using standard GMM techniques on the following moment conditions:

$$E\left[\xi_{it} | l_{i,t-1}, k_{it}, m_{i,t-1}\right] = 0. \qquad \text{(IA.B3)}$$

Once we obtain a consistent set of production-function coefficients, we calculate a company's time-varying (log) TFP as follows:

$$\widehat{\omega}_{it} = y_{it} - \hat{\beta}_l l_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_m m_{it}. \qquad \text{(IA.B4)}$$

We note that company-level expenditures on materials and staff costs are not always available in Orbis. In particular, some countries (Greece, Kazakhstan, Latvia, Lithuania, Russia, Turkey, and

[35] We use Rev. 2 of NACE as our industry grouping.
[36] We prefer using total staffing costs instead of number of employees. Staffing costs better capture the skill composition of a company's workforce assuming that more skilled employees receive higher wages. Our TFP estimates are then less affected by the skill composition of a company's labor force.
[37] Deflators for capital goods and output are separately available for most of the countries in our sample at the 2-digit NACE Rev. 2 industry level either through Eurostat or the OECD. At its most detailed level, this corresponds to 64 industries, although deflators for capital goods are typically provided at a more aggregate level. Where Eurostat or the OECD does not provide deflators for sample countries, we rely on local sources such as national central banks and statistical institutes or the World Bank's World Development Indicators to obtain this information.

Ukraine) provide better coverage for total cost of goods sold than for materials and staff costs separately. In these cases, we follow De Loecker, Eeckhout, and Unger (2020) and estimate a production function with two (rather than three) inputs. Specifically, for this subset of countries, we estimate the following production function by industry:

$$y_{it} = \beta_k k_{it} + \beta_v v_{it} + \omega_{it} \tag{IA.B5}$$

where v_{it} denotes total cost of goods sold, subject to the productivity process in equation (IA.B2). The two-step estimation procedure that uses the moment conditions in equation (IA.B3) and described above then yields consistent estimates of the coefficients on cost of goods sold alongside capital. We then calculate (log) TFP as:

$$\widehat{\omega}_{it} = y_{it} - \hat{\beta}_k k_{it} - \hat{\beta}_v v_{it}. \tag{IA.B6}$$

IA.C Estimating price-cost markups

We follow De Loecker and Warzynski (2012) in deriving company-level markups from a production-function framework. De Loecker and Warzynski's approach assumes cost-minimizing producers who have access to a variable input of production (e.g., materials or labor) and relies on the insight that the output elasticity of this variable input equals its expenditure share in total revenue when price equals marginal production cost (i.e., when markup = price/marginal cost = 1). Under imperfect competition, companies can charge a price above marginal cost, thereby introducing a wedge between the input's revenue share and its output elasticity. The ratio of any input's output elasticity to the input's revenue share then provides a consistent estimate of a company's markup.

We obtain estimates of output elasticities for variable inputs from our production-function estimation as described in Section IA.B in the Internet Appendix. We choose materials as the variable input of production to calculate markups, since materials are more likely to respond to productivity shocks than labor, which is subject to potentially large hiring and firing costs. Using materials, we recover markups from

$$\mu_{it} = \hat{\beta}_m / \alpha_{it}^M, \tag{IA.C1}$$

where $\hat{\beta}_m$ is the estimated output elasticity of materials from equation (IA.B1) and α_{it}^M is the share of expenditures on materials in total company revenue. Following De Loecker and Warzynski (2012), we correct markup estimates for the presence of measurement error in revenues. That is, we calculate α_{it}^M as the ratio of reported expenditures on materials to predicted company revenues from equation (IA.B1).

As mentioned in Section IA.B in the Internet Appendix, countries vary in terms of their reporting of materials and staffing costs in the Orbis database. The methodology by De Loecker and Warzynski (2012) allows us to estimate markups consistently using the cost of goods sold alongside capital when a more detailed breakdown of variable input use—i.e., labor costs and material costs—is unavailable. We therefore follow De Loecker, Eeckhout, and Unger (2020) in calculating markups based on estimates from a production function with two inputs for the set of countries listed in Section IA.B. In particular, the price-cost markup in these countries is given by

$$\mu_{it} = \hat{\beta}_v / \alpha_{it}^V,$$

where $\hat{\beta}_v$ is the estimated output elasticity of cost of goods sold from equation (IA.B5) and α_{it}^V is the share of cost of goods sold in total company revenues. We again correct markup estimates for the presence of measurement error as in De Loecker, Eeckhout, and Unger (2020).

Ideally, we would like to have quantity data on output and inputs so that price differences across companies (e.g., due to variation in quality or transfer pricing) do not distort estimation. De Loecker and Warzynski (2012) show that when relying on company revenue data, only the level of the markup is potentially affected by lack of data on physical output, but not the estimate of the correlation between markups and company-level characteristics or how markups change within a company over time. This means that we are fortunate: while we do not observe measures of physical output, our focus is on understanding how a portfolio company's markups change over time and how this change correlates with other company-level characteristics.

IA.D A closer look at PE value creation plans

We have detailed information about the PE firm's value creation plan for 1,136 of the 1,580 deals in our sample.[38] Value creation plans (or playbooks) consist of one or more overarching strategies and specific action items. Based on Gompers et al.'s (2016) survey of PE firms' sources of value creation and our reading of the playbooks in our sample, we distinguish five strategies: financial engineering, operational improvements, cash management, top-line growth, and governance engineering. As Table 2, Panel A in the paper shows, the two most popular strategies in our sample are operational improvements and top-line growth, which feature in 84% and 74% of sample playbooks, respectively. Governance engineering and financial engineering feature in roughly half (48%) and a third (35%) of playbooks, respectively. Improvements in cash management feature less often (14%).[39]

It is common for playbooks to span multiple strategies. In our sample, 929 playbooks (or 82%) do so. Table IA.D1 shows that the 10 most popular combinations account for 80% of sample playbooks (twice as many as in a uniform distribution). Eight of the top 10 involve either operational improvements or top-line growth or both. Governance engineering features in six of the top 10, with financial engineering and cash management in three and two of the top 10 combinations, respectively. The three most popular combinations involve both operational improvements and top-line growth, either with no other strategy (18%) or in combination with governance engineering (15%), or with governance and financial engineering (11%).

We track 23 distinct action items, which Table 2, Panel A groups into our five strategies. (See the Appendix in the paper for detailed definitions.) PE firms follow a rich variety of plans to add value to their portfolio companies. Table IA.D2 provides a breakdown of the 10 most popular combinations, which account for 11.6% of sample playbooks—a vastly greater fraction than if combinations were distributed uniformly, suggesting bunching. All 10 include planned purchases/upgrades of physical assets. The most popular combination features in 3.5% of deals. It includes two action items: in addition

[38] We lose 124 deals for which we cannot find pre-deal documentation, even though the EBRD's archive contains post-deal documentation. These 124 investments do not look observably different from our other sample deals, mitigating selection concerns. We lose a further 320 unexited deals with investment dates in 2015-2017 because we require five years of post-investment textual data and our textual data end in 2019.

[39] In 38 deals (3.3%), the PE fund did not formulate a value creation plan at the outset, though it did so post-investment.

to asset purchases/ upgrades, the plan is to optimize the portfolio company's capital structure.

Table IA.D3 provides a breakdown of strategies and action items over time, aggregated into five-year periods starting in 1992. Figure IA.D1(a) illustrates the trends at the strategy level. The popularity of growth strategies has doubled, from 41% of deals in 1992-1996 to 83% in 2012-2017. Governance engineering has become three times more popular, increasing from 24% to 74% of deals, while financial engineering has nearly quadrupled, from 13% to 51% of deals. The popularity of operational improvements—always high—has increased from 76% to 81% of deals. Strategies aimed at cash management have never been particularly popular in our sample, though even they have seen an increase, from 7% to 18% of deals. Each of these time trends is statistically significant at the 5% level or better.

At the action item level, there is much more variation in popularity over time. Notably, "purchases/upgrades of physical assets" have become relatively less popular (falling from 71% to 58% of deals), and "add-on acquisitions" were particularly popular during the 2007-2011 period, which coincides with the global financial crisis.

Value creation plans vary systematically with deal type, fund ownership, growth strategy, and geographic focus. By way of overview, we find that PE firms formulate plans that are more hands-on in buyouts than in early-stage deals or turnarounds, when they have majority ownership, when they pursue inorganic growth strategies, and when they manage a regional fund rather than a country-focused fund. We next discuss these patterns in greater detail.

Table IA.D4 shows that strategies differ considerably across deal types. Operational improvements are popular in all deal types, while the popularity of top-line growth, governance engineering, and financial engineering varies significantly. The popularity of top-line growth and governance engineering strategies increases as the maturity of deals increases, with 56% of early-stage, 77% of growth, and 88% of buyout deals planning to boost top-line growth and 39% of early-stage, 47% of growth, and 62% of buyout deals planning governance engineering. Buyouts stand out for their focus on financial engineering, which 54% of deals intend to engage in. Secondaries look similar to buyouts on most dimensions, except with less focus on financial engineering (32%), suggesting diminishing marginal returns to optimizing capital structure and incentive systems as buyout targets are sold on to the next PE owner. Turnaround deals are the least focused on top-line growth (53%) and plan on governance engineering around as rarely as do early-stage deals (41%) but show the greatest focus on financial engineering (59%). Cash management does not vary significantly in popularity across deal types. Figure IA.D1(b) illustrates these patterns graphically.

Which action items PE firms include in their value creation plans depends on the type of deal. Buyouts tend to focus on optimizing capital structure, add-on acquisitions, changing the product or service mix, and replacing senior or middle management. Early-stage and growth deals, on the other hand, tend to focus primarily on capital expenditures and pursue other action items more opportunistically.

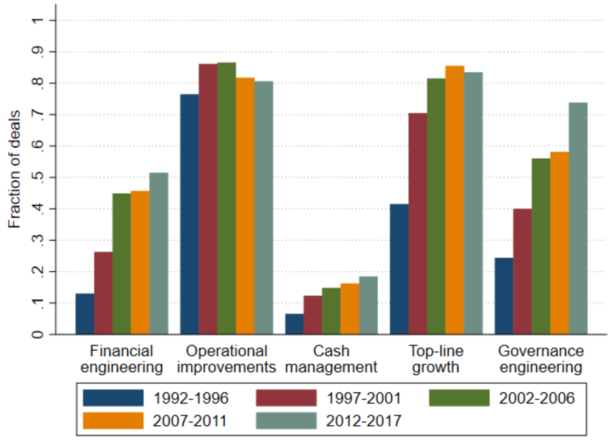
Our data allow us to observe each deal's ownership structure. Most deals (71%) are minority investments (see Table IA.D5). PE firms tend to pursue hands-on strategies significantly less often in minority-owned deals. In particular, they plan to pursue growth, governance engineering, and financial engineering in 72%, 45%, and 31% of their minority-owned deals, compared to 79%, 56%, and 43% of their majority-owned deals. Strategies aimed at operational improvements and cash management do not vary significantly with ownership. Figure IA.D1(c) illustrates.

Portfolio companies can grow organically (by increasing the sales and revenues of existing or new products) or inorganically (by acquiring other companies). Around a third of sample deals plan to grow through acquisition (see Table 2, Panel D in the paper). Inorganic growth is associated with a significantly greater focus on other action items in the top-line growth bucket, such as "target market share," "change product/services mix," and "pursue international expansion." Inorganic deals also more often plan to implement strategies aimed at governance engineering (59% vs. 43%), financial engineering (42% vs. 31%), and cash management (17% vs. 12%). Figure IA.D1(d) illustrates.

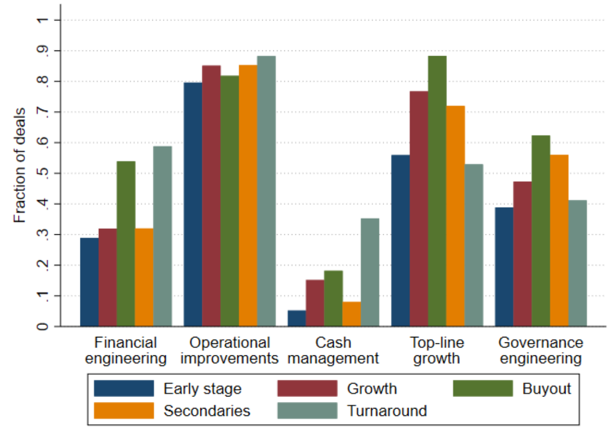
The final breakdown in Table IA.D5 is by geographic focus. A little over half of sample deals (54%) are managed by single-country funds; the remainder involve a regional fund investing in more than one country. Regional funds pursue top-line growth and governance engineering strategies significantly more frequently than single-country funds, often because they consolidate companies across countries (say, the Baltics) and can tap into wider networks of managers and board members. Figure IA.D1(e) illustrates.

It is common for playbooks to be revised over time. We define a revision as the introduction of a new action item after the holding period's first year. 77.3% of sample deals see revisions, but they tend to be minor, as Table IA.D6 and Figure IA.D2 illustrate. The most common newly added action item is cost reduction, which 31% of deals add at some point during the holding period, perhaps to create additional value, perhaps because the deal has underperformed relative to expectations or has experienced an external shock (such as a recession). New plans to optimize the capital structure (20%) and change the CEO (19%) are also relatively common, presumably for similar reasons.

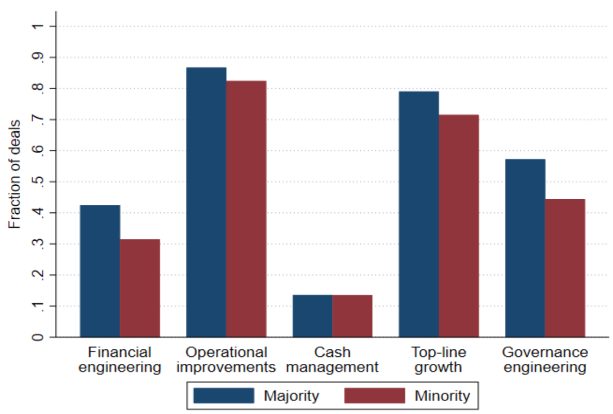
Figure IA.D1. Breakdown of playbook strategies.
The figure shows the share of deals pursuing each value creation strategy by deal vintage, deal type, fund ownership, growth strategy, and geographic focus. The sample size is 1,136 deals. See Tables IA.D3 through IA.D5 for the full set of statistics.



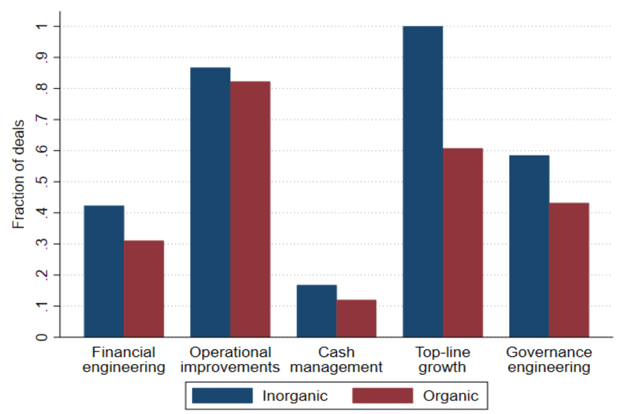
(a) Playbook strategies by vintage year



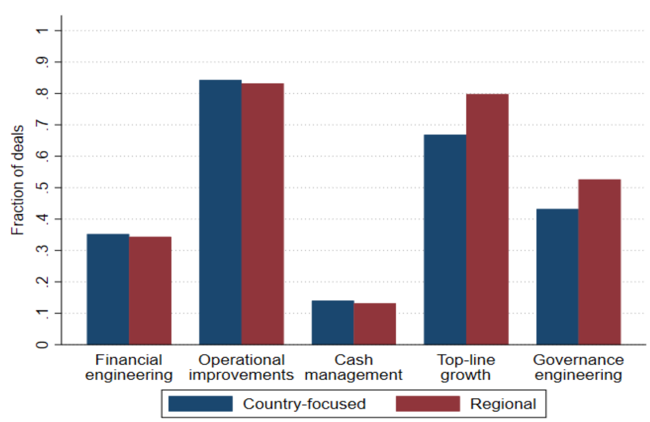
(b) Playbook strategies by deal type



(c) Playbook strategies by fund ownership



(d) Playbook strategies by growth strategy



(e) Playbook strategies by fund geographic focus

Figure IA.D2. Revisions of initial playbook.

The figure shows the share of deals pursuing individual action items in the initial value creation plan and in a revised plan. We define a revision as the introduction of a new action item after the first year of the holding period. The sample size is 1,136 deals. For variable definitions and details of their construction see the Appendix. See Table IA.D6 for the full set of statistics on plan revisions.

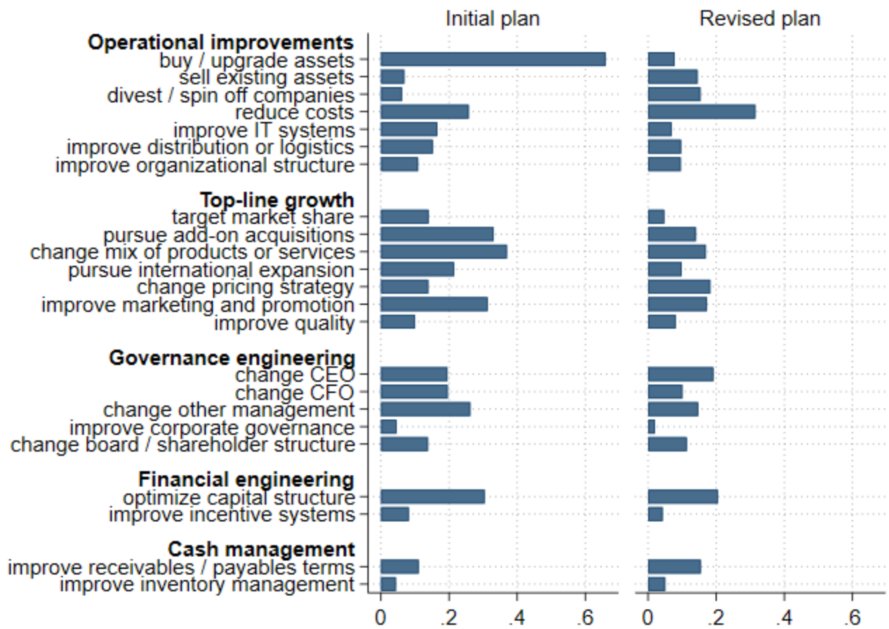


Table IA.D1. Top 10 most popular combinations of value creation strategies.
The table reports the most popular combinations of value creation strategies. Fractions in the last column are reported with respect to total deal count. Combinations are ordered from high to low in terms of frequency.

	Strategy						
Rank	Operational improvements	Top-line growth	Governance engineering	Financial engineering	Cash management	Freq.	Fraction
1	Yes	Yes	-	-	-	201	17.7
2	Yes	Yes	Yes	-	-	174	15.3
3	Yes	Yes	Yes	Yes	-	126	11.1
4	Yes	-	-	-	-	104	9.2
5	Yes	Yes	-	Yes	-	90	7.9
6	Yes	Yes	Yes	-	Yes	48	4.2
7	-	Yes	-	-	-	44	3.9
8	Yes	Yes	Yes	Yes	Yes	43	3.8
9	-	Yes	Yes	-	-	41	3.6
10	Yes	-	Yes	-	-	39	3.4

Table IA.D2. Most popular combinations of value creation action items.
The table reports the 10 most popular combinations of action items, conditional on a value creation plan including at least two action items. Fractions are reported with respect to total deal count (N=982). Combinations are ordered from high to low in terms of frequency.

Combination #	1	2	3	4	5	6	7	8	9	10
Frequency	34	17	11	10	10	8	7	7	5	5
Percentage	3.46	1.73	1.12	1.02	1.02	0.81	0.71	0.71	0.51	0.51
Operational improvements										
…buy / upgrade assets	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
…sell existing assets	-	-	-	-	-	-	-	-	-	-
…divest / spin off companies	-	-	-	-	-	-	-	-	-	-
…reduce costs	-	-	-	-	Yes	-	-	-	-	-
…improve IT systems	-	-	-	-	-	-	-	-	-	-
…improve distribution or logistics	-	-	-	-	-	Yes	-	-	-	Yes
…improve organizational structure	-	-	-	-	-	-	-	-	-	-
Top-line growth										
…target market share	-	-	-	-	-	-	-	-	-	-
…pursue add-on acquisitions	-	Yes	-	-	-	-	-	-	Yes	-
…change product/services mix	-	-	Yes	-	-	-	-	-	Yes	-
…pursue international expansion	-	-	-	Yes	-	-	-	-	-	-
…change pricing strategy	-	-	-	-	-	-	-	-	-	-
…improve marketing/promotion	-	-	-	-	-	-	-	Yes	-	Yes
…improve quality	-	-	-	-	-	-	-	-	-	-
Governance engineering										
…change CEO	-	-	-	-	-	-	-	-	-	-
…change CFO	-	-	-	-	-	-	-	-	-	-
…change other management	-	-	-	-	-	-	-	-	-	-
…improve corporate governance	-	-	-	-	-	-	-	-	-	-
…change board / shareholder structure	-	-	-	-	-	-	Yes	Yes	-	-
Financial engineering										
…optimize capital structure	Yes	-	-	-	-	-	-	-	-	-
…improve incentive systems	-	-	-	-	-	-	-	-	-	-
Cash management										
…improve receivables/payables	-	-	-	-	-	-	-	-	-	-
…improve inventory management	-	-	-	-	-	-	-	-	-	-

Table IA.D3. Value creation plans: Breakdown by deal vintage.
The table provides a breakdown by deal vintage of the 1,136 value creation plans in our sample, grouped into five quinquennia starting in 1992. For variable definitions and details of their construction see the Appendix in the paper. The Pearson's χ^2 tests test for equal fractions across vintages.

	Fractions by deal vintage					χ^2 test
	1992-1996	1997-2001	2002-2006	2007-2011	2012-2017	(p-value)
Total deal count	123	453	216	241	103	
Operational improvements	0.76	0.86	0.87	0.82	0.81	0.05
…buy/upgrade assets	0.71	0.74	0.62	0.56	0.58	0.00
…sell existing assets	0.02	0.04	0.10	0.11	0.10	0.00
…divest/spin off companies	0.01	0.06	0.10	0.05	0.08	0.02
…reduce costs	0.11	0.25	0.30	0.31	0.26	0.00
…improve IT systems	0.07	0.13	0.24	0.19	0.26	0.00
…improve distribution or logistics	0.12	0.15	0.17	0.15	0.17	0.80
…improve organizational structure	0.03	0.08	0.15	0.14	0.18	0.00
Top-line growth	0.41	0.70	0.81	0.85	0.83	0.00
…target market share	0.06	0.12	0.16	0.18	0.19	0.00
…pursue add-on acquisitions	0.11	0.23	0.42	0.54	0.37	0.00
…change product/services mix	0.20	0.33	0.43	0.44	0.45	0.00
…pursue international expansion	0.13	0.17	0.25	0.24	0.39	0.00
…change pricing strategy	0.07	0.12	0.17	0.19	0.13	0.01
…improve marketing/promotion	0.14	0.32	0.32	0.35	0.39	0.00
…improve quality	0.09	0.12	0.09	0.08	0.08	0.33
Governance engineering	0.24	0.40	0.56	0.58	0.74	0.00
…change CEO	0.07	0.17	0.25	0.22	0.28	0.00
…change CFO	0.07	0.12	0.26	0.27	0.39	0.00
…change other management	0.09	0.17	0.35	0.36	0.47	0.00
…improve corporate governance	0.00	0.02	0.05	0.08	0.10	0.00
…change board/shareholder structure	0.07	0.14	0.11	0.16	0.22	0.01
Financial engineering	0.13	0.26	0.45	0.46	0.51	0.00
…optimize capital structure	0.12	0.25	0.38	0.37	0.46	0.00
…improve incentive systems	0.01	0.02	0.13	0.13	0.20	0.00
Cash management	0.07	0.12	0.15	0.16	0.18	0.05
…improve receivables/payables	0.02	0.10	0.12	0.15	0.16	0.00
…improve inventory management	0.04	0.04	0.06	0.04	0.07	0.59

Table IA.D4. Value creation plans: Breakdown by deal type.
The table provides a breakdown by deal type of the 1,136 value creation plans in our sample. For variable definitions and details of their construction see the Appendix in the paper. The Pearson's χ^2 tests test for equal fractions across deal type.

	Fractions by deal type					
	Early stage	Growth	Buyout	Second-aries	Turn-around	χ^2 test (*p*-value)
Total deal count	211	679	154	75	17	
Operational improvements	0.80	0.85	0.82	0.85	0.88	0.26
…buy/upgrade assets	0.69	0.70	0.48	0.59	0.71	0.00
…sell existing assets	0.01	0.07	0.12	0.11	0.12	0.00
…divest/spin off companies	0.02	0.06	0.11	0.07	0.18	0.00
…reduce costs	0.13	0.25	0.40	0.33	0.47	0.00
…improve IT systems	0.11	0.18	0.18	0.15	0.18	0.16
…improve distribution or logistics	0.11	0.16	0.15	0.20	0.12	0.28
…improve organizational structure	0.05	0.10	0.18	0.20	0.18	0.00
Top-line growth	0.56	0.77	0.88	0.72	0.53	0.00
…target market share	0.05	0.15	0.25	0.12	0.12	0.00
…pursue add-on acquisitions	0.14	0.34	0.58	0.35	0.12	0.00
…change product/services mix	0.31	0.34	0.55	0.45	0.18	0.00
…pursue international expansion	0.15	0.21	0.34	0.21	0.24	0.00
…change pricing strategy	0.08	0.14	0.19	0.17	0.12	0.02
…improve marketing/promotion	0.32	0.31	0.36	0.28	0.12	0.26
…improve quality	0.05	0.12	0.06	0.09	0.24	0.00
Governance engineering	0.39	0.47	0.62	0.56	0.41	0.00
…change CEO	0.13	0.19	0.29	0.24	0.18	0.00
…change CFO	0.09	0.19	0.34	0.25	0.18	0.00
…change other management	0.19	0.25	0.43	0.24	0.35	0.00
…improve corporate governance	0.02	0.05	0.05	0.07	0.00	0.36
…change board/shareholder structure	0.13	0.14	0.12	0.16	0.18	0.90
Financial engineering	0.29	0.32	0.54	0.32	0.59	0.00
…optimize capital structure	0.28	0.28	0.44	0.24	0.59	0.00
…improve incentive systems	0.02	0.07	0.20	0.12	0.06	0.00
Cash management	0.05	0.15	0.18	0.13	0.14	0.99
…improve receivables/payables	0.04	0.12	0.16	0.11	0.12	0.36
…improve inventory management	0.02	0.05	0.03	0.03	0.03	0.22

Table IA.D5. Value creation plans: Breakdown by fund ownership, growth strategy, and geographic focus.
The table provides breakdowns of the 1,136 value creation plans in our sample by fund ownership, growth strategy (whether intended growth is organic or inorganic), and geographic focus. Inorganic deals are those in which the PE fund includes a buy-and-build (M&A) action item in its initial playbook. For variable definitions and details of their construction see the Appendix in the paper. The Pearson's χ^2 tests test for equal fractions across fund ownership, growth strategy, and geographic focus.

	Fractions by fund ownership			Fractions by growth strategy			Fractions by geographic focus		
	Majority	Minority	χ^2 test (*p*-value)	Organic	Inorganic	χ^2 test (*p*-value)	Single country	Regional	χ^2 test (*p*-value)
Total deal count	333	803		760	376		528	608	
Operational improvements	0.86	0.83	0.10	0.82	0.87	0.06	0.84	0.83	0.63
…buy/upgrade assets	0.68	0.65	0.31	0.68	0.61	0.01	0.72	0.61	0.00
…sell existing assets	0.09	0.06	0.11	0.06	0.08	0.30	0.05	0.09	0.02
…divest/spin off companies	0.07	0.06	0.50	0.04	0.11	0.00	0.04	0.08	0.02
…reduce costs	0.25	0.26	0.67	0.23	0.31	0.00	0.23	0.28	0.10
…improve IT systems	0.22	0.14	0.00	0.14	0.22	0.00	0.15	0.18	0.31
…improve distribution or logistics	0.17	0.14	0.25	0.14	0.18	0.09	0.14	0.16	0.22
…improve organizational structure	0.13	0.10	0.16	0.09	0.15	0.00	0.08	0.13	0.00
Top-line growth	0.79	0.72	0.02	0.61	1.00	0.00	0.67	0.80	0.00
…target market share	0.13	0.15	0.39	0.09	0.24	0.00	0.08	0.19	0.00
…pursue add-on acquisitions	0.40	0.30	0.00	0.00	1.00	0.00	0.26	0.39	0.00
…change product/services mix	0.40	0.36	0.14	0.34	0.43	0.00	0.34	0.39	0.10
…pursue international expansion	0.29	0.19	0.00	0.17	0.30	0.00	0.13	0.29	0.00
…change pricing strategy	0.16	0.13	0.15	0.13	0.16	0.22	0.14	0.14	0.94
…improve marketing/promotion	0.36	0.29	0.03	0.30	0.35	0.10	0.30	0.33	0.22
…improve quality	0.10	0.10	0.90	0.11	0.09	0.32	0.10	0.10	1.00
Governance engineering	0.56	0.45	0.00	0.43	0.59	0.00	0.43	0.53	0.00
…change CEO	0.23	0.18	0.03	0.17	0.25	0.00	0.16	0.22	0.02
…change CFO	0.27	0.16	0.00	0.15	0.29	0.00	0.15	0.24	0.00
…change other management	0.36	0.22	0.00	0.23	0.32	0.00	0.23	0.29	0.01
…improve corporate governance	0.05	0.04	0.39	0.03	0.07	0.00	0.04	0.05	0.37
…change board/shareholder structure	0.14	0.14	0.85	0.13	0.15	0.36	0.14	0.13	0.60
Financial engineering	0.43	0.31	0.00	0.31	0.42	0.00	0.35	0.34	0.76
…optimize capital structure	0.37	0.28	0.00	0.28	0.35	0.02	0.32	0.29	0.35
…improve incentive systems	0.15	0.05	0.00	0.06	0.12	0.00	0.06	0.10	0.05
Cash management	0.13	0.14	0.68	0.12	0.17	0.03	0.14	0.13	0.67
…improve receivables/payables	0.12	0.11	0.67	0.10	0.14	0.06	0.11	0.11	0.77
…improve inventory management	0.03	0.05	0.14	0.04	0.05	0.29	0.05	0.04	0.28

Table IA.D6. Revisions of initial playbooks.

The table reports the number and fraction of deals in which an initial strategy or action item is revised. We code the introduction of new strategies and action items after the first year as revisions. For variable definitions and details of their construction see the Appendix in the paper.

	Fund revises initial playbook	
	Deal count (1)	Fraction of sample (2)
Operational improvements	588	0.55
…buy/upgrade assets	84	0.08
…sell existing assets	156	0.15
…divest/spin off companies	165	0.15
…reduce costs	338	0.31
…improve IT systems	74	0.07
…improve distribution or logistics	104	0.10
…improve organizational structure	103	0.10
Top-line growth	529	0.49
…target market share	51	0.05
…pursue add-on acquisitions	151	0.14
…change product/services mix	182	0.17
…pursue international expansion	106	0.10
…change pricing strategy	196	0.18
…improve marketing/promotion	186	0.17
…improve quality	87	0.08
Governance engineering	420	0.39
…change CEO	206	0.19
…change CFO	108	0.10
…change other management	158	0.15
…improve corporate governance	22	0.02
…change board/shareholder structure	122	0.11
Financial engineering	252	0.23
…optimize capital structure	220	0.20
…improve incentive systems	46	0.04
Cash management	191	0.18
…improve receivables/payables	166	0.15
…improve inventory management	54	0.05

IA.E Further materials and robustness tests

Table IA.E1. The effect of PE ownership on portfolio-company performance: Early-stage deals.

The table summarizes estimates of company-level changes in 17 outcome variables during the PE holding period as a function of the presence or absence of an action item related to the outcome variable in question for four of the five strategies in our playbooks. (We lack outcome variables related to the fifth strategy, governance engineering.) Each row corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized early-stage deals as well as control firms matched on size, country, industry, and investment year. The number of observations varies depending on the availability of individual data items in Orbis. For each outcome/action-item pair, we estimate regression (1) in the paper: $y_{it}^{o} = \beta_0 + \beta_1 PE_i * post_{it} + \beta_2 PE_i * post_{it} * plan_i^{o} + \omega_{ct} + \varepsilon_{it}$, where y_{it}^{o} is company i's outcome o in year t, $plan_i^{o}$ is an indicator capturing the presence of an action item related to outcome o in company i's playbook, and PE_i is an indicator set equal to 1 for portfolio companies and 0 for their matched controls. We track each portfolio company and its matched controls, which constitute a "cell" c, from (up to) five years before the PE investment to (up to) five years after the exit or the fifth anniversary if unexited as of the end of our sample period. For each portfolio company and its matched controls, $post_{it}$ equals 1 from the year the PE firm invests in the portfolio company and 0 before, while $plan_i^{o}$ takes the same value in a given cell. Each regression includes a full set of cell-by-year fixed effects, ω_{ct}. For variable definitions and details of their construction see the Appendix in the paper. Heteroskedasticity-consistent standard errors clustered at the company level are shown in italics next to the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

		Effect for companies without action item: β_1: $PE * post$		Plan differential: β_2: $PE * post * plan$		Effect for companies with action item: $\beta_1 + \beta_2$			
Action item	Outcome variable	coeff. (1)	s.e. (2)	coeff. (3)	s.e. (4)	coeff. (5)	s.e. (6)	R-sq. (7)	N (8)
Operational improvements									
buy/upgrade assets	net investment	-0.014	*0.010*	0.043**	*0.020*	0.029*	*0.017*	0.400	842
	capital intensity	0.170	*0.294*	0.695	*0.431*	0.865***	*0.315*	0.613	841
	net assets	0.029	*1.792*	-0.317	*1.883*	-0.288	*0.577*	0.378	860
pursue add-on acquisitions	acquisitions	-0.056	*0.064*	0.049	*0.064*	-0.007	*0.008*	0.322	1,048
sell existing assets	divestments	0.011*	*0.006*			0.011*	*0.006*	0.420	1,048
reduce costs	labor productivity	0.432***	*0.124*	-1.809***	*0.494*	-1.378***	*0.478*	0.684	863
	employment	-0.376	*0.387*	0.866	*0.675*	0.490	*0.553*	0.762	892
	TFP	0.110	*0.108*	-0.879***	*0.158*	-0.768***	*0.115*	0.698	822
	EBITDA	-2.228	*1.460*	1.318	*1.700*	-0.910	*0.871*	0.298	956
Top-line growth									
top-line growth	sales	0.194	*0.493*	-0.348	*0.680*	-0.154	*0.468*	0.756	970
	sales growth	0.022	*0.031*	0.081	*0.102*	0.103	*0.097*	0.388	855
target market share	market share	0.025	*0.028*	-0.031	*0.028*	-0.006**	*0.002*	0.372	970
change pricing strategy	markup	0.054	*0.158*	1.174***	*0.258*	1.228***	*0.204*	0.691	822
Financial engineering									
optimize capital structure	leverage	0.009	*0.029*	0.126**	*0.057*	0.135***	*0.049*	0.390	774
	net debt to EBITDA	-0.652	*0.614*	2.834**	*1.156*	2.183**	*0.979*	0.272	748
Cash management									
improve receivables/	working capital	-0.066	*0.065*	0.117	*0.121*	0.051	*0.103*	0.309	873
payables	collection period	11.008	*6.962*	4.723	*10.700*	15.731*	*8.125*	0.315	858

Table IA.E2. The effect of PE ownership on portfolio-company performance: Growth deals.

The table summarizes estimates of company-level changes in 17 outcome variables during the PE holding period as a function of the presence or absence of an action item related to the outcome variable in question for four of the five strategies in our playbooks. (We lack outcome variables related to the fifth strategy, governance engineering.) Each row corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized growth deals as well as control firms matched on size, country, industry, and investment year. The number of observations varies depending on the availability of individual data items in Orbis. For each outcome/action-item pair, we estimate regression (1) in the paper: $y_{it}^o = \beta_0 + \beta_1 PE_i * post_{it} + \beta_2 PE_i * post_{it} * plan_i^o + \omega_{ct} + \varepsilon_{it}$, where y_{it}^o is company i's outcome o in year t, $plan_i^o$ is an indicator capturing the presence of an action item related to outcome o in company i's playbook, and PE_i is an indicator set equal to 1 for portfolio companies and 0 for their matched controls. We track each portfolio company and its matched controls, which constitute a "cell" c, from (up to) five years before the PE investment to (up to) five years after the exit or the fifth anniversary if unexited as of the end of our sample period. For each portfolio company and its matched controls, $post_{it}$ equals 1 from the year the PE firm invests in the portfolio company and 0 before, while $plan_i^o$ takes the same value in a given cell. Each regression includes a full set of cell-by-year fixed effects, ω_{ct}. For variable definitions and details of their construction see the Appendix in the paper. Heteroskedasticity-consistent standard errors clustered at the company level are shown in italics next to the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

		Effect for companies without action item: β_1: $PE * post$		Plan differential: β_2: $PE * post * plan$		Effect for companies with action item: $\beta_1 + \beta_2$			
Action item	Outcome variable	coeff. (1)	s.e. (2)	coeff. (3)	s.e. (4)	coeff. (5)	s.e. (6)	R-sq. (7)	N (8)
Operational improvements									
buy/upgrade assets	net investment	-0.001	*0.005*	0.024***	*0.008*	0.022***	*0.005*	0.377	11,294
	capital intensity	-0.040	*0.188*	0.335	*0.229*	0.295**	*0.131*	0.500	10,750
	net assets	0.430	*0.591*	0.756	*0.709*	1.186***	*0.393*	0.387	10,747
pursue add-on acquisitions	acquisitions	0.026	*0.023*	0.075**	*0.037*	0.101***	*0.029*	0.272	13,288
sell existing assets	divestments	-0.005	*0.011*	0.039	*0.024*	0.033	*0.021*	0.292	13,288
reduce costs	labor productivity	0.363***	*0.089*	-0.208	*0.157*	0.154	*0.129*	0.544	11,142
	employment	0.694***	*0.092*	-0.112	*0.158*	0.581***	*0.128*	0.668	11,530
	TFP	0.139***	*0.040*	-0.183**	*0.076*	-0.043	*0.064*	0.752	10,340
	EBITDA	1.450***	*0.400*	-1.646**	*0.736*	-0.196	*0.617*	0.346	12,317
Top-line growth									
top-line growth	sales	0.908***	*0.283*	0.209	*0.313*	1.117***	*0.133*	0.678	12,683
	sales growth	0.070**	*0.033*	-0.056	*0.036*	0.014	*0.015*	0.390	11,333
target market share	market share	0.049***	*0.015*	0.004	*0.028*	0.053**	*0.024*	0.490	12,686
change pricing strategy	markup	-0.048	*0.051*	0.054	*0.101*	0.006	*0.087*	0.597	10,340
Financial engineering									
optimize capital structure	leverage	0.069***	*0.019*	0.041	*0.029*	0.110***	*0.022*	0.325	10,376
	net debt to EBITDA	0.723**	*0.349*	1.640***	*0.591*	2.363***	*0.477*	0.260	9,923
Cash management									
improve receivables/ payables	working capital	-0.075***	*0.026*	-0.006	*0.055*	-0.082*	*0.048*	0.358	11,226
	collection period	-5.990	*3.641*	-15.397*	*8.187*	-21.387***	*7.333*	0.398	10,000

Table IA.E3. The effect of PE ownership on portfolio-company performance: Buyout deals.

The table summarizes estimates of company-level changes in 17 outcome variables during the PE holding period as a function of the presence or absence of an action item related to the outcome variable in question for four of the five strategies in our playbooks. (We lack outcome variables related to the fifth strategy, governance engineering.) Each row corresponds to a separate regression. The estimation sample in each regression includes both realized and unrealized buyout deals as well as control firms matched on size, country, industry, and investment year. The number of observations varies depending on the availability of individual data items in Orbis. For each outcome/action-item pair, we estimate regression (1) in the paper: $y_{it}^{o} = \beta_0 + \beta_1 PE_i * post_{it} + \beta_2 PE_i * post_{it} * plan_i^{o} + \omega_{ct} + \varepsilon_{it}$, where y_{it}^{o} is company i's outcome o in year t, $plan_i^{o}$ is an indicator capturing the presence of an action item related to outcome o in company i's playbook, and PE_i is an indicator set equal to 1 for portfolio companies and 0 for their matched controls. We track each portfolio company and its matched controls, which constitute a "cell" c, from (up to) five years before the PE investment to (up to) five years after the exit or the fifth anniversary if unexited as of the end of our sample period. For each portfolio company and its matched controls, $post_{it}$ equals 1 from the year the PE firm invests in the portfolio company and 0 before, while $plan_i^{o}$ takes the same value in a given cell. Each regression includes a full set of cell-by-year fixed effects, ω_{ct}. For variable definitions and details of their construction see the Appendix in the paper. Heteroskedasticity-consistent standard errors clustered at the company level are shown in italics next to the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

		Effect for companies without action item: β_1: $PE * post$		Plan differential: β_2: $PE * post * plan$		Effect for companies with action item: $\beta_1 + \beta_2$			
Action item	Outcome variable	coeff. (1)	s.e. (2)	coeff. (3)	s.e. (4)	coeff. (5)	s.e. (6)	R-sq. (7)	N (8)
Operational improvements									
buy/upgrade assets	net investment	0.011**	*0.006*	-0.004	*0.009*	0.008	*0.007*	0.410	4,433
	capital intensity	0.194	*0.226*	-0.114	*0.338*	0.079	*0.252*	0.529	4,131
	net assets	0.591	*0.533*	-1.208	*0.898*	-0.618	*0.723*	0.385	3,733
pursue add-on acquisitions	acquisitions	-0.024	*0.041*	0.084	*0.057*	0.060	*0.040*	0.318	4,927
sell existing assets	divestments	-0.014	*0.013*	0.056**	*0.023*	0.042**	*0.019*	0.338	4,927
reduce costs	labor productivity	0.163	*0.140*	0.095	*0.211*	0.258	*0.157*	0.537	4,214
	employment	0.360**	*0.181*	0.022	*0.252*	0.382**	*0.176*	0.548	4,277
	TFP	0.148**	*0.072*	-0.142	*0.123*	0.006	*0.099*	0.667	4,361
	EBITDA	-0.436	*0.682*	1.252	*0.970*	0.817	*0.690*	0.292	4,581
Top-line growth									
top-line growth	sales	1.121***	*0.260*	-0.507*	*0.299*	0.614***	*0.146*	0.618	4,818
	sales growth	0.023	*0.039*	0.028	*0.044*	0.050**	*0.020*	0.407	4,491
target market share	market share	0.030**	*0.015*	-0.021	*0.038*	0.009	*0.036*	0.483	4,819
change pricing strategy	markup	-0.138*	*0.083*	0.093	*0.202*	-0.045	*0.184*	0.690	4,361
Financial engineering									
optimize capital structure	leverage	0.085***	*0.026*	0.065	*0.053*	0.150***	*0.046*	0.385	3,902
	net debt to EBITDA	0.615	*0.568*	0.159	*0.821*	0.775	*0.594*	0.252	3,840
Cash management									
improve receivables/	working capital	-0.003	*0.038*	-0.017	*0.087*	-0.020	*0.078*	0.338	4,263
payables	collection period	-8.803	*7.639*	6.406	*12.335*	-2.398	*9.684*	0.392	3,627

Table IA.E4. Company-level changes and the cross-section of deal-level returns: Interactions with deal type.
The table summarizes regression results of equation (2) estimated in the cross-section of portfolio companies, as in Table 4 in the paper, but interacting the company-level change variable with deal type. Each row represents one regression. Panels A, B, C, and D show results for our four deal-level return measures. The estimation sample in each regression includes exited deals and unexited deals that are held in a fund's portfolio for at least four years. The number of observations included in each regression varies depending on data availability in Orbis. The variable of interest in each regression is the company-level change in one of 17 outcome variables measured over the PE holding period. Both returns and outcomes are winsorized at the 1% level and standardized to have mean 0 and standard deviation 1. Log deal size, log deal duration, and entry and exit year fixed effects are included but not shown to conserve space. For variable definitions and details of their construction see the Appendix in the paper. Heteroskedasticity-consistent standard errors are shown in italics next to the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Panel A: PME											
	x early-stage		x growth		x buyout		x secondaries		x turnaround			
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	R-sq.	no.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Op. improvements												
net investment	-0.009***	*0.002*	0.002	*0.002*	0.013	*0.044*	2.284	*3.517*	-0.511	*0.511*	0.122	545
capital intensity	-0.071	*0.087*	0.047	*0.038*	0.240*	*0.133*	0.815*	*0.433*	0.086	*0.088*	0.167	468
net assets	0.047	*0.045*	0.085***	*0.032*	0.131***	*0.035*	0.160	*0.121*	-0.140	*0.148*	0.228	321
acquisitions			0.477*	*0.253*	1.332***	*0.445*	-0.495	*0.743*	-0.812	*0.517*	0.115	758
divestments			0.379	*0.678*	1.894	*2.357*	1.298	*0.807*			0.107	758
labor productivity	-0.193***	*0.052*	0.046	*0.045*	0.239**	*0.119*	0.208	*0.257*	0.048	*0.044*	0.150	473
employment	0.177*	*0.095*	0.105**	*0.047*	0.244*	*0.139*	-0.313**	*0.157*	-0.035	*0.059*	0.152	523
TFP	-0.450**	*0.190*	0.107	*0.092*	0.216	*0.186*	-0.190	*0.500*	0.058	*0.078*	0.121	483
EBITDA	-0.023	*0.025*	0.036***	*0.010*	0.060***	*0.019*	-0.020	*0.033*	-0.013	*0.038*	0.140	595
Top-line growth												
sales	-0.052	*0.037*	0.113***	*0.031*	0.331**	*0.133*	0.051	*0.115*	0.001	*0.067*	0.155	603
sales growth	0.001***	*0.000*	0.000	*0.000*	0.001***	*0.000*	0.008	*0.008*	-0.014	*0.010*	0.118	528
market share	-0.313	*0.344*	-0.159	*0.165*	0.625*	*0.320*	0.888	*0.663*	0.186	*0.169*	0.130	599
markup	-0.233	*0.367*	-0.088	*0.106*	-0.008	*0.311*	-0.089	*0.513*	-0.473	*0.595*	0.114	483
Financial engineering												
leverage	-0.001	*0.707*	-0.901**	*0.417*	0.911	*0.587*	-4.865	*3.311*	-0.550	*1.088*	0.148	522
net debt to EBITDA	0.001	*0.001*	-0.000	*0.001*	0.006	*0.004*	-0.012	*0.015*	-0.001	*0.001*	0.122	515
Cash management												
working capital	-0.426	*0.279*	-0.001	*0.191*	-0.045	*0.465*	-2.377	*1.777*	0.738	*0.469*	0.137	552
collection period	0.001	*0.001*	-0.000	*0.001*	-0.000	*0.002*	-0.000	*0.001*	-0.007***	*0.002*	0.167	309

	Panel B: MOIC											
	x early-stage		x growth		x buyout		x secondaries		x turnaround			
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	*R*-sq.	no.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Op. improvements												
net investment	-0.022***	*0.004*	0.002	*0.003*	0.027	*0.054*	3.849	*5.132*	-0.776	*0.531*	0.170	545
capital intensity	-0.185	*0.162*	0.136*	*0.071*	0.371**	*0.188*	1.113*	*0.604*	0.063	*0.118*	0.214	468
net assets	0.108	*0.072*	0.110**	*0.045*	0.208***	*0.054*	0.274	*0.169*	-0.022	*0.223*	0.242	321
acquisitions			0.947**	*0.467*	2.613**	*1.077*	-1.155	*1.390*	-0.334	*0.611*	0.163	758
divestments			0.392	*0.933*	8.714	*6.934*	1.478	*1.150*			0.153	758
labor productivity	-0.278***	*0.075*	0.116	*0.080*	0.324*	*0.167*	0.087	*0.327*	0.025	*0.042*	0.187	473
employment	0.410*	*0.210*	0.214***	*0.072*	0.393*	*0.212*	-0.404**	*0.192*	0.017	*0.048*	0.193	523
TFP	-0.472	*0.298*	0.119	*0.132*	0.209	*0.254*	-0.374	*0.697*	0.060	*0.085*	0.164	483
EBITDA	-0.002	*0.054*	0.048***	*0.014*	0.076***	*0.025*	-0.029	*0.051*	0.040	*0.060*	0.174	595
Top-line growth												
sales	-0.033	*0.064*	0.180***	*0.052*	0.447**	*0.177*	0.042	*0.164*	0.021	*0.052*	0.191	603
sales growth	0.001***	*0.001*	0.000	*0.001*	0.001***	*0.000*	0.035*	*0.018*	-0.011	*0.012*	0.162	528
market share	-0.214	*0.525*	-0.120	*0.227*	0.808*	*0.412*	1.441	*0.924*	0.234	*0.223*	0.164	599
markup	-0.348	*0.520*	-0.081	*0.165*	0.293	*0.493*	-0.269	*0.698*	-0.713	*0.679*	0.162	483
Financial engineering												
leverage	0.280	*1.362*	-1.023*	*0.603*	1.561*	*0.887*	-7.130	*4.998*	-0.416	*1.089*	0.196	522
net debt to EBITDA	0.002	*0.002*	0.001	*0.001*	0.006	*0.005*	-0.024	*0.026*	-0.000	*0.001*	0.171	515
Cash management												
working capital	-0.191	*0.407*	-0.187	*0.284*	-0.586	*0.774*	-3.485	*2.640*	0.878	*0.613*	0.172	552
collection period	0.001	*0.001*	0.001	*0.001*	0.000	*0.004*	0.001	*0.002*	-0.008***	*0.003*	0.202	309

	Panel C: Unlevered return											
	x early-stage		x growth		x buyout		x secondaries		x turnaround			
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	*R*-sq.	no.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Op. improvements												
net investment	0.000	*0.000*	0.000	*0.000*	-0.015	*0.025*	-0.628	*0.414*	-0.325	*0.263*	0.263	357
capital intensity	-0.041	*0.035*	0.013*	*0.008*	-0.006	*0.024*	0.150	*0.092*	-0.068	*0.089*	0.322	307
net assets	-0.003	*0.008*	0.008**	*0.004*	0.005	*0.004*	0.038**	*0.017*	-0.026	*0.024*	0.290	305
acquisitions			0.094**	*0.042*	0.136**	*0.059*	0.212	*0.218*			0.245	489
divestments			0.120	*0.186*	0.146	*0.218*	1.522***	*0.249*			0.246	489
labor productivity	-0.058*	*0.032*	0.006	*0.008*	-0.011	*0.031*	0.216***	*0.064*	0.025	*0.027*	0.314	309
employment	0.057	*0.039*	0.042***	*0.014*	0.008	*0.028*	0.036	*0.154*	-0.042	*0.037*	0.312	340
TFP	-0.052	*0.084*	-0.004	*0.024*	0.071	*0.089*	-0.169**	*0.076*	-5.983	*6.853*	0.206	332
EBITDA	-0.008	*0.011*	0.002	*0.002*	0.005	*0.004*	0.012	*0.016*	0.016	*0.023*	0.231	395
Top-line growth												
sales	-0.018	*0.018*	0.023***	*0.007*	0.050***	*0.016*	0.124**	*0.052*	0.002	*0.046*	0.288	396
sales growth	0.000***	*0.000*	0.000	*0.000*	0.000	*0.000*	-0.010***	*0.002*	-0.165	*0.193*	0.261	342
market share	0.245	*0.283*	-0.033	*0.085*	0.212	*0.150*	0.895*	*0.540*	0.575	*2.016*	0.260	396
markup	0.064	*0.090*	-0.001	*0.032*	0.025	*0.051*	-0.079*	*0.047*	-3.381***	*1.070*	0.207	332
Financial engineering												
leverage	0.262	*0.236*	-0.087	*0.067*	0.118	*0.091*	-0.329	*0.731*	-0.263	*0.301*	0.252	340
net debt to EBITDA	-0.002	*0.002*	-0.000	*0.000*	0.000	*0.001*	-0.013	*0.008*	0.009***	*0.003*	0.260	335
Cash management												
working capital	-0.035	*0.055*	-0.033	*0.051*	0.011	*0.084*	-0.549	*0.425*	0.154	*0.157*	0.216	367
collection period	-0.000	*0.000*	-0.000	*0.000*	-0.000	*0.000*	-0.001***	*0.000*	-0.002***	*0.000*	0.269	294

	Panel D: Abnormal performance											
	x early-stage		x growth		x buyout		x secondaries		x turnaround			
	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	coeff.	s.e.	*R*-sq.	no.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Op. improvements												
net investment	0.000	*0.000*	0.001*	*0.000*	-0.016	*0.019*	-0.779*	*0.413*	-0.343	*0.419*	0.222	357
capital intensity	-0.043	*0.036*	0.009	*0.008*	-0.007	*0.016*	0.148*	*0.088*	-0.030	*0.081*	0.285	307
net assets	-0.001	*0.007*	0.009***	*0.003*	0.004	*0.004*	0.032*	*0.017*	-0.009	*0.033*	0.236	305
acquisitions			0.133***	*0.045*	0.143**	*0.059*	0.257	*0.227*			0.218	489
divestments			0.149	*0.171*	0.206	*0.149*	1.552***	*0.237*			0.217	489
labor productivity	-0.060*	*0.033*	0.002	*0.008*	-0.003	*0.023*	0.222***	*0.058*	0.023	*0.024*	0.281	309
employment	0.074*	*0.041*	0.042***	*0.014*	0.004	*0.024*	0.077	*0.150*	-0.030	*0.034*	0.292	340
TFP	-0.058	*0.087*	-0.007	*0.022*	0.047	*0.075*	-0.146*	*0.087*	-7.483	*5.130*	0.168	332
EBITDA	-0.005	*0.011*	0.003	*0.002*	0.007*	*0.004*	0.015	*0.015*	0.037	*0.035*	0.200	395
Top-line growth												
sales	-0.016	*0.018*	0.021***	*0.007*	0.046***	*0.013*	0.126**	*0.055*	0.017	*0.047*	0.248	396
sales growth	0.000**	*0.000*	0.000	*0.000*	0.000	*0.000*	-0.011***	*0.002*	-0.164	*0.206*	0.220	342
market share	0.109	*0.283*	0.032	*0.080*	0.252**	*0.118*	0.859*	*0.520*	2.215	*3.105*	0.222	396
markup	0.063	*0.097*	-0.006	*0.029*	-0.021	*0.037*	-0.041	*0.049*	-2.854**	*1.168*	0.166	332
Financial engineering												
leverage	0.266	*0.269*	-0.074	*0.065*	0.027	*0.084*	-0.289	*0.622*	-0.414	*0.391*	0.190	340
net debt to EBITDA	-0.001	*0.002*	-0.000	*0.000*	0.000	*0.000*	-0.009	*0.007*	0.009***	*0.003*	0.194	335
Cash management												
working capital	-0.079	*0.066*	-0.023	*0.047*	0.020	*0.095*	-0.607	*0.374*	0.259	*0.260*	0.176	367
collection period	-0.000	*0.000*	-0.000	*0.000*	-0.000	*0.000*	-0.001***	*0.000*	-0.001***	*0.000*	0.230	294

Table IA.E5. Determinants of plan achievement.

The table reports regression results of playbook achievement at the deal-by-action-item level (column 1), the deal-by-strategy level (column 2), and the deal level (columns 3 and 4). The unit of analysis in column 1 is action item j pursued in deal i. The unit of analysis in column 2 is strategy k pursued in deal i. The unit of analysis in columns 3 and 4 is deal i. Column 1 is estimated as a linear probability model. Columns 2 through 4 are estimated as fractional logits given that the dependent variable is a fraction bounded on the interval [0,1]. In these columns, we report marginal effects instead of coefficients. For variable definitions and details of their construction see the Appendix in the paper. The estimation sample includes only fully realized deals. (In columns 3 and 4, we lack cash flow data and hence deal size for 13 of the 959 fully realized deals.) Standard errors, shown in italics below the coefficient estimates, are clustered at the fund level. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Planned action item achieved? (1)	Share of action items achieved per strategy (2)	Share of action items achieved in a deal (3)	Share of strategies achieved in a deal (4)
Market environment				
GDP growth	0.001	-0.001	-0.003	-0.003
	0.005	*0.005*	*0.004*	*0.004*
MSCI index growth	0.090*	0.104	0.008	0.020
	0.048	*0.070*	*0.024*	*0.014*
Industry revenue growth	0.013	0.030	0.023	0.006
	0.043	*0.034*	*0.042*	*0.032*
Idiosyncratic factors				
Bad luck	-0.042***	-0.034**	-0.003	0.004
	0.016	*0.017*	*0.018*	*0.016*
Playbook characteristics				
# action items per strategy	-0.044**	-0.034**		
	0.017	*0.017*		
# action items per deal			0.064***	
			0.012	
# strategies per deal				0.199***
				0.014
… squared	0.005	0.003	-0.004***	-0.034***
	0.003	*0.003*	*0.001*	*0.003*
Deal characteristics				
Log deal duration	0.089***	0.076***	0.086***	0.073***
	0.023	*0.018*	*0.016*	*0.017*
Log deal size	0.023***	0.022**	0.024*	0.014*
	0.007	*0.009*	*0.012*	*0.008*
Majority ownership	0.035**	0.030*	0.021	0.012
	0.017	*0.017*	*0.021*	*0.018*
Inorganic growth	-0.042*	-0.040	-0.046	-0.014
	0.023	*0.025*	*0.030*	*0.026*
Deal sequence number	0.001	0.000	-0.001	-0.000
	0.001	*0.001*	*0.001*	*0.001*
Fixed effects				
Deal year FE	Yes	Yes	Yes	Yes
Deal type FE	Yes	Yes	Yes	Yes
PE firm FE	Yes	Yes	Yes	Yes
Unit of analysis	Deal by action item	Deal by strategy	Deal	Deal
R-squared/pseudo R-squared	0.092	0.127	0.169	0.331
Number of obs.	4,088	2,326	946	946

Sources of value creation in private equity buyouts of private firms[✧]

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ABSTRACT

Despite the prevalence of private equity (PE) buyouts of private firms, little is known about how these transactions create value. We provide evidence that PE acquirers disproportionately target private firms with weak operating profitability and those that have growth potential but are highly levered and dependent on external financing. Target firms grow rapidly post-buyout, especially those undertaking add-on acquisitions, and profitability increases for both profitable and unprofitable targets. Our evidence suggests that PE acquirers create value by relaxing financing constraints for firms with strong investment opportunities and improving the performance of weak firms, while financial engineering plays a limited role.

JEL Classification codes: G34, G32, H25
Keywords: Private Equity Buyouts, Private Firms, Financing Constraints, Capital Structure

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1. Introduction

Although private equity (PE) buyouts of publicly-traded firms have received headline attention for many years, the market for buyouts of already-private firms has grown rapidly. Over the past decade, "private firm buyouts" outnumber PE buyouts of publicly-traded firms in the U.S. by more than 30 to one.[1] Yet, our understanding of how these buyouts create value remains limited, especially in the U.S., which represents the world's largest buyout market. Given substantial differences between public and private firms in size, ownership structure, and access to capital, as well as the specialization of different PE firms in the markets for public and private targets, the potential sources of value creation in private firm buyouts may differ significantly from those involving public buyout targets.

This study investigates sources of value creation in private firm buyouts. We analyze a sample of 288 private firms acquired by PE sponsors between 1995 and 2009 using firm-level financial data obtained from U.S. corporate tax returns.[2] We assess the importance of three potential sources of value in private firm buyouts: improvements in profitability, financial engineering, and relaxation of financial constraints to unlock profitable growth opportunities. The third source is uniquely important for buyouts of private firms, which are often constrained in their ability to raise capital to fund growth. The IRS dataset allows us to overcome data limitations that make it challenging to study private firms in the U.S. in general.

We begin our analysis by identifying firm-level characteristics that predict which private firms PE acquirers target in buyouts. We find a non-monotonic relationship between profitability and the likelihood that a private firm is a buyout target, with PE acquirers disproportionately

[1] This figure is based on buyouts of U.S. public and private firms as reported in Capital IQ.
[2] The dataset, obtained from the Internal Revenue Service (IRS), includes all U.S. C corporations with at least $10 million in assets. It does not include companies organized as pass-through entities (e.g., S corporations, partnerships, and limited liability companies).

targeting firms in the highest and lowest quintiles of profitability in the overall sample of private firms. Firms in the lowest quintile of profitability plausibly represent turnaround opportunities; firms in the highest quintile could serve as growth platforms if high average profitability indicates high potential marginal returns to investment. Among private firms with high profitability, PE acquirers are more likely to target firms with high leverage that operate in external capital-dependent industries. Arguably, these firms disproportionately have profitable growth options but lack the financial capacity to pursue them.

Next, we analyze changes in financial performance after private firm buyouts. We find a moderate increase in profitability, both in absolute terms and relative to industry peers and to a control sample of propensity-score matched private firms not acquired in buyouts. The median increase in pre-interest return on sales from the year before a buyout to the second year after the buyout relative to the industry median change is 1.9 percentage points.[3] We observe an increase in profitability among both low and high pre-buyout profitability target firms. The increase is sizeable for the least profitable firms, consistent with turnaround opportunities representing an important source of value creation in private firm buyouts. The post-buyout increase for low-profitability firms remains positive but shrinks somewhat once we account for the level and trend in pre-buyout profitability, suggesting that a portion of the post-buyout increase for poor performers could reflect mean reversion in profitability.

Most strikingly, we find consistent evidence of a large and rapid increase in sales growth after private firm buyouts. The median increase in sales growth from the pre-buyout year to the second post-buyout year is 61.7 percentage points greater than the industry median change in sales

[3] We focus on return on sales rather than return on assets to assess post-buyout profitability because firms often write up or write down asset values at the time of an acquisition, making it difficult to compare pre- and post-buyout return on assets. See Guo, Hotchkiss, and Song (2011) and Cohn, Mills, and Towery (2014) for detailed discussions of write-ups and write-downs for buyouts.

growth over the same period. This increase likely reflects both organic and acquisition-driven growth. While we lack the data to disaggregate sales growth into these two components, we are able to identify add-on acquisitions post-buyout for 41.3% of the buyouts in our sample. Nearly every add-on acquisition is in the same industry as the related buyout firm, and many occur within a year of completion of the buyout, consistent with the initial buyout target serving as a platform for subsequent acquisitions of related firms. Buyouts with identifiable subsequent add-on acquisitions grow faster than those without add-on acquisitions. Together, the targeting by PE acquirers of private firms likely to have untapped growth potential and the rapid growth in private firms post-buyout – especially those undertaking add-on acquisitions – suggest that relaxing financing constraints and facilitating growth is an important source of value creation in private firm buyouts.

Finally, we examine changes in financial structure after private firm buyouts. The median firm in our sample increases its debt-to-assets ratio by 11.2 percentage points from the pre-buyout year to the first post-buyout year. This increase is meaningful in absolute terms but small relative to the increase in leverage after buyouts of *public* U.S. firms (Cohn, Mills, and Towery, 2014). However, private buyout targets tend to be highly-levered pre-buyout, with a 59% mean debt-to-assets ratio, and may therefore lack the capacity to increase leverage substantially. The fraction of firms paying corporate income tax remains unchanged after buyouts for our sample, suggesting increases in profitability offset any increase in interest tax shields due to an increased debt load. We also find that PE acquirers frequently inject equity capital into the target firm at the time of the buyout (66% of the buyouts in our sample) and over the first three years post-buyout (78%). Overall, our evidence suggests that financial engineering is not a first-order source of value creation in private firm buyouts.

Our paper contributes to the literature on the role of acquisitions, including buyouts, in relaxing financing constraints and promoting growth, which is limited primarily to studies of European firms. Erel, Jang, and Weisbach (2015) find significant increases in growth after acquisitions by operating companies in Europe. Similarly, Bergström, Grubb, and Jonsson (2007) and Boucly, Sraer, and Thesmar (2011) find increases in sales after PE buyouts of mostly private firms in Sweden and France, respectively. In contrast, Cohn, Mills, and Towery (2014) find little evidence of increased sales growth after PE buyouts of public firms in the U.S. Our estimates of the mean sales growth following U.S. private firm buyouts, which range from 115% to 221%, are an order of magnitude larger than the 12% growth rates following Swedish and French buyouts documented by Bergström, Grubb, and Jonsson (2007) and Boucly, Sraer, and Thesmar (2011), respectively. This difference likely reflects the relatively liquid market for acquisitions of U.S. private firms, which enables PE acquirers to use a portfolio company as a platform to acquire other small firms. We further contribute to this literature by demonstrating that PE acquirers systematically target firms that likely have substantial untapped growth potential. In contrast, prior work finds a *negative* relationship between growth opportunities and buyout likelihood for *public* firms (Opler and Titman, 1993; Cohn, Mills, and Towery, 2014), which is generally interpreted as reflecting agency conflicts in public firms that incentivize overinvestment.

We also contribute to the literature examining the effects of PE buyouts on operating performance. Due to data availability, studies have historically focused on public-to-private buyouts. The conclusions of these studies may not be informative about private-to-private buyouts because there are substantial differences in the nature of public and private firms as well as in the identities of PE buyers active in the two markets.[4] Bergström, Grubb, and Jonsson (2007) and

[4] The evidence for public firm PE buyouts in the U.S. is mixed, with earlier papers finding evidence of significant increases in profitability (Kaplan, 1989; Smith, 1990; Smart and Waldfogel, 1994), but more recent work finding little

Boucly, Sraer, and Thesmar (2011) analyze the effects of private-to-private buyouts on operating performance in Sweden and France, respectively, with evidence supporting significant increases in profitability and growth. In addition to the small number of countries studied (two), results for Swedish and French buyouts need not translate to the U.S., where private firms and the market for private firms differ on important dimensions, including the nature of pre-buyout ownership. Another recent strand of the literature analyzes the effects of private-to-private buyouts on non-financial metrics of performance relevant to non-financial stakeholders such as customers and employees, which may not be informative about value creation for investors given the costs of generating such improvements.[5] Our paper adds to the literature by examining measures of performance that should correlate with value creation for investors in U.S. private-to-private buyouts.

Finally, our paper is also the first, to our knowledge, to examine the financing of private firm buyouts. Increases in interest tax shields generate a significant portion of the gains to investors in public firm buyouts (Guo, Hotchkiss, and Song, 2011; Jenkinson and Stucke, 2011; Cohn, Mills, and Towery, 2014). Our results suggest that such financial engineering plays a much smaller role in creating value in private firm buyouts. Our findings also complement prior work documenting the extent to which PE acquirers inject equity into formerly public portfolio firms (Cohn, Mills,

evidence of improvements (Guo, Hotchkiss, and Song, 2011; Cohn, Mills, and Towery, 2014). Acharya et al. (2013) present evidence of increases in profitability after public firm buyouts in Western Europe. Davis et al. (2014) measure significant increases in total factor productivity after PE buyouts but do not distinguish between buyouts of public and private targets in their analysis.

[5] Bernstein and Sheen (2016) find evidence of reductions in health code violations after buyouts of restaurants in the U.S. Eaton, Howell, and Yannelis (2020) find increases in student enrollment after buyouts of for-profit colleges. Fracassi, Privitero, and Sheen (2020) find limited increases in pricing after grocery store buyouts. Gupta et al. (2021) find increases in mortality rates after U.S. nursing home buyouts, though it is unclear what fraction of these acquisitions involve private firms. Cohn, Nestoriak, and Wardlaw (2021) find evidence of improvements in workplace safety records only after *public* firm buyouts.

and Towery, 2014) and financially distressed portfolio firms (Hotchkiss, Smith, and Strömberg, 2021).

2. Sources of Value Creation in PE Buyouts of Private Firms

In this section, we outline three potential sources of value creation in PE buyouts of private firms: improvements in operating performance, relaxation of financing constraints that limit growth, and increased debt tax shields (i.e., financial engineering). We then describe the empirical implications of each source of value creation.

2.1 Sources of Value Creation

We first consider operational improvements as a source of value creation in private firm buyouts. The potential for operational improvements in PE buyouts of public firms may arise from agency frictions in public firms due to the separation of ownership and control (Jensen, 1989). Agency conflicts are generally less of a concern in private firms, where owners typically exert direct control over their firms. Further, private firm targets do not suffer from the potential effects of short-termism due to scrutiny by public market investors or market-based management incentives (Edmans, Fang, and Lewellen, 2017). On the other hand, private firms may be held back by a lack of professional expertise which PE firms might provide. Opportunities for operational improvements are likely to be especially large among poorly performing private firms.

The second source of value creation we consider in private firm buyouts is the relaxation of financing constraints that limit the realization of growth opportunities. Financing constraints are particularly acute for private firms, which typically rely on debt financing to fund growth and have limited access to capital markets. A highly levered private firm could be forced to forgo positive NPV investments because of debt overhang (Myers, 1977). Erel, Jang, and Weisbach (2015) find that acquisitions by operating companies relax target firms' financing constraints. Injections of capital into portfolio firms at the time of the PE buyout and/or after the buyout may similarly relax financing constraints and allow firms to pursue previously untapped growth opportunities. In addition, improvements in cash management as part of overall operational improvements could free up internal resources to finance growth.

The third potential source of value creation in private firm buyouts that we consider is financial engineering. Guo, Hotchkiss, and Song (2011) find that a significant fraction of value creation in *public* firm buyouts is attributable to an increase in interest tax shields, and Cohn, Mills, and Towery (2014) report that additional interest tax shields generated in PE buyouts of public firms result in target firms paying no corporate taxes for several years post-buyout. Tax shields may also be an important source of value creation in private firm buyouts. However, as mentioned above, because most private firms lack access to large amounts of equity, they typically meet their external capital needs through debt financing from banks. As a result, many private buyout targets already have highly leveraged balance sheets, limiting the scope for financial engineering. Further, private firms are typically smaller than public firms, and smaller firms face larger bankruptcy costs as a proportion of assets (Altman, Hotchkiss, and Wang, 2019), which may discourage PE acquirers from heavily increasing the leverage of these firms.

2.2 Empirical Implications

Each of the three sources of value creation we consider has different empirical implications for both the types of private firms PE acquirers should target and for expected outcomes post-buyout. If operational improvements are an important source of value creation in private firm buyouts, then we should observe PE acquirers targeting less profitable private firms, where the scope for improving operations is large. We should also subsequently observe increases in profitability, especially among firms that are less profitable pre-buyout.

If relaxing financing constraints is an important source of value creation in private firm buyouts, then we should observe PE acquirers targeting private firms with valuable growth opportunities that they are unable to finance themselves. Specifically, we expect PE acquirers to target profitable firms, which presumably have better growth opportunities, that rely heavily on external financing and already have high debt loads pre-buyout. If relaxing financing constraints is an important source of value creation, we should also observe slow sales growth before buyouts followed by increased growth post-buyout, which can take the form of organic growth, acquisitions, or both. In addition, we should observe capital injections at both the time of the buyout and in subsequent years to support this growth. We note that if both improving profitability and relaxing financing constraints are important sources of value creation, we may observe PE firms targeting private companies at both the low and high ends of the profitability distribution.

Finally, if financial engineering is an important source of value creation in private firm buyouts, we should observe PE acquirers targeting low-leverage firms with the greatest scope for increasing interest tax shields. We should also observe large increases in debt loads in the year of the buyout that persist post-buyout. More directly, we should observe a decrease in the fraction of firms paying corporate income tax after buyouts, as the objective of increasing interest tax shields is to minimize corporate tax payments.

3. Data and Sample

In this section, we describe our data sources, the construction of our sample, the variables we use in our analyses, and characteristics of sample deals. Appendix A provides definitions for each of our variables.

3.1 Data Sources

We identify PE buyouts of private firms using data from Thomson Financial's Securities Data Corporation (SDC) Platinum Mergers database and Capital IQ (CIQ). We obtain financial information from confidential corporate tax return data in the IRS Business Return Transaction File (BRTF) for all C corporations with at least $10 million of total assets. Given this data source, our analysis is informative about buyouts of private C corporations involving firms with at least $10 million of assets and may not generalize to other private firm buyouts. However, we note that the buyouts in our sample are likely to disproportionately represent the largest and hence most economically important private firm buyouts. Our dataset includes select line items from U.S. Corporation Income Tax Return Form 1120, including income and expense data (Form 1120 Page 1) and balance sheet data (Form 1120 Schedule L). The advantage of these data relative to traditional sources of financial information is that all corporations, both publicly-traded and private, are required to file tax returns, which enables us to examine firms that are private both before and after a PE buyout.

3.2 Sample

Table I Panel A summarizes the construction of our sample. We first identify buyouts of private U.S. firms between 1995 and 2009 that appear in both SDC and Capital IQ. For each potential buyout, we use news sources to verify the transaction. We exclude transactions that were not completed and those for which we are unable to verify completion. While this approach likely excludes some valid private firm buyouts, the excluded buyouts likely involve firms too small to meet the $10 million minimum total assets threshold for inclusion in the BRTF dataset. Using CIQ and news sources, we are able to verify 1,504 valid transactions. We then remove misclassified buyouts, buyouts of bankrupt firms, partial buyouts, and REIT buyouts.[6] Because the BRTF dataset only includes C corporations, we also remove firms that are not organized as C corporations (partnerships, LLCs, and S corporations). Appendix B.1 provides deal characteristics for comparison to our sample of C corporation buyouts. These filters yield an initial sample of 639 verified PE buyouts of private, non-bankrupt C corporations. From this initial sample, we also remove: (i) 87 buyouts where the target firm has less than $10 million in assets pre-buyout, and (ii) 110 buyouts where the acquired firm is merged with another operating entity in the PE acquirer's portfolio concurrently with the buyout. We exclude the latter because we cannot perform valid pre- to post-buyout comparisons for such firms. This process leaves us with a sample of 442 buyouts that we attempt to match to the BRTF.

--- Insert Table I about here ---

Of the 442 remaining buyout firms, we are able to identify 403 firms present in the BRTF in at least one year based on the name of the target firm. We manually search for each target firm in the BRTF data using the target firm's name. Of these 403 firms, 288 are present in the BRTF in year *t-1*, which is necessary for measuring pre-buyout characteristics. We use this sample of 288 private buyout targets in our tests of buyout determinants.

We also analyze the evolution of private buyout firms from before to after PE acquisitions. For this part of our analysis, we further require data for at least years *t+1* and *t+2* post-transaction. Two complications arise here. First, the name of the acquired firm sometimes changes at the time of the buyout. For example, the PE acquirer in some cases creates a holding company that acquires

[6] We rely primarily on SDC for our sample of buyouts for comparability to Cohn, Mills, and Towery (2014), who also use BRTF data. CIQ reports significantly more private firm buyouts, with 12,567 reported during our sample period. However, a majority of the additional buyouts appear to be small firms. For example, only 2,047 of the buyouts report transaction values exceeding $10 million, and even those with more than a $10 million transaction value may involve firms with less than $10 million of total assets – the minimum size for inclusion in the BRTF. In addition, many small firms are organized as S corporations and partnerships, and are thus excluded from the BRTF dataset.

the target firm and is the surviving legal entity. We use information from CIQ and news sources to identify as many of these name changes as possible. Second, in some cases, the acquired firm is converted from a C corporation to a flow-through entity at the time of the acquisition and therefore disappears from the BRTF data. We see no obvious reason why the loss of these firms from the post-buyout sample should induce any biases in our analysis.[7] The post-buyout data requirement leaves us with a sample of 240 buyouts for which we can compare pre- and post-buyout firm characteristics.

Table I Panel B presents the number of transactions by year. The number of private firm buyouts grows substantially from 2003 to 2008, before decreasing during the height of the financial crisis in 2009. Of the 288 buyouts in our determinants sample, 214 (74.3%) are completed between 2003 and 2009. Though increased market coverage by SDC and CIQ likely explains some of the increase over time, the increase is also consistent with the tremendous growth in U.S. PE buyouts in the mid-2000s (Kaplan and Strömberg, 2009). Table I Panel B also presents the distribution of buyouts over the sample period for subgroups based on the availability of post-buyout data. As noted above, 48 firms used in our buyout determinants analysis are no longer in the BRTF data in years *t+1* and *t+2*. The sample size falls more significantly by year *t+4*. We address potential survivorship bias in Section 5.5.

We present the number of transactions by Fama-French 12 industry in Table I Panel C. The most common industries represented in our sample are Manufacturing (21.9% of PE buyout transactions) and Wholesale, Retail, and Some Services (23.6% of PE buyout transactions). Still, Panel C shows that a broad range of industries are included in our sample.

[7] Based on discussions with PE sponsors, the two primary reasons for a post-buyout change in organizational form are: (i) limitations in the possible form based on the pre-buyout ownership structure, and (ii) limitations on the type of income that can be allocated to certain tax exempt limited partners of the purchasing fund.

3.3 Variable Construction

We construct several of our variables using the BRTF data. We define *ln(TotalAssets)* as the natural logarithm of total assets (*TotalAssets*) reported on Form 1120 Schedule L Line 15. We construct two measures of operating performance. First, we define pre-interest return on assets (*PreInterestROA*) as *PreInterestInc* divided by total assets. *PreInterestInc* equals taxable income (*TaxableInc* from Form 1120 Page 1 Line 28) plus the interest deduction (*IntDeduction* from Form 1120 Page 1 Line 18). We focus on pre-interest income because we are interested in studying operating profitability, without regard to financing. *PreInterestInc* is the tax return-based analog of earnings before interest and taxes (EBIT) as computed from financial statements. Second, we define pre-interest return on sales (*PreInterestROS*) as *PreInterestInc* divided by *Sales* (Gross Receipts or Sales from Form 1120 Page 1 Line 1). We focus on *PreInterestROS* rather than *PreInterestROA* when we study changes in operating performance post-buyout because write-ups and write-downs of reported asset values at the time of a buyout cause changes in the denominator of *PreInterestROA* that are unrelated to actual changes in profitability.

We define *SalesGrowth* as the one-year percentage growth in *Sales*. Our leverage measure (*DebtToAssets*) equals interest-bearing liabilities (*IntBearingLiab*) divided by *TotalAssets*, where *IntBearingLiab* equals short-term and long-term mortgages, notes, and bonds payable (Form 1120 Schedule L Lines 17 and 20).[8] The indicator variable *PosTaxPdInd* is equal to one if a firm's taxes paid in a given year (total tax reported on Form 1120 Page 1 Line 31) are positive and zero otherwise. Finally, we define *Contributions* as the one-year change in paid-in capital from Form

[8] We provide two caveats with respect to our leverage measure. First, some of a private firm's debt may be owed to the owners of the firm, likely in the form of subordinated debentures. To the extent that this debt closely resembles equity, our leverage measure will overstate a firm's true leverage. Second, some have argued that operating leases should be treated as debt for purposes of calculating leverage ratios, but we do not observe operating leases because they are not included in debt.

1120 Schedule L Line 23. Our analysis of contributions is based on a smaller number of observations because we only observe paid-in capital beginning with the 2005 tax year.

We construct two additional variables using Compustat data. *IndustryQ* is defined as the median value of Tobin's Q for all publicly-traded firms in a firm's 3-digit NAICS code industry. We define Tobin's Q as market value of assets divided by book value of assets, where the market value of assets equals the market value of equity plus the book value of debt. As is common in the literature, we treat Tobin's Q as a proxy for growth opportunities. We use an industry-level measure rather than a firm-level measure because equity market values are not available for private firms. *ExtFinDep* captures the extent to which a firm is likely to depend on external financing to fund growth. We follow Rajan and Zingales (1998) and define *ExtFinDep* as industry-level capital expenditures less industry-level net cash flow from operating activities plus industry-level change in net working capital, divided by industry-level capital expenditures. We winsorize all continuous variables at the 2.5th and 97.5th percentiles to limit the influence of potential outliers.

3.4 Private Firm Sample Characteristics

Table II provides descriptive statistics as of year *t-1* (pre-buyout) for the sample of 288 private PE buyout firms included in our determinants analysis. Not surprisingly, our sample firms are substantially smaller than public firms targeted in PE buyouts. The mean (median) value of *TotalAssets* for our sample is $97.5 million ($45.2 million). For comparison, the public buyout firms studied by Cohn, Mills, and Towery (2014) have mean (median) *TotalAssets* of $921 million ($253 million), calculated using the same BRTF data. Firms in our sample have a significant amount of debt pre-buyout, with median *DebtToAssets* of 58.6%, consistent with private firms relying primarily on debt financing. In contrast, Cohn, Mills, and Towery (2014) report median *DebtToAssets* of 43.2% in year *t-1* for public-to-private buyout firms. The high pre-buyout debt levels of private targets may limit the additional leverage that PE buyers choose to add in the buyout itself, a possibility to which we return later.

--- Insert Table II about here ---

Interestingly, mean and median pre-buyout *SalesGrowth* are both negative. We observe wide variation in pre-buyout profitability. 56.9% of buyout firms have positive taxable income in the year before the buyout. Panels B and C report summary statistics for subsamples of buyout firms in the top and bottom quintiles of pre-buyout profitability, respectively, within the overall distribution of *PreInterestROA* across all firms in the BRTF data. Reporting separate descriptive statistics for buyout firms in the top and bottom quintiles of pre-buyout profitability reveals substantial differences between the highest and lowest performing target firms. Firms in the top quintile of pre-buyout profitability have mean (median) *PreInterestROA* of 0.218 (0.202), while the mean and median *PreInterestROA* for firms in the bottom quintile of pre-buyout profitability are negative. Firms in the top quintile of performance pre-buyout have median sales growth of 11.6%, while those in the bottom quintile have median sales growth of -47.0%.

PE acquirers undertaking buyouts of private firms in our sample have limited overlap with PE acquirers undertaking buyouts of publicly-traded firms studied in prior research. Specifically, of the 200 different PE acquirers for the sample of public-to-private buyouts that Cohn, Mills, and Towery (2014) analyze, only 58 are involved in any of the private-to-private buyouts in our sample. Moreover, none of the ten most active PE acquirers in their sample are among the ten most active in our sample. PE acquirers focusing on acquiring private firms typically have smaller fund sizes than those focusing on acquiring public firms (Hotchkiss, Smith, and Strömberg, 2021).

This lack of overlap is potentially important because it is unclear *a priori* that the smaller PE acquirers that specialize in acquiring private firms have the resources to provide the types of operational engineering services to their portfolio firms that larger PE acquirers often do. Bulge-bracket PE acquirers such as Apollo, Blackstone, and KKR, which target larger firms, often maintain large operational consulting staffs that they can deploy to portfolio firms; smaller PE firms generally do not have the resources to maintain such staffs. On the other hand, even small PE firms may be able to professionalize management, a potentially important lever for improving the performance of private firms specifically.

In Table III, we further examine non-financial characteristics of the 288 private firm buyouts with pre-buyout tax return data available.[9] Almost 30% of private firm buyouts in our sample are structured as management buyouts, which is substantially greater than the percentage of management buyouts in recent studies of public firm buyouts (e.g., Cohn, Mills, and Towery, 2014). The target firm CEO remains a significant shareholder following the buyout transaction nearly half of the time (42.8%). The seller is the founder or a member of the founder's family only 14.4% of the time. This percentage is substantially lower than the percentage of founder sellers in the sample of French buyouts that Boucly, Sraer, and Thesmar (2011) study, which consists primarily of family-controlled businesses, and is too small to allow for meaningful comparisons of buyouts of founder-owned and non-founder owned firms. We also note that the mean (median) target firm in our sample is 34 (27) years old, suggesting that our sample firms are not start-up firms. The 'shakeup' from a buyout may be necessary for mature private firms to adapt to technological and marketplace changes.

--- Insert Table III about here ---

[9] Missing observations in Table III are due to a small number of buyouts for which information could not be verified from CIQ, SDC, Preqin, Factset, or news articles. These deals also do not have post-buyout tax return data available and are therefore not included in our analysis of post-buyout performance.

4. Determinants of Private Firm Buyouts by PE Acquirers

We begin our analysis by examining the empirical determinants of private firm buyouts by PE acquirers. Doing so allows us to shed light on the sources of value creation as reflected by PE firms targeting certain types of private firms. To our knowledge, ours is the first paper to examine these determinants for private PE buyout targets. To predict which private firms PE acquirers target, we estimate a linear probability regression model using 199,646 private firm-year observations included in the BRTF data. The dependent variable, *BuyoutInd*, is an indicator variable equal to one if a firm is acquired by a PE firm during the year, and zero otherwise. Table IV presents the results.

--- Insert Table IV about here ---

We present five regression specifications. We begin with a basic specification in column (1) with *ln(Assets)* and *PreInterestROA* as the explanatory variables. If PE acquirers are motivated by the opportunity to improve operating performance for poorly-performing firms, then we expect a higher buyout likelihood for firms with lower *PreInterestROA*. On the other hand, PE acquirers could target private firms with untapped growth opportunities because of financial constraints. With declining returns to scale, a financially constrained firm should exhibit both high average and marginal returns on investment. Thus, if private firm buyouts are motivated largely by the opportunity to relax financing constraints for firms with unrealized growth opportunities, then we might observe a higher buyout likelihood for firms with higher *PreInterestROA*. We observe the latter: Among private firms, PE buyers appear to target relatively profitable firms. They also target larger firms, which is not surprising. To the extent that there are fixed costs of completing buyouts and of overseeing and implementing changes in target firms post-buyout, we should observe PE acquirers disproportionately targeting larger private firms, all else equal.

While the results in column (1) appear more consistent with PE acquirers targeting firms with greater growth potential rather than greater scope for operating improvements, the two possibilities are not mutually exclusive. A simple linear specification makes it impossible to assess whether both motives affect PE acquirers' choice of buyout targets. To assess this possibility, in column (2), we replace the continuous measure of *PreInterestROA* with *PreInterestROA* quintile indicator variables. Doing so provides a simple way to test whether PE acquirers systematically target private firms at both ends of the profitability distribution. We define *PreInterestROAQn* as one if a firm is in the *nth* quintile of *PreInterestROA*, for $n = 1, ..., 5$, and zero otherwise, with the least profitable firms in quintile 1 and the most profitable firms in quintile 5. The coefficient on *PreInterestROAQn* represents the difference in the probability of being acquired in a PE buyout between quintile *n* and quintile 3, the omitted quintile.

The results in column (2) suggest that the positive coefficient on *PreInterestROA* reported in column (1) obscures a non-monotonic relation between PE buyout likelihood and profitability. All four of the quintile indicator coefficients are positive. However, the coefficients on *PreInterestROAQ1* and *PreInterestROAQ5* are substantially larger than the coefficients on *PreInterestROAQ2* and *PreInterestROAQ4* and are both statistically significant at the 1% level. Because these four coefficients represent estimates relative to firms in the middle quintile of profitability, the estimates indicate a U-shaped relationship between PE buyout likelihood and profitability. This non-monotonicity hints at the possibility that the opportunity to turn around struggling firms and the opportunity to unlock faster growth at better-performing firms are both motives for PE buyouts of private firms.

If unlocking growth is an important source of value creation in private firm buyouts, then we should observe PE acquirers targeting firms that, in addition to having valuable investment opportunities, lack the financing capacity to pursue these investments. We identify firms with high debt loads and a greater reliance on external financing as being less able to finance investment opportunities. If financial engineering is an important source of value creation, then we should observe PE acquirers targeting firms with low leverage because more debt can be added to the balance sheets of these firms without inducing financial distress, all else equal.

Motivated by these arguments, we add three additional explanatory variables to the regression in column (3): *DebtToAssets*, *ExtFinDep*, and *IndustryQ*. For the sake of parsimony, we remove the second and fourth profitability quintile indicator variables from the model, leaving only the extreme profitability quintile indicator variables. The coefficients on the indicator variables for the two extreme quintiles of profitability represent the difference in the probability of being a target relative to firms in the middle three quintiles. Consistent with PE acquirers targeting more highly levered private firms with better growth opportunities, the coefficients on *DebtToAssets* and *IndustryQ* are both positive, though only the former is statistically significant. However, the coefficient on *ExtFinDep* is negative, and we acknowledge that these three variables could proxy for other firm or industry characteristics. The positive coefficient on *DebtToAssets*, indicating that more highly levered firms have a greater likelihood of a buyout, appears inconsistent with financial engineering being a primary motive for PE buyouts of private firms.

We sharpen our analysis of the role of relaxing financing constraints by examining whether firms at the intersection of favorable growth opportunities, high leverage, and external financing dependence are disproportionately represented among private firm buyout targets rather than examining the relevance of these factors separately, as in column (3). Specifically, in column (4), we add the two-way interactions between *IndustryQ*, *ExtFinDep*, and *DebtToAssets* as well as their three-way interaction. The coefficient on the triple interaction (the last variable included in column

(4)) is positive and statistically significant at the one percent level. All else equal, this finding suggests that PE acquirers target private firms with substantial growth potential that are dependent on external financing for growth but are already higher levered – firms where the ability to relax constraints and unlock untapped growth opportunities is likely to be especially valuable.

Finally, we substitute the high profitability indicator variable (*PreInterestROAQ5*) for *IndustryQ* as the measure of growth opportunities in the triple interaction term and present the results in column (5). Consistent with the results in column (4) and further supporting our interpretation, the coefficient on the triple interaction of *PreInterestROAQ5*, *ExtFinDep*, and *DebtToAssets* is positive and statistically significant at the one percent level.

Overall, our analysis of private firm buyout determinants supports two motives driving these acquisitions – (i) the opportunity to turn around struggling firms and (ii) the opportunity to unlock growth potential by alleviating the financing constraints that private firms often face. In the next section, we examine post-buyout changes in profitability, growth, and leverage after buyouts to shed further light on the sources of value creation in private firm buyouts.

5. Evolution of Profitability, Growth, and Capital Structure Around Private Firm Buyouts

In this section, we examine the evolution of profitability, sales growth, and capital structure around the PE buyouts of the 240 private firms for which we have at least the first two years of post-buyout data. We compare the changes in *PreInterestROS*, *SalesGrowth*, and *Leverage* for private PE buyout firms to three different benchmarks: (i) the median change for firms in the same 3-digit NAICS industry code over the same period of time, (ii) the change for a propensity score-matched control sample, and (iii) the change for a matched control sample based on pre-buyout profitability.

Comparing private PE buyout firms to other firms in the same industry filters out industry-wide time-series variation in business conditions and financial incentives. Propensity score matching allows us to compare buyout firms to non-buyout firms that are similar on multiple dimensions. We construct the propensity score-matched sample by matching each buyout firm with an unacquired control firm based on year *t-1* characteristics using the model shown in Table IV Column (4). Matching on pre-buyout profitability helps ensure that we compare firms with similar levels of profitability pre-buyout.[10] To construct the profitability-matched control sample, we match each buyout firm with an unacquired firm in the same industry with *PreInterestROA* within one percentage point of the buyout firm's *PreInterestROA* in each of years *t-1* and *t-2*.[11] We match on both year *t-1* and year *t-2* profitability to ensure that control firms in the performance-matched sample are similar to acquired firms not only in terms of pre-buyout profitability, but also in terms of the *trend* in pre-buyout profitability.

5.1 Changes in Profitability after Private Firm Buyouts

Figure 1 plots the trends in profitability as measured by *PreInterestROS* for years *t-2* through *t+3* relative to the buyout year *t*. The figure plots the median value of *PreInterestROS,* as well as the median values relative to each of our three benchmarks. We focus on medians rather than means in much of the remaining analysis because, even after winsoring, a few cases with particularly large reported values distort the means. The figure shows a decline in *PreInterestROS* from year *t-2* to year *t-1* but substantial increases in *PreInterestROS* post-buyout. Median *PreInterestROS* increases by more than four percentage points from year *t-1* to year *t+2* relative to the propensity score-matched control sample and by more than three percentage points relative

[10] Barber and Lyon (1996) and Lie (2001) emphasize the importance of matching on pre-event performance.

[11] We define industries using 3-digit NAICS codes. If multiple firms meet our matching criteria, we select the match firm with the closest *PreInterestROA* in year *t-1*. If there are no match firms with the same 3-digit NAICS code, we relax this criterion and look for matching firms with the same 2-digit NAICS code or 1-digit NAICS code.

to the performance-matched control sample. The recovery of unadjusted, industry-adjusted, and propensity score match-adjusted *PreInterestROS* in the post-buyout period after the decline from year *t-2* to year *t-1* might have occurred even absent the buyout due to mean reversion. However, the increase in performance match-adjusted *PreInterestROS* post-buyout helps to allay concerns about counterfactual mean reversion because matching on both year *t-1* and year *t-2* profitability ensures the absence of differential pre-buyout trends.

--- Insert Figure 1 about here ---

We more formally estimate changes in profitability after private firm buyouts by calculating the change in *PreInterestROS* from year *t-1* to years *t+1*, *t+2*, *t+3*, *t+4*, and the year of the PE acquirer's exit for each buyout firm, both in absolute terms and relative to each of its three benchmarks.[12] Table V reports the changes in *PreInterestROS* for all 240 firms (Panel A) and for firms in the top and bottom quintiles (Panels B and C) of pre-buyout profitability, respectively.

--- Insert Table V about here ---

Consistent with Figure 1, the results in Panel A of Table V show a significant increase in *PreInterestROS*, both in absolute terms and relative to each of the three benchmarks. The mean and median changes in *PreInterestROS* relative to year *t-1* are positive over all horizons and relative to all benchmarks. These changes are large in magnitude and are statistically significant in all but a few cases. Even the performance-adjusted increase in *PreInterestROS* is statistically significant in most cases, further allaying concerns about potential mean reversion in profitability.

Comparing the top pre-buyout profitability group (Panel B) with the bottom pre-buyout profitability group (Panel C), firms in the bottom pre-buyout profitability group appear to

[12] If a buyout firm is still owned by the PE acquirer or the date of exit is not identified, we use the last available year of BRTF data as the exit year.

experience larger increases in *PreInterestROS* post-buyout than firms in the top pre-buyout profitability group, both in absolute terms and relative to the industry and propensity-matched benchmarks. However, performance-adjusted changes in *PreInterestROS* appear more similar for the low pre-buyout profitability group and the high pre-buyout profitability group. The fact that the performance-adjusted increases are larger than increases relative to the other benchmarks for the top pre-buyout profitability group and smaller for the bottom pre-buyout profitability group suggests that mean reversion may explain some of the post-buyout changes. However, the fact that changes in *PreInterestROS* remain positive for both groups suggests that profitability increases after buyouts, even after accounting for the possibility of mean reversion. Overall, the results in Table V suggest that increases in profitability after private firm buyouts are larger for firms with low profitability pre-buyout, suggesting that turning around struggling firms is one mechanism through which PE acquirers create value in private firm buyouts.

5.2 Changes in Sales Growth after Private Firm Buyouts

Figure 2 plots the trends in median *SalesGrowth* for years *t-2* through *t+3* relative to the buyout year *t*, both in absolute terms and relative to each of our three benchmarks. Although *SalesGrowth* declines from year *t-2* to year *t-1* in the pre-buyout period relative to the industry and propensity score-matched benchmarks, it does not decline meaningfully relative to the performance-matched sample. The plot shows a significant increase in *SalesGrowth* after buyouts. Sales growth jumps in the first post-buyout year and remains somewhat elevated the second year post-buyout before falling to near zero in the third year post-buyout.

--- Insert Figure 2 about here ---

Like our analysis of changes in profitability, Table VI reports the changes in *SalesGrowth* from year *t-1* to years *t+1, t+2, t+3, t+4,* and the year of the PE acquirer's exit for each buyout firm in absolute terms and relative to each of its three benchmarks. Panel A reports results for all PE buyouts, while Panels B and C report results for the top and bottom pre-buyout profitability quintile groups, respectively. Consistent with Figure 2, Panel A of Table VI shows a large, sustained increase in sales growth after private firm buyouts, both in absolute terms and relative to benchmarks. The mean (median) percentage increase in sales from year *t-1* to year *t+1* is 139.0% (52.5%) relative to the industry benchmark and 114.9% (37.1%) relative to the performance benchmark. The growth appears to result in permanently higher sales, with no reversal through at least year *t+4*.

--- Insert Table VI about here ---

Sales growth increases sharply after PE buyouts for firms in both the highest quintile of pre-buyout profitability (Panel B) and the lowest quintile of pre-buyout profitability (Panel C). The increases for these two groups likely have slightly different interpretations. The increase for the high-profitability group is consistent with high return on sales corresponding to large unrealized growth opportunities due to financing constraints. The increase for the low profitability group could reflect an increase in the optimal scale of the firm due to the increase in profitability documented in Table V Panel C. At a minimum, the increase in sales growth does not appear to come at the expense of profit margins given that *PreInterestROS* generally increases post-buyout for both the high and low profitability groups.

We further investigate the role of add-on acquisitions in driving sales growth. Using Capital IQ and Preqin, we identify add-on acquisitions post-buyout for 44.2% of the buyout firms in our sample. However, this figure likely understates the fraction of firms acquired in private firm buyouts that undertake add-on acquisitions because add-on acquisitions often involve buying small firms, for which data coverage is generally limited. Nevertheless, our finding that more than 40%

of the buyouts in our sample involve post-buyout add-on acquisitions suggests that a portion of the post-buyout sales growth increase documented in Table VI likely reflects growth through acquisitions.

To further assess the importance of add-on acquisitions in fueling post-buyout growth, we divide our sample of buyouts into two subsamples – those where we are able to identify post-buyout acquisitions by the target firm and those for which we can identify no such add-on acquisitions. We then re-estimate changes in sales growth for each of these two subsamples. Table VI Panels D and E report the results. The results show that sales growth is much larger for firms that undertake identifiable add-on acquisitions, though it is large even for those that do not. Although we cannot decompose total sales growth into that driven by organic versus external growth, our results suggest that add-on acquisitions play a role in explaining total sales growth after private firm buyouts.

5.3 Changes in Capital Structure after Private Firm Buyouts

Figure 3 plots the trends in *DebtToAssets* for years *t-2* through *t+3* relative to the buyout year *t*. We observe little change in *DebtToAssets* from year *t-2* to *t-1* but a significant increase in the ratio from year *t-1* to year *t*. While we do not observe the amount of debt used to finance the buyout itself, the increase likely represents the effect of buyout debt on the target firm's balance sheet. Leverage continues to increase gradually in years *t+1* through *t+3*. As the summary statistics in Table II show, private firms acquired in buyouts have relatively high leverage pre-buyout.

--- Insert Figure 3 about here ---

We formally estimate changes in leverage after private firm buyouts by calculating the change in *DebtToAssets* from year *t-1* to years *t+1*, *t+2*, *t+3*, *t+4*, and the year of the PE acquirer's exit for each buyout firm. Table VII reports the means and medians of these changes for all firms

(Panel A) and for firms in the top and bottom quintiles of pre-buyout profitability (Panels B and C), respectively.

--- Insert Table VII about here ---

The mean (median) increase in debt-to-assets from year *t-1* to year *t+1* in excess of the industry benchmark is 0.18 (0.14) and in excess of the performance benchmark is 0.15 (0.11). The increase in leverage reported by Cohn, Mills, and Towery (2014) for public-to-private PE buyouts using the same tax return data is substantially larger, with a mean (median) increase of 0.30 (0.32). However, they also document considerably lower pre-buyout leverage ratios for public-to-private PE buyout targets. The relatively high pre-buyout leverage and small leverage increase after private firm buyouts suggest that financial engineering plays a more limited role in creating value for investors in private firm buyouts.[13]

To further investigate the possible role of financial engineering as a source of value creation in private firm buyouts, we also compute the fraction of buyout firms reporting positive tax payments in the years around the buyout. Figure 4 plots this fraction for each of the years *t-1* through *t+3*. This fraction remains virtually unchanged throughout the window around the buyout, falling from 55% in year *t-1* to 53% in year *t*, before rising to 55% again in year *t+1*. While Figure 3 and Table VII show that private firms do increase leverage after buyouts, rising operating profitability appears to offset the increase in tax shields due to higher interest payments for many firms. The lack of a decline in the fraction of buyout firms paying taxes raises further doubts about the importance of financial engineering in creating value for investors in private firm buyouts.

--- Insert Figure 4 about here ---

5.4. Equity Injections into Portfolio Firms

[13] Guo, Hotchkiss, and Song (2011) show that tax benefits from increased debt explain as much as one third of the total return to PE acquirers' invested capital in buyouts of public firms.

We complete our analysis of how private PE buyout targets evolve after being acquired by a PE firm by examining equity injections that take place around the time of the buyout. Our finding that PE acquirers target firms that likely have untapped growth options, combined with our finding that target firms' sales grow rapidly post-buyout, suggests that facilitating growth by relaxing financing constraints plays an important role in creating value. One way for PE acquirers to relax financing constraints is to inject equity capital into the target firm. PE acquirers may inject capital both as part of the buyout transaction and after the transaction as needed. Table VIII reports equity contributions for the subset of sample firms with sufficient data to observe these contributions.

--- Insert Table VIII about here ---

Mean (median) equity contributions in the year of the buyout, most of which are likely tied to the buyout itself, are \$22.5 million (2.3 million). We also observe mean equity contributions of \$3.1 million, \$4.2 million, and \$7.5 million in years *t+1*, *t+2*, and *t+3*, respectively. While fewer than half of acquired firms receive a positive equity contribution in each of these three years, 78.2% of firms receive equity contributions in at least one of these three years (untabulated). The sizable equity injections both in the year of the buyout and in subsequent years provide further evidence that relaxing financing constraints is a significant source of value creation in private firm buyouts.

5.5 Identification Challenges and Additional Robustness Tests

In analyzing changes in profitability, sales growth, and capital structure around private firm PE buyouts, we account for counterfactual changes by comparing raw changes to an industry benchmark, a propensity-matched control sample, and a performance-matched control sample. However, survivorship bias is a potential concern. Our initial sample of 288 buyouts, used to examine determinants of buyouts, requires firms to have data in year *t-1*. We do not observe post-buyout data for 48 of these buyouts, and we therefore exclude them from our analysis of pre- to post-buyout changes. Further, firms that we include in our analysis of post-buyout performance may not have data available for all post-buyout years through year *t+4*. There are three primary reasons a firm would not have post-buyout data: it converts from a C corporation to a flow-through entity, its total assets fall below the $10 million threshold for inclusion in the BRTF tax return data, or it ceases to exist or is acquired by another operating company.

Because the BRTF made available to us only includes C corporations, a buyout firm will cease to be present in the BRTF data if it converts to an S corporation, a partnership, or an LLC (i.e., a flow-through entity). The double taxation of C corporation income might motivate such a conversion. If the decision of whether to convert organizational form is based primarily on factors independent of firm characteristics, then bias in our estimates due to conversions should be limited.

Buyout firms could also leave our sample because they fall below the $10 million total assets threshold for inclusion in the BRTF tax return data in the post-buyout period. The omission of these firms from the sample would likely cause us to overestimate average sales growth post-buyout. To address this specific concern, we increase the total assets threshold for entering our sample to $20 million and re-estimate changes in profitability, sales growth, and leverage for the resulting sample as a robustness test. Increasing the threshold for entry into the sample to $20 million in total assets greatly reduces the likelihood of a firm falling out of the sample due to shrinkage because even the smallest firm's assets would need to decrease by at least 50% to fall below the $10 million tax return data threshold. We report the results from these tests in Appendix B.2. The results change little when we impose this sample restriction.

Finally, buyout firms could leave our sample because they go public via an IPO, are acquired by other companies, or fail. The first of these possibilities would bias our estimates of changes in performance for remaining firms downwards (as successful firms leave the sample), the impact of the second is ambiguous, and most importantly the third (firm failures) would bias our estimates upwards. For the 48 firms not included in our analysis of post-buyout performance, we find only one firm that filed for bankruptcy before year *t+2* and none that go public via IPO in that window, with the remaining firms largely acquired or merged into other PE portfolio companies. This analysis suggests that survivorship bias due to firms going bankrupt before year *t+2* is small. Firms that exit the sample quickly after the buyout due to an acquisition are often merged into another portfolio company of the same PE investor.

For our reported analysis of post-buyout performance, firms often leave our sample because data is not available following the PE exit; therefore, we also report in Tables V to VII the firm level changes from year *t*-1 to the earlier of year *t+4* or the year of the PE exit. To better understand why firms depart the sample, we search Capital IQ for information about how and when each of the 240 firms in our sample exit buyout status. Appendix B.3 presents this breakdown. We find that 76.2% exit via sale to either a strategic buyer, another PE buyer, or another portfolio company, 9.2% exit via bankruptcy, and even fewer (3.8%) exit via IPO, which is not surprising given the relatively small size of most firms in our sample. Moreover, the bankruptcies we observe do not occur within the first few post-buyout years (among target firms in our sample that go bankrupt, the median time to bankruptcy is 5.5 years post-buyout). Thus, it appears unlikely that survivorship bias due to failures is likely to substantially affect our estimates.

We also consider whether the choice to operate as a private firm pre-buyout could proxy for other firm characteristics that explain the differences in changes in profitability, sales growth, and capital structure between private firm buyouts and public firm buyouts. Most importantly, private targets are much smaller than public buyout targets, and it is possible that the effects of a buyout on profitability, sales growth, and leverage are a function of firm size. We address this possibility by constructing a subsample of PE private buyout firms from our full sample with total assets of at least $92.7 million at the time of the buyout and re-estimating changes in our outcome variables after buyouts in this subsample. We choose $92.7 million as the cutoff, as this amount represents the 25th percentile of total assets in the sample of public firm buyouts analyzed by Cohn, Mills, and Towery (2014) and thus should provide some comparability. The results are presented in Appendix B.4. We observe few differences between changes in profitability, sales growth, and leverage in this subsample in comparison to our full sample results. This analysis provides some assurance that firm size does not explain the different pattern of results for public and private firm buyouts.

6. Conclusion

This paper investigates the sources of value creation in PE buyouts of private firms in the U.S. Using financial data from U.S. corporate tax returns, our evidence suggests that unlocking growth opportunities by relaxing financing constraints is a primary source of value creation for private firm buyouts. Unlike publicly-traded firms, private firms cannot generally issue equity to raise capital needed to finance investment and may become financially constrained when they reach their debt capacities. While many have argued that PE acquirers purchase *public* firms to solve *overinvestment* problems, our results highlight the role of *private* firm buyouts in solving *underinvestment* problems.

We also provide evidence that improvements in profitability play a role in creating value in private firm buyouts and that PE acquirers are able to turn around struggling firms. We find little evidence to support financial engineering as an important source of value creation in private firm buyouts. Private firm targets tend to already be highly levered at the time of the buyout, increases in leverage in conjunction with the buyout tend to be relatively small, and the fraction of firms paying taxes is essentially unchanged from before to after buyouts. Overall, our analysis of PE buyouts of private firms highlights important contrasts with PE buyouts of public firms and provides insight into the mechanisms by which PE sponsors add value for this large segment of the buyout market.

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Figure 1. Trend in pre-interest ROS. This figure presents trends in median *PreInterestROS* for private PE buyout firms. Year *t* represents the buyout year.

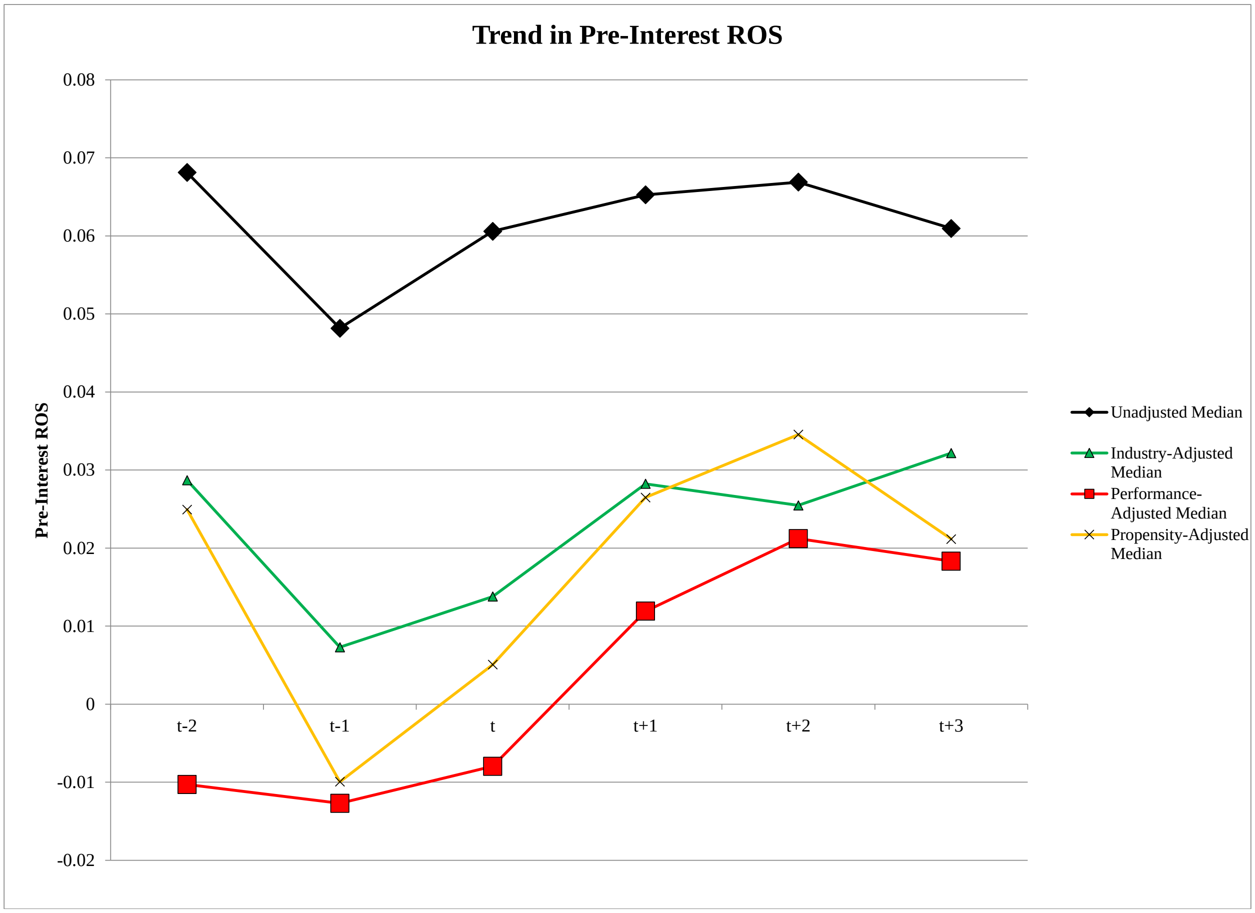

Figure 2. Trend in sales growth. This figure presents trends in median *SalesGrowth* for private PE buyout firms. Year *t* represents the buyout year.

Figure 3. Trend in leverage. This figure presents trends in median *DebtToAssets* for private PE buyout firms. Year *t* represents the buyout year.

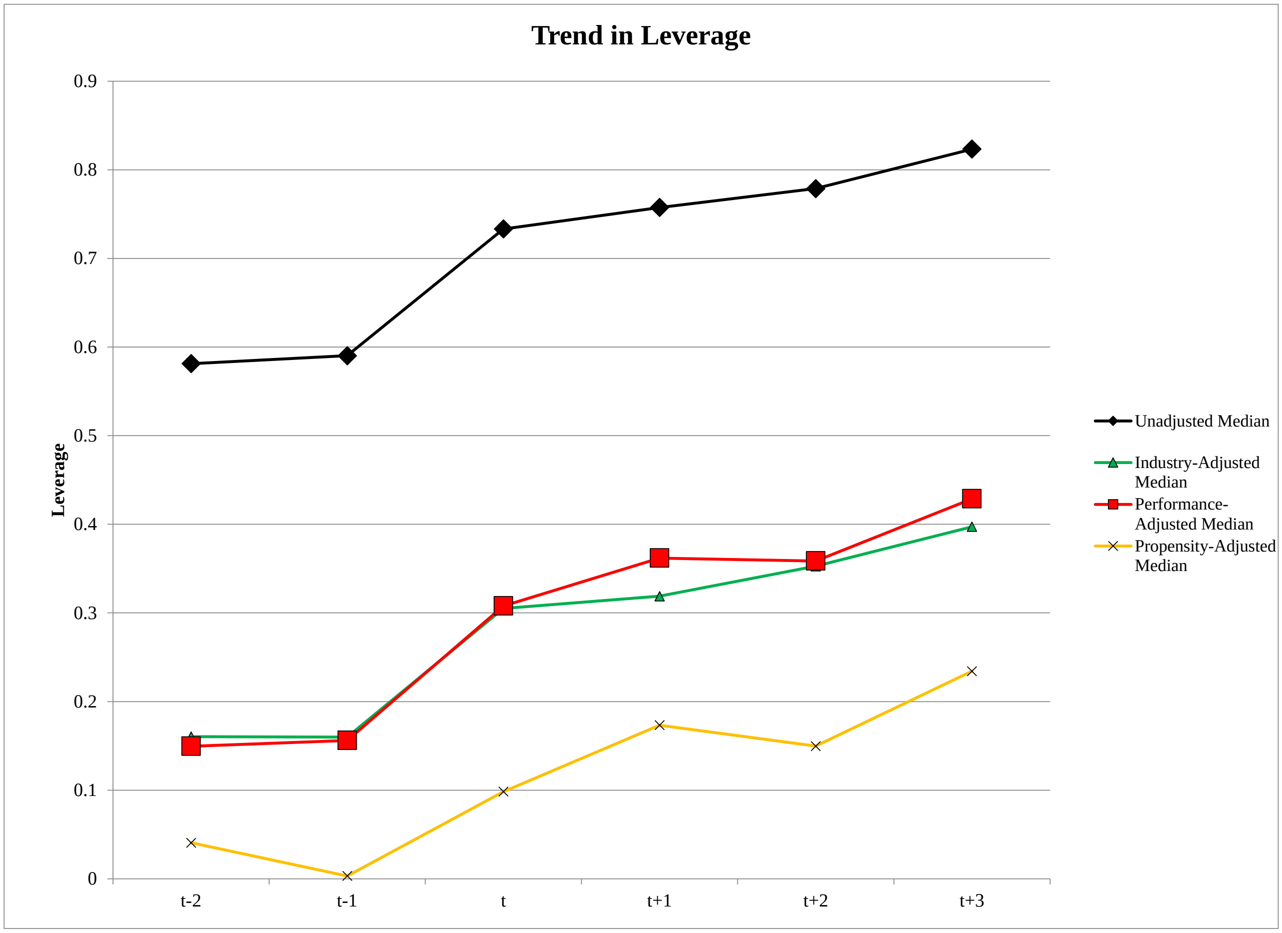

Figure 4. Percentage of buyout firms with positive tax payments. This figure presents the trend in the percentage of private PE buyout firms with positive tax payments. Year *t* represents the buyout year.

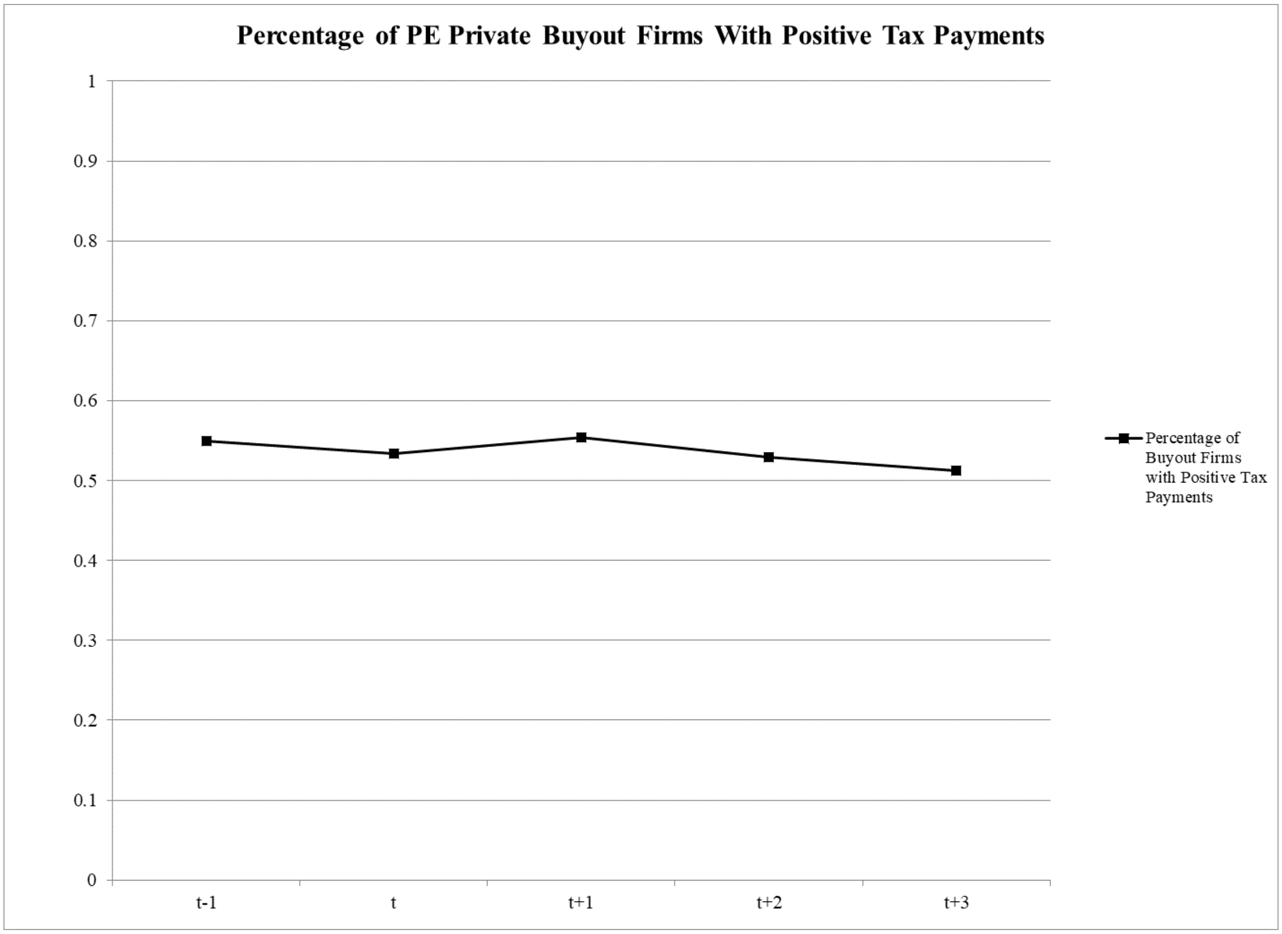

Table I. Sample derivation

This table presents the sample derivation process. Panel A provides the aggregate number of PE transactions, Panel B presents the number of PE transactions by year, and Panel C presents the number of PE transactions by Fama-French 12 industry.

Panel A: Aggregate PE transactions	
Number of PE buyouts of private non-bankrupt C corporations from 1995 to 2009	639
Less: Buyout firms with <$10 million total assets	(87)
Less: Buyout firms merged into other entities	(110)
Number of PE buyout firms to be matched with IRS data	442
Less: PE transactions not matched with IRS data in event window	(93)
Less: PE transactions not having year *t-1* IRS data	(61)
Initial sample for determinants model	288
Less: PE transactions not having year *t+1* and year *t+2* IRS data	(48)
Final sample	**240**

Panel B: PE Transactions by year

	Initial sample	*t-1* to *t+2*	*t-1* to *t+3*	*t-1* to *t+4*
1995	3	3	3	3
1996	7	4	4	3
1997	11	8	7	7
1998	7	5	4	4
1999	7	7	6	6
2000	22	17	14	14
2001	11	7	7	6
2002	6	5	5	5
2003	8	8	8	7
2004	27	20	19	18
2005	39	33	29	27
2006	42	36	32	29
2007	38	38	36	34
2008	46	40	31	13
2009	14	9	4	1
	288	240	209	177

Panel C: PE transactions by Fama-French 12 industry

	Determinants Sample	Final Sample
Business Equipment -- Computers, Software, and Electronic Equipment	30	24
Chemicals and Allied Products	<5	<5
Consumer Durables -- Cars, TV's, Furniture, Household Appliances	11	10
Consumer NonDurables -- Food, Tobacco, Textiles, Apparel, Leather, Toys	23	19
Finance	15	9
Healthcare, Medical Equipment, and Drugs	17	14
Manufacturing -- Machinery, Trucks, Planes, Off Furn, Paper, Com Printing	63	53
Oil, Gas, and Coal Extraction and Products	<5	<5
Other -- Mines, Constr, BldMt, Trans, Hotels, Bus Serv, Entertainment	49	43
Utilities & Telephone and Television Transmission	<5	<5
Wholesale, Retail, and Some Services (Laundries, Repair Shops)	68	60
	288	240

Table II. Descriptive statistics

This table presents descriptive statistics for the buyout determinants sample. Panel A provides the descriptive statistics measured at year *t-1* for all PE transactions. Panel B (C) provides the descriptive statistics measured at year *t-1* for high performance (low performance) PE transactions. Year *t* represents the PE buyout year.

Panel A: All PE transactions						
	N	Mean	SD	Q1	Median	Q3
PreInterestInc	288	7.1	34.7	0.0	2.9	7.3
IntDeduction	288	3.2	6.0	0.2	1.0	3.6
IntBearingLiab	288	54.1	83.3	9.5	25.8	52.0
TotalAssets	288	97.5	154.5	25.0	45.2	104.4
DebtToAssets	288	0.575	0.360	0.291	0.586	0.817
Sales	288	117.4	208.3	29.2	55.3	120.4
SalesGrowth	273	-0.067	0.584	-0.321	-0.023	0.144
PosTaxPdInd	288	0.569	0.496	0.000	1.000	1.000
PreInterestROA	288	0.083	0.132	-0.001	0.076	0.165
PreInterestROS	288	0.029	0.146	-0.001	0.051	0.109
Panel B: PE transactions in Top Quintile Pre-Interest ROA						
	N	Mean	SD	Q1	Median	Q3
PreInterestInc	108	17.8	50.7	3.9	6.7	11.6
IntDeduction	108	2.9	6.3	0.2	0.7	2.5
IntBearingLiab	108	39.7	69.1	6.7	20.6	42.8
TotalAssets	108	81.0	151.0	20.5	38.7	68.6
DebtToAssets	108	0.557	0.382	0.240	0.567	0.835
Sales	108	131.5	226.4	37.3	57.8	131.9
SalesGrowth	98	0.165	0.324	0.012	0.116	0.251
PosTaxPdInd	108	0.852	0.357	1.000	1.000	1.000
PreInterestROA	108	0.218	0.076	0.154	0.202	0.270
PreInterestROS	108	0.127	0.079	0.072	0.107	0.162
Panel C: PE transactions in Bottom Quintile Pre-Interest ROA						
	N	Mean	SD	Q1	Median	Q3
PreInterestInc	77	-8.2	14.3	-11.1	-3.3	-0.8
IntDeduction	77	2.9	4.9	0.1	0.6	2.7
IntBearingLiab	77	62.3	86.1	8.5	26.7	81.2
TotalAssets	77	99.4	135.7	26.0	47.3	116.5
DebtToAssets	77	0.595	0.380	0.268	0.718	0.829
Sales	77	80.2	161.7	14.6	33.6	90.6
SalesGrowth	75	-0.321	0.892	-0.751	-0.470	-0.121
PosTaxPdInd	77	0.000	0.000	0.000	0.000	0.000
PreInterestROA	77	-0.077	0.063	-0.155	-0.051	-0.020
PreInterestROS	77	-0.155	0.129	-0.300	-0.135	-0.023

Table III. Characteristics of PE buyouts of private firms

This table summarizes deal characteristics for our sample of 288 PE buyouts of private firms based on information reported by Capital IQ, Preqin, Factset, and news sources. "Pre-buyout CEO participates" indicates that the pre-buyout CEO retains an equity stake in the deal. N indicates the number of observations for which information is available.

	% of buyouts	N
Management buyout (MBO)	29.30%	283
Pre-buyout CEO participates	42.80%	278
Seller is founder or family	14.40%	278
Secondary buyout	16.30%	283
> 1 buyer (Club deal)	24.40%	283
PE acquirer is in Cohn, Mills, and Towery (2014) sample	33.20%	283
Firm age at buyout (mean)	34 years	276
Firm age at buyout (median)	27 years	276

Table IV. Buyout determinants

This table presents the results from estimating a linear probability model of the likelihood of undergoing a PE buyout. Asterisks *, **, *** denote two-tailed statistical significance at 10%, 5%, and 1%, respectively. Standard errors are clustered by firm. T-statistics are reported in parentheses. See Appendix A for variable definitions.

	Dependent Variable: *BuyoutInd*				
	(1)	(2)	(3)	(4)	(5)
Intercept	-0.0011	-0.0024 **	-0.0025 **	-0.0014	-0.0016
	(-1.14)	(-2.42)	(-2.42)	(-1.35)	(-1.61)
ln(*Assets*)	0.0001 **	0.0002 ***	0.0001 **	0.0001 **	0.0001 **
	(2.4)	(3.03)	(2.29)	(2.13)	(2.06)
PreInterestROA	0.0027 **				
	(2.2)				
PreInterestROAQ1		0.0015 ***	0.0010 ***	0.0009 ***	0.0011 ***
		(5.49)	(3.8)	(3.7)	(4.25)
PreInterestROAQ2		0.0002			
		(1.21)			
PreInterestROAQ4		0.0005 **			
		(2.43)			
PreInterestROAQ5		0.0020 ***	0.0020 ***	0.0020 ***	0.0003
		(7.23)	(7.36)	(7.22)	(0.92)
DebtToAssets			0.0018 ***	-0.0009	0.0010 ***
			(6.78)	(-0.98)	(4.25)
ExtFinDep			-0.0012 ***	-0.0023	-0.0013 ***
			(-3.58)	(-1.34)	(-3.41)
IndustryQ			0.0002	-0.0003	
			(1.22)	(-1.46)	
*DebtToAssets*ExtFinDep*				-0.0100 **	-0.0002
				(-2.46)	(-0.26)
*DebtToAssets*IndustryQ*				0.0015 ***	
				(2.91)	
DebtToAssets PreInterestROAQ5*					0.0049 ***
					(4.26)
*ExtFinDep*IndustryQ*				0.0003	
				(0.3)	
*ExtFinDep*PreInterestROAQ5*					-0.0016
					(-1.22)
*DebtToAssets*ExtFinDep* IndustryQ*				0.0066 ***	
				(2.87)	
*DebtToAssets*ExtFinDep* PreInterestROAQ5*					0.0111 **
					(2.48)
Number of buyout observations	288	288	288	288	288
Number of non-buyout observations	199,358	199,358	199,358	199,358	199,358
Adjusted R-squared	0.01%	0.04%	0.08%	0.09%	0.12%

Table V. Changes in pre-interest ROS

This table tests for changes in pre-interest ROS using t-tests and Wilcoxon rank tests. Year *t* represents the PE buyout year. Asterisks *, **, *** denote two-tailed statistical significance at 10%, 5%, and 1%, respectively. See Appendix A for variable definitions.

Panel A: Change in Pre-Interest ROS for all PE transactions

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.066	***	0.061	***	0.062	***	0.066	***	0.048	***
	Median	0.020	***	0.012	***	0.007	**	0.007	**	0.000	
	N	240		240		207		175		240	
Industry-Adjusted	Mean	0.071	***	0.068	***	0.073	***	0.078	***	0.058	***
	Median	0.026	***	0.019	***	0.020	***	0.020	***	0.013	***
	N	240		240		207		175		240	
Propensity-Adjusted	Mean	0.124	***	0.095	***	0.124	***	0.138	***	0.121	***
	Median	0.042	***	0.027	***	0.040	***	0.057	***	0.041	***
	N	240		240		207		157		240	
Performance-Adjusted	Mean	0.014		0.059	***	0.022		0.095	***	0.078	***
	Median	0.018	**	0.035	***	0.030	**	0.018	**	0.018	**
	N	213		213		182		131		213	

Panel B: Change in Pre-Interest ROS for PE transactions in Top Pre-Interest ROA Quintile

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.044	**	0.031	*	0.023		0.006		0.001	
	Median	0.007		0.004		-0.007		-0.017		-0.012	
	N	104		104		92		80		104	
Industry-Adjusted	Mean	0.048	**	0.038	**	0.036	*	0.019		0.013	
	Median	0.015		0.013		0.010		-0.012		-0.008	
	N	104		104		92		80		104	
Propensity-Adjusted	Mean	0.055	**	0.042	*	0.052	**	0.000		0.019	
	Median	0.029		0.007		0.020		0.014		0.008	
	N	104		104		92		74		104	
Performance-Adjusted	Mean	0.031		0.052	*	0.075	*	0.061		0.055	
	Median	0.029	**	0.041	**	0.042	***	-0.005		0.009	
	N	86		86		76		56		86	

Panel C: Change in Pre-Interest ROS for PE transactions in Bottom Pre-Interest ROA Quintile

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.138	***	0.170	***	0.173	***	0.210	***	0.180	***
	Median	0.042	***	0.066	***	0.109	***	0.120	***	0.104	***
	N	48		48		41		32		48	
Industry-Adjusted	Mean	0.147	***	0.179	***	0.185	***	0.222	***	0.192	***
	Median	0.060	***	0.086	***	0.100	***	0.123	***	0.121	***
	N	48		48		41		32		48	
Propensity-Adjusted	Mean	0.195	***	0.213	***	0.217	***	0.239	**	0.249	***
	Median	0.117	***	0.116	***	0.120	***	0.198	***	0.191	***
	N	48		48		41		29		48	
Performance-Adjusted	Mean	0.014		0.129	**	0.022		0.035		0.092	
	Median	-0.001		0.054	***	0.041	*	0.020		0.050	
	N	42		42		35		24		42	

Table VI. Changes in sales growth

This table tests for changes in Sales Growth using t-tests and Wilcoxon rank tests. Year *t* represents the PE buyout year. Asterisks *, **, *** denote two-tailed statistical significance at 10%, 5%, and 1%, respectively. See Appendix A for variable definitions.

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Panel A: Change in Sales Growth for all PE transactions											
Unadjusted	Mean	1.328	***	1.613	***	1.776	***	1.804	***	1.657	***
	Median	0.447	***	0.536	***	0.631	***	0.833	***	0.663	***
	N	240		240		209		177		240	
Industry-Adjusted	Mean	1.390	***	1.673	***	1.859	***	1.851	***	1.698	***
	Median	0.525	***	0.617	***	0.739	***	0.894	***	0.688	***
	N	240		240		209		177		240	
Propensity-Adjusted	Mean	1.556	***	1.955	***	2.120	***	2.032	***	2.217	***
	Median	0.598	***	0.843	***	0.890	***	0.991	***	0.749	***
	N	240		240		209		161		240	
Performance-Adjusted	Mean	1.149	***	1.545	***	1.630	***	1.698	***	1.502	***
	Median	0.371	***	0.532	***	0.637	***	0.715	***	0.552	***
	N	213		213		183		136		213	
Panel B: Change in Sales Growth for PE transactions in Top Pre-Interest ROA Quintile											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	1.105	***	1.346	***	1.547	***	1.621	***	1.457	***
	Median	0.467	***	0.563	***	0.754	***	0.818	***	0.668	***
	N	104		104		93		82		104	
Industry-Adjusted	Mean	1.165	***	1.412	***	1.636	***	1.674	***	1.515	***
	Median	0.532	***	0.674	***	0.806	***	0.887	***	0.755	***
	N	104		104		93		82		104	
Propensity-Adjusted	Mean	1.324	***	1.705	***	1.839	***	2.083	***	1.923	***
	Median	0.588	***	0.907	***	1.051	***	1.064	***	0.841	***
	N	104		104		93		77		104	
Performance-Adjusted	Mean	1.009	***	1.331	***	1.512	***	1.708	***	1.315	***
	Median	0.480	***	0.657	***	0.757	***	0.841	***	0.648	***
	N	86		86		76		59		86	
Panel C: Change in Sales Growth for PE transactions in Bottom Pre-Interest ROA Quintile											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	2.154	***	2.501	***	2.686	***	2.547	***	2.677	***
	Median	0.420	***	0.400	***	0.875	***	1.194	***	1.015	***
	N	48		48		42		32		48	
Industry-Adjusted	Mean	2.238	***	2.565	***	2.758	***	2.578	***	2.708	***
	Median	0.459	***	0.567	***	0.955	***	1.172	***	0.952	***
	N	48		48		42		32		48	
Propensity-Adjusted	Mean	2.754	***	3.175	***	3.414	***	2.799	***	3.925	***
	Median	0.858	***	1.146	***	1.509	***	1.527	***	1.556	***
	N	48		48		42		29		48	
Performance-Adjusted	Mean	1.931	***	2.474	***	2.673	***	2.046	***	2.759	***
	Median	0.251	**	0.532	***	0.888	***	1.142	***	1.104	***
	N	42		42		36		24		42	

Table VI. Changes in sales growth (continued)

This table tests for changes in sales growth using t-tests and Wilcoxon rank tests. Year *t* represents the PE buyout year. Asterisks *, **, *** denote two-tailed statistical significance at 10%, 5%, and 1%, respectively. See Appendix A for variable definitions.

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Panel D: Change in Sales Growth for PE transactions with Add-on Acquisitions											
Unadjusted	Mean	1.281	***	1.699	***	2.032	***	2.129	***	1.801	***
	Median	0.500	***	0.834	***	0.962	***	0.997	***	0.918	***
	N	106		106		99		86		106	
Industry-Adjusted	Mean	1.338	***	1.761	***	2.121	***	2.188	***	1.837	***
	Median	0.584	***	0.939	***	1.059	***	1.084	***	0.942	***
	N	106		106		99		86		106	
Propensity-Adjusted	Mean	1.620	***	2.189	***	2.510	***	2.387	***	2.627	***
	Median	0.757	***	1.141	***	1.342	***	1.228	***	1.094	***
	N	106		106		99		78		106	
Performance-Adjusted	Mean	1.053	***	1.641	***	1.958	***	1.910	***	1.737	***
	Median	0.478	***	0.775	***	0.979	***	0.969	***	0.945	***
	N	90		90		83		62		90	
Panel E: Change in Sales Growth for PE transactions without Add-on Acquisitions											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	1.366	***	1.545	***	1.546	***	1.497	***	1.543	***
	Median	0.373	***	0.423	***	0.379	***	0.527	***	0.281	***
	N	134		134		110		91		134	
Industry-Adjusted	Mean	1.432	***	1.604	***	1.623	***	1.533	***	1.587	***
	Median	0.419	***	0.434	***	0.467	***	0.528	***	0.316	***
	N	134		134		110		91		134	
Propensity-Adjusted	Mean	1.505	***	1.770	***	1.768	***	1.699	***	1.893	***
	Median	0.433	***	0.519	***	0.623	***	0.729	***	0.441	***
	N	134		134		110		83		134	
Performance-Adjusted	Mean	1.219	***	1.474	***	1.357	***	1.521	***	1.330	***
	Median	0.290	***	0.374	***	0.336	***	0.547	***	0.320	***
	N	123		123		100		74		123	

Table VII. Changes in leverage

This table tests for changes in leverage using t-tests and Wilcoxon rank tests. Year *t* represents the PE buyout year. Asterisks *, **, *** denote two-tailed statistical significance at 10%, 5%, and 1%, respectively. See Appendix A for variable definitions.

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Panel A: Change in Leverage for all PE transactions											
Unadjusted	Mean	0.151	***	0.176	***	0.242	***	0.279	***	0.253	***
	Median	0.112	***	0.121	***	0.138	***	0.156	***	0.161	***
	N	240		240		209		177		240	
Industry-Adjusted	Mean	0.178	***	0.198	***	0.253	***	0.279	***	0.266	***
	Median	0.138	***	0.157	***	0.166	***	0.175	***	0.195	***
	N	240		240		209		177		240	
Propensity-Adjusted	Mean	0.178	***	0.195	***	0.264	***	0.270	***	0.261	***
	Median	0.145	***	0.154	***	0.170	***	0.178	***	0.227	***
	N	240		240		209		161		240	
Performance-Adjusted	Mean	0.151	***	0.155	***	0.269	***	0.194	***	0.188	***
	Median	0.111	***	0.126	***	0.192	***	0.164	***	0.168	***
	N	213		213		183		136		213	
Panel B: Change in Leverage for PE transactions in Top Pre-Interest ROA Quintile											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.149	***	0.163	***	0.222	***	0.274	***	0.260	***
	Median	0.109	***	0.106	***	0.090	***	0.135	***	0.146	***
	N	104		104		93		82		104	
Industry-Adjusted	Mean	0.169	***	0.174	***	0.219	***	0.258	***	0.259	***
	Median	0.135	***	0.112	***	0.118	***	0.179	***	0.177	***
	N	104		104		93		82		104	
Propensity-Adjusted	Mean	0.151	***	0.173	***	0.257	***	0.298	***	0.265	***
	Median	0.132	***	0.140	***	0.166	***	0.178	***	0.223	***
	N	104		104		93		77		104	
Performance-Adjusted	Mean	0.143	***	0.133	**	0.233	***	0.156	**	0.134	*
	Median	0.074	***	0.077	**	0.136	***	0.122	**	0.091	
	N	86		86		76		59		86	
Panel C: Change in Leverage for PE transactions in Bottom Pre-Interest ROA Quintile											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.121	*	0.173	**	0.249	***	0.249	*	0.199	**
	Median	0.056	*	0.060	**	0.101	***	0.142		0.114	*
	N	48		48		42		32		48	
Industry-Adjusted	Mean	0.153	**	0.202	***	0.276	***	0.268	*	0.224	**
	Median	0.093	**	0.087	***	0.157	***	0.164	**	0.164	**
	N	48		48		42		32		48	
Propensity-Adjusted	Mean	0.163	*	0.176	**	0.224	**	0.185		0.146	
	Median	0.123	*	0.097	*	0.033	*	0.102		0.126	
	N	48		48		42		29		48	
Performance-Adjusted	Mean	0.115		0.153	*	0.278	**	0.142		0.202	**
	Median	0.108		0.061	*	0.191	***	0.135		0.162	**
	N	42		42		36		24		42	

Table VIII. Analysis of contributions by equityholders

This table presents the trend in cash contributions made by equityholders from year *t-1* to year *t+3* for buyout firms, where year *t* is the year of the buyout.

	N	Mean	SD	Q1	Median	Q3
$Contributions_{t-1}$	84	-2.29	16.25	0.00	0.00	0.21
$Contributions_{t}$	120	22.46	42.48	0.00	2.31	33.63
$Contributions_{t+1}$	156	3.05	15.64	0.00	0.01	2.09
$Contributions_{t+2}$	158	4.20	22.70	0.00	0.01	1.09
$Contributions_{t+3}$	116	7.50	29.72	0.00	0.04	1.45

Appendix A: Variable Definitions

BuyoutInd	=	One if a firm is acquired by a private equity buyer during the year, and zero otherwise
Contributions	=	One-year change in paid-in-capital reported on Form 1120 Schedule L Line 23
DebtToAssets	=	*IntBearingLiab* divided by *TotalAssets*
ExtFinDep	=	Industry [capital expenditures less net cash flow from operating activities plus change in net working capital, divided by capital expenditures, where net working capital is defined as inventory plus accounts receivable less accounts payable] (adopted from Rajan and Zingales (1998))
IndustryQ	=	Median industry Tobins Q [market value of assets divided by the book value of assets (Compustat AT), where the market value of assets equals the book value of debt (Compustat LT) plus the market value of equity (Compustat PRCC_F * Compustat CSHO)]
IntBearingLiab	=	Short-term and long-term mortgages, notes, and bonds payable reported on Form 1120 Schedule L Lines 17 and 20
IntDeduction	=	Interest deduction reported on Form 1120 Page 1 Line 18
PosTaxPdInd	=	One if total tax reported on Form 1120 Page 1 Line 31 is positive, and zero otherwise
PreInterestInc	=	*TaxableInc* plus *IntDeduction*
PreInterestROA	=	*PreInterestInc* divided by lagged *TotalAssets*
PreInterestROS	=	*PreInterestInc* divided by *Sales*
Sales	=	Gross receipts or sales reported on Form 1120 Page 1 Line 1
SalesGrowth	=	One-year percentage change in *Sales*
TaxableInc	=	Taxable income before net operating loss deductions and special deductions reported on Form 1120 Page 1 Line 28
TotalAssets	=	Total assets reported on Form 1120 Schedule L Line 15

Appendix B: Supplemental Tables

Appendix B.1 Characteristics of PE Buyouts of Private Firms Organized as Flow-through Entities

This appendix summarizes deal characteristics and outcomes for 543 buyouts of private firms not included in this study~~paper~~'s sample because they are organized as flow-through entities rather than C Corporations pre-buyout. Statistics are based on information reported by Capital IQ, Preqin, and news sources. "CEO participates" indicates that the pre-buyout CEO retains an equity stake in the deal.

Management buyout (MBO)	16.8%	
Pre-buyout CEO participates	33.9%	
Seller is founder or family	19.3%	
Secondary buyout	4.8%	
> 1 buyer (Club deal)	22.1%	
PE acquirer is in Cohn, Mills, and Towery (2014) sample	23.4%	
Firm age at buyout (mean)	38 years	(N= 520)
Firm age at buyout (median)	22 years	(N= 520)

Appendix B.2 Changes in Pre-Interest ROS, Sales Growth, and Leverage & Trends in Tax Status and Equity Contributions for PE buyout firms with Assets ≥ $20 million

This appendix presents the changes in pre-Interest ROS, sales growth, and leverage and the trends in tax status and equity contributions for PE buyout firms with Assets ≥ $20 million. Year *t* represents the PE buyout year. Asterisks *, **, *** denote two-tailed statistical significance at 10%, 5%, and 1%, respectively.

Change in Pre-Interest ROS for PE transactions with Assets > $20 million

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.070	***	0.066	***	0.071	***	0.080	***	0.058	***
	Median	0.023	***	0.012	***	0.011	***	0.008	***	0.003	*
	N	192		192		167		141		192	
Industry-Adjusted	Mean	0.075	***	0.072	***	0.082	***	0.093	***	0.069	***
	Median	0.029	***	0.018	***	0.022	***	0.028	***	0.018	***
	N	192		192		167		141		192	
Propensity-Adjusted	Mean	0.137	***	0.100	***	0.141	***	0.158	***	0.137	***
	Median	0.043	***	0.032	***	0.043	***	0.078	***	0.054	***
	N	192		192		167		125		192	
Performance-Adjusted	Mean	0.011		0.050	**	0.013		0.101	***	0.080	***
	Median	0.018	*	0.032	**	0.030	**	0.039	***	0.038	**
	N	176		176		151		107		176	

Change in Sales Growth for PE transactions with Assets > $20 million

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	1.353	***	1.598	***	1.787	***	1.778	***	1.544	***
	Median	0.474	***	0.498	***	0.631	***	0.880	***	0.663	***
	N	192		192		168		142		192	
Industry-Adjusted	Mean	1.410	***	1.654	***	1.870	***	1.826	***	1.585	***
	Median	0.517	***	0.559	***	0.747	***	0.908	***	0.688	***
	N	192		192		168		142		192	
Propensity-Adjusted	Mean	1.567	***	1.896	***	2.106	***	1.915	***	2.057	***
	Median	0.598	***	0.740	***	0.890	***	0.972	***	0.757	***
	N	192		192		168		128		192	
Performance-Adjusted	Mean	1.180	***	1.504	***	1.603	***	1.579	***	1.404	***
	Median	0.372	***	0.512	***	0.661	***	0.684	***	0.528	***
	N	176		176		152		112		176	

Change in Leverage for PE transactions with Assets > $20 million

		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.147	***	0.190	***	0.265	***	0.307	***	0.281	***
	Median	0.108	***	0.125	***	0.147	***	0.167	***	0.162	***
	N	192		192		168		142		192	
Industry-Adjusted	Mean	0.175	***	0.213	***	0.280	***	0.308	***	0.292	***
	Median	0.132	***	0.157	***	0.190	***	0.186	***	0.213	***
	N	192		192		168		142		192	
Propensity-Adjusted	Mean	0.165	***	0.211	***	0.299	***	0.281	***	0.287	***
	Median	0.129	***	0.165	***	0.191	***	0.168	***	0.250	***
	N	192		192		168		128		192	
Performance-Adjusted	Mean	0.160	***	0.184	***	0.312	***	0.256	***	0.231	***
	Median	0.122	***	0.131	***	0.200	***	0.182	***	0.183	***
	N	176		176		152		112		176	

Trend in tax status for PE transactions with Assets > $20 million

	N	Mean	SD	Q1	Median	Q3
$PosTaxPdInd_{t-1}$	192	0.55	0.50	0.00	1.00	1.00
$PosTaxPdInd_{t}$	192	0.52	0.50	0.00	1.00	1.00
$PosTaxPdInd_{t+1}$	192	0.55	0.50	0.00	1.00	1.00
$PosTaxPdInd_{t+2}$	192	0.50	0.50	0.00	0.50	1.00
$PosTaxPdInd_{t+3}$	168	0.48	0.50	0.00	0.00	1.00

Trend in equity contributions for PE transactions with Assets > $20 million

	N	Mean	SD	Q1	Median	Q3
$Contributions_{t-1}$	75	-2.58	17.19	0.00	0.00	0.26
$Contributions_{t}$	99	24.07	45.00	0.00	5.34	34.09
$Contributions_{t+1}$	126	3.29	17.19	0.00	0.01	2.44
$Contributions_{t+2}$	126	3.94	23.51	0.00	0.00	1.09
$Contributions_{t+3}$	94	8.34	30.60	0.00	0.08	1.55

Appendix B.3 Post-buyout Outcomes

The appendix summarizes the outcomes of buyout transactions based on information reported by Capital IQ, Preqin, and news sources.

			Time until exit (years)	
Outcomes:	# of Buyouts	% of Buyouts	Mean	Median
Sale to strategic buyer	80	33.30%	5.1	5.1
Sale to financial buyer (PE)	89	37.10%	5.5	5
Sale to another PE portfolio company	14	5.80%	6.1	5.1
Bankruptcy or liquidation	22	9.20%	5.9	4.8
IPO	9	3.80%	4.3	3
PE exit type not determined; firm still operates	7	2.90%	4.4	5
Still owned by PE or undetermined	19	7.90%	N/A	N/A
All buyout outcomes	240		5.5	5.1

Appendix B.4 Changes in Pre-Interest ROS, Sales Growth, and Leverage & Trends in Tax Status and Equity Contributions for PE buyout firms with Assets ≥ $92.7 million

This appendix presents the changes in pre-Interest ROS, sales growth, and leverage and the trends in tax status and equity contributions for PE buyout firms with Assets ≥ $92.7 million. Year t represents the PE buyout year. Asterisks *, **, *** denote two-tailed statistical significance at 10%, 5%, and 1%, respectively.

Change in Pre-Interest ROS for PE transactions with Assets > $92.7 million											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.093	***	0.076	**	0.078	***	0.072	**	0.068	**
	Median	0.028	***	0.009	*	0.016	**	-0.006		0.006	
	N	65		65		55		48		65	
Industry-Adjusted	Mean	0.097	***	0.081	***	0.087	***	0.084	**	0.078	**
	Median	0.035	***	0.017	**	0.024	***	0.007		0.013	*
	N	65		65		55		48		65	
Propensity-Adjusted	Mean	0.201	***	0.092	**	0.158	***	0.173	***	0.174	***
	Median	0.034	***	0.033	*	0.046	***	0.056	**	0.057	***
	N	65		65		55		42		65	
Performance-Adjusted	Mean	0.057		0.085	*	0.032		0.134	**	0.098	*
	Median	0.018		0.036		0.019		0.001		0.018	
	N	63		63		53		38		63	

Change in Sales Growth for PE transactions with Assets > $92.7 million											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	1.505	***	1.863	***	2.215	***	2.249	***	2.169	***
	Median	0.475	***	0.534	***	0.782	***	0.893	***	0.755	***
	N	65		65		56		48		65	
Industry-Adjusted	Mean	1.563	***	1.913	***	2.292	***	2.299	***	2.214	***
	Median	0.579	***	0.563	***	0.908	***	0.931	***	0.770	***
	N	65		65		56		48		65	
Propensity-Adjusted	Mean	1.607	***	2.039	***	2.443	***	2.292	***	2.575	***
	Median	0.581	***	0.843	***	1.073	***	0.970	***	0.810	***
	N	65		65		56		44		65	
Performance-Adjusted	Mean	1.283	***	1.779	***	2.099	***	1.873	***	2.004	***
	Median	0.419	***	0.532	***	0.899	***	0.707	***	0.690	***
	N	63		63		54		41		63	

Change in Leverage for PE transactions with Assets > $92.7 million											
		t-1 to t+1		t-1 to t+2		t-1 to t+3		t-1 to t+4		t-1 to exit	
Unadjusted	Mean	0.187	***	0.254	***	0.280	***	0.324	***	0.333	***
	Median	0.170	***	0.199	***	0.162	***	0.180	***	0.227	***
	N	65		65		56		48		65	
Industry-Adjusted	Mean	0.218	***	0.285	***	0.314	***	0.345	***	0.347	***
	Median	0.227	***	0.276	***	0.238	***	0.239	***	0.290	***
	N	65		65		56		48		65	
Propensity-Adjusted	Mean	0.215	***	0.229	***	0.240	***	0.260	***	0.295	***
	Median	0.145	***	0.126	***	0.126	***	0.128	**	0.272	***
	N	65		65		56		44		65	
Performance-Adjusted	Mean	0.187	***	0.209	***	0.312	***	0.285	***	0.260	***
	Median	0.130	***	0.137	***	0.254	***	0.239	***	0.189	***
	N	63		63		54		41		63	

Trend in tax status for PE transactions with Assets > $92.7 million						
	N	Mean	SD	Q1	Median	Q3
PosTaxPdInd $_{t-1}$	65	0.55	0.50	0.00	1.00	1.00
PosTaxPdInd $_{t}$	65	0.57	0.50	0.00	1.00	1.00
PosTaxPdInd $_{t+1}$	65	0.65	0.48	0.00	1.00	1.00
PosTaxPdInd $_{t+2}$	65	0.48	0.50	0.00	0.00	1.00
PosTaxPdInd $_{t+3}$	56	0.54	0.50	0.00	1.00	1.00

Trend in equity contributions for PE transactions with Assets > $92.7 million						
	N	Mean	SD	Q1	Median	Q3
Contributions $_{t-1}$	28	-1.77	18.83	0.00	0.00	4.27
Contributions $_{t}$	33	42.01	58.61	0.00	21.25	75.32
Contributions $_{t+1}$	44	4.34	20.56	0.00	0.26	4.62
Contributions $_{t+2}$	44	2.58	30.33	-0.01	0.09	2.22
Contributions $_{t+3}$	33	16.71	44.87	0.00	0.30	4.18

The performance of private equity portfolio companies during the COVID-19 pandemic *

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Abstract

We study the performance of PE-backed companies during the COVID-19 pandemic. Our findings suggest that PE-backed firms' sales, employment, and earnings were more resilient compared to closely matched industry peers during the pandemic. These effects are found to be insignificant among firms which were the most vulnerable at the onset of the pandemic, and firms in the most exposed industries. These more vulnerable firms appear to have been less active in obtaining additional financing during the pandemic, and consequently, suffered a significantly higher incidence of distress. However, non-PE-backed firms in distress had a higher incidence of liquidation, while PE-owned firms more often negotiated formally with creditors to continue trading. Our analysis shines light on how PE investors may respond to a large, exogenous shock such as the COVID-19 pandemic.

Keywords: Private equity buyouts; firm performance; COVID-19 pandemic.
JEL Classification: G01, G23, G32, G34

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1 Introduction

Private equity (PE)-backed firms have previously been shown to be more resilient in the face of economic uncertainty and downturns, and specifically through the 2008 financial crisis (Wilson et al. (2012), Bernstein et al. (2019)). In this study, we examine the performance of PE-backed firms during the recent COVID-19 pandemic. COVID-19 represented a significant exogenous shock to the global economy and economic activity and perhaps the largest ever collapse in UK economic activity. The UK saw a GDP decline of 25.1% in April 2020 followed by a deep recession (-9.9% GDP, 2020). Although economic activity revived over the spring and summer of 2020 as the economy reopened, further lockdowns during the autumn and winter of 2020/21 saw economic activity fall again, and the recovery has been slow.

The pandemic period was characterised by a significant and unprecedented level of policy intervention in response to the impact on business and society. The UK government initiated a range of interventions in response to the pandemic. Initially the focus was on introducing measures to protect livelihoods and jobs, whereby the Coronavirus Job Retention Scheme (CJRS) offered grants to cover a proportion of the salaries of furloughed staff. A range of business loan schemes, with the government acting as guarantor, were launched providing some £80bn in guaranteed loans to businesses of all sizes. The Coronavirus Business Interruption Loan Scheme (CBILS), Bounce Back Loan Scheme (BBLS) and the Covid-19 Corporate Financing Facility (CCFF), amongst others, were aimed at providing finance to help prevent otherwise viable businesses from failing. Moreover, the government introduced some permanent and temporary changes to insolvency legislation (i.e. the Corporate Insolvency and Governance Act 2020).[1]

[1]The latter introduced measures to give companies the 'breathing space' to maximise their chance of survival; measures to temporarily suspend parts of insolvency law to allow companies to continue trading through the pandemic without the threat of liability for wrongful trading; and measures to protect compa-

There are both similarities and significant differences in the effects of the global financial crisis (an endogenous shock) and the pandemic (an exogenous shock) on economic activity and businesses. Both crises share uncertainty as a major factor, but the financial crisis resulted from imbalances between the financial sector and real economy and saw a significant contraction in money and credit. There were no such imbalances before the COVID-19 downturn. Moreover, the abundance of government support and liquidity made available to firms, coupled with increased creditor forbearance, distinguishes the pandemic from the financial crisis. Thus, the nature of the crisis was different, but the timing and severity was also unique. The rapid spread of new virus variants continually changed the business landscape due to lockdowns and restrictions. As a result, given the systematic differences in nature and timing compared to previous downturns, the COVID-19 pandemic provides a unique setting for studying firm ownership and performance.

The pandemic had significant implications for PE investors and how they conduct their business. Travel restrictions disrupted the due diligence process, preventing on-site visits to witness first hand business operations. Similarly, conference call meetings can hamper an investor's ability to gauge team dynamics and culture compared to in-person meetings. However, from a PE fund perspective, survey evidence suggests that investors had a greater intensity of interaction with their portfolio companies during the pandemic and that investment horizons were extended (Gompers et al. (2022)). In wider financial markets, credit markets cooled as lender caution increased amidst the market uncertainty, meaning PE investors had greater difficulty in accessing the substantial amounts of debt necessary to execute larger leveraged buyouts. Consequently, and unsurprisingly, the number of PE buyouts completed globally in 2020 fell by 28% from 2019 (see Figure 1).

nies from creditor action. Of relevance were changes to the process for establishing 'Company Voluntary Arrangements' and the 'cram out' procedure (discussed later). Moreover, there were temporary easements on company filing requirements and annual general meetings.

The pandemic, the resulting lockdowns, and the consequent economic contraction had considerable consequences for companies, their operating behaviour and their financial performance (Alekseev et al. (2023)). Firm-level turnover, earnings and cash flow were hampered by fluctuations in consumer behaviour and demand, and uncertainty concerning the lifting of lockdowns and restrictions affected the ability of firms to accurately forecast their financial performance and make new investment decisions. Unsurprisingly, recent evidence has shed light on firms' operating performance and corporate hiring declining during the pandemic (Campello et al. (2020), Fahlenbrach et al. (2021), Papanikolaou and Schmidt (2022)). However, when the pandemic hit, some firms held large cash buffers, had little leverage, and had limited exposure to rollover risk. Given their ability to finance an unanticipated cash flow shortfall, these firms had greater financial flexibility (Fahlenbrach et al. (2021)). On the other hand, firms with a higher degree of leverage and less cash holdings enjoy less flexibility, and unanticipated shocks to their cash flow can have more severe repercussions for their financial health. Indeed, Fahlenbrach et al. (2021) show that firms which had lower financial flexibility at the onset of the pandemic (i.e. firms with less cash and higher leverage) experienced weaker operating performance and lower stock returns compared to firms which had stronger balance sheets. Similarly, some industries were more exposed to the consequences of the pandemic than others. There was a supply shock due to government-mandated lockdowns which caused production to fall for industries dependent on employees being on-site and close to one other. At the same time there was a demand shock as demand fell for firms whose customers have to interact with the firm and its employees in-person (Papanikolaou and Schmidt (2022), Fahlenbrach et al. (2021)). Consequently, certain industries were far more exposed to the COVID-19 pandemic relative to others.

In a recent study, Bernstein et al. (2019) posit that since PE investors are wellnetworked with strong ties with banks and providers of credit, and have financial resources (e.g. dry powder) with which they can provide additional funding to companies, PE investors are likely to redeploy human capital from searching for new deals to assisting their current portfolio companies during times of need. This supports the hypothesis that PE-backed firms will be more resilient to economic downturns compared to other firms. Survey evidence from the COVID-19 pandemic in Gompers et al. (2022) supports this hypothesis. PE investors spent more time interacting with current portfolio companies, and investors whose portfolio was severely affected by the pandemic were less active in pursuing new investments. More generally, they find that PE investors were active in helping portfolio companies access liquidity and refinancing debt during the pandemic.

Empirical firm-level evidence from the most global financial crisis of 2008-09 is consistent with this. Wilson et al. (2012) study UK companies and find significant, positive differentials in firm productivity and profitability for PE-owned companies relative to matched peers. Bernstein et al. (2019) provide evidence that sponsored firms decreased their investment by less than matched industry peers, and that they experienced greater equity and debt inflows, higher asset growth, and increased market share during the crisis. The authors note that the positive effect of PE on firm investment during the crisis is particularly strong among companies which were more financially constrained during the crisis, and that these firms particularly benefited from debt issuances to alleviate financing constraints. That is, PE investors appear to have been able to help those who needed it the most.

There are two competing hypotheses concerning PE firms and their role in mitigating constraints during the pandemic period. First, PE firms may have focused their efforts on weaker firms and on those in more exposed industries, and the outperformance of PE-backed firms may be strongest amongst these companies. Bernstein et al. (2019) show that the positive impact of PE ownership on firm investment during the global financial crisis is stronger in companies which were ex-ante more likely to be constrained during the crisis. A second perspective may reflect the exogenous nature of the shock. The global financial crisis was a credit crisis, with a widespread reduction in lending being the primary direct impact on businesses. In this case, PE investors were able to directly alleviate the primary effect of the crisis and help firms access credit, whereas other firms struggled to do so. In turn, constrained PE-backed firms could then outperform their peers (Bernstein et al. (2019)). The exogenous shock of the pandemic, coupled with widespread policy intervention, was very different, however, and there may have been cases where PE firms "cut their losses" on their weakest portfolio firms, or those most exposed to the pandemic, and instead focused their attention on firms which they deemed to be more manageable and "treatable".

In this paper we study the performance and resilience of PE-backed firms during the COVID-19 pandemic. Using a sample of over 800 UK companies which were under PE ownership at the onset of the COVID-19 pandemic, we study the ensuing performance of these companies during the pandemic relative to matched non-PE-backed firms. We match PE-owned firms to controls across a number of observable characteristics, such as industry, size, profitability, and leverage (Boucly et al. (2011), Bernstein et al. (2019), Cohn et al. (2021)). We compare company performance during the pandemic across several measures including sales, assets, employment, and earnings. We find significant evidence of outperformance of PE-backed firms during the COVID-19 pandemic. For example, sponsored firms increased their sales by around 5% relative to control firms, and their earnings by approximately 6%. We find that their employment and assets were likewise more resilient during the pandemic. Importantly, when we examine the timing of the effect, PE-backed and matched control firms are similar during the pre-pandemic period.

However, at the onset of the pandemic in 2020, we see considerable divergence in the two groups of firms across performance and growth variables such as sales, employment, and earnings. These results are robust to a battery of robustness checks including controlling for a range of pre-pandemic observable firm controls, and making several adjustments to our matching technique. In particular, we tighten and loosen the matching bandwidths used, and use alternative matching parameters. In other checks, we control for attrition bias, control for UK government support during the pandemic in the way of various COVID support loans which were issued to firms, and finally, tighten our selection of firms which are PE-backed at the onset of the pandemic. We continue to find that, on average, PE-backed firms outperform during the pandemic period.

Of course, the impact of the pandemic was not homogeneous across all types of firms and industries, with empirical evidence supporting this (Campello et al. (2020), Fahlenbrach et al. (2021), Barry et al. (2022)). When we study the cross-section, we uncover interesting results. The outperformance of PE-backed firms is not being driven by the most vulnerable and exposed firms, but by firms which were better-performing prior to the pandemic, and had greater financial flexibility, and operated in industries which were less exposed to the pandemic. Indeed, the most vulnerable and exposed firms do not outperform. This finding is robust across many definitions of firm vulnerability and industry exposure. The results suggest that PE investors were less able to alleviate constraints facing the worst-affected firms during the pandemic.

We then explore the potential mechanisms through which PE-backed firms may be able to outperform during the pandemic period. PE investors typically help their portfolio through three dimensions: Operational engineering, governance engineering, and financial engineering (Gompers et al. (2016), Gryglewicz and Mayer (2023)). We test various channels and find strong evidence of a financial engineering channel at play, and some weaker evidence of operational changes helping firms outperform. Our results suggest that PE-backed firms had better access to both equity and debt financing, particularly the latter. This is consistent with survey evidence in Gompers et al. (2022), where PE investors stress the importance of accessing debt financing more so than additional equity injections as a means of helping companies during the pandemic. When we study the cross-section of firms, we find that the positive impact of PE ownership on equity and debt issuance during the pandemic is concentrated in firms which were in less vulnerable positions at the onset of the pandemic. Where the most vulnerable firms are concerned, they do not appear to have superior access to external financing during the pandemic. This suggests that PE investors may have "cut their losses" and been less active with their most exposed portfolio firms.

Finally, we study incidence of financial distress throughout the pandemic period. Using data gathered on all UK company insolvency filings at Companies House and formal notices in the London/Edinburgh Gazettes from 1998 to 2022, we track the incidence of distress among PE-backed companies and matched control firms and examine whether the probability of PE-backed firms filing for insolvency increases or decreases during the pandemic period relative to matched non-PE-backed firms. We document that PE-backed firms which were more vulnerable at the onset of the pandemic, or which operate in more exposed industries, were significantly more likely to enter into distress during the pandemic. The probability of more vulnerable PE-backed firms entering into insolvency increases by around 1 to 2 percentage points in the pandemic period relative to control firms. Firms which were less vulnerable were no more likely to enter into distress.

However, we find differences in the restructuring of distressed firms between both samples of firms. PE-backed firms have a considerably lower incidence of liquidation suggesting PE investors are proactive in negotiating with creditors through Company Voluntary Arrangements (CVA), whereby the owners/directors maintain control over the firm, to keep distressed portfolio companies trading relative to other owners. While PE owners may manage financial distress at a lower cost (see Hotchkiss et al. (2021), Hartman-Glaser et al. (2023)), we provide an example where we are able to see this process "in action" during a real crisis.

The CVA process is rarely used by other private companies because of the complexities, legal process and expertise required. Moreover, CVA's can be blocked by one creditor, and this is often HM Revenue & Customs, acting on behalf of the taxpayer. During the pandemic period a new process was introduced in the UK known as 'cram down' whereby the blocking actions of a creditor could be challenged in court and potentially ruled out by a judge. It is interesting, therefore that PE firms were actively using this process to protect their assets and increase survival chances. This appears to suggest that PE investors may be faster at adopting new law or regulations relative to other forms of ownership, which is another dimension of the operational expertise PE firms can bring to the table during times of distress.

In summary, our findings suggest that, while, on average, PE-backed firms outperformed closely matched industry peers during the pandemic, the most vulnerable and exposed firms did not. These firms appear to have been less active in obtaining external financing during the pandemic, and consequently, suffered a significantly higher incidence of distress. However, PE-owned firms in distress were more likely to settle out of court and to continue trading relative to non-PE-backed firms.

Our paper is related to an extensive body of research which studies how PE ownership impacts firm behaviour and outcomes (see for example Kaplan (1989), Harris et al. (2005), Boucly et al. (2011), Acharya et al. (2013), Cohn et al. (2014), Bernstein et al. (2017), Lerner et al. (2019), Eaton et al. (2020), Cohn et al. (2021), Fracassi et al. (2022)). In particular, we relate to a smaller body of literature exploring the performance of PE-backed firms during economic downturns (Thomas (2010), Wilson et al. (2012), Bernstein et al. (2019), Johnston-Ross et al. (2021)). We also contribute to the literature which is building an understanding for firm dynamics and performance during the COVID-19 pandemic (Campello et al. (2020), Fahlenbrach et al. (2021), Papanikolaou and Schmidt (2022), Alekseev et al. (2023)). Survey evidence has shown COVID-19 to have had a considerable impact on private equity and venture capital investors (Gompers et al. (2021), Gompers et al. (2022). We extend this work to measure the impact on PE portfolio companies compared to matched industry peers.

2 Data

2.1 Private equity buyouts

Our data on PE buyouts comes from S&P Capital IQ and from Pitchbook, each of which have been widely used in recent PE literature (see for example Faccio and Hsu (2017), Bernstein et al. (2019), Braun et al. (2020), Fuchs et al. (2021), Fracassi et al. (2022)). We take all PE buyouts, excluding venture capital deals, follow-on rounds of financing of the same portfolio company by the same PE investor, and excluding bolt-on acquisitions. Moreover, we only include deals which have been completed, and where a private equity buyer is defined.

We take all relevant information, such as the transaction date, the name(s) and location(s) of the investor(s), the transaction value (if disclosed), and the type of buyout transaction. In order to identify how and when the private equity investor exits a deal in each case, we use a variety of resources. We use Capital IQ's merger & acquisition database to search for sales to trade buyers and sales to other private equity investors (secondary buyouts). We also use Factiva and manual searches of financial news for acquisitions, initial public offerings, and bankruptcies/liquidations involving the target firms. In some instances, we conduct extensive web searches on a deal-by-deal basis in order to understand the ultimate outcome of the transaction.

2.2 Company financial accounts

To source companies' financial accounts, we use the FAME database, published by Bureau Van Dijk Electronic Publishing (BvDEP). This database sources historical accounts of companies in the UK from Companies House, the national UK register. The reliability of the source of companies' financial data (Companies House) and the coverage of both public and private firms is a key strength of the data. Unsurprisingly, recent empirical studies in corporate finance have acknowledged that the UK is an excellent setting in which to study private firms (see for example Brav (2009), Saunders and Steffen (2011), Michaely and Roberts (2012), Bernstein et al. (2019)). The extent of the requirement to disclose financial information in the UK, however, varies with the size of the company. Smaller companies are allowed to file abridged accounts or micro-entity accounts.[2] Since the amount of information small firms disclose to Companies House (and hence in the FAME data set) can be very limited, some of these firms may not feature in our empirical analysis. We download companies' financial accounts (balance sheets and profit & loss statements) and other firm information (such as industry codes, location, date of incorporation) for all companies in the FAME database for 2001 through 2021.

The next step is to match target firms from our list of PE buyouts in Capital IQ and Pitchbook to the FAME database. In order to maximize our matches, we do so manually. An advantage of FAME in this case is that it tracks firms' prior names. If company names differ between our list of transactions from Pitchbook and FAME, we verify that we are

[2]The thresholds for company size and the level of financial accounting disclosures in the UK as of March 2022 are available at: https://www.gov.uk/government/news/accounts-filing-options-for-small-companies

tracking the correct company by cross-checking that information such as reported sales, total assets, and company address or website are consistent between the two sources. We also use Companies House in this respect.

2.3 Insolvency filings

We gather data on all UK company insolvency filings at Companies House and formal notices in the London/Edinburgh Gazettes from 1998 to 2022. This includes company filings for administration, receivership, company voluntary arrangements (CVA), and liquidations. We are then able to match this information to our samples of PE-backed and control firms using companies' registration numbers. In doing so, we can identify precisely when PE-backed and control firms file for insolvency in our sample and the type of insolvency filing. This allows us to study whether the probability of PE-backed firms filing for insolvency increases or decreases during the pandemic period relative to matched non-PE-backed firms.

2.4 Final sample

Finally, given that we interested in examining portfolio firm performance during the COVID-19 pandemic, we reduce our sample to firms which were under PE ownership during the pandemic period. We follow Bernstein et al. (2019) (who study the performance of portfolio companies during the financial crisis) and limit our sample of PE target firms to those which had been acquired by a PE investor by the end of 2019, and had not experienced an exit by the PE investor by the end of 2020. This reduces our sample to 1,516 PE-backed firms which were under PE ownership at the onset of the pandemic.

3 Firm performance and growth

3.1 Constructing a matched control sample

For a difference-in-differences estimation, we construct a group of control firms which are similar in nature to our sample of PE-backed firms at the onset of the pandemic based on their observable characteristics. In order to do so, we follow matching methodologies used in recent PE literature (Boucly et al. (2011), Bernstein et al. (2019), Cohn et al. (2021)). Specifically, we match firms in such a way that each control firm meets the following criteria: 1) has the same two-digit SIC code as the treated PE-backed firm; 2) has total assets in the pre-pandemic year (2019) within a 30% bandwidth as the treated firm; 3) has leverage (defined as total debt divided by total assets) within a 30% bandwidth in the pre-pandemic year; 4) has return on assets within a 30% bandwidth in the pre-pandemic year. We match each PE-backed firm to up to five control firms. If a target firm matches to more than five control firms based on this matching, we select the closest five based on the quadratic distance computed based on the variables. This matching technique allows us to match 828 PE-backed firms to a total of 2,825 control firms.

Of course, given the nature of the study, how we construct the matched control group has implications for the size of the sample and the results we generate. To ensure the robustness and validity of our results, we generate several other treated-control samples of firms by adjusting our matching technique. To do so, we tighten and loosen the matching bandwidths of 30%, and we also include other matching parameters, as well as reducing the number of matching parameters included. We also control for a wide set of observable firm-level characteristics in the pre-pandemic period. These adjustments are described in detail in section 3.5.

3.2 Descriptive statistics

Table 1 shows extensive firm characteristics for matched PE-backed and control firms at the onset of the pandemic. We look at numerous variables covering firms' size, profitability, debt, cash holdings, productivity, and working capital. The matching algorithm appears to work well, with differences between PE-backed and control firms' mean and median values being minimal. Moving a step further, Table 2 shows pre-pandemic growth rates in firm characteristics of treated and control firms. Again, the mean and median growth rates across treated and control firms are very similar, suggesting that our matching process has worked well. Overall, Tables 1 and 2 provide comforting evidence suggesting that the parallel trends assumption is satisfied.

Figure 2 provides a visual interpretation of the evolution of firm performance variables around the pandemic. Specifically, the graphs present the α_t of the following equation:

$$y_{it} = \alpha_t + \alpha_i + \varepsilon_{it} \tag{1}$$

where y_{it} is the outcome variable for firm i at time t. α_t captures year fixed effects and α_i denotes firm fixed effects. We use the year before the pandemic, 2019, as the base period, and we normalize its corresponding coefficient to zero. We estimate the equation separately for both the PE-backed and matched control samples, with standard errors clustered at the firm level. We consider firms' sales, assets, employment, and earnings (as measured as EBITDA). All variable definitions are noted in the appendix.

In each instance, the PE-backed and control firms appear to follow similar paths in the pre-pandemic years, and then at the onset of the pandemic, there is a divergence. In the cases of sales, employment, and earnings, there is a marked decline for both firms when the pandemic hits in 2020, but less so for PE-backed firms relative to the control firms. As for total assets, the control firms experience a plateau while the PE-backed firms continue an upward trend. These graphs plotting year effects estimates around the pandemic offer an initial insight into the potential cushioning effect of PE ownership on firm performance during the COVID-19 pandemic. At first glance, PE-backed firms appear to have been more resilient to the negative impact of the pandemic.

3.3 Model

We then move to a formal econometric estimation of the impact of the COVID-19 pandemic on PE-backed companies in the UK using a difference-in-differences technique. Following other recent papers studying the impact of PE ownership on firm outcomes (Bernstein et al. (2019), Cohn et al. (2021)), our baseline difference-in-differences model is as follows:

$$y_{it} = \alpha_t + \alpha_i + \beta_1 PE_i * Post_t + \theta X_i * Post_t + \varepsilon_{it} \tag{2}$$

where i is a firm index, and t is a year index. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. The choice of 2020 as the first year of the pandemic is consistent with the first nationwide UK lockdown being announced on the 23 March 2020. The model also includes year fixed effects, α_t, and firm fixed effects, α_i. Standard errors are clustered at the firm-level. We also include several firm-level controls variables, X_i, to control for firm characteristics in the pre-pandemic period. Specifically, we control for firm age, size (total assets), cash holdings, sales growth, leverage, and profitability. Following Bernstein et al. (2019), these controls are taken in the pre-pandemic year and are interacted with the $Post_t$ variable. The main coefficient of interest is β_1, which will capture the estimated change in PE-backed firms' performance from before the pandemic to after the pandemic outbreak, relative to control firms. A positive coefficient would reveal that PE-backed firms are more resilient to the negative impact of the pandemic.

To further validate the parallel trends assumption, we extend the analysis to gain an insight into how firm performance evolves over time around the pandemic. Figure 2 show that the divergence in treated and control firms' performance appears to have coincided with the onset of the pandemic. We formally explore how firm performance evolves over time around the pandemic in more detail by estimating the following equation, which shows year-by-year effects of private equity ownership around the pandemic:

$$y_{it} = \alpha_t + \alpha_i + \Sigma\beta_k(PE_i) + \varepsilon_{it} \tag{3}$$

Where we estimate a different β_k for each year between 2017 and 2021, using the pre-pandemic year, 2019, as the reference year. Given our matching methodology, we expect the effect of private equity ownership on firm performance to appear only at the onset of the pandemic.

3.4 Results

Panel A of Table 3 shows the baseline results of estimating equation 2 where we study whether PE-backed firms outperformed matched control firms during the COVID-19 pandemic period. In each specification we include firm, and year fixed effects. We report coefficient estimates and standard errors clustered at the firm-level. Even-numbered columns include a vector of firm control variables, taken in the pre-pandemic year and interacted with the *Post* variable. These include firm age, size, cash holdings, sales growth, leverage, and profitability.

The results are striking. Across all measures of firm performance, the results suggest that PE-backed firms outperformed other similar firms, in line with our hypothesis. The results are strongly statistically significant, and meaningful in terms of the size of their economic magnitude. In columns 1 and 2, the coefficients imply that PE-backed firms' increased their sales by approximately 5% during the pandemic period relative to control firms. This is consistent with Figure 2, where we examine the year effects estimates around the time of the pandemic. There appears to be a divergence in PE-backed and control firms' sales at the onset of the pandemic.

In columns 3 to 8, we study other measures of firm performance. The results are similar. For example, in column 3, we find that PE-backed firms assets increased by around 10% during the pandemic relative to control firms. Again, this is consistent with Figure 2, where matched non-PE-backed firms' asset growth appears to stall at the beginning of the pandemic, while PE-backed firms continue their upward trend. With regards to unemployment, UK redundancies reached record levels during the pandemic in 2020. Over 400,000 redundancies were recorded between September and November of 2020, which was a record high in the UK (Powell et al. (2022)). When we study employment in our matched sample of firms, we find that PE-backed firms' employment was less affected during the pandemic relative to that of similar, non-PE-backed firms. That is, their employment levels increased by approximately 7% relative to control firms' employment in the pandemic period. This aligns with the divergence during the pandemic shown in Figure 2. PE-backed firms' earnings likewise increase compared to the control group. Specifically, the coefficient estimates in columns 7 and 8 suggests an increase of around 6% in earnings relative to the control group during the pandemic period.

While the estimates in panel A of Table 3 capture the average change in firm performance from before to after the onset of the pandemic, they do not shed light on the timing of these changes. Panel B of Table 3 shows the results of estimating equation 3, which indicates the time-varying behaviour of the treatment effects of our dependent variables.

The results corroborate those in panel A. For example, in columns 1 and 2, there are no divergent trends in PE-backed and control firms' level of sales prior to the pandemic. The divergence appears in 2020 when the pandemic hits. Similar trends appear in columns 3 to 8 when we look at firm assets, employment, and earnings. Any pre-pandemic differences are weakly statistically significant, if significant at all. Overall, the results here support the lack of statistically significant patterns before the pandemic and that firm performance diverged considerably at the onset of the pandemic.

3.5 Robustness

We perform several robustness checks to ensure the validity of our results. To begin, we make several adjustments to our matching technique, including altering the matching bandwidths, and adjusting the matching parameters. The results of these modifications are shown in Table 4. First, in panel A, we use a 50% matching bracket for firms' total assets, ROA, and leverage (as opposed to 30%). This yields a sample of 1,022 PE-backed firms, and 4,054 control firms. Second, in panel B we narrow the matching bracket to 20%. Naturally, this reduces our sample size and we have 636 treated PE-backed firms, and 1,782 control firms. Third, following Boucly et al. (2011) and Bernstein et al. (2019), in panel C, we drop firm leverage from the matching algorithm. This results in a larger sample of 1,243 PE-backed and 5,115 control firms. Fourth, we use different matching parameters in panel D. Specifically, we match on firm industry, and on total assets, cash holdings, as measured by the ratio of cash to total assets, and sales growth being within a 30% bracket in the pre-pandemic year. In this case, we obtain a smaller sample of 97 PE-backed firms, and 300 control firms. Across all panels of Table 4, whilst the magnitude of the coefficients varies slightly, the results continue to show the outperformance of PE-backed firms during the pandemic period relative to matched control firms.

Next, following Bernstein et al. (2019), we control for attrition bias by dropping all firms which experience an exit via acquisition or bankruptcy during the sample period. This results in a sample of 642 PE-backed firms and 2,196 control firms. The results are displayed in the appendix, and we continue to find that PE-backed firms outperform closely matched control firms during the COVID-19 pandemic.

The systemic nature of pandemic shock precipitated Government intervention to support businesses and a range of support schemes to mitigate against the impacts of Covid-19. A policy tool used in the Covid-19 pandemic was the Loan Guarantee Scheme. In total the government guaranteed more than £80bn of loans and 1,061,046 UK limited companies received some form of bank loan, guaranteed by the government, during the pandemic period. We have access to the LGS loan portfolio and therefore can identify firms in the sample that received a loan. Thus, in case this government support financing is impacting our results, we exclude all firms that received a guaranteed loan from the sample. The results, which are shown in the appendix, are strongly consistent with the baseline results.

Finally, to further validate our findings, we next drop all buyouts which completed in 2019, which reduces our sample to 703 PE-backed firms and 2,395 control firms. This alleviates any concern that some of our PE buyouts occurred too close to the beginning of the pandemic and that the ownership change and the impact of the PE investor may not have taken full effect by the time of the pandemic. This leaves us with a sample of PE-backed firms which were acquired by PE investors some time before the onset of the crisis. While our sample size is reduced by 15%, our findings are upheld. That is, PE-backed firms continue to outperform relative to matched control firms during the pandemic period. These results are provided in the appendix.

4 Firm vulnerability and industry exposure to the pandemic

The impact of the pandemic was not homogeneous across all firms and industries. Firms naturally differ in their financial structure, balance sheet composition, and growth trajectory at any given moment in time. When the pandemic hit, some firms held large cash buffers, had little leverage, and had limited exposure to rollover risk. Given their consequent ability to finance an unexpected cash flow shortfall, these firms have increased financial flexibility (Fahlenbrach et al. (2021)). On the other hand, firms with a higher degree of leverage and less cash holdings enjoy less flexibility, and unanticipated shocks to their cash flow can have more severe repercussions for their financial health. Indeed, Fahlenbrach et al. (2021) show that firms which had lower financial flexibility at the onset of the pandemic (i.e. firms with less cash and higher leverage) experienced weaker operating performance and lower stock returns compared to firms which had stronger balance sheets. Similarly, Campello et al. (2020) show that credit constrained firms (firms with lower cash holdings or firms with no credit lines to tap into) cut job postings by more than that of less constrained firms. Barry et al. (2022) provide evidence that greater workplace, investment, and financial flexibility helped to mitigate the impact of the pandemic on firms real activities.[3]

As for different industries, certain sectors were naturally more exposed to the pandemic and the resulting restrictions and lockdowns than others. There was a supply shock due to government-mandated lockdowns which caused production to fall for firms and industries which are dependent on workers being close to one other. At the same time there was a demand shock as demand fell for firms whose customers have to interact with the firm and its employees in-person (Papanikolaou and Schmidt (2022), Fahlenbrach et al. (2021)).

[3] Ding et al. (2021) show that the pandemic-induced drop in stock returns was milder among firms with stronger pre-2020 finances (more cash and undrawn credit, less debt, and greater profits.

Consequently, certain industries were far more exposed to the COVID-19 pandemic relative to others. Fahlenbrach et al. (2021) show that industries which were more exposed to lockdowns and the need for social distancing performed poorer during the pandemic, based on their stock returns and their operating performance. Likewise, Papanikolaou and Schmidt (2022) document evidence that industries where a higher fraction of the workforce could not work remotely experienced larger falls in employment and expected revenue growth, weaker stock market performance, and a higher probability of default.

With regards to the role of PE firms and their role in mitigating constraints during the pandemic period, PE firms may have focused their efforts on weaker firms and on those in more exposed industries, and the outperformance of PE-backed firms may be particularly strong amongst these companies. Indeed, Bernstein et al. (2019) find evidence consistent with this during the global financial crisis. They find that the positive impact of PE ownership on firm investment during the crisis is stronger in companies which were ex-ante more likely to be constrained. The authors note two primary channels through which PE investors can help to mitigate constraints during an economic downturn. Firstly, through providing further equity injections. Secondly, through easier access to debt markets given PE firms often enjoy strong ties with banks and other lenders (Ivashina and Kovner (2011)).

Alternatively, the nature of the crisis may have reduced the ability of PE firms to help the most vulnerable firms outperform in that they were unable to reverse the impact of the pandemic on firms. That is, they could not make halt the spread of the virus, or overturn lockdown decisions, and consequently re-introduce demand which had evaporated due to the spread of the pandemic. This contrasts with the global financial crisis, which was a credit crisis in nature, where PE firms were able to directly mitigate the effect of the crisis and help firms access credit, which other firms struggled to access. In turn, these PE-backed firms could then outperform their peers (Bernstein et al. (2019)). Moreover, the pandemic differed in that there was an unprecedented level of policy intervention, including furlough schemes, and various business loan schemes made available to businesses of all sizes. Given the widespread availability of government aid, this may have reduced the ability of PE investors to provide something above and beyond what other non-sponsored firms were able to access. This is in stark contrast to the global financial crisis, where lending to businesses and the availability of credit sharply contracted. A more sinister perspective of this would be that PE firms may have "cut their losses" on their weakest or most exposed firms, and instead focused their attention on firms which they deemed to be more manageable and "treatable".

Our findings so far suggest that, on average, PE-backed firms outperformed closely matched control firms during the pandemic period. In this section, we now study whether PE firms were able to help the most exposed firms outperform matched peers during an exogenous shock with abundant policy intervention such as the COVID pandemic, or whether the outperformance is driven by firms which were in better health at the onset of the pandemic and in less-exposed industries. We do so using a wide range of measures of firm vulnerability and industry exposure to the pandemic.

4.1 Measures of firm vulnerability and industry exposure

We use nine measures of firm vulnerability. We first categorise firms as being more or less vulnerable on the basis of their growth trajectory when the exogenous pandemic shock occurred. First, we consider a firm to be more vulnerable if its one- or two-year growth in sales in 2019 is in the bottom quartile.[4] Firms which were already on a weaker growth trend in the pre-pandemic period are likely to have been even more impacted by the

[4]In unreported regressions we also use three-year growth rates. We also classify firms as being vulnerable if their one-, two-, or three-year growth in sales, EBITDA, or employment growth is negative in 2019. These results are very similar and are available upon request.

unanticipated, exogenous shock to demand and cash flow caused by the pandemic. Next, we do similar for growth in both firm EBITDA, and firm employment. We then classify firms as being more or less vulnerable on the basis of their level of financial flexibility, following Fahlenbrach et al. (2021). Specifically, we classify a firm as being more vulnerable if its ratio of cash over assets is in the bottom quartile of the distribution at the onset of the pandemic. Next, we consider firm leverage, and define a firm as being more vulnerable if its ratio of short term debt over total assets is in the top quartile. The intuition is that firms with greater cash reserves and less short-term debt have greater financial flexibility to finance an unanticipated shock to their cash flow. Lastly, we also use a measure of labor intensity, defined as the number of employees over total sales, as more labor intensive firms are more likely to have had a higher exposure to the pandemic (Fahlenbrach et al. (2021)). In this case, we classify firms that are in the top quartile of labor intensity as being more vulnerable.

Next, we focus on the industry-level exposure to the pandemic. We use four measures of industry exposure. First, we follow the Office for National Statistics (ONS) definition for "high contact" industries which are more reliant on physical interaction and so were more adversely affected by the restrictions that were put in place. These are wholesale and retail; transportation and storage; accommodation and food services; arts, entertainment and recreation; and other services. Second, we follow Bloom et al. (2020) who use the UK Decision Maker Panel survey data to gauge the impact of the pandemic on UK firms. We categorise exposed industries as those in top five most affected in terms of the expected impact on their sales and employment from 2020Q2 to 2021Q1. These are: accommodation and food; administration and support; recreational services, construction, and other services. A concern may be that these first two definitions of exposure are too broad. We therefore use two further measures of industry exposure at a more granular level. First, we follow Koren and Pető (2020) who classify industries at the three-digit NAICS level based on how they are affected by social distancing. Following Fahlenbrach et al. (2021), exposed firms belong to industries in the top quartile of the affected share distribution in Koren and Pető (2020). Second, we use the manual classification of industries at the six-digit NAICS level in Fahlenbrach et al. (2021).[5] The authors manually review six-digit NAICS industries to eliminate industries where selling takes place online rather than in-person, as a three-digit NAICS industry could have businesses that are brick and mortar businesses or online businesses. These two types of businesses have very different exposures to the pandemic and the accompanying lockdowns and restrictions. As an example from Fahlenbrach et al. (2021), while the three-digit NAICS industry 454 may be classified as exposed, they reclassify the subindustry 454110 "Electronic Shopping and Mail-Order Houses" as less exposed (e.g., Amazon.com is a member of that subindustry). Exposed firms are therefore part of the industries manually classified by Fahlenbrach et al. (2021) as having a greater exposure to the pandemic.

4.2 Results

We run equation 2 on subsamples of firms which are more and less vulnerable at the onset of the crisis, and firms which operate in more and less exposed industries. The results are shown in Table 5 and in Table 6. Firstly, in Table 5 we study firm-level vulnerability at the onset of the crisis. In panel A, we see that, across all nine measures of vulnerability, the positive impact of PE ownership on firm sales during the pandemic is stronger for firms which were in less vulnerable positions at the onset of the pandemic. The coefficients on the more vulnerable firms are largely insignificant, whereas the estimates on the sample of less vulnerable firms are strongly statistically significant suggesting that these firms

[5] We are extremely grateful to the authors for kindly sharing this classification with us.

significantly outperformed during the pandemic period. For example, in panel A, the outperformance of their sales during the pandemic period is found to be between 5 and 10 percentage points. This is consistent across a range of definitions of firm vulnerability, including pre-pandemic firm growth, financial flexibility, and labor intensity. In panels B to D, we observe similar results for firm assets, employment, and earnings. That is, the effect of PE ownership on firm performance during the pandemic is only statistically significant for companies that were ex-ante less likely to be constrained; i.e, firms with stronger pre-pandemic growth, firms which held larger cash buffers, firms with less short-term debt, and less labor-intensive firms. For example, in panel C where we look at firm employment, the coefficients on less vulnerable firms are positive and statistically significant at the 1% confidence level, implying their employment increases by over 6 percentage points, whereas the coefficients on more vulnerable firms are smaller in economic magnitude and statistically insignificant.

Moving to Table 6, we then study industry-level exposure to the pandemic. The results echo somewhat those in Table 5. That is, we find that the impact of PE ownership on firm performance during the pandemic is stronger on firms operating in less-exposed industries compared to those in more-exposed industries. For example, in panel A, the impact of being PE-owned on firm sales during the pandemic is largely statistically insignificant for firms in the most exposed industries. In contrast, the coefficients for firms in less exposed industries imply that they outperformed by approximately 5-7 percentage points. We observe similar results for firm assets, employment, and earnings in panels B to D. Firms in less exposed industries assets increased by over 10 percentage points, while the impact on firms in more exposed industries is statistically insignificant. Panels C and D show likewise for employment and earnings. PE ownership has a positive and strongly statistically significant impact on firms operating in less exposed industries employment and earnings, while the impact on firms in more exposed industries is found to be statistically insignificant.

Overall, the results of this section suggests that the impact of PE ownership on firm performance during the pandemic was not homogeneous across all firms. In particular, the positive effect of PE ownership on firm performance during the pandemic was stronger among firms which were less vulnerable when the shock occurred, and in firms which operate in industries which were less-exposed to the pandemic. The performance of more vulnerable PE-backed and control firms, or those in more exposed sectors, was very similar. This suggests that PE investors were less able to help the most vulnerable and exposed firms outperform during the pandemic, contrasting with the findings of Bernstein et al. (2019) during the global financial crisis. This may be a reflection of the nature of the shock itself. The financial crisis was characterised by firms being starved of credit, and PE firms, with their deep pockets and strong connections with banks, were able to fill this gap. During the pandemic, however, PE firms were less able to undo the immediate impact of the shock. That is, they were unable to reverse the government-mandated restrictions and lockdowns which hampered demand and consequently deteriorated firms' cash flow. An alternative take may be that PE firms may have cut their losses on firms in weaker positions, and focused their efforts on other firms which were less exposed to the immediate impact of the pandemic. We study this in the next section.

5 Did PE firms cut their losses on weaker firms?

5.1 Channels of growth and value-added

Having established results which indicate, on average, an outperformance of PE-backed firms during the pandemic, we next turn our attention to the potential channels and mechanisms through which the impact may work. That is, we ask what enabled PE-backed firms to outperform during the pandemic. Literature to date acknowledges three primary channels through which PE investors can help their portfolio companies: Operational engineering, governance engineering, and financial engineering (see for example Gompers et al. (2016), Hammer et al. (2017), Cohn et al. (2022), Gryglewicz and Mayer (2023)). Operational engineering relates to actions such as reducing costs, making bolt-on acquisitions, expanding overseas and providing strategic guidance. Governance engineering includes making changes to the board or senior management, and helping to hire managers and directors. Lastly, financial engineering captures activity such as injecting further equity into companies, accessing other sources of liquidity, such as debt finance, and facilitating a high value exit. These various channels are, of course, not mutually exclusive from one another. Survey evidence from Gompers et al. (2016) concludes that investors create value from a combination of operational, governance, and financial engineering. Empirical evidence of PE buyouts in various countries supports this notion (see for example Boucly et al. (2011), Bernstein and Sheen (2016), Bernstein et al. (2019), Wilson et al. (2022)).

In this section, we look to empirically study the ways in which PE-backed companies were able to outperform industry peers during the pandemic. We focus on both operational and financial engineering measures.[6] We consider three types of operational engineering: reduction of costs, improved working capital, inventory and cash flow management, and stronger capital investment. Through the operational engineering channel, we would expect

[6]We do not consider governance engineering measures for several reasons. The primary reason is due to data availability. We are only able to observe the appointment of directors to the board, but do not observe the often informal hiring of interim managers (from the PE firms pool of experienced and specialist managers), which is sometimes on a consultancy-type basis. In interviews with several UK PE GPs, investors spoke of managers they used across several of their portfolio companies who came in for a short period of time to help affected companies. These were not formal appointments of directors, so such actions are difficult to observe as data points. This is reflected in Gompers et al. (2016) where they speak of the introduction of "shared" services where PE investors can help several of their companies simultaneously. Moreover, Gompers et al. (2022) document evidence that operational and financial engineering were considerably more prevalent as sources of value during the pandemic compared to governance engineering.

PE-backed firms to have more resilient levels of investment, better cash flow management, and potential reductions in their cost base relative to control firms (Boucly et al. (2011), Gompers et al. (2016), Gompers et al. (2022)). As for channels of financial engineering, we examine both debt and equity financing. PE firms can often inject further equity into portfolio companies to helps resolve any distress concerns (Bernstein et al. (2019), Hotchkiss et al. (2021)). Similarly, portfolio firms can raise debt finance as source of liquidity during downturns (Bernstein et al. (2019), Gompers et al. (2022)). It is well-documented that PE investors have strong ties with banks, which they may be able to leverage during downturns (Ivashina and Kovner (2011). We track both equity and debt financing of companies from before to after the pandemic period.

To examine the mechanisms through which PE portfolio companies may be outperforming their peers, we use a similar DiD model as in equation 2. To capture operational engineering, we use three measures. First, following Boucly et al. (2011), we measure cost reduction as the ratio of intermediate inputs to sales, where intermediate inputs are measured as the cost of sales plus administration expenses, less remuneration. A negative coefficient would suggest that PE-backed firms cut costs more aggressively than their peers during the pandemic. Secondly, we study firms working capital, inventory, and cash flow management. To do so, we examine firms' cash conversion cycle (CCC). The CCC combines the cycles of inventories, accounts receivable, and accounts payable and refers to the time elapsed from the moment the firm pays for its inputs to the moment it receives payment for the goods it sells. It is a widely used metric to assess the effectiveness of a firm's management and the liquidity needed for external financing (Wang (2019)). In particular, a lower CCC implies a firm is better able to manage its working capital and is in a better liquidity position. A higher CCC suggests that firms have to wait longer before they can receive cash from their sales and therefore have a higher need for external financing for their working capital (Raddatz (2006), Tong and Wei (2011)). Lastly, we study firm investment during the pandemic, where investment is defined as the change in fixed assets plus any depreciation for the year, and is scaled by assets (Bernstein et al. (2019)). Where financial engineering is concerned, we study both equity and debt financing. Following Bernstein et al. (2019) and Haque et al. (2022) we define equity financing as the change in book value of equity, less profit, and scale by assets. Debt issuance is the change in total debt, and is scaled by assets. Under the financial engineering mechanism, we would expect that the debt and equity issuance of PE-backed firms is more resilient during the pandemic compared to their nonsponsored peers.

5.1.1 Results

Table 7 presents the results for both operational and financial engineering channels. Panel A shows the baseline difference-in-differences estimate of equation 2, while panel B shows the year effects estimates of equation 3. Columns 1 to 3 cover the three operational mechanisms while columns 4 and 5 show the financial engineering measures. In columns 1 to 3 of panel A, we find weak evidence of PE firms adding value via operational measures during the pandemic. In column 1, we find that cost reduction in PE-backed firms was not significantly different from control firms during the pandemic. In column 2, we observe that PE-backed firms experienced a drop in their cash flow conversion cycle relative to control firms, suggesting they had more efficient management of their inventory, cash and working capital cycle during the pandemic. In particular, PE-backed firms' cash conversion cycle fell by around 6 percentage points compared to control firms, albeit the estimate is only statistically significant at the 10% level. Lastly, we ask if the outperformance during the pandemic can be explained by stronger levels of capital investment. The point estimate in column 3 implies that PE-backed firms' investment fell by around two percentage points less than that of control firms during the pandemic, but, again, the estimate is only weakly statistically significant.

We then move to the financial engineering channel where we study firms' equity and debt issuance. We find that both equity and debt issuance were significantly higher for PE-backed firms during the pandemic relative to control firms. The estimates in columns 4 and 5 of panel A indicate that the impact on debt issuance was around twice the size of that of equity issuance in terms of it's economic magnitude. Specifically, the results imply that PE-backed firms' debt issuance was around two percentage points stronger than that of the control group. Bernstein et al. (2019) find that, while on average, debt issuance declined during the 2008 financial crisis, this decline was 4% smaller for PE-backed firms relative to control firms. Our estimates imply similar results during the COVID-19 pandemic, albeit of a smaller economic magnitude. The results concerning equity issuance are consistent with those of Haque et al. (2022) in the US. The coefficient suggests that PE-backed firms' equity issuance was around one percentage point higher during the pandemic. Overall, our results concerning the financial engineering mechanism reflect the survey evidence in Gompers et al. (2022), where PE investors highlight the importance of accessing debt financing more so than further equity injections as a means of helping companies during the pandemic. They find that accessing debt was more prevalent as a means of helping companies compared to further equity infusions during the pandemic. Our empirical evidence supports this notion. In summary, we find evidence suggesting that financial engineering played an important role for PE-backed firms during the pandemic period.[7]

[7] In table 4 of the appendix, we also exploit the cross-section of PE sponsors, and show that access to debt financing appears to have been stronger for portfolio companies backed by more reputable PE sponsors. This is consistent with the relationship and reputation stories in Demiroglu and James (2010) and Ivashina and Kovner (2011), and with the renegotiation channel proposed by Haque and Kleymenova (2023).

5.1.2 Firm vulnerability and industry exposure

Having found evidence suggesting that the outperformance of PE-backed firms during the pandemic was driven by firms which were less vulnerable to the downturn and which operated in less exposed industries, and having observed that the primary observable channel of value-added from PE investors during the pandemic was via financial engineering (both debt and equity issuance), we now study whether or not the financial engineering mechanism was more or less prominent across different types of PE portfolio firms.

The results are presented in Tables 8 and 9. First, in Table 8, we study whether the financial engineering channel differed across firms which were more and less vulnerable at the onset of the pandemic. In panel A we study equity issuance, and in panel B we consider debt issuance. Across both debt and equity issuance, the results are striking. The estimates imply that the positive impact of PE ownership on equity and debt issuance during the pandemic is stronger for firms which were in less vulnerable positions at the onset of the pandemic. Where more vulnerable firms are concerned, they do not outperform closely matched peers in terms of their access to external financing during the pandemic. The coefficients on more vulnerable firms are insignificant, whereas the estimates on the sample of less vulnerable firms are strongly statistically significant suggesting that these firms' debt and equity issuance was considerably higher compared to closely matched control firms during the pandemic period. The point estimates suggest that less vulnerable firms equity issuance was approximately 1.1 to 1.4 percentage points higher during the pandemic, while their debt issuance was around 1.3 to 1.9 percentage points higher. This is consistent across our nine definitions of firm vulnerability, which include weaker pre-pandemic firm growth, less financial flexibility, and a higher labor intensity.

We find similar results in Table 9. That is, the effect of PE ownership on debt and equity issuance seems to be stronger among firms in industries less exposed to the pandemic.

In panel A, the impact of being PE-owned on equity issuance during the pandemic is statistically insignificant for firms in the most exposed industries, relative to matched controls, implying their access to external financing during the pandemic was similar. In contrast, the coefficients for firms in less exposed industries imply that they outperformed by approximately one percentage point. We observe similar results for debt issuance in panel B. Debt issuance of firms in less exposed industries was around two percentage points higher.

5.2 COVID loan analysis

The systemic nature of the pandemic shock precipitated Government intervention to support businesses and a range of support schemes to mitigate against the impacts of COVID-19. A policy tool used in the COVID-19 pandemic was the Loan Guarantee Scheme. Loan Guarantee schemes are policy instruments that address the imperfections in the market for finance, particularly for smaller firms. In the case of COVID business loans, the UK government extended the existing Loan Guarantee Scheme encouraging the banking sector to advance loans to businesses with the government (British Business Bank) acting as a guarantor in the event of default. In total the government guaranteed more than £80bn of loans and over one million UK limited companies received some form of bank loan, guaranteed by the government, during the pandemic period.

In Table 10 we present summary statistics on COVID-19 loan activity across our samples of PE-backed and controls firms, as well as comparing to the general population of all UK limited companies. Panel A shows loan activity across all companies and we can see that PE-owned firms were very active in obtaining COVID loans, and considerably more so than the control group. Almost a quarter of the PE sample obtained some form of loan, whilst only 15% of control firms did so. This reflects our previous findings in Table

7, that PE-backed firms were more engaged in accessing financing during the pandemic. The repayment rate on loans for both samples is very similar, while PE-owned firms have a higher default rate, albeit the number of defaults in the sample is very low (only ten in total).

In panel B, we observe the loan terms. PE-backed firms appear to have secured larger cheque sizes, with an average loan size of £1.8m versus an average of £1.1m for the control group. This difference in means is strongly statistically significant. The loan term and interest rate charged are very similar across both groups of firms. In summary, PE-owned firms appear to have been slightly more active in accessing COVID loans, and secured larger cheque sizes, on average.

5.3 Financial distress

In the final section of our empirical analysis, we study incidence of financial distress during the pandemic. To do so, we use data on all historical UK company insolvency filings at Companies House and formal notices in the London/Edinburgh Gazettes. This includes company filings for administration, receivership, company voluntary arrangements (CVA), and liquidations. Administration involves handing over the control of the firms to an Insolvency Practitioner who will attempt to restructure a business, with the aim of either turning it into a profitable company or effecting a sale of the business to preserve some value and employment. A CVA sets out a plan for the repayment of the company's outstanding debts and occurs where creditors take action to recover debt. It typically involves minimal court involvement and allows directors to retain control of the business. A company has the option to continue trading whilst under a CVA or cease trading - the decision depends on the company's situation and its creditors. Thus the firms' management have been proactive to retain control, under the supervision of an independent insolvency practitioner, and enter into an agreement with creditors to pay some or all outstanding debts over a specified period. In this case the firm may recover from the insolvency and continue to trade. Lastly, liquidation is the end stage of a company whereby the assets are sold and proceeds distributed to creditors. This rich data set allows us to identify when a company in our data set files for insolvency, and what type of filing they use. We can then study whether the probability of PE-backed firms filing for insolvency increases or decreases during the pandemic period relative to closely matched non-PE-backed firms.

To formally test whether PE-backed firms were more susceptible to filing for insolvency during the pandemic period relative to matched control firms, we estimate the following equation, where the dependent variable, $Insolvency_{it}$, is a dummy variable equalling one if a company files for insolvency in a given year, and zero otherwise:

$$Prob(Insolvency_{it} > 0) = \alpha_t + \alpha_i + \beta_1(PE_i * Post_t) + \varepsilon_{it} \tag{4}$$

Table 11 shows estimates from treated-control linear probability estimations on the likelihood of filing for distress during the pandemic period. As before, we estimate the equation on subsamples of firms which were more and less vulnerable at the onset of the crisis (panel A), and which operate in industries which were more or less exposed to the pandemic (panel B).

The probability of PE-backed firms entering into distress during the pandemic appears to be driven by firms which were more vulnerable at the onset of the pandemic, whereby they had weaker growth, less financial flexibility, or more short-term debt, and by firms which operate in industries which were more exposed to the consequences of the pandemic. Incidence of financial distress appears to be driven by these subsets of PE-backed firms. In panel A, the coefficient on the $PE_i * Post_t$ variable is positive and strongly statistically significant in each specification for the sample of more vulnerable firms, suggesting that PE-backed firms which were vulnerable at the onset of the shock were significantly more likely to enter into insolvency during the pandemic compared to nonsponsored firms. The effect is economically significant. The probability of more vulnerable PE-backed firms entering into insolvency increases by around 1 to 2 percentage points in the pandemic period relative to control firms. When we categorise firms based on their industry exposure in panel B, we see similar results. The positive impact of PE ownership on financial distress is concentrated in firms operating in industries which were more exposed to the pandemic. In panels B and C of Table 12 we show the distribution of distressed firms across our measures of vulnerability and exposure. Unsurprisingly, relatively more PE-backed firms which file for insolvency are more vulnerable at the onset of the pandemic, and are in more exposed industries.

However, we find differences in the restructuring of distressed firms between both samples of firms. Panel D of Table 12 details the type of insolvency filings across both PE-backed and control firms. No PE-backed firms in the sample which file for insolvency are liquidated, while over half secure a CVA, suggesting that PE investors are proactive in negotiating with creditors to keep distressed portfolio companies trading relative to other owners, as discussed earlier. This is consistent with recent evidence on the insolvency risk and restructuring of PE-backed firms in distress (Wilson and Wright (2013), Hotchkiss et al. (2021)). Hartman-Glaser et al. (2023) show that firms with better access to equity financing, i.e., PE-backed firms, obtain more cash flow-based financing and are less likely to be liquidated. In distress, these firms are continued as going-concern, akin to Chapter 11 bankruptcy. Hotchkiss et al. (2021) find that, while PE-backed firms default at higher rates than other companies borrowing in leveraged loan markets, they restructure faster, avoid bankruptcy court more often, and liquidate less often compared to other firms in distress. The authors conclude that while PE-backed firms may be more likely to default, PE investors appear to manage financial distress at a lower cost. Our findings are consistent with this, and show evidence of this in action during a real economic crisis.

In summary, our analyses suggest that, while, on average, PE-backed firms outperformed closely matched industry peers during the COVID-19 pandemic, the most vulnerable and exposed firms did not. These firms appear to have been less active in obtaining additional financing during the pandemic, and consequently, suffered a significantly higher incidence of distress. However, distressed PE-backed firms were more likely to restructure out of court and with their owners keeping control, relative to other firms.

6 Conclusion

In this paper, we investigate the performance of PE portfolio companies during the recent COVID-19 pandemic. Our findings suggest that, on average, PE-backed firms outperformed closely matched industry peers during the pandemic period. However, the positive impact of PE ownership on firm performance during the COVID-19 pandemic appears to have been stronger among firms which were less vulnerable and exposed to the pandemic. Firms which had lower growth, fewer cash holdings, and more short-term debt in the pre-pandemic period, and firms which operated in industries more exposed to the consequences of the pandemic did not outperform.

In subsequent analysis, we find that differences in access to financing may explain this pattern. Our results suggest that the primary channel of value-added from PE during the pandemic was enhancing access to equity and debt financing rather than operational changes made to firms. The impact of PE on debt and equity issuance was particularly strong in less vulnerable and exposed firms. This suggests that PE investors may have been less active in working with firms which were more vulnerable when the pandemic hit, and may have been willing to cut their losses on firms which were already in weaker positions at the onset of the shock, or which operate in industries which were considerably more exposed to the shock, in order to focus their efforts on firms they deemed to be more treatable. Consequently, more vulnerable and exposed PE-backed firms had a significantly higher chance of filing for financial distress. However, in cases of distress, PE-backed firms were more sophisticated in their use of insolvency procedures and more often restructure through formal negotiations with creditors (CVAs), and were less often liquidated compared to distressed matched peers which were not backed by a PE investor.

These conclusions shine a new and important light on how PE investors may respond to a large, exogenous shock like the COVID-19 pandemic. Unlike an endogenous shock such as the global financial crisis which was characterised by credit markets freezing and businesses struggling to access finance, and where PE investors could reverse the main impact of the shock on the most constrained firms by facilitating this access (Bernstein et al. (2019)), the COVID-19 pandemic was an exogenous shock. PE investors, despite their obvious strategic know-how, their deep pockets, and their wide networks of consultants, banks, and lenders, were restricted in their capability of reversing the most severe consequences of the pandemic, such as government-mandated lockdowns, social distancing measures, and demand in certain industries drying up. Consequently, they were less able to aid the most severely-affected firms outperform during the pandemic period. Our findings suggest that they focused efforts on portfolio firms which were less vulnerable and exposed to the pandemic.

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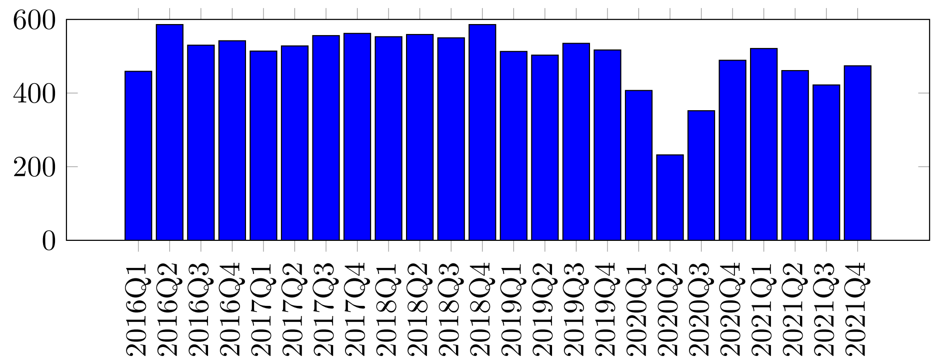
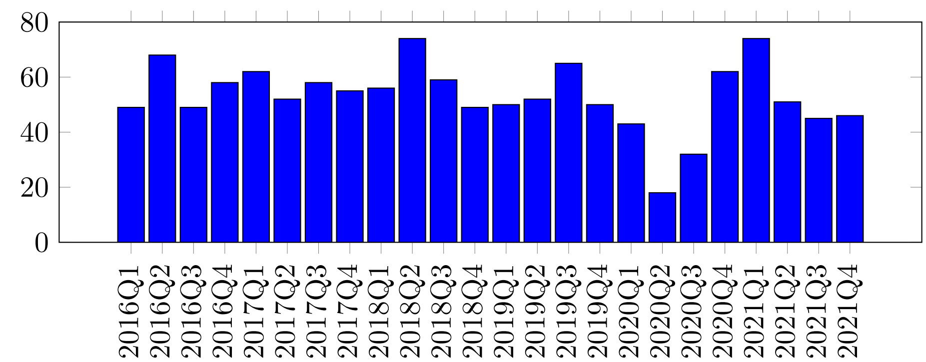
Figures

Figure 1: The number of PE buyouts over time

These charts show the number of PE buyouts of global companies and of UK companies each quarter from 2016Q1 to 2021Q4. Data comes from S&P Capital IQ.



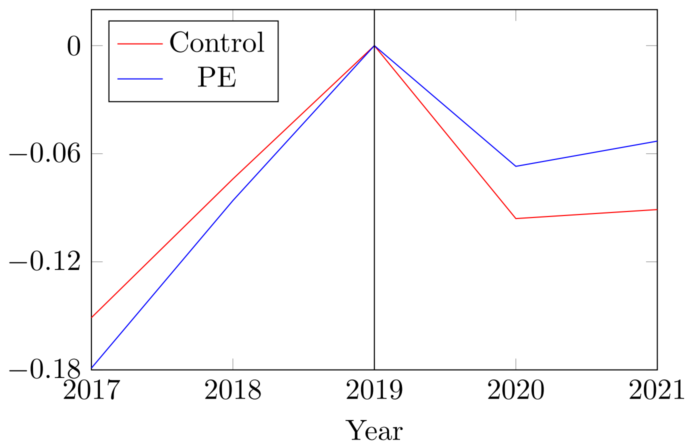
(a) Global buyouts



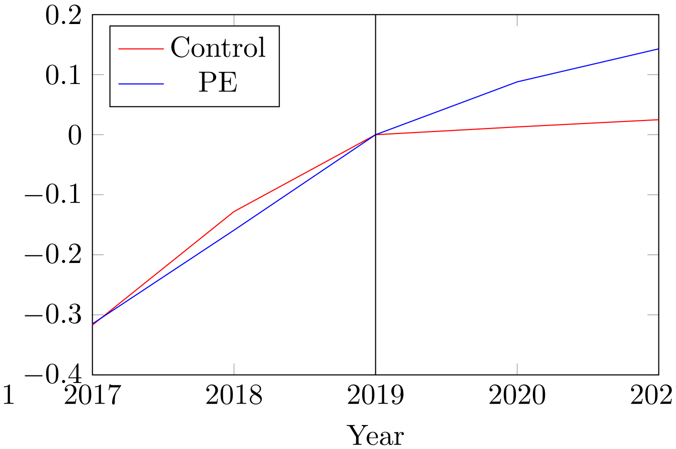
(b) UK buyouts

Figure 2: The effect of PE ownership on firm performance during the COVID-19 pandemic

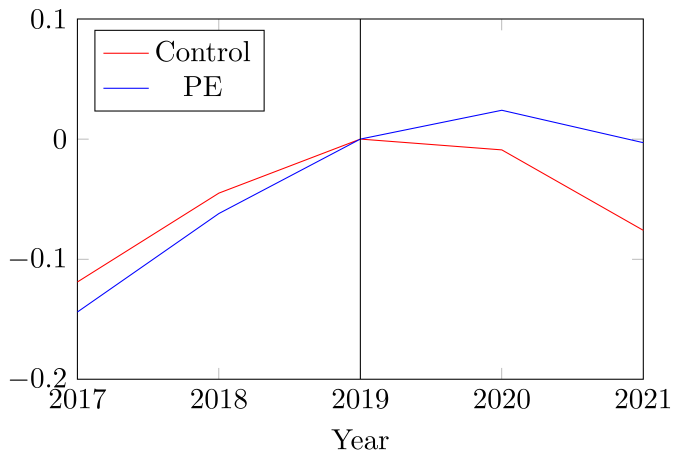
This figure reports the α_t of the following equation: $y_{it} = \alpha_t + \alpha_i + \varepsilon_{it}$, where α_t captures year fixed effects and α_i captures firm fixed effects. The year before the pandemic, 2019, is the base period, and its corresponding coefficient is normalized to zero. The equation is estimated separately for both the PE-backed and control firms, with standard errors clustered at the company level.



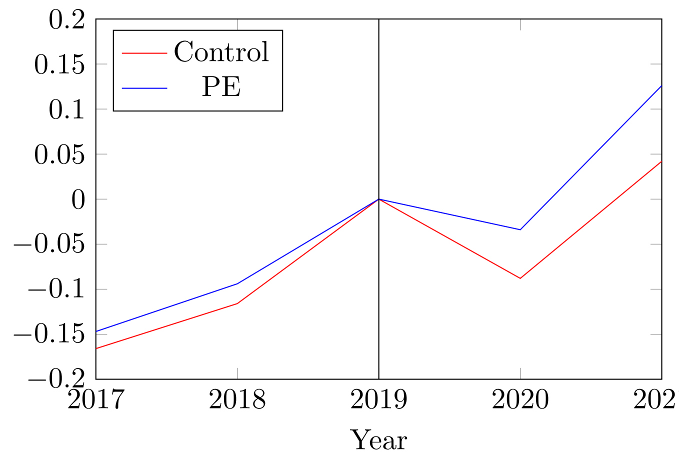
(a) Sales



(b) Assets



(c) Employment



(d) Earnings

Tables

Table 1: Matched treated and control pre-pandemic descriptive statistics

The table reports summary statistics for the pre-pandemic year (2019) across PE-backed companies and control firms. *PE-backed* refers to all PE-backed companies; *Control* refers to a sample of non-PE-backed firms, matched on their two-digit SIC code, total assets, ROA (net income/total assets), and leverage (total debt/total assets) within a 30% bracket in the pre-pandemic year. All ratios are winsorized at the 2% level. Variable definitions are provided in the appendix. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

	PE				Control				Difference	
Variable	N	Mean	Median	SD	N	Mean	Median	SD	Mean	Median
Age	827	22	18	17.12	2,827	22	17	19.56.10	0	1
Employment	811	450	148	1,221	2,502	401	129	885	49*	19
Total assets (£000)	827	101,183	20,578	556,172	2,827	90,673	19,130	351,572	10,510	1,448
Total debt (£000)	827	45,138	4,500	288,013	2,827	39,811	4,287	193,033	5,327	213
Total cash (£000)	813	5,937	1,791	17,378	2,630	6,792	1,601	31,007	-855	190
Sales (£000)	821	67,165	23,391	175,319	2,755	62,196	22,006	235,074	4,969	1,385
EBITDA (£000)	825	7,696	2,484	30,812	2,812	6,138	2,155	15,975	1,558	329
Return on assets	827	0.05	0.05	0.15	2,827	0.05	0.06	0.15	0.00	-0.01
EBITDA margin	820	0.12	0.10	0.17	2,753	0.13	0.10	0.19	-0.01	0.00
Debt/assets	827	0.32	0.24	0.32	2,827	0.33	0.24	0.34	-0.01	0.00
Cash/assets	827	0.12	0.07	0.13	2,827	0.13	0.07	0.16	-0.01	0.00
Debt/EBITDA	825	2.47	0.95	6.32	2,810	2.67	0.94	6.81	-0.20	0.01
Working capital/assets	827	0.20	0.21	0.34	2,826	0.18	0.19	0.36	0.02	0.02
Labour productivity	707	62.5	50.0	46.5	2,025	67.1	50.9	58.3	-4.6	-0.9
Total factor productivity	573	5.27	5.27	0.58	1,544	5.24	5.22	0.71	0.03	0.05*
Investment	799	0.25	0.20	0.37	2,494	0.22	0.18	0.35	0.03	0.02
Equity issuance	818	-0.02	0.00	0.10	2,768	-0.03	0.00	0.11	0.01	0.00
Debt issuance	816	0.06	0.04	0.19	2,763	0.05	0.02	0.21	0.01	0.02

Table 2: Matched treated and control pre-pandemic growth rates

The table reports one year growth rates for the pre-pandemic year (2019) across PE-backed companies and control firms. *PE-backed* refers to all PE-backed companies; *Control* refers to a sample of non-PE-backed firms, matched on their two-digit SIC code, total assets, ROA (net income/total assets), and leverage (total debt/total assets) within a 30% bracket in the pre-pandemic year. All ratios and growth rates are winsorized at the 2% level. Variable definitions are provided in the appendix. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

	PE				Control				Difference	
Variable	N	Mean	Median	SD	N	Mean	Median	SD	Mean	Median
Employment	797	0.09	0.07	0.22	2,429	0.07	0.06	0.18	0.02	0.01
Total assets	818	0.18	0.11	0.41	2,769	0.16	0.09	0.44	0.02	0.02
Total debt	723	0.19	0.06	0.91	2,472	0.15	0.05	0.88	0.04	0.01
Total cash	803	0.37	0.04	1.10	2,569	0.39	0.05	1.14	-0.02	-0.01
Sales	780	0.13	0.07	0.30	2,593	0.12	0.05	0.34	0.01	0.02*
EBITDA	783	0.01	0.03	1.04	2,651	0.03	0.04	1.04	-0.02	-0.01
Return on assets	785	-0.08	-0.05	1.44	2,665	-0.06	-0.04	1.46	-0.02	-0.01
EBITDA margin	778	-0.10	-0.05	0.87	2,589	-0.07	-0.03	0.84	-0.03	-0.02
Debt/assets	723	0.04	0.00	0.35	2,472	0.04	-0.01	0.41	0.00	0.01
Cash/assets	803	0.17	0.00	0.88	2,569	0.19	0.01	0.91	-0.02	-0.01
Debt/EBITDA	704	0.14	-0.11	1.64	2,382	0.10	-0.13	1.49	0.04	0.02
Working capital/assets	817	0.02	0.01	0.82	2,763	-0.01	0.00	0.77	0.03	0.01
Labour productivity	650	0.02	0.00	0.28	1,865	0.04	0.01	0.26	-0.02	-0.01
Total factor productivity	527	0.01	0.01	0.18	1,400	0.02	0.02	0.17	-0.01	-0.01
Investment	782	0.27	-0.13	2.08	2,351	0.24	-0.15	2.36	0.03	0.02
Equity issuance	655	-0.23	-0.11	2.21	2,331	-0.38	-0.04	2.38	0.15	-0.07
Debt issuance	805	-0.81	-0.76	1.72	2,636	-0.79	-0.82	1.69	-0.02	0.06

Table 3: Firm performance during the pandemic

We estimate all specifications using a difference-in-differences estimator. In panel A, we present the results from our baseline difference-in-differences model, equation 2. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. In panel B, we show the estimates from regression equation 3, where we estimate a different β_k for each year between 2017 and 2021, using the pre-pandemic year, 2019, as the reference year. Standard errors, reported in the parentheses, are clustered at the firm-level. Even-numbered columns include firms controls which are taken in the pre-pandemic year, 2019, and are interacted with the Post dummy. These include firm age, size, leverage (debt divided by assets), return on assets, cash holdings scaled by assets, and sales growth. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

	Sales		Total Assets		Employment		Earnings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Baseline difference-in-differences								
PE*Post	0.056***	0.049**	0.107***	0.105***	0.077***	0.073***	0.061**	0.058**
	(0.021)	(0.022)	(0.027)	(0.026)	(0.022)	(0.022)	(0.022)	(0.025)
Panel B: Year-by-year effects								
PE*2017	-0.037*	-0.035	0.002	0.005	-0.031	-0.029	-0.004	-0.006
	(0.022)	(0.024)	(0.033)	(0.033)	(0.024)	(0.025)	(0.018)	(0.019)
PE*2018	-0.030	-0.26	-0.051	-0.050	-0.021	-0.022	-0.024	-0.025
	(0.025)	(0.025)	(0.032)	(0.033)	(0.019)	(0.019)	(0.020)	(0.021)
PE*2020	0.034***	0.032**	0.075***	0.075***	0.033**	0.031**	0.044**	0.042**
	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.019)	(0.020)
PE*2021	0.059***	0.055***	0.108***	0.106***	0.068***	0.064**	0.062**	0.060**
	(0.014)	(0.015)	(0.028)	(0.027)	(0.028)	(0.028)	(0.033)	(0.034)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	16,511	16,511	17,430	17,430	15,739	15,739	14,505	14,505

Table 4: Alternative matching techniques

We estimate all specifications using a difference-in-differences estimator. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. Standard errors, reported in the parentheses, are clustered at the firm-level. All specifications include firms controls which are taken in the pre-pandemic year, 2019, and are interacted with the Post dummy. These include firm age, size, leverage (debt divided by assets), return on assets, cash holdings scaled by assets, and sales growth. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

Panel A: Matching on industry, and total assets, ROA, and leverage within 50% brackets

	Sales	Total Assets	Employment	Earnings
PE*Post	0.056***	0.111***	0.071***	0.079**
	(0.019)	(0.024)	(0.018)	(0.043)
Observations	21,045	22,245	20,164	16,863

Panel B: Matching on industry, and total assets, ROA, and leverage within 20% brackets

	Sales	Total Assets	Employment	Earnings
PE*Post	0.059***	0.086**	0.054**	0.079**
	(0.021)	(0.031)	(0.024)	(0.028)
Observations	8,903	9,426	8,514	7,908

Panel C: Matching on industry, and total assets and ROA within 30% brackets

	Sales	Total Assets	Employment	Earnings
PE*Post	0.086***	0.151***	0.089***	0.073**
	(0.021)	(0.020)	(0.017)	(0.031)
Observations	27,941	30,185	26,083	23,547

Panel D: Matching on industry, and total assets, cash holdings and sales growth within 30% brackets

	Sales	Total Assets	Employment	Earnings
PE*Post	0.173***	0.175***	0.136***	0.161*
	(0.051)	(0.057)	(0.047)	(0.079)
Observations	1,868	1,913	1,792	1,636
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes

Table 5: Firm vulnerability

We estimate all specifications using a difference-in-differences estimator. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. In column 1, *Vulnerable* is a dummy variable equal to one if the one-year sales growth in 2019 is in the bottom quartile of the distribution, and in column 2 it is equal to one if the two-year sales growth in 2019 is in the bottom quartile. Columns 3 and 4 do likewise for EBITDA growth rates, while columns 5 and 6 do similar for the growth in the number of employees. In column 7, *Vulnerable* is equal to one if the firm is in the lowest quartile of cash holdings, as measured by the ratio of cash to total assets, in 2019. In column 8 it equals one where the firm is in the top quartile of the ratio of short term debt to total assets, while in column 9 it equals one where the firm is in the top quartile of labor intensity, as measured by the ratio of employees to sales. Standard errors, reported in the parentheses, are clustered at the firm-level. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

					Panel A: Sales				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vulnerable = 1									
PE*Post	0.014	0.062	0.027*	-0.025	0.052	0.041	0.006	0.011	0.063
	(0.026)	(0.068)	(0.016)	(0.054)	(0.053)	(0.048)	(0.062)	(0.070)	(0.048)
Observations	3,901	3,713	3,459	3,324	3,729	3,650	3,824	4,121	3,785
Vulnerable = 0									
PE*Post	0.103**	0.069**	0.079**	0.083***	0.072***	0.075**	0.065***	0.067***	0.048***
	(0.039)	(0.024)	(0.022)	(0.024)	(0.025)	(0.026)	(0.025)	(0.024)	(0.027)
Observations	11,968	11,330	10,744	10,215	11,115	10,872	12,687	12,390	11,346
					Panel B: Total Assets				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vulnerable = 1									
PE*Post	0.042	-0.014	0.049*	-0.018	0.035	0.033	0.011	0.052	0.080
	(0.055)	(0.059)	(0.031)	(0.054)	(0.045)	(0.046)	(0.059)	(0.062)	(0.064)
Observations	4,002	3,765	3,597	3,410	3,835	3,752	4,053	4,439	3,938
Vulnerable = 0									
PE*Post	0.104***	0.118***	0.089***	0.149***	0.131***	0.129***	0.121***	0.130***	0.115***
	(0.031)	(0.030)	(0.032)	(0.031)	(0.032)	(0.031)	(0.029)	(0.031)	(0.032)
Observations	12,172	11,421	10,933	10,332	11,654	11,358	13,377	12,991	11,728

Continued on the next page

Panel C: Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vulnerable = 1									
PE*Post	0.056	0.036	0.014	0.012	0.068*	0.006	0.019	0.032	0.086
	(0.046)	(0.061)	(0.039)	(0.045)	(0.044)	(0.053)	(0.058)	(0.052)	(0.059)
Observations	3,422	3,210	3,209	3,025	3,790	3,716	3,507	3,941	3,896
Vulnerable = 0									
PE*Post	0.066***	0.073***	0.105***	0.109***	0.065***	0.092***	0.094***	0.090***	0.074***
	(0.024)	(0.022)	(0.024)	(0.023)	(0.022)	(0.021)	(0.022)	(0.024)	(0.022)
Observations	11,374	10,703	10,058	9,541	11,536	11,268	12,232	11,798	11,580

Panel D: Earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vulnerable = 1									
PE*Post	0.023	-0.022	0.016	-0.092	-0.038	-0.019	-0.035	0.016	0.027
	(0.019)	(0.049)	(0.069)	(0.079)	(0.083)	(0.092)	(0.083)	(0.091)	(0.084)
Observations	3,233	3,055	2,922	2,716	3,091	3,057	3,321	3,627	3,098
Vulnerable = 0									
PE*Post	0.050**	0.057**	0.048*	0.068**	0.043**	0.041**	0.053**	0.062**	0.057*
	(0.022)	(0.019)	(0.020)	(0.026)	(0.014)	(0.017)	(0.020)	(0.017)	(0.036)
Observations	10,641	10,130	10,377	9,891	9,955	9,732	11,184	10,878	10,134
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Industry exposure

We estimate all specifications using a difference-in-differences estimator. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. In column one, *Exposed* is a dummy variable equal to one if the firm operates in a high-contact industry in the UK, as defined by the Office for national Statistics (ONS) (see here). In columns 2, *Exposed* equals one if the firm operates in one of the top five most affected industries in terms of the change in sales and employment from 2020Q2 to 2021Q1 based on the UK Decision Maker Panel (DMP) data (see here). In column 3, *Exposed* firms belong to industries in the top quartile of the affected share distribution, where the affected share is as defined by Koren and Pető (2020). In column 4, *Exposed* equals one if the firm operates in an industry defined as exposed in the manual classification by Fahlenbrach et al. (2021). Standard errors, reported in the parentheses, are clustered at the firm-level. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

	Panel A: Sales			
	(1)	(2)	(3)	(4)
Exposed = 1				
PE*Post	0.037	-0.016	0.025*	-0.027
	(0.047)	(0.059)	(0.015)	(0.106)
Observations	3,254	5,066	1,652	1,879
Exposed = 0				
PE*Post	0.061**	0.078***	0.045**	0.073***
	(0.027)	(0.022)	(0.024)	(0.022)
Observations	13,257	11,445	14,859	14,632
	Panel B: Total Assets			
	(1)	(2)	(3)	(4)
Exposed = 1				
PE*Post	0.059	0.010	0.079*	0.052
	(0.052)	(0.047)	(0.056)	(0.054)
Observations	3,377	5,353	1,708	1,943
Exposed = 0				
PE*Post	0.119***	0.146***	0.089***	0.115***
	(0.032)	(0.033)	(0.029)	(0.030)
Observations	14,053	12,077	15,722	15,487

	Panel C: Employment			
	(1)	(2)	(3)	(4)
Exposed = 1				
PE*Post	0.062*	0.057*	0.064	0.059
	(0.042)	(0.038)	(0.082)	(0.053)
Observations	3,263	4,899	1,282	1,875
Exposed = 0				
PE*Post	0.131***	0.125***	0.149**	0.124**
	(0.035)	(0.034)	(0.065)	(0.048)
Observations	12,476	10,840	14,457	13,864
	Panel C: Earnings			
	(1)	(2)	(3)	(4)
Exposed = 1				
PE*Post	-0.153	-0.070	0.029	-0.076
	(0.094)	(0.073)	(0.041)	(0.127)
Observations	2,889	4,422	1,549	1,607
Exposed = 0				
PE*Post	0.065**	0.052**	0.068**	0.047*
	(0.023)	(0.019)	(0.029)	(0.028)
Observations	11,616	10,083	12,956	12,898
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Table 7: Channels of growth and value added

We estimate all specifications using a difference-in-differences estimator. In panel A, we present the results from our baseline difference-in-differences model, equation 2. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. In panel B, we show the estimates from regression equation 3, where we estimate a different β_k for each year between 2017 and 2021, using the pre-pandemic year, 2019, as the reference year. Standard errors, reported in the parentheses, are clustered at the firm-level. Even-numbered columns include firms controls which are taken in the pre-pandemic year, 2019, and are interacted with the Post dummy. These include firm age, size, leverage (debt divided by assets), return on assets, cash holdings scaled by assets, and sales growth. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

	Operational engineering			Financial engineering	
	Int. inputs/sales	CCC	Investment	Equity issuance	Debt issuance
	(1)	(2)	(3)	(4)	(5)
Panel A: Baseline difference-in-differences					
PE*Post	-0.014	-0.063*	0.021*	0.009**	0.018***
	(0.009)	(0.038)	(0.013)	(0.004)	(0.006)
Panel B: Year-by-year effects					
PE*2017	-0.001	-0.024	-0.005	-0.004	-0.003
	(0.008)	(0.027)	(0.009)	(0.005)	(0.004)
PE*2018	0.003	0.003	-0.002	-0.001	-0.008
	(0.009)	(0.022)	(0.010)	(0.004)	(0.010)
PE*2020	-0.017*	-0.006	0.019*	0.010**	0.016**
	(0.009)	(0.019)	(0.010)	(0.003)	(0.006)
PE*2021	-0.005	-0.060**	0.014	0.004**	0.006*
	(0.006)	(0.025)	(0.010)	(0.001)	(0.003)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Observations	13,987	7,077	15,524	16,549	17,427

Table 8: Firm vulnerability: financial engineering

We estimate all specifications using a difference-in-differences estimator. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. In column 1, *Vulnerable* is a dummy variable equal to one if the one-year sales growth in 2019 is in the bottom quartile of the distribution, and in column 2 it is equal to one if the two-year sales growth in 2019 is in the bottom quartile. Columns 3 and 4 do likewise for EBITDA growth rates, while columns 5 and 6 do similar for the growth in the number of employees. In column 7, *Vulnerable* is equal to one if the firm is in the lowest quartile of cash holdings, as measured by the ratio of cash to total assets, in 2019. In column 8 it equals one where the firm is in the top quartile of the ratio of short term debt to total assets, while in column 9 it equals one where the firm is in the top quartile of labor intensity, as measured by the ratio of employees to sales. Standard errors, reported in the parentheses, are clustered at the firm-level. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

				Panel A: Equity issuance					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vulnerable = 1									
PE*Post	-0.006	-0.002	-0.003	-0.002	0.004	0.001	-0.010	-0.001	0.014*
	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.007)	(0.008)
Observations	3,874	3,676	3,458	3,319	3,709	3,647	3,847	4,122	3,704
Vulnerable = 0									
PE*Post	0.012***	0.013***	0.011**	0.011**	0.012**	0.013***	0.014***	0.011**	0.012**
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Observations	11,766	11,190	10,618	10,140	11,090	10,868	11,748	12,426	11,191
				Panel B: Debt issuance					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vulnerable = 1									
PE*Post	0.008	-0.001	0.025	0.007	0.009	0.011	0.022	0.026*	0.016
	(0.016)	(0.017)	(0.017)	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.014)
Observations	4,031	3,765	3,597	3,410	3,835	3,751	4,053	4,313	3,937
Vulnerable = 0									
PE*Post	0.018**	0.019***	0.013**	0.017**	0.015**	0.013*	0.014**	0.017***	0.015**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)
Observations	12,143	11,421	10,933	10,332	11,653	11,358	12,376	13,112	11,728
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: Industry exposure: financial engineering

We estimate all specifications using a difference-in-differences estimator. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. In column one, *Exposed* is a dummy variable equal to one if the firm operates in a high-contact industry in the UK, as defined by the Office for national Statistics (ONS) (see here). In columns 2, *Exposed* equals one if the firm operates in one of the top five most affected industries in terms of the change in sales and employment from 2020Q2 to 2021Q1 based on the UK Decision Maker Panel (DMP) data (see here). In column 3, *Exposed* firms belong to industries in the top quartile of the affected share distribution, where the affected share is as defined by Koren and Pető (2020). In column 4, *Exposed* equals one if the firm operates in an industry defined as exposed in the manual classification by Fahlenbrach et al. (2021). Standard errors, reported in the parentheses, are clustered at the firm-level. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

	Panel A: Equity issuance			
	(1)	(2)	(3)	(4)
Exposed = 1				
PE*Post	0.010	0.007	-0.011	-0.005
	(0.009)	(0.008)	(0.015)	(0.012)
Observations	3,211	5,075	1,630	1,861
Exposed = 0				
PE*Post	0.009**	0.008*	0.010**	0.010**
	(0.003)	(0.004)	(0.004)	(0.004)
Observations	13,338	11,474	14,919	14,688
	Panel B: Debt issuance			
	(1)	(2)	(3)	(4)
Exposed = 1				
PE*Post	0.028**	0.010	-0.004	0.025*
	(0.014)	(0.013)	(0.020)	(0.016)
Observations	3,377	5,353	1,708	1,942
Exposed = 0				
PE*Post	0.016**	0.021***	0.020***	0.016**
	(0.007)	(0.008)	(0.007)	(0.007)
Observations	14,050	12,074	15,719	15,485
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Table 10: COVID-19 loan statistics

The table reports the loan terms on the COVID-19 loans granted to PE-backed firms, matched control firms, and the entire population of UK limited companies which were granted a loan through the Loan Guarantee Scheme. Panel A shows statistics on loan activity, and panel B shows statistics on loan terms Column 3 in panel B reports the p-value from a t-test of means between the PE-backed and control group samples. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level..

Panel A: Loan activity

	PE-backed		Control		All limited companies	
	(N)	(%)	(N)	(%)	(N)	(%)
Obtained a loan	198	23.9	436	15.4	1,106,861	na
Repayment rate	74	37.4	161	36.9	156,660	14.2
Default rate	6	3.0	4	0.9	189,624	17.1

Panel B: Loan terms

	PE-backed mean	Control mean	Difference in means	All limited companies mean
	(1)	(2)	(3)	(4)
Loan amount (£000)	1,795	1,083	0.01	57
Loan term (months)	57	59	0.24	82
Interest Rate (%)	4.57	4.51	0.25	2.78

Table 11: The probability of financial distress during the pandemic

We estimate all specifications using a difference-in-differences estimator. PE_i is a dummy variable that equals one for PE-backed companies, and zero for the control group. $Post_t$ is a dummy variable that equals one for observations during the pandemic period of 2020 to 2021, and 0 in the pre-pandemic years of 2017 to 2019. The dependent variable is a dummy variable equal to one if a firm files for insolvency in that year, and zero otherwise. Panel A consider nine measures of firm vulnerability. In column 1, *Vulnerable* is a dummy variable equal to one if the one-year sales growth in 2019 is in the bottom quartile of the distribution, and in column 2 it is equal to one if the two-year sales growth in 2019 is in the bottom quartile. Columns 3 to 4, and 5 to 6 do likewise for EBITDA and employment growth rates. In column 7, *Vulnerable* is equal to one if the firm is in the lowest quartile of cash-to-assets in 2019. In column 8 it equals one where the firm is in the top quartile of the ratio of short term debt to total assets, while in column 9 it equals one where the firm is in the top quartile of labor intensity, as measured by the ratio of employees to sales. Panel B considers four measures of industry exposure to the pandemic. In column one, *Exposed* is a dummy variable equal to one if the firm operates in a high-contact industry in the UK, as defined by the Office for national Statistics (ONS) (see here). In columns 2, *Exposed* equals one if the firm operates in one of the top five most affected industries in terms of the change in sales and employment from 2020Q2 to 2021Q1 based on the UK Decision Maker Panel (DMP) data (see here). In column 3, *Exposed* firms belong to industries in the top quartile of the affected share distribution, where the affected share is as defined by Koren and Pető (2020). In column 4, *Exposed* equals one if the firm operates in an industry defined as exposed in the manual classification by Fahlenbrach et al. (2021). All specifications include firms controls which are taken in the pre-pandemic year, 2019, and are interacted with the Post dummy. These include firm age, size, leverage (debt divided by assets), return on assets, cash holdings scaled by assets, and sales growth. Standard errors, reported in the parentheses, are clustered at the firm-level. *** denotes statistical significance at the 1% level, ** denotes the 5% level, and * denotes the 10% level.

Panel A: Firm vulnerability									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Vulnerable = 1									
PE*Post	0.009**	0.010**	0.009**	0.016***	0.015**	0.013**	0.010**	0.018***	0.020***
	(0.003)	(0.003)	(0.004)	(0.006)	(0.007)	(0.006)	(0.004)	(0.006)	(0.007)
Observations	4,250	3,940	3,780	3,565	4,035	3,932	4,290	4,567	4,123
Vulnerable = 0									
PE*Post	0.007	0.008*	0.004*	0.003	0.004*	0.005*	0.006*	0.004	0.002
	(0.005)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Observations	12,615	11,825	11,300	10,660	12,092	11,740	12,290	13,698	12,275
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Industry exposure				
	(1)	(2)	(3)	(4)
Exposed = 1				
PE*Post	0.031***	0.024***	0.041***	0.032***
	(0.010)	(0.007)	(0.015)	(0.011)
Observations	3,515	5,615	1,760	2,488
Exposed = 0				
PE*Post	0.002	0.002	0.003	0.002
	(0.003)	(0.002)	(0.002)	(0.003)
Observations	14,753	12,653	16,508	15,780
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes

Table 12: Insolvencies during the COVID-19 pandemic

The below table shows the number and types of insolvencies in our sample of PE-backed and matched non-PE-backed firms.

	PE	Control
Panel A: Total insolvencies		
Total firms	828	2,825
Insolvencies during the pandemic	16	15
Insolvency %	1.9%	0.5%
Panel B: Firm vulnerability definitions		
Bottom quartile one-year sales growth	5	3
Bottom quartile two-year sales growth	5	3
Bottom quartile one-year EBITDA growth	3	4
Bottom quartile two-year EBITDA growth	6	2
Bottom quartile one-year employment growth	7	5
Bottom quartile two-year employment growth	6	5
Bottom quartile cash holdings	6	3
Top quartile short-term debt/assets	8	2
Top quartile labour intensity	10	3
Panel C: Industry exposure definitions		
ONS high-contact industries	11	4
Bloom et al. (2020) DMP most affected	12	5
Koren and Pető (2020) affected share top quartile	8	1
Fahlenbrach et al. (2021) manual classification	9	2
Panel D: Insolvency type		
Administration	7 (44%)	10 (67%)
Company Voluntary Arrangement	9 (56%)	1 (7%)
Liquidation	0 (0%)	4 (27%)

FRANK HAWKINS KENAN INSTITUTE OF PRIVATE ENTERPRISE REPORT

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Have Private Equity Returns Really Declined?

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ABSTRACT

In a recent paper, "Demystifying Illiquid Assets – Expected Returns for Private Equity," Ilmanen, Chandra and McQuinn (of AQR) give a perspective on the past, present, and expected future performance of private equity. They conclude that "private equity does not seem to offer as attractive a net-of-fee return edge over public market counterparts as it did 15-20 years ago from either a historical or forward-looking perspective." This analysis provides our perspective based on more recent and, we think, more reliable data and performance measures – the historical perspective is more positive than Ilmanen et al. portray.

INTRODUCTION

Over the past 20 years, as new and higher quality datasets have emerged, there has been a growing body of research on the performance of private equity funds. This research has studied the returns of the asset class in absolute terms and relative to public equity, its risk-adjusted returns, attempts to replicate returns with public equities, as well as the persistence of returns. Conclusions on the performance of private equity have differed by data source, by methodology and benchmark, and by author. In a recent paper, "Demystifying Illiquid Assets – Expected Returns for Private Equity," Ilmanen, Chandra and McQuinn (of AQR) give their perspective on the past, present, and expected future performance of private equity. They conclude that "private equity does not seem to offer as attractive a net-of-fee return edge over public market counterparts as it did 15-20 years ago from either a historical or forward-looking perspective." They also conjecture that the greater attraction to private equity is "investors' preference for the return-smoothing properties of illiquid assets in general."

In this analysis, we use high quality data from Burgiss to provide our perspective on these questions using up-to-date numbers on the historical absolute and relative returns of private equity. We then discuss the implications of different variables for future expected returns.

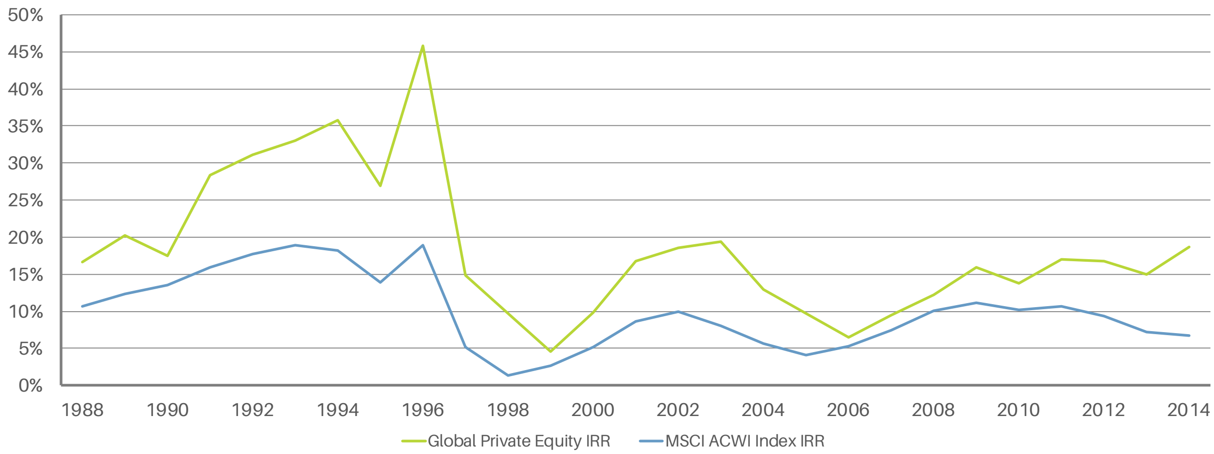
Exhibit 1A shows the annualized returns by vintage year and Exhibit 1B shows the Kaplan-Schoar (2005) public market equivalents (PMEs) by vintage year of global private equity funds against the contemporaneous total returns of the MSCI ACWI index. The exhibits use the most recent data from Burgiss. Burgiss sources its data directly from institutional limited partners (LPs), so the data are up to date and relatively free of selection bias. In these exhibits, we include in private equity the categories of buyout, venture, growth, and generalist private equity funds. In the rest of the paper, we focus on the largest category, U.S. buyout funds.

As can be seen, private equity returns have been higher than the MSCI in every single vintage year. The PMEs are greater than one for every single vintage year.[1] While one can debate, whether the MSCI ACWI is an appropriate benchmark for private equity, it is a reasonable place to start for the average institutional investor's public equity exposure.

While excess returns and PMEs have declined post-2005, they have still exceeded the returns to public markets. It seems likely that these persistent excess returns are the main reason for the past and current popularity of private equity. While it is probable that investors do not mind any perceived return smoothing that comes with illiquid assets, it seems unlikely that smoothing is a first order source of demand given the historical performance.

[1] Returns of private and public equities for less mature vintage years are still subject to change as portfolio investments are exited and valuation estimates are converted to cash returns.

Exhibit 1A: IRRs of Global Private Equity and the MSCI ACWI by Vintage Year



Source: Burgiss Private iQ, as of September 30, 2018. Global Private Equity includes buyout, venture, growth, and generalist private equity funds. Contemporaneous IRRs of the MSCI ACWI are derived via Direct Alphas as per Gredil, Griffiths, and Stucke (2014).

Exhibit 1B: PMEs of Global Private Equity against the MSCI ACWI by Vintage Year

Source: Burgiss Private iQ, as of September 30, 2018. Global Private Equity includes buyout, venture, growth, and generalist private equity funds. PMEs as per Kaplan and Schoar (2005).

THE PERFORMANCE OF U.S. BUYOUT FUNDS

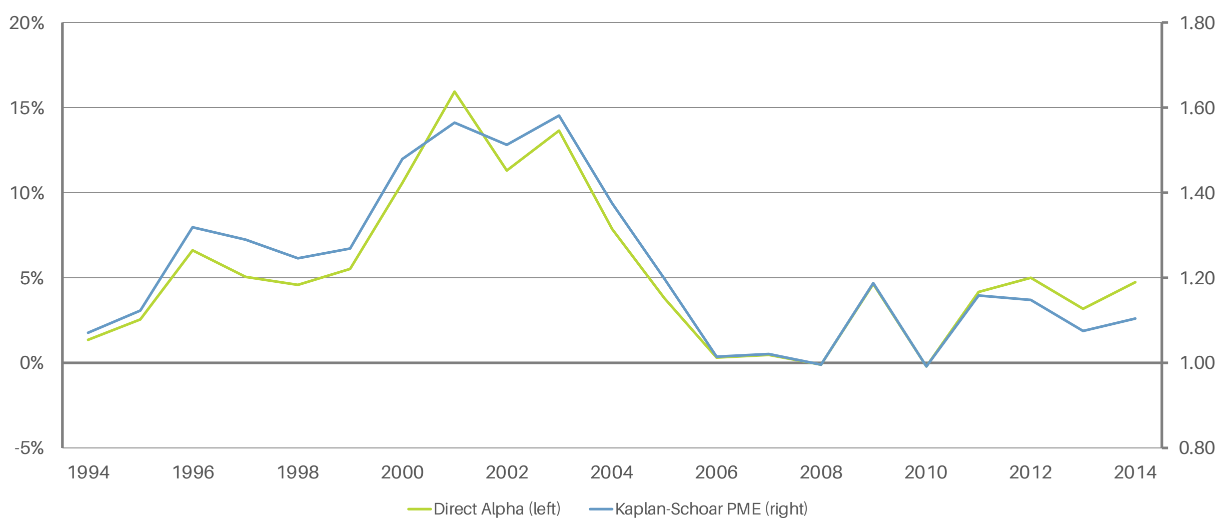
Ilmanen et al. (2019) focus on U.S. buyout funds, which represent the largest part of global private equity. Based on time-weighted returns back to 1986, they estimate an excess return over the S&P 500 of 2.3%.

This number appears low.[2] Using the latest fund cash flow data from Burgiss as of the third quarter of 2018, we calculate an average Direct Alpha of 4.8% and an average PME of 1.22 for 1986 to 2014 vintage years.[3] Accounting for the different amounts of capital in each vintage year leaves an excess return of 3.5% or 1.15. In other words, U.S. buyouts have historically outperformed the S&P 500 by a fairly wide margin.

Ilmanen et al. reference research by L'Her et al. (2016) who found that U.S. buyout fund returns for 2009 to 2014 vintage years were roughly equal to those of the S&P 500. As it turns out, this finding was probably driven in part by the immature nature of those more recent vintage years.[4] As of the third quarter of 2018, funds from 2009 to 2014 have generated an average Direct Alpha of 3.9% and a PME of 1.11. This is quite healthy performance and in line with expectations of returns that are 2% to 3% above public markets.

Exhibit 2 shows Direct Alphas and PMEs back to 1994.[5] Capital-weighted average excess returns over this period are 3.6% and the average PME is 1.15. The highest excess returns are for 2000 to 2004 vintages. The lowest are for the 2006 to 2008 vintages. The 2009 to 2014 vintages look most like the vintages of the mid- to late-1990s, albeit slightly lower. Note that the funds for more recent vintage years are not fully realized. PMEs will increase if funds continue to generate returns in excess of the S&P. (Of course, they will decrease if the reverse is true.)

Exhibit 2: Direct Alphas and PMEs of U.S. Buyout Funds against the S&P 500 by Vintage Year



Source: Burgiss Private iQ, as of September 30, 2018.

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[2] See Harris, Jenkinson, and Kaplan (2014), and Higson and Stucke (2012).

[3] See Kaplan and Schoar (2005), and Gredil, Griffiths, and Stucke (2014) for a derivation of Direct Alpha.

[4] In their paper, the authors acknowledge the preliminary nature of the returns for more recent vintage years.

[5] Results for earlier vintage years are more volatile due to a much smaller number of funds in each vintage year. Capital-weighted average Direct Alphas and PMEs across 1980 to 1993 vintage years are 4.1% and 1.19, respectively.

SMART-BETA FACTORS AND U.S. BUYOUT FUNDS

It is no secret that buyouts use more leverage than and are smaller than the typical company in the S&P 500. There also is a perception that buyouts are more like value investments than growth investments. The question, then, is what is the appropriate benchmark to use for buyout fund investments.

One possibility is that the S&P 500 is just fine. Sorensen and Jagannathan (2015) show that this is a reasonable assumption if investors have log utility. And, of course, the primary objective of institutional investors is to generate returns in excess of their public equity portfolio.

An alternative is to try to adjust for leverage and the level of market risk (i.e., the CAPM beta). Ilmanen et al. assume that the market risk inherent in a portfolio of U.S. buyout funds is equivalent to having a beta of 1.2 and adjust accordingly. Because buyout funds are illiquid, it is difficult to estimate betas directly. The academic literature on this is inconclusive with betas typically ranging from 1.0 to 1.3.[6] In general, using a beta above 1.0 has the effect of lowering the PMEs and Direct Alphas of buyout funds because the stock market goes up on average. We note that, empirically, beta does not do a good job of explaining realized returns, i.e., a portfolio of higher beta public stocks does not perform much differently from a portfolio of low beta stocks. Evidence for this comes from Frazzini and Pedersen (2014). It is further not clear, to what extent risk measures based on volatility and covariance are particularly meaningful for illiquid investments, where cash flows are at the discretion of the fund manager.

Another alternative is to adjust for size. Portfolios of smaller capitalization stocks perform differently over different periods than portfolios of larger stocks. And buyout investments tend to be in companies that are more like smaller capitalization stocks.

A final alternative is to adjust for value as opposed to growth. Again, portfolios of value stocks and growth stocks perform differently over different periods. As with beta, it is difficult to know exactly what value adjustment to make for buyout funds.[7] While it is unclear which adjustments make the most sense, if any, we show the effects of making different adjustments. Exhibit 3 presents Direct Alphas using the S&P 500 index, the Russell 2000 and the Russell 2000 Value indices.

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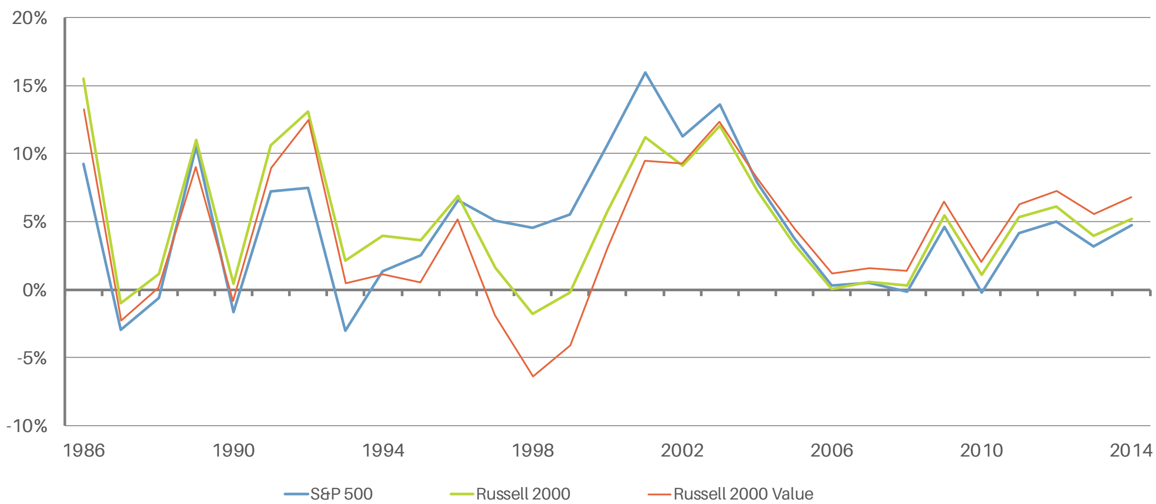
[6] See Kaplan and Sensoy (2015) and Korteweg (2018) for a survey of this and other evidence.

[7] Buyouts are priced at entry and exit because these valuations form the basis for eventual investment returns. For buyouts of privately-held companies, the valuation process usually starts with earnings multiples of a group of industry- and size-matched public peers as well as recent private equity or M&A transactions. A potential acquirer then determines a maximum bid based on its investment thesis (that includes operational improvements and strategic adjustments), the debt used to fund the transaction, the estimated cash flows and the valuation of the company at final exit. For buyouts of publicly-listed companies, the acquirer typically has to pay a premium of 20% to 40% to the selling shareholders. In summary, while buyout valuations are informed by recent valuations in the public market, those are only one part of the overall equation.

Exhibit 3: Direct Alphas – S&P 500 vs. Russell 2000 and Russell 2000 Value by Vintage Year



Source: Burgiss Private IQ, as of September 30, 2018.

If U.S. buyouts were indeed subject to a size and value premium in public equity markets, this should be accounted for when using the Russell 2000 indices as benchmarks. Since the 2008 vintage year, excess returns of U.S. buyout funds have been consistently higher against the Russell 2000 index than against the S&P 500. Since 2004, excess returns have been consistently higher against the Russell 2000 Value index. The advantage of small-cap value stocks over the S&P 500 is concentrated in the 1997 to 2001 vintages. This fact is typically ignored by research that attempts to replicate long-term buyout returns with small-cap value stocks. (That research also ignores potential capacity constraints in public markets – the market capitalization of the entire Russell 2000 Value index of about $1.5 trillion compares to uncalled capital by U.S. buyout funds of about $500 billion.)[8]

We also estimate the effects of assuming a beta of 1.2 using the S&P 500, the Russell 2000, and the Russell 2000 Value indices. Exhibit 4 presents the Direct Alphas and PMEs over different time periods. What is clear from these calculations is that buyout performance has exceeded the leveraged indices for the vintages from 1986 to 2014 and over the two more recent different sub-periods.

It is worth pointing out that outperformance has been the greatest against the Russell 2000 Value index for 2009 to 2014 vintage years and, inversely, it has been at the lower end against the Russell 2000 Value index in earlier periods, especially in the 1990s. This observation raises questions as to whether buyouts are at all subject to a small-cap and value premium as historically observed for public equities, particularly given the increased size and competitiveness of the buyout industry over the past 10-15 years.[9]

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[8] See Chingono and Rasmussen (2014), and Stafford (2017).

[9] Using a benchmark with a greater size and value tilt, such as customized Fama-French portfolios of small-cap value stocks also generates positive PMEs and Direct Alphas for post-2000 vintages.

Exhibit 4: Direct Alphas and PMEs against a simulated Beta of 1.2

	Direct Alpha				KS-PME			
From	1986	2000	2000	2009	1986	2000	2000	2009
To	2014	2014	2008	2014	2014	2014	2008	2014
S&P 500	2.1%	1.9%	2.0%	1.3%	1.09	1.07	1.09	1.04
Russell 2000	0.8%	1.0%	0.6%	2.4%	1.03	1.04	1.03	1.07
Russell 2000 Value	0.9%	1.8%	1.2%	3.9%	1.04	1.07	1.06	1.11

Source: Burgiss Private IQ, as of September 30, 2018. Direct Alphas and PMEs are calculated based on capital-weighted, vintage year concurrent cash flows.

EXPECTED OR FUTURE RETURNS FOR U.S. BUYOUT FUNDS?

Ilmanen et al. conclude their paper by attempting to estimate expected returns for buyout funds going forward. They conclude that the expected buyout fund returns relative to public markets are likely to be lower than the past – on the order of 80 basis points. They base this on several considerations.

First, they note that buyout fund returns appear to have declined post-2005 to almost equal public market returns. They also point out that this coincided with private market purchase multiples have been in line with public market multiples since 2006 (suggesting that there is no longer a valuation discount to buyouts). As we showed above, the conclusion that buyout fund returns equal public market returns for post-2008 vintages just does not hold using the most recent data. As an aside, by the same logic, one might conclude that the value premium is also a figment of the past as it has performed quite poorly the last decade.

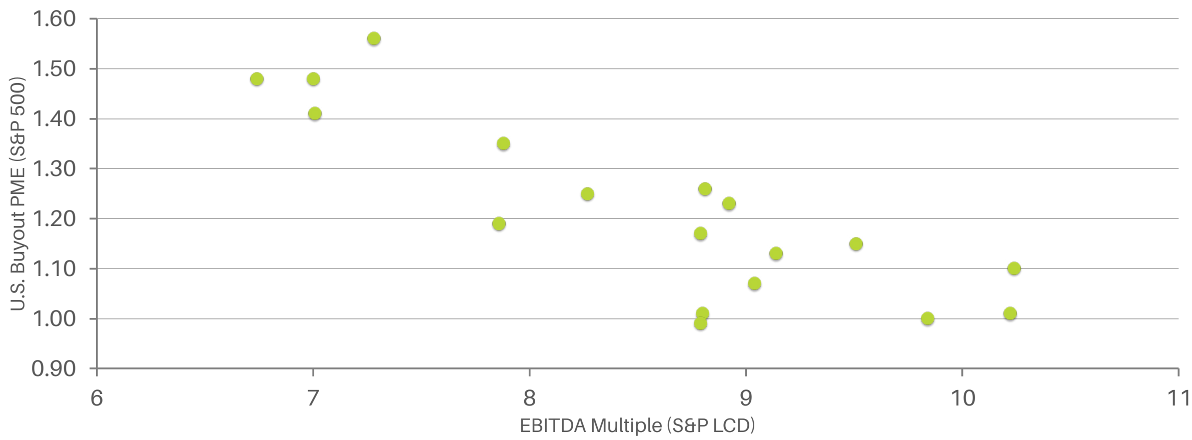
Second, they point out that buyout fundraising has been substantial over the last five years and that high fundraising has been associated with lower subsequent returns. We agree that this is, indeed, a concern. There is a negative correlation historically between PMEs and buyout fundraising. However, that correlation is to some extent backward looking. The correlation has been smaller in real time. See Brown et al. (2018).

Finally, they point out that buyout earnings yields are relatively low today. Under certain assumptions, that implies relatively low future returns. That is another way of saying that buyout multiples are historically high. On this, we agree. The high multiples being paid are a cause for concern.

Historically, higher multiples are associated with lower PMEs and Direct Alphas. Exhibits 5A and 5B show the relationship between PMEs and Direct Alphas (relative to the S&P 500) and EBITDA multiples paid in deals worth more than $500 million in enterprise value according to the S&P LCD. Consistent with this, a regression of EBITDA multiples on PMEs yields a negative and significant coefficient of -0.13.

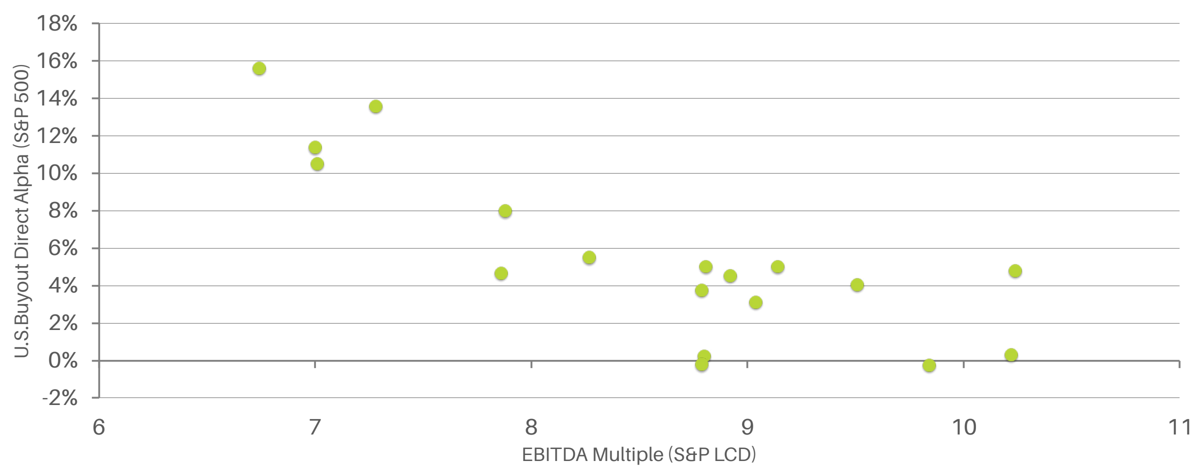
This correlation is concerning because EBITDA Multiples averaged 10.9 in 2017 and 2018. At those multiples, the regression coefficients imply performance for those vintages will be less than the S&P 500 with PMEs of 0.90. That said, EBITDA multiples have been above 10 for vintages since 2014. Despite that, the 2014 and 2015 vintages currently have PMEs above one and Direct Alphas well above zero.

Exhibit 5A: PMEs Versus EBITDA Multiples from 1997 to 2014



Source: Burgiss Private IQ, as of September 30, 2018. S&P LCD.

Exhibit 5B: Direct Alphas Versus EBITDA Multiples from 1997 to 2014



Source: Burgiss Private IQ, as of September 30, 2018. S&P LCD.

CONCLUSION

U.S. buyout funds have historically outperformed public market indices, even more recently. This remains true even after making reasonable adjustments for leverage (beta) and a potential small-cap and value premium.

That said, there are two forces that will make it more difficult for buyout firms to continue that performance. First, the amount of capital raised by buyout funds is at historically high levels. Second, purchase price multiples also are at historically high levels. In the past, realized buyout returns have been lower when capital and, particularly, multiples have been high.

While those two forces operate, buyout firms have faced similar headwinds in the past. Perhaps surprisingly, buyout firms have been able to offset those headwinds in every vintage year in the last twenty-five years, to perform at least as well as the S&P 500.

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KENAN INSTITUTE of Private Enterprise

We put knowledge to work.

Long-Term Private Equity Performance: 2000 to 2022[1]

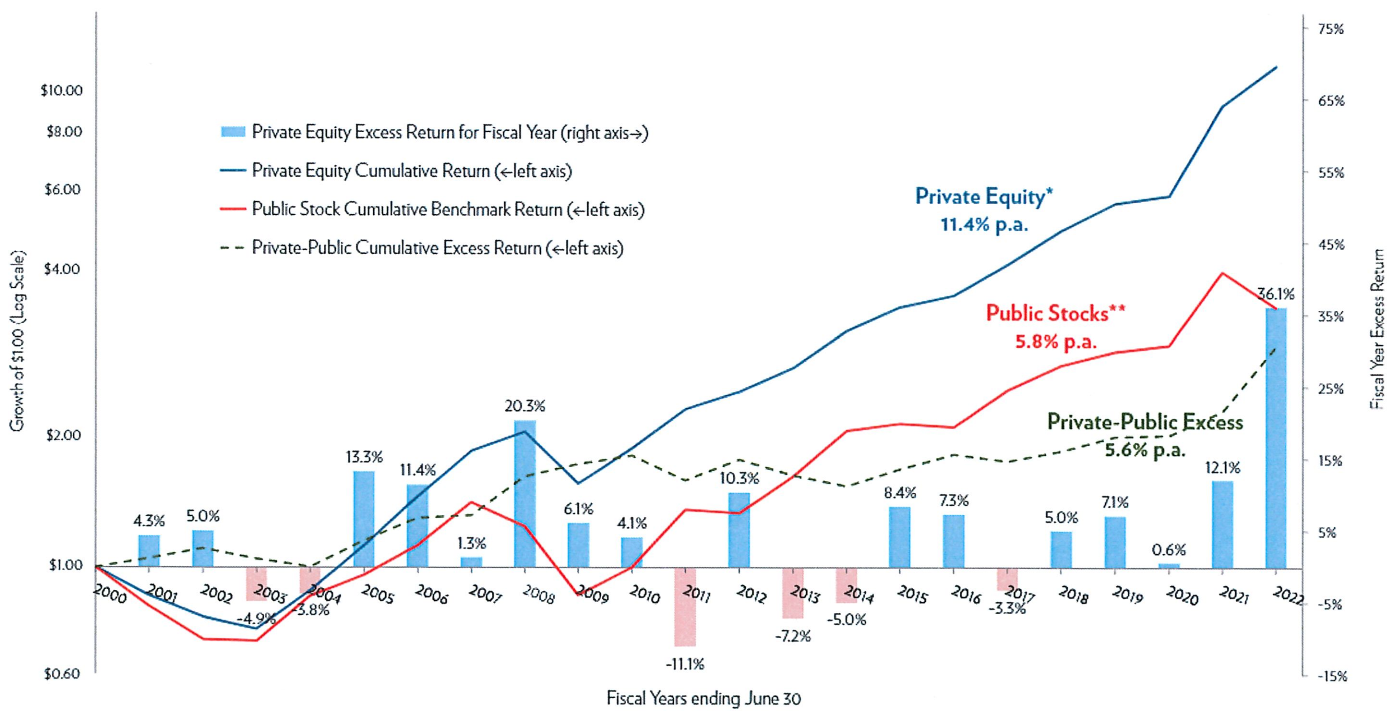
Updated February 28, 2023

Press coverage of private equity focuses almost exclusively on second order concerns over disclosure, fees, layoffs, debt, and an occasional scandal. Rarely is attention given to the first order benefit of high risk-adjusted returns.

Our annual performance study now includes 2022[2], a year that produced a 21% return for private equity, a record 36% better than the public stock markets. The 36% excess return is likely overstated to some degree by the timing of private equity valuations at June 30, 2022, and will be adjusted when 2023 returns are reported. Nonetheless, ***over a 22-year time period ending June 30, 2022, private equity allocations by state pensions produced a 11.4% net-of-fee annualized return, exceeding by 5.6% the 5.8% annualized return that otherwise would have been earned by investing in public stocks.***

The higher private equity returns did not come with higher volatility. The annualized standard deviation of returns for private equity equaled 15.8% for the 22-year period, compared to 17.5% for public stocks.

Exhibit 1: Composite Private Equity Performance across State Pensions:
Covering 22 Years starting June 30, 2000, and ending June 30, 2022
Growth of $1.00 (left axis) and Annual Excess Return (right axis)



* An equal-weighted average of all state funds who reported private equity returns in annual ACFRs for June 30 fiscal years 2001-2022. The equal-weighted average return of 19 state funds who reported private equity returns across all 21 fiscal years equaled 11.9% per annum.
** A public stock benchmark weighted 70% to the Russell 3000 Index (6.62% annualized return over 22 years) and 30% to the MSCI ACWI ex US Index (3.53% annualized return over 22 years), with assigned weights reflecting regression-based weightings (a.k.a. "style analysis").

[1] This is the sixth edition of our private equity performance report, which was first printed in 2017.
[2] Fiscal year 2022 from June 30, 2021, to June 30, 2022.

Private Equity Performance

Exhibit 1 plots cumulative returns[3] for the Private Equity Composite, the Public Stock Benchmark, and the return difference (excess return) between private and public equities. Annualized returns for the entire 22-year period are reported. The 11.4% annualized return for private equity for the entire 22-year period is impressive compared to the 5.6% annualized return for the Public Stock Benchmark and the resulting 5.6% annualized return difference exceeds the 3% annual premium or excess return generally associated with return objectives for private equity. Also shown in Exhibit 1 are bars representing individual fiscal year return differences ("excess returns") between the Private Equity Composite and Public Stock Benchmark.

Study Data and Design

We draw our findings from data provided in Annual Comprehensive Financial Reports ("ACFRs") published by 94 state pension systems. We selected this data source because, unlike commonly used commercial universes, it is a closed group with no selection biases, and represents actual results achieved by large institutional investors. The list of 94 is narrowed to 65 state systems that use the same June 30 fiscal year-end date to achieve consistent performance measurement periods. The list is reduced again to 53 state systems that reported private equity returns for all or part of the study period. Nineteen (19) of the 53 state systems operated private equity portfolios for all 22 fiscal years.

The study period was selected partly for ease of data collection but also because it covers three full market cycles, encompassing three bear markets and three bull markets. We create a "Private Equity Composite" return series calculated by taking the average of all state systems reporting private equity portfolio returns for that fiscal year. The number of state systems included in the yearly average grew steadily over the study period from 19 to 61.

Most state systems have a private equity objective to outperform public equity by some percentage point amount, the most frequent amount being 300 basis points (3%), net of all fees. The 3% incremental return is intended to compensate investors for the loss of liquidity and complexity associated with private equity, but investors do differ on the appropriate return spread for private equity over public equity. The equity index used to represent public equity varies as well with some state systems targeting a U.S. benchmark like the S&P 500 or Russell 3000 Index and others using a global equity index like the MSCI ACWI Index.[4]

We create a "Public Stocks Benchmark" by calculating a weighted average of the Russell 3000 Index (70%) and the MSCI ACWI ex US Index (30%), rebalanced annually. The 70% and 30% weights are, in our judgment, reflective of the typical mix of U.S. and non-U.S. private equity investments in large, diversified portfolios. The weightings are confirmed through a statistical analysis of periodic state pension private equity returns as the dependent variable and the Russell 3000 and MSCI ACWI ex US index returns as independent variables. Regression coefficients on the Russell 3000 and MSCI ACWI ex US variables were found to be exactly 70% and 30%, respectively, indicating that our Public Stocks Benchmark best reflects the geographic risks found in private equity allocations.

The return calculations in this study follow the reporting practices of state pension systems as described in most ACFRs. Reported fiscal year private equity returns are typically *internal rates of return*, which are then linked in a *time-weighted* fashion to create multiperiod returns. The *internal rate of return* calculation is often used in measuring private equity performance in part because it represents a better measure of return when cash flows are very large in relation to portfolio values and because managers control the timing of cash flows. These two conditions are less relevant for state private equity portfolios that aggregate

[3] Cumulative returns are presented in Exhibit 1 using a "Growth of $1.00" scale, measuring how an initial $1.00 investment would have grown if it earned the average private equity return of reporting state systems or the Public Stocks Benchmark return.
[4] MSCI ACWI Index represents all global public equity markets. The MSCI ACWI ex US Index excludes the U.S. equity market. "ACWI" is an acronym for All Country World Index.

many underlying private equity funds. First, aggregated private equity cash flows (both inflows and outflows) tend to be modest relative to the size of the overall portfolio. Second, at the aggregate level the timing of cash flows is also controlled by the pension system itself through its "capital budgeting". For example, a fund manager's eagerness to distribute cash proceeds may be offset by increased new fund commitments by the pension system to preserve "vintage diversification".

Convergence

Not too long ago, a familiar narrative was that private equity returns were failing to deliver the excess return over public stocks compared to years past.[5] Our study finds no such evidence. Private equity returns are tested for convergence through a simple regression analysis that uses fiscal private equity excess returns reported in Exhibit 1 as the dependent variable and time as the independent variable. Last year (excluding 2022) the resultant coefficient on time (year) was virtually zero. With strong comparative private equity performance in 2022 the resultant coefficient on time is now +39 basis points per year, without statistical significance. Convergence may be a legitimate investor concern, but so far there has been no evidence of its presence.

Strong performance from private equity is expected. Private asset classes, including private equity, private debt and private real estate, should outperform their public equivalents as investors demand higher returns for loss of liquidity. This "illiquidity premium" has generally been estimated at two percentage points in extra return across private asset classes.[6]

Conclusion

Our updated study focuses on the private equity performance achieved by large state pension systems over a 22 fiscal year period from 2000 to 2022. This data is different from return universe data available on individual private equity funds which ignores selection, weighting, co-investment, and other decision factors that state pensions make in managing a private equity portfolio.

The study finds that private equity produced a meaningful 5.6% annualized excess return over public equity. We test for any diminution of excess return over time and find no evidence of private equity and public stock return convergence.

Private equity has consistently been one of the strongest performing asset classes within state pension portfolios.

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[5] A recent example is contained in the otherwise excellent *Bain Global Private Equity Report 2020*, recently reported on by *Pensions & Investments* under the headline "U.S. Private, Public Equity Returns Starting to Converge."
[6] See, for example, Private Debt: Yield, Safety and the Emergence of Alternative Lending, Stephen L. Nesbitt, John Wiley & Sons © 2023, Chapter 11, for illiquidity premiums in private direct loans.

The (Heterogenous) Economic Effects of Private Equity Buyouts

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8 July 2021

Abstract: The effects of private equity buyouts on employment, productivity, and job reallocation vary tremendously with macroeconomic and credit conditions, across private equity groups, and by type of buyout. We reach this conclusion by examining the most extensive database of U.S. buyouts ever compiled, encompassing thousands of buyout targets from 1980 to 2013 and millions of control firms. Employment shrinks 13% over two years after buyouts of publicly listed firms – on average, and relative to control firms – but expands 13% after buyouts of privately held firms. Post-buyout productivity gains at target firms are large on average and much larger yet for deals executed amidst tight credit conditions. A post-buyout tightening of credit conditions or slowing of GDP growth curtails employment growth and intra-firm job reallocation at target firms. We also show that buyout effects differ across the private equity groups that sponsor buyouts, and these differences persist over time at the group level. Rapid upscaling in deal flow at the group level brings lower employment growth at target firms.

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This paper develops evidence of a remarkable heterogeneity in the economic effects of private equity (PE) buyouts. Specifically, the effects on employment, job reallocation, and productivity differ markedly by type of buyout, with credit conditions that prevail when the buyout closes, with the evolution of macroeconomic and credit conditions after the buyout, and across the PE groups that sponsor buyouts. To carry out our study, we tap multiple sources to identify and characterize about 9,800 PE buyouts of U.S. firms from 1980 to 2013. For roughly 6,000, we successfully merge information about the buyout with comprehensive Census micro data on firm-level and establishment-level outcomes. Armed with this database, we estimate the effects of buyouts on target firms relative to contemporaneous developments at comparable firms not backed by private equity. We focus on outcomes over the first two years after the buyout.

Our large sample, long time period, high-quality data, and ability to track firms and establishments enable a careful look at heterogeneity in the real-side effects of buyouts. We can, for example, investigate how buyout effects on independent, privately held firms compare to those of publicly listed firms. Because our sample period encompasses huge swings in credit market tightness and macroeconomic performance, we can address recurring questions about how these external conditions affect the relative performance of target firms. By tracking individual PE groups over time, we can assess whether they differ in their impact on target firms, and whether and how much those differences persist over time.

Our chief findings include the following:

- Post-buyout labor productivity gains at target firms are large relative to contemporaneous outcomes at control firms, and much larger yet for deals executed in tight credit conditions.
- Relative employment at targets rises 13 percent, on average, in firms previously under private ownership, whereas it falls 13 percent in buyouts of publicly listed firms.

- A post-buyout widening of credit spreads, or a slowdown in GDP growth, lowers employment growth and intra-firm job reallocation at targets (again, relative to controls).
- The mix of buyout types and PE sponsor characteristics varies over time, but there is little evidence that changes in this mix drive the sensitivity of buyout effects to market conditions.
- Buyout effects on employment differ among the PE groups that sponsor buyouts, and these differences persist over time at the group level.[2]
- Rapid upscaling in deal flow at the PE group level brings weaker post-buyout employment performance at target firms, conditional on the group's performance history, time effects, and a battery of other controls.

In short, the real-side effects of buyouts on target firms are more complex and varied than either PE champions or detractors claim. Indeed, the effects are highly circumstance-specific in a manner, we argue, that aligns well with financial theory and with evidence on the financial performance of PE buyouts.

Our study builds on and draws inspiration from many previous works. Early studies on the real-side outcomes associated with PE buyouts include Kaplan (1989) and Lichtenberg and Siegel (1990). More recent research considers larger samples, often by exploiting a combination of proprietary and government data sources. Examples include Boucly, Sraer, and Thesmar (2011), Cohn, Mills, and Towery (2014), Davis et al. (2014), Farcassi, Previtero, and Sheen (2018), and Cohn, Nestoriak, and Wardlaw (2019). Beginning with Bernstein and Sheen (2016), many recent studies consider the impact of private equity in particular industry settings. Relative to Davis et al.

[2] Echoing persistent financial performance differences at the group level (e.g., Kaplan and Schoar, 2005, and Harris et al., 2020). Unlike the case of financial performance, however, we see no evidence of a weakening over time in the group-level persistence of real-side effects.

(2014), we improve on their empirical methods, extend their sample period to cover the financial crisis and its aftermath, draw on previous research to explain why we anticipate heterogeneity in the real-side effects of PE buyouts, and provide a rich set of new findings on how buyout effects vary with macroeconomic and credit conditions, by type of buyout, across the PE groups that sponsor buyouts, and with the scale of buyout activity at the group level. Below, we offer many additional remarks about how our study and findings relate to previous research.

The next section reviews theoretical perspectives and prior empirical research that help understand the heterogenous effects of PE buyouts. Section II discusses the creation of our database. Section III sets forth our empirical approach and describes our results. Section IV concludes the paper. Appendices provide additional information about our data and empirical methods and additional results.

I. Sources of Heterogeneity in the Effects of Private Equity Buyouts

There are several theoretical reasons to anticipate heterogeneity in the real-side economic impact of various buyouts.[3] This section reviews some of the key literature to help frame the analyses that follow.

A. Differences across Buyout Types

There is little theoretical work on the heterogeneous effects of PE buyouts *per se*, but the tradeoffs between publicly traded and privately held ownership are the subject of an extensive literature. Among the hypothesized advantages of public ownership are lower equity costs, higher firm values, and relaxed capital constraints. Zingales (1995), for instance, hypothesizes that public listings put firms "on the radar screen" of potential acquirers and, under certain conditions,

[3] The main text focuses on productivity, employment and job reallocation effects. Appendix D also presents estimated buyout effects on firm-level mean wages, which appear to be heavily influenced by buyout-related shifts in management compensation.

maximize firm value. Pagano et al. (1998) argue that access to public equity markets reduces the cost of credit by giving firms more bargaining power with their banks, thereby maximizing capital availability and enhancing firm value. Chemmanur and Fulghieri (1999) suggest that private investors may demand a risk premium, and value firms accordingly. Brau et al. (2003) argue that IPOs create publicly traded shares that a firm can use as "currency" when acquiring other companies. Maksimovic and Pichler (2001) model firms that conduct IPOs to increase publicity or reputation value, thereby improving capital market access and raising firm value.

Being publicly traded also comes at a cost. The vulnerability of publicly traded firms to agency problems has been understood since Jensen and Meckling (1976). Due to weaknesses in the market for corporate control, difficulties in monitoring by dispersed shareholders, problematic incentives of corporate directors, compensation schemes that reward empire building and myriad other reasons, publicly traded firms can be especially prone to value-destroying activities. Jensen (1989) proposed that buyouts are optimized to resolve these problems. Axelson et al. (2013) report that buyouts of publicly traded firms are more associated with high debt burdens, which Jensen hypothesized create pressure on management to take cost-cutting steps they might otherwise resist.

These arguments and observations suggest that the consequences of PE buyouts differ between publicly traded and privately held targets. For targets that trade publicly before the buyout, PE groups may focus on tackling the agency problems sketched above – whether manifested as excess headcounts, wasteful perquisites, or value-destroying "pet projects."[4] For

[4] Job losses after public-to-private and divisional buyouts could also be interpreted along the lines of the workforce re-contracting hypothesis that Shleifer and Summers (1988) advance in the context of hostile corporate takeovers. They stress the role of implicit long-term contracts in fostering relationship-specific investments by the firm's stakeholders. According to the re-contracting hypothesis, takeovers that break implicit contracts can be profitable for shareholders, at least in the short run.

privately held targets that face fewer agency problems but find it harder to access capital markets, it makes sense for PE groups to devote greater attention to investments that drive growth. Insofar as PE buyouts lead to productivity gains at target firms, these observations also suggest that the mechanisms at work may be quite different for publicly listed and privately held targets.

Case evidence illustrates some of these points. In late 1987, Berkshire Partners bought out the Lake States Transportation division of the Soo Line (the U.S. subsidiary of the publicly traded Canadian Pacific), renaming it Wisconsin Central. The new management cut operating employees per train from 4.8 to 2.2 and cut wages by 15%.[5] As a result, labor costs dropped from the historical 50% of revenue to 32% in 1988. In later years, Wisconsin Central continued to improve labor productivity through the application of better information technology and tight management, with revenue ton-miles per workhour rising 54% from 1989 to 1995 (Jensen, Burkhardt and Barry, 1989, and SEC filings).[6] In another case, Brazos Partners acquired 80% of privately held Cheddar's Restaurants in 2003, buying out many "friends and family" investors who were reluctant to put additional capital into the firm. Brazos' own funds, its banking connections, and its industry relationships enabled the firm to greatly accelerate its pace of restaurant openings, bring in new managers to rationalize the practices of an extremely informal organization, and develop new chain concepts. The founder, having liquidated much of his equity in the firm and diversifying his asset holdings, still retained a significant equity stake but nonetheless became more willing to pursue high-risk, high expected-return strategies (Hardymon and Leamon, 2006).

Existing empirical work also finds evidence consistent with these hypotheses. For instance, Boucly, Sraer, and Thesmar (2011) analyze a sample of largely private-to-private buyouts of

[5] Most of the division's employees opted to remain with Soo Line, as the new owners made clear that transferred employees would lose seniority rights and work in a non-union environment.
[6] Jensen, Burkhardt, and Barry (1989) and SEC filings.

French firms. They conclude that these buyouts eased financing constraints at target firms, enabling their expansion. Large productivity gains fit well with evidence in Bloom, Sadun, and van Reenen (2015), who survey a sample of buyouts of middle-market firms, where private-to-private deals predominated. They find that these PE deals led to investments that resulted in better management practices. Hellman and Puri (2002) provide evidence that venture capital helped drive the professionalization of recruitment, human resource policies, marketing, and the use of stock options in high-tech start-up firms. Similarly, private equity may drive professionalization in the buyouts of privately held firms in particular, especially when sales growth or market conditions have outpaced legacy management structures and processes.

B. Differences over the Economic Cycle

Axelson, Stromberg, and Weisbach (2007) offer a useful framework for understanding why the consequences of buyouts may vary over the economic cycle.[7] In their setting, privately-informed firms (e.g., the general partners of PE groups) raise funds from less-informed investors. Informational asymmetries create a temptation on the part of general partners to overstate the potential of their investments. Axelson et al. show that the (second-best) solution ties the compensation of PE investors to the collective performance of a fund, rather than that of individual

[7] More generally, fluctuations in credit availability have long pre-occupied economists (e.g., Kindleberger, 1978). One concern involves the incentives that drive credit decisions. In Rajan's (1994) model, for example, the desire to manage short-term earnings drives bankers to make value-destroying loans in good times and curtail lending abruptly in bad times. A second concern involves the banking system's capacity to supply credit. Bernanke and Gertler (1987) develop a theory in which negative shocks to bank capital cause them to forego value-creating loans. A third set of concerns surrounds the effects of credit availability on the broader economy. According to the "financial accelerator" mechanism in leading macro models (e.g., Bernanke, Gertler and Gilchrist, 1999), endogenous swings in credit availability amplify and propagate the effects of shocks to the macroeconomy. Credit availability and debt levels are also a key focus in many post mortems of economic crises (e.g., Reinhart and Rogoff, 2009; Campello, Graham, and Harvey, 2010; and Schularick and Taylor, 2012) and a first-order concern for central bankers.

buyouts. In this way, the general partners have less incentive to invest in bad deals. Moreover, it makes sense for fund managers to invest equity alongside outside debt raised on a deal-by-deal basis, thus providing a further check on the temptation to do lower-quality deals with funds raised.

Even when employing this optimal financing structure, however, Axelson et al. show that PE groups are tempted to overinvest during hot markets. Conversely, during recessions, value-creating projects may languish unfinanced. These distortions amplify the normal ebb and flow of the business cycle, resulting in an intense pro-cyclicality of PE deal-making activity.

This theoretical work focuses on the cyclicality of PE activity and the financial returns to buyouts, but it has implications as well for their real-side consequences. In particular, deals done when financing is plentiful may end up underperforming for two reasons. First, due to overfunding, PE groups may move "down their own demand curve" when financing is easy, selecting inferior deals with less scope for value creation in the form of operational improvements. Second, if the supply of experienced PE managers is not fully elastic in the short term, a larger deal flow may dilute the attention paid to any given portfolio company. Both reasons lead to weaker post-buyout operating performance for deals executed amidst easy-credit conditions. While we cannot pin down which of these two reasons (or both) might be at work, this line of thinking says that the marginal benefits of PE buyouts in the form of productivity gains are countercyclical.

The post-buyout evolution of macroeconomic and credit conditions is also likely to affect the performance of target firms. As Appendix Table D.4 shows, firms backed by private equity are far more likely to engage in acquisitions than their peers. To the extent that market conditions influence the ability of target firms to undertake post-buyout acquisitions and divestitures, a deterioration in macroeconomic or credit conditions can affect overall employment growth and reallocation in target firms, although not necessarily the organic parts.

There is also previous empirical research on the relationship between buyouts and credit cycles. Pioneering work by Kaplan and Stein (1993) presents evidence that fits "a specific version of the overheated buyout market hypothesis… [that] the buyouts of the later 1980s [were] both more aggressively priced and more susceptible to costly financial distress." Twenty-five of 66 deals in their sample executed during the easy-credit period from 1986 to 1988 later underwent a debt default, an attempt to restructure debt, or a Chapter 11 bankruptcy filing. In glaring contrast, only one of 41 deals executed from 1980 to 1984, when credit conditions were much tighter, experienced one of these forms of financial distress. Axelson et al. (2013) look at a broader sample of deals and show that credit market conditions drove leverage in buyouts far more than in publicly listed firms. Kaplan and Schoar (2005), among others, find that easier credit conditions bring greater inflows into buyout funds and lower fund-level returns.[8] In short, the literature suggests that when economic growth booms and credit spreads narrow, PE funds attract larger inflows, their deals involve more leverage and higher valuations, and investors ultimately receive lower returns. We investigate the impact of these forces on the real-side outcomes at target firms, which has received little attention to date.[9]

Much less is written, from either a theoretical or empirical perspective, about the *interaction* of buyout type and economic cycles: whether the sensitivity of buyouts to external conditions differs by type of buyout. Nonetheless, we hypothesize that productivity gains and the pace of reallocation are more procyclical in public-to-private than private-to-private deals. Public-

[8] Other papers that touch in various ways on market cycles and private equity include Ivashina and Kovner (2011), Hotchkiss, Strömberg, and Smith (2014), Harris, Jenkinson, and Kaplan (2016), and Bernstein, Lerner, and Mezzanotti (2019).
[9] One exception is the survey data in Bernstein, Lerner, and Mezzanotti (2019), which provide evidence that PE groups devoted more attention to the operating performance and strategic decision making of their portfolio companies during the financial crisis of 2008-09.

to-private deals are both more leveraged (Axelson et al. 2013) and more likely to encounter financial distress (Stromberg, 2008). Thus, tight financial conditions during downturns may divert the attention of management at the target firm and the PE group itself away from operating performance. In addition, insofar as the typically larger buyouts of publicly listed firms are more dependent on continuing capital market access (e.g., to finance ongoing acquisitions and divestitures that reshape the firm), economic downturns may especially impair restructuring and performance in public-to-private buyouts.

C. Differences across Private Equity Groups

Economists have become increasingly attuned to the role of persistent organization-specific attributes that affect productivity. Syverson (2011) provides an overview of many key studies, and Autor et al. (2020) examine a related phenomenon. Many differences in firm performance reflect heterogeneity in management practices (Bloom and van Reenen, 2007). Bertrand and Schoar (2003) show that top executives influence firm performance and key strategic choices, an effect that holds even when focusing on switchers who move from one firm to the next.

These differences can manifest across PE groups as well. Practitioner accounts (e.g., Bain, 2020) suggest that PE groups often have well-defined specializations, not just in regard to industry (e.g., the focus of ABRY on telecommunications or Vista on software), but in their approaches to value creation. One frequently encounters claims that some PE groups place greater emphasis on value creation through operational improvements while others stress financial engineering. Anecdotal accounts suggest that PE groups have "playbooks" and shared-value systems, which they apply to their investments in buyout after buyout (Lerner, Tagade, and Shu, 2018, and Wulf and Waggoner, 2010). PE groups are also remarkably stable in terms of their key senior management (Lerner and Noble, 2021), which may drive persistence in their approaches.

In line with these remarks, Kaplan and Schoar (2005) and Harris et al. (2020) find strong persistence in the financial performance of buyout groups, at least for funds formed through 2000. Thus, in addition to the effects of market cycles and buyout types, we hypothesize the presence of PE group-specific differences in their effects on portfolio firms.

II. Creating the Leveraged Buyout Sample

A. Identifying Private Equity Buyouts

Our study builds on the data work and analysis in Davis et al. (2014) to consider transactions involving later-stage companies with changes in ownership and control, executed and partly financed by PE firms. In these deals, the (lead) PE firm acquires a controlling equity stake in the target firm and retains significant oversight until it exits by selling its stake. The buyout typically involves a shift toward greater leverage in the capital structure of the target firm and, sometimes, a change in its management. Bank loans are key sources of the credit that facilitate the leveraged nature of PE buyouts.

We made major efforts to construct our sample of buyouts and ensure its integrity, expending thousands of research assistant hours. The specific process is described in Appendix A. The resulting sample contains 9,794 PE-led leveraged buyouts of U.S. companies from January 1, 1980 to December 31, 2013. We sort the sample buyouts into four main deal types based on descriptions in CapitalIQ and our reviews of other databases, press accounts, and securities filings.

Figure 1 displays quarterly counts of PE-sponsored buyouts in our sample for the four deal types.[10] As noted in other studies, PE buyout activity grew enormously in recent decades. The

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[10] Appendix Table D.1 reports average quarterly counts before, during and after the financial crisis. Because we lack non-Census data on deal size for much of our sample, especially in more recent years, we cannot construct a size-weighted version of Figure 1 without matching to Census micro data. Once we match, however, we become subject to Census disclosure rules that preclude a granular depiction of deal flow as in Figure 1.

expansion is especially striking for private-to-private buyouts, which saw a huge increase in deal flow over time. The flow of new PE buyouts crashed during the financial crisis, as credit conditions tightened and the economy contracted. Interestingly, the flow of new public-to-private buyouts dropped off well before the onset of the financial crisis, and remained at modest levels through the end of our sample. Counts for private-to-private and secondary (where PE groups are on both sides of the buyout) transactions rebounded sharply as the economy recovered from the 2008-09 recession and maintained a robust pace until the end of our sample in 2013.

To set the stage for the analysis below, Table 1 presents evidence on how deal flow relates to economic and credit conditions. Specifically, we regress the natural log of quarterly buyout counts on buyout type indicators, a linear time trend, and the deal-type indicators interacted with market conditions. We consider conditions when the buyout closed (top panel) and changes over the next two years (bottom panel). We use real GDP growth to characterize economic conditions and the yield spread between below-investment-grade corporate bonds and one-month LIBOR for credit conditions. (See Section III.D for precise definitions.)

The results are striking. The top panel says that deal volumes are higher when real GDP growth is above its sample median and credit spreads are narrower than the median. Buyout counts are 28 log points (32%) higher for private-to-private deals, 66 log points (93%) higher for public-to-private deals, and 41 log points (51%) higher for divisional sales in periods with above-average GDP growth, conditional on the credit-spread interaction variables and the controls. Buyout counts are 18-27 log points lower when credit spreads are wider than average, conditional on the other regressors. The credit spread results are considerably stronger when using an upper tercile split. (See Appendix Table D.2.) Axelson et al. (2013), among others, also document the relationship of credit spreads to buyout activity and to the extent of leverage and valuations.

The bottom panel in Table 1 says that periods with high buyout volume are associated with rising credit spreads over the next two years and, except for secondary sales, higher than average GDP growth over the next two years. Again, the associations are large in magnitude. For example, buyout counts are 20-68 log points higher in periods that precede above-average increases in credit spreads. This pattern – most pronounced for public-to-private buyouts – says that target firms often face a tightening of credit conditions after the buyout, an issue that we explore below.

Appendix Table D.3 shows how the industry mix of PE buyouts differs by deal type. For instance, public-to-private deals are relatively prevalent in Consumer Staples (e.g., food and household products) and Healthcare, while divisional deals are relatively prevalent in Information Technology and Utilities. A Pearson chi-squared test rejects the hypothesis that the industry distribution of buyouts is independent of deal type. The distributions of PE buyouts by industry, firm size, and firm age also differ greatly from the corresponding distributions of private sector employment (Davis et al., 2014). Given these patterns, our econometric investigations below compare buyout targets to control firms within cells defined by the full cross product of industry, firm size categories, firm age categories, multi-unit status, and buyout year.

B. Matching Private Equity Buyouts to Census Micro Data

The Longitudinal Business Database (LBD) is a longitudinal version of the Census Bureau's comprehensive Business Register (BR), which contains annual data on U.S. businesses with paid employees. The LBD covers the entire nonfarm private sector and, in recent years, has roughly 7 million establishment records and 5 million firm records per year.[11] It draws on a wide range of administrative records and survey sources for data inputs. Firms are defined based on

[11] An establishment is a physical location where economic activity occurs. A firm is a legal entity that owns and operates one or more establishments.

operational control, and all establishments majority owned by a parent firm are included in the parent's activity measures. Core data items include employment, payroll, four-digit Standard Industrial Classification (SIC) or six-digit North American Industrial Classification (NAICS) codes, employer identification numbers, business names, and location information.

To merge our buyout data to Census data on firms and establishments, we match business name and address information for the buyout targets to the name and address records in the BR. Table 2 summarizes our sample of PE buyouts matched to Census micro data. Panel A reports the number of establishments operated by our 6,000 matched target firms and their employment, with breakdowns by deal type. Panel B considers the 5,100 matched buyouts that closed from 1980 to 2011. Compared to the 1980-2003 sample in Davis et al. (2014), our new 1980-2011 analysis sample has 2.3 times as many matched targets, reflecting high deal flow after 2003. Private-to-private deals account for about half of our 1980-2011 sample, as in our earlier work. The 22% share of secondary sales is nearly twice as large as in our earlier work, reflecting the large flow of these deals in recent years. The share of divisional buyouts is somewhat smaller in our new sample.

In our econometric analysis below, we limit attention to matched buyouts that closed from 1980 to 2011, so we can track their outcomes through 2013 in the LBD. We also drop target firms that we match to Census micro data using only taxpayer EINs (and not other firm IDs). As explained in Appendix A, we are not confident we can identify all establishments operated by the target firm in these EIN cases. Finally, we restrict our regression analysis to firms that we confidently track for two years post buyout. That leaves roughly 3,600 target firms in our regression analyses below, identified as "Two-year continuers" in Panel B of Table 2. Private-to-private deals account for 29% of target employment as of the buyout year in this sample, public-to-private deals account for 36%, divisional deals account for 11%, secondary sales account for 19%, and buyouts of unknown type for the rest.

Panel C compares matched buyouts in our new sample to those in Davis et al. (2014) for their 1980-2003 analysis period. Our new sample has about 20% fewer buyouts in the overlapping period, which reflects the more rigorous matching criteria that we now apply. Our new sample of two-year continuer targets (excluding EIN cases) has 10% fewer matched buyouts. The mix of buyout types in our new 1980-2003 sample is similar to the one in our earlier work.

III. Empirical Results

A. *Regression Specification, Weighting, and Identification*

We estimate firm-level regressions of the following form by least squares

$$Y_{i,t+2} = \alpha + \sum_c D_{cit}\theta_c + \lambda_1 LEST_{it} + \lambda_2 LFIRM_{it} + \gamma PE_{it} + \varepsilon_{it}, \quad (1)$$

where $Y_{i,t+2}$ is the change in the outcome variable of interest from buyout year *t* to two years later for firm *i*.[12] The D_{cit} are cell-level dummy variables defined on the full cross product of buyout year *t*, the firm's three-digit NAICS, its size category, its age category, and an indicator for whether it owns multiple establishments. $LEST_{it}$ and $LFIRM_{it}$ are controls for the firm's pre-buyout growth history. To construct $LEST_{it}$, we consider the set of establishments owned by firm *i* in buyout year *t* and compute their employment growth rate from $t-3$ to $t-1$. To construct $LFIRM_{it}$, we consider the parent firm that owned these establishments in $t-3$ and compute its growth rate from $t-3$ to $t-1$. If ownership was split across multiple firms in $t-3$, we select the firm with the

[12] It is often impossible to track target firms over several years post buyout. However, Davis et al. (2014) track employment at target and control *establishments* for five years after buyout events. They find that establishment-level buyout effects over five years are about 90 percent larger than over two years, which suggests that results based on (1) understate the cumulative impact of buyout events on firm-level outcomes over several years.

largest share of employment among these establishments. Often, but not always, these two control variables take on the same value. PE_{it} is a dummy variable equal to 1 for a target firm.

Buyout effects can vary with firm characteristics and economic conditions and by industry, deal type, and time period. However, there is surely more heterogeneity in treatment effects than we can estimate with precision. Faced with this heterogeneity, our goal is to obtain a consistent estimate for the activity-weighted mean treatment effect on treated units (i.e., buyout targets) under the under assumptions of conditional mean independence (CMI) and stable unit treatment value (SUTVA). To do so, we weight each target firm by its share of aggregate target activity, where "aggregate" refers to the sum over all buyouts in the regression sample. We weight each control unit in proportion to its employment share in its control cell, and rescale to equate the sum of weights on control units in a cell to the sum of weights on targets in the same cell. See Appendix B for additional discussion.

Our rich set of controls lends greater plausibility to the CMI assumption than in most previous work on PE buyouts. Even if CMI fails, our results provide useful evidence for formulating and evaluating theoretical models of PE behavior and its effects. The SUTVA assumption could fail if treatment effects on targets alter product demand and factor supply conditions facing controls, or if they exert competitive pressures that drive higher productivity at controls. Since targets typically account for modest activity levels relative to controls, these effects are likely to be quite small in our setting. Another possibility is that buyout targets implement superior technologies or business strategies that controls then emulate. The scope for such imitation effects also seems quite small within our two-year post-buyout time frame.

B. *The Average Economic Effects of Private Equity Buyouts*

Table 3 reports the estimated γ coefficients and associated standard errors for regressions of the form (1). Coefficients are approximate percentage point changes from the buyout year t to $t+2$. The "All Buyouts" column covers firms that underwent buyouts from 1980 to 2011 and matched control firms in the same cells. There are about 3,600 targets and 6.4 million total firm-level observations in the regressions that consider employment growth and reallocation outcomes. The underlying number of establishments is much larger, because many target firms (and the corresponding control firms) have multiple facilities. We have fewer usable observations for labor productivity, as discussed below.

According to the "All Buyouts" column in Panel A, employment at target firms shrinks (on average) by a statistically insignificant 1.4 percentage points relative to control firms in the first two years after the buyout. Employment shrinks by 4.4 percentage points relative to controls when omitting post-buyout acquisitions and divestitures. These "bottom line" effects of PE buyouts on target firm employment are a bit larger than we found in Davis et al. (2014): -0.9 percentage points overall, and -3.7 points for organic growth. Appendix Table D.4 provides more detail on how target-control employment growth outcomes differ by margin of adjustment. To summarize the largest differences, target firms are more aggressive than control firms in shutting establishments from t to $t+2$ and in acquiring new establishments from t to $t+2$.

While the net employment effects of PE buyouts attract much interest, buyouts have larger effects on the pace of job reallocation. Overall job reallocation for a firm is the sum of its gross job gains due to new, expanding, and acquired establishments and its gross job losses due to exiting, shrinking, and divested establishments. Dividing overall job reallocation by base employment yields the job reallocation rate. A firm's *excess* reallocation rate is the difference between its job reallocation rate and the absolute value of its net employment growth rate. If a firm changes employment in the same direction at all of its establishments, its excess reallocation is zero. To the extent that a firm expands employment at some units and contracts employment at others, it has positive excess reallocation. If a firm adds jobs at some establishments and cuts an equal number at other establishments, its excess reallocation equals its overall job reallocation.[13]

According to Panel B in Table 3, the job reallocation rate is higher by 7.1 percentage points (of base employment) at targets for organic employment changes over two years after the buyout and by 11.5 points when including acquisitions and divestitures, both highly significant. These results confirm that PE buyouts accelerate the pace of reallocation at target firms, more so when including acquisitions and divestitures. Turning to Panel C, excess reallocation is 5.0 percentage points higher at target firms for all changes, but insignificantly different for organic changes. The implication is that the faster pace of job reallocation induced by buyouts mainly involves greater reallocation of jobs across firms rather than within target firms. In other words, PE buyouts lead to net job losses at some target firms (relative to control firms) and net job gains at other target firms. The extra between-firm reallocation of jobs induced by PE buyouts equals 6.5 (11.5 - 5.0) percent of base employment over the first two years after the buyout.

Panel D in Table 3 provides evidence on how PE buyouts affect firm-level labor productivity, measured as the natural log of revenue per worker.[14] Relative to Panels A-C, we lose observations for three reasons in Panel D. First, we cannot calculate productivity changes for firms that close all establishments by *t+2*. When we drop a target that dies in this sense, we also drop

[13] The excess reallocation concept is often used in the literature on gross job flows to analyze job reallocation within and across regions, industries and other categories. Examples include Dunne, Roberts, and Samuelson (1989) and Davis and Haltiwanger (1992, 1999). Here, we apply the same concept to the reallocation of jobs across establishments within the firm.

[14] RE-LBD labor productivity data are available in real terms using deflators at the NAICS2 and NAICS3 levels. These deflators have no effect on our estimates, which reflect productivity changes at targets relative to contemporaneous changes at controls within the same NAICS3.

controls in the cell associated with that target. If we drop a cell with many controls, we lose many observations. Second, even for targets that survive, some control firms in the cell do not – leading to the loss of additional observations. Third, we drop observations for which firm-level productivity is more than 200 log points from its mean in the same NAICS6-year cell in either the buyout year *t* or in *t+2*. We drop these outliers to guard against large productivity deviations due to errors in the revenue data, errors in linking revenue and employment at the firm level, and errors in the assignment of firms to industries. See Haltiwanger et al. (2017) for a discussion of how these errors can arise in the RE-LBD and why revenue data are unavailable for many firms.

To address the potential selection bias introduced by missing productivity observations, we construct inverse propensity score weights as in Haltiwanger et al. (2017) and similarly to Davis et al. (2014). These weights ensure that the re-weighted RE-LBD is representative of the LBD universe with respect to the size, age, employment growth rate, industry sector, and multi-unit status of firms. We apply these weights and the activity weights described in Section II in our regression analysis of how PE buyouts affect productivity growth.

Turning to the results, labor productivity rises by 7.5 percentage points at targets relative to controls from buyout year *t* to *t+2*. In undisclosed results, we find the largest post-buyout productivity gains at older and larger targets. Davis et al. (2014) estimate that PE buyouts raise total factor productivity by about 2.1 percentage points for target firms in the manufacturing sector. Here, we find a considerably larger effect of PE buyouts on labor productivity when looking across all industry sectors. To help understand this result, Panel C of Appendix Table D.4 decomposes this productivity gain into two pieces: one due to larger workforce reductions at targets, and the other due to greater revenue growth at targets. More than 80 percent of the estimated productivity gain reflects greater revenue growth at targets. We cannot decompose labor productivity gains into markup changes and physical productivity changes, given our data. However, Farcassi, Previtero, and Sheen (2018) show that the rapid post-buyout sales growth of retail and consumer products firms reflects the launch of new products and geographic expansion, not markup hikes.

C. *How the Effects Differ by Buyout Type*

Table 3 also reports estimated effects by type of buyout. According to Panel A, target employment shrinks by 12.6% (relative to controls) after private-to-public buyouts and by 11.5% after divisional buyouts. It rises by 12.8% after private-to-private buyouts and by 9.9% after secondary buyouts. Isolating organic changes, target employment shrinks by 10.0% after private-to-public buyouts and by 16.0% after divisional buyouts; it rises by 3.1% after private-to-private buyouts and by 6.1% after secondary buyouts. All of these estimates are statistically significant at the 1% or 5% level. Thus, we find strong evidence of buyout-induced employment effects that differ greatly by type of buyout. An F-test rejects the null hypothesis of no differences across buyout types in the estimated effects on target employment growth.[15]

Appendix Table D.5 provides more detail. For example, private-to-private and secondary buyouts create new job positions in new facilities at a faster clip than control firms – to the tune of 2.5% and 4.2% of base employment, respectively. In contrast, job creation at new facilities falls by 2.1% at targets relative to controls in public-to-private deals. Gross job destruction in the wake of divisional targets exceeds that of controls by 16% of base employment, mostly due to jobs lost in facility closures. A weaker version of the same pattern holds for public-to-private buyouts. Again, the key message is that employment effects of PE buyouts vary greatly by type of buyout.

[15] To implement the tests (and those in Tables 5 and 6), we replace the γPE_{it} in regression specification (1) with a set of four dummy variable terms, one for each buyout type. We then test for equality of the coefficients on these four dummy variables.

Perhaps this heterogeneity should not surprise. As discussed above, public-to-private deals (and many divisional deals, which are typically carved out of public firms) involve targets with highly dispersed ownership. These firms may suffer from poor corporate governance before the buyout and face an intense need for cost cutting. Meanwhile, buyouts of privately held firms may more often be motivated by a desire to professionalize management or improve access to financing.

Turning to Panels B and C in Table 3, buyouts bring more reallocation, but the effect again differs greatly by deal type. In divisional deals, overall (excess) target job reallocation rises by 19.4% (10.0%) of base employment relative to controls, 17.1% (7.6%) when netting out the role of acquisitions and divestments. In private-to-private deals, acquisitions and divestments entirely drive the post-buyout reallocation uptick at targets relative to controls. Buyouts bring higher job reallocation at targets in public-to-private deals but no statistically significant impact on excess job reallocation. This evidence implies – in line with our earlier discussion – that the extra job reallocation reflects a downsizing of some target firms (relative to controls) and an upsizing of others. Thus, targets show virtually no extra excess reallocation in public-to-private deals. By way of contrast, extra excess reallocation at target firms accounts for one-half to two-thirds of the extra buyout-induced job reallocation in the other deal types. The differences are significantly different at the 5% confidence level when examining the measures of organic reallocation.

Turning to productivity effects, we again find large differences by type of buyout, collectively significant at the 10% confidence level. Target firms in private-to-private deals experience a 14.7 percent productivity gain relative to controls. Targets in public-to-private deals enjoy similarly large gains, but the imprecise estimate precludes a sharp inference. Estimated productivity effects are smaller for other buyouts and statistically insignificant.

Taken together, the results in Table 3 on differences by buyout type suggest that there is little basis for treating private-to-private, public-to-private, divisional, and secondary buyouts as homogeneous in their effects on jobs, reallocation, and productivity. But they are broadly consistent with the limited evidence in previous research on the real-side effects of PE buyouts. According to our evidence, *private-to-private* deals exhibit high post-buyout employment growth (largely but not entirely via acquisitions) and large productivity gains. Meanwhile, *public-to-private* deals exhibit large job losses, often through facility closures, and large (imprecisely estimated) productivity gains. *Divisional* buyouts similarly involve large employment losses and massive reallocation effects. Finally, *secondary* deals exhibit high target employment growth, largely organic, high reallocation and few discernible effects otherwise.[16]

D. *How Buyout Effects Vary with Market Conditions at Close*

We now investigate how the economic effects of PE buyouts vary with market conditions when the deal closes. To do so, we estimate richer regression specifications of the form,

$$Y_{i,t+2} = \alpha + \sum_c D_{cit}\theta_c + \lambda_1 LEST_{it} + \lambda_2 LFIRM_{it} + \gamma PE_{it} + \beta\, PE_{it} * MktCondition_t + \varepsilon_{it}, \quad (2)$$

where the new term, $\beta\, PE_{it} * MktCondition_t$, captures the interaction between buyout status and market conditions. When using intra-year variation in market conditions, we also include the $MktCondition_t$ main effect. When using only annual variation, we cannot separately identify the main effect, since our cell-level controls encompass annual time effects.

We consider two measures of market conditions at the buyout close: the log change in real GDP over the four quarters leading up to (and including) the closing quarter, and the spread

[16] Secondary deals are somewhat of a grab bag, with PE groups on both sides of the transaction. That makes it hard to interpret the effects of secondary buyouts. Hence, and in the interest of brevity, we do not report breakouts for secondary deals in the rest of the paper.

between high-yield U.S. corporate bonds and the one-month U.S. LIBOR in the closing month.[17] Similar spread measures are widely used in the finance literature to characterize debt market conditions. Notably for our analysis, Axelson et al. (2013) show that this spread varies negatively in the extent the buyout transaction is levered and with the EBITDA-multiple paid, and positively with the ultimate financial return on the buyout to PE investors.

The macroeconomics literature offers multiple interpretations for the relationship of spreads to real activity. Viewed through the lens of the q-theory of investment, low bond prices (a high spread) reflect low expected returns to capital (Philippon, 2009). Gilchrist and Zakrajšek (2012) advance a different view. They highlight a major role for movements in "the compensation demanded by investors – above and beyond expected losses – for bearing exposure to corporate credit risk." As they also show, movements in this excess bond premium mirror movements in the equity valuations of financial intermediaries and in their credit default swap premiums. This evidence is broadly in line with our interpretation: a high spread reflects tight credit conditions.

Turning to the results in Table 4, we find no evidence that the post-buyout performance of target firms (again, relative to controls) varies with GDP growth in the four quarters leading up to the close. The β coefficients on the interaction term are imprecisely estimated and statistically insignificant for each dependent variable. In contrast, higher credit spreads at close involve large, statistically significant effects on excess reallocation and productivity growth.[18] Raising the credit

[17] GDP data are from the U.S. Bureau of the Economic Analysis, and the interest rate measures are from Datastream. For the bond rate, we use the yield to maturity on the Bank of America Merrill Lynch U.S. High Yield Index.

[18] From 1980 Q1 to 2013 Q4, the correlation between (a) the credit spread at quarter's end and (b) real GDP growth from four quarters previous to the quarter in question is a modest -0.288. In unreported results, we tried two other measures of external financial conditions: the credit spread measure of Gilchrist and Zakrajšek (2012), and equity market valuations, measured as the ratio of end-of month equity prices to the trailing twelve-month earnings S&P 500 firms. These alternative measures yielded broadly similar, but somewhat noisier, results.

spread by one standard deviation corresponds to a post-buyout productivity gain of 20.3 percent for targets relative to controls and an increase in excess reallocation of 4.6 percent of base employment. These large effects come on top of the baseline effects reported in Table 3.

The positive association between excess reallocation rates and productivity gains as credit conditions vary suggests that PE buyouts achieve productivity improvements by shifting inputs toward better uses within target firms. In a similar spirit, Davis et al. (2014) find that buyouts lead to TFP gains at target firms in the manufacturing sector, mainly due to the reallocation of activity from less productive plants to more productive ones. Here, we find that high credit spreads at the time of the buyout lead to greater productivity gains and greater reallocation activity in target firms in the two years after the buyout. Both sets of results link buyout-induced productivity gains to an accelerated, purposefully directed reallocation of activity within target firms.

Our credit spread results in Table 4 also suggest that PE groups have multiple tools for earning returns on their investments in portfolio firms. When credit is cheap and readily available, it may be more attractive to rely on financial engineering tools to generate returns, e.g., by issuing new debt to fund additional dividend payments to equity holders. When credit is costly and tight, financial engineering is less feasible and PE groups may generate returns through operational improvements that raise productivity in portfolio firms.

E. *How Buyout Effects Vary with the Evolution of Market Conditions After the Close*

We now consider how buyout effects vary with the evolution of market conditions after the close of the deal. We measure post-buyout changes in market conditions from March (or the first quarter) of the buyout year *t* to March (first quarter) of year *t+2*.[19] Table 5 focuses on the post-buyout change in credit conditions, and Table 6 focuses on post-buyout growth in real GDP.

Consider the results for all buyouts. Faster GDP growth in the two-year interval after buyouts brings greater post-buyout employment growth at targets relative to controls and greater excess reallocation. These effects are statistically significant and large: A unit standard deviation rise in the post-buyout GDP growth rate raises employment growth at targets relative to controls by 3.2 percent of base employment, and it raises target excess reallocation by 3.0 percent of base employment. A rise in credit spreads after buyouts brings slower post-buyout employment growth at targets relative to controls and slower excess reallocation. These effects are statistically significant and similar in magnitude to the ones associated with a unit standard deviation change in the GDP growth rate.

Figure 2 illustrates how post-buyout employment growth and excess reallocation at target firms (relative to controls) vary with the evolution of GDP growth and credit spreads. In the top panel, the baseline employment growth effect depicted in the center bar is of modest size, in line with our results in Table 3. However, the relative post-buyout employment performance of targets is highly sensitive to the evolution of market conditions. For example, a post-buyout decline in GDP growth by two standard deviations lowers the relative employment growth of targets by 7%. Changing credit spreads lead to a similar pattern in the lower panel. Excess reallocation rates at target firms are also sensitive to the post-buyout evolution of market conditions.

Tables 5 and 6 also report results by deal type. Recall that average buyout effects vary greatly by deal type (Table 3), and the mix of buyouts by deal type varies over the economic and

[19] Similar results obtain when using the change from the buyout closing date in year *t* to March of year *t+2*.

credit cycles (Figure 1 and Table 1). In line with remarks in Section I.B, Tables 5 and 6 provide evidence that the productivity effects are more sensitive to post-buyout macroeconomic and credit conditions for public-to-private than private-to-private deals, with divisional deals in the middle. In particular, when GDP grows faster or credit spreads narrow, the productivity growth of target firms is even higher (relative to controls) for the targets of public-to-private and divisional buyouts. A similar pattern holds for excess reallocation, except divisional buyouts show a greater sensitivity than public-to-private deals to post-buyout macroeconomic and credit conditions.

As articulated above, one explanation is that high leverage in public-to-private deals prevents management and investors from implementing pre-buyout operating plans when market conditions deteriorate or credit tights, with negative implications for productivity and reallocation. A similar dynamic may hold for divisional buyouts, which are likely to resemble public-to-private deals along important dimensions. Interestingly, the pattern goes the other way in private-to-private deals: deteriorating economic conditions or tighter credit conditions lead to greater productivity gains at targets relative to controls.

We do not find strong differences across buyout types in the responsiveness of target employment levels to post-buyout economic and credit conditions. For the most part, these interaction effects on target employment growth are statistically insignificant.

F. *Two Robustness Checks*

We now address two potential concerns about the forgoing analysis of market conditions. First, perhaps the results reflect our particular metrics for market conditions. Second, the results might be largely driven by the many buyouts undertaken in the run-up to the global financial crisis.

Table 7 addresses the first concern. Rather than looking at how buyout effects vary with a continuous measure of market conditions, we now take a simpler approach. Specifically, we interact the buyout indicator with a dummy for whether (a) the deal was executed during a recession or (b) the U.S. economy entered a recession in the two years after deal execution. Recession years are those for which at least half the months were part of NBER-designated recessions (i.e., 1981-82, 1990, 2001, and 2008-09). Table 7 reports these results for specifications and samples that parallel the ones in Table 4 and the "All Buyouts" columns in Tables 5 and 6.

The relative employment responsiveness of target firms to recessions is, if anything, stronger than when using continuous metrics. Both overall and organic employment growth at targets worsens (relative to controls) when the economy enters a recession after the buyout. Relative employment growth at targets is stronger for deals executed during a recession. Also, akin to results in Tables 5 and 6, deteriorating economic conditions post-buyout involve less reallocation at targets. Coefficients on the recession interaction variable in these cases are roughly equal to a three standard deviation shift in the continuous interaction variables in Tables 5 and 6. The productivity regressions, however, show smaller coefficients for the interaction variables and less statistical significance than obtained with continuous measures of economic conditions. Nevertheless, Table 7 indicates that our results continue to hold when using the recession indicator of market conditions rather than the continuous measures considered above.

Turning to the second concern, Figure 1 shows a huge surge in buyout activity in the quarters leading up to the GFC. Recall that the economy appeared strong in 2007 but then tumbled into a deep recession in 2008-09. To assess whether the runup in buyouts before the GFC drives our results, we re-estimated our models after dropping buyouts done in 2007. Table 8 repeats models considered in Tables 4, 5 and 6, but now omitting all targets and controls for buyouts in 2007. (Results are similar when also dropping buyouts in 2006.) By and large, the results are similar to before: Deals done amidst higher credit spreads show much greater productivity growth at targets. And, widening credit spreads and greater GDP growth after buyouts are associated with more reallocation at targets. The coefficients remain roughly the same size, but the responsiveness of target employment growth to economic conditions is weaker than before. In short, our results are not particularly driven by deals done in the run-up to the GFC.

G. *Market Conditions, or Deal Mix Changes over Time?*

Recall that public-to-private buyout volume is more pro-cyclical than that of other buyout types, especially private-to-private deals (Table 1). So, perhaps the greater job losses at target firms in public-to-private buyouts (Table 3) reflect a greater pro-cyclicality in their deal volume.

The sensitivity of our estimated buyout effects to market conditions could also reflect changes in the mix of PE sponsor characteristics over time. Gompers and Lerner (1999) and Kaplan and Schoar (2005), among others, show that the number of first-time funds is especially pro-cyclical. If the targets of young buyout groups have more adverse employment outcomes and are concentrated around market peaks, it could drive a cyclical pattern in our estimated effects of PE buyouts. More generally, a changing mix of active PE funds could drive time variation in the estimated PE effects. If true, that would be an interesting finding, but it would put our earlier results on the sensitivity of buyout effects to market conditions in a somewhat different light.

To explore these matters, we first undertook another large data collection effort to identify and characterize the PE sponsors of our nearly ten thousand buyouts. For 89% of the buyouts, we found information about the PE group in Preqin, Refinitiv, and other public sources. We assigned each PE group an identifier that follows the organization through spin-outs and name changes, as explained in Appendix C. We also gathered information about the organization type of the PE group, the number and dollar volume of its previous funds raised, and the group's historical track record (when available). We then merged these new data with our other data.

To analyze whether changes over time in the mix of buyout types and PE sponsor characteristics explain our results on how buyout effects vary with market conditions, we adopt a simple approach that lends itself to a useful decomposition, as we will explain. Specifically, for each buyout we create a "cell-adjusted" performance measure equal to the change from buyout year t to $t+2$ for the target minus the contemporaneous mean change for controls in the same cell (defined as before). We now dispense with controls for pre-buyout growth. We then sort observations by high and low values of a market conditions variable. Then we regress the cell-adjusted outcomes for buyout targets on a constant and the market conditions indicator, weighting buyout observations in the same way as before.

Panel A of Table 9 confirms that this simpler approach yields results very similar to the earlier ones on how target outcomes vary with market conditions. Specifically, relative target employment growth and reallocation rates increase when post-buyout GDP growth is high, and relative target productivity gains are greater for deals that close when credit spreads are high.

Next, we implement a Blinder-Oaxaca decomposition of the difference in relative target performance between high and low values of the market conditions variable. This type of decomposition has a long history in labor economics (Jann, 2008), but it can be readily applied to decompose the estimated difference between any two groups. In our application, the two groups are buyouts associated with high and low values, respectively, of a market conditions variable. For each subsample (i.e., the high-value and low-value observations), we regress the cell-adjusted buyout performance measure on indicator variables for buyout types and four measures of PE sponsor characteristics: the number of funds raised by the sponsor in the five years prior to its buyout of the target firm; the dollar amount it raised in the five years prior to the deal, divided by total U.S. PE fundraising in the same period; a dummy for whether the sponsor was independent, as opposed to a bank or corporate affiliate; and the number of buyouts undertaken by the sponsor in a five-year period around the deal in question. These measures quantify PE sponsor attributes related to the scale of its buyout activity, its fundraising success (a proxy for past performance), and its organization type.

The subsample regressions provide the ingredients of a Blinder-Oaxaca decomposition for the high-low difference in Panel A. We can express the decomposition as

$$(\bar{X}_H - \bar{X}_L)'\hat{\beta}_H + \bar{X}_H'\big(\hat{\beta}_H - \hat{\beta}_L\big) + (\bar{X}_H - \bar{X}_L)'\big(\hat{\beta}_L - \hat{\beta}_H\big), \qquad (3)$$

where $\bar{X}_H$ and $\bar{X}_L$ are vectors whose elements are the mean values of the explanatory variables in the "high" and "low" regression samples, respectively; and the $\hat{\beta}_H$ and $\hat{\beta}_L$ are the corresponding least squares regression coefficient vectors. The first term of (3) quantifies the contribution of changes in the mix of buyout types and PE sponsor characteristics to the high-low difference, the second term quantifies the contribution of market conditions, and the third term captures the interaction of between-group differences in the $\bar{X}$ and $\bar{\beta}$ vectors.

Panel B reports the decomposition results. The values in row (2) are statistically significant and roughly the same size as the corresponding high-low differences in Panel A. That is, the between-sample differences in the estimated coefficients largely account for the high-low differences in panel A. Moreover, for the employment growth rate and excess reallocation rate, the other two terms in the decomposition are small and statistically insignificant. Thus, for employment growth and excess reallocation, Table 9 confirms that buyout effects vary strongly with market conditions, *and* there is little role for temporal variation in the mix of buyout types and PE sponsor characteristics.

The message for buyout effects on productivity is murkier in two respects: the individual terms on the right side of (3) are imprecisely estimated because of the small sample, and the first and third terms are large and nearly offsetting. Our earlier claim that buyouts executed amidst tight credit conditions yield stronger productivity gains at targets still holds. However, we cannot say with any confidence whether, and to what extent, this result reflects time variation in PE sponsor characteristics or types of buyouts.

H. *Do Buyout Effects Differ across Private Equity Groups?*

Thus far, we have provided evidence that the real-side effects of PE buyouts differ with market conditions post buyout, with market conditions at close, by type of buyout, and with interactions between market conditions and buyout type. Another potential driver of heterogeneity in buyout effects are systematic differences among the PE sponsors themselves.[20]

As noted above, PE groups are characterized by management stability and distinct investment styles. In addition, persistence in financial performance has often been seen as a distinguishing feature of private equity groups, in contrast to hedge funds and mutual funds. (See Carhart (1997) and Brown, Goetzmann, and Ibbotson (1999) on hedge funds and mutual funds.) Studies that document persistence in the returns of PE groups include Kaplan and Schoar (2005), Ewens and Rhodes-Kropf (2015), Braun, Jenkinson and Stoff (2017), and Harris et al. (2020). We now investigate whether there are also persistent differences across PE groups in the employment effects of their buyouts. While it would be interesting to analyze persistence in productivity effects as well, we have too few buyouts with productivity data for an informative analysis.

To explore the impact of PE groups, Table 10 presents a series of regressions inspired by Kaplan and Schoar (2005) and Harris et al. (2020, and its 2014 predecessor). These authors examined persistence of financial performance at the fund level. Because it is hard for us to

[20] Table 9 speaks to whether PE sponsor characteristics explain *differences* in buyout effects between periods with high and low market conditions. This section and the next investigate whether PE sponsors and their characteristics influence buyout effects *on average*.

associate buyouts with particular funds, we instead aggregate all transactions associated with a given PE group in each of seven non-overlapping periods (1980-84, 1985-89, …, 2005-09, and 2010-11). As in the preceding section, we use the cell-adjusted employment growth rate over the two years after each buyout. For each period and PE group, we then compute the mean value of the cell-adjusted growth rates. We regress this period-by-PE group mean on its own lagged value (for the previous five-year period), dropping PE groups with buyouts in only one five-year period. We include time period dummies as well.

The results in columns (1) and (6) of Table 10 point to persistence over time at the PE group level in the employment effects of their buyouts. Persistence is much stronger, and statistically significant, for organic employment growth. The coefficient of 0.12 on lagged organic growth in regressions (6) through (8) compares to that of 0.17 in the public-market-equivalent buyout analysis of Kaplan-Schoar (2005; 8th regression in Table VII). This pattern supports the view that PE groups differ in their approach to operational improvements at target firms, leading to systematic differences in buyout effects on organic employment growth, while target-specific considerations influence decisions to acquire and divest. These results are robust to adding controls for the PE sponsor's volume of transactions in the five-year period and the change in its volume from the previous five-year period, as seen in columns (2), (3), (7) and (8).

The remaining columns contain two additional results. First, when we add an interaction between the date (expressed here as years since January 1960) and lagged employment growth (again at the group level), no evidence emerges of falling persistence in the group-specific growth effects. In contrast, the work of Harris et al. (2020) suggests that persistence in the financial performance of buyout funds dropped sharply after 2000. While PE groups may no longer show persistent differences in their ability to monetize their distinct approaches, our results say they continue to show persistent differences in how they affect target firms. Second, when we add firm fixed effects in columns (6) and (10), we obtain results similar to those in Table 9 of Harris et al. (2014): the coefficient on lagged performance turns sharply negative, which says there is regression to the (group-specific) mean in the employment growth rates of buyout targets.

I. *How Does Scaling at the Group Level Affect Employment in Portfolio Firms?*

Our final analysis investigates how scaling in buyout activity at the group level affects employment outcomes at targets. Previous research finds a detrimental impact of increasing fund size on fund manager returns – see, for example, Fung et al. (2008) for hedge funds (2008) and Chen et al. (2004) for mutual funds. Similarly, the work of Lopez-de-Silanes, Phalippou, and Gottschalg (2015) and Rossi (2019) suggests a negative relationship between the upscaling in buyout activity and the financial performance of PE groups.

Motivated by these earlier works, we investigate how the scaling of buyout activity by PE groups relates to the employment growth of their portfolio companies. To do so, we expand specification (1) to include variables that directly measure aspects of scaling or proxy for it, while also adding controls for buyout type. We consider four measures of scaling: funds raised by the PE group from $t-4$ to t for buyouts in t, divided by total buyout funds raised in the same period; financial performance of the group's last two buyout funds raised in the window from $t-12$ to $t-5$, calculated as returns as a multiple of invested capital (MoIC) minus the benchmark MoIC raised in the same period; the number of buyouts executed by the PE group in the five-year period (1980-84, 1985-89, and so forth) that contains the buyout year; and the change in the number of buyouts from the previous to the current five-year period. Appendix C explains how we constructed these scaling measures.

As shown in Table 11, upscaling in buyout activity at the group level involves lower post-buyout employment growth at target firms (again, relative to controls). The estimated upscaling effects are statistically significant at the 10 percent level for all scaling measures except for the change in the number of buyout deals. The estimated magnitudes differ a good deal. For example, a unit standard deviation increase in Adjusted Financial Performance involves a 4.4 percentage point decrease in the relative growth of organic employment at target firms, whereas a unit standard deviation increase in Funds Raised in Prior Five Years (Number of Buyouts in Current Five-Year Period) involves a decrease of only 0.7 (0.1) percentage points.[21]

In summary, upscaling in PE buyout groups is associated with a more negative employment impact on target firms, even after controlling for buyout type, the target's pre-buyout growth history and cell-level fixed effects. Of course, the scaling of PE groups is not exogenous (Rossi, 2019). In particular, past performance has a profound influence on the ability to raise new funds (Chung et al., 2012). Seen in this light, the results in Table 11 suggests that past success encourages a PE group to scale up its buyout activity, diluting the attention that key group personnel devote to individual portfolio companies. In turn, this dilution of attention leads to weaker employment growth among portfolio companies.

IV. Concluding Remarks

In his presidential address to the American Finance Association, Zingales (2015) makes the case that we "cannot argue deductively that all finance is good [or bad]. To separate the wheat from the chaff, we need to identify the rent-seeking components of finance, i.e., those activities that while profitable from an individual point of view are not so from a societal point of view."

[21] The larger implied effect for the Adjusted Financial Performance measure arises, at least in part, because its values are more highly dispersed across PE groups, as reflected in its relatively large coefficient of variation (ratio of standard deviation to mean).

Our study takes up that challenge for private equity buyouts, a major financial enterprise that critics see as dominated by rent-seeking activities with little in the way of societal benefits. We find that the real-side effects of buyouts on target firms and their workers vary greatly with market conditions, by type of buyout, across the private equity groups that sponsor buyouts, and with the sponsor's scale of buyout activity. To continue the metaphor, separating wheat from chaff in private equity requires a fine-grained analysis.

This conclusion cast doubts on the efficacy of "one-size-fits-all" policy prescriptions for private equity. Buyouts are associated with large productivity gains in many but not all circumstances. They are associated with large job losses in some circumstances and large job gains in others. This mixture of consequences presents serious challenges for policy design, particularly in an era of slow productivity growth (which ultimately drives living standards) and concerns about economic inequality.

There is a keen need to better understand the link between PE buyouts and productivity growth. Our evidence that buyouts executed amidst easy credit conditions bring smaller productivity gains suggests that PE groups exercise some latitude in how they create value for their investors. When credit is cheap and easy, PE groups may select buyouts – or structure them – to deliver private returns via financial engineering rather than operating improvements. Many PE groups were founded and seeded by investment bankers that historically relied on financial engineering to create private value, employing strategies such as repeatedly re-leveraging firms and dividending out excess cash (Gompers, Kaplan, and Mukharlyamov, 2016). In this light, it is unsurprising if PE groups de-emphasize operating improvements when leverage and dividends deliver high private returns. That said, our study provides evidence that buyout can, and often do, drive large productivity improvements in target firms. Policies that harness the power of PE buyouts to drive productivity gains can bring high social returns along with high private returns.

Our results reinforce some concerns about public-to-private deals, which account for 10% of PE buyouts from 1980 to 2013 and 31% of employment in target firms. In particular, public-to-private deals proliferate in advance of credit market tightening, and their targets exhibit poor productivity performance during aggregate downturns and when credit spreads widen.

Our study also points to several important outstanding questions: Do public-to-private and divisional buyouts cause avoidable employment losses? Or were targets in dire need of restructuring and retrenchment to prevent worse outcomes at a later date? More broadly, are job losses after certain types of buyouts essential to achieve post-buyout productivity gains and, if so, is the tradeoff an acceptable one? Does the pro-cyclical employment impact of buyouts reflect socially undesirable risk-taking by private equity or a preferred point on the risk-return frontier with social benefits in the form of high expected productivity gains? Resolving these questions is likely to require guidance from theory and novel identification techniques, but we hope our study helps pave the way to future research on these issues. Future studies that encompass more buyouts will be able to more fully examine the heterogeneous economic effects of buyouts.

Another important avenue for exploring these questions is to link private equity transactions to the Longitudinal Employee-Household Dynamics (LEHD) database. LEHD records on individuals will allow economists to study buyout effects on the compensation, unemployment spells, and employment trajectories of workers and to investigate spillover effects on local economies. We hope to pursue this research agenda in the years to come.

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Table 1. Market Conditions and Private Equity Buyout Frequency by Deal Type, Quarterly Data, 1980-2013

We regress 100 times the natural log of (type-specific PE buyout count) in quarter *t* on deal-type indicators interacted with market conditions at buyout close (top panel) and over the following two years (bottom panel), while controlling for deal type and a linear time trend. To characterize contemporaneous market conditions for buyouts that close in quarter *t*, we consider whether the credit spread in *t* is above or below its sample median value and whether real GDP growth from *t-4* to *t* is above or below its median. To characterize the evolution of market conditions over the next two years, we consider whether the change in the credit spread and real GDP from quarter *t* to *t*+8 are above or below their median values. After dropping quarter-type cells with no buyouts, each regression has 454 observations. In unreported results, we obtain very similar results when using the inverse hyperbolic sine transformation of the buyout count and retaining observations with zero buyouts. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Dependent Variable: 100**ln*(type-specific buyout count in quarter *t*)

	Coefficient on Market Conditions (row) interacted with Deal-Type Indicator (column)					
Market Conditions	Private to Private	Public to Private	Divisional Sales	Secondary Sale	R^2	Equality of Coefficients (p-value)
A. At Buyout Close						
High GDP Growth	28.2***	66.0***	41.2***	1.7		
	[9.5]	[16.1]	[15.6]	[14.4]	0.74	0.000
Wide Credit Spread	-20.7**	-26.6*	-18.1	-24.9*		
	[9.9]	[14.7]	[14.9]	[15.0]		0.019
B. Over Next 2 Years						
High GDP Growth	11.9	44.9***	52.3***	-40.7***		
	[11.2]	[14.7]	[16.3]	[15.3]		0.000
Widening Credit Spread	21.2*	67.8***	32.5**	20.0	0.75	
	[11.2]	[14.2]	[14.8]	[13.9]		0.000

Table 2. Summary Statistics for Private Equity Buyouts Matched to Census Micro Data

Panel A considers all matched targets in our 1980-2013 sample period. The first row in Panel B considers all matched targets in the 1980-2011 period, the second row excludes those matched using EIN numbers only, and the third row further restricts attention to "Two-year continuers," which include target firms that shut down all establishments by the second year after the buyout year. Panel C considers the same 1980-2003 period as the analysis sample in Davis et al. (2014).

	Number of Matched Buyouts (Target Firms)	Number of Target Establishments in the Buyout Year	Employment at Target Establishments in the Buyout Year
A. All, 1980-2013	6,000	177,000	6,890,000
Private-to-private	2,600	42,000	1,800,000
Public-to-private	600	67,000	2,130,000
Divisional Sales	1,300	25,000	1,120,000
Secondary Sales	1,300	31,000	1,280,000
Unknown Type	200	12,000	560,000
B. All, 1980-2011	5,100	164,000	6,400,000
After excluding EIN cases	4,500	144,000	5,690,000
Two-year continuers,	3,600	127,000	4,970,000
Private-to-private	1,800	32,000	1,450,000
Public-to-private	500	58,000	1,800,000
Divisional Sales	400	11,000	470,000
Secondary Sales	800	20,000	920,000
Unknown Type	100	6,000	330,000
C. All, 1980-2003	1,800	69,000	2,990,000
After excluding EIN cases	1,500	59,000	2,630,000
Two-year continuers,	1,200	49,500	2,210,000
Private-to-private	600	21,000	900,000
Public-to-private	200	16,000	690,000
Divisional Sales	200	5,000	210,000
Secondary Sales	150	3,600	180,000
Unknown Type	80	3,900	230,000

Table 3. Estimated Buyout Effects on Employment, Job Reallocation, and Productivity

The sample contains matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are (approximate) percentage amounts from the buyout year *t* to *t+2*. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a separate weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Appendix B for an explanation of how we weight observations. Results for "All Margins" include the contribution of post-buyout acquisitions and divestitures, while results for "Organic Margins" exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. The final column presents the p-value from F-tests of the equality of the coefficients of the four buyout type variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary		p-Value,
Dependent Variable	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	F-test
A. Employment Growth, All Margins	-1.4 [2.2]	0.32	12.8*** [2.5]	0.37	-12.6*** [2.9]	0.38	-11.5** [4.7]	0.32	9.9*** [2.5]	0.32	0.000
Organic Margins	-4.4** [1.9]	0.29	3.1** [1.5]	0.33	-10.0*** [2.4]	0.39	-16.0*** [4.2]	0.29	6.1*** [2.3]	0.31	0.000
B. Job Reallocation, All Margins	11.5*** [1.8]	0.39	11.7*** [2.7]	0.39	9.6*** [2.3]	0.45	19.4*** [4.5]	0.43	9.4*** [2.7]	0.39	0.638
Organic Margins	7.1*** [1.8]	0.39	2.5 [1.9]	0.44	6.2*** [2.0]	0.44	17.1*** [4.4]	0.41	6.4** [2.8]	0.41	0.032
C. Excess Reallocation, All Margins	5.0*** [1.1]	0.40	5.5** [2.3]	0.42	1.7 [1.6]	0.39	10.0*** [1.9]	0.44	7.1*** [2.4]	0.45	0.175
Organic Margins	0.6 [1.5]	0.35	-3.8 [3.4]	0.40	-1.7 [1.8]	0.36	7.6*** [2.3]	0.37	4.2 [2.8]	0.40	0.030
Observations (000s)	6,400		3,900		400		2,300		600		
D. Labor Productivity	7.5* [4.1]	0.47	14.7*** [4.5]	0.44	14.3 [11.1]	0.62	-5.0 [7.6]	0.38	0.7 [5.6]	0.43	0.080
Observations (000s)	911		411		17		620		40		

Table 4. How Buyout Effects Vary with Macroeconomic and Credit Conditions at the Close

This table considers the same outcome measures, estimation method and samples as Table 3, but we expand the regression specification to include market conditions at the buyout close and its interaction with the buyout indicator. We measure market conditions using the Credit Spread or GDP Growth variable defined in the text and consider them in separate regressions. For each outcome measure, the table entries report the estimated coefficient on the interaction variable, its standard error, and the coefficient multiplied by the standard deviation of the interaction variable, which ranges from 3.1 to 3.5 Credit Spread across samples and from 1.6 to 1.9 for GDP Growth. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.

		Interaction Variable	
Dependent Variable		Credit Spread	GDP Growth
A. Employment Growth, All Margins	Coefficient	0.28	-0.24
	[St. Error]	[0.77]	[1.28]
	Unit S.D. Effect	1.0	-0.4
Organic Margins	Coefficient	-0.12	0.14
	[St. Error]	[0.62]	[1.08]
	Unit S.D. Effect	-0.4	0.3
B. Excess Reallocation, All Margins	Coefficient	1.32***	-0.66
	[St. Error]	[0.45]	[0.69]
	Unit S.D. Effect	4.6	-1.2
C. Labor Productivity	Coefficient	5.86**	-3.58
	[St. Error]	[2.56]	[4.47]
	Unit S.D. Effect	20.3	-6.8

Table 5. How Buyout Effects Vary with the Credit Spread Change in the Two Years after the Buyout

The outcome measures, samples, weighting method and regression specifications in this table follow Table 3 except for two extra explanatory variables in each regression: the change in the credit spread in the two years after buyout close and its interaction with the buyout indicator. For each outcome measure, table entries report the estimated coefficient on the interaction variable, its estimated standard error, and the coefficient multiplied by the sample standard deviation of the Credit Spread. This standard deviation ranges from 4.3 to 4.9 across the regression samples. The final column presents the p-value from F-tests of the equality of the coefficients of the three reported buyout type variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.

Dependent Variable		All Buyouts	Private-to-private	Public-to-private	Divisional	p-Value, F-test
A. Employment Growth,	Coefficient	-0.57*	-1.04**	-0.64	0.62	0.083
All Margins	[St. Error]	[0.30]	[0.48]	[0.39]	[0.66]	
	Unit S.D. Effect	-2.8	-4.9	-2.6	2.1	
	Coefficient	-0.30	0.25	-0.51	0.36	0.003
Organic Margins	[St. Error]	[0.26]	[0.25]	[0.34]	[0.56]	
	Unit S.D. Effect	-1.5	1.2	-2.1	1.2	
	Coefficient	-0.64***	-0.19	-0.49*	-1.14**	0.127
B. Excess Reallocation,	[St. Error]	[0.18]	[0.22]	[0.25]	[0.46]	
All Margins	Unit S.D. Effect	-3.1	-0.9	-2.0	-3.9	
	Coefficient	-1.43	1.70*	-4.94**	-1.83**	0.014
C. Labor Productivity	[St. Error]	[0.91]	[1.01]	[2.18]	[0.83]	
	Unit S.D. Effect	-6.1	9.2	-25.7	-4.6	

Table 6. How Buyout Effects Vary with the GDP Growth Rate in the Two Years after the Buyout

The outcome measures, samples, weighting method and regression specifications in this table follow Table 3 except for two extra explanatory variables in each regression: the GDP Growth Rate in the two years the buyout close and its interaction with the buyout indicator. For each outcome measure, table entries report the estimated coefficient on the buyout-GDP interaction variable, its standard error, and the coefficient multiplied by the sample standard deviation of the GDP Growth Rate, which ranges from 3.4 to 3.6 across the regression samples. The final column presents the p-value from F-tests of the equality of the coefficients of the three reported buyout type variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.

Dependent Variable		All Buyouts	Private-to-private	Public-to-private	Divisional	p-Value, F-test
A. **Employment Growth**, All Margins	Coefficient	0.96*	0.28	-0.05	1.82	0.106
	[St. Error]	[0.54]	[0.67]	[0.72]	[1.14]	
	Unit S.D. Effect	3.2	1.0	-0.1	6.3	
Organic Margins	Coefficient	0.34	-1.21***	-0.04	1.18	0.000
	[St. Error]	[0.40]	[0.34]	[0.53]	[0.84]	
	Unit S.D. Effect	1.1	-4.2	-0.1	4.1	
B. **Excess Reallocation**, All Margins	Coefficient	0.88***	-0.56	1.03***	1.67**	0.064
	[St. Error]	[0.28]	[0.40]	[0.35]	[0.74]	
	Unit S.D. Effect	3.0	-1.9	2.8	5.8	
C. Labor Productivity	Coefficient	0.98	-2.29*	4.86*	2.68*	0.216
	[St. Error]	[1.17]	[1.23]	[2.65]	[1.55]	
	Unit S.D. Effect	3.6	-10.4	16.4	10.0	

Table 7. How Buyout Effects Vary with NBER Recessions

This table considers the same outcome measures, estimation method, and samples as Table 4 and the first columns of Tables 5 and 6, but we modify the regression specification (2) in the paper to instead include interactions between the buyout indicator and (a) the presence of an NBER-defined recession during the year of the buyout close (column 1) or (b) the inception of such a recession within two years of the transaction (column 2) instead of interactions with market conditions using the Credit Spread or GDP Growth variable. For each outcome measure, the table entries report the estimated coefficient on the interaction variable and its standard error. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.

		Interaction Variable	
Dependent Variable		Recession at Close	Recession Afterwards
A. Employment Growth, All Margins	Coefficient	10.75***	-10.85***
	[St. Error]	[3.19]	[3.87]
Organic Margins	Coefficient	11.57***	-7.36***
	[St. Error]	[2.67]	[3.17]
B. Excess Reallocation, All Margins	Coefficient	2.08	-12.11***
	[St. Error]	[3.25]	[3.82]
C. Labor Productivity	Coefficient	-5.82	-12.82
	[St. Error]	[6.51]	[9.61]

Table 8. The Impact of Excluding Deals Most Impacted by the Global Financial Crisis

This table considers the same outcome measures, estimation method, and samples as Table 4, and the first columns of Tables 5 and 6, but we modify the sample by excluding observations from 2007. We measure market conditions using the Credit Spread or GDP Growth variable defined in the text and consider them in separate regressions. For each outcome measure, the table entries report the estimated coefficient on the interaction variable and its standard error. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.

		Table 4		***Table 5***	***Table 6***
Dependent Variable		Credit Spread	GDP Growth	Credit Spread	GDP Growth
A. Employment Growth, All Margins	Coefficient	-0.12	0.93	-1.71	0.08
	[St. Error]	[0.80]	[0.96]	[1.21]	[0.68]
Organic Margins	Coefficient	-0.40	1.35**	-1.34	-0.66
	[St. Error]	[0.60]	[0.66]	[1.03]	[0.44]
B. Excess Reallocation, All Margins	Coefficient	0.51	-0.59	-1.25**	0.86**
	[St. Error]	[0.46]	[0.43]	[0.58]	[0.44]
C. Labor Productivity	Coefficient	4.71*	-0.29	-8.02	-1.92*
	[St. Error]	[2.51]	[1.66]	[4.89]	[0.98]

Table 9. Blinder-Oaxaca Decompositions. The sample contains matched two-year continuers that underwent PE buyouts from 1980 to 2011 with sponsors that can be linked to fund-level commercial data sources. For each buyout, we compute the outcome measure from buyout year *t* to *t+2* for the target firm minus the corresponding average value for control firms in the same cell defined by the full cross product of firm-age category, firm-size category, industry, multi-unit status, and buyout year. Panel A presents coefficients from a regression of the outcome measure on a dummy for being above the median in (a) GDP growth in the two years after the buyout or (b) the credit spread at deal close. Panel B presents Blinder-Oaxaca decompositions of the difference between high and low values of the market conditions variable. The decompositions are based on separate regressions of the outcome measure on buyout type indicators and PE sponsor characteristics in subsamples defined by high and low values of the market conditions variable. See text for the full variable list. We use buyouts in the high-value subsample as the reference group when implementing the decomposition. *** p<0.01, ** p<0.05, * p<0.1.

	Outcome Measure (Target Minus Average of Controls)		
	Employment Growth Rate	Excess Reallocation Rate	Productivity Growth
A. Simple regression fit to all buyouts in sample			
Coefficient on a Dummy for High GDP Growth Post-Buyout – the "High-Low Difference"	7.14***	3.60***	
	[1.56]	[0.74]	
Coefficient on a Dummy for High Credit Spread at Close – the "High-Low Difference"			13.85**
			[6.02]
B. Blinder-Oaxaca Decompositions			
(1) Contribution of changes in buyout types and PE sponsor characteristics to the High-Low difference in Panel A	1.81	0.77	12.99***
	[1.57]	[0.63]	[4.01]
(2) Contribution of changes in coefficients on buyout types and PE sponsor characteristics to the High-Low difference in Panel A	6.10***	3.22***	15.37*
	[1.76]	[0.79]	[9.00]
(3) Contribution of interactions effects to the High-Low difference in Panel A	0.77	-0.39	-14.52
	[1.64]	[0.70]	[8.89]
Observation Counts	3,900	3,900	500

Table 10. The Persistence of Buyout Effects on Employment Growth at the Level of Private Equity Groups. The unit of observation for the regressions in this table is the average cell-adjusted target outcome for the buyouts of a given private equity sponsor in a particular five-year period (1980-84, 1985-89,…). Results for "Total Employment Growth" include the contribution of post-buyout acquisitions and divestitures, while results for "Organic Employment Growth" do not. See text for a full description of the explanatory variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

	Average Total Employment Growth Rate					**Average Organic Employment Growth Rate**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lagged Average Employment Growth	0.0747	0.0744	0.0753	-0.0989	-0.3045***					
	[0.0461]	[0.0462]	[0.0462]	[0.3545]	[0.0747]					
Lagged Average Organic Growth						0.1229***	0.1226***	0.1226***	0.0504	-0.3174***
						[0.0465]	[0.0466]	[0.0464]	[0.3754]	[0.0842]
# of Buyouts in Current Five-Year Period		-0.0387***					-0.0361***			
		[0.0132]					[0.0117]			
Change in # of Buyouts from Previous to Current Five-Year Period			-0.4234					-1.292		
			[0.6443]					[0.8403]		
Time trend X Lagged Growth				0.3773					0.1569	
				[0.8043]					[0.8526]	
R^2	0.0128	0.0132	0.013	0.0131	0.7485	0.0283	0.0288	0.0307	0.0284	0.7725
Period fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
PE group fixed effects					YES					YES

Table 11. The Impact of Scaling at the PE Group Level on Employment Growth of Target Firms. The sample consists of matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status and buyout year. Some firms serve as controls for more than one buyout type. The dependent variables are percentage changes from the buyout year *t* to *t+2* in "Total Employment Growth," which includes the contribution of post-buyout acquisitions and divestitures, and "Organic Employment Growth," which excludes them. Each reported effect is the coefficient estimate [standard error] on a buyout dummy interacted with the indicated group-level scaling measure. See text for a full description of the scaling measures. The mean and standard deviation values of the scaling measures pertain to the set of all buyouts from 1980 to 2011 for which we could identify the PE sponsor and collect information about the sponsor. This set is larger than the set of buyouts in the regression samples, which also involve matching to Census data on targets and controls. We estimate a separate weighted least-squares regression for each column and report Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Scaling Measure for the Private Equity Group that Sponsored the Buyout	***Dependent Variable***							
	Total Employment Growth				Organic Employment Growth			
Funds raised in prior five years, normalized (Mean=0.0047, S.D.=0.0138)	-111.8*				-50.22*			
	[58.96]				[29.30]			
Adjusted financial performance of prior two funds (Mean=0.25, S.D.=1.12)		-7.32***				-3.97***		
		[1.66]				[0.93]		
Number of Buyouts in Current Five-Year Period (Mean=6.7, S.D.=8.5)			-0.0113*				-0.0122**	
			[0.0066]				[0.00556]	
Change in Number of Buyouts from Previous to Current Five-Year Period (Mean=0.43, S.D.=2.11)				-0.0709				-0.356
				[0.269]				[0.236]
R^2	0.396	0.260	0.394	0.393	0.355	0.236	0.355	0.355
Cell fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Deal type fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Controls for pre-buyout growth history of target firm	YES	YES	YES	YES	YES	YES	YES	YES

Figure 1. Quarterly Buyout Counts by Type, 1980 to 2013

Each panel shows buyout closings for the indicated deal type in quarter t, overlaid with the contemporaneous credit spread and the log change in real GDP from t-4 to t. We exclude about 300 buyouts that we cannot classify as to deal type. See Section I.A for an explanation of how we construct our sample of 9,794 leveraged buyouts sponsored by private equity firms.

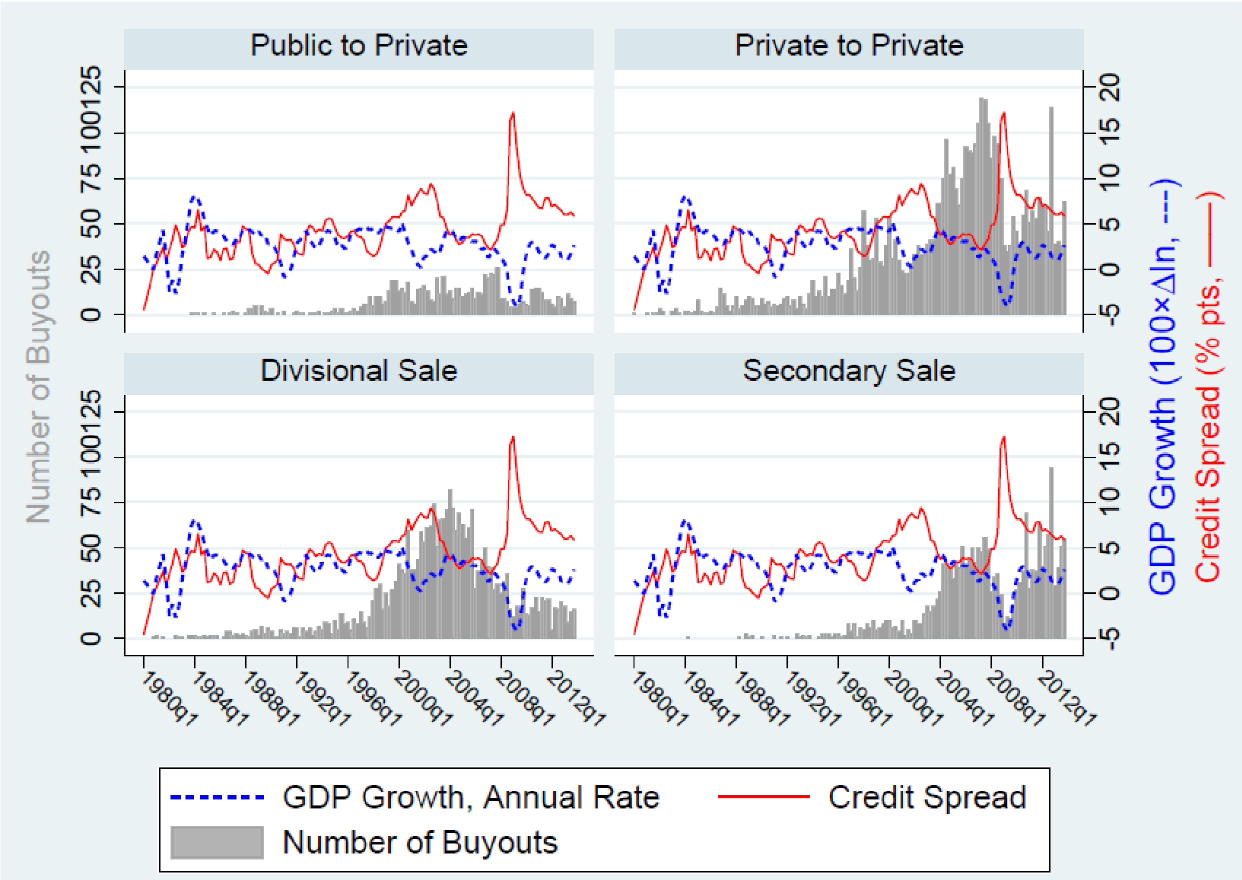
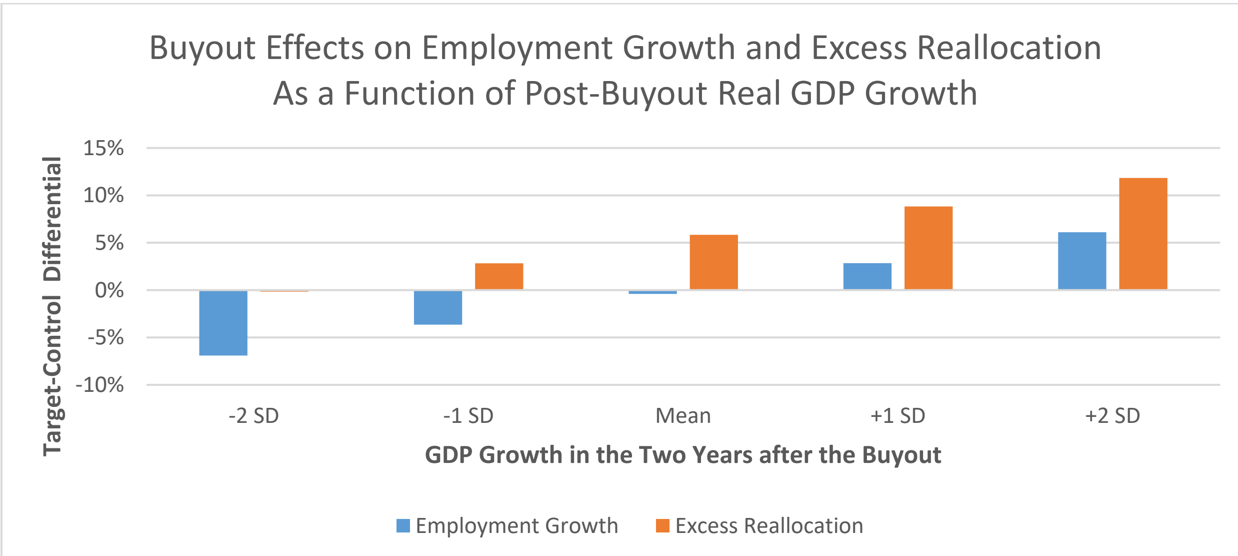
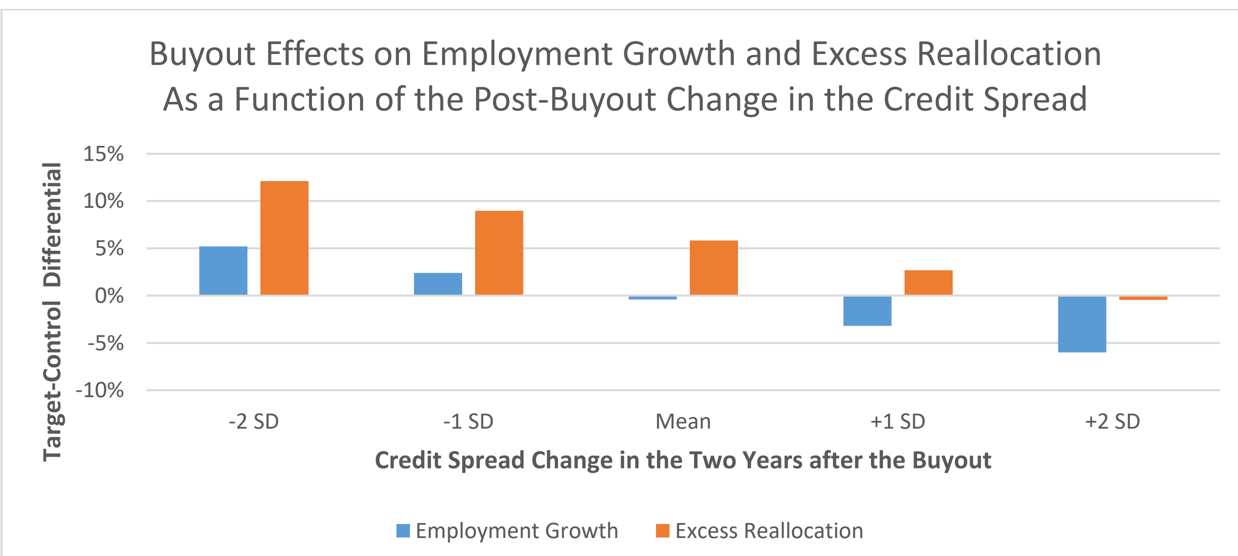


Figure 2. How Buyout Effects Vary with the Post-Buyout Evolution of Market Conditions
This figure uses the estimated interaction effects in Tables 5 and 6 to depict how the post-buyout employment growth rate and excess reallocation rate at targets (relative to controls) vary with the post-buyout evolution of market conditions. The center bars show the estimated target-control differential when evaluating at the sample mean of the market condition measures. The other bars show the target-control differential when evaluating the market condition measures at -2, -1, +1, and +2 standard deviations below or above their respective sample means.





Internet Appendix A: Sample Construction and Matching

1. Overview

We combine information on private equity buyouts from CapitalIQ and other sources with firm-level and establishment-level data held by the U.S. Census Bureau.

Specifically, we undertook a two-part effort, following Strömberg (2008). The first part drew on the CapitalIQ database to create a base sample of PE-sponsored leveraged buyouts. We selected all M&A transactions in CapitalIQ classified as a "leveraged buyout," "management buyout," or "JV/LBO" (joint venture/leveraged buyout) that closed between January 1, 1980 and December 31, 2013. To this sample, we added all M&A transactions undertaken by a financial sponsor classified as investing in "buyouts." We excluded management buyouts not sponsored by a PE firm and startup firms backed by venture capitalists. Although CapitalIQ has back-filled its database using various sources since starting its data service in 1999, its coverage remains incomplete in the early years of our sample. For this reason, the second part of our sample construction efforts relied on other databases,[22] the business press, and buyout lists for the 1980s compiled by other researchers.

The overlap between our initial sample of PE buyouts and lists of LBOs with a financial sponsor compiled by other researchers is high. For instance, 62 of the 77 buyouts in Kaplan's (1989) hand-selected sample of LBOs completed between 1980 and 1986 are captured by our CapitalIQ sample, a coverage rate of 81%. We added these 15 missing buyouts to our sample, as we did for other PE buyouts identified using various lists and other sources beyond CapitalIQ.

In the course of our investigations, we discovered that CapitalIQ classifies certain buyout fund transactions as "private placements" rather than acquisitions. In most cases, these private

[22] These include Dealogic, Preqin, and Thomson Reuters.

placements involve minority stakes or follow-on investments and, hence, are unsuitable for inclusion in our sample. Still, the distinction between buyouts and private placements is not always clear. In addition, some transactions reported as LBO deals were actually venture capital investments, which are not the object of our study. We sought to err on the side of caution by excluding ambiguous transactions and, as a result, may miss some bona fide LBOs.

We also excluded acquisitions not yet completed by the end of 2013, acquisitions of non-control stakes (typically associated with growth and venture deals, not classic buyouts), purchases of firms with foreign headquarters, stakes in public companies that remained publicly traded (PIPES), and other misclassified transactions. We identified these transactions through the careful review of text fields in CapitalIQ records and our own detailed research using other commercial databases, securities filings, and media accounts.

We then match these buyout deals to target firms and their establishments in the Census Bureau's comprehensive Business Register (BR). Our basic approach is as follows. First, we use name and address information to match a particular deal to a specific unit in the BR. Because the matching algorithm relies partly on address information, this step identifies a specific establishment owned by the target firm, which is often but not always a headquarters facility. Second, we use the BR link between that establishment's ID and its parent firm ID to identify the target firm in the BR. In most cases, this method identifies the target firm in the BR and all of its establishments.

We describes our matching process in detail below. The process yields a mapping to one or more firms in the BR for about 7,600 of the 9,794 U.S. buyouts that we identified from CapitalIQ and other sources. Of these 7,600 buyouts, about 4,100 match to BR identifiers for a single firm, while the other 3,500 map to identifiers for multiple firms. We resolved about 2,000 of these 3,500 cases to a unique match, leaving about 6,000 buyouts that we confidently match to a unique firm in the BR in the period from 1980 to 2013. The approximately 6,000 matched target firms acquired in PE buyouts from 1980 to 2013 operated about 177,000 establishments as of the buyout year and had nearly 7 million workers on their payrolls as of March in the buyout year.

The main reason we cannot confidently resolve the other 1,500 cases to a unique firm in the BR is because many targets undergo a complex reorganization during the buyout or shortly thereafter. The reorganization can involve the sale of multiple firm components to multiple parties, the emergence of multiple new firm IDs, and the introduction of a complex array of holding company structures. These cases present considerable matching challenges. Rather than include matches of dubious quality, we exclude them from our analysis.

Once matched to the BR, we can identify establishments owned by the target firm as of its buyout year. LBD longitudinal links let us compute employment changes for establishments and firms and track their entry, exit, and ownership changes. We supplement the LBD with firm-level revenue data drawn from the Census BR to obtain a revenue-enhanced version of the LBD (RE-LBD). The revenue data, available from 1996 to 2013, let us study the impact of PE buyouts on labor productivity, defined as real revenue per worker. About 20 percent of LBD firm-year observations cannot be matched to BR revenue data because firms report income under EINs that fall outside the set of EINs that Census considers part of that firm for employment purposes**.**

Treatment of Timing Matters

Given our interest in employment dynamics, the relationship of the LBD employment measure to the timing of PE buyouts requires careful treatment. The LBD reports total employment in the payroll period containing the week of March 12. Accordingly, for buyouts that close before October 1, LBD employment in March of the same calendar year serves as our contemporaneous employment measure. We assign buyouts that close on or after October 1 in calendar year *t* to the LBD employment value in March of *t+1*. October is the natural cutoff because it lies midway between March-to-March employment changes in the LBD.[23]

Henceforth, our references to buyout activity in year *t* refer to deals that closed from October of calendar year *t-1* through September of calendar year *t*. In particular, buyouts that closed in October, November or December of 2013 are shifted forward to 2014, beyond the time span covered by our LBD data. As a result, these matched targets are not part of our analysis.

Tracking Firms after the Buyout and Forming Our Analysis Sample

Of necessity, much of our analysis restricts attention to target firms that we can track after the buyout. While we can readily track establishments over time in the LBD, tracking firms is more challenging for two main reasons: the disappearance of firm identifiers (IDs) and irregularities in Census Bureau tracking of PE targets involved in certain divisional sales. We elaborate on these two reasons in turn.

Firm ID Disappearance. The disappearance of a firm ID in the LBD can occur for various reasons. One is the death of a firm and the closure of all of its establishments. Firm death in this sense presents no problem: we capture such events whether they involve target or control firms. A more difficult situation involves a target firm ID that vanishes in the first or second year after the buyout, even though some of its establishments (as of the buyout year) continue to operate. This situation can arise when the various components of the original firm are acquired by multiple firms. It is inherently difficult to define and measure firm changes when the original legal entity ceases

[23] Fractional-year mistiming of buyout deals is unavoidable when matching to the LBD, given its annual frequency. When buyouts are uniformly distributed over the year, an October cutoff minimizes the mean absolute mistiming gap. See Davis et al. (2018) for additional discussion. As an empirical matter, buyout closing dates are distributed fairly evenly over the calendar year.

to exist and has no obvious successor. We exclude these cases from our firm-level longitudinal analyses. To reduce the number of observations lost for this reason and other challenges in tracking firms over time, we restrict our longitudinal analyses to the buyout year and the next two years.

Divisional Buyouts. In principle, the annual Company Organization Survey lets Census accurately track the business units involved in divisional sales. However, we discovered divisional sales in which the firm ID of the (new) target firm remained the same as the firm ID of the selling firm. This situation indicates that the new firm created in the course of the divisional buyout did not receive a new firm ID, at least not in a timely manner. This problem does not preclude an establishment-level analysis, because we can often use an alternative identifier – the Employer Identification Number (EIN) – to accurately identify, as of the buyout year, the establishments involved in divisional sales. Unfortunately, EINs are unsuitable for tracking firms through time, because new and acquired establishments may obtain new EINs. Thus, we exclude divisional buyouts from our firm-level longitudinal analyses when the LBD lacks an accurate firm ID for the newly created target firm. We exclude some secondary buyouts for the same reason.

After matching to the BR, we use the Longitudinal Business Database (LBD) – essentially a longitudinal version of the BR – to follow target firms and their establishments over time. We also use the LBD to identify control units (comparable firms and establishments) and to follow them over time as well. In addition, we exploit common alphanumeric identifiers to incorporate other Census micro data for some aspects of our analysis.

The LBD tracks establishments and parent firms using a combination of administrative records and survey collections that include the Company Organization Survey (COS), the Economic Censuses, and the Annual Surveys of Businesses (e.g., the Annual Survey of Manufactures). Information about company structure is incorporated into the LBD by attaching firm identifiers to records for establishments. Ownership changes are identified when establishments switch parent firms through mergers, acquisitions, and divestitures.

The Census Bureau assigns a unique firm ID to all establishments under common ownership and control in a given year, including establishments that belong to subsidiaries under control of the parent corporation. This firm ID is distinct from a taxpayer ID such as the employer identification number (EIN).[24] The relationships among the various IDs are as follows. In any given year, an establishment is uniquely associated with a single taxpayer ID and a single firm ID. Moreover, each taxpayer ID is uniquely associated with a firm ID. For multi-establishment firms, a parent firm ID has multiple affiliated establishment IDs and potentially multiple EINs. Put differently, the EIN as a unit of observation is somewhere between an establishment and a firm.

2. Matching Buyout Targets to the Business Register (BR)

From Capital IQ and other sources, we obtain several pieces of information about the acquired entity in a private equity buyout. These pieces include the name of the seller, the name of the acquisition target, the target's address, and the acquisition date. The seller and target are typically the same in whole-firm acquisitions but not in partial-firm acquisitions – for example, when the private equity firm acquires one division of a multi-division company.

We match acquisition targets to firms in the BR using the data matching algorithms that are part of the SAS DQMatch procedure. This is an improved version of the matching algorithm and code we used in Davis et al. (2014). Our DQMatch implementation proceeds through 16 rounds of matching from the strictest criteria (requiring a perfect match on name and address) to progressively looser criteria that allow for fuzzier matching (exact name and fuzzy address, fuzzy

[24] The EIN is an employer tax identifier that may or may not change when ownership changes. It is often helpful in matching and tracking target firms and establishments involved in complex reorganizations.

name and exact address, exact name and zip code, etc.) Results from each pass are flagged and the results are stored for use in later analyses. For brevity, we do not discuss the DQMatch matching criteria and the algorithm used to identify matches in detail.[25] Here, we describe our overall matching strategy, explain how we resolve buyout deals that match to multiple target firm candidates in the BR, and discuss issues that arise in tracking firms over time.

A. A Simple Case

Suppose a private equity firm acquires firm A in its entirety during year *t* and places it under new ownership, possibly with a new name. A simplified version of our matching algorithm in this case works as follows: First, we find an establishment in the BR as of year *t* located at the target address and owned by a firm with the target name. Second, with this match in hand, we use the firm-establishment links in the BR to identify the full set of establishments operated by the target firm in *t*. From this point, we can measure the activity of the target firm in *t* and follow the firm (and its establishments) forward from *t* using the LBD.

B. Challenges that Arise in the Matching Process

In practice, several challenges arise in the matching process. First, because name and address data are noisy, we may find multiple BR firms that are candidate matches for the acquisition target.[26] All but one of these candidates, and perhaps all of them, are false positives.

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[25] Programs to implement the DQMatch algorithm and master batch files to run them are available on the computing cluster servers in the Federal Statistical Research Data Centers.

[26] We use both physical and mailing address from the Business Register when available to generate matches. There is some noise in the addresses for new units in the Business Register that is typically resolved in an Economic Census. Our use of a multi-year window helps to partly overcome this source of noise. However, we did not find that our match rates peaked in Census years, suggesting that business name clarification in Economic Census years is not a big issue for our purposes.

Second, to cope with timing differences between datasets, we search for matches in the BR over a three-year window centered on the buyout year. While this approach can pick up good matches that we would otherwise miss, it can also introduce additional false positive matches. Whenever we have multiple candidate matches, we need some way to resolve to a unique match. When we cannot do so with sufficient confidence, we drop the acquisition target from our analysis.

Third, it can be hard to distinguish the seller firm from the acquisition target in some cases. For example, suppose a private equity firm acquires establishments e_1 and e_2 from firm A to form a new firm B in year *t*. In this case, the activity of establishments e_1 and e_2 are associated with both firms A and B in *t*, because each firm files tax records that cover e_1 and e_2 for part of the year. Thus, when we match the target address to an establishment, that establishment may link to two parent firms in the BR in the buyout year. In this situation as well, we need some way to resolve to a unique match.

Fourth, some private equity buyouts involve complex reorganizations of target entities that lead to the creation of multiple new firms or the piecemeal sale of the target entity to multiple parties. In these cases, even when we successfully match the target address to an establishment and correctly identify that establishment's parent firm, we may identify and track only some of the establishments acquired as part of the buyout. Indeed, there can be multiple true successor firms to the target entity in such cases, and we may capture and track only one of them.

Fifth, another challenge involves divisional buyouts, whereby the private equity firm acquires only part of a multi-division firm. For divisional buyouts, we could not always identify the correct target firm in the BR after matching the deal to a specific establishment. These instances arose because, in some cases, the Census firm ID associated with the matched establishments did not change to reflect the ownership change of the division involved in the buyout deal. We identified these problematic cases by observing that the matched target establishment remained affiliated with the parent seller firm even after the buyout. It is our understanding that the Census Bureau on occasion had difficulty tracking the new firm in divisional buyouts because of nonresponse on the COS or other survey instruments.

We thus had two types of divisional cases. The first are those where we could accurately identify the target firm using our main method, and the second where we could not. Even in those cases, we were able to link the matched establishment to at least a part of the target firm through the EIN (taxpayer ID). The complete target firm may or may not be identified in such cases, because the divisional business involved in the buyout may have operated with multiple EINs. In the main text and this appendix, we refer to such cases as EIN cases. In these EIN cases, we can accurately identify a part of the target firm in the buyout year and at least some of the corresponding target establishments, but we cannot be confident that we captured the entire target firm. We exclude EIN cases in our firm-level longitudinal analyses, because the EIN is not suitable for tracking firms over time. For example, if a target firm (i.e., an EIN case) creates or acquires a new establishment, it may obtain a new EIN for that establishment for accounting or tax reasons. In such cases, we would not know that the new establishment is part of the target firm.

C. How We Proceed

As explained above, our matching algorithm may initially yield zero, one or multiple candidate matched firms in the BR for a given buyout target. We now provide information about the frequency of these outcomes and describe our process for de-duplicating buyouts that match to multiple Census firm IDs.

<u>No Match</u>

In about 2000 of the 9794 deals in CapitalIQ, no companies within the BR matched even using the loosest matching criteria. Here and below, we provide rounded figures for counts of matched Census firms because of data disclosure restrictions.

Unique Matches

As noted above, we search for candidate matches in the BR over a three-year window centered on the buyout year, *t*. First, we select a year (*t-1*, *t* or *t+1*) in the three-year window for the buyout in question. Second, given the year, our algorithm proceeds through 16 rounds using progressively less stringent matching criteria. Third, if we obtain at least one candidate match in a given round, we do not proceed to later rounds for that year. For example, suppose a buyout target matches to a single BR entity in round 4 of our algorithm for year *t*. Even if the target firm matches to other BR entities in later rounds (which involve less stringent criteria), we stop in round 4 for year *t*. This process can lead to one or more candidate matches in each of *t-1, t* and *t+1.*

For about 4,000 of the 9,794 buyouts that we identified using CapitalIQ and other sources, the process described in the preceding paragraph yields a single match candidate. That is, the process yields at most one candidate in each of *t-1, t* and *t+1;* and, moreover, when it yields a candidate match in two or three of the years, it is the same firm in each year.

Non-Unique Matches and De-Duplications

The remaining set of about 3500 buyout deals match to multiple BR entities. This could happen, for example, if we find an exact match on address, but there are multiple firms in a single building with similar company names in the same year. As another example, Census often redefines the target firm's firm ID after the buyout. When it does, we often detect two match candidates within our three-year window centered on the buyout year: one match to the pre-buyout firm ID, and one to the post-buyout firm ID. We use three methods to arrive at a unique match between the buyout target and the Census firm ID in these and other cases that yield multiple candidate matches.

The first method for de-duplicating is to check the EINs of the match candidates. For about 25 percent of the duplicates, multiple match candidates have the same EIN. That tells us that each match candidate is owned by the same parent firm, and we proceed on that basis. This method is especially helpful in resolving duplicates that arise when Census changes the firm ID associated with the firm in question within the three-year centered window around the buyout transaction.

The second method for de-duplicating is to exploit the timing pattern of the matches. We consider cases with two candidate matches for the same deal. A common pattern in such cases is that one candidate is the birth of a new firm ID at time t or $t+1$, and the other candidate is a death at time $t-1$ or t. In this context, a "birth" is when a new firm ID appears at time t or $t+1$, one that did not appear earlier (in $t-1$ for births in t, or $t-1$ and t for births in $t+1$). A "death" is when a firm ID disappears in time t or $t+1$. We investigated cases that fit this pattern and determined that they likely reflect PE-precipitated reorganizations. Since these candidate matches satisfy name and address matching criteria, they are unlikely to be spurious. This second step uniquely resolves about 200 additional firm IDs in the BR to a particular target firm in a PE buyout.

If the first and second methods do not yield a unique match, we deploy a third method as follows. First, for the set of candidate matches, rank firm IDs by the strictness of the criteria that generated their inclusion as match candidates. Then create three flags:

- Set Flag 1 to 1 for those firm IDs with the highest rank among the match candidates. If there are two candidate matches, for example, one for year $t+1$ with an exact name and address match and one for year t that matches exactly only on the name, set Flag 1 to 1 for the one that matches exactly on both name and address.

- Among candidate matches with the highest rank, set Flag 2 to 1 for firm IDs that are present in year *t+1*.
- Among candidate matches present in year *t+1*, set Flag 3 to 1 for firm IDs that achieve the highest rank.

If one, and only one, firm ID satisfies Flag 1 = Flag 2 = Flag 3 =1, we treat that firm as the true match and use it in our analysis. This three-flag method resolves about 1000 additional buyouts to a Census firm ID. Altogether, our three resolution methods yield about 2000 additional matched deals. This gives us the total sample of approximately 6000 matched buyout deals.

3. Tracking Firms and Establishments after the Buyout

As explained in the main text, we cannot always track target firms with confidence in the years after the buyout. Tracking difficulties can arise because (a) a target is broken into many pieces, some or all of which are re-sold to other firms, and (b) errors and ambiguities in Census data prevent us from following the firm with confidence after the buyout. Thus, our econometric analysis in Section III examines the sample of "Two-Year Continuers" that we track with confidence. Our concept of "Continuers" includes firms that die in the sense that all of its establishments in the buyout year *t* cease to operate by *t*+2.

Tracking establishments in Census data is typically much easier than tracking firms. However, even establishments are challenging to track in certain limited circumstances. Every five years, the Census Bureau obtains a full list of establishments owned by multi-unit firms from the Economic Censuses. It obtains a full list of establishments owned by large multi-unit firms (250 or more employees before 2013) from the annual Company Organization Survey (COS). The COS also samples smaller multi-unit firms in a targeted manner based on information that they underwent rapid growth or organizational change. When this information is incomplete, Census may not promptly recognize new establishments operated by small, multi-unit firms in intercensal years. To address this matter, the LBD retimes the intercensal entry and exit of some establishments operated by small multi-unit firms. Still, the timing of M&A activity for small multi-units not covered by the COS or other Census surveys exhibits some bunching in Economic Census years. We do not think this limited bunching is a serious concern for our analysis, in part because small units get little weight in our employment-weighted regressions.

Internet Appendix B: Empirical Methods and Identification Assumptions

This appendix provides details about several aspects of our empirical methods. The first relates to how we track business outcomes over time. While we focus on firm-level outcomes, we exploit the establishment-level data in the LBD in several ways: to distinguish organic changes at the firm level from acquisitions and divestitures; to capture new facilities opened after the buyout; and to decompose firm-level employment changes into the gross job creation and destruction components associated with growing and shrinking establishments, respectively. The LBD's capacity to isolate each of these adjustment margins is one of its major strengths.

A second aspect relates to aggregation and the measurement of growth rates. Let E_{it} denote employment at establishment or firm i in year t – i.e., the number of workers on payroll in the pay period covering March 12. We measure the employment growth rate of unit i from $t-k$ to t as $g_{it,t-k} = (E_{it} - E_{i,t-k})/X_{it,t-k}$, where $X_{it,t-k} = 0.5(E_{it} + E_{i,t-k})$. This growth rate measure is symmetric about zero and lies in the interval [-2, 2], with endpoints corresponding to death and birth.[27] Employment growth at higher levels of aggregation is then given by $g_{t,t-k} = \sum_i (X_{it,t-k}/X_{t,t-k}) g_{it,t-k}$, where $X_{t,t-k} = \sum_i X_{it,t-k}$. Using these formulas, we can easily and consistently aggregate from establishments to firms, from individual units to industries, and over time periods. This approach to growth rates and aggregation also works for gross job creation and destruction, job reallocation, and employment changes along particular dimensions such as acquisitions and divestitures or continuing establishments.

[27] This growth rate measure has become standard in analyses of establishment and firm dynamics, because it shares some useful properties of log differences while also handling entry and exit. See Davis, Haltiwanger, and Schuh (1996) and Törnqvist, Vartia, and Vartia (1985).

A third aspect relates to the selection of control units for comparison to buyout targets in our regression models. We need suitable control units because the distribution of PE buyouts across industries and business characteristics is not random. Target firms are larger and older than the average firm and disproportionately concentrated in manufacturing, information technology, accommodations, and food services (Davis et al., 2014). They also differ by deal type, as shown above. Moreover, growth and volatility vary greatly by firm size and age, and workplaces and technologies differ greatly by industry.[28] Hence, we sort target firms into cells defined by industry, size, age, multi-unit status, and buyout year. We then identify all firms not backed by private equity that fall into the same cell as the given target firm(s), and treat those firms as control units for the target firm(s) in that cell. Specifically, we define our control cells as the full cross product of about 90 industries (at the three-digit NAICS level), ten firm size categories, six firm age categories, a dummy for firms with multiple establishments, and 32 distinct buyout years from 1980 to 2011.[29] This classification yields over 10,000 control cells per year. Of course, many cells are unpopulated, but the flexibility and richness of our approach to control units is clear.

Fourth, we estimate the effects of buyouts using a difference-in-difference approach. That is, we compare changes in jobs and productivity at target firms in the wake of buyouts to

[28] Much previous research highlights sharp differences in employment growth and the pace of job reallocation by firm size, firm age, and industry. See, for example, Davis, Haltiwanger, and Schuh (1996) and Haltiwanger, Jarmin, and Miranda (2013).
[29] We define industry for multi-unit firms based on the modal industry of their establishments, computed on an employment-weighted basis. Our firm size categories are 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500-4999, 5000-9999, and 10000 or more employees. Our firm age categories are 0-5 years, 6-10, 11-15, 16-20, and 21 or more years. Following Davis et al. (2014), when a firm first appears in the LBD, we assign it the age of its oldest establishment. We then increment the firm's age by one year for each year it continues as a legal entity in the LBD. In this way, we avoid arbitrary increases or decreases in firm age due to the sale and purchase of establishments.

contemporaneous changes at their matched control units.[30] This approach, together with our control variables, facilitates an apples-to-apples comparison when estimating buyout effects.

A fifth aspect pertains to how we weight observations in the estimation. In this regard, we are mindful that buyout effects can vary with firm characteristics and economic conditions and by industry, deal type, and time period. Indeed, the main text documents material differences in the effects of buyouts on these dimensions. However, there is surely more heterogeneity in treatment effects than we can estimate with precision. Faced with this heterogeneity, our goal is to obtain a consistent estimate for the activity-weighted mean treatment effect on treated units under two common identification assumptions in regression studies of treatment effects:

- **CMI** (conditional mean independence): Conditional on controls and the treatment indicator, outcomes for treated and non-treated units are independently distributed within cells.
- **SUTVA** (stable unit treatment value): Treating one unit has no effect on the outcomes of other units.[31]

To achieve our estimation goal, we adopt two principles in weighting the observations:[32]

- **TS** (target-share weighting)**:** Weight each target (and each target cell) by its share of aggregate target activity, where "aggregate" refers to the sum over all buyouts in the regression sample.
- **SCT** (set control weights to targets): Set the sum of weights on controls in a given cell to the cell's target activity share.

[30] In Davis et al. (2014), we find that propensity score matching estimators yield very similar results. We stick with the control cell approach in this paper for simplicity.
[31] See Chapter 18 in Wooldridge (2002) for an extended discussion of CMI and SUTVA in panel regression studies of treatment effects.
[32] Neither equal weighting nor simple activity weighting of regression observations recovers the average treatment effect of interest.

To be precise, suppose we have two target firms in two separate control cells, and we are interested in target-control comparisons from t to $t+k$. The targets have activity levels $X_{1,t+k,t} = 0.5(E_{1,t+k} + E_{1t})$ and $X_{2,t+k,t} = 0.5(E_{2,t+k} + E_{2t})$. The first target's share of aggregate target activity is $\omega_{1,t+k,t} \equiv X_{1,t+k,t}/(X_{1,t+k,t} + X_{2,t+k,t})$, and the second's share is $\omega_{2,t+k,t} \equiv X_{2,t+k,t}/(X_{1,t+k,t} + X_{2,t+k,t})$. Since each control cell has a single target, these are also the control cell weights.[33] Principle SCT requires $\sum_{j}^{\mathbb{C}=1} \omega_{j,t+k,t} = \omega_{1,t+k,t}$ and $\sum_{j}^{\mathbb{C}=2} \omega_{j,t+k,t} = \omega_{2,t+k,t}$, where $\mathbb{C}$ indexes control cells, and j indexes control units in the cell.

Principle TS helps recover an average treatment effect that reflects the distribution over cells of target activity levels. Principle SCT has a similar motivation**.** It also ensures that the influence of control units on the coefficient estimates for covariates reflects the distribution over cells of target activity levels. Principle SCT is silent on exactly how to set control unit weights within cells, as long as they sum to the cell's share of aggregate target employment. In practice, we weight each control unit in proportion to its share of employment among the control units in the cell. After obtaining these proportions, we rescale them to satisfy SCT. We experimented with other approaches to weighting control units that comply with SCT. In particular, we tried equal weights for all control units within a given cell. We also tried winsorizing the weights of very large control units before rescaling to comply with SCT. These alternative approaches to weighting control units led to results similar to the ones reported below.[34]

[33] Note that we define a unit's activity level as the average of its employment at the start and end of the time interval under consideration. This practice conforms to our overall approach to aggregation and growth rate measurement, as discussed above.

[34] A subtle issue with weighting had to do with divisional buyouts, where one unit is spun out of a larger entity. Here we use employment in the spun-out entity after the buyout transaction, not that of its pre-buyout corporate parent.

Three concerns motivated our experimentation with alternative schemes that give less weight to larger control units, while still adhering to principle SCT. First, very large employment values for certain control units could reflect measurement error. This concern might apply to targets as well, but since our sample has only a few thousand targets, we scrutinize them carefully. We believe we have identified (and corrected) gross errors in target outcomes. A similarly careful approach for controls is infeasible, since there are so many of them. Second, it is often hard to fit very large firms into a particular industry category, even at the three-digit NAICS level. The classification challenges presented by such large firms raise concerns about the suitability of the treatment-control comparison. Third, the very largest control firms can be much larger than the corresponding target firm. The vast difference in size raises a different source of concern about the suitability of the treatment-control comparison. By applying equal weights to control units in a given cell or winsorizing the weights, we mitigate these concerns.

Recall that we aim to recover the average treatment effect on the treated (buyout) firms under CMI and SUTVA. A standard approach, which we took in Davis et al. (2014), is to fit a regression model with heterogeneous treatment effects, average over the treatment effect estimates, and compute the standard error for the average treatment effect by the delta method. (See Chapter 18 in Wooldridge, 2002.) Weighting principles TS and SCT afford a simpler econometric approach that recovers the average treatment effect of interest from a specification with a homogenous treatment effect. Under this simpler approach, we need not resort to the delta method to obtain standard errors. We can instead obtain them directly from the standard output for weighted least squares regressions in STATA and other widely used statistical packages. That is the approach we take in this paper.

Internet Appendix C: Creating the Private Equity Group-Transaction Sample

This appendix describes how we supplemented our database to capture information about the PE groups (also referred to as general partners, or GPs) that sponsor buyouts.

We transformed our original data for present purposes from being at the buyout level (as in Tables 3 through 8) to being at the buyout-PE group level. Thus, a single buyout transaction identifier may have multiple entries (with the same CapitalIQ transaction identifier), if there is a "club" (or syndicated) transaction with participation by multiple PE groups. In the Capital IQ database, there are 9809 distinct transactions meeting our criteria. In 89% of the cases, we obtained at least minimal information (organization type and fundraising history) about at least one buyout group active in the transaction. Because some transactions involve multiple PE groups, there are 11,606 distinct observations of PE group-buyout pairs. The most-active PE groups in the sample by deal count include some of the largest and most recognizable PE organizations, as well as leading specialists in middle market deals. (As we document in Table 2, sample sizes shrink once these data are matched to the Census information.)

PE Group Identifier

We assigned each PE group an identification number. That was simple when the group began as and remained an independent entity. Cases involving a change in control (and sometimes a name change), often associated with an acquisition or spin-off, were more complex.

Where there was a spin-out, we considered the spun-out entity to be a new PE group (with a new identifier), unless we were highly confident that it encompassed essentially all the PE investment activity of the predecessor group. In the latter case, a group might change status over time: i.e., from part of a bank or a family office to independent. When an independent group was acquired by another group, it was subsumed into the acquiring group after the acquisition, and its investments assigned to the acquirer's identifier. In addition to relying on entries in Capital IQ (the "Firm Description" field, a text description of the firm's history, its investment profile, and more), Refinitiv Thomson One, and Preqin databases, we undertook extensive online research to make these determinations. We especially relied here on searches using Factiva and Lexis-Nexis for historical information that was not accessible through searches of the unrestricted Internet.

In many cases, it was helpful to use information about the year the organization was founded in these determinations. Capital IQ often reports the year founded, sometimes in the "Firm Description" field instead of the "Year Founded" field. In cases where this information was missing, we used the start dates reported in Preqin.

Three complexities, however, arose in the determination of founding dates:

- Firms spun off from another institution (typically a bank) sometimes recorded their start date as that of the spin-off, and other times when the predecessor group was established. We standardized these (to the extent possible) to the year the predecessor group was established within the old parent institution. If instead of the spin-off of a clearly delineated group within the old parent institution, the creation of the firm entailed the departure of a few individuals within a larger body, the date of the actual firm formation was used.
- Groups that were still parts of a parent institution sometimes used the year the first PE program was set up at the parent, the year the specific initiative was established, or the year the parent was established. We standardized these to the extent possible to the year the first PE program was set up at the parent institution. If we could not get a start date of the first PE program or the specific investing program's inception, we left this information blank.

- Few corporations, family offices, and institutional investors disclosed when their private investment programs began, simply reporting when the overall entity was founded. If we could not get a date for the program's inception, we left this information blank.

Type of Organization

CapitalIQ was used to classify types of PE groups, particularly the fields "Primary Industry," "Institution Type," and "Firm Description." For groups whose status changed (e.g., due to an acquisition or divestiture), we used the information as it stood at the time of investment. Again, these changes were confirmed and precise dates identified using online searches. Where this information was incomplete, we supplemented it with online searches.

We used the following scheme to classify firms.

0- PE groups or diversified investors where private equity is an important component (e.g., Blackstone, Carlyle). This includes organizations with a "fundless" structure (e.g., who are investing off their balance sheet or on a "deal by deal" basis), as well as those who raised there last fund many years earlier.

1- Investment arms that are subsidiaries of other financial institutions, including investment/commercial banks, insurance companies, mutual funds, and brokerage houses, whether investing through funds or directly from these entities' balance sheets.

2- Investment arms that are subsidiaries of non-financial operating corporations, whether investing through funds or directly from these entities' balance sheets. In some cases, investment groups are identified by their largest holding, making them difficult to distinguish from operating companies. Other entities are unclear whether they are a business or investment company. A key test is whether there is a recognizable "core" business in a single or set of related industries. Berkshire Hathaway and GE are perhaps extreme cases. BH could be regarded as a (fundless) PE group, an insurer, or an operating company; we classified as a 0. On the other hand, General Electric or Mitsubishi's various financing subsidiaries could be regarded for much of the period as a (funded or fundless) PE group, a financial services firm, or an operating company; we classified them as a 2. Because the sample is limited to PE buyout transactions (i.e., excluding traditional strategically motivated acquisitions), no transactions by Danaher Corp., one of the most active acquirers of U.S. manufacturing firms, are included in the database. Thus, the only corporate transactions are those where the firms are either (a) undertaking their own PE-type transactions, often through a financing arm, or (b) co-investing with a large limited partner.

3- Investment arms that are subsidiaries of institutional and family investors, such as pensions, sovereign wealth funds, university endowments, and the like, whether investing through funds or (more commonly) directly from these entities' balance sheets. Again, we also include co-investments with PE groups; but as we point out elsewhere, the coverage of co-investment by CapitalIQ does not seem comprehensive and indeed biased (Fang, et al. [2015]).

4- Organizations with the bulk of their assets (90%+) in debt, hedge, and real estate funds or who primary lend off their balance sheets (excluding commercial banks, who are included in 1), but who do some PE investing on the side. This category does not include diversified investment managers who also own some of these funds.

Prior Fundraising

Fundraising data was bulk-downloaded from Preqin. The Preqin firm names were matched to the firm names in the Capital IQ data. This matching, in many cases, took considerable background research using online sources, due to the plethora of groups with similar sounding names (e.g., Pine Brook, Pine Creek, Pine Street, Pine Tree Equity, Pine Tree Growth, and PineBridge, not to mention various variants of White Pine).

Coverage of funds (amounts raised and performance) in Preqin is imperfect, particularly before 2000. We supplemented the Preqin information with fundraising data from Refinitiv Thomson One for those entities with no fundraising data in the relevant period. (Again, this took considerable research to resolve name matching.) In other cases, we found fundraising material online (e.g., state pension web sites) that summarized the timing and size of a group's funds.

We summed the count and size of the funds closed in the year between the deal year in the original data and four years before (t-4 to t), covering PE funds. All fund totals are expressed in millions of current U.S. dollars, converted from foreign currencies (if necessary) using the exchange rate at the mid-point of the year of the investment contained in the U.S. Federal Reserve Bank's H-10 series. We identified which funds to use based on the organization of the firm as of the time of the investment. Thus, for transactions in 2007 and before, we would look only at the funds raised by GSO Capital Partners. After its 2008 acquisition by Blackstone, we would compute the total raised by Blackstone (and GSO) in the five-year period.

We also created a normalized series: the funding divided by total funding raised at the beginning of the deal year in the original data and four years before (t-4 to t). These fundraising totals were for the years from 2000 to 2013 from Preqin (for U.S. based buyout and balanced private equity funds only) and from 1980 to 1999 from Thomson Reuters (North American-based buyout, mezzanine and growth funds).[35]

Prior Fund Performance

For these firms, performance data (as of the end of 2019 or the closest date prior to this point) for any funds from years t-12 to t-5 were also collected. We captured funds for banks and

[35] For years before 1980, we assumed based on press accounts an annual fundraising rate of $100 million per year in 1978 and 1979, and $50 million per year in 1976 and 1977.

corporations that raised funds under different divisions and programs (e.g., for General Electric, entities raising funds included GE Capital, GE Commercial Finance, and GE Holdings). These data were primarily taken from Preqin, but complemented with information from PitchBook and state public pension disclosure.

We focused on the performance of the most recent U.S. (or global) PE funds in that period with performance data and the two most recent funds. We looked at internal rate of return (IRR) and multiple of invested capital (MoIC), since this information was most readily available in Preqin. (Coverage of public market equivalents was much thinner.) In each case, we subtracted the benchmark performance calculated as the pooled IRR and weighted MoIC. The sources of the benchmark performance information were as follows:

- For vintages 1985, 1987 to 1988, and from 1990 to 2008: Preqin database, using data on North American buyout funds
- For vintages 1986 and 1989: Cambridge Associates via ThomsonOne, using data on US buyout and growth equity funds.
- For vintage years 1976 to 1984: Venture Economics, 1998 Investment Benchmark Reports, Newark, 1998, using data on US buyout funds (data on the 1976-83 period is consolidated in the report).
- For vintage years 1968 to 1975: Venture Economics, Venture Capital Performance 1989, Waltham, 1989, using data on all US private capital funds (data on the 1970-76 period is consolidated in the report).

In cases where we used multiple funds, we took a fund-weighted average of the fund net performance. Because this averaging process is more correct for TVPIs (an average of two IRRs may be quite different than the IRR of the combined cash flows), we focused in the TVPI measure in the paper.

This performance information was typically missing for groups that invested off their own balance sheet (which included many financial institutions, family offices, and corporations, and some private equity groups with fundless structures). In some cases, groups invested through both funds and their balance sheets, whether PE groups that have raised outside capital at the management company level (e.g., KKR) or more typically, banks and corporations. To cite one example, the amount raised through its funds was a small fraction of what GE invested, since most was done through its balance sheet. Unfortunately, there is no way to create a fund-like measure for balance sheet assets, since capital designated for investments is typically not segregated in financial reports. We thus computed the total for the formal funds.

We did a variety of diagnostic tests to verify the information and to catch potential errors. These exercises included:

- Looking at all PE investment entities with a start year before 1945. (Some entities did indeed start earlier, but we sought to be extra careful here.)
- Looking at PE investment entities with the same identification number but a different classification. (Again, due to spin-offs, some organizations did change status, but we were extremely careful and conservative here.)
- Looking at PE investment entities with the same identification number but a different start year. (Again, such cases could result as a result of a spin-off—see the rules delineated under Step 1—but we wanted to be sure.)

- Looking at PE investment entities with the same identification number and transaction year, but a different number and volume of funds raised in the prior five years. (These were typically the result of miscoding organizations with complex organizational histories.)
- Looking at PE investment entities with the same identification number, but with substantial discrepancies in the number and volume of funds raised in the prior five years between adjacent years. (These were typically a consequence of miscoding organizations with complex organizational histories.)

Internet Appendix D: Additional Results

Table D.1 tabulates the data presented in Figure 1 for three periods selected to highlight how PE deal flow sank during the financial crisis and recovered afterwards. Table D.2 follows Table 1 in the main text, except for using upper tercile splits rather than median splits for the GDP growth and credit spread variables. Table D.3 provides information about the distribution of PE buyouts by industry sector and deal type. It also uses the same sample as Figure 1.

In Table D.4, Panel A breaks down the overall employment change by establishment status. Here, "Continuers" refer to establishments that operate under ownership of the same firm (target or control) throughout the period from t to $t+2$. Continuer employment at target firms shrinks by (a statistically insignificant) 1.5% relative to control counterparts in the two years after buyout. The rate of employment change at growing continuers is essentially identical for buyouts and controls, as indicated by the "Creation" results. In contrast, contracting continuers shrink more rapidly at targets, as indicated by the "Destruction" results. Target firms experience 4.0% larger employment losses from shuttered establishments ("Deaths") and 1.2% greater employment gains due to new facilities ("Births"). They also add more jobs through acquisitions to the tune of 3.7% of base employment. All three of these differences are statistically significant. The difference in job changes from divestitures, however, is neither economically or statistically significant.

Because the regressions are employment weighted, we can sum the coefficients. Consider first the results for "Continuers" and "Deaths," which capture all employment changes for establishments owned and operated by targets and controls in the buyout year. Summing these two components yields a two-year employment growth rate differential of -5.6 percentage points (-1.53 – 4.03). That is, establishments operated by target firms as of the buyout year shed 5.6% of employment relative to controls over the next two years, largely through establishment shutdowns.

Factoring in the greater propensity of target firms to create more new jobs at new establishments adds 1.2 points to this sum. That yields a net differential of -4.4 percentage points for targets, the same as the organic growth change in the second row. Further factoring in the role of acquisitions and divestitures adds 3.0 points, yielding an overall buyout effect on firm-level employment of -1.4 percentage points over two years. The other panels in Table D.4 consider various results for job reallocation (overall and excess), compensation per worker, and labor productivity.

Table D.5 reports estimated buyout effects on employment by adjustment margin and buyout type.

Finally, Table D.6 provides evidence on the wage effects of PE buyouts using a larger, broader sample than previous studies. How buyouts affect wages has long been controversial. Critics argue that buyouts lead to lower wages, as formalized by Shleifer and Summers (1988). Indeed, Lichtenberg and Siegel (1990) find that buyouts lead to lower compensation for white-collar workers. More recently, Agrawal and Tambe (2016) suggest that buyouts can enhance human capital in target firms, particularly by developing employee knowledge of information technology. Survey evidence in Gompers, Kaplan, and Mukharlyamov (2016) is consistent with this view.

Our wage measure in Table D.6 is the change from buyout year *t* to *t+2* in the firm's gross annual compensation per employee.[36] The wage sample is smaller than in Panels A-C of Table 3

[36] Barth et al. (2014) provide a detailed description of the LBD wage measure: "The data follow the definition of salaries and wages used for calculating the federal withholding tax. They report the gross earnings paid in the calendar year to employees at the establishment prior to such deductions as employees' social security contributions, withholding taxes, group insurance premiums, union dues, and savings bonds. Included in gross earnings are all forms of compensation such as salaries, wages, commissions, dismissal pay, paid bonuses, vacation and sick leave pay, and the cash equivalent of compensation paid in kind. Salaries of officers of the establishment, if a corporation, are included. Payments to proprietors or partners, if an unincorporated concern, are excluded. Salaries and wages do not include supplementary labor

for the same three reasons discussed in regard to productivity in the text. In addition, compensation data are unavailable for some firms in the LBD.

The first column in Table D.6, Panel A reports a statistically insignificant wage drop of 0.28% at target firms relative to controls over two years post buyout. Because we derive this estimate as a difference-in-difference, it nets out persistent target-control differences in workforce composition. However, it does not control for changes from the buyout year *t* to *t+2* in firm-level workforce composition. Establishment births, deaths, acquisitions, and divestitures are potentially important sources of such changes in firm-level workforce composition.[37]

Panel A suggests that buyout-induced wage effects also differ greatly by type. Compensation per worker rises by 11% in divisional targets relative to controls over two years post buyout, while falling by 6% in private-to-private deals. We find smaller, statistically insignificant wage declines for public-to-private and secondary deals. Large post-buyout wage gains at divisional targets may partly reflect what practitioners call "job title upgrading." When a corporate division becomes a new stand-alone firm, the divisional general manager (or his replacement) becomes CEO, the divisional controller becomes CFO, and so on. The new titles and firm-wide responsibilities often come with (much) higher pay. The Carlyle Group's divisional buyout of DuPont Performance Coatings (renamed Axalta Coating Systems) in February 2013

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costs such as employer's Social Security contributions and other legally required expenditures or payments for voluntary programs." Thus, our wage measure includes management compensation except for stock option grants, which are typically constructed to defer tax obligations until exercise or sale. Buyouts often tilt the compensation of senior management toward stock options (Leslie and Oyer, 2008), so we may slightly understate the true wage change at target firms.

[37] Appendix Table D.4. explores these firm-level adjustment margins and show that they are especially active at target firms in the wake of buyout deals.

offers a case in point.[38] Panels B and C display the relationship between the differences in wage changes and economic conditions at and after the buyouts, and find few significant relationships.

[38] The top five personnel of Axalta received compensation in 2013 of $17.2 million, including the aggregate fair value of stock option awards as of the grant date. While the reporting of option grants may differ for tax purposes (and hence in our data), even the total non-option compensation of the five individuals was $6.1 million. We cannot directly observe the compensation of the top five employees of DuPont Performance Coatings in 2012, but web sites such as Glassdoor suggest that senior divisional managers at DuPont received contemporaneous compensation packages in the mid-six figures. See Axalta Coating Systems, Schedule 14A, March 23, 2015 and Lerner and Tuzikov (2018). Thus, the compensation of top Axalta personnel in 2013 was much greater than what they, or their counterparts, likely earned as senior divisional managers before the buyout.

Table D.1. Private Equity Deal Flow Before, During, and After the Financial Crisis. The table reports the quarterly flow of private equity buyouts, overall and by deal type, in selected periods. It also reports the average value of the credit spread in the closing month and the annual real GDP growth rate over the four quarters that end in the closing quarter. The table entries are tabulated from the data plotted in Figure 1.

	All PE Buyouts	*Private to Private*	*Public to Private*	*Divisional Sales*	*Secondary Sales*
A. *Pre-Crisis, January 2004 to December 2007*					
Buyouts Closed Per Quarter	203	88	15	52	43
Average Credit Spread	3.27%				
Average Real GDP Growth Rate	2.85%				
B. *Crisis, October 2008 to June 2010*					
Buyouts Closed Per Quarter	87	46	5	17	18
Average Credit Spread	11.79%				
Average Real GDP Growth Rate	-1.40%				
C. *Post-Crisis, July 2010 to December 2013*					
Buyouts Closed Per Quarter	133	58	9	17	49
Average Credit Spread	6.81%				
Average Real GDP Growth Rate	1.97%				

Table D.2. Market Conditions and Private Equity Buyout Frequency by Deal Type, Quarterly Data, 1980-2013, Upper Tercile Split Instead of the Median Split in Table 1 in the Main Text. We regress 100 times the natural log of the PE buyout count in quarter *t* on deal-type indicators interacted with market conditions at buyout close (top panel) and over the following two years (bottom panel), while controlling for deal type and a linear time trend. The sample is the same as in Figure 1. To characterize contemporaneous market conditions for buyouts that close in quarter *t*, we consider whether the credit spread in *t* is in the top tercile or not and whether real GDP growth from *t-4* to *t* is in the top tercile or not. Similarly, to characterize the evolution of market conditions over the next two years, we consider whether the change in the credit spread and real GDP from *t* to *t*+8 are in the top tercile or not. After dropping quarter-type cells with no buyouts, each regression has 454 observations. *** p<0.01, ** p<0.05, * p<0.1.

Dependent Variable: 100**ln*(type-specific buyout count in quarter *t*)

	Coefficient on Market Conditions (row) interacted with Deal-Type Indicator (column)					
Market Conditions	Private to Private	Public to Private	Divisional Sales	Secondary Sale	R^2	Equality of Coefficients (p-value)
A. At Buyout Close						
High GDP Growth	17.4	75.0***	39.1***	-11.4		
	[11.2]	[14.3]	[13.0]	[15.6]	0.74	0.000
Wide Credit Spread	-40.5***	-37.4**	-34.4*	-26.3**		
	[10.2]	[16.1]	[18.7]	[14.3]		0.000
B. Over Next 2 Years						
High GDP Growth	-3.9	9.9	12.9	-40.9**		
	[12.4]	[14.2]	[13.9]	[17.3]		0.120
Widening Credit Spread	19.7*	61.5***	24.5*	22.7	0.73	
	[11.3]	[14.8]	[14.1]	[14.8]		0.000

Table D.3. Private Equity Buyouts by Industry Sector and Deal Type, 1980-2013. Each column reports the percentage breakdown of buyouts for the indicated deal type, using the Standard & Poor's 2018 Global Industry Classification Standard (GICS). The sample is the same as in Figure 1.

		Buyout Type				
Sector	*GICS code*	*Private-to-Private*	*Public-to-Private*	*Divisional*	*Secondary*	*Total*
Energy	10	2.9	2.2	2.6	2.2	2.6%
Materials	15	8.1	5.7	9.3	8.6	8.3%
Industrials	20	28.9	19.0	23.4	28.6	26.5%
Consumer staples	25	18.6	24.6	18.8	20.7	19.6%
Consumer discretionary	30	7.4	4.6	4.0	6.2	6.0%
Health care	35	10.1	12.0	8.0	10.3	9.7%
Financials	40	3.9	4.7	4.7	2.7	3.9%
Information technology	45	11.5	15.8	17.7	12.3	13.7%
Communications services	50	7.2	7.5	8.1	7.4	7.5%
Utilities	55	0.6	1.0	2.1	0.8	1.1%
Real estate	60	0.8	3.1	1.3	0.2	1.0%
		100.0%	100.0%	100.0%	100.0%	100.0%

Note: A test of the null hypothesis that the industry distribution of buyouts is independent of deal type yields a Pearson Chi-squared statistic of 260.7 with a p-value of 0.000.

Table D.4. Buyout Effects by Adjustment Margin at Target Relative to Control Firms. The sample contains matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are (approximate) percentage amounts from the buyout year *t* to *t+2*, unless otherwise noted. All results in Panel A are expressed as percentages of firm-level base employment. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. A positive coefficient in each case indicates that activity on that dimension is greater for buyouts. See Section II in the main text for an explanation of how we weight observations. Results for "All Margins" include the contribution of post-buyout acquisitions and divestitures, while results for "Organic Margins" exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

A. Employment Growth	Buyout Effect	Standard Error	R^2
All Margins	-1.35	[2.17]	0.32
Organic Margins	-4.38**	[1.90]	0.29
By Establishment Status			
Continuers	-1.53	[1.15]	0.28
Creation	0.20	[0.41]	*0.34*
Destruction	1.73*	[0.96]	*0.27*
Deaths	4.03***	[1.24]	0.30
Births	1.17**	[0.51]	0.34
Acquisitions	3.69***	[0.97]	0.38
Divestitures	0.65	[0.41]	0.26
Number of Firm Observations (000s)	6,400		

B. Reallocation (% of Base Employment)	Buyout Effect	Stan. Err.	R^2
Excess Reallocation, All Margins	4.95***	[1.14]	0.40
Excess Reallocation, Organic Margins	0.61	[1.54]	0.35
Job Reallocation, All Margins	11.47***	[1.82]	0.39
Job Reallocation, Organic Margins	7.13***	[1.76]	0.39
Number of Firm Observations (000s)	6,400		

C. Productivity Change at Targets Relative to Controls, and Separate Contributions of Revenue and Employment Changes

	Buyout Effect	Standard Error	R^2
Revenue Per Employee	0.0752*	[0.0406]	0.47
Revenue Contribution	0.0618	[0.0398]	0.47
Employment Contribution	-0.0133	[0.0230]	0.39
Number of Firm Observations (000)	911		

Table D.5. Buyout Effects on Employment by Adjustment Margin and Buyout Type. The sample contains matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are employment changes from the buyout year *t* to *t+2*, expressed as a percentage of firm-level base employment. A positive coefficient in each case indicates that activity on that dimension is greater for buyouts. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a separate weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the main text for an explanation of how we weight observations. Results for "All Margins" include the contribution of post-buyout acquisitions and divestitures, while results for "Organic Margins" exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

	Private-to-private		Public-to-private		Divisional		Secondary	
By Adjustment Margin:	Buyout Effect	R^2	Buyout Effect	R^2	Buyout Effect	R^2	Buyout Effect	R^2
Continuers	0.55 [1.04]	0.30	-1.59 [1.20]	0.33	-7.64*** [2.74]	0.29	2.63** [1.28]	0.36
Creation	0.27 [0.57]	0.36	0.23 [0.56]	0.29	-0.86 [0.96]	0.28	2.10* [1.08]	0.43
Destruction	-0.28 [0.77]	0.32	1.82* [0.99]	0.32	6.78*** [2.45]	0.33	-0.53 [1.02]	0.29
Deaths	-0.03 [1.04]	0.34	6.26*** [2.05]	0.44	9.76*** [2.00]	0.28	0.70 [1.58]	0.29
Births	2.51*** [0.77]	0.40	-2.13*** [0.71]	0.33	1.42 [1.20]	0.37	4.16*** [1.22]	0.42
Acquisitions	9.53*** [2.59]	0.44	0.40 [0.57]	0.42	3.32** [1.54]	0.38	3.29*** [0.96]	0.39
Divestitures	-0.27 [0.53]	0.20	3.01*** [1.04]	0.35	-1.02** [0.49]	0.23	-0.36 [0.61]	0.22
Observations (000s)	3,700		400		2,300		600	

Table D.6. Buyout Effects on Wages by Establishment-Level Adjustment Margin. The table presents tables identical to those in the paper, but for wages. Panel A replicates Table 3; Panel B, Table 5; and Panel C, Tables 5 and 6 (first column only). See tables in the paper for more information.

Panel A: Estimated Buyout Effects.

	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary		p-Value
Dependent Variable	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	SUR test
Annual Compensation Per Employee	-0.28 [1.6]	0.22	-5.9* [3.4]	0.13	-1.8 [1.6]	0.81	11.0*** [3.4]	0.41	-3.0 [2.5]	0.37	0.040
Observations (000s)	3,700		2,100		200		1,500		300		

Panel B: How Buyout Effects Vary with Macroeconomic and Credit Conditions at the Close.

		Interaction Variable	
Dependent Variable		Credit Spread	GDP Growth
	[St. Error]	[0.45]	[0.69]
	Unit S.D. Effect	4.6	-1.2
Annual Compensation Per Employee	Coefficient	0.66	-0.65
	[St. Error]	[0.62]	[0.78]
	Unit S.D. Effect	2.0	-1.1

Panel C: How Buyout Effects Vary with the Credit Spread Change and GDP Growth in the Two Years after the Buyout.

Dependent Variable		Credit Spread	GDP Growth
Annual Compensation Per Employee	Coefficient	0.33*	-0.24
	[St. Error]	[0.20]	[0.41]
	Unit S.D. Effect	1.4	-0.8

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The Operational Consequences of Private Equity Buyouts: Evidence from the Restaurant Industry

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ABSTRACT

Do private equity buyouts disrupt company operations to maximize short-term goals? We document significant operational changes in 103 restaurant chain buyouts between 2002 and 2012 using health inspection records for over 50,000 stores in Florida. Store-level operational practices improve after private equity buyout, as restaurants become cleaner, safer, and better maintained. Supporting a causal interpretation, this effect is stronger in chain-owned stores than in franchised locations -- "twin restaurants" over which private equity owners have limited control. Private equity targets also reduce employee headcount, lower menu prices, and experience a lower likelihood of store closures -- a proxy for poor financial performance. These changes to store-level operations require monitoring, training, and better alignment of worker incentives, suggesting PE firms improve management practices throughout the organization.

Keywords: Private Equity, Management Practices, Operational Performance, Restaurants.
JEL Classifications: G24, G34, J24, J28, M11, M54

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The 2012 presidential campaign reignited a long-standing debate over the merits and costs of the private equity (PE) industry. Labor and political leaders often argue that PE transactions are largely financial engineering schemes, adding little operational value. Moreover, PE firms are commonly accused of practicing "strip and flip" strategies, in which portfolio companies' high leverage cause an excessive focus on short-term financial goals, leading to adverse cost-cutting adversely affecting customers, employees, and the firm as a whole. For example, discussing the recent Burger King acquisition by 3G Capital, a New York Times article argues, "financial engineering has been part of the Burger King story for so long that it's hard to believe there is still anything worth plucking from its carcass."[1]

Jensen (1989) argues instead that leveraged buyouts are a superior governance form leading to better managed companies. Specifically, PE firms mitigate management agency problems through the disciplinary role of debt, concentrated and active ownership, and high-powered managerial incentives, which lead managers to improve operations. Consistent with this view, a substantial body of literature documents significant improvements in profitability and increases in operating income of buyout targets (e.g., Kaplan (1989), Guo, Hotchkiss, and Song (2008), among others). However, as noted by Kaplan and Stromberg (2009), the decline in capital expenditures found in the literature, raises the possibility that private equity firms may trade off an increase in current cash flows for deterioration in long run operations. Moreover, most of the evidence is based on leveraged buyouts completed before the latest private equity wave.[2]

[1] "Burger King, the Cash Cow," New York Times, June 2012

[2] Transaction activity of private equity buyouts exhibits substantial cyclicality. Transaction values peaked in 1988, fluctuated during the 1990s, and increased dramatically from 2005 to 2007. Afterwards, deal activity decreased substantially in the wake of the financial crisis. During the last wave of private equity transactions, more than

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In this work, we explore whether private equity buyouts disrupt firm operations in an attempt to maximize short-term goals. To do so, we focus on a single industry, restaurants, with its unique data and institutional setting, during the recent wave of the private equity buyout activity. We focus on a variety of micro-level operational dimensions to explore the consequences of private equity acquisitions. We find evidence that PE firms are active investors that improve firm operations. The improvements we document require better monitoring, training, and alignment of worker incentives, suggesting PE firms improve management practices throughout the organization.

Identifying whether private equity firms causally affect firm operations is challenging.[3] Ideally, we would compare two identical firms: one treated with PE ownership and one untreated. To achieve a close variation of such an experiment, we exploit the dual ownership structure pervasive in the restaurant industry in which, within a chain, stores can be owned by either the parent company or a franchisee. Franchisees are legally independent entities that acquire a turnkey business format from a franchisor, to which royalties and fixed fees are paid. Franchised outlets have the same brand, menus, and appearance as those owned directly by the chain. Beyond such contractual specifications, however, headquarters has limited ability to influence the decision-making of franchisees (Kidwell, Nygaard, and Silkoset, 2007; Vroom and Gimeno, 2007). Hence, this setting allows us to compare *twin* stores that differ only in their ownership structure and thus degree of PE influence.

5,000 private equity transactions occurred globally, with an enterprise value of $1.6 trillion (in 2007 dollars), accounting for 43 percent of the total real transaction value since 1984 (Kaplan and Stromberg 2009).

[3] The standard approach in the literature is to match PE-backed firms with control firms selected using observable characteristics. Such counterfactuals will generate unbiased estimates under the assumption that these characteristics are precisely the ones that led PE to invest in the portfolio company in the first place. Given the lengthy due diligence and high stakes involved, this is quite a strong assumption.

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Determining how PE firms affect operations is difficult primarily because data is scarce. PE-backed firms are private companies and therefore not required to disclose financial information. While prior literature has focused mostly on financial statements of companies that either issued public debt or went public, Cohn et al. (2012) illustrate that such an approach leads to biased estimates.[4] Even absent such biases, financial statements shed light only on aggregate firm performance. We peer into micro-level firm operations through the lens of health inspections, which provide a backstage view of restaurants' operating practices as defined by the Food and Drug Administration (FDA). All restaurants in the United States, public and private, are subject to periodic surprise inspections aimed at identifying threats that may lead to foodborne illnesses.[5] Restaurants are evaluated on operational practices such as food handling, kitchen maintenance, consumer advising, and employee training. Thus, these inspections provide a unique view of practices and routines employed by restaurant managers.[6]

We compile every restaurant inspection conducted in Florida between 2002 and 2012.[7] Private equity firms acquired 103 restaurant chains with a presence in Florida over this period, accounting for approximately 3,700 individual restaurants out of over 50,000 in operation.[8] We first employ a difference-in-difference analysis to explore the overall treatment effect of private equity firms on chain stores. The use of store data allows us to include zip code by year

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4 Exceptions are papers that focus on non-financial performance margins such as innovation (Lerner, Sorensen and Stromberg 2012), and employment (Davis et al. 2013)

5 Each year roughly 48 million people get sick, 128,000 are hospitalized, and 3,000 die of foodborne diseases in the United States (Center for Disease Control and Prevention).

6 We provide a complete list of practices examined by inspectors in the Appendix.

7 Health inspections in the U.S. are commonly conducted at the level of the county. Each county has its own inspection standards and grading system, making cross-county health inspection comparisons difficult. The choice to conduct the study in Florida was motivated by the fact that health inspections in Florida are conducted at the state level, allowing consistent comparison of inspection outcomes across a larger sample.

8 Recent buyouts of restaurant chains by private equity funds include Burger King, Sbarro, California Pizza Kitchen, Chilis, Quiznos, PF Changs, Outback Steakhouse, among others.

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fixed effects in the analysis, controlling for varying customer demographics and local demand shocks.

Our key result is that restaurants commit fewer health violations after being acquired by a private equity firm. The improvement is concentrated in those practices whose potential hazards are deemed by the FDA most dangerous for customers. The effect remains strong with store fixed effects and when we control for changes in number of employees and number of seats per store. In addition, we show that there are no pre-existing trends in health inspections before private equity takes over, and the treatment effect increases steadily over five years after the private equity buyout.

These operational practices matter. Jin and Leslie (2003) show that a reduction in violations, triggered by the introduction of hygiene quality grade cards, improved store revenue and reduced the number of foodborne illness hospitalizations. We find that such violations are also strongly correlated with customer reviews posted on Yelp.com. Moreover, we show that deterioration in such operational practices is correlated with future likelihood of restaurant closures, a proxy for store profitability.

Why would such practices ever be violated in a restaurant? Arguably, serving food at an appropriate temperature, storing toxic substances properly, or sanitizing food surfaces, are all practices whose implementation is cheap relative to the hazards they entail. Violations arise frequently, however, potentially because workers responsible for the actions are not the ones likely to suffer the consequences. The restaurant manager's role is to mitigate such agency problems and ensure workers internalize the externalities they generate in their actions.

Are these operational improvements driven by active PE involvement or mere selection and chain expected trajectory? We find a differential treatment effect within a chain using the twin restaurants analysis: improvements in health-related practices are concentrated

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in directly owned restaurants where private equity firms have more influence. This difference maintains even when including zip code by year fixed effects, controlling for local demand shocks and varying customer demographics. Moreover, we show that both directly owned stores and franchisees experience similar pre-existing trends prior the PE buyout. These results indicate an active involvement of private equity firms in the operations of their portfolio companies.

Interestingly, we also find evidence of spillover effects, as franchisees located in the same zip code as directly owned restaurants catch up over time and improve their practices as well, in contrast to franchisees located in areas with no proximate directly owned restaurants. This suggests that competitive pressures lead franchisees to adopt the improved practices.

Next, we explore whether these changes are driven by other margins of restaurant operations. Are these improvements accompanied by the hiring of more employees or increases in menu prices? We find the opposite. PE-backed restaurants slightly reduce employee headcount at the store level. Moreover, using a panel of menu samples from nearly 2,200 restaurant chains from 2005 to 2012, we find that PE-backed restaurants lowered prices relative to those of similar menu items sold by direct competitors.

The evidence so far suggests that private equity acquisitions induce significant operational changes at the level of the individual store. It is natural to wonder whether such changes translate into increased profitability. While we do not observe store-level financial information, we ask whether the likelihood of store closure, a proxy for poor financial performance, changes following the PE buyout. We find that store closure risk declines following the PE buyout. Moreover, this decline is concentrated in directly owned stores, in which PE firms have greater influence, and where operational improvements are largest.

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We interpret the results as evidence that private equity ownership improves existing operations by mitigating agency problems through the improvement of management practices in the organization. While we do not observe changes in employee contracts directly, improving such practices in a restaurant chain requires not simply appropriate capital budgeting but rather better training, monitoring, and alignment of worker incentives throughout the chain. In that regard, this paper is related to an extensive literature that explores the consequences of private equity ownership (e.g., Kaplan 1989; Lichtenberg and Siegel 1990; Boucly, Sraer and Thesmar 2011; and John, Lang and Netter 1992). Davis et al. (2013) provide evidence for productivity improvements. Our evidence complements their paper by illustrating that such improvements are potentially driven by better management practices employed in the organization. We also contribute to the literature with a new approach to identifying whether private equity buyouts causally affect their portfolio companies.

A second related literature is that on the impact of human resource management (HRM) on productivity, illustrating a link between management practices and firm performance.[9] Our findings illustrate that PE firms improve operations management practices, consistent with Bloom, Sadun and Van Reenen (2009) who survey over 4,000 firms in Asia, Europe, and the U.S. to assess their management methods. They show that PE-backed firms are on average the best-managed group in the sample. However, they cannot rule out the possibility that these firms were better managed before private equity takeovers. Our paper is also closely related to Matsa (2011), who explores the impact of leverage on product quality, finding that firms that undertook high leverage appear to degrade their products' quality. Our

[9] For example, Bartel, Ichniowski, and Shaw, (2007), Black and Lynch (2001), Bloom and Van Reenen (2007), Ichniowski, Shaw, and Prennushi (1997), and Lazear (2000).

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contrasting finding may be explained by our focus on the recent wave of private equity buyouts, as the nature of PE buyouts and amounts of leverage taken has changed over time (Guo, Hotchkiss and Song 2011).

The remainder of the paper proceeds as follows. Section I describes the data sources and the nature of health violations. Section II provides empirical results on the impact of private equity on restaurant operations, and Section III concludes.

I. Data description

The data in this analysis is constructed from several sources combining information on PE buyouts (CapitalIQ), health inspection results and restaurant ownership in Florida (Florida Department of Business and Professional Regulation), store level employment (InfoUSA), restaurant menu prices (Datassential) and restaurant consumer reviews (Yelp.com). In this section we also illustrate key characteristics of the health inspection results and their correlation with consumer satisfaction and restaurant closures.

A. Health Inspections

The focus of this paper is on operational practices related to sanitation and food-hazard safety. Such practices are correlated with store revenue (Jin and Leslie 2003), and in section I.C we also show correlation with overall consumer satisfaction and restaurant closures. But safety and sanitation practices are important in their own right, as their violation poses a threat to public health safety. Each year in the U.S. roughly one in six people get sick (48 million people), 128,000 are hospitalized, and 3,000 die of foodborne diseases in the United States (Center for Disease Control and Prevention). Most of these outbreaks originate from commercial food facilities through food held at improper temperature, poor personal

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hygiene of workers, food handling, and cross contamination (Collins 1997). Due to such concerns, all restaurants in the United States are subject to periodic health inspections conducted by trained specialists in food service evaluation certified by the Food and Drug Administration. Failed inspections can result in fines, suspensions, and closure.

We gather health inspection data from the Florida Department of Business and Professional Regulation. This data encompasses every restaurant inspection conducted in the state of Florida from 2002 through 2012. U.S. health inspections are typically organized and conducted at the county level, and each county is free to use its own criteria and scoring methodology. There is no common standard used across states and counties. The advantage of using data from Florida is that inspections here are conducted at the state level using consistent criteria, and historical records are available back to 2002. Each record gives the name of the restaurant, the address, the date of the inspection, and lists violations across 58 different operational practices.

Florida health inspections divide violations into critical and non-critical. Critical violations are those "likely to directly contribute to food contamination, illness or environmental degradation." Examples of critical violations are improper disposal of waste, improper temperatures for cooked or stored food, dirty restrooms, and contaminated food surfaces. Non-critical violations "do not directly relate to foodborne illness risk, but preventive measures are required." Examples include clean non-food contact surfaces, adequate lighting, clean clothes and hair restraints. A complete description of inspection violations is provided in Appendix A. Inspections fall primarily into three categories: routine surprise, follow-up, and initial setup. We consider only surprise inspections for this study. Follow-ups are arranged in response to violations that need to be fixed and, like startup inspections, occur on known dates, which allow restaurants to put their best foot forward.

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B. Other data sources

We supplement the inspection data with restaurant ownership data, also from the Florida Department of Business and Professional Regulation. Restaurants need to renew licensing agreements with the state each year. These licenses, available from 2002 to 2012, provide the name and address of the owner at each restaurant. This allows us to separate restaurant branches into those owned directly by the parent brand, for which the owner name and address coincide with those of the parent firm, and those that have been franchised to independent owners. We incorporate data from InfoUSA, which makes phone calls to establishments to gather, among other data items, the number of full-time equivalent employees. This data is also gathered on an annual basis. Employee count is matched to the inspection database by name, address, and geocode coordinates. We collect median income at the county level from the Bureau of Economic and Business Research (BEBR) at the University of Florida.

We gather restaurant-pricing information from Datassential. This provider samples a representative menu from over 2,000 chains each year from 2005 to 2012. These menus give each item name, food category, and price. Datassential also categorizes each restaurant by price range and cuisine type. We also collect information on restaurant consumer reviews from Yelp.com.

To determine which of these restaurants were acquired by private equity firms, we download from Capital IQ all Leveraged Buyout, Management Buyout, and Secondary LBOs in the restaurant industry. We research each deal to find the names of the restaurant chains

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involved and record the date the deal closes. There are 103 separate deals involving 117 distinct brand names and approximately 3,500 individual restaurant locations in Florida.

Table I provides summary statistics on inspections and the restaurant sample. Panel A shows that over 20,000 eating establishments are inspected roughly twice each year.[10] The mean number of critical violations found is 4.5 with a standard deviation of 4.3. Panels B-D show that restaurants acquired by private equity firms are not observably different from restaurants in general. Chains acquired by PE firms have, on average, 175 outlets in Florida. These stores generate 3.8 critical violations per inspection, similar to violation counts in untreated chains with at least 5 stores in Florida. Treated and untreated stores appear similar on employee counts, size, and county income as well. Panel C shows that private equity is present in all types of cuisines, with a greater relative presence in hamburger chains, and panel D shows that treated and untreated chains have similar price distribution.

C. Correlation between health inspections and other restaurant outcomes

Before introducing the impact of private equity, we begin by studying the determinants of restaurant violations generally. In Table II, columns 1 and 3, we regress critical and non-critical violations on various store characteristics. Larger restaurants—those with more seats and employees—have more violations. Richer neighborhoods see fewer violations. The more units in the restaurant chain, the better the inspection outcomes. This may be evidence of professional management; a firm running multiple stores has more experience and better controls and procedures in place to monitor operational practices than a proprietor opening her first store. By cuisine type, Asian establishments fare the worst, while donut shops, ice

[10] We include only those stores for which we have employee and seat counts. There are fewer inspections in 2002 because the data do not cover the entire year.

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cream parlors, and beverage stores are the cleanest. These latter categories offer simpler items and less variety, which may explain fewer violations. Columns 2 and 4 add restaurant chain fixed effects and drop chain-invariant variables. The remaining results are unchanged. Interestingly, higher median county income leads to fewer violations even within the same chain.

We extract data from Yelp.com, a consumer review website, to explore whether health-related operational practices are correlated with overall customer satisfaction. People who register as users with Yelp by providing a valid email address can leave star ratings, ranging from 1-5, and comments on restaurants and other businesses. Anyone can read these reviews. In Florida, Yelp reviews are sparse before 2010 and increase significantly by 2012. We thus do not have a sufficient panel structure to examine the impact of PE on consumer satisfaction, but we exploit the cross-sectional correlation between this review-based restaurant quality measure and health violations in Table III, panel A.

For the year 2012, we average at the chain level the number of critical violations found in all inspections for all branches. We also average the number of stars given in Yelp for that chain. Column 1a shows the results of a simple univariate regression of stars on critical violations. The coefficient on critical violations is -0.025 and highly significant. A four-violation increase (one standard deviation) is thus associated with a rating lower by 1/10 of a star. This is meaningful given that 90% of ratings fall between 2 and 5 stars, and half-stars are associated with significant changes in revenue (Luca 2011). Column 2a adds restaurant price range by cuisine fixed effects (e.g., \$10-\$15 check size – Asian). Violations and customer satisfaction are strongly negatively related even among similar restaurants. Column 3a shows the results of a robustness check requiring at least five Yelp reviews for a restaurant or chain,

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and the results remain the same. Columns 4a – 6a add non-critical violations. These are also negatively related to Yelp scores but not as strongly as are critical violations.

This relationship between health-related practices and perceived quality could be a direct effect—customers downrate stores with poor hygiene levels. The correlation may also reflect more broadly that a restaurant that sustains good practices may also perform better on other quality dimensions such as service and food. Both explanations suggest that our findings may have a broader interpretation on customer satisfaction.

Panel B of Table III shows that poor practices are correlated with even more dire outcomes—restaurant closure, a proxy for store profitability.[11] For each individual restaurant, we average all inspection scores received each year. We then create the dummy variable *store closure* which equals one in the year a store closes, if it closes. Closure is defined as having no inspection record in a given year or in subsequent years. The inspection database is comprehensive, and every restaurant is inspected at least once and usually twice each year. Thus, if no inspections occur in a given year, we assume it must have closed. In column 1b, we regress store closure on the number of critical violations received in the year of closure and the year before (*lagged annual critical violations*) as well as year and store fixed effects. The coefficient on annual critical violations is 0.001 and highly significant. A one standard deviation increase in critical violations is associated with a nearly ½ percent increase in the likelihood of closure that year. This is not small considering the unconditional likelihood of closure is 7 percent per year, implying a 7.1 percent increase in closure likelihood. The number of violations the prior year has more than double this impact, suggesting that an increase in one standard deviation in critical violations in prior year is associated with almost

[11] Mandatory closures that are enforced due to poor health inspections are rare. The closures discussed here are voluntary ones decided by the restaurant owners.

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15 percent increase in likelihood of store closure. Non-critical violations, added in columns 2b and 3b, are again not as strong a factor.

II. Results on Private Equity Ownership

A. Health Inspections and Private Equity Ownership

We turn to the relationship between private equity ownership and health violations. We create a variable, *PostPE*, which equals one if an inspection at a particular restaurant occurs after it was acquired by a private equity firm. Panel A of Table IV regresses critical violations on *PostPE*. The sample here consists of all restaurants, not just those purchased by private equity. Year fixed effects are included to pick up any changes in violations over time. Hence, the other restaurants in Florida serve as the counterfactual for PE treated chains. In column 1a chain fixed effects are included to control for different baseline levels of operational standards so that the impact of PE entry can be isolated. The coefficient on *PostPE* is -0.662 and significant at the 1% level. Given that inspections average approximately 4 critical violations, this is a sizable decline of 15%. Is the effect driven by changes in restaurant workforce? Column 2a includes seats and employees as controls, motivated by Table II. The larger the restaurant, the more critical violations, but the *PostPE* coefficient maintains similar magnitude. This suggests that health-related practices improve following the PE acquisition regardless of changes in restaurant size and number of employees.

Critical health violations at the chain fall when private equity takes over. Two distinct effects could drive this. Individual restaurants could be getting cleaner, or poor performing branches could be closing. To explore within-store changes, columns 3a and 4a replace chain fixed effects with individual store fixed effects. The coefficient on *PostPE* remains the same

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with slightly lower significance, now at the 5% level, in this stricter test. Thus a given restaurant sees improvement in operational practices.

We introduce an even more precise counterfactual in columns 5a and 6a by replacing year fixed effects with zip code-by-year fixed effects.[12] This specification compares PE treated restaurants to competitors in the same zip code. Restaurants serve different demographics and experience different economic conditions across neighborhoods, possibly leading to different patterns in performance. Even after adjusting for location, critical violations still decline after PE entry.

Panel B of Table IV replaces critical with non-critical violations. In all six specifications, the effect of private equity management is essentially zero. Non-critical violations have a much smaller effect on health outcomes, and as illustrated in Table III, have a much weaker effect on customer satisfaction and store closures. It is not surprising that improvements appear to be concentrated where violated practices matter more.

Figure 1 shows the path of critical and non-critical violations around private equity takeover. The red bars plot the coefficients of a regression in which critical violations are regressed on private equity entry event year dummies[13]. Violations are flat in the three years before PE entry. Thus there does not appear to be a pre-deal trend. This helps mitigate endogeneity concerns that private equity was simply capitalizing on a trend of improved health and sanitation. The decline in critical violations then occurs steadily over the subsequent four years (becoming statistically significant in year 3 onward). This is consistent with anecdotal

[12] In practice, it is computationally difficult to estimate a regression that has so many layers of fixed effects. Fortunately, algorithms have recently been developed that can handle such high-dimensional fixed effect regressions. In our analysis, we use the iterative algorithm of Guimaraes and Portugal (2010). See Gormley and Matsa (2013).

[13] This regression include store fixed effects, zip code by year fixed effects, log number of seats and log number of employees. The regression results are in the Appendix, Table 1A. The average number of critical violations was added to coefficients in the graph to illustrate the relative size of the coefficients.

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evidence on the speed of operational change in restaurants (Gompers, Mugford and Kim 2012). The blue bars plot the evolution of non-critical violations. There appears to be no pattern before or after the PE buyout.

To provide a better understanding of the critical violations that drive the results, Table V breaks these critical and non-critical violations down into specific categories. Appendix A provides a list of which violations belong to which category. Improvements are concentrated in practices such as food handling, kitchen maintenance, and consumer advising. Changes in practices such as food handling cannot be simply achieved by capital reallocation within the firm, but rather likely involve substantial training and monitoring of store employees. These results suggest that better operations are likely to be achieved through improvement in management practices following the PE buyout.

B. *Identification Strategy*

B.1 Empirical Design

The results thus far indicate that after private equity firms take over a restaurant chain, health inspection outcomes improve. It could be the case, however, that private equity firms are simply passive owners who target brands that would have experienced improvement regardless of the buyout. We employ a number of strategies to address this concern. First, as illustrated in Figure 1, there was no pre-existing trend in health scores in the three years leading up to the deal. PE firms would need predictive power to anticipate these improvements. Second, we implicitly match our treated stores with non-treated restaurants by including all restaurants in Florida and year fixed effects in our regressions. If there is an overall trend for health scores, the other restaurants will pick that up and control for it. The homogeneity of restaurants ensures these are relevant matches. Further, because our analysis

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is at the establishment level, we can include zip code by year fixed effects. Therefore, for example, the counterfactual for a McDonalds restaurant is a Burger King branch in the same neighborhood. The two restaurants cater to similar demographics, compete in the same market, and likely experience similar fluctuations in demand.

The ideal counterfactual experiment, however, would be to compare two identical stores, one treated with PE ownership and one without. The prevalence of the franchising model in the restaurant industry allows us to run a close variation of this experiment. In a franchising arrangement, a parent franchisor sells a business format, typically including a brand, operating strategies, and design concepts, to a franchisee. Franchisees range from a single proprietor running a single restaurant to publicly traded firms that operate hundreds of restaurants across multiple brands. In return for an "off-the-shelf" business, the franchisee supplies the capital for the restaurant and pays royalties and fixed fees to the franchisor, typically based on the sales of the franchised outlet. Importantly, a franchise is a legally independent business not vertically integrated with the parent company and has a connection to headquarters only through contractual agreements.[14] Such contracts are typically written for 10 to 20 years.

Restaurant chains vary in the fraction of individual stores that are franchised. Each Olive Garden, for example, is run directly by parent company Darden Restaurants, while Subway sandwich shops are all franchised, and half of TGI Friday's nationwide are franchised. For chains that employ a mixture of outsourcing and direct ownership, there thus exist outwardly identical restaurants that differ only in ownership. When a private equity firm acquires a chain, legally, they only acquire the company-owned branches and the contractual

[14] Since franchisees are independent legal entities, their capital structure is separate and thus they do not experience any increases in debt loads following the PE buyout.

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obligation of the franchisees to pay royalties. While the name of the store, its logo, basic menu and food are the same, there are substantial differences in the ability of PE owners to actively affect the operations of franchisees relative to directly owned restaurants.

These differences can be dramatic. In many instances franchisors' formal control over franchisees has limited impact (Kidwell, Nygaard, and Silkoset, 2007; Vroom and Gimeno, 2007) and is insufficient to ensure compliance (Shane, 1996). In addition, PE buyers inherit the existing structure of franchisees in the chain, as well as pre-existing contracts. Such contracts are often written loosely to allow adoption of franchisees to local markets (Bradach, 1997; Sorenson and Sørensen, 2001). Because the franchisees are the residual claimants of their business, they often have discretion over actions not explicitly contracted with the firm. For example, private equity owned Burger King faced numerous lawsuits in 2010 from the Burger King National Franchisees Association (NFA), a group representing a majority of their independent operators in the United States. The franchisees "opposed a company mandate [to] sell a double cheeseburger for $1," "challenged a mandate that they keep their restaurants open late at night," and "haven't upgraded their checkout terminals as quickly as management wanted" (Wall Street Journal, 5/17/10). Hence, our prediction is that any effects of private equity takeover of a parent will manifest more strongly in company-owned than in franchised stores.

There can, of course, be endogeneity in the decision to franchise. Why are certain stores company-owned, and do these same underlying reasons drive the hygiene results? The literature on franchising (see Lafontaine and Slade (2007) for a recent survey) explores the determinants of the variation across firms in the degree of vertical integration of retail branches. One prediction from a moral-hazard model borne out in the data is that when individual store effort matters more, franchising is more common. Some additional variables

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modeled and tested include branch size and complexity of tasks to be performed by store managers. As units of a *particular* restaurant chain are nearly identical, however, most of these cross-sectional predictions cannot explain why Burger King chooses to own store A but franchise store B. One theory that can apply, because stores of a chain do differ in location, is that stores further from headquarters are more likely to be franchised. This is because it is more costly for HQ to monitor product quality for more distant stores, and thus incentives need to be stronger for distant managers to do the right thing. This is achieved by giving them claim over residual franchise profits. This can be a concern if distance to HQ is correlated with operational practices through channels other than PE degree of control. For example, areas closer to HQ may have higher income, and higher income areas may exhibit a greater response to managerial changes. We mitigate with this concern by including zip code-by-year fixed effects in our regressions.

Figure 2 presents an example of franchising outcomes. Of the 21 Burger Kings in Tampa, Florida in 2012, eight are owned by franchisees. These stores are dispersed among the company owned units. The Figure compares two stores, one direct owned and one franchised, a few miles apart. The appearance is similar and customers cannot trivially detect whether a store is franchised or directly owned.

B.2 Main Results

For the sake of this test, we are interested in chains that have a mixture of both franchises and directly owned restaurants. Therefore, our sample only includes chains that employ franchising for at least 5% of its units and no more than 95% of its units in Florida.[15]

[15] Results remain similar if we use a 10% top/bottom cutoff.

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In Table VI we regress critical violations on *PostPE* but now also include the indicator variable *DirectOwn* and the interaction *PostPE* * *DirectOwn*. We have the licensed owner each year at each address, and thus *DirectOwn* equals one if the storeowner is the same as the ultimate parent. This specification allows extraction of a differential private equity effect on directly owned versus franchised units. We also include store and year fixed effects.

In column 1 the interaction term is negative and significant. The coefficient on *PostPE* * *DirectOwn* is -0.32, while the coefficient on *PostPE* alone is still negative at -0.22 but is insignificant.[16] Thus the reduction in critical violations is concentrated in directly owned stores. In column 2 we include the number of employees and seats and results are similar, suggesting that improvements at health practices at directly owned restaurants are not driven by changes to the number of employees or number of seats. Moreover, these improvements in health-related practices cannot be driven by hidden variation in strength of brand, popularity of food genre, or advertising strategy because all branches are identical along these dimensions. In columns 3 and 4 we replace year fixed effects with zip code-by-year fixed effects to address concerns regarding franchisee location choice. The results are unchanged. Overall, these results suggest that within the organization, improvements in health and sanitation practices are concentrated in stores in which PE has greater control.

Figure 3 explores the evolution of operational practices of franchisees and directly owned stores around private equity takeover. The red bars plot the coefficients from a regression in which critical violations are regressed on private equity entry event year dummies, focusing on directly owned stores only.[17] Violations are flat in the three years before PE entry,

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[16] The independent variable *DirectOwn* does not drop out of the regression with store fixed effects because some stores switch between parent and franchise ownership.
[17] This regression is identical to the one reported in column (1) of Table 1A, but the sample is restricted to directly owned stores only. The regression includes store fixed effects, zip code by year fixed effects, log number

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mitigating concerns that private equity was targeting a chain because of an upward trend in its directly owned stores. The decline in critical violations then occurs steadily over the subsequent four years (becoming statistically significant from year 2 onward). In contrast, the blue bars plot the evolution of critical violations of the franchisees around the PE buyout. Similarly to the directly owned stores, no trends exist in the years leading to the buyout. In addition, franchisees as a whole do not seem to improve their operational practices in the years following the buyout, as none of the event year coefficients are statistically different from zero.

B.3 Spillovers

Are all franchisees equally reluctant to implement changes? We find evidence of management spillover effects. We hypothesize that a franchisee that sees the impact of private equity or feels the competitive pressure from a better-managed store will be more likely to improve its own operations. In Table VII, we separate franchised branches into those with and without a same-brand, company owned store in the same zip code. Rather than singling out directly owned stores (as in Table VI), we focus on franchised stores in Table VII. The variable *CloseBy* equals one for a franchised store if a directly owned store of the same chain exists in the same zip code in a given year. Column 1 shows that franchisees have significantly more critical violations after PE entry than company stores—a mirror image of the result in Table VI. The negative coefficient on the triple interaction *PostPE * Franchisee * CloseBy* shows, however, that those franchisees located in the same zip code as directly owned restaurants

of seats and log number of employees. The regression results are in column (3) of Table 1A, in the Appendix. The average number of critical violations was added to coefficients in the graph to illustrate the relative size of the coefficients.

behave more similarly to PE controlled stores. Columns 2 and 3 register the post-PE effect only one and two years after PE firms actually enter.

Operational practices in franchisees that are *CloseBy* appear to converge to their directly-owned counterparts over time, as this interaction term grows over time both in terms of magnitude and statistical significance. This suggests that within-chain competitive pressures lead franchisees to adopt the improved practices.[18]

C. Employment

Private equity firms may make operational changes to restaurants along margins besides health-related practices. Their effect on employment is controversial. The popular press often chides private equity for eliminating jobs for debt service and short-term profits, while Davis, et al. (2013) find that private equity transactions result in only modest net impact on employment.

We explore the effect on this stakeholder in Table VIII. The dependent variable is the log of the number of employees at the level of the store. In column (1) we include year fixed effects and explore variation within a store by adding individual store fixed effects. The coefficient on *PostPE* is negative and significant, suggesting that PE firms do appear to operate existing restaurants with fewer employees than before. The magnitude is fairly modest, as the coefficient equals -0.028, suggesting a 2.8% decline in a store's workforce. To control for the possibility that PE targets are located in areas that, perhaps due to varying economic conditions, have employment patterns different from other restaurants, we include zip code-by-year fixed effects in column (2). PE restaurants still see a decline in workers even when

[18] One may be concerned that the main results is that franchisee restaurants are optimally operated and therefore operational improvements occur at directly owned stores only. This evidence on spillover effects illustrates that this is not the case, as franchisees adopt practices once they are subject to competitive pressures.

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adjusting for geographic variation and local shocks. In columns 3 and 4 of Table VIII we include the *PostPE * DirectOwn* interaction to see if the employment effect is stronger in directly controlled branches. To do so, we restrict the sample to chains that employ franchising for at least 5% of its units and no more than 95% of its units in Florida as in Section B. The interaction is essentially zero, meaning both company-owned and franchised outlets see a similar decline in headcount. It is possible that relative to health-related practices, employee counts are more easily contractible and hence easier for the parent to mandate. Franchisees may also be more amenable to suggestions that lower their costs.

D. Menu prices

To continue identifying operational changes at private equity owned restaurants, we turn to pricing. Do the improved operational practices come at the expense of higher prices? Or is cost cutting passed on to the consumer? We gather annual menus from 2005-2012 for 2,178 restaurant chains from Datassential. Datassential draws a representative menu each year from each of these chains. There can be regional differences in pricing; we assume that the randomly drawn menu is representative of the entire chain. Unlike with inspections and employment, our pricing analysis will thus necessarily be at the overall chain level, as individual store pricing is not widely available. The menu data includes the restaurant name, every menu item (e.g., "Hot and sour soup"), its price, and its broad item category ("Soup—appetizer"). Each restaurant is also categorized into one of four segments (Quick service, Casual, Midscale, Fine dining) and one of 24 cuisine types (e.g., Chinese).

For each restaurant-year, we first generate *itemtype_price*, which averages the prices of all items in each broad category. Thus instead of having five soups with different prices, we collapse these into a single average "soup" price for each restaurant, each year. We also again

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create the variable *PostPE* which equals one for all restaurant-year menus drawn after a private equity firm has acquired the chain. The unit of observation is restaurant's *itemtype_price* each year. In Table IX, column 1, *itemtype_price* is regressed on *PostPE* and chain and year fixed effects. The coefficient is -0.29 and weakly significant. This means, relative to average prices for all restaurants, the average menu item is 29 cents cheaper in years after PE takeover, reflecting a 4.4% decline in menu prices.

We refine this analysis by using only close competitor pricing as a counterfactual. Holding steak prices constant is actually a relative decline if other steakhouses charge more. We replace year fixed effects with "year × cuisine type × segment × item type" fixed effects. The unit of observation in these regressions is a restaurant's *itemtype_price* each year. For Applebee's "cold sandwich" price in 2005, then, the new fixed effect controls for "cold sandwich" (item type) prices sold by all other American (cuisine type), Casual (segment) restaurants in 2005[19]. The regression in column 2 with these fixed effects shows a coefficient of -30 cents on *PostPE*, still significant at 10% with magnitude corresponding to a 4.7% decline in average menu prices. Thus private equity restaurant prices fall relative to those of their closest competitors. Regressions 3-7 look at pricing changes in specific categories. Entrées, the most expensive menu item, show the largest and most significant declines.

Overall, food prices go down following the PE buyout, suggesting that improvements in operational practices and food safety do not translate into higher prices for consumers.

E. Store Closures

[19] For these fixed effects to provide meaningful comparisons, we drop observations without at least 10 cuisine type × segment × item type competitors. "Italian, Fine Dining, Fried Chicken" data points, for example, would likely be dropped. For consistency, we also apply this cutoff in column 1 of Table VIII.

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So far, the evidence suggests that private equity induces significant operational changes to health practices, workforce, and menu prices. It is natural to wonder whether these changes translate into increased profitability. While we do not observe store-level revenue and profits, store closures can be used as a proxy for poor financial performance. In this section, we explore how PE buyouts affects store closures.

The store closures analysis is reported in Table X. The dependent variable is a dummy variable indicating the last year in which a store operated.[20] In column 1 we control for year and store fixed effects, as well as number of employees and number of seats in the restaurant. The coefficient on *PostPE* is negative and statistically significant, suggesting that PE firms do reduce closure hazard. The magnitude of the effect is meaningful, as following the PE buyout, closure likelihood drops by almost 4%. Since closure likelihood may vary across regions and demographics, we control for zip-code by year fixed effects in column 2. PE restaurants still illustrate a decline in workers even when adjusting for geographic variation and local shocks.

In columns 3 and 4 of Table X, we explore whether both franchisees and directly owned stores experience a reduction in closure likelihood. We include *PostPE * DirectOwn,* and as in previous analysis, restrict the sample to chains that employ franchising for at least 5% of its units and no more than 95% of its units in Florida. The interaction is negative and statistically significant, implying that the decline in closure likelihood is concentrated in directly owned restaurants.

These results are consistent with the earlier finding that improvements in operational practices are concentrated in directly owned stores as well. Consistent with Jin and Leslie (2003), the results suggest that improvements in health-related operational practices translate into store profitability.

[20] Since our panel data ends in 2012, we cannot detect store closures in that year.

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III. Conclusion

We study the operational consequences of private equity buyouts in the restaurant industry. This industry provides a unique and detailed view of the daily operational practices of firms. We find that restaurants improve operational practices following the PE buyout and commit fewer health violations. These effects are driven by those practices that pose critical hazards for customers and public health and also most correlate with customer satisfaction and restaurant closures.

We illustrate that the effect is causal and not a mere outcome of the initial investment decision of the PE firm. Within the same chain and region, the effects are strongest in stores over which PE firms have complete control. Franchisees, which are otherwise identical, do not see the same initial improvement, suggesting that PE firms cause these changes. However, franchisees do improve subsequently if they are facing competition from directly owned stores, illustrating spillover effects. We also find that PE-backed restaurants slightly reduce employee headcount at existing stores, lower menu prices, and are less likely to close.

These findings suggest that PE firms take an active role in the firms they acquire and improve operational practices. Improving such practices requires not only capital budgeting, but also, perhaps more importantly, better training, monitoring, and alignment of worker incentives throughout the chain. We interpret this as evidence that private equity firms mitigate agency problem and improve management practices in the organization.

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Figure 1: Critical and non-critical violations around private equity deal date.
This figure plots the coefficients and 95% confidence interval bands of regressions of critical and non-critical violations on event year dummy variables around the date private equity acquires a restaurant. Additional control variables are restaurant fixed effects, year fixed effects, number of employees, and number of seats. Standard errors are clustered at the level of the chain. Event year 0 is the omitted variable, corresponding to inspections that occur from 1 to 365 days after the deal close date. The average number of critical and non-critical violations was added to coefficients in the graph.

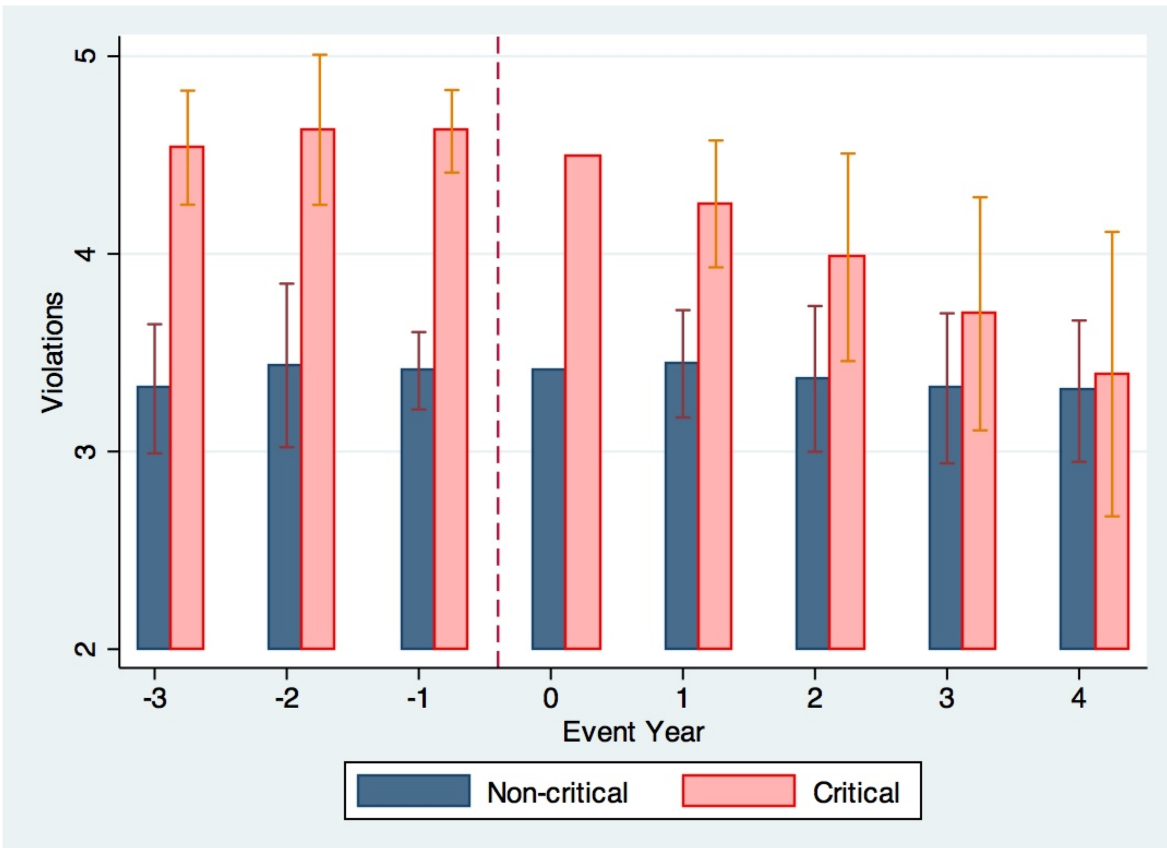

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Figure 2: Franchised vs. directly owned Burger King restaurants – Tampa, Florida

Directly-owned

Franchised

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Figure 3: Franchisees and directly-owned restaurants around private equity deal date.
This figure plots the coefficients and 95% confidence interval bands of regressions of critical violations by directly-owned and franchised stores on event year dummy variables around the date private equity acquires a restaurant. Additional control variables are restaurant fixed effects, year fixed effects, number of employees and number of seats. Standard errors are clustered at the level of the chain. Event year 0 is the omitted variable, corresponding to inspections that occur from 1 to 365 days after the deal close date. The average number of critical violations was added to coefficients in the graph.

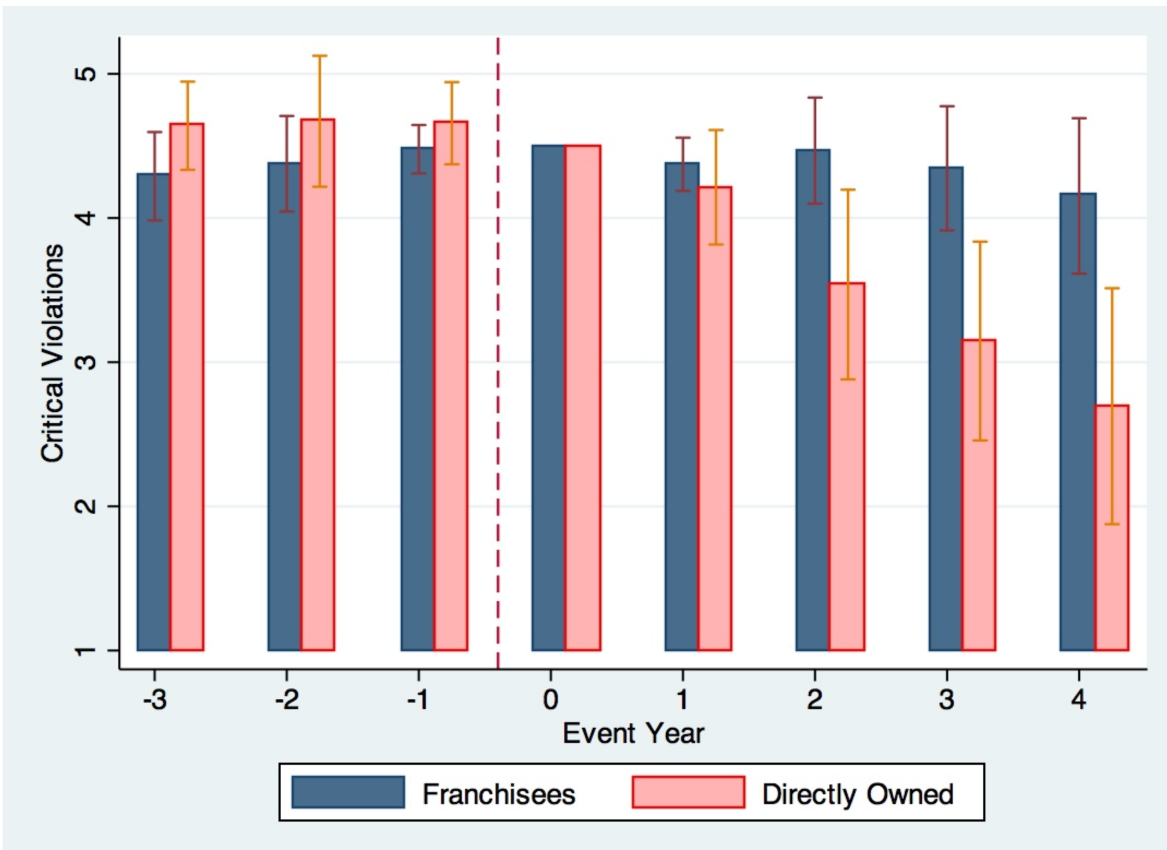

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Table I

Inspection Summary Statistics

This table summarizes the Florida restaurant health inspection data. Critical violations are those "likely to directly contribute to food contamination, illness or environmental degradation." Non-critical violations "do not directly relate to foodborne illness risk, but preventive measures are required." Only routine, surprise inspections are counted.

Panel A – Distribution of Inspections over time

Year	Inspections Conducted	Restaurants Inspected	Inspections per restaurant	Average critical violations	Average non-critical violations
2002	21,676	15,656	1.38	1.67	2.31
2003	45,658	19,024	2.40	2.17	3.07
2004	37,374	18,910	1.98	2.31	3.68
2005	38,419	19,352	1.99	2.80	4.22
2006	42,981	20,701	2.08	4.85	4.72
2007	43,472	21,079	2.06	6.62	4.00
2008	50,519	21,755	2.32	6.19	3.95
2009	61,103	23,959	2.55	5.34	3.26
2010	66,997	27,240	2.46	5.25	3.25
2011	69,460	27,396	2.54	4.70	2.76
2012	63,503	25,800	2.46	4.56	2.71
Mean	49,197	21,897	2.20	4.49	3.41

Panel B – Restaurant Characteristics

	Ever Treated Stores		Never Treated Stores		Never Treated Stores (at least 5 stores)		All Stores	
	Mean	Std	Mean	Std	Mean	Std	Mean	Std
Violations	7.21	5.19	8.19	6.17	6.91	5.25	8.09	6.08
Critical Violations	3.79	3.15	4.69	4.01	3.80	3.31	4.59	3.94
Non-critical Violations	3.42	2.82	3.50	3.07	3.11	2.71	3.49	3.04
Log Seats	4.16	1.28	3.77	1.51	3.56	1.69	3.81	1.49
Log Employees	3.07	0.83	2.39	1.08	2.83	1.03	2.46	1.07
Chain units (in Florida)	175.01	168.18	109.82	247.54	282.14	331.85	116.62	241.31
Log County income	10.50	0.20	10.53	0.21	10.51	0.20	10.53	0.21
Store-year observations	25,752		221,228		85,511		246,980	
Unique Stores	3,727		48,451		13,192		52,178	
Unique Chains	103		30,257		376		30,360	

Panel C – Distribution across cuisines (%)

Cuisine	Ever Treated Stores	Never Treated Stores	Never Treated Stores (at least 5 stores)	All Stores
American	18.36	27.32	17.29	25.95
Asian	0.90	10.94	8.59	9.41
Chicken	11.96	6.44	10.25	7.28
Donut, Ice Cream, Beverage	5.09	5.00	5.88	5.01
Hamburgers	32.42	10.74	17.73	14.05
Other Ethnic	6.64	8.64	5.68	8.33
Pizza, Past, and Italian	7.77	13.63	13.89	12.74
Sandwiches, Soups, and Salads	9.16	12.70	17.35	12.16
Steak, Seafood, and Fish	7.70	4.60	3.35	5.07

Panel D – Distribution across price categories (%)

Average Restaurant Check	Ever Treated Stores	Never Treated Stores	Never Treated Stores (at least 5 stores)	All Stores
Under $7	50.52	31.12	48.08	34.12
$7 to $10	29.47	26.41	28.11	26.88
$10 to $15	15.58	36.68	21.8	33.42
Over $15	4.43	5.79	2.02	5.58

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Table II

Drivers of Restaurant Health and Cleanliness

This table reports general determinants of restaurant health inspection outcomes. Violations are as defined in Table I. *Units in chain* counts the total number of separate stores of that particular restaurant chain in Florida each year. *Median county income* is the median income each year in the restaurant's county. Standard errors are omitted for cuisine types for brevity. Standard errors are clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	Critical violations	Critical violations	Non-critical violations	Non-critical violations
	(1)	(2)	(3)	(4)
Log(Units in chain)	-0.238***		-0.112***	
	(0.026)		(0.020)	
Log(Seats)	0.262***	0.195***	0.223***	0.141***
	(0.024)	(0.027)	(0.022)	(0.022)
Log(Employees)	0.065**	0.079***	0.179***	0.096***
	(0.033)	(0.020)	(0.024)	(0.015)
Log(Median county income)	-0.535***	-0.334**	-0.509***	-0.345***
	(0.101)	(0.132)	(0.074)	(0.088)
Average check under $7	-0.632**		-0.232	
	(0.275)		(0.219)	
$7 - $10	-0.394**		-0.095	
	(0.164)		(0.128)	
$10 - $20	0.220*		0.207*	
	(0.132)		(0.120)	
<u>Cuisine type</u>				
American- omitted category				
Asian	1.628***		1.050***	
Chicken	0.032		0.543***	
Donut, ice cream, beverage	-0.542**		-0.530**	
Hamburgers	-0.240		-0.433	
Other ethnic	-0.101		-0.224	
Pizza, pasta, Italian	0.178		-0.136	
Sandwiches, soup, deli	-0.417*		-0.594***	
Steak, seafood	-0.263		-0.038	
Year fixed effects	X	X	X	X
Chain fixed effects		X		X
Observations	345,489	345,489	345,489	345,489
R2	0.20	0.32	0.10	0.21

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Table III

Health Violations, Customer Satisfaction, and Store Closure

This table presents results from OLS regressions of customer satisfaction and restaurant closure on restaurant sanitation. In panel A, each observation is a restaurant chain. The dependent variable *Avg Yelp stars* is the average star rating (which can range from 1 to 5) for all reviews given to all branches in a chain in 2012 on the website Yelp.com. The independent variable *Avg critical (non-critical) violations* averages the critical (non-critical) violations for all inspections for all branches in a chain in 2012. The restriction "5 or more reviews" refers to the number of Yelp reviews for the chain in 2012. In panel B, each observation is a store-year. The dependent indicator variable equals one if the store closed in that year. *Annual critical violations* is the average number of such violations in all inspections at that store that year. Lagged violations average those the year before the closure year. Standard errors are in parentheses and clustered by restaurant chain in panel B. *, **, *** indicate significance at 10%, 5%, 1%.

	Panel A: Dependent variable = Avg Yelp stars					
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
Avg critical violations	-0.0252***	-0.0247***	-0.0228***	-0.0190***	-0.0208***	-0.0189***
	(0.0030)	(0.0031)	(0.0031)	(0.0038)	(0.0038)	(0.0039)
Avg non-critical violations				-0.0153**	-0.0099*	-0.0101
				(0.0060)	(0.0059)	(0.0068)
Price × Cuisine fixed effects		X	X		X	X
5 or more reviews			X			X
Observations	5,876	5,876	2,814	5,876	5,876	2,814
R2	0.012	0.048	0.099	0.013	0.048	0.100

	Panel B: Dependent variable = Store closure		
	(1b)	(2b)	(3b)
Annual critical violations	0.0010***		0.0012***
	(0.00027)		(0.00029)
Lagged annual critical violations	0.0024***		0.00213***
	(0.00026)		(0.00027)
Annual non-critical violations		-0.00013	-0.00064**
		(0.00028)	(0.00030)
Lagged non-annual critical violations		0.0018***	0.00068**
		(0.00028)	(0.00028)
Log(Seats)	0.0085**	0.0092**	0.0085**
	(0.0038)	(0.0038)	(0.0038)
Log(Employees)	0.0090***	0.0089***	0.0090***
	(0.0013)	(0.0013)	(0.0013)
Year fixed effects	X	X	X
Firm fixed effects	X	X	X
Observations	219,179	219,179	219179

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R2	0.53	0.52	0.53

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Table IV

Violations under Private Equity Ownership

This table presents results from OLS regressions of restaurant inspection results on private equity ownership and store characteristics. An observation is an inspection on a specific date at a specific restaurant address. The dependent variables are defined in Table I. *PostPE* is a dummy variable which equals one if a restaurant is owned by a private equity firm on that inspection date. *Log(Seats)* and *Log(Employees)* count the number of seats and full-time equivalent employees at the restaurant in the year of the inspection. Zip × Year fixed effects use the zip code of each restaurant. Standard errors are in parentheses and clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	Panel A: Dependent variable = Critical violations					
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
PostPE	-0.662***	-0.647***	-0.627**	-0.625**	-0.614**	-0.612**
	(0.238)	(0.240)	(0.249)	(0.251)	(0.252)	(0.253)
Log(Seats)		0.212***		0.239***		0.178***
		(0.0257)		(0.0503)		(0.054)
Log(Employees)		0.0676***		-0.0289		-0.028
		(0.0169)		(0.0188)		(0.020)
Chain fixed effects	X	X				
Store fixed effects			X	X	X	X
Year fixed effects	X	X	X	X		
Zip × Year fixed effects					X	X
Observations	541,147	541,147	541,147	541,147	541,147	541,147
R2	0.122	0.134	0.122	0.137	0.536	0.535

	Panel B: Dependent variable = Non-Critical violations					
	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
PostPE	0.0526	0.0685	0.087	0.084	0.024	0.023
	(0.139)	(0.141)	(0.154)	(0.156)	(0.148)	(0.150)
Log(Seats)		0.155***		0.006		0.037
		(0.0208)		(0.041)		(0.037)
Log(Employees)		0.0841***		-0.007		-0.003
		(0.0134)		(0.012)		(0.012)
Chain fixed effects	X	X				
Store fixed effects			X	X	X	X
Year fixed effects	X	X	X	X		
Zip × Year fixed effects					X	X
Observations	541,147	541,147	541,147	541,147	541,147	541,147
R2	0.031	0.046	0.030	0.031	0.471	0.470

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Table V

Restaurant Health Violations by Category

This table presents results from OLS regressions of violations in disaggregated categories of restaurant maintenance and sanitation on private equity ownership and store characteristics. An observation is an inspection on a specific date at a specific restaurant address. Appendix A details the specific critical and non-critical violations that belong to each category. The independent variables are as defined in Table IV. Standard errors are in parentheses and clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	Food Handling	Maintenance (Kitchen)	Maintenance (Non-Kitchen)	Consumer Advising	Training/ Certification
	(1)	(2)	(3)	(4)	(5)
PostPE	-0.351***	-0.0274*	-0.0743	-0.0929***	-0.0167
	(0.133)	(0.0163)	(0.0530)	(0.0321)	(0.0194)
Log(Seats)	0.107***	0.0111	0.0301**	0.0151*	0.00854
	(0.0301)	(0.00873)	(0.0125)	(0.00828)	(0.00758)
Log(Employees)	-0.0112	-0.000521	-0.00705	-0.00524*	-0.00574**
	(0.0107)	(0.00237)	(0.00551)	(0.00278)	(0.00235)
Store fixed effects	X	X	X	X	X
Year fixed effects	X	X	X	X	X
Observations	541,147	541,147	541,147	541,147	541,147

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Table VI

Inspection Results in Directly Owned versus Franchised Stores

This table presents results from OLS regressions of critical violations on private equity ownership and store characteristics. An observation is an inspection on a specific date at a specific restaurant address. The independent variable *DirectOwn* is a dummy variable which equals one if the restaurant is owned and operated by its brand's parent company in a given year. *DirectOwn* equals zero if the restaurant is run by an independent franchisee. The remaining variables are as defined in Table IV. Standard errors are in parentheses and clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	Critical Violations	Critical Violations	Critical Violations	Critical Violations
	(1)	(2)	(3)	(4)
PostPE	-0.223	-0.222	-0.160	-0.159
	(0.306)	(0.306)	(0.266)	(0.266)
PostPE * DirectOwn	-0.316**	-0.315**	-0.319**	-0.319**
	(0.150)	(0.150)	(0.141)	(0.141)
DirectOwn	0.106	0.102	0.114	0.111
	(0.133)	(0.134)	(0.121)	(0.121)
Log(Seats)		0.232***		0.146*
		(0.071)		(0.086)
Log(Employees)		-0.034		-0.023
		(0.024)		(0.025)
Store fixed effects	X	X	X	X
Year fixed effects	X	X		
Zip × Year fixed effects			X	X
Observations	179,390	179,390	179,390	179,390
R2	0.107	0.111	0.520	0.520

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Table VII

Spillovers from Directly-Owned Stores to Franchisees

This table presents results from OLS regressions of critical violations on private equity ownership and store characteristics. An observation is an inspection on a specific date at a specific restaurant address. PE entry year of "1 year lag" treats PE entry dates as if they occurred one year later. The independent variable *Franchisee* is a dummy variable which equals one if the restaurant is owned and operated by an independent franchisee in a given year. *Franchisee* equals zero if the restaurant is run by the brand's parent company. *Closeby* is a dummy variable equal to one if a store is franchisee-owned and there exists a company-owned branch of the same chain in the same zip code. All cross-terms are included but not reported for brevity. The remaining variables are as defined in Table IV. Standard errors are in parentheses and clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	Critical violations	Critical violations	Critical violations
PE entry year	Actual entry	1 year lag	2 year lag
	(1)	(2)	(3)
PostPE	-0.479	-0.701*	-0.924***
	(0.299)	(0.364)	(0.278)
PostPE * Franchisee	0.332**	0.378***	0.484***
	(0.149)	(0.131)	(0.137)
PostPE * Franchisee * CloseBy	-0.25	-0.362*	-0.590***
	(0.232)	(0.197)	(0.212)
Log(Seats)	0.147*	0.151*	0.155**
	(0.0858)	(0.0821)	(0.0787)
Log(Employees)	-0.0229	-0.0221	-0.0167
	(0.0249)	(0.0248)	(0.0247)
Store fixed effects	X	X	X
Zip × Year fixed effects	X	X	X
Observations	179,390	179,390	179,390

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Table VIII

Restaurant Employment Under Private Equity Ownership

This table presents results from OLS regressions of restaurant employment on private equity ownership. The dependent variable *Employees* is the average number of full-time equivalent employees at a store in a given year. The remaining variables are as defined in Table VI. Standard errors are in parentheses and clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	(1)	(2)	(3)	(4)
	Employees	Employees	Employees	Employees
PostPE	-0.028**	-0.022**	-0.027***	-0.021**
	(0.009)	(0.011)	(0.011)	(0.009)
PostPE * DirectOwn			0.003	-0.001
			(0.009)	(0.009)
DirectOwn			0.008	0.016
			(0.009)	(0.011)
Log(Seats)	0.022***	0.028***	0.014*	0.022***
	(0.006)	(0.007)	(0.007)	(0.008)
Store fixed effects	X	X	X	X
Year fixed effects	X		X	
Zip × Year fixed effects		X		X
Observations	238,415	238,415	74,067	74,067
R2	0.004	0.971	0.019	0.426

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Table IX

Restaurant Prices under Private Equity Ownership

This table presents results from OLS regressions of restaurant menu prices on private equity ownership. An observation is a menu item type at a particular restaurant in a given year. The dependent variable *Item type price* is the average price of all menu items in a food category (e.g., "cold sandwiches") sold by a particular restaurant in a given year. An example of a Year × Cuisine × Segment × Item type fixed effect is "2005, Chinese, Fine dining, dessert." The data comprise menus from 2,178 restaurant chains sampled annually from 2005-2012. Standard errors are in parentheses and clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

Menu items	All	All	Appetizer	Beverage	Dessert	Entrée	Side
	Dependent variable = Item type price						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PostPE	-0.285*	-0.304*	-0.144	-0.178*	-0.318	-0.456**	-0.0896
	(0.171)	(0.180)	(0.374)	(0.0976)	(0.523)	(0.208)	(0.115)
Chain fixed effects	X	X	X	X	X	X	X
Year fixed effects	X						
Year × Cuisine × Segment × Item type fixed effects		X	X	X	X	X	X
Observations	374,891	374,891	65,281	67,757	32,635	116,190	77,076
R2	0.185	0.497	0.51	0.426	0.523	0.479	0.427

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Table X

Store Closure Under Private Equity Ownership

This table presents results from OLS regressions of restaurant closure likelihood on private equity ownership. An observation is a store-year. The dependent variable *Close* is an indicator variable equal to one for store-years which occur in the last year in which a store operates. The remaining variables are as defined in Table VI. Standard errors are in parentheses and clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	(1)	(2)	(3)	(4)
	Close	Close	Close	Close
PostPE	-0.0398***	-0.019*	-0.006	0.003
	(0.011)	(0.011)	(0.006)	(0.004)
PostPE * DirectOwn			-0.007*	-0.0105**
			(0.003)	(0.004)
DirectOwn			0.006	0.006
			(0.004)	(0.005)
Log(Seats)	0.003	0.005	-0.001	-0.005
	(0.003)	(0.004)	(0.005)	(0.006)
Log(Employees)	0.009***	0.016***	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.003)
Store fixed effects	X	X	X	X
Year fixed effects	X		X	
Zip × Year fixed effects		X		X
Observations	238,415	238,415	74,067	74,067
R2	0.041	0.458	0.019	0.415

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Appendix

Table 1A

Year-by-year Impact of Private Equity on Violations

This table replaces the variable *PostPE* replaced with event year dummies for the year relative to PE entry. Event year 0, corresponding to inspections that occur from 1 to 365 days after the deal close date, is omitted. The dependent variable in columns (1), (3) and (4) is critical violations, and non-critical violations in column (2). In columns (1) and (2), the sample includes restaurants. In columns (3) and (4) samples consist of directly-owned or franchised restaurants respectively. The coefficients in columns (1) and (2) are plotted in Figure 1, and columns (3) and (4) are plotted in Figure 2. Standard errors are clustered by restaurant chain. *, **, *** indicate significance at 10%, 5%, 1%.

	(1)	(2)	(3)	(4)
Dependent Variable	Critical Violations	Non-critical Violations	Critical Violations	Critical Violations
Sample	All	All	Directly Owned	Franchisees
Year -3	0.0473	-0.092	0.150	-0.200
	(0.175)	(0.199)	(0.186)	(0.186)
Year -2	0.138	0.025	0.181	-0.114
	(0.231)	(0.252)	(0.276)	(0.202)
Year -1	0.130	-0.002	0.168	-0.012
	(0.127)	(0.119)	(0.173)	(0.102)
Year 1	-0.237	0.033	-0.277	-0.117
	(0.195)	(0.165)	(0.241)	(0.112)
Year 2	-0.507	-0.042	-0.951**	-0.022
	(0.319)	(0.224)	(0.400)	(0.224)
Year 3	-0.793**	-0.090	-1.343***	-0.146
	(0.358)	(0.231)	(0.419)	(0.262)
Year 4	-1.098**	-0.105	-1.795***	-0.337
	(0.438)	(0.217)	(0.497)	(0.328)
Log (Seats)	0.162***	0.030	0.149**	0.098
	(0.051)	(0.038)	(0.059)	(0.081)
Log (Employees)	-0.019	0.001	-0.039*	-0.020
	(0.020)	(0.012)	(0.023)	(0.030)
Store fixed effects	X	X	X	X
Zip × Year fixed effects	X	X	X	X
Observations	510,457	510,457	347,406	163,051
R2	0.538	0.475	0.549	0.525

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Inspection Violation Descriptions

Critical violations recorded by the Florida Department of Business and Professional Regulation:

- Food obtained from approved source
- Original container; properly labeled, date marking, shell stock tags
- Consumer advisory on raw/undercooked animal products
- Cold food at proper temperatures during storage, display, service, transport and cold holding
- Foods properly cooked/reheated
- Foods properly cooled
- Unwrapped or potentially hazardous food not re-served
- Food protection during storage, preparation, display, service, transportation
- Foods handled with minimum contact
- Personnel with infections restricted
- Hands washed and clean, good hygienic practices (observed), alternative operating plan
- Food contact surfaces clean and sanitized
- Restrooms with self-closing doors, fixtures operate properly, facility clean, supplied with hand soap, disposable towels or hand drying devices, tissue, covered waste receptacles
- Toxic substances properly stored
- Employee training verification
- Facilities to maintain product temperature
- Thermometers provided and conspicuously placed
- Potentially hazardous foods properly thawed
- Potential for cross-contamination, storage practices; damaged food segregated
- Dishwashing facilities designed, constructed, operated
- Sanitizing temperature
- Water source safe, hot and cold under pressure
- Sewage and waste water disposed properly
- Cross-connection, back siphonage, backflow
- Toilet and hand washing facilities, number, convenient, designed, installed
- Presence of insects/rodents. Animals prohibited
- Outer openings protected from insects, rodent proof
- Fire extinguishers - proper and sufficient
- Exiting system - adequate, good repair
- Electrical wiring - adequate, good repair
- Gas appliances - properly installed, maintained
- Flammable/combustible materials - properly stored
- Current license, properly displayed
- False/misleading statements published or advertised relating to food/beverage

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Non-Critical violations recorded by the Florida Department of Business and Professional Regulation:

- In use food dispensing utensils properly stored
- Food contact surfaces designed, constructed, maintained, installed, located
- Wiping cloths clean, used properly, stored
- Clean clothes, hair restraints
- Non-food contact surfaces designed, constructed, maintained, installed, located
- Pre-flushed, scraped, soaked
- Wash, rinse water clean, proper temperature
- Non-food contact surfaces clean
- Single service articles not re-used
- Plumbing installed and maintained
- Containers covered, adequate number, insect and rodent proof, emptied at proper intervals, clean
- Outside storage area clean, enclosure properly constructed
- Floors properly constructed, clean, drained, coved
- Walls, ceilings, and attached equipment, constructed, clean
- Lighting provided as required. Fixtures shielded
- Rooms and equipment - vented as required
- Employee lockers provided and used, clean
- Premises maintained, free of litter, unnecessary articles. Cleaning and maintenance equipment properly stored. Kitchen restricted to authorized personnel
- Complete separation from living/sleeping area, laundry
- Clean and soiled linen segregated and properly stored
- Other conditions sanitary and safe operation
- Florida Clean Indoor Air Act

We subdivide all violations into categories for use in Table IV:

Food Handling

- Approved source
- Food Out of Temperature
- Unwrapped or potentially hazardous food not re-served
- Potentially hazardous food properly thawed
- Food protection, cross-contamination
- Foods handled with minimum contact
- Personnel with infections restricted
- Hands washed and clean, good hygienic practices, eating/drinking/smoking
- Sanitizing concentration or temperature
- Food contact surfaces of equipment and utensils clean
- Toxic items properly stored, labeled and used properly
- In use food dispensing utensils properly stored
- Food contact surfaces designed, constructed, maintained, installed, located

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- Wiping cloths clean, used properly, stored
- Clean clothes, hair restraints

Kitchen Equipment Maintenance

- Facilities to maintain product temperature
- Thermometers provided and conspicuously placed
- Dishwashing facilities designed, constructed, operated
- Thermometers, gauges, test kits provided

Restaurant Maintenance (non-kitchen)

- Sewage and wastewater disposed properly
- Toilet and hand-washing facilities, number, convenient, designed, installed
- Presence of insects/rodents. Animals prohibited. Outer openings protected from insects, rodent proof
- Restrooms with self-closing doors, fixtures operate properly, facility clean, supplied with hand-soap, disposable towels or hand drying devices, tissue, covered waste receptacles
- Cross-connection, back siphonage, backflow
- Water source safe, hot and cold under pressure
- Non-food contact surfaces designed, constructed, maintained, installed, located
- Pre-flushed, scraped, soaked
- Wash, rinse water clean, proper temperature
- Non-food contact surfaces clean
- Storage/handling of clean equipment, utensils
- Single service items properly stored, handled, dispensed
- Single service articles not re-used
- Plumbing installed and maintained
- Containers covered, adequate number, insect and rodent proof, emptied at proper intervals, clean
- Outside storage area clean, enclosure properly constructed
- Floors properly constructed, clean, drained, coved
- Walls, ceilings, and attached equipment, constructed, clean
- Lighting provided as required. Fixtures shielded
- Rooms and equipment - vented as required
- Employee lockers provided and used, clean
- Premises maintained, free of litter, unnecessary articles. Cleaning and maintenance equipment properly stored. Kitchen restricted to authorized personnel
- Complete separation from living/sleeping area, laundry
- Clean and soiled linen segregated and properly stored
- Other conditions sanitary and safe operation
- Florida Clean Indoor Air Act

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Consumer Advising

- Original container: properly labeled, date marking, consumer advisory
- False/misleading statements published or advertised relating to food/beverage

Training/Certification

- Current license properly displayed
- Food management certification valid / Employee training verification
- Hospitality Education Program information provided (information only – not a violation)

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Growth LBOs[1]

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Abstract

Using a dataset of 839 French deals, we look at the change in corporate behavior following an LBO relative to an adequately chosen control group. In the three years following a leveraged buyout, targets become more profitable, grow much faster than their peer group, issue additional debt and increase capital expenditures. We then provide evidence consistent with the idea that in our sample, private equity funds create value by relaxing credit constraints, allowing LBO targets to take advantage of hitherto unexploited growth opportunities. First, post-buyout growth is concentrated among private-to-private transactions, i.e. deals where the seller is an individual, as opposed to divisional buyouts or public-to-private LBOs where the seller is a private or a public firm. Second, the observed post-buyout growth in size and post-buyout increase in debt and capital expenditures are stronger when the targets operate in an industry that is relatively more dependent on external finance. These results contrast with existing evidence that LBO targets invest less or downsize.

[1] We gratefully acknowledge financial support from the World Economic Forum, and thank Per Stromberg for invaluable help with the Capital IQ dataset. We also thank INSEE, the French statistical office, for letting us access firm level data. Because the data is confidential, the empirical work in this paper was conducted while David Sraer was a member of the "Markets and Firms Strategy" division at INSEE, and Quentin Boucly was visiting the division. We are very grateful to an anonymous referee for his useful and constructive comments. This paper also benefited from insightful comments by Vicente Cuñat, Steve Davis, Anu Gurung, John Haltiwanger, Yael Hochberg, Josh Lerner, Andreï Shleifer, Per Stromberg. We also thank Laurent Bach for allowing us to access his dataset on private firms ownership. All remaining errors are our own.
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1. Introduction

This paper provides evidence that many LBOs foster firm growth by alleviating credit constraints. This finding contrasts with most of the available literature, which argues that the main source of value creation in LBOs is cost cutting. Studying large public-to-private transactions of the 1980s, Kaplan (1989) shows that LBO targets increase their profitability by cutting down investment, selling off assets, while maintaining operating income constant. Lichtenberg and Siegel (1989) find that privately held LBO targets tend to reduce white-collar employment and wages. More recently, Amess and Wright (2007) and Davis et al (2008), studying the UK and the US market over a longer time period, find that initially privately-held LBO targets experience some employment reduction following a leveraged buyout. Finally, Chevalier (1995) and Chevalier and Scharfstein (1996) provide evidence that, in the 1980s, LBOs in the supermarket industry led to under-investment in market shares. However, such evidence may not be fully representative of today's typical LBO transaction for at least two reasons.

First, many of these papers were written in the 1980s. But the 1980s was a decade of intense corporate restructuring, in the face of international competition and deregulation of many industries. Against this background, financial pressure served to implement painful cost-cutting policies (Jensen, 1993). Since then, the business model of the private equity industry may have changed (Stromberg, 2008). For instance, Guo et al. (2009) find weaker effects on profitability for recent large public-to-private deals, suggesting that the huge gains reaped by private equity investors in the 1980s have vanished.[3] The private equity industry may have devised new sources of value creation: one hypothesis is that some funds may now be targeting underdeveloped, credit constrained, firms, and help them grow faster.[4]

Second, nearly all studies have concentrated on the US and the UK, where capital and credit markets are large and well functioning.[5] In countries where this is not the case, LBOs may help relax targets' credit constraints, allowing them to take advantage of hitherto unexploited growth opportunity. Even if they do not provide direct financing, there are several reasons why private equity funds could increase targets' debt capacity. For instance, private equity funds may be perceived as transparent and activist shareholders. Because they would monitor the firm better than the previous owner(s), they would exert a positive externality on debt holders, who are more senior claimants. Also, private equity funds may bring financial expertise and connections to hitherto financially unsophisticated firms. Last, private equity sponsors may introduce new, more competent members to the executive suite, which may reassure bankers.

France provides us with a natural testing ground for our main hypothesis that LBOs may foster growth by alleviating targets' credit constraints. First, France is a country with many family managed businesses (see e.g. Faccio and Lang (2002) for evidence on publicly listed firms), which may sometimes lack the managerial and financial expertise needed to take

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[3] Another interpretation is that firms now cut their costs without outside intervention, under the pressure of either stockholders or product market competitors (Giroud and Mueller, 2009).

[4] As an illustration, AXA PE, a large French private equity group, argues on its wesite that its Eastern Europe LBO Small Cap fund « will seek to identify targets whose growth opportunities are large but limited by capital constraints ».

[5] Regarding France, the only exception we are aware of is Desbrières and Schatt (2002).

advantage of all growth opportunities.[6] Thus, focusing on France allows us to study an economy with many "sleeping beauties", i.e. potential private targets with significant margins of improvement and growth. Second, the French credit and stock markets are both much less developed than those of the US and the UK.[7] Against this background, it seems at least plausible that in France, private equity groups could help previously credit-constrained firms get access to outside source of finance.

Using two separate sources of data, we identify 839 deals over 1994 – 2004. Our sample is representative of the typical LBO deal in international data. First, sponsors of LBO deals in our sample are mostly large private equity funds: more than 40% of them are not French firms (mostly UK and US funds). Second, the average target's enterprise value in our sample is $395m (in 2007 dollars), compared with an average deal size of $280m in the UK, and $389m in the US (figures from Stromberg, 2008).[8] In terms of number of deals, our sample is smaller than the UK sample of Amess and Wright (2007), who study 1,350 deals, and the US sample of Davis et al (2008), who look at more than 5,000 deals (over a longer period). Once adjusted for the size of the economy however, the LBO market in France appears to be quite similar to that of the US, and only slightly less dynamic than that of the UK.

We then track the corporate behavior of targets before and after the deal, using accounting data extracted from tax files. We compare their evolutions with a carefully constructed control group of similar firms that are not targets of an LBO. Like most existing studies, we first document that LBOs lead to a large and statistically significant increase in the target's profitability. This result is robust: it holds for small or bigger targets, different time periods, and is independent of the target's pre-buyout ownership structure. In this sense, our paper yields results similar to most of the literature on post LBO performance (as, e.g., in Kaplan (1989) and Acharya et al. (2009)).

However, in contrast to existing studies, we find that LBO targets grow after the deal significantly more than comparable firms, in terms of employment, sales and capital employed. This effect is statistically significant and economically large: between the four years preceding the transaction, and the four subsequent years, employment, asset and sales growths of LBO targets are respectively 18, 12 and 12% higher than their control firms. Our own estimates are extremely robust and are not due to differences in methodology with previous studies. In particular, the strong post-LBO growth we observe in our data is robust across time periods. This increase in post-LBO growth is also accompanied with a strong increase in capital expenditures (24% higher for LBOs relative to their control firms). We also find that, after the deal, LBO targets issue additional debt to finance asset growth: this additional debt represents about one third of the average asset growth observed among LBO targets.

[6] Using a restricted sample of medium sized, privately held firms, Bloom and Van Reenen (2007) find that management practices tend to be poor in family managed firms. Besides, all studies find that family firms tend to be, on average, smaller than non-family firms (see Sraer and Thesmar, 2007, for France).

[7] According to Djankov, Mc Liesh and Shleifer (2006), the ratio of private credit to GDP in France is 0.9, as opposed to 1.4 in the UK and the US. According to recent data put together by Beck et al (2000), the ratio of private credit plus stock market capitalization to GDP is equal to 1.7 in France, versus 2,7 in the UK, 3.5 in the US. Finally, France scores low on many dimensions of investor protection, such as the creditor rights index reported in Djankov et al (2006).

[8] In France as in the UK or the US, public-to-private deals, involving a large publicly listed target, are the exception rather than the norm.

Interestingly, these effects depend strongly on the target's pre-buyout ownership structure: the post-buyout increase in firm size, capital expenditures and post LBO debt increase is concentrated among private-to-private transactions. These are deals where the seller is an individual, in most cases a family cashing out of its business. In contrast, divisional buyouts (where the seller is a larger conglomerate) and public-to-private transactions (where the target is listed on the stock market before the buyout) do not spur growth, even though these deals also create an increase in the target's profitability.[9] As targets of private-to-private deals are more likely to be credit constrained before the buyout than former divisions of larger companies or publicly-held firms, our interpretation of these first results is that private equity funds help targets that were previously limited in their access to capital take advantage of unexploited growth opportunities.[10]

We provide further evidence consistent with this credit-constraint hypothesis. In the spirit of Rajan and Zingales (1998), we focus on industries where internal funds are typically insufficient to finance investment (financially dependent industries). We find that post-LBO growth in size, post-LBO debt issues and increase in capital expenditures are larger in these industries, but only for private-to-private transactions. For divisional, secondary, and public-to-private transactions – i.e. for transactions where credit constraints concerns are less likely to be relevant – the target's post-buyout behavior does not depend on its industry financial dependence. In other words, post-buyout growth in size, post-LBO debt issues and increase in capital expenditures are concentrated among private companies that operate in more financially dependent industries. These findings are consistent with the idea that private equity funds increase their portfolio firms' debt capacity.

The remainder of the paper is organized as follows. Section 2 describes the data used. Section 3 establishes the fact that target growth is accelerated following an LBO. Section 4 provides evidence that financial constraints are relaxed after the deal. Section 5 concludes.

2. Dataset

a. Data construction

To analyze the impact of LBO transactions at the company level, we use three different databases: SDC Platinum and Capital IQ (to isolate transactions) and BRN (for financial statements).

First, we retrieve all the deals from SDC Platinum with the following characteristics: (i) they are completed between January 1994 and December 2004 (ii) the target company is incorporated in France (iii) deals are classified as "LBO" by this database.[11] 603 deals match these criteria. We then improve our coverage with transactions from Capital IQ. There, we

[9] Secondary buyouts, where the target already belongs to a private equity group, also experience some post-buyout growth, although to a lesser extent than what is observed for private-to-private transactions.

[10] Consistent with our own findings, Chung (2009) also finds evidence of spectacular post-LBO growth among 170 private-to-private targets, which he also attributes to the relaxation of credit constraints since in his data, public to private deals lead to firm downsizing.

[11] Definition of an LBO according to SDC: an "LBO" occurs when an investor group, investor, or firm offers to acquire a company, taking on an extraordinary amount of debt, with plans to repay it with funds generated from the company or with revenue earned by selling off the newly acquired company's assets. SDC considers an LBO if the investor group includes management or the transaction is identified as such in the financial press and 100% of the company is acquired.

select all 972 deals that were (i) announced between 1994 and 2004 (ii) either "closed" or "effective" (iii) reported by Capital IQ as being "LBOs". The two datasets overlap: thus we start with 1,193 transactions.

Most of the targets are medium sized, privately held firms. We obtain financial statements from tax files (called BRN) available from the statistical office (INSEE).[12] Our transaction and accounting data do not have the same identifier so we match them by company name. Names are not always identical in both databases, so in case of ambiguity we resort to company websites and annual reports. The matching process for selecting a group of control firms, which we describe more extensively in Section 2b below will further reduce sample size to 839 deals.

One possible concern at this stage is that our data construction technique does not account for the group structure that is so prevalent among French firms. Many firms have subsidiaries that are 100% controlled and that may hold more assets or employees than the parent company. If an LBO is followed by a simplification of the corporate structure that leads to the consolidation of all assets and jobs in the target firm, we will overestimate the post-LBO growth of the firm. We deal with this important issue in three ways. First, notice that the assets of subsidiaries are in general already included in the fixed assets of the parent firm via the value of the parent's equity holdings (in the financial fixed assets accounting item). Hence, post LBO simplification may lead, for the parent firm, to a mechanical increase in employment and sales, but not in total fixed assets. Second, for each LBO target, we try to make sure that we focus on the main entity with most real activity, instead of a holding which would own various subsidiaries but no real operation. We do this using company websites and annual reports and by looking at employment and sales figures. Third, we use another data source (LIFI, for "Liaisons Financières"), available from the statistical office, which collects ownership links between parents and subsidiaries. The limitation of this database is that it is a survey, but coverage is good during the time period that we consider. Using this survey, we find that only 20% of our targets have one subsidiary or more. As a result, we do not report results using subsidiary data in most regressions, but use this information in a robustness check.

A second concern is that we may have missed many divisional buyouts, as in such cases the target may not be an independent legal entity before the transaction (but just a division of the selling firm). As it turns out, among our 839 LBOs with non-missing accounting data, there are still 233 divisional buyouts (28%) according to SDC and Capital IQ. This fraction is not changed much by the matching process: before matching, 31% of our transactions (out of 1,193 deals) are divisional buyouts. It means that divisions that are sold tend to be independent legal entities before the transaction, so that they have their own financial statements. On this front, the group structure of the selling firm, very common in France, helps us in following the LBO target before and after the transaction.

All in all, we find that the total number of employees in firms that underwent an LBO between 1994 and 2004 stands at 171,507. This represents approximately 1.4% of employment in our accounting data, and some 0.9% of total French employment. This is smaller than the figure obtained by Davis et al (2008) in their study of US LBOs.

[12] The BRN contains tax files for all French firms, public or private, whose annual sales exceed 100,000 Euros in the service sector and 200,000 Euros in other sectors. See Bertrand, Schoar and Thesmar (2007) for a description of these data.

Using financial statements reported on tax files, we retrieve the following variables: number of employees, fixed assets, working capital (measured as trade receivables plus inventories minus payables), total debt, EBITDA, amortization and depreciation, net income, capital expenditures and industry classification (2 and 4 digit). We measure vertical integration as the ratio value added (sales minus intermediary inputs) divided by sales. The share of export is the ratio of exports to total sales. Profitability is measured through ROA, i.e. EBITDA divided by assets (as measured by fixed assets plus working capital). All the ratios are windsorized at the median plus or minus 5 inter-quartile ranges.

Target leverage is the ratio of target debt to target assets. It is important to note that this measure of leverage uses the accounting information of the target itself, and therefore excludes debt raised for the LBO operation itself. The debt raised for the LBO operation is typically borne by a holding company, which in turn owns the target, so it does not appear in the unconsolidated accounts that we have access to. We believe this information is, however, interesting, since it will inform us on the ability of the target to raise debt *after* the LBO, beyond what has already been raised to finance the deal. In the following, we will refer to our measure of leverage as "target leverage", as opposed to "deal leverage" which is the ratio of debt used in the LBO to enterprise value.

b. Building the Control Group

In order to analyze the impact of LBO operations, we compare the targets of such transactions to similar companies that did not go through a LBO. A matching company (a “control firm”) meets the three following criteria: (1) it belongs to the same 2-digit sector as the target (2) the number of employees one year before the LBO is in the +/-50% bracket of the employment of target company (3) Return on Asset one year before the LBO is in the +/-50% bracket of the ROA of the target company. If there are more than five control firms, we just keep the five neighbors nearest to the target.[13] The choice of ROA and employment is clearly driven by the fact that profitability and size dynamics are the focus of our investigation, and that they tend to mean revert. Regarding the +/- 50% bracket, there is a trade-off between matching accuracy and the need to get a control firm for as many LBO targets as possible. At this 50% level, 85 targets have no control firm and are thus dropped from the sample. If we require employment and ROA to be both at most 20% away from the target, the number of targets with no control firm rises to more than 100 leading to an important decrease in the number of observations.[14]

[Insert Table 1 about here]

The matching methodology allows us to add 3,994 control firms to the sample, i.e. 4.76 control firms by target. By construction, the two groups are not too different, as evidenced by Table 1, which presents pre-buyout descriptive statistics for targets and the median of each group of control firms. The median target has 64 employees, and sales of some 13m€. The median control firm is somewhat smaller (60 employees and 7.8 m€ of sales). The distribution of ROA and pre-deal leverage is almost identical for control and target firms. Finally, pre-LBO growth is slightly lower for control firms. This is comforting given that we did not match on pre-buyout growth. Hence, before the transaction, LBO targets and control are on similar trends.

[13] Distance is defined by the sum of the squares of the difference between the target and the control firm’s ROA and the target’s and the control firm’s number of employees.
[14] However, regression results presented below are almost unchanged with this smaller sample.

Nevertheless, we have to acknowledge that our matching approach has an important limitation since LBOs are not exogenous events. For instance, private equity funds could target firms that are on the verge of expanding. Controlling for pre-buyout characteristics, as we do here, help make this concern less stringent. The fact that growth occurs precisely at the moment of the LBO is also comforting. Yet, in the absence of a proper source of exogenous variation in the probability to be involved in a deal, our results may be subject to an endogeneity bias and should therefore be interpreted as descriptive more than causal.

c. *How Different are French LBOs from the Rest of the World*

Figure 1 shows the number of LBOs per year in our sample. Overall, the number of deals first peaks in 1999, after which LBO activity stagnates until 2003 and then picks up in 2004. This pattern is similar to the evolution recorded by Davis et al. (2008) in their US sample. While there are, in total, less deals in our sample (they have more than 5,000 deals over the 1981-2004 period), part of the reason for this is that the US economy is larger than the French one (GDP is six time bigger). Adjusted for the size of the economy, French LBO activity looks comparable to the US.

The types of sellers involved in our French transactions do not differ much from the typical LBO in the world. 4.3% of the deals are public-to-private transactions in France, as this is the case for Stromberg (2008)'s sample of LBOs around the world. 52% are pure "private-to-private" transactions, in France as in the world.[15] 27.2% are divisional buyouts, against 26% in Stromberg's sample.[16] We have slightly more secondary buyouts (15.4% versus 13%), i.e. transactions involving a financial vendor (most often another private equity fund). Finally, less than 1% of our targets are labeled "distressed", but this figure is also very small in Stromberg's sample (2%).

Deal size and capital structure are also very similar to international data. According to Stromberg (2008), who uses Capital IQ only, the median deal size (in terms of enterprise value) is $64 millions in the US, and $36 millions in the UK. In our French extract of Capital IQ, it is $63 million (deal size is not well reported in SDC). We also have reasons to believe that the use of debt is as pervasive in our French sample as in the transactions studied in previous papers. Unfortunately, there are no comprehensive data on deal structures, especially for the older deals we are investigating in this paper. The "target leverage" variable that we use is based on the target's accounts only, and excludes the amount of debt raised for the control transfer itself (as argued above such debt is borne by a separate holding company which our accounting data does not track). We can nevertheless bring two pieces of evidence suggesting that French deals have comparable leverage to the US and the UK. First, looking at deals made between 2003 and 2006, S&P reports a mean debt to EBITDA ratio of 4.8 for France (232 observations) and the UK (240), and 4.2 for the US (410). Second, we restricted ourselves to the 245 LBOs in Capital IQ in 2005 and have used LoanConnector to retrieve the

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[15] By definition, private-to-private transactions should be deals where an individual owns the target, i.e. where the target is a "family firm". To be sure, we hand-collected the identity of the target's ultimate shareholder for the 47 private-to-private transactions in our data in 2004. Among the 40 targets for which this information was available, we found only three cases where an individual did not own the firm. In one case, the target was a coop, in another it was held by a financial institution and in another by an industrial company.

[16] Looking at pre-deal data, we find that targets of private-to-private deals, i.e. "family firms", are smaller than targets of divisional buyouts (122 vs. 286 employees on average). However, they also grow faster (7% vs. 3% of annual employment growth) and are more profitable (average ROA of 0.25 vs. 0.14).

amount of debt used in these deals.[17] Among them, only 22 have information on both debt used in the deal and enterprise value. Among these 22 deals, average debt to enterprise value is 0.58. It is somewhat smaller than the .67 found by Axelson et al. (2008) in the global sample but their focus is on large LBO transactions. It is, nevertheless, in a comparable range. Interestingly, deal leverage for the 22 deals we have information on is somewhat similar across LBO types and in particular, divisional buyouts do not appear to be more levered pre-buyout than private-to-private deals, at least on this small sample. Let us note, however, that these 22 deals are far from being representative of our whole sample. The average target in our sample has about 8m euros in assets, while the average entreprise value in the 22 deals is 400m. Hence, a deal leverage of about 0.67 which is the norm among larger deals (in France or elsewhere) may very well be different for smaller deals which are more representative of our sample.

The LBO sponsors in our sample are quite representative of the universe of private equity funds around the world. Among the 104 sponsors backing our deals, we have both very large sponsors (such as 3I, Axa PE, CVC Capital Partners, Permira, etc.) as well as smaller ones. There is a majority of French private equity firms (58%), which are on average small (1.4bn € of assets under management). US and UK funds are common (28% of the deals in our sample) and larger on average (3bn € of assets under management for US sponsors and 5.6bn for UK sponsors). All in all, domestic funds are prevalent but there is an important fraction of larger US/UK based funds.

There is, however, one notable difference between US LBOs and the deals in our sample: our transactions involve firms that are older than the typical US targets studied by Davis et al (2008). In their sample, about 50% of targets are more than 10 years old and 25% are less than five years old. In our sample firms are older: 85% of our targets are more than 10 years old, and only 5% younger than five. This difference is consistent with the idea that LBOs involve more mature firms in continental Europe than in the US or the UK. It is important to emphasize, however, that targets do not systematically differ from their control firms on the age dimension, even though age was not a criterion in the matching procedure. This comforts us in thinking that our results are not driven by the mere effect of firm age on firm growth.

d. Industry Level variables

In Section 4, we will use industry-level measures of dependence on external finance and exposure to labor market rigidities. We measure financial dependence at the industry level using the universe of firms present in the tax files with more than 100 employees. We follow the methodology of Rajan and Zingales (1998). For each firm in this sample and for each year, we calculate the difference between capital expenditures and gross cash flows, normalized by investment. Gross cash flows are computed by taking Net Income plus Depreciation and Amortization.[18] This ratio thus measures the fraction of investment that is financed externally. We then remove outliers and compute the average by 2-digit industry

[17] We use 2005 because LoanConnector coverage is really too incomplete before this year and the debt market in 2005 is not yet as over-heated as in 2006/7.

[18] In alternative (non reported) specifications, we used a measure closer to cash flows from operations (Gross cash flows plus interest payments). This alternative measure gives very similar results both in terms of statistical and economic significance. A second alternative is to use, instead of Gross cash flows, Gross cash flows minus change in operating working capital (increase in receivables plus increase inventories minus increase in payables). This second alternative is closer to the actual measure of cash flows from operations, and provides very similar estimates. In the main text, we report regression results with Gross cash flows because these are the ones used in Rajan and Zingales' (1998) paper.

over the 1990 – 2006 period, using all firms present in the tax files. Again, this ratio is computed only on the sample of firms with more than 100 employees. The reason is that this measure is meant to capture the "technological" financial dependence of an industry and should thus be computed using firms that are less likely to be credit constrained (Rajan and Zingales (1998)). As larger firms tend to be less credit constrained, we therefore restrict the sample to firms with more than 100 employees (this corresponds to the two top percentiles of the size distribution). In the appendix, we check the robustness of our results, using 50, 200 or 500 as alternative cutoffs.

The US firms-based measure of financial dependence, as initially computed by Rajan and Zingales (1998) and our measure based on French data are clearly correlated. The linear correlation coefficient is .48 and is significantly different from 0 at the 1.2% confidence level. The spearman rank correlation coefficient is equal to .51, and the test of (possibly non-linear) independence of the two variables is rejected at 0.7%. Hence, the industry variation we capture is similar to the one studied by Rajan and Zingales (1998).[19]

We measure industry exposure to labor market rigidity by using the 1998 wave of the REPONSE survey.[20] This survey is run every six years by the French ministry of labor, and collects information about working conditions at the employee level in a large number of French firms. We use two variables that we first compute at the firm level. The first variable is the fraction of workers that belong to a union. It measures the ability of workers to resist restructuring. The second variable is the firm level fraction of workers that are hired under fixed term contracts. It measures the fraction of the labor force that is "flexible", since, although it is costly to anticipate the termination of FTC, the firm does not have to renew them when they mature (typical duration is one year). We then separately aggregate these two firm level rigidity measures at the 2-digit industry level.

3. Post LBO profits and growth: Evidence and Robustness

a. *Profitability*

We first start by documenting the impact of LBOs on target profitability in our sample. Figure 2 presents the evolution of mean profitability before and after the transaction, compared to control firms. In the spirit of Kaplan (1989), we first compute, for each target and each year before or after the LBO, the difference between ROA and the median ROA of its control firms taken the same year. We call this the "excess ROA" of the target. We then compute the change in excess ROA between each year t and 3 years before the deal. Finally, we compute the mean of such evolutions of excess ROAs, and report this in Figure 2. The average deal is followed by an improvement in operating profitability of around four percentage points. The timing of the improvement offers convincing evidence that something massive happens to LBO targets around the deal. The relative flatness of the evolution of ROA prior to the deal year gives us confidence in the construction of our control firms. Unreported t-tests (as well

[19] To get a better sense of the robustness of our measure of financial dependence, we correlate it with various industry characteristics. We find that financially dependent industries are growth industries and that they are marginally more capital intensive (and hence need investment). They are also more concentrated (which is consistent with capital intensity and credit constraints acting as barriers to entry). And they have lower productivity, perhaps because financing constraints prevent them from reaching the efficient scale.

[20] For a description of this dataset, see Acemoglu et al (2007).

as a Wilcoxon test of median equality) suggest that this sharp increase in ROA of target firms relative to their control firms is highly significant (at the 1% confidence level) from year 1.

To formalize our statistical tests, we perform the following regression:

$$Y_{jt} = \alpha_j + \delta_t + POST_{jt} + POST_{jt} \times LBO_j + \varepsilon_{jt} \quad (1)$$

where j is a firm index and t a time (year) index. Y_{jt} is the performance variable (in this subsection, ROA). If firm j is an LBO target, $POST_{jt}$ equals 1 after the deal and zero before. If j is a control firm, $POST_{jt}$ equals 1 when the target corresponding to j has undergone the LBO, and zero before. LBO_j is equal to 1 for targets, and zero for control firms. This regression includes firm and time fixed effects. As recommended by Bertrand, Duflo and Mullainathan (2003), we cluster error terms at the firm x POST level. Results using ROA and log EBITDA as dependent variables are reported in Table 2, column 1 and 2 respectively.

[Insert Table 2 about here]

LBOs in our sample are associated with an increase of 4.4 percentage points in operating performance. This is economically large. The sample mean of operating profitability is 19% and its standard deviation is about 29%. LBOs are thus associated with a 15% standard deviation increase in ROA. Confirming this result, column 2 of Table 2 reports that target firms' EBITDA increase, relative to their control firms, by a significant 18% following the deal.

A potential concern with our data is that small privately firms have incentives to underreport their earnings in order to avoid the corporate income tax. As a consequence, when these firms are taken over by a private equity fund, their earnings could increase simply because underreporting stops. According to a recent report by the French branch of auditing firm Deloitte (2005), manipulations can be one of four types: (1) optimizing the depreciation/amortization schedule, (2) optimizing the valuation of inventories, (3) smoothing income through exceptional items, and (4) under-reporting sales. In unreported regressions, we show that after a deal (i) depreciation accelerates (ii) changes in inventories remain constant and (iii) exceptional items are not more frequent. Hence, accounting manipulations such as (1), (2) or (3), cannot explain the observed increase in performance following a buyout. Unfortunately, our data does not allow us to address directly manipulation (4), i.e. outright sales under-reporting. However, the firms in our sample are incorporated companies of reasonable size (the median target in the sample has 63 employees the year of the deal) so that sales under-reporting should not be a frequent behavior.[21]

Another concern is that these results on profitability could be driven by asset write-ups at the time of the LBO. If depreciation is accelerated after the buyout, then ROA could increase without a real improvement in the firm's profitability. The results of column 2 of Table 2,

[21] Another related concern is that, before an LBO, managers could be expensing private consumption items to the firm, thus decreasing its accounting profitability. After the LBO, the new management team would stop such behavior, which would automatically increase its accounting profitability. It is hard to test for this channel empirically. Nevertheless, one would suspect that this concern should be stronger for smaller firms where these "private" expenses represent a larger share of overall operational profits. However, we know -- from unreported regressions available from the authors upon request -- that the post-LBO increase in profitability is similar for small *and* large targets.

using log(EBITDA) as a dependent variable, already suggests that profits increase following a leveraged buyout. We also present in column 2 of Appendix Table 1 the estimation of equation (1) where the dependent variable is return on sales (ROS), i.e. EBITDA normalized by the firm total sales instead of assets. This measure of profitability is immune to changes in the depreciation schedule. We find that ROS increases significantly by 1.4 percentage points following an LBO. As the standard deviation of pre-buyout ROS is .19, this represents a 7% standard deviation increase in ROS. The effect is statistically significant at the 1% confidence level. Thus in our sample and however measured, profitability increases following a leveraged buyout.

b. Growth

We turn to the main contribution of this paper: the evidence on growth, debt and capital expenditures. As an illustration, we start with a graphical display of the timing of job creation and capital expenditures after the LBO in Figure 3 and 4. To construct these figures, we first compute, for each firm in the target and control samples, the change in log employment and log capital expenditures between 3 years before the LBO and each year t. We then compute, for each target, the median value of such cumulative employment and capital expenditure growth of control firms, and subtract it from the target's own cumulative growth. We then compute the average of such "excess growth" over all targets and for each year, starting three years before the LBO. Figure 3 shows strong employment growth following LBOs in our sample, up to 18% four years after the transaction. Approximately a third of this employment growth takes place in the year following the buyout. This differential job growth is statistically significant, whether we use a Wilcoxon median test or a student t-test. Similarly, Figure 4 shows very strong capital expenditure growth following LBOs in our sample, up by 40% four years after the transaction. Again, this differential investment growth is statistically significant at the 1% level.

A regression analysis complements this graphical evidence. In Table 2, we estimate equation (1) using as dependent variables different measures of size of operations: log employment (column 3), log sales (column 4), log of fixed assets plus working capital (column 5)), target (not deal) leverage (column 6) and log capital expenditures (column 7). We find that capital expenditures of LBO targets increases by 24% more than their control firms, leading to a relative increase of 7% in assets (fixed assets plus working capital). Both these increases are significant at the 1% confidence level. The surge in operating assets is also accompanied by a strong and significant increase in sales (12%) and employment (12%). Target's leverage (i.e. excluding LBO related debt) also increases by about 2.6 percentage points: hence, after the deal, the target issues additional debt to finance part of its asset growth. All these results are significant at the 1% confidence level.

The increase in post-buyout capital expenditures is particularly remarkable. An important question then is whether this increase in investment takes place through external or internal growth. Our data do not allow us to break down directly capital expenditures into acquisitions and organic growth. However, we can use plant-level data to see whether plant creations/destructions are affected by buyouts. In column 1 of Appendix Table 2, we show that the number of plants created/destructed by LBO targets after the deal is not significantly different from that of their control firms. This suggests that most of the post-LBO growth we observe is likely to arise through organic growth, not through acquisitions.

c. *Potential Sources of Value and Growth Creation*

[Insert Table 3 about here]

In Table 3, we look at further measures of corporate behavior that may explain the source of post-LBO growth. One possibility is that targets outsource part of their production to more cost effective domestic or foreign firms. This can be measured through the ratio of intermediate input consumption (the difference between sales and value added) to total sales, which we use as dependent variable in column 1. As far as this ratio is concerned, there is no difference in its evolution between LBO targets and their control firms. In column 2 of Table 3, we look at the share of working capital in total assets. It is often argued that part of the wealth creation in LBOs comes from leaner inventories and faster payments by customers, which together reduce working capital and allows putting the firm's cash to more productive use. Statistically, however, LBO targets and their control firms exhibit a similar working capital evolution. Finally, we ask if post LBO growth can be explained by an expansion on international markets. We do find that LBO targets, compared to their control firms, increase significantly their sales to foreign markets. However, this effect is economically small. The share of export in sales increases by 1.3 percentage point. For the sake of comparison, the sample mean of the share of exports in total sales is 12%. One possibility is that our linear model does not fit the data very well, as 40% of the firms in our sample do not export at all.

d. Robustness checks

The magnitude of these effects raises some concerns. First, as we mentioned earlier, it could be that private equity funds simply pick targets that were already growing very fast before the transaction. Absent an instrument for the LBO decision, we can partially address this valid concern by including an interaction term designed to control for pre LBO growth:

$$Y_{jt} = \alpha_j + \delta_t + POST_{jt} + POST_{jt} \times LBO_j + POST_{jt} \times GR_j + \varepsilon_{jt} \tag{2}$$

where GR_j is mean firm sales growth, in the three years preceding the transaction. The additional interaction term, $POST_{jt}xGR_j$ captures the fact that LBO targets, compared to their control firms, may initially start with stronger growth. We run these regressions in Table 4, panel A, for each dependent variable used in Table 2. Pre-LBO growth is a strong predictor of post-LBO growth, but it does not affect our initial estimates. This is not too surprising given the descriptive statistics of Table 1: targets and control firms tend to have similar pre-buyout growth, which is confirmed by the fact that figures 3 and 4 show a clear break in differential trend after the deal.

[Insert Table 4 about here]

Second, we have also checked that our results are not driven by the fact that we pool the 4 years following the transaction into a single "post" period. This is already apparent in the graphical evidence of Figure 2, 3 and 4. Nevertheless, we looked at this more formally by computing year-by-year "median-adjusted" changes as for instance in Kaplan (1989) and Guo et al. (2009). More precisely, we compute changes in targets behavior at different points in time and compare them to the median change in behavior of their control firms. We report these results in Panel A of Appendix Table 3, where the changes in behavior are observed from t-2 to t-1 (column 1), t-1 to t+1 (column 2), t-1 to t+2 (column 3) and t-1 to t+3 (column 4), where t is the year of the ownership change. We find the results in Table 2 to be robust to this different specification: size (measured by employment, sales or capital employed), profitability, post buyout debt increase and capital expenditures significantly increases immediately after the LBO and this increase remains significant at least up to three years after the transaction.

A third concern related to our results in Table 2 is that our evidence are based on recent deals, while older LBOs, even in France, were essentially motivated by the need to cut costs and downsize. Under this interpretation, the results in Table 2 would simply reflect the fact that we use a more recent sample than previous studies. This is not entirely true, as some papers (Amess and Wright, 2007, Davis et al, 2008) also use data from recent transactions, yet still find strong evidence of downsizing. As additional evidence, we run our regressions separately for years before and after 2000, and report the results in panel B, Table 4. Although post-buyout employment and sales growths are slightly lower post-2000, all the effects described in Table 2 remains strongly significant in both periods, and not statistically different across sub-periods.

[Insert Table 5 about here]

Fourth, we need to take into account the fact that our financial statements are not consolidated. LBO targets may initially have subsidiaries that are part of the entity bought out.[22] One possible outcome of the LBO could be a formal simplification where the buyout target is merged with its subsidiaries. This would mechanically increase the employment and perhaps even the assets of the target. To check this, Table 5 presents two separate robustness checks. First, we restrict the sample to firms that have no subsidiary according to INSEE data (both control firms and targets). This reduces the sample by approximately one third. Estimates are reported in columns 1 (log employment) and column 3 (log of fixed tangible assets, a subset of total fixed assets reported in Table 2). Post-LBO employment growth remains similar to the estimates provided in Table 3 (14 vs. 12%). Second, we use all targets, but add the employment and tangible fixed assets of the firms in our sample to that of their potential subsidiaries.[23] This is done in column 2 (for employment) and column 4 (for tangible assets). Again, these estimates are very close to the original ones in Table 2.

A fifth concern is attrition. While some buyouts create jobs, others may simply lead to firm destruction. When a firm closes a plant, it still appears in our sample the year after, so that the negative contribution of plant closure to firm job growth would still indirectly appear in our data. But when the firm itself disappears from our sample, in principle job growth should be -100%, while our data would report the firm missing. In the data, attrition is, however, not likely to be a concern. Looking directly at the attrition rate from our tax files, we find that 15.24% of our targets exit the tax files in the 3 years following the deal compared to 15.4% for control firms. Since firm exit from tax files may be a bad proxy of actual job destruction, we also used actual bankruptcy files from the statistical office, which report the identifying numbers and date of filing of all bankrupt firms in France. We find no difference in bankruptcy rates between targets and control firms after the deal. 6.67% of targets and 6.7% of control firms will be bankrupt at some point. 4.58% of control firms and 4.17% of targets will be bankrupt within the three years following the buyout. Overall, it does not seem that attrition from the sample, either because of bankruptcy or takeover, is significantly different for targets and their control firms.

A sixth concern comes from the fact that the rigidity of labor laws in France may be driving our results. For instance, the OECD ranks French law as the sixth most protective amongst the 28 member countries (OECD, 2004). Because it is difficult to lay off workers in France, it could be that we do not observe in our data the "cost cutting" buyouts that are possibly more prevalent in the US and the UK, where labor markets are supposed to be more flexible. Hence, the observed post-buyout growth would be mechanically higher in the French sample. To test for this selection effect, we ask if post-LBO growth is on average higher in industries where employment rigidity is higher. We run the following modified version of equation (1):

$$\begin{aligned} Y_{jt} &= \alpha_j + \delta_t + POST_{jt} + POST_{jt} \times LBO_j \\ &\quad + POST_{jt} \times RIGID_j + RIGID_j \times POST_{jt} \times LBO_j + \varepsilon_{jt} \end{aligned} \tag{3}$$

where Y_{jt} stands for employment, assets (in logs) or ROA. $RIGID_j$ is one of the two measures of employment "rigidity" and is defined at the level of the industry of firm j: fraction of unionized workers and fraction of workers under fixed term contracts (see Section 2.d).

[Insert Table 6 about here]

[22] We defer the reader to Section 2.a. on data construction for more details on conglomerates in our sample.

[23] We focus on tangible fixed assets since these are the assets than can be consolidated between a target and its subsidiaries. For a parent firm, another part of fixed assets is financial assets, which includes equity holding in, and loans to, subsidiaries. Consolidating such assets would amount to double counting the subsidiaries' assets.

Estimates of the above equation are reported in Table 6. Panel A uses the fraction of unionized workers as proxy for employment rigidity and panel B uses the fraction of workers under fixed term contracts. It is apparent from column 3 to 8 of Table 6 that post-buyout growth is not stronger in industries where employment is more rigid: none of the interaction coefficients is significant at the 5% confidence level and the point estimates of the interaction are small. This is not consistent with the hypothesis that post-LBO growth in France is larger because employees are more protected. Additionally, column 1 and 2 of Table 6 indicates that post-LBO increase in performance is not higher in industries with less rigid employment. We view this as additional proof that value creation does not seem to come primarily from cost-cutting strategies.

4. Financial Constraints and Post LBO Growth

Our results so far appear to be very dissimilar from pre-existing studies, in particular those focusing on large public-to-private transactions (Kaplan, 1989, Guo et al., 2009) but also smaller deals (Amess and Wright, 2007, and Davis et al, 2008). Both types of studies find evidence consistent with private equity funds implementing measures that aim at downsizing targets operations, while maintaining its ability to create value (i.e. holding EBITDA constant). Such a discrepancy between these results and ours begs for a more thorough investigation, which we attempt to provide here.

In this Section, we provide evidence consistent with the following hypothesis: French LBO targets tend to be credit constrained firms with growth opportunities, and private equity funds help these firms get access to additional sources of outside finance. Some of these funds, affiliated with local banks, may be able to help firms grow through (almost direct) lending.[24] But we believe the mechanism is much more pervasive that this, because PE sponsors help make their portfolio firms more credible borrowers on credit markets. First, as better monitors but still residual claimants, they make debt relatively safe and attractive to bankers. Second, private equity sponsors may introduce new, often financially savvy, members to the executive suite, which probably reassures creditors. Third, because of their long lock-up periods, private equity funds may be more patient than families, who need dividends to consume, and as a result more ready to reinvest free cash flows into the company. Last, an oft-cited argument in the profession is also that capital gains being less taxed than dividends, private equity funds are encouraged by their investors to reinvest cash flows instead of paying out dividends.

a. Private-to-private Transactions versus Divisional Buyouts

One first implication of our hypothesis is that firms that are initially financially unconstrained should not grow after the LBO. Subsidiaries of larger industrial groups are a good example of firms less likely to be credit constrained, since they initially benefit from internal capital markets (Hoshi, Kashyap and Scharfstein, 1991). Publicly listed firms are another example of firms that are less likely to be credit constrained.

[Insert Table 7 about here]

[24] Consistent with this idea, Demiroglu and James (forthcoming) find that reputable private equity groups obtain narrower bank spreads to finance their acquisitions, suggesting that some of these groups may be able to decrease the financing costs of their portfolio firms.

Following this intuition, we break down the sample into four different groups based on pre-buyout ownership: (1) private-to-private LBOs, where the seller typically is an individual or the founding family[25] (2) divisional LBOs, where the target is initially an affiliate of a conglomerate (3) secondary LBOs where the seller is another private equity group and (4) public-to-private LBOs, where the target is publicly traded before the deal. We report regression results in four separate panels in Table 7. We first remark from column 1 and 2 that the increase in profitability following an LBO is pervasive across these four groups. It is slightly larger for private-to-private transactions and public-to-private deals, but the difference between these deals and secondary and divisional deals is not statistically different from 0. This is not entirely surprising since irrespective of the type of deal, an LBO is supposed to generate value for the private equity fund. However, this leaves open the question of whether this increase in profitability observed across LBO types is necessarily achieved through a "growth" strategy.

In columns 3, 4 and 5, we look at firm size (employment, sales and capital employed), and find striking differences between the various types of LBOs. Most notably, we find that post-buyout growth in divisional buyouts is small (-2.6% for capital employed, 3.5% for sales and 6% only for employment), and statistically insignificant. Public-to-private deals (in Panel D) lead to a decline in firm size (-16% for sales growth and -5.8% for capital employed). Consistently with Kaplan (1989), downsizing for this sample is strong but statistically non significant, probably due to small sample size. Following a private-to-private LBO, however, employment (resp. sales and capital employed) increases by about 18% (resp. 18% and 13%). Such growth is not only statistically significant, but is also statistically different (smaller) from growth observed in public to private transactions. Analyzing a British dataset of private-to-private buyouts, Chung (2009) finds similar levels post LBO growth: for instance sales of LBO targets grow by 30% more than the rest of the industry.

As further evidence that LBOs are indeed alleviating credit constraints for targets in private-to-private transactions, we find that these deals are followed by a significant 36% increase in capital expenditures (column 7), compared to an insignificant increase of 11% for divisional buyouts and a -31% decrease for public-to-private deals (the difference is statistically significant). The evidence regarding post buyout debt increase reported in column 4 is also consistent with our credit constraint hypothesis. On the one hand, target leverage in private-to-private transactions increase by a significant 4 percentage points; on the other hand, it remains constant for targets of divisional buyouts. This difference is again significant at the 3% confidence level. Private equity groups, when they acquire firms that are likely to be credit constrained (i.e. family firms as opposed to divisions of conglomerates) thus seem to help these targets increase their debt capacity, allowing them to increase their capital expenditures and eventually to grow faster.

Finally, Panel C of Table 7 shows that secondary LBOs look very much like private-to-private LBOs on many dimensions. For these deals, ROA increases by a significant 3.4 percentage points (column 1), and employment (resp. sales and capital expenditures) increases by a significant 11% (resp. 17% and 31%). The post-buyout growth in capital employed is a bit

[25] The label "private to private transaction" comes from Capital IQ or SDC. We have crosschecked it for a subsample of our transactions. Using company websites, the business press and the DIANE dataset, we have manually checked the seller's identity for all 47 private-to-private deals that took place in 2004. We could find that information for 40 sellers only: only 3 of them were not individuals or families. One of them was a coop, another one had a financial institution as majority shareholder, and a manufacturing firm controlled the last one. We thank Laurent Bach for letting us access his dataset on private firms ownership.

smaller (6.6% significant at the 10% confidence level) but the difference with private-to-private capital growth is not significant at the 10% confidence level. An important distinction, however, between these secondary LBOs and private-to-private LBOs is that secondary buyouts are not followed by a post buyout increase in debt issuance: target leverage does not rise. One possible interpretation of these results is that the "infusion" of capital made at the time of the first LBO, as well as retained earnings, may be sufficient to allow the secondary fund to keep on implementing a "growth" strategy, but without a need to further access to the credit market.

Another source of cross-sectional heterogeneity we have explored in order to test for the credit constraint hypothesis is target size. Indeed, firm size is a widely used indicator of credit constraints in corporate finance (since at least Fazzari et al. (1988)). In unreported regressions, we split our sample by pre-buyout target size. Large LBO targets are those whose number of employees is larger than the median one year prior to the transaction. Small targets are the rest. Using this breakdown, we find that post-LBO growth in employment and capital employed is significantly larger (by 16% and 22% respectively) in small firms relative to large firms.[26] This last result is also interesting in that it can reconcile our results with the previous literature on LBO. This literature uses mostly large deals and finds no (or a negative) effect of LBOs on growth: in our sample too, large LBOs generate less growth.

All in all, these and the previous results[27] suggest that, for firms that are more likely to be initially credit constrained (privately-held family firms, small firms, stand-alone firms), leveraged buyouts lead to an increase in debt capacity, corporate investment and eventually firm growth in assets, jobs, and sales.

b. Financial Constraints and Post LBO growth

In this Section, we propose an additional test for the hypothesis that private equity sponsors help previously credit-constrained firms access additional sources of outside finance, leading to an increase in investment and firm growth. As Rajan and Zingales (1997) have shown, some industries rely more on outside finance to fund investment while firms that belong to other, less "financially dependent", industries suffer less from credit constraints (they tend to finance investment internally). As a consequence, post-buyout growth, post buyout debt increase (as measured by target's leverage, i.e. excluding LBO related debt) and capital expenditures should be concentrated among targets that operate in such industries.

More specifically, we run the following set of regressions:

$$\begin{aligned} Y_{jt} &= \alpha_j + \delta_t + POST_{jt} + POST_{jt} \times LBO_j \\ &\quad + POST_{jt} \times FD_j + FD_j \times POST_{jt} \times LBO_j + \varepsilon_{jt} \end{aligned} \tag{4}$$

for firm i in year t. Y_{jt} stands for ROA, log EBITDA, log employment, log sales, log capital employed, target leverage and log capital expenditures. FD_j is the industry level measure of a firm's dependence on external finance, whose construction is described in Section 2.d.

[26] Note that this is not a return-to-the-mean effect, as this higher post-LBO growth is relative to control firms, which have similar sizes than LBO targets by construction.
[27] Note that in Panel B, C, D and E of Appendix Table 3, we test the robustness of these results to the alternative specification presented in section 4.d., i.e. where we adjust each target's behavior for the median of the group of its control firms. The results are broadly similar with the results in Table 7.

[Insert Table 8 about here]

Regression results are reported in Table 8. Panel A focuses on private-to-private LBOs. Columns 1 and 2 show that financial dependence has little explanatory power on the post-buyout increase in profitability of private-to-private transactions. If anything, ROA seems to experience less of an increase following the buyout in more financially dependent industries. One possible interpretation is that targets in non-financially constrained industries are also able to increase their profits, but through cost-cutting strategies rather than growth strategy.

More interestingly, column 3 to 7 of Panel A in Table 8 show that, for private-to-private LBOs (i.e. these deals where targets are the most likely to suffer from credit constraints), post-buyout growth and increase in target leverage and capital expenditures are concentrated among targets that operate in industries that are more financially dependent: all the estimates of the interaction term *FD x POST x LBO* are statistically significant. These effects are also economically important. Going from the 25th percentile to the 75th percentile of financial dependence increases the level of financial dependence by about 0.6. Hence, post-LBO employment (resp. sales, capital employed and capital expenditure) growth difference between two such industries would be around 10% (resp. 7%, 12% and 21%). This has to be compared with an average excess post-LBO growth, for private-to-private deals, of about 18% for employment, 18% for sales, 12% for capital employed and 36% for capital expenditures (see Table 7). Hence, greater growth in financially dependent industries explains a large fraction of the average post LBO growth in the sample. Similarly, and consistently with the idea that initial credit constraints are at the origin of post-LBO growth in private-to-private deals, we find in column 6 of Panel A, Table 8, that the post-buyout increase in debt following private-to-private LBOs is stronger for targets that operate in industries that are more financially dependent. Going from the 25th to the 75th percentile of financial dependence leads to an increase in the post-buyout increase in target's leverage of 1.7 percentage points, i.e. around 40% of the overall post-buyout effect for private-to-private LBOs (column 6 of Panel A in Table 7).

Interestingly, panel B through D of Table 8 show that none of the interaction terms (*FD x POST x LBO*) is positive and significant when estimated on other LBO types. While the financial dependence of a target’s industry has a strong predictive power on post-buyout growth for private-to-private transactions, it does not explain targets behavior following divisional, secondary or public-to-private LBOs. Also, these results do not depend on the specific definition of financial dependence we use. Indeed, we present in Appendix Table 4 the estimation of equation (4) when we use two alternative measures of industry financial dependence (see section 2.d. for a definition of these measures). As is apparent from this table, the results are left unchanged with these alternative measures. Overall, all the results presented in Table 8 and Appendix Table 4 are very consistent with the idea that (1) divisions of conglomerates, public firms and firms owned by a private equity group are less likely to suffer from credit constraints and (2) the relaxation of credit constraints is at the heart of the post-buyout growth and increase in capital expenditures and post buyout debt issue observed in our sample of LBOs.

5. Conclusion

Like most commentators in the public debate, many financial economists have come to see LBOs as a way to implement drastic, “cost cutting” measures that the target was initially reluctant to put in place. This view is largely influenced by studies on large, US-based, public-to-private deals in the 1980s, or more recent studies that focus exclusively on US and UK data. This paper provides some evidence that LBOs may alleviate credit constraints, and be an actual engine of growth for small- and medium-sized enterprises. In France, LBO targets experience a very strong growth in sales, assets and employment, in particular when they were previously more likely to be credit constrained. Hence, instead of reinforcing credit constraints, modern LBOs can make them less tight. This effect is large in France, but existing studies have not yet demonstrated that this growth motivation is absent from smaller US or UK transactions.

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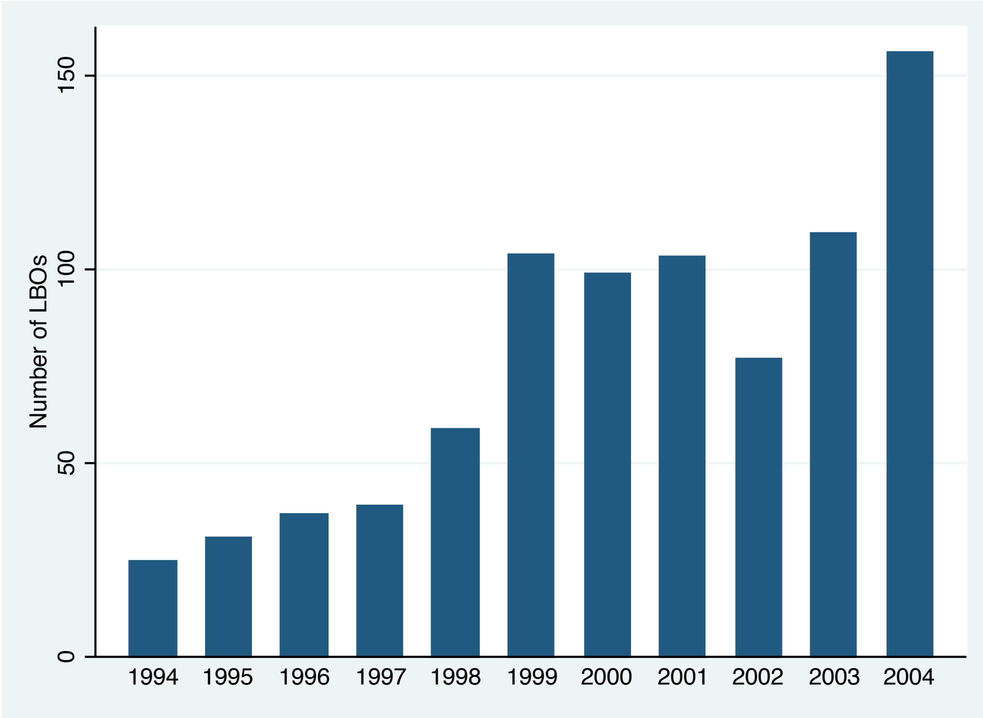
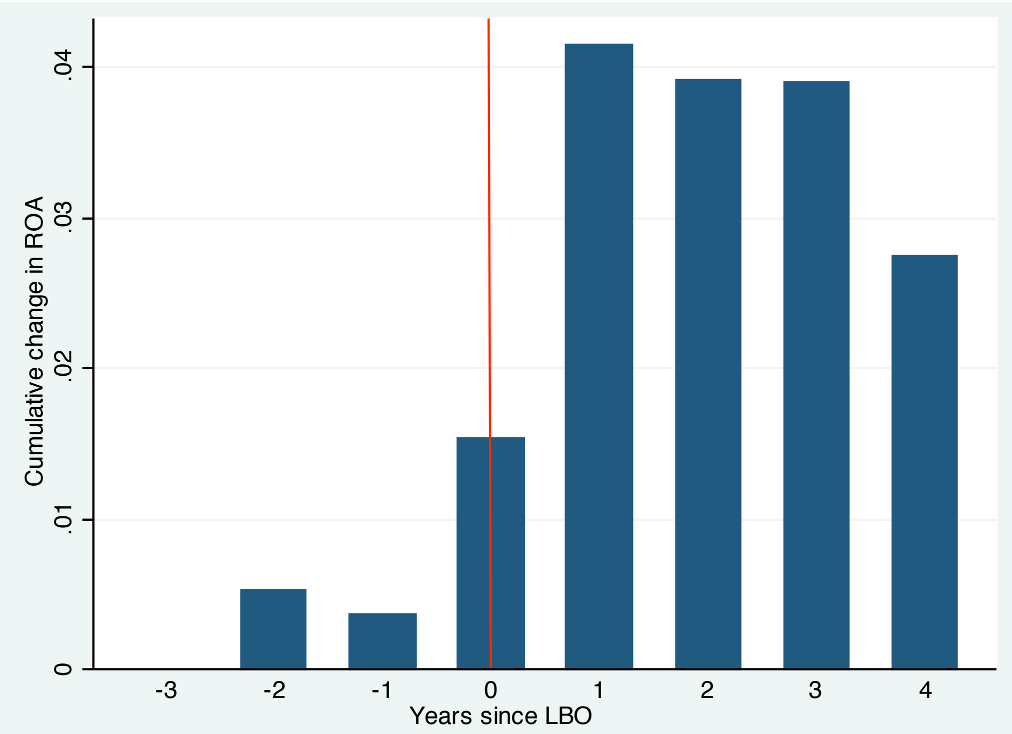
Figure 1: Number of LBOs per year in the Sample



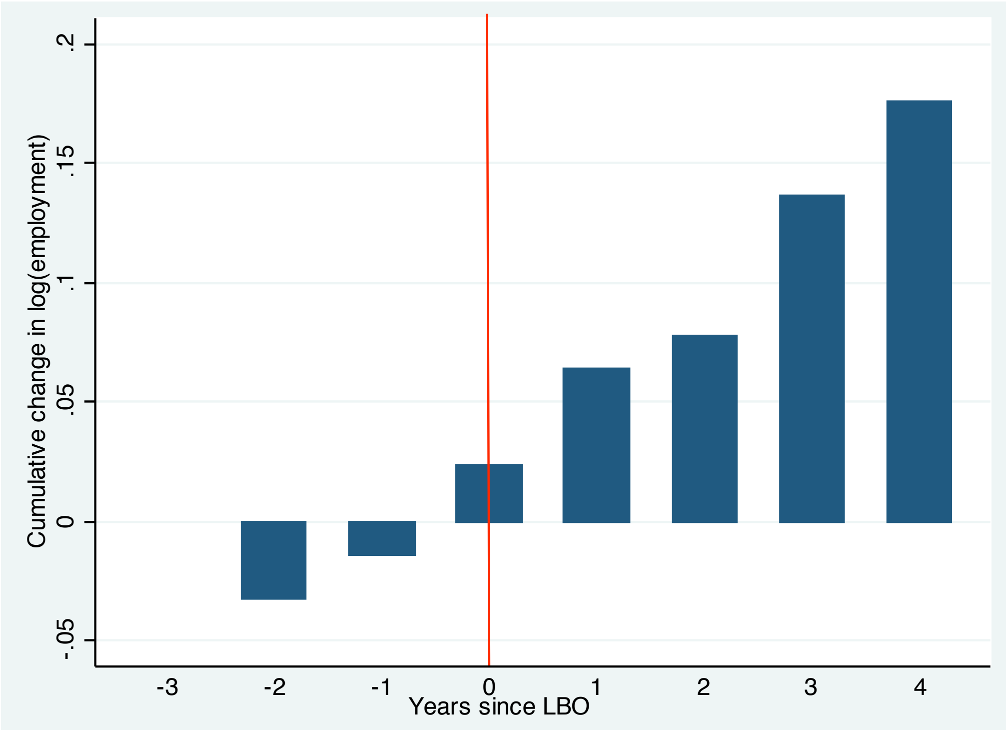
Source: SDC and Capital IQ; firms for which accounting data are available in the tax files. See text for details.

Figure 2: Mean-adjusted increase in profitability around the LBO



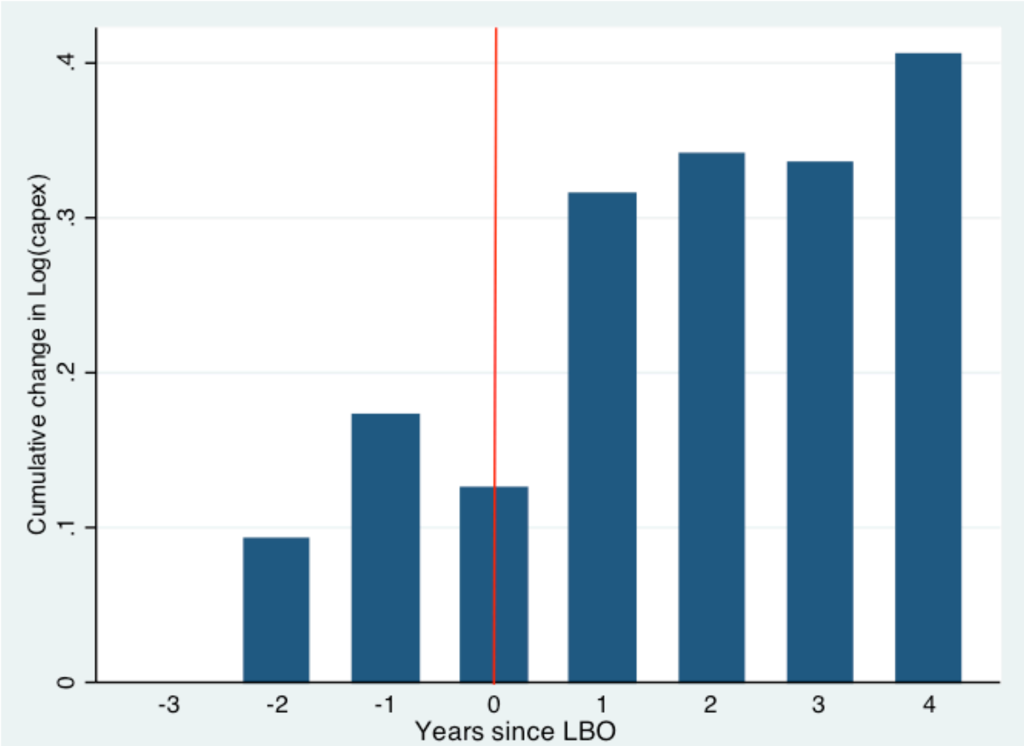
Note: Let t be then number of years since LBO: for instance, t=-2 means "two years before the deal", t=+2 means "two years after the deal". For each t and each LBO target, we first compute change in ROA between t and three year before the deal (t=-3). For each LBO target, we then take all (maximum 5) control firms, and compute the mean change in ROA between -3 and t. We then compute the difference between the ROA change of target and mean ROA change of controls: this is the adjusted change of ROA at the target level. Finally, we compute the average adjusted change in ROA across all targets in our sample. Reading: between three years prior to the deal and one year after, on average, target ROA increases by about 4 percentage points more than for control firms.

Figure 3: Mean-adjusted employment increase around the LBO



Note: Let t be then number of years since LBO: for instance, t=-2 means "two years before the deal", t=+2 means "two years after the deal". For each t and each LBO target, we first compute cumulative employment growth between t and three year before the deal (t=-3). For each LBO target, we then take all (maximum 5) control firms, and compute the cumulative employment growth between -3 and t. We then compute the difference between the target's employment growth and mean employment growth of controls: this is the adjusted job growth at the target level. Finally, we compute the average adjusted job growth across all targets in our sample. Reading: between three years prior to the deal and three year after, on average, target employment increases by about 13 percentage points more than for control firms.

Figure 4: Mean-adjusted increase in capital expenditures around the LBO



Note: Let t be then number of years since LBO: for instance, t=-2 means "two years before the deal", t=+2 means "two years after the deal". For each t and each LBO target, we first compute change in log investment (CAPX) between t and three year before the deal (t=-3). For each LBO target, we then take all (maximum 5) control firms, and compute the change in log investment between -3 and t. We then compute the difference between the target's investment growth and mean investment growth of controls: this is the adjusted investment growth at the target level. Finally, we compute the average adjusted investment growth across all targets in our sample. Reading: between three years prior to the deal and three year after, on average, target investment increases by about 33 percentage points more than for control firms.

Table 1: Characteristics of LBO targets and their control firms.

Variable	Median	Mean	S.D	Q1	Q3	Number of deals
Panel A: Targets						
Sales (m€)	13.09	32.64	46.52	4.79	40.93	839
Employment	64	173	242	27	229	839
Capital Employed (m€)	7.77	21.27	31.12	2.47	22.26	839
Capital Expenditures (m€)	0.39	1.27	1.91	0.1	1.38	839
Sales growth	0.08	0.11	0.21	-0.01	0.19	832
Employment growth	0.03	0.05	0.15	-0.02	0.11	837
CE Growth	0.08	0.11	0.26	-0.01	0.2	836
ROA	0.18	0.2	0.27	0.05	0.33	839
Leverage	0.2	0.24	0.21	0.08	0.37	836
Panel B: Control firms						
Sales (m€)	7.75	23.64	35.59	2.66	29.82	839
Employment	60	153.1	207.45	26	203.5	839
Capital Employed (m€)	3.25	10.55	16.02	0.93	12.34	839
Capital Expenditures (m€)	0.16	0.62	0.96	0.04	0.7	839
Sales growth	0.06	0.06	0.08	0.02	0	832
Employment growth	0.01	0.03	0.05	0	0.05	837
CE Growth	0.05	0.06	0.09	0	0	836
ROA	0.18	0.19	0.25	0.06	0.31	839
Leverage	0.18	0.21	0.15	0.08	0	836

Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. For each firm in the sample, each variable is averaged over the four years preceding the transaction. This table shows the distribution of this pre-transaction outcome for actual LBO targets (Panel A) and for the median of each group of control firms (Panel B). Capital Employed is the sum of fixed assets and operating working capital. ROA is EBITDA normalised by shareholder's equity plus debt minus trade payables. Leverage is financial debt divided by capital employed. Other variables are self-explanatory.

ble 3: Post-buyout behavior

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
post x LBO	.044***	.18***	.12***	.12***	.07***	.026***	.24***
	(.007)	(.029)	(.021)	(.024)	(.02)	(.0063)	(.045)
post	-.025***	-.12***	-.066***	-.083***	-.038***	-0,0024	-.11***
	(.0048)	(.018)	(.009)	(.013)	(.012)	(.0038)	(.032)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	33 003	27 478	32 887	33 061	32 735	32 366	32 744
Number of deals	839	793	839	936	839	838	839
Adj. R^2	0,53	0,87	0,93	0,92	0,93	0,61	0,72

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels*

	Intermediate inputs / sales (1)	WC / (FA + WC) (2)	Exports / sales (3)
st x LBO	-0,0013	0,0086	.013***
	(.0052)	(.0058)	(.0035)
st	.0081***	-0,00055	-0,0022
	(.0032)	(.0047)	(.002)
m FE	Yes	Yes	Yes
ar FE	Yes	Yes	Yes
servations	33 061	33 102	33 061
mber of deals	836	839	836
lj. R^2	0,77	0,62	0,86

*te: Sample of LBO targets and their control firms (see text for details). mple period: 1994-2004. OLS estimates. All regressions include firm and ar fixed effects. Error terms are clustered at the deal x post level. *, **, mean statistically significant at the 10, 5 and 1% levels*

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: including controls for pre-buyout growth							
post x LBO	.043***	.18***	.12***	.11***	.06***	.026***	.23***
	(.007)	(.028)	(.019)	(.023)	(.019)	(.0063)	(.044)
post	0,0056	.91***	.97***	1.4***	1.1***	.027**	.72***
x pre LBO sales growth	(.017)	(.071)	(.044)	(.059)	(.055)	(.014)	(.1)
post	-.025***	-.22***	-.17***	-.24***	-.15***	-0,0055	-.19***
	(.0051)	(.02)	(.011)	(.015)	(.014)	(.004)	(.034)
Observations	32 861	27 454	32 755	33 014	32 596	32 229	32 603
Number of deals	839	793	839	936	839	838	839
Adj. R^2	0,53	0,87	0,94	0,92	0,93	0,61	0,72
Panel B: Sub Period Robustness							
Year of deal <=2000							
post x LBO	.036***	.15***	.18***	.15***	.084***	.028***	.22***
	(.0095)	(.042)	(.03)	(.038)	(.03)	(.0093)	(.061)
post	-.03***	-.14***	-.075***	-.098***	-.051***	-0,0052	-0,072
	(.0068)	(.028)	(.014)	(.019)	(.017)	(.0056)	(.046)
Observations	16 473	14 137	16 405	16 532	16 355	16 124	16 340
Number of deals	394	377	394	393	394	393	394
Adj. R^2	0,48	0,86	0,93	0,91	0,94	0,59	0,73
Year of deal >2000							
post x LBO	.052***	.22***	.067**	.083***	.056**	.025***	.27***
	(.01)	(.04)	(.028)	(.03)	(.026)	(.0083)	(.066)
post	-.013*	-.078***	-.054***	-.068***	-0,021	-0,0017	-.16***
	(.0071)	(.027)	(.013)	(.017)	(.017)	(.0052)	(.05)
Observations	16 530	13 341	16 482	16 529	16 380	16 242	16 404
Number of deals	445	416	445	443	445	445	445
Adj. R^2	0,56	0,88	0,94	0,93	0,93	0,63	0,72
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. Panel A controls for pre LBO growth by adding to the right hand side of Equation (1) an interaction between firm level pre LBO sales growth and the POST dummy. Panel B estimates the basic specification of Equation (1) separately for pre and post 2000 transactions. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels*

Table 5: Post-buyout behavior -- alternative measures of employment and assets.

	Log(employment)		Log(tangible fixed assets)	
	Stand Alones only	Target + Subsidiaries	Stand Alones only	Target + Subsidiaries
	(1)	(2)	(3)	(4)
post x LBO	.14***	.11***	.13***	.093***
	(.027)	(.021)	(.038)	(.024)
post	-.061***	-.057***	-.028**	-.032**
	(.011)	(.011)	(.014)	(.014)
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	18458	32960	18141	32538
Number of deals	316	839	315	838
Adj. R^2	0,94	0,91	0,94	0,93

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels*

Table 6: Post-buyout behavior and employment rigidity.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: Rigidity is unionization rate							
post x LBO x Rigidity	-0,005	0,31	0,14	-0,3	-0,26	-.14*	-0,71
	(.086)	(.28)	(.14)	(.27)	(.23)	(.084)	(.54)
post x LBO	.056***	.11*	.076**	.16***	.1**	.048***	.32***
	(.017)	(.063)	(.034)	(.048)	(.045)	(.016)	(.1)
lbo x rigidity	0,054	-1,3	-1,6	-0,67	-1,3	-0,0074	-3,3
	(.29)	(.83)	(1.1)	(.77)	(.82)	(.26)	(2)
post x rigidity	-0,013	0,063	-0,012	0,0033	.2***	.095***	0,1
	(.031)	(.11)	(.053)	(.075)	(.079)	(.027)	(.18)
rigidity	-0,081	-0,19	-0,39	-0,096	0,073	.22***	-0,54
	(.12)	(.37)	(.37)	(.43)	(.39)	(.09)	(.72)
post	-.029***	-.092***	-.046***	-.048***	-.037*	-.019***	-0,08
	(.0091)	(.032)	(.013)	(.019)	(.021)	(.0071)	(.051)
Observations	17 218	14 859	17 274	17 344	17 028	16 838	17 095
Number of deals	438	423	438	437	438	438	438
Adj. R^2	.54	.87	0,95	0,94	0,94	0,62	0,75
Panel B: Rigidity is fraction of FTC in industry							
post x LBO x Rigidity	0,13	0,39	0,24	0,085	0,29	-0,012	-0,17
	(.099)	(.32)	(.19)	(.19)	(.25)	(.08)	(.39)
post x LBO	.063***	.19***	.12***	.11***	.075**	.021**	.18***
	(.012)	(.049)	(.029)	(.035)	(.034)	(.0099)	(.068)
lbo x rigidity	.83**	-1,3	-0,55	-0,91	-1,9	-0,15	-2,2
	(.36)	(1)	(1)	(.93)	(1.2)	(.34)	(2.3)
post x rigidity	-0,055	-0,22	-.14**	-0,12	0,14	.086**	-0,16
	(.049)	(.14)	(.071)	(.08)	(.11)	(.04)	(.21)
rigidity	-.51**	-0,15	0,42	.62*	.96**	.41*	1,5
	(.23)	(.65)	(.33)	(.34)	(.44)	(.24)	(1)
post	-.034***	-.096***	-.057***	-.055***	0,0079	0,0034	-.073*
	(.0076)	(.026)	(.012)	(.015)	(.017)	(.0056)	(.043)
Observations	17 218	14 859	17 274	17 344	17 028	16 838	17 095
Number of deals	438	423	438	437	438	438	438
Adj. R^2	.54	.87	0,95	0,94	0,94	0,62	0,75
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents estimates of Equation (1), where all terms are interacted with an industry level measure of labor market rigidity. In Panel A, rigidity is measured through the fraction of unionized workers in the industry. In Panel B, rigidity is minus the fraction of workers under fixed term contracts in the industry. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Table 7: Post-buyout behavior by LBO types.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: Private-to-private LBOs							
post x LBO	.051***	.21***	.18***	.18***	.13***	.04***	.36***
	(.01)	(.042)	(.03)	(.035)	(.028)	(.0077)	(.062)
post	-.035***	-.17***	-.058***	-.09***	-.031**	0,0005	-.11***
	(.0068)	(.025)	(.012)	(.017)	(.016)	(.0048)	(.043)
Observations	17 767	15 208	17 715	17 819	17 600	17 398	17 580
Number of deals	438	421	438	438	438	437	438
Adj. R^2	0,52	0,85	0,92	0,91	0,93	0,6	0,71
Panel B: Divisional LBOs							
post x LBO	.034***	.16***	.062*	0,035	-0,026	0,0043	0,11
	(.012)	(.056)	(.038)	(.047)	(.043)	(.014)	(.091)
post	0,0055	-0,015	-.077***	-.045*	-0,039	-0,0052	-.11*
	(.0093)	(.037)	(.019)	(.025)	(.027)	(.008)	(.063)
Observations	8 647	6 924	8 619	8 664	8 588	8 472	8 600
Number of deals	229	216	229	228	229	229	229
Adj. R^2	0,49	0,88	0,93	0,92	0,92	0,6	0,73
Test divisional = private-to-private	(.31)	(.46)	(.02)**	(.01)***	(.005)***	(.03)**	(.03)**
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table estimates Equation 1 for the different LBO types: Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. "Test divisional=private-to-private" (resp. "Test secondary =private-to-private" and "Test public = private-to-private") is the p-value of a test of equality of the post x LBO coefficient obtained using private-to-private deals only and the post x LBO coefficient obtained using divisional (resp. secondary and public) buyouts only. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Table 7 (continued): Post-buyout behavior by LBO types.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel C: Secondary LBOs							
post x LBO	.034**	.17***	.11**	.17***	.066*	0,016	.31***
	(.017)	(.056)	(.048)	(.048)	(.037)	(.015)	(.1)
post	-.036***	-.13***	-.068***	-.12***	-0,041	-0,0013	-0,061
	(.012)	(.048)	(.021)	(.033)	(.031)	(.01)	(.087)
Observations	4 943	4 111	4 917	4 935	4 912	4 871	4 929
Number of deals	129	120	129	127	129	129	129
Adj. R^2	0,57	0,87	0,95	0,92	0,93	0,64	0,72
Test secondary = private-to-private	(.38)	(.65)	(.25)	(0.79)	(0.17)	(.13)	(.70)
Panel D: Public-to-private LBOs							
post x LBO	.05*	0,069	0,0011	-0,16	-0,058	.053*	-0,31
	(.029)	(.16)	(.076)	(.12)	(.063)	(.03)	(.21)
post	-0,015	0,0086	-.099*	-0,1	-.09*	-0,014	-.29*
	(.017)	(.1)	(.053)	(.078)	(.05)	(.02)	(.18)
Observations	1 438	1 076	1 430	1 434	1 428	1 424	1 430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	0,58	0,91	0,94	0,93	0,97	0,65	0,75
Test public = private-to-private	(.96)	(.36)	(.03)**	(.01)***	(.01)***	(.69)	(.01)***
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table estimates Equation 1 for the different LBO types: Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. "Test divisional=private-to-private" (resp. "Test secondary =private-to-private" and "Test public = private-to-private") is the p-value of a test of equality of the post x LBO coefficient obtained using private-to-private deals only and the post x LBO coefficient obtained using divisional (resp. secondary and public) buyouts only. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Table 8: Post-buyout behavior and industry financial dependence.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel A: Private-to-private LBOs							
post x LBO	-.035*	0,062	.16***	.11**	.2***	.028**	.35***
x Fin. Dep.	(.02)	(.078)	(.037)	(.049)	(.048)	(.014)	(.12)
post x LBO	.064***	.17***	.094***	.11***	0,038	.025***	.18**
	(.013)	(.047)	(.023)	(.031)	(.03)	(.0091)	(.078)
post x Fin. Dep.	.06***	.09***	0,025	0,015	-0,013	-0,0093	0,066
	(.0082)	(.033)	(.016)	(.021)	(.02)	(.006)	(.051)
LBO x Fin. Dep.	0,021	0,099	-.49***	-.44***	-0,081	-.044*	-.47**
	(.035)	(.15)	(.069)	(.092)	(.084)	(.025)	(.24)
Fin. Dep.	-.12***	-.9***	-.5***	-.78***	-.12***	-0,012	-1.6***
	(.018)	(.08)	(.037)	(.048)	(.044)	(.013)	(.12)
post	-.062***	-.2***	-.068***	-.095***	-0,025	0,0053	-.14***
	(.0076)	(.028)	(.014)	(.019)	(.018)	(.0055)	(.047)
Observations	17 501	14 957	17 438	17 542	17 338	17 139	17 373
Number of deals	438	421	438	438	438	437	438
Adj. R^2	0,53	0,85	0,93	0,92	0,93	0,61	0,71
Panel B: Divisional LBOs							
post x LBO	0,018	0,16	-0,019	0,014	-.19**	-.05**	0,28
x Fin. Dep.	(.026)	(.12)	(.059)	(.075)	(.083)	(.023)	(.19)
post x LBO	.029*	0,083	.06*	0,017	0,039	.028**	-0,024
	(.016)	(.07)	(.036)	(.045)	(.051)	(.014)	(.11)
post x Fin. Dep.	0,017	0,028	-.065**	-0,041	.087**	0,0095	0,049
	(.012)	(.055)	(.027)	(.033)	(.037)	(.01)	(.084)
LBO x Fin. Dep.	.086**	0,19	-0,15	.39***	.39***	0,05	-0,34
	(.043)	(.26)	(.11)	(.13)	(.14)	(.039)	(.32)
Fin. Dep.	-0,025	-.48***	-.48***	-.37***	-.43***	-0,0066	-.94***
	(.021)	(.11)	(.058)	(.065)	(.068)	(.019)	(.16)
post	-0,0027	-0,027	-.043**	-0,024	-.071**	-0,0095	-.13*
	(.01)	(.042)	(.022)	(.028)	(.032)	(.0087)	(.072)
Observations	8 540	6 833	8 511	8 554	8 481	8 366	8 533
Number of deals	229	216	229	228	229	229	229
Adj. R^2	0,5	0,88	0,93	0,92	0,92	0,6	0,73
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents, for each type of LBO, estimates of Equation (1), where all terms are interacted with an industry level measure of financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Table 8 (continued): Post-buyout behavior and industry financial dependence.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Panel C: Secondary LBOs							
post x LBO	-.066**	-.35**	-0,077	-0,1	-0,11	-.048*	0,17
x Fin. Dep.	(.034)	(.16)	(.058)	(.09)	(.087)	(.027)	(.24)
post x LBO	.067***	.29***	.1***	.18***	.11**	.031*	0,24
	(.021)	(.079)	(.035)	(.056)	(.054)	(.017)	(.15)
post x Fin. Dep.	.071***	.12**	.094***	.12***	.094***	-.02*	.81***
	(.013)	(.059)	(.023)	(.036)	(.034)	(.011)	(.094)
LBO x Fin. Dep.	.2***	0,45	-2.1***	-1.4***	-0,2	-0,072	-1.2**
	(.072)	(.46)	(.13)	(.19)	(.19)	(.058)	(.5)
Fin. Dep.	-.089***	-.34***	-0,038	-.31***	-.41***	.057***	-1.5***
	(.027)	(.13)	(.053)	(.079)	(.069)	(.022)	(.19)
post	-.069***	-.17***	-.11***	-.18***	-.088***	0,0096	-.46***
	(.013)	(.051)	(.023)	(.036)	(.034)	(.011)	(.094)
Observations	4 918	4 087	4 892	4 910	4 887	4 847	4 913
Number of deals	129	120	129	127	129	129	129
Adj. R^2	0,57	0,87	0,96	0,93	0,93	0,64	0,73
Panel D: Public-to-private LBOs							
post x LBO	-0,014	0,2	0,11	-0,025	0,0083	0,029	0,47
x Fin. Dep.	(.034)	(.27)	(.12)	(.14)	(.096)	(.036)	(.38)
post x LBO	.064**	-0,0043	-0,066	-0,14	-0,067	0,036	-.6*
	(.029)	(.19)	(.099)	(.13)	(.084)	(.031)	(.33)
post x Fin. Dep.	0,02	0,089	-.098**	-0,046	.086**	0,0015	.36**
	(.014)	(.11)	(.048)	(.061)	(.04)	(.015)	(.16)
LBO x Fin. Dep.	-.18*	-0,26	-0,14	-0,059	0,38	-0,066	-0,56
	(.096)	(.64)	(.33)	(.41)	(.28)	(.1)	(1.1)
Fin. Dep.	-.054*	-1.6***	-.77***	-1***	-0,044	-0,049	-0,51
	(.029)	(.23)	(.098)	(.12)	(.085)	(.031)	(.32)
post	-.03*	-0,078	-0,057	-0,11	-.15***	-0,017	-.56***
	(.018)	(.12)	(.061)	(.078)	(.052)	(.019)	(.21)
Observations	1 438	1 076	1 430	1 434	1 428	1 424	1 430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	0,58	0,91	0,94	0,94	0,97	0,65	0,76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents, for each type of LBO, estimates of Equation (1), where all terms are interacted with an industry level measure of financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Appendix Table 1: Post-buyout performance -- ROA and ROS.

	All transactions		*Private-to-private*		*Divisional*		*Secondary*		*Public-to-private*	
	ROA	EBITDA /sales	ROA	EBITDA /sales	ROA	EBITDA /sales	ROA	EBITDA /sales	ROA	EBITDA /sales
post x lbo	.044***	.014***	.051***	.011**	.034***	.018**	.034**	0,012	.05*	0,028
	(.007)	(.0046)	(.01)	(.0051)	(.012)	(.0084)	(.017)	(.014)	(.029)	(.044)
post	-.025***	-.0083***	-.035***	-.0089**	0,0055	-0,0016	-.036***	-.014*	-0,015	-0,014
	(.0048)	(.0029)	(.0068)	(.0038)	(.0093)	(.0051)	(.012)	(.0079)	(.017)	(.025)
Observations	33 003	32 440	17 767	17 502	8 647	8 517	4 943	4 818	1 438	1 396
Number of deals	839	831	438	435	229	228	129	126	36	35
Adj. R^2	0,53	0,59	0,52	0,52	0,49	0,53	0,57	0,69	0,58	0,7
test equality with private-to-private LBOs					0,31	0,47	0,38	0,92	0,96	0,69

*Note: Sample of targets of LBOs and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents estimates of Equation (1) by types of LBOs, where the dependent variable is ROA (column 1, 3, 5, 7 and 9) and Return on Sales (column 2, 4, 6, 8 and 10) . Column 1 and 2 use the whole sample of LBOs; column 3 and 4 use private-to-private deals only; column 5 and 6 use divisional buyouts only; column 7 and 8 use secondary LBOs only; column 9 and 10 use public-to-private transactions only. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Appendix Table 2: Post-buyout increase in number of plants, by LBO types.

	(# of plants > # of plants(t-1))				
	Whole sample (1)	Private-to-private LBOs (2)	Divisional LBOs (3)	Secondary LBOs (4)	Public-to-private LBOs (5)
post x lbo	0,012	0,011	0,028	0,0053	-0,038
	(.0092)	(.012)	(.019)	(.023)	(.038)
post	-0,0035	0,0053	-.034**	0,025	-.077*
	(.0074)	(.0092)	(.016)	(.021)	(.044)
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	24 084	13 508	6 169	3 273	1 020
Number of deals	762	409	205	107	36
Adj. R^2	0,22	0,22	0,22	0,24	0,22

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents, for each type of LBO, estimates of Equation (1), where the dependent variable is a dummy variable equal to 1 if the firm has increased the number of its plants from year t to year t+1. Column 1 uses the whole sample; column uses private-to-private transactions only; column 3 uses divisional buyouts only; column 4 uses secondary buyouts only; column 5 uses public-to-private deals only. Error terms are clustered at the deal x post level. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Appendix Table 3: Changes in behavior from pre-buyout to post-buyout period

	Median of the differential change between targets and controls			
	From -2 to -1	From -1 to +1	From -1 to +2	From -1 to +3
	(1)	(2)	(3)	(4)
Panel A: All LBOs				
ROA	0,005	0.014***	0.022***	0.021***
	[810]	[757]	[719]	[556]
Employment	0	0.007***	0.057***	0.075***
	[810]	[773]	[732]	[567]
Sales	0.013***	0.021***	0.017***	0.047***
	[803]	[767]	[726]	[562]
FA+WC	0.005**	0.039***	0.031***	0.095***
	[813]	[769]	[728]	[564]
Leverage	0,001	0.01*	0.012**	0.017***
	[793]	[743]	[701]	[535]
CAPEX	0.023***	0.087***	0***	0.091***
	[797]	[752]	[711]	[555]

Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. In this table we compute, for each target, the change in performance between year T+t and year T+t', where T is the year of the deal. For ROA and leverage, we compute level changes from year t to t'; for all other variables, we compute percentage changes from year t to year t'. We adjust each target's change in performance by subtracting the median change in performance of its control firms over the same period. The table reports the median of this adjusted-change in performance for all transactions in the sample. In column (1), t=-2 and t'=-1; in column (2), t=-1 and t'=1; in column (3), t=-1 and t'=2; in column (4), t=-1 and t'=3. Significance levels of medians are based on a two-tailed Wilcoxon rank test.

Appendix Table 3 (continued): Changes in behavior from pre-buyout to post-buyout period by LBO types

	Median of the differential change between targets and controls			
	From -2 to -1	From -1 to +1	From -1 to +2	From -1 to +3
	(1)	(2)	(3)	(4)
Panel B: Private-to-private LBOs				
ROA	0,002	0.016***	0.023*	0.02*
	[423]	[394]	[373]	[297]
Employment	0	0.026***	0.086***	0.097***
	[421]	[403]	[379]	[301]
Sales	0.023***	0.025***	0.025**	0.086***
	[417]	[399]	[375]	[298]
FA+WC	0.006*	0.063***	0.08***	0.146***
	[424]	[400]	[376]	[299]
Leverage	0	0.015***	0.016***	0.042***
	[416]	[387]	[366]	[289]
CAPEX	0.027**	0.157***	0.043***	0.115***
	[411]	[386]	[362]	[291]
Panel C: Divisional LBOs				
ROA	0,011	0,012	0.028**	0,006
	[220]	[208]	[198]	[163]
Employment	-0.023*	-0,009	0,025	0,034
	[220]	[214]	[204]	[168]
Sales	-0,031	-0,003	0,013	-0,011
	[218]	[212]	[202]	[166]
FA+WC	-0,002	0.025*	0,005	0,06
	[222]	[213]	[203]	[167]
Leverage	0,007	-0,007	0,004	0,001
	[215]	[205]	[190]	[156]
CAPEX	0.022**	-0,037	-0,049	0.058**
	[221]	[212]	[202]	[167]

Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. In this table we compute, for each target, the change in performance between year T+t and year T+t', where T is the year of the deal. For ROA and leverage, we compute level changes from year t to t'; for all other variables, we compute percentage changes from year t to year t'. We adjust each target's change in performance by subtracting the median change in performance of its control firms over the same period. The table reports the median of this adjusted-change in performance, by LBO type. In column (1), t=-2 and t'=-1; in column (2), t=-1 and t'=1; in column (3), t=-1 and t'=2; in column (4), t=-1 and t'=3. Significance levels of medians are based on a two-tailed Wilcoxon rank test.

Appendix Table 3 (continued): Changes in behavior from pre-buyout to post-buyout period by LBO types

	Median of the differential change between targets and controls			
	From -2 to -1	From -1 to +1	From -1 to +2	From -1 to +3
	(1)	(2)	(3)	(4)
Panel D: Secondary LBOs				
ROA	0,004	0.033***	0,003	0,029
	[124]	[116]	[111]	[74]
Employment	0.01**	0.07***	0.055***	0.086**
	[126]	[117]	[112]	[76]
Sales	0.04***	0.081***	0.03*	0.053**
	[125]	[117]	[112]	[76]
FA+WC	0,009	0.025**	-0,008	0,003
	[124]	[117]	[112]	[76]
Leverage	-0,001	0,020	-0,003	0,011
	[121]	[113]	[110]	[70]
CAPEX	-0,041	0.254***	0	0.236*
	[123]	[116]	[111]	[76]
Panel E: Public-to-private LBOs				
ROA	0,003	-0,005	0,012	0.08**
	[36]	[34]	[32]	[21]
Employment	-0,016	-0,02	-0,014	0,11
	[36]	[34]	[32]	[21]
Sales	-0,028	-0,03	-0,037	0,167
	[36]	[34]	[32]	[21]
FA+WC	0,036	-0,043	-0,107	-0,133
	[36]	[34]	[32]	[21]
Leverage	-0,010	0,007	0.024*	-0,003
	[36]	[34]	[31]	[19]
CAPEX	0,171	-0,441	-0,383	-0,453
	[35]	[33]	[31]	[20]

Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. In this table we compute, for each target, the change in performance between year T+t and year T+t', where T is the year of the deal. For ROA and leverage, we compute level changes from year t to t'; for all other variables, we compute percentage changes from year t to year t'. We adjust each target's change in performance by subtracting the median change in performance of its control firms over the same period. The table reports the median of this adjusted-change in performance, by LBO type. In column (1), t=-2 and t'=-1; in column (2), t=-1 and t'=1; in column (3), t=-1 and t'=2; in column (4), t=-1 and t'=3. Significance levels of medians are based on a two-tailed Wilcoxon rank test.

Appendix Table 4: Post-buyout behavior and financial dependence -- alternative measures of financial dependence.

	ROA	log(EBITDA)	log(Empl)	log(Sales)	log(FA+WC)	Leverage	log(CAPEX)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Financial dependence is computed over firms with employees > 50							
Panel A: Private-to-private LBOs							
post x LBO	-.041**	0,0087	.13***	.085*	.21***	0,021	.37***
x Fin. Dep.	(.02)	(.079)	(.038)	(.05)	(.048)	(.015)	(.13)
post x LBO	.067***	.2***	.11***	.13***	0,038	.028***	.18**
	(.013)	(.047)	(.024)	(.031)	(.03)	(.0092)	(.079)
post x Fin. Dep.	.055***	.076**	0,025	0,00025	-0,02	-0,0086	0,054
	(.0083)	(.034)	(.016)	(.021)	(.02)	(.0061)	(.052)
LBO x Fin. Dep.	0,026	0,17	-.4***	-.32***	-0,0098	-0,028	-0,31
	(.034)	(.14)	(.067)	(.089)	(.081)	(.024)	(.23)
Fin. Dep.	-.1***	-.77***	-.44***	-.68***	-.091**	-0,0098	-1.5***
	(.018)	(.077)	(.036)	(.046)	(.042)	(.013)	(.11)
post	-.059***	-.2***	-.069***	-.09***	-0,022	0,0049	-.14***
	(.0076)	(.028)	(.014)	(.019)	(.018)	(.0055)	(.047)
Observations	17 501	14 957	17 438	17 542	17 338	17 139	17 373
Number of deals	438	421	438	438	438	437	438
Adj. R^2	0,53	0,85	0,93	0,91	0,93	0,61	0,71
Panel B: Divisional LBOs							
post x LBO	0,0083	0,092	-0,0015	-0,015	-.19**	-.056**	0,11
x Fin. Dep.	(.027)	(.13)	(.061)	(.078)	(.086)	(.024)	(.19)
post x LBO	.033**	0,11	0,052	0,03	0,039	.031**	0,058
	(.016)	(.071)	(.037)	(.046)	(.052)	(.014)	(.12)
post x Fin. Dep.	0,0081	0,041	-.083***	-.068**	.088**	0,011	0,11
	(.012)	(.057)	(.027)	(.034)	(.038)	(.01)	(.086)
LBO x Fin. Dep.	.1**	0,26	-0,13	.45***	.34***	.07*	-0,36
	(.043)	(.25)	(.1)	(.13)	(.14)	(.038)	(.31)
Fin. Dep.	-0,028	-.47***	-.5***	-.41***	-.46***	-0,0043	-1.1***
	(.021)	(.11)	(.057)	(.065)	(.068)	(.019)	(.16)
post	0,0011	-0,031	-0,036	-0,012	-.071**	-0,01	-.15**
	(.01)	(.043)	(.023)	(.028)	(.032)	(.0088)	(.072)
Observations	8 540	6 833	8 511	8 554	8 481	8 366	8 533
Number of deals	229	216	229	228	229	229	229
Adj. R^2	0,5	0,88	0,93	0,92	0,92	0,6	0,73
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents, for each type of LBO, estimates of Equation (1), where all terms are interacted with an industry level measure of financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Appendix Table 4 (continued): Post-buyout behavior and financial dependence -- alternative measures of financial dependence.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Financial dependence is computed over firms with employees > 50							
Panel C: Secondary LBOs							
post x LBO	-.087***	-.43**	-.12**	-.16*	-.16*	-.056**	0,15
x Fin. Dep.	(.035)	(.17)	(.06)	(.093)	(.09)	(.028)	(.24)
post x LBO	.078***	.32***	.12***	.21***	.13**	.035**	.25*
	(.021)	(.084)	(.036)	(.058)	(.055)	(.017)	(.15)
post x Fin. Dep.	.071***	.12**	.086***	.12***	.094***	-.02*	.85***
	(.014)	(.061)	(.023)	(.037)	(.035)	(.011)	(.096)
LBO x Fin. Dep.	.23***	0,16	-2.1***	-1.3***	-0,21	-0,065	-1.1**
	(.071)	(.53)	(.13)	(.19)	(.18)	(.057)	(.49)
Fin. Dep.	-.085***	-.41***	-0,031	-.33***	-.38***	.056***	-1.5***
	(.025)	(.13)	(.048)	(.072)	(.064)	(.02)	(.18)
post	-.071***	-.18***	-.11***	-.19***	-.09***	0,0098	-.49***
	(.013)	(.052)	(.023)	(.036)	(.035)	(.011)	(.095)
Observations	4 918	4 087	4 892	4 910	4 887	4 847	4 913
Number of deals	129	120	129	127	129	129	129
Adj. R^2	0,57	0,87	0,96	0,92	0,93	0,64	0,73
Panel D: Public-to-private LBOs							
post x LBO	-0,016	0,077	0,096	-0,12	-0,03	0,0085	0,5
x Fin. Dep.	(.033)	(.27)	(.11)	(.14)	(.093)	(.035)	(.37)
post x LBO	.063**	0,04	-0,063	-0,075	-0,044	0,049	-.64*
	(.03)	(.19)	(.1)	(.13)	(.085)	(.032)	(.34)
post x Fin. Dep.	0,02	0,14	-.095**	-0,035	.092**	-0,00037	.38**
	(.014)	(.11)	(.047)	(.06)	(.039)	(.015)	(.16)
LBO x Fin. Dep.	-0,03	0,23	-0,064	0,0047	.54**	-0,094	-0,53
	(.085)	(.54)	(.29)	(.37)	(.24)	(.09)	(.96)
Fin. Dep.	-.047*	-1.4***	-.62***	-.9***	-0,074	-.046*	-.49*
	(.027)	(.21)	(.089)	(.11)	(.079)	(.028)	(.3)
post	-.032*	-0,11	-0,054	-0,11	-.16***	-0,016	-.59***
	(.018)	(.12)	(.063)	(.079)	(.052)	(.02)	(.21)
Observations	1 438	1 076	1 430	1 434	1 428	1 424	1 430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	0,58	0,91	0,94	0,94	0,97	0,65	0,76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents, for each type of LBO, estimates of Equation (1), where all terms are interacted with an industry level measure of financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Appendix Table 4 (continued): Post-buyout behavior and financial dependence -- alternative measures of financial dependence.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Financial dependence is computed over firms with employees > 200							
Panel A: Private-to-private LBOs							
post x LBO	-0,026	0,012	.13***	.12***	.22***	0,022	.41***
x Fin. Dep.	(.019)	(.076)	(.036)	(.048)	(.046)	(.014)	(.12)
post x LBO	.061***	.2***	.11***	.12***	0,03	.029***	.15**
	(.013)	(.046)	(.023)	(.031)	(.03)	(.0091)	(.077)
post x Fin. Dep.	.055***	.1***	.029**	0,0082	-0,02	-0,0093	0,072
	(.0079)	(.031)	(.015)	(.02)	(.019)	(.0058)	(.049)
LBO x Fin. Dep.	0,01	.46***	-.31***	-.34***	.16*	-.06**	-0,26
	(.035)	(.15)	(.071)	(.094)	(.084)	(.025)	(.23)
Fin. Dep.	-.1***	-.81***	-.4***	-.67***	-.09**	-0,017	-1.5***
	(.018)	(.078)	(.038)	(.048)	(.044)	(.013)	(.12)
post	-.06***	-.21***	-.07***	-.092***	-0,023	0,0054	-.14***
	(.0076)	(.028)	(.014)	(.019)	(.018)	(.0055)	(.047)
Observations	17 501	14 957	17 438	17 542	17 338	17 139	17 373
Number of deals	438	421	438	438	438	437	438
Adj. R^2	0,53	0,85	0,93	0,91	0,93	0,61	0,71
Panel B: Divisional LBOs							
post x LBO	0,02	0,056	-0,022	0,06	-.18**	-.047**	0,17
x Fin. Dep.	(.026)	(.12)	(.057)	(.072)	(.08)	(.022)	(.18)
post x LBO	.029*	.13*	0,053	-0,00065	0,028	.027**	0,016
	(.016)	(.067)	(.035)	(.043)	(.049)	(.014)	(.11)
post x Fin. Dep.	0,016	0,0075	-.058**	-0,045	.094***	0,012	0,13
	(.011)	(.052)	(.026)	(.032)	(.035)	(.0097)	(.08)
LBO x Fin. Dep.	0,035	0,17	-.2*	0,14	.25*	.079**	-0,45
	(.042)	(.24)	(.1)	(.13)	(.13)	(.038)	(.31)
Fin. Dep.	-.038*	-.4***	-.43***	-.35***	-.45***	-.045**	-1***
	(.021)	(.1)	(.057)	(.064)	(.066)	(.018)	(.15)
post	-0,0019	-0,017	-.045**	-0,022	-.07**	-0,01	-.15**
	(.0098)	(.041)	(.022)	(.027)	(.031)	(.0085)	(.07)
Observations	8 540	6 833	8 511	8 554	8 481	8 366	8 533
Number of deals	229	216	229	228	229	229	229
Adj. R^2	0,5	0,88	0,93	0,92	0,92	0,6	0,73
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents, for each type of LBO, estimates of Equation (1), where all terms are interacted with an industry level measure of financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Appendix Table 4 (continued): Post-buyout behavior and financial dependence -- alternative measures of financial dependence.

	ROA (1)	log(EBITDA) (2)	log(Empl) (3)	log(Sales) (4)	log(FA+WC) (5)	Leverage (6)	log(CAPEX) (7)
Financial dependence is computed over firms with employees > 200							
Panel C: Secondary LBOs							
post x LBO	-.068**	-.3**	-0,045	-0,066	-0,079	-0,034	0,12
x Fin. Dep.	(.033)	(.16)	(.056)	(.087)	(.084)	(.026)	(.23)
post x LBO	.067***	.28***	.086**	.16***	.096*	0,027	.24*
	(.021)	(.079)	(.035)	(.056)	(.054)	(.017)	(.15)
post x Fin. Dep.	.068***	.1*	.071***	.078**	.07**	-.018*	.76***
	(.013)	(.059)	(.022)	(.035)	(.034)	(.011)	(.093)
LBO x Fin. Dep.	.14*	0,37	-2***	-1.5***	-0,22	-0,046	-1.2**
	(.073)	(.51)	(.13)	(.19)	(.19)	(.058)	(.5)
Fin. Dep.	-.068***	-.23**	-.12**	-.32***	-.39***	.058***	-1.6***
	(.026)	(.12)	(.05)	(.074)	(.066)	(.021)	(.19)
post	-.068***	-.17***	-.1***	-.16***	-.076**	0,0088	-.44***
	(.013)	(.051)	(.023)	(.036)	(.034)	(.011)	(.094)
Observations	4 918	4 087	4 892	4 910	4 887	4 847	4 913
Number of deals	129	120	129	127	129	129	129
Adj. R^2	0,57	0,87	0,96	0,93	0,93	0,64	0,73
Panel D: Public-to-private LBOs							
post x LBO	0,00075	0,17	0,14	-0,046	-0,046	0,013	0,49
x Fin. Dep.	(.033)	(.26)	(.11)	(.14)	(.095)	(.036)	(.38)
post x LBO	.055*	0,0024	-0,096	-0,13	-0,027	0,045	-.62*
	(.03)	(.19)	(.1)	(.13)	(.085)	(.032)	(.34)
post x Fin. Dep.	0,017	0,14	-.096**	-0,029	.078**	0,0053	.37**
	(.014)	(.1)	(.047)	(.06)	(.04)	(.015)	(.16)
LBO x Fin. Dep.	-.17**	0,18	0,21	0,29	0,25	0,012	-0,43
	(.071)	(.51)	(.24)	(.31)	(.2)	(.077)	(.81)
Fin. Dep.	-.056*	-1.5***	-.76***	-1***	-0,055	-.056*	-.56*
	(.029)	(.23)	(.096)	(.12)	(.084)	(.031)	(.32)
post	-0,029	-0,11	-0,055	-0,12	-.15***	-0,02	-.58***
	(.018)	(.12)	(.062)	(.079)	(.052)	(.02)	(.21)
Observations	1 438	1 076	1 430	1 434	1 428	1 424	1 430
Number of deals	36	32	36	36	36	36	36
Adj. R^2	0,58	0,91	0,94	0,94	0,97	0,65	0,76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Note: Sample of LBO targets and their control firms (see text for details). Sample period: 1994-2004. OLS estimates. All regressions include firm and year fixed effects. This table presents, for each type of LBO, estimates of Equation (1), where all terms are interacted with an industry level measure of financial dependence. Panel A uses private-to-private transactions only; Panel B uses divisional buyouts only; Panel C uses secondary buyouts only; Panel D uses public-to-private deals only. *, **, *** mean statistically significant at the 10, 5 and 1% levels.*

Private Equity Buyouts and Workplace Safety*

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Abstract

This paper presents evidence of a large, persistent decline in establishment-level workplace injury rates after private equity (PE) buyouts of publicly traded U.S. firms. We find that firms experience fewer OSHA safety violations after buyouts and that a larger decline in injury rates is associated with an increased probability of exit via IPO. Employment reductions after buyouts are concentrated in relatively low-injury-risk establishments. Overall, our results suggest that buyouts improve workplace safety and that PE acquirers benefit from this improvement. We explore possible causes of these changes through interviews with executives of companies acquired in buyouts and through cross-sectional analysis. (*JEL* G32, G34, J28)

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While private equity (PE) buyouts generate excess returns for investors (Harris, Jenkinson, and Kaplan, 2014), many commentators have expressed concerns that workers of acquired companies face layoffs and reduced wages after these transactions. However, employment risk and compensation are not the only margins on which a buyout might affect worker well-being. One particularly important and often-overlooked factor affecting production-level workers specifically is workplace safety. Labor historians generally attribute the rise of labor unions in the early 1900s primarily to concerns about dangerous working conditions (Dubofsky and McCartin, 2017). Despite substantial improvements in working conditions over the past century, U.S. private sector workers still experienced more than 100 million workplace injuries requiring treatment beyond first aid over the period 1990–2015.[1] Mounting evidence that may have profound implications for workplace safety indicates that substantial changes in operational structure and policies often follow buyouts (e.g. Davis et al., 2014). Yet, evidence on the effect of buyouts on this important margin of worker well-being is lacking.

This paper studies the evolution of workplace safety records after PE buyouts, analyzing establishment-level data from the Bureau of Labor Statistics' (BLS) Survey of Occupational Illnesses and Injuries (SOII). We find a large, sustained decline in workplace injury rates after buyouts of publicly traded companies. Compared to similarly sized control establishments in the same industry, annual injuries per employee fall by an average of 0.74 to 1.00 percentage points from the 4 years before to 4 years after an establishment's parent company is acquired in a buyout, or 11.1% to 15.0% of the prebuyout mean. For context, a comparable decline in workplace injury rates across all establishments in the United States would result in between 650,000 and 880,000 fewer workplace injuries per year. The decline appears the second year post-buyout, persists through at least the fourth year post-buyout, and is evident across

[1]The International Labour Organization (ILO) reports 430 million occupational injuries and illnesses and 355,000 fatalities globally per year (among 3 billion workers), which collectively cost an estimated 4% of global gross domestic product (ILO, 2003). Estimates of the compensating wage differential required for a statistical workplace injury in the United States range from US$(2018)20,000 to US$(2018)70,000 (Viscusi and Aldy, 2003).

multiple industries. While we lack the data to explore any compensating wage differentials, these findings suggest a novel dimension on which buyouts may positively affect workers.

We also explore the consequences of reductions in workplace injury rates after buyouts for firms. Cohn and Wardlaw (2016) find a negative association between workplace injury rates and firm value for publicly traded firms, suggesting that improvements in workplace safety could be a source of value creation for investors. Analyzing auxiliary data on workplace health and safety inspections from the Occupational Safety and Hazard Administration (OSHA), we find that inspected establishments are less likely to be cited for violations after buyouts. As violations carry fines, this finding points to a concrete dimension on which improvements in workplace safety after buyouts may benefit firms and their PE owners. This finding also helps allay concerns that the decline in workplace injuries we observe after buyouts could reflect changes in *reporting* practices rather than actual improvements in workplace safety. We also find that a firm is more likely to exit buyout status through an initial public offering (IPO) when its injury rate falls more post-buyout. As an IPO is typically the most profitable form of exit (Guo, Hotchkiss, and Song, 2011), this finding provides another piece of evidence that PE owners benefit from improvements in workplace safety.

We also use the workplace injury data to shed new light on employment dynamics after buyouts. We find that employment decreases after buyouts, confirming the findings of prior research (Davis et al., 2014). We also find that more dangerous establishments experience smaller decreases in injury rates after buyouts.[2] While we do not observe which specific jobs are eliminated, this finding suggests that job reductions are more likely to occur in establishments with excess back office staff, which are exposed to low levels of injury risk.[3]

To better understand the nature of the operational changes driving our results, we con-

[2]We also find that lower injury rate establishments are less likely to be reobserved in the data post-buyout. Because only a fraction of establishments is surveyed in the SOII in any given year, we note that we cannot distinguish between an establishment that closes and one that is simply not resurveyed.

[3]Consistent with this finding, Antoni, Maug, and Obernberger (2015) find that job reductions after buyouts in Germany are primarily concentrated in back-office jobs.

ducted interviews with both executives of companies acquired in buyouts and PE executives responsible for overseeing portfolio companies. We describe these interviews in detail in Section 6. The executives we interviewed broadly indicated that they were aware of post-buyout declines in workplace injury rates. Moreover, they indicated that these declines were a result of operational changes within the acquired company and, in some cases, were an explicit objective. Specific operational changes that executives linked to a decline in workplace injury rates include refocusing on core operations and increased monitoring at all levels of the organization.[4]

We also examine cross-sectional variation in the change in workplace injury rates after buyouts. We find that workplace injury rates decline more in companies with more physical assets, where the overall exposure to workplace injury risk is likely to be higher. The decline in injury rates is smaller after more highly levered buyouts, though our estimates here are less precise because we only observe post-buyout leverage for a subsample of buyouts. This finding dovetails both directionally and quantitatively with the conclusions of Cohn and Wardlaw (2016) that workplace injury rates in public firms increase with leverage. The fact that *average* injury rates decline after buyouts, which typically involve significant increases in leverage, suggests that other changes after buyouts, such as those discussed above, outweigh the effects of leverage on the balance.

One possible explanation for the decline in workplace injury rates after buyouts is the systematic automation or offshoring of dangerous jobs. A decrease in injury rates due simply to the elimination of dangerous jobs would be difficult to square with the concentration of employment reductions in low- rather than high-injury-rate establishments. However, automation or offshoring could lead to the replacement of high-injury-risk jobs with higher-skilled, low-injury-risk jobs in an establishment, even if total employment does not fall. While

[4]Increased monitoring may be both a motivation for and a by-product of the increase in information technology investment after buyouts that Agrawal and Tambe (2016) document.

we do not observe automation or offshoring directly, Autor and Dorn (2013) conclude that the jobs most susceptible to automation and offshoring involve the performance of "routine tasks." We find no relationship between the change in injury rates and industry-level routine-task intensity measured before a buyout. Our discussions with executives suggest that the role of automation in particular may be subtle. Many of the executives refer to what we characterize as "soft" forms of automation that streamline employee workflow and reduce physical touches, which make jobs safer rather than eliminate jobs.

We also consider the role that reductions in agency conflicts due to strengthening of governance might play in driving the decline in workplace injury rates after buyouts. The decline in workplace injury rates is not related to observable governance changes, such as replacement of the board chair, addition of directors to the board, or addition of the PE firm's own executives to the board, and is actually smaller when the CEO is replaced. However, as with automation, our interviews with executives suggest subtler changes in governance that might affect workplace safety. A common theme from these interviews is an increase in the amount of information collected, including workplace safety information specifically in some instances, and monitored throughout the organization after buyouts. Operational improvements due to stronger monitoring are likely to contribute to improvements in workplace safety.

The decline in workplace injury rates is greater in firms with positive abnormal accruals, high levels of analyst coverage, and significant holdings by high-turnover institutional shareholders. These findings could indicate a connection between the decline in workplace injury rates and reductions in "short-termism" as a result of the buyout, since workplace safety is best characterized as a long-term asset from a firm's standpoint. There is an ongoing debate about whether market scrutiny of short-term performance and executive compensation contracts with short horizons cause a bias toward short-term cash flows when publicly traded firms make decisions. Relatedly, we find that workplace injury rates after buyouts of the publicly traded companies that constitute our primary sample decline *relative to* the change after a smaller sample of private company buyout establishments. While our interviews suggest a lengthening of decision horizons after buyouts in general, any conclusions here are speculative, since our proxies for short-termism could also proxy for other firm characteristics, and public and private buyout targets may differ along many dimensions. Nevertheless, our findings suggest that further investigation into changes in investment horizons after PE buyouts could be a fruitful direction for future research.

Our paper contributes to the literature on the impact of PE ownership on a firm's employees.[5] Existing work generally documents a reduction in employment and compensation after buyouts, consistent with the popular view that buyouts are harmful to workers.[6] Our analysis points to at least one dimension along which buyouts may actually benefit employees. Agrawal and Tambe (2016) find that exposure to information technology (IT) investment after buyouts increases the value of employee human capital. However, they find that these benefits hold only for white-collar workers and managers. In contrast, our evidence relates to the well-being of production-level workers, a larger and more vulnerable segment of the workforce.

Our paper also adds to recent work on changes in operating performance after PE buyouts. Davis et al. (2014) document significant improvements in total factor productivity after PE buyouts, most of it driven by reallocation of resources from low- to high-productivity establishments.[7] In contrast, we document substantial *within-establishment* improvements in a specific facet of operations. In the same general vein, Bernstein and Sheen (2016) find that restaurants' health ratings improve after their parent firms are acquired in PE buyouts.

[5] Papers studying the impact of PE buyouts on nonemployee stakeholders include those by Fracassi, Previtero, and Sheen (2017) (supermarket buyers) and Eaton, Howell, and Yannelis (2018) (for-profit college students).

[6] See, for example, Kaplan (1989), Muscarella and Vetsuypens (1990), Lichtenberg and Siegel (1990), Wright, Thompson, and Robbie (1992), Amess and Wright (2007), Boucly, Sraer, and Thesmar (2011), Davis et al. (2014), Antoni, Maug, and Obernberger (2015), and Davis et al. (2019).

[7] Brav, Jiang, and Kim (2015) find similar results following shareholder activism campaigns.

Our paper complements theirs by studying buyouts across a broad set of industries and focusing on a previously unexplored low-level dimension of operational improvements. Our interviews with executives of companies acquired in PE buyouts and PE executives responsible for overseeing portfolio companies also shed further light on the nature of operational changes after buyouts.[8]

1 Data and Sample Construction

In this section, we describe the data that we use in the paper as well as the process we use to match buyouts with establishment-level workplace injury data from the BLS' SOII. We also describe matched samples of establishments of acquired firms and control establishments that we use to conduct the difference-in-differences analysis.

1.1 Data sources

We obtain our sample of PE buyouts from Cohn, Mills, and Towery (2014). This paper builds a sample of whole-firm buyouts of publicly traded companies taking place between 1995 and 2007 using data from SDC Platinum and Dealogic, supplemented with news articles to remove improperly classified transactions. It consists of buyouts of nonbankrupt U.S. "C" corporations with at least \$10 million in assets.[9] In later supplemental analysis, we also use a sample of private firm buyouts covering the same time period obtained from Cohn, Hotchkiss, and Towery (2015).

[8] A large literature studies changes in accounting measures of operating performance after PE buyouts, including work by Kaplan (1989), Muscarella and Vetsuypens (1990), Smith (1990), Wright, Thompson, and Robbie (1992), Smart and Waldfogel (1994), Amess and Wright (2007), Guo, Hotchkiss, and Song (2011), Boucly, Sraer, and Thesmar (2011), Cohn, Mills, and Towery (2014), and Cohn, Hotchkiss, and Towery (2015). Kaplan (1989), Denis (1994), and Kaplan (1994) describe specific operational improvements in case studies of four separate buyouts. Bernstein, Lerner, Sorensen, and Strömberg (2016) find that industries in which PE firms invest tend to grow as a whole, suggesting effects spill over within an industry.

[9] The restriction to "C" corporations excludes "pass-through entities," such as partnerships, "S" corporations, and limited liability companies (LLCs).

The BLS conducts the SOII each year by collecting injury and illness data based on Occupational Safety and Health Administration (OSHA) recordkeeping requirements. This process involves gathering data for hundreds of thousands of establishments in a stratified sampling process. Employers covered under the Occupational Safety and Health Act and employers selected to be part of the BLS survey are required to maintain a log recording any injuries "that result in death, loss of consciousness, days away from work, restricted work activity or job transfer, or medical treatment *beyond* first aid." These employers must make their injury logs available to OSHA inspectors and supply the data contained in the logs to the BLS. The SOII is primarily used to produce aggregate statistics on the state of occupational risk in various industries in the United States. Annual establishment-level SOII data are available starting in 1996.

Each establishment in the SOII data has a unique identifier. Each establishment-year record contains the establishment's name, location, SIC code, number of injuries during the year ($Injuries$), number of injuries resulting in days away from work, restricted activity, or job transfer ($DARTInjuries$), average number of employees during the year ($Employees$), and total number of hours worked ($HoursWorked$). We use this data to construct annual measures of the injury rate at each establishment. Our primary injury rate measure is $Injuries/Employee$, which is $Injuries$ divided by $Employees$. We also construct the measure $DARTInjuries/Employee$, which is $DARTInjuries$ divided by $Employees$, and which captures the rate of relatively serious injuries. Finally, we compute $log(Employees)$, which is the natural log of an establishment's reported average employment over the year, and $HoursWorked/Employee$, which is $HoursWorked$ divided by $Employees$, further divided by 1,000 for convenience to reduce the number of significant digits we need to report. The only firm-level identifier in the SOII data is the parent firm's employer identification number (EIN).

The SOII microdata contain no additional information about the details of injury incidents. However, Table 1 shows the percentage of injuries in the United States in 2014 by different causes (panel A) and types (panel B) as reported in the BLS' annual news release on employer-related workplace injuries and illnesses (BLS, 2015).[10] The leading causes of workplace injuries are contact with objects, falls, and physical overexertion, whereas the most common injury types are sprains, strains, or tears; soreness and pain; bruises and contusions; cuts and lacerations; and fractures.

— Insert Table 1 here —

We supplement our SOII injury data analysis using establishment-level data on safety inspections and violations from OSHA. OSHA conducts approximately 100,000 safety inspections annually. Data on these inspections and any resultant violations going back to 1970 can be found at the website of the Department of Labor.[11] The data include information on whether advance notice was given before the inspection, whether the inspection resulted in the finding of a violation, and, if so, whether the violation was considered serious or not. In our analysis of the OSHA inspections data, we only examine only surprise inspections, that is, inspections in which OSHA gave no advance notice.

Finally, we obtain data on various characteristics of each target firm, buyer, and transaction, which we use in cross-sectional analysis. We obtain financial data from Compustat, analyst coverage data from I/B/E/S, institutional investor holdings data from the Thomson Reuters 13(f) filings database, PE firm information from Capital IQ, and executive and director information from Captial IQ's People Intelligence database. See Appendix Appendix A. for definitions of all of the variables. We also identify IPOs of PE-backed firms using data from SDC Platinum that is hand-checked for accuracy.

[10]This information is available only for injuries resulting in at least one lost work day.
[11]https://enforcedata.dol.gov/views/data_summary.php

1.2 Linking PE buyouts to workplace safety records

Because establishment-level BLS data are available starting in 1996, we consider only buyouts taking place in 1997 and later. Thus, our buyout sample period is 1997–2007. This period includes the buyout wave of the mid-2000s. Before merging the buyout data with the BLS data, we remove buyouts of firms in the finance industry (12 buyouts) or that engage in franchising (20 buyouts). We make the latter determination by visiting company websites and searching for other internet-based information about franchising opportunities. Removing franchisers is important because a franchiser may have limited control over the operational practices of its franchisees.[12] This process results in a starting sample of 285 public-firm buyouts and 547 private-firm buyouts.

We start by using EINs from Compustat to match establishments in the BLS data to buyout firms. However, Compustat provides only a single EIN, while firms often have multiple EINs, and different establishments belonging to the same firm often report different EINs. An added challenge is that EINs are available in the BLS data only for the period 2002–2012. To address this limitation, we assign a parent firm to an establishment-year in the 1996–2001 period if the establishment is matched to that parent firm based on EIN after 2001.

After identifying establishments of firms acquired in buyouts via EIN, we obtain additional matches by manually comparing each buyout firm's name to establishment names in the BLS data. In addition to looking for obvious matches, we use information from corporate websites, Bloomberg Business, and news articles to identify other names under which a firm operates. If we cannot determine with near-certainty that an establishment belongs to a given buyout firm, we do not create the match. For the supplemental sample of private firm

[12] Motivated by the same logic, Bernstein and Sheen (2016) treat franchised restaurants as a control group in their analysis of the effect of PE buyouts on restaurant health code violations. We cannot employ this approach because our data do not allow us to identify whether a given location is firm or franchisee owned. In addition, the number of franchisers in our sample is small.

buyouts, we can only match based on name.[13] We refer to establishments in the BLS data belonging to PE-acquired firms as "buyout establishments."

We match 13,452 unique establishments to 244 unique public buyout targets (approximately 55 establishments per buyout) and 2,051 unique establishments to 316 unique private buyout targets (approximately 6.5 establishments per buyout). It is not surprising that we match more establishments to public targets than to private targets, as public buyout targets tend to have far more establishments than private targets. Davis et al. (2019) report approximately 112 establishments per public buyout target and 16 establishments per private buyout target based on census data. It is also not surprising that we identify fewer establishments per firm than they do, as only a fraction of establishments is surveyed in the SOII in any given year. We match establishments in the OSHA inspection data to buyout firms based on establishment name.

1.3 Matched treatment and control sample formation

Our primary empirical strategy, which we describe in Section 2, is a generalized difference-in-differences approach. Specifically, we compare changes in injury rates at establishments of acquired firms from the 4 years before to 4 years after buyouts to changes over the same period for matched control establishments. Thus, we only consider buyout establishments in the SOII in at least one of the 4 years before and at least one of the 4 years after the buyout. We form matched buyout and control samples by matching each buyout establishment to up to five establishments that were never acquired in buyouts during the sample period. We consider only potential controls that are (a) in the same four-digit SIC industry as the buyout establishment, (b) present in the SOII in at least the same years in the 8-year window around the buyout as the buyout establishment, and (c) within 50% to 200% of the public

[13] The resultant link files for both public and private buyout establishments are stored at the BLS and can be made available to researchers on-site.

establishment's size based on number of employees. Within that set of candidate controls, we choose those closest in terms of $log(Employees)$. For each selected control establishment, we only retain establishment-years that coincide with the years the buyout establishment is in the SOII.[14]

In our main analysis, we restrict attention to buyout establishments (and their matched controls) with at least 100 employees in the most recent prebuyout year in the injury data. Meaningful injury rates are difficult to calculate for small establishments because the inability of an employee to suffer a fractional injury results in a preponderance of both zero and very high injury rates for these establishments, adding noise to ordinary least squares (OLS) regressions where injury rate is the dependent variable. We also do not require that control establishments matched to establishments of public (private) firms acquired in buyouts belong to public (private) firms themselves since doing so makes it more difficult to obtain matches and therefore considerably shrinks the overall sample size. We relax both of these assumptions in robustness checks, which we describe at the end of this section, and our estimates change little. Table 2 summarizes the sample formation.

— Table 2 here —

Panel A reports the number of firms, establishments, and establishment-years at each step in the formation process for both public and private buyout samples. The need for an establishment to appear in the data in both the pre- and post-buyout windows produces the most attrition in the final sample of buyout establishments. Because the BLS only surveys a fraction of establishments each year, many establishments go 4 years without being surveyed. While, as a result, the sample we can analyze represents only a fraction of

[14]As an example, if the establishment of a firm acquired in a 2003 buyout is in the SOII in 1999, 2002, 2005, and 2007, then we only consider potential control establishments also observed in the SOII in at least 1999, 2002, 2005, and 2007. A control establishment also might be in the SOII in additional years during the 8-year window around the buyout, say, 2001 and 2008, but we exclude these nonoverlapping years when forming our data set.

all acquired establishments, we see no reason why the largely random survey-based limitation would result in estimation bias.

Panel B reports the number of establishments in the final sample matched based on EIN and name. We match 64% of public target buyout establishments in the main sample on the basis of EIN and 36% based on name. Because we do not have an EIN for private buyout targets, 100% of private target buyout establishments in the main sample are matched on the basis of name. Panel C reports the number of buyout establishment-year observations in the final sample by year relative to the year of the buyout. Attrition in the post-buyout period appears fairly minimal. Finally, panel D reports the number of control establishments for each buyout establishment in the sample. It shows that most buyout establishments are matched to five control establishments (the maximum number possible).

Appendix Appendix B. presents alternative variants of our main analysis. These variations include matching establishments of publicly traded firms to establishments of other publicly traded firms only (B1), identifying public buyout establishments on the basis of name only, that is, without the use of EINs (B3), matching each buyout establishment to one control establishment, not five (B4), using propensity score matching to match based on multiple establishment characteristics instead of just industry and size (B2), and lowering the minimum establishment size for inclusion in the sample from 100 to 50 employees (B5). We also estimate count models where we impose no restriction on establishment size (B6).

Table 3 presents summary statistics for the characteristics of the firms and establishments in our final sample. Panel A reports means of several characteristics for buyouts establishments and their matched control establishments. The means of all of the observable characteristics we examine are similar for the buyout and control samples, despite the fact that we match only on industry and establishment size. While we cannot rule out the possibility that buyout and control establishments vary along unobserved dimensions, the homogeneity between treatment and control establishments provides some assurance that the as-if random assignment assumption for valid difference-in-differences estimation is likely satisfied. It is also worth noting the similarity in the size (number of employees) of public- and private-target buyout establishments that make it into the final sample; size similarity makes comparisons between the two groups at least somewhat meaningful.

— Table 3 here —

Panel B reports the breakdown of establishments into four broad industry categories. These categories are manufacturing (SIC codes in the 2000s and 3000s), transportation (SIC codes in the 4000s), trade (SIC codes in the 5000s), and services (SIC codes in the 7000s and 8000s).[15] Panel C reports financial characteristics of public-firm buyout targets in the main sample, calculated using Compustat data as of the last fiscal year-end prior to the buyout. For comparison, the means and medians of these characteristics for the Compustat universe during the sample period are shown to the right. Buyout firms in our sample tend to be significantly larger than Compustat firm in terms of medians but slightly smaller in terms of means. They also tend to have lower Tobin's q. Along other dimensions, buyout targets are similar to Compustat firms in general.

We use a similar approach to assign control establishments to each buyout establishment in the supplemental OSHA inspection data. Specifically, we match each inspected buyout establishment to inspected control establishments in the same four-digit SIC code experiencing the same inspection type and scope. We retain the five closest-sized establishments to the buyout establishment in terms of log employment immediately prior to the buyout. Within our sample, approximately 60% of inspections are safety related, with the remainder health related. Approximately 65% of inspections uncovered a violation and 41% a serious violation.

[15] Disclosure limitations prevent us from providing a superfine breakdown.

2 Empirical Methodology

We employ a standard generalized difference-in-differences approach to estimate changes in establishment-level injury rates after PE buyouts, relative to changes at control establishments. Denoting establishment by i, year by t, and four-digit SIC code industry by j, our primary regression specification is the following:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + \gamma BuyoutFirm_i * PostBuyout_{it} + \epsilon_{it}. \quad (1)$$

We use $Injuries/Employee$ and $DARTInjuries/Employee$ as measures of $InjuryRate$ in estimating Equation (1). The indicator $BuyoutFirm$ equals one for buyout establishments and zero for control establishments. The indicator $PostBuyout$ equals zero in the 4-year prebuyout period and one for observations in the 4-year post-buyout period. We exclude establishment-year observations from the buyout year itself because the parent firm is independent part of that year and under PE ownership part of the year. We include both establishment fixed effects (α_i) and four-digit SIC code-by-year fixed effects (ϕ_{jt}) to account for any unobserved time-invariant establishment factors and time-varying industry factors that might affect injury rates.[16] The coefficient γ captures the estimated change in injury rate from before to after a buyout for buyout establishments relative to control establishments.

While estimates of regression Equation (1) capture the average change in injury rates from the 4 years before to 4 years after a buyout, they do not indicate the timing of these changes. We explore how injury rates evolve over time after buyouts in more detail by

[16] Because the buyout year varies across establishments, we can distinguish industry-year fixed effects from the treatment effect. Note that the main effect of $BuyoutFirm$ is not included, because it does not vary within establishment and is therefore absorbed by the establishment fixed effects.

estimating the following regression:

$$
\begin{aligned}
InjuryRate_{it} = \alpha_i + \phi_{jt} + &\sum_{K\in(-4,1)\cup(1,4)} \beta_K YearRelBuyoutK_{it} \\
&+ \sum_{K\in(-4,1)\cup(1,4)} \gamma_K BuyoutFirm_i * YearRelBuyoutK_{it} + \epsilon_{it}. \qquad (2)
\end{aligned}
$$

Here, $K = -4, -3, -2, -1, 1, 2, 3, 4$ represents the number of years an observation occurs relative to the year of the buyout year. We include buyout-year observations (i.e., $K = 0$) in estimating this regression, unlike our estimation of Equation (1), where we do not. The buyout year is the omitted year in the regression. The γ_K coefficients capture the difference between injury rate in year K relative to the buyout year and injury rate in the buyout year.

Finally, we examine how changes in workplace injury rates vary with observable characteristics of the target firm, the buyer, and the transaction itself. While the results from this analysis may be open to multiple interpretations, it provides insight into the scenarios in which changes in workplace injury rates are more likely. This cross-sectional analysis involves estimating regressions of the following form:

$$
\begin{aligned}
InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + &\gamma BuyoutFirm_i * PostBuyout_{it} \\
&+ \eta PostBuyout_t * Characteristic_i \\
&+ \delta BuyoutFirm_i * PostBuyout_{it} * Characteristic_i + \epsilon_{it}, \qquad (3)
\end{aligned}
$$

where $Characteristic$ is a firm- or transaction-level characteristic.[17] The coefficient δ on the triple interaction term $BuyoutFirm * PostBuyout * Characteristic$ captures the cross-sectional variation of the change in injury rates with the given characteristic.

[17] The main effects of $BuyoutFirm$ and $BuyoutFirm * Characteristic$ are both fully absorbed by the establishment fixed effects α_i and are therefore omitted from the regression equation.

3 Evolution of Injury Rates around PE Buyouts

We begin our analysis by presenting a series of plots of workplace injury rates at buyout and control establishments in each year around the buyout year. We then turn to formal estimation based on the methodology described in Section 2.

3.1 Graphical analysis of injury rates around buyouts

Figure 1 plots the mean injury rates for our sample. Figures 1a and 1b plot the mean $Injuries/Employee$ and $DARTInjuries/Employee$, respectively. Figures 1c and 1d plot the industry-adjusted rates, where we first subtract the mean rate for all establishments in the same year and the four-digit SIC code industry. The points in these latter two plots are equivalent to the mean residuals from a regression of injury rates on industry-year fixed effects.

— Figure 1 here —

The plots reveal similar patterns. A comparison of prebuyout injury rate trends in buyout and control establishments reveals no obvious differences, suggesting that the parallel trends assumption required for valid difference-in-differences estimation is likely to be satisfied. The plots also show that injury rates for public-firm buyout establishments fall below those of control establishments in the second year post-buyout and remain below through the fourth year after the buyout. These patterns hold for both the overall injury rate and the rate of more serious DART injuries. The patterns are consistent with injury rates declining after public-firm buyouts with a short lag, as one would expect if operational changes implemented after buyouts take time to translate into observable outcomes.

3.2 Difference-in-differences estimates

Table 4 presents estimates based on regression Equation (1). We report standard errors, clustered at the firm level, below each point estimate, both in this table and in all of the remaining tables in the paper. The dependent variable in columns 1 through 3 in each panel is $Injuries/Employee$. Column 1 reports estimates excluding establishment fixed effects (industry-year fixed effects are included). This exclusion allows us to estimate the main effect of $BuyoutFirm$. Columns 2 and 3 report estimates of Equation (1) with establishment fixed effects, first excluding and then including establishment-level controls. Columns 4 through 6 present the same three regressions, where the dependent variable is $DARTInjuries/Employee$.

— Table 4 here —

The small and statistically insignificant coefficients for $BuyoutFirm$ in columns 1 and 4 suggest no differences in prebuyout injury rates in public-firm buyout and control establishments. The statistically insignificant coefficients for $PostBuyout$ in all columns suggest that control establishments do not experience unexplained changes in injury rates from before to after the buyout year. The negative coefficients for the interaction between $BuyoutFirm$ and $PostBuyout$ in columns 1 through 3 support a decline in injury rates at buyout establishments relative to control establishments after buyouts. The interaction coefficient is statistically significant at the 5% or better level in all three regressions.

The point estimates indicate an average fall in annual injuries per employee of 0.74 to 1.00 percentage points, or 11.1% to 15.0% of the prebuyout mean of 0.0669 for buyout establishments (see Table **??**). For the average size public buyout establishment (448 employees), this fall translates into 3.3 to 4.5 fewer workplace injuries per year, or 13.3 to 17.9 over the first 4 years post-buyout (the post-buyout estimation window). To contextualize these estimates, we note that a comparable decline in injury rates across all private-sector establishments in the United States would result in between approximately 650,000 and 880,000 fewer workplace injuries per year, based on the size of the private-sector workforce in 2002, the middle year of our buyout sample period.[18] Noting that the aggregate rate of workplace injuries per employee in the United States has steadily fallen by about 0.25 percentage points per year over the last 40 years, the reduction in injury rates after buyouts is comparable to advancing 3 to 4 years relative to the aggregate trend.

The coefficients for the interaction between $BuyoutFirm$ and $PostBuyout$ are also negative in columns 4 through 6 and are statistically significant at the 1%, 10%, and 5%levels, respectively. The point estimates indicate an average decline in injuries requiring days away from work or temporary restrictions or transfer per employee of 7.0% to 12.9% relative to the prebuyout mean of 0.0341 for buyout establishments. Thus, the decrease in workplace injury rates after buyouts holds even when we restrict attention to only the most serious injuries.

The positive coefficient for $HoursWorked/Employee$ in both panels is not surprising, since time spent working represents an employee's exposure to the arrival risk of injuries. Note that, because we normalize $HoursWorked/Employee$ by dividing it by 1,000, the coefficient of -0.0137 indicates an expected decrease of 0.0274 injuries per year per full-time (i.e., 2,000 hours per year) employee. Of course, less-mechanical reasons could explain this association as well. Employees who work too hard may experience a higher injury risk. Alternatively, employees who work too little may become rusty and thereby increase their injury risk. The negative coefficients for $log(Employees)$ could reflect the relative sophistication of larger establishments' operations, which are likely to be more heavily automated due to economies of scale in automation.

[18] We calculate the hypothetical response implied by a coefficient of 0.0074 as $(0.0074/0.053) \times 4.7M = 656,226$, where 4.7M is the number of nonfatal workplace injuries in 2002, and 0.053 is the number of injuries per equivalent full-time worker, both per a BLS (2002) news release on workplace injuries and illnesses.

3.3 Timing of changes in injury rates after buyouts

The results in Table 4 indicate a fall in injury rates after public-firm buyouts but do not give any indication of the exact timing of the fall. Operational changes generally take time to produce observable improvements in workplace safety outcomes (Clark and Margolis, 2000). It therefore would be difficult to attribute a decline in injury rates taking place immediately after a buyout to workplace safety improvements due to post-buyout operational changes. Table 5 presents estimates of the evolution of injury rates relative to controls each year around the buyout based on regression Equation (2), with six specifications mirroring those of Table 4.

— Table 5 here —

Here, the patterns are consistent with those shown in Figure 1. No clear patterns emerge for either treatment or control establishments prebuyout. The small, statistically insignificant coefficient for $BuyoutFirm * PostBuyoutYr1$ in the first column indicates that injury rates in public-firm buyout establishments remain effectively unchanged relative to those of nonbuyout establishments the first year after the buyout. The remaining interaction terms indicate that injury rates at acquired establishments fall substantially below those of control establishments the second year after the buyout and remain low through at least the fourth year after the buyout. Again, no discernible pattern materializes for the period after private-firm buyouts.

Workplace injuries decrease after public-firm buyouts on average. To assess the breadth of this phenomenon, we next break the full sample into four broad industry categories based on the SIC code of the buyout establishment. These categories are manufacturing (SIC 2000s and 3000s), transportation (4000s), trade (5000s), and services (6000s). We estimate regression Equation (1) for each of the four subsamples. Table 6 presents the results.

— Table 6 here —

The coefficients for $BuyoutFirm*PostBuyout$ are negative and large in magnitude across all four industry-category subsamples, ranging from 0.80 to 1.22 percentage points. These coefficients are statistically significant for the trade and services categories at the 5

4 Consequences for PE Investors

In this section, we consider the consequences of reductions in workplace injuries for investors. Reductions in injury rates may benefit firms in several ways: decreased downtime, fewer lawsuits, lower compensating wage differentials, and increased employee morale and productivity. Studying publicly traded firms, Cohn and Wardlaw (2016) find a negative relationship between firm value, as measured by Tobin's q, and workplace injury rates. Their estimates, applied to the firms in our sample, imply that the estimated fall in injury rates after public-firm buyouts would be associated with an average predicted increase in firm value of 1.2% to 1.6%.[19] However, workplace injury rates could proxy for operational performance more generally, and whether these estimates apply to PE-owned firms is unclear.

We conduct two sets of tests to shed further light on the implications of reductions in workplace injury rates for PE owners. First, we examine changes in the incidence of OSHA safety violations after buyouts. While most of the costs of an unsafe workplace to the employer are unobserved and in many cases intangible, OSHA violations result in fines and can create litigation risk and compliance problems that make conducting business more difficult. An added advantage of examining OSHA violations is that they represent the conclusions of an OSHA inspector and are not reported by the firm itself, sidestepping concerns that changes in reporting behavior might drive the decline in workplace injury

[19] Cohn and Wardlaw (2016) estimate that a one-unit increase in injuries per 1,000 hours worked is associated with a 3.19-unit decrease in Tobin's q in the following year. Noting that mean hours worked per employee in our sample and Tobin's q are 1,751 and 1.16, respectively, a 0.0074 decrease in injuries per employee would translate into a predicted $[3.19 \times (1,000/1,751) \times 0.0074]/1.16 = 1.2\%$ increase in firm value.

rates we document in Section 3. Second, we examine the relationship between the likelihood that a PE-acquired firm exists buyout status through an IPO, often considered a sign of a successful buyout, and the change in its workplace injury rate post-buyout.

4.1 Analysis of OSHA violations data

Here, the sample consists of establishment-years in which OSHA conducted an inspection of a given establishment. We estimate a linear probability model (LPM) variant of the generalized difference-in-differences Equation (1). The dependent variable is an indicator equal to one if the given inspection resulted in the finding of a violation and zero otherwise. We include industry-year fixed effects. Because of the infrequency of repeat observation of establishments in the data, we do not include establishment fixed effects. However, we do include inspection-type fixed effects.[20] We estimate four specific regressions based on combinations of using either one or five matched control establishments and using all violations or only serious violations to determine the dependent variable. Table 7 presents the regression estimates.

— Table 7 here —

The negative coefficients for the $BuyoutFirm \times PostBuyout$ interaction terms in all of the regressions indicate that the probability of an OSHA violation declines at buyout establishments post-buyout, relative to control establishments. The coefficients in the first and third columns imply an 8.8% or 11.6% decline in the probability of a violation relative to the prebuyout mean probability of 60.2%, while those in the second and fourth columns imply a 22.1% or 29.3% decline in serious violations relative to the prebuyout mean probability of 33.1%. It appears, then, that a reduction in OSHA violations represents one specific tangible

[20]OSHA identifies 12 inspection types: accident, complaint, referral, monitoring, variance, follow-up, unprogrammed related, planned, programmed related, unprogrammed other, programmed other, other, and fatality/catastrophe. We exclude planned inspections from our analysis.

dimension on which a firm itself may benefit from a buyout. This finding also provides some comfort that the reduction in reported workplace injury rates is not a change in the reporting rather than the actual incidence of workplace injuries.

4.2 Changes in workplace injury rate and probability of an IPO

For each buyout firm in the final matched sample, we compute the average number of injuries per employee across establishment-years in the 4 years before and 4 years after the buyout, as well as the comparable numbers for all control establishments matched to that firm's establishments. We then compute $InjuryRateChange$ as the change in average injury rate for the buyout firm minus the change for its control establishments. We also compute $IndAdjInjuryRateChange$ as an alternative measure, substituting injury rates relative to four-digit SIC code-year means for raw injury rates. We then estimate OLS regressions where the dependent variable is an indicator for whether the firm exited buyout status via IPO and the explanatory variable is one of the two measures of injury rate changes. Table 8 presents the results from these regressions.

— Table 8 here —

The coefficients for the change in injury rate variables are negative and statistically significant at the 10

5 Employment Dynamics after Buyouts

One of the most salient observable within-establishment changes after buyouts with clear evidence is a substantial reduction in employment (Davis et al., 2014; Antoni et al., 2015; Davis et al., 2019). In this section, we explore the connections between employment changes and injury risk around buyouts. We also consider their implications for interpretation of the results in Section 3.

We begin by estimating difference-in-differences models where $log(Employees)$ and $HoursWorked/Employee$ are dependent variables using the same matched sample we have used throughout. We first estimate the relative changes in the variables unconditionally after buyouts. We then estimate triple-difference regressions using two measures of establishment-level injury risk. The injury-risk measures are the establishment's injury rate the most recent year in the data prior to the buyout ($EstabInjuryRate$) and the four-digit SIC industry-average injury rate for that year ($IndustryInjuryRate$). Table 9 presents the results.

— Table 9 here —

The dependent variables are $log(Employees)$ in the first three columns of each panel and $HoursWorked/Employee$ in the final three columns. The estimates in column 1 imply a 13% average within-establishment reduction in employment relative to prebuyout levels, almost identical to the estimate of Davis et al. (2019). The estimates in columns 2 and 3 suggest that employment falls more in relatively low-injury-risk establishments. This result is consistent with PE owners primarily laying off administrative staff, which generally faces low workplace injury risk, rather than production workers, consistent with the conclusions of **?** for German buyouts.

Column 4 reveals that hours worked per employee increases slightly unconditionally post-buyout. Any decline in hours worked per employee could help explain the decline in injuries per employee after buyouts, since fewer hours worked implies less exposure to injury risk. The fact that hours per employee increases is evidence against this idea. Column 5 reveals that hours worked per employee increase less in establishments with previously high injury rates. However, column 6 reveals that it increases slightly more in establishments in industries with high injury rates historically. It is therefore unclear whether hours worked per employee decreases more or less in more dangerous establishments within a firm.

The greater fall in employment in relatively safe establishments that we document in columns 2 and 5 of Table 9 at least partly offsets the negative effects of declines in injury rates within establishment on overall injury rates. However, this offsetting effect proves immaterial. Figure 2 shows that the aggregate injury rates for the buyout and control groups (injuries summed across all establishments divided by employees summed across all establishments) in the sample, which account for changes in establishment size over time, exhibit patterns similar to those in Figure 1.

— Figure 2 here —

While changes in employment and hours worked per employee represent changes on the intensive margin of labor activity, firms also make changes on the extensive margin after PE buyouts, by closing and creating establishments (Davis et al., 2014). We also examine the probability with which an establishment sampled prebuyout is subsequently resampled post-buyout. We do so using the full set of establishments belonging to PE-acquired firms that are present in the BLS data in at least 1 of the 4 years immediately prior to the buyout. We regress an indicator variable equal to one if an establishment reappears in the BLS data in year $t + n$ post-buyout on its prebuyout injury risk measures, for each of $n = 1, 2, 3, 4$ separately. Table 10 presents the results.

— Table 10 here —

Reobservation rates are higher for establishments with higher prebuyout workplace injury rates, compared to controls. One important caveat is that establishments in our sample are not surveyed each year, so we cannot distinguish between a closed establishment from one that, by chance, is not reobserved in the data.

6 Discussion

In this section, we discuss how a firm's operational policies and practices affect workplace safety and how a PE buyout might change workplace safety by altering these practices. Subsection 6.1 discusses the impact of operational practices on workplace safety. This discussion is based largely on conversations with industrial safety practitioners and a case study on safety at Alcoa by Clark and Margolis (2000) and borrows from Cohn and Wardlaw (2016). Subsection 6.2 discusses the nature of operational changes after buyouts, with a focus on changes with important implications for workplace safety. This discussion is based in part on interviews we conducted with executives working for firms acquired in buyouts as well as PE executives responsible for overseeing the operations of portfolio companies.

6.1 Workplace safety and operating policies and practices

Factories, warehouses, stores, and other places of business make myriad operational decisions over time. Even in developed economies, such as the U.S. economy, many workers still toil in inherently physical jobs, such as, in construction, manufacturing, servicing, distribution (e.g., warehouses), and even many retail jobs. Risk of on-the-job injury is real for these workers, and the safety of the conditions in which they work is a first-order driver of their well-being.

Corporate objectives and policies influence the operational decisions of individual establishments within a corporation. For example, establishments may respond to corporate cost-cutting initiatives by cutting corners on maintenance, training, supervision, repair work, and other operational policies that promote workplace safety. As an extreme example, regulators investigating the 2005 explosion at BP's Texas City Refinery that killed 15 workers and injured 180 others found that management had removed replacement of a pressure valve from draft budgets in each of the 2 years before the explosion because of intense cost-cutting pressure.[21] They concluded that failure of this pressure value contributed to the accident. More generally, Cohn and Wardlaw (2016) present evidence that an establishment's injury rate rises when its parent company has fewer financial resources available to fund operational spending.

Other corporate objectives and policies may contribute positively to workplace safety. The adoption of modern production practices aimed at increasing productivity are likely to improve workplace safety as well. For example, lockout-tagout procedures that prevent the operation of machinery in need of repair not only reduce the risk of damage to machinery and extended downtime but also reduce the risk of employees being injured by malfunctioning equipment.[22] The intense monitoring of production processes necessary to implement "six sigma" production reduces the risk of undetected faults that could cause injuries. In general, minimizing movement of employees, inventory, and equipment with an establishment in order to reduce overhead costs also reduces employee exposure to injury risk.

Firms also make operating decisions with the explicit aim of reducing workplace injuries, such as mandating the use of safety equipment, holding regular safety meetings, and establishing written procedures for handling dangerous equipment or toxic materials. As a specific example, recoil from cables that break under tension is a common hazard in fields such as shipping and distribution. While more expensive, most firms in these industries now use synthetic fiber rather than traditional steel cable because synthetic fiber cables have fewer sharp edges when they fracture, reducing hazards due to snapping cables. Firms benefit from improved workplace safety in a variety of ways. Fewer workplace injuries mean less lost work time. Forty-five percent of workplace injuries in the United States result in at least 1 day away from work, restricted work activity, or job transfer (BLS, 2016), and operations

[21] See the U.S. Chemical Safety Board's report at https://www.csb.gov/file.aspx?DocumentId=5596.

[22] Lockout procedures involve isolating and disabling power sources in dangerous machinery in a systematic, step-by-step way. Tagout procedures ensure that only specific employees can unlock and untag a machine, ensuring that malfunctioning equipment is not accidentally brought back online before it is repaired.

may be idled while the cause of the injury is investigated and mitigated. Safer workplaces also mean fewer lawsuits and OSHA violations, lower compensation wage differentials and workmen's comp insurance premiums, and higher employee morale.

In a well-chronicled example, Alcoa Corporation reorganized its entire operational architecture around reducing workplace injuries and fatalities in the 1970s. This process involved compensating managers based on workplace safety records and implementing numerous low-level changes designed to reduce workplace accident risk. For example, Alcoa introduced a forklift speed limit of 4 miles per hour on its production floors after an employee was killed in a forklift collision (Clark and Margolis, 2000). The leading source of workplace injuries in the United States in 2014 was floors, walkways, and ground surfaces (BLS, 2015). While Alcoa explicitly targeted workplace safety improvements, management expected that steps to improve workplace safety would also increase productivity. According to Charles Duhigg, who has written extensively about workplace safety, the focus on worker safety at Alcoa "led to an examination of an inefficient manufacturing process - one that made for suboptimal aluminum and danger for workers" (Clark and Margolis, 2000).[23]

6.2 Buyouts, operations, and workplace safety

When raising funds, PE firms increasingly emphasize operational improvements made in portfolio companies and downplay other potential sources of value creation, such as financial engineering or multiple expansions. Recent research finds evidence that firms do, indeed, implement considerable operational changes after buyouts. Observed changes include closing efficient establishments and shifting production to more productive establishments.

Historically, many politicians and journalists have characterized PE firms as aggressive

[23]The coupling of workplace safety and production quality seems to have been understood throughout the company. Bert Harris, a smelting department superintendent in Alcoa at the time observed, "In many ways, what we are facing in safety is exactly like the problems we face in quality-there are a lot of ways to foul up; it's only through attention to details that we find the right way to get the job done, and it's only through discipline that we get the job done that way" (Clark and Margolis, 2000).

cost-cutters who seek to squeeze cash flow out of the companies they acquire without regard to the long-term consequences. Such arguments can be difficult to square with rational expectations, as investors to whom a PE firm eventually sells a portfolio company should account for worse expected long-run performance when they value the company. A more nuanced view is that firms may extract additional cash flow by expropriating workers. Shleifer and Summers (1988) argue that an acquirer may expropriate workers by abrogating implicit contracts between workers and prior owners. While concerns usually center on layoffs and reduced wages, firms may also cut corners on workplace safety in order to reduce costs, at least in the short run, for example, by deferring maintenance, shrinking training budgets, and eliminating supervisory positions.

Our finding that workplace injuries *decline* after buyouts is at odds with this view. PE firms themselves often argue that they focus on fundamentally improving the operations of the firms they acquire rather than cutting costs. This argument could be self-serving, since it is likely to appeal to investors and to serve PE firms' public relations objectives. However, evidence suggests that operations do fundamentally improve after buyouts. For example, Bernstein and Sheen (2016) find a reduction in health code violations after restaurant buyouts. More broadly, Davis et al. (2014) find a significant increase in total factor productivity after buyouts.

To better understand the nature of operational changes after buyouts, we interviewed both executives working for companies acquired in buyouts and PE firm executives responsible for overseeing portfolio companies. We started with the list of firms in our sample. From this list, we attempted to contact people who were involved with either the target company or the buyout firm. We were able to speak to several individuals on both sides of buyout transactions, some of whom were willing to speak on background and some of whom were willing to be quoted directly.

Executives at multiple companies describe a renewed emphasis on operational execution after buyouts, with an emphasis on "the boring stuff," accompanied by divestiture of potentially distracting noncore assets built up over time in a form of "mission creep." This narrative is consistent with the closing of inefficient establishments and redistribution of jobs after public-firm buyouts that Davis et al. (2014) document and improvements in health ratings after restaurant buyouts that Bernstein and Sheen (2016) document. As a specific example, Garden Ridge Pottery closed urban locations to focus on its traditional suburban markets after its 2000 buyout. An executive there also described adoption of an employee retention plan that reduced annual turnover among store employees substantially and replacement of peg board displays with shelves and racks, which require less time to stock.

Restructuring may also result in a significant shift in the mix of labor and capital a firm employs. Olsson and Tåg (2017) find evidence of systematic elimination of "routine-task" jobs, which are most exposed to automation and outsourcing risk (Autor and Dorn, 2013), after PE buyouts in Sweden. However, the specific types of routine-task jobs that are lost is unclear. Antoni et al. (2015) find that German buyouts are followed by a loss of predominantly administrative jobs. Our conclusion that employment reductions are greater in establishments with lower injury rates suggests that the focus of job reductions after U.S. buyouts may be similar. As an anecdotal example, we offer the experience of Garden Ridge Pottery, where an executive described a reduction in administrative staff from 350 to 60 after the buyout there.[24]

Executives at multiple companies also described increased organization-wide data collection and monitoring after buyouts to support efforts to improve operational execution, which required managers to "be on their game a lot more." Many executives described an expansion of the "scorecard" used to evaluate operations to incorporate more detailed operational

[24]A comparison of U.S. and non-U.S. buyouts is challenging, since buyouts outside the United States almost exclusively involve private companies. Independent private companies may be ripe for capital-intensive investments that improve efficiency since they have limited access to capital markets.

metrics relating to throughput, downtime, and production variances.[25] An increased emphasis on data collection and monitoring is broadly consistent with the increased investment in information technology after buyouts that Agrawal and Tambe (2016) document. At a higher level, the Garden Ridge Pottery executive described a general partner from Three Cities Research, the PE buyer, working various retail wage jobs throughout the company after the buyout to generate a more complete picture of operations.

Several executives we interviewed indicated that PE buyers were willing to accept lower profitability in the short run as a part of restructuring and implementation of increased monitoring. For example, a former executive with the large mid-market buyout specialist Welsh, Carson, Anderson, and Stowe suggested that "earnings were sometimes explicitly projected to go down for the first two years after a buyout, reflecting investments that would drive up earnings and growth in years four or five" and that "you don't have that luxury in a public environment." While the view that PE ownership allows for longer decision horizons than public market ownership is controversial, it does reconcile significant improvements in operational efficiency that Davis et al. (2014) document with a lack of improvements in profitability in at least the first couple of years after buyouts (Guo et al., 2011; Cohn et al., 2014).

Several of the executives we interviewed expressed awareness of improvements in workplace safety under PE ownership. Some executives identified improvements in workplace safety as a specific plank in a broader platform of operational improvements. A former PE executive we interviewed in the energy industry who is now with Total Safety, a safety consultancy, characterized the view on improved workplace safety as follows: "Fewer compliance problems, less scrutiny from regulators, sure, but the really good companies recognize that safe working environments increase morale, decrease turnover, and impact wage negotia-

[25] A couple of executives we interviewed specifically mentioned the formal introduction of data-intensive six sigma management.

tions."

An important factor to consider is the timing of any improvements in workplace safety due to operational changes after buyouts. Large operational changes generally take time to implement. Employees need time to adapt to new work routines and often initially resist procedural changes, including those that improve workplace safety, because of the extra effort required to abide by them (Clark and Margolis, 2000). Even after such changes are successfully implemented, a period of learning and refinement may be required before injury risk declines substantively. The timing of decline in workplace injury rates we observe after buyouts appears consistent with such a delay, since the decline appears primarily starting in the second year after a buyout.

7 Cross-Sectional Analysis

This section explores the sensitivity of the change in workplace injury rates after PE buyouts to various firm, acquirer, and transaction characteristics by estimating variations of regression equation (3). Our primary sample consists of establishments belonging to publicly traded companies acquired in PE buyouts. However, we also have a smaller matched sample of establishments, described in Section 1, belonging to private companies acquired in buyouts. Recent research finds significant differences in the nature of operational changes after buyouts of public and private companies (e.g., Davis et al., 2014).

To shed further light on possible differences between public and private firm buyouts, we compare the change in workplace injury rates after buyouts of publicly traded and private firms.[26] We do so by combining the public and private buyout matched samples and esti-

[26]Because the private firm buyout sample of establishments is small, we do not independently analyze it in detail. However, we do present difference-in-differences estimates mirroring those in Table 4 based on the private firm buyout sample in Table B7 in the appendix. This table reveals no change in workplace injury rates after these buyouts, though we would expect the tests to have limited statistical power because of the small sample size.

mating the triple interaction regression defined in (3), where *Characteristic* in the triple interaction is *WasPublic*. This variable takes a value of one if the observation is in the matched public firm buyout sample and zero if it is in the matched private firm buyout sample. Table 11 presents the results, with six specifications mirroring those of Table 4.

— Table 11 here —

The coefficients for the triple interaction term $BuyoutFirm * PostBuyout * WasPublic$ are negative, large, and statistically significant at least at the 10% level in all six specifications. This negative coefficient implies that workplace injuries fall in establishments of public companies acquired in buyouts not just relative to control firms, but also relative to establishments of private companies acquired in buyouts. While we cannot observe the causes of these differences, and public and private firms acquired in buyouts could differ in many ways, this finding nevertheless adds to the evidence of important differences in operational changes after buyouts of public and private firms.

In our final analysis, we examine cross-sectional differences in the changes in workplace injury rates after public firm buyouts by estimating (3), using just the public firm buyout matched sample. Here, we set *Characteristic* in the triple interaction to various observable firm, acquirer, and transaction characteristics. Table 12 reports the results. Panel A reports estimates where the dependent variable is $Injuries/Employee$. Panel B reports estimates where the dependent variable is $DARTInjuries/Employee$. We only report coefficients for $BuyoutFirm * PostBuyout$ and $BuyoutFirm * PostBuyout * Characteristic$ in the table for the sake of brevity. We are careful not to draw strong conclusions, since many of the cross-sectional variables could proxy for multiple underlying target, acquirer, and transaction characteristics.

— Table 12 here —

Workplace injury rates decline more after buyouts of firms with more tangible assets.

This finding is intuitive - the scope for reducing injury rates is likely to be higher in firms where production is more physical. The change in injury rates is not related to the size of the acquired firm. It is also not related to the fraction of employees in the firm's industry involved in routine task work. This nonresult may shed light on the role that changes in job composition due to restructuring play in the reduction in workplace injury rates after buyouts. Autor and Dorn (2013) argue that routine-task jobs are most prone to automation and offshoring. If workplace injury rates fall after buyouts because PE buyers systematically target firms with large potential savings through automation or offshoring, then one would expect a larger drop in workplace injury rates in firms where these opportunities are larger, that is, those in industries in which routine-task jobs are prevalent. However, one would need job-level data to more definitely test the role of such restructuring or of changes in job composition more generally. Moreover, our executive interviews suggest that "soft" forms of automation that reduce physical touches in the production process could be important, even if replacement of employees with robots is not a first-order driver of the change in injury rates. Workplace injury rates decline more after buyouts of firms with positive abnormal accruals and high levels of analyst coverage and high-turnover shareholders prebuyout. These characteristics could be interpreted as proxies for a greater tendency toward short-termism, defined as overweighting of short-term cash flows. Efforts to reduce workplace injury rates are likely to be costly in the short run, even if they create value in the long run. A reduction in short-termism as a result of going private could also explain the decrease in workplace injury rates after buyouts of public firms relative to private firms (Table 11). However, whether public firms are subject to a short-termism bias that a PE buyout might alleviate is itself subject to debate, and testing this hypothesis is challenging.[27] Our proxies for

[27]See Graham, Harvey, and Rajgopal (2005), Edmans, Fang, and Huang (2017), Edmans, Fang, and Lewellen (2017), and Ladika and Sautner (2020) for evidence in support of the hypothesis and Jiang (2018) for evidence against. Jiang (2018) also points out that evidence from Van Binsbergen et al. (2012), Schulz (2016), and Cohen et al. (2013) is at odds with investors overweighting short-term cash flows.

short-termism are coarse and may capture other firm characteristics. For example, analyst coverage is highly correlated with firm size, while abnormal accruals could proxy for growth. The way in which PE firms structure operational change in these classes of firms may be different. Nevertheless, the results are consistent with views expressed about the lengthening of decision horizons after buyouts expressed by many of our executive interviews.

The decline in workplace injury rates after buyouts shows no relationship with observable buyer characteristics. However, it is worth making two observations here. First, we observe only a limited set of buyer characteristics and cannot rule out the possibility that the decline in injury rates varies with unobserved characteristics. Second, even within the set of observable characteristics, we are limited in our analysis by disclosure restrictions, which, for example, would prevent us from estimating a change in workplace injury rates specific to each PE firm or small subset of PE firms.

Workplace injury rates decrease less after buyouts in establishments that reduce employment post-buyout. Simple downsizing, then, does not appear to explain our results. The decline in injury rates does not differ depending on whether the firm replaces its board chair, adds new directors, or adds directors employed by the PE firm post-buyout. Moreover, the decline is, if anything, smaller when the firm replaces its CEO post-buyout. We therefore cannot link the decline in workplace injury rates to observable governance-related changes. However, the increased monitoring throughout the organization after buyouts that many of our interviews describe could be certainly interpreted as strengthening oversight.

Finally, workplace injuries decline less after buyouts involving larger increases in debt, though the relationship is only statistically significant (at the 10

8 Conclusion

Overall, the results presented in this paper suggest a positive effect of PE buyouts on workplace safety. While recent research provides evidence that at least some workers experience increased unemployment risk and lower wages after buyouts, those that remain employed do appear to experience an improvement in working conditions. Thus, our evidence helps to paint a more nuanced picture of how PE buyouts affect production-level workers. Of course, buyouts are not random events, and one must be careful in reaching conclusions about causality. Nevertheless, the results suggest a “bright side” of PE buyouts for production-level workers and lend further support to the argument that buyouts of public firms improve operational performance. Future work considering how injury rates and wages evolve together around buyouts would be useful for further understanding the impact of these transactions on employees.

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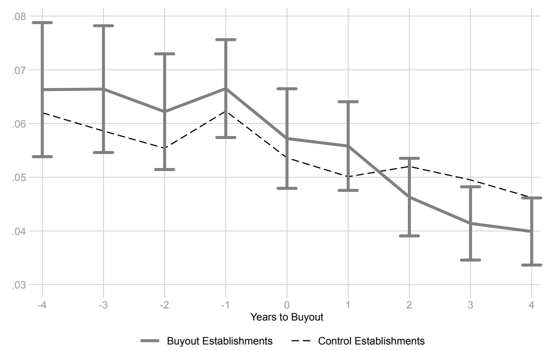
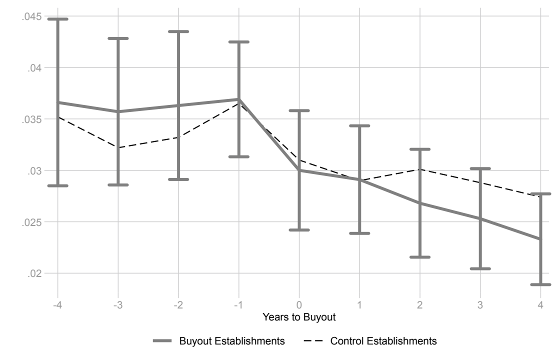
Figure 1: Injury rates around public firm buyouts

This figure presents the mean injury rates and DART injury rates for public firm buyouts and control establishments around the buyout year. Figure 1a presents $Injuries/Employee$. Figure 1b presents $DARTInjuries/Employee$. Figure 1c presents four-digit SIC code industry-adjusted $Injuries/Employee$. Figure 1d presents four-digit SIC code industry-adjusted $DARTInjuries/Employee$. Error bars represent a 95% confidence interval around the difference between the two series.

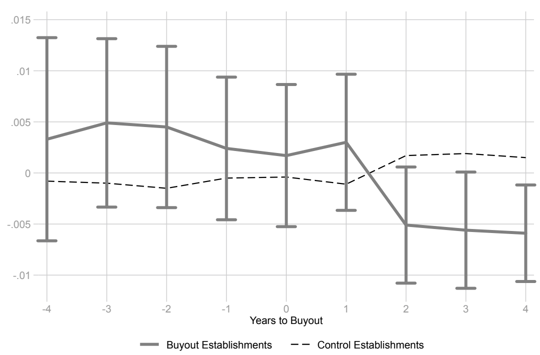
(a) $Injuries/Employee$



(b) $DARTInjuries/Employee$



(c) Industry-adjusted $Injuries/Employee$



(d) Industry-adjusted $DARTInjuries/Employee$

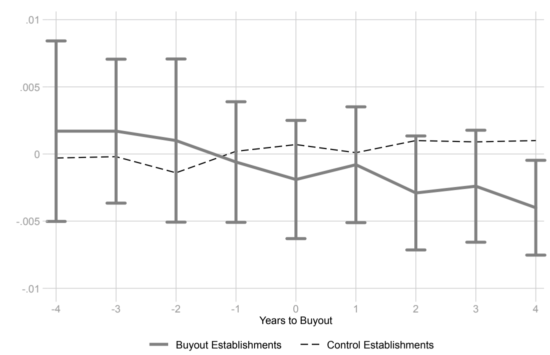


Figure 2: Pooled injury rate around public firm buyouts

This figure presents pooled injury rates across buyout and control establishments around the buyout year. These pooled injury rates are calculated by summing $Injuries$ and $Employees$ separately for all buyout and control establishments in each year relative to the buyout year and then dividing the summed injuries by the summed employees. Note that the figure does not depict error bands because the data are collapsed to a single observation per year for each of the buyout and control establishment samples.

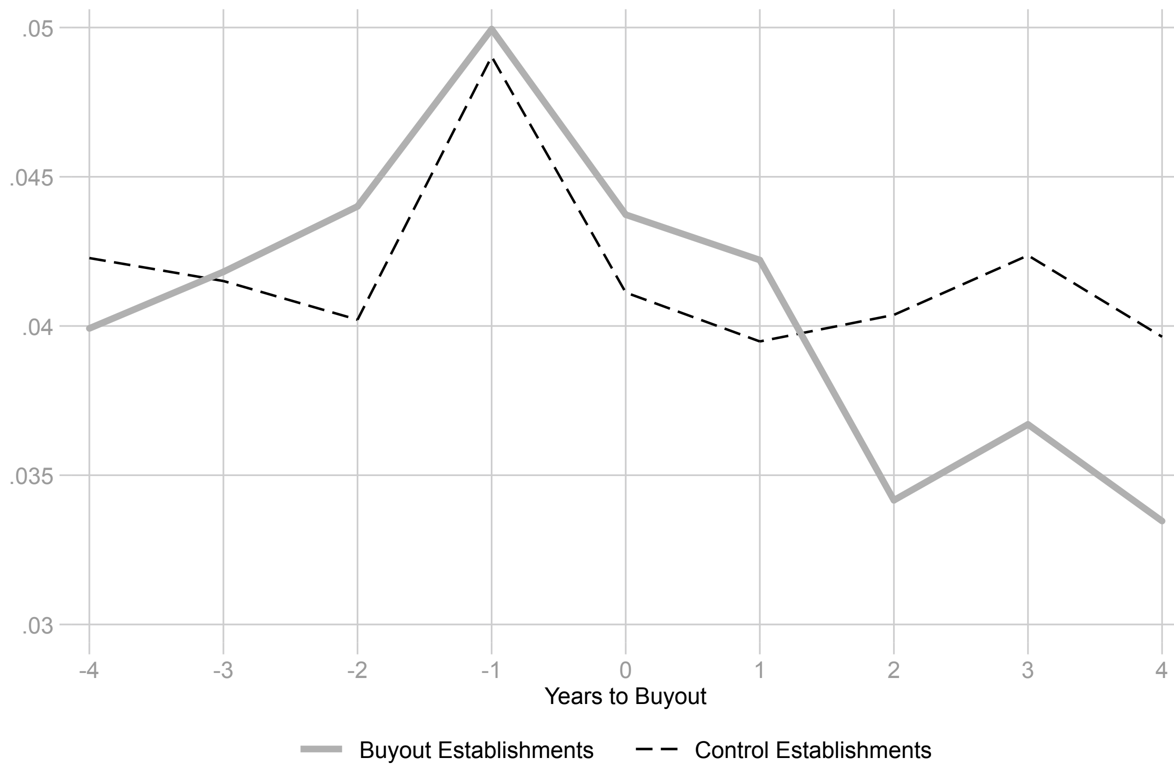


Table 1: Injuries by type and cause

This table shows the percentage of private sector U.S. workplace injuries in 2014 by nature (panel A) and cause (panel B), as reported by the BLS. We computed these percentages from the incident rates available at https://www.bls.gov/news.release/archives/osh$2_1$1192015.*pdf*.

A. Percentage of injuries by nature

Nature of injury	%
Sprains, strains, tears	36.7
Soreness, pain, including back	16.6
Fractures	8.9
Bruises, contusions	7.9
Cuts, lacerations	7.6
Multiple traumatic injuries and disorders	2.9
Heat (thermal) burns	1.5
Carpal tunnel syndrome	0.7
Amputations	0.5
Chemical burns	0.4
Tendonitis (other or unspecified)	0.2
All other natures	15.5

B. Percentage of injuries by cause

Cause of injury	%
Contact with objects	31.4
Fall on same level	21.7
Overexertion in lifting/lowering	14.0
Fall to lower level	6.9
Transportation incidents	6.7
Exposure to harmful substances or environments	5.2
Slips or trips without fall	5.2
Violence and other injuries by persons or animal	5.2
Repetitive motion	3.5
Fires and explosions	0.1

Table 2: Sample formation

This table presents information about the buyout firms in the sample. Panel A describes the sample's construction. Panel B reports the sources of matches with the BLS injury data. Panel C tabulates the number of control establishments for each establishment belonging to a PE-acquired firm ("buyout establishment") in the sample. Panel D reports the number of establishment-year observations for buyout establishments by year relative to the buyout year.

A. Buyout sample formation

	Public-firm buyouts			Private-firm buyouts		
	Firms	Estabs	Estab-years	Firms	Estabs	Estab-years
Starting buyout sample	285			547		
Buyout-BLS data matches	244	13,452	24,213	316	2,051	5,384
Observations in (-4,+4) window around buyout	228	10,356	16,493	288	1,345	2,792
Present in (-4,-1) AND (+1,+4)	152	1,565	5,227	121	199	743
At least one valid control	149	965	3,256	120	194	713
Employment $\geq$ 50 at buyout	134	614	2,341	104	152	606
Employment $\geq$ 100 at buyout (main sample)	114	395	1,639	78	108	474

B. Types of buyout establishment matches

Type of match	Public-firm buyouts	Private-firm buyouts
EIN	253	0
Name	142	108
Total	395	108

D. Control establishments per buyout establishment

	Number of control establishments					
	1	2	3	4	5	Total
Public-firm buyouts	53	38	32	13	259	395
Private-firm buyouts	24	12	10	8	54	108

E. Establishment-year observations by year relative to buyout year

	Number of observations									
	$t-4$	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$	$t+4$	Total
Public-firm buyouts	115	141	160	245	182	216	203	194	183	1,639
Private-firm buyouts	32	38	38	64	59	62	63	59	59	474

Table 3: Summary statistics

This table presents information about the establishments in the sample. Panel A reports means of various establishment characteristics the last year in the sample prior to the buyout for establishments of PE-acquired firms ("buyout establishments") and control establishments. Panel B reports the number of buyout establishments in the final sample in each of the Fama and French (1997) five-industry categories. Panel C reports summary statistics for characteristics of buyout firms in the sample from the year prior to the buyout, along with means and medians for the Compustat universe during the sample period. *Assets* equals total reported assets. *Sales* equals total reported sales. $Debt/Assets$ equals book debt divided by book assets. $Tobin'sq$ equals the ratio of the firm's market value to its book value. $CashFlow/Assets$ equals the sum of income before extraordinary items and depreciation, divided by lagged assets. $Capex/Assets$ equals capital expenditures divided by lagged assets. *$p < .1$; **$p < .05$; ***$p < .01$ (based on a two-tailed t-test).

A. Means of buyout and control establishment characteristics prebuyout

	Public-firm buyouts			Private-firm buyouts		
	Buyout estabs	Control estabs	t-stat	Buyout estabs	Control estabs	t-stat
Number	395	1,583		108	380	
Employees	426.3	415.4	-0.29	372.42	398.86	-0.71
log(Employees)	5.607	5.546	-1.32	5.6390	5.6831	-0.53
HoursWorked/Employee	1.751	1.744	-0.29	1,628.28	1,643.52	-0.51
Injuries/Employee	0.06686	0.0637	-0.94	0.0708	0.0701	0.11
DARTInjuries/Employee	0.03406	0.0333	-0.42	0.0405	0.0389	0.75

B. Buyout establishments by broad industry category

Industry category	Public-firm buyouts	Private-firm buyouts
Consumer durables, Nondurables, Wholesale, retail, and some services (laundries, repair shops)	166	39
Manufacturing, energy, and utilities	59	23
Business equipment, telephone, and television transmission	40	6
Health care, medical equipment, and drugs	48	18
Other	82	22

C. Public buyout firm prebuyout characteristics

	Sample firms					Compustat universe	
	Mean	SD	10th pctile	Median	90th pctile	Mean	Median
Assets	\$1,370M	\$3,562M	\$73M	\$387M	\$2,929M	\$1,592M	\$107M
Sales	\$1,220M	\$2,037M	\$83M	\$391M	\$3,197M	\$1,251M	\$90M
Debt/assets	0.251	0.223	0.000	0.217	0.587	0.262	0.205
Tobin's q	1.168	0.728	0.543	0.910	2.158	2.332	1.250
CashFlow/assets	0.090	0.084	0.016	0.083	0.184	0.065	0.060
Capex/assets	0.071	0.096	0.013	0.049	0.141	0.079	0.044

Table 4: Injury rate changes after PE buyouts: Difference-in-differences estimates

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms in our sample relative to control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, we include only observations from the 4 years before and 4 years after the buyout in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0057			0.0011		
	(0.0038)			(0.0018)		
PostBuyout	0.0034	0.0003	0.0012	0.0035	0.0000	0.0002
	(0.0059)	(0.0033)	(0.0031)	(0.0045)	(0.0038)	(0.0036)
BuyoutFirm * PostBuyout	-0.0100***	-0.0074**	-0.0091***	-0.0044***	-0.0024*	-0.0032**
	(0.0025)	(0.0029)	(0.0029)	(0.0015)	(0.0013)	(0.0015)
log(Employees)			-0.0083***			-0.0043***
			(0.0024)			(0.0015)
HoursWorked/Employee			0.0137***			0.0045*
			(0.0032)			(0.0028)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,942	6,942	6,942	6,942	6,942	6,942
Adjusted R^2	.3153	.6624	.6703	.2505	.6234	.6266

Table 5: Evolution of injury rates after PE buyouts

This table presents estimates of variation in establishment-level injury rates over the 4 years before and 4 years after PE buyouts for establishments of PE-acquired public firms relative to control establishments. Separate results are shown for the public- and private-target samples). In each case, the sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before, year of, and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. We estimate the following regression:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \sum_{K\in(-4,1)\cup(1,4)} \beta_K YearRelBuyoutK_{it}$$
$$+ \sum_{K\in(-4,1)\cup(1,4)} \gamma_K BuyoutFirm_i * YearRelBuyoutK_{it} + \epsilon_{it}.$$

$BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $YearRelBuyoutK$ is the year relative to the buyout year. $YearRelBuyout0$ (i.e., the buyout year) is excluded from the regressions. That is, all estimates are relative to the buyout year. Standard errors, clustered at the firm level, are shown to the right of each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	Public targets		Private targets	
YearRelBuyout-4	.0231	(0.0122)	-0.0156*	(0.0093)
YearRelBuyout-3	0.0195**	(0.0091)	-0.0170	(0.0151)
YearRelBuyout-2	0.0070	(0.0060)	-0.0047	(0.0072)
YearRelBuyout-1	0.0041	(0.0031)	-0.0155	(0.0212)
YearRelBuyout1	-0.0027	(0.0033)	0.0137	(0.0231)
YearRelBuyout2	-0.0105*	(0.0060)	-0.0285	(0.0339)
YearRelBuyout3	-0.0141*	(0.0081)	-0.0041	(0.0208)
YearRelBuyout4	-0.0157	(0.0108)	-0.0111	(0.0294)
BuyoutFirm * YearRelBuyout-4	0.0018	(0.0053)	-0.0121	(0.0078)
BuyoutFirm * YearRelBuyout-3	-0.0002	(0.0033)	-0.0123*	(0.0064)
BuyoutFirm * YearRelBuyout-2	0.0011	(0.0027)	-0.0068	(0.0072)
BuyoutFirm * YearRelBuyout-1	-0.0028	(0.0039)	0.0027	(0.0053)
BuyoutFirm * YearRelBuyout1	-0.0033	(0.0030)	-0.0011	(0.0052)
BuyoutFirm * YearRelBuyout2	-0.0103***	(0.0037)	-0.0094*	(0.0052)
BuyoutFirm * YearRelBuyout3	-0.0099***	(0.0034)	-0.0048	(0.0061)
BuyoutFirm * YearRelBuyout4	-0.0102***	(0.0028)	0.0061	(0.0062)
Observations	7,789		2,092	
Adjusted R^2	.6747		.7302	

Table 6: Injury rate changes after PE buyouts by industry category

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments across four different industry categories. The buyout sample includes only public-target buyouts. In each case, the sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable is $Injuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. All regressions include establishment and industry-year fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. *$p < .1$; **$p < .05$; ***$p < .01$ (based on a two-tailed t-test).

Industry category SIC codes	Manufacturing (2000s & 3000s)	Transportation (4000s)	Trade (5000s)	Services (7000s & 8000s)
PostBuyout	-0.0005	-0.0258***	-0.0004	0.0116*
	(0.0034)	(0.0084)	(0.0038)	(0.0071)
BuyoutFirm * PostBuyout	-0.0080	-0.0122	-0.0084**	-0.0093**
	(0.0052)	(0.0111)	(0.0042)	(0.0041)
Observations	1,518	1,320	1,631	2,430
Adjusted R^2	.7867	.5608	.6222	.6679

Table 7: OSHA inspections and violations

This table presents difference-in-differences estimates of OSHA inspection violation incidence changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to inspected establishments of firms acquired in buyouts between 1995 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched with up to either five control establishments (first two columns) or one control establishment (last two columns) matched on establishment SIC code, inspection year, inspection type, inspection scope, and owner type. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. The dependent variable in columns 1 and 3 is an indicator equal to one if any violation was reported and zero otherwise. The dependent variable in columns 2 and 4 is an indicator equal to one if a serious violation was reported and zero otherwise. *BuyoutFirm* is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. *PostBuyout* is an indicator equal to one in the year after the buyout year and zero before. All regressions include industry-year and inspection type fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. *$p<.1$; **$p<.05$; ***$p<.01$ (based on a two-tailed t-test).

N matches	5 controls per buyout establishment		1 control per buyout establishment	
	All	Serious	All	Serious
Buyout firm	-0.00847	-0.0231	-0.0247	-0.0374*
	(0.0170)	(0.0173)	(0.0208)	(0.0203)
PostBuyout	0.0358	0.0734**	0.0700	0.119**
	(0.0373)	(0.0368)	(0.0508)	(0.0525)
BuyoutFirm × PostBuyout	-0.0532*	-0.0733**	-0.0699**	-0.0971**
	(0.0279)	(0.0313)	(0.0300)	(0.0458)
Observations	6,208	6,208	2,410	2,410
Adjusted R^2	.047	.025	.065	.036

Table 8: Probability of exit via IPO and changes in injury rates after PE buyouts

This table presents estimates from probit regressions of whether or not a firm exited buyout status via an initial public offering (IPO) on the change in its injury rate after the buyout relative to controls. Observations are at the firm level. The dependent variable is an indicator equal to one if the firm exited buyout status via IPO and zero otherwise. The explanatory variable *InjuryRateChange* is constructed as follows. For each buyout firm in the final matched sample, we compute the average number of injuries per employee across establishment-years before and after the buyout, as well as the comparable numbers for all control establishments matched to that firm's establishments. In doing so, we use establishment-year observations in the 4 years before and 4 years after the buyout, as in our difference-in-differences analysis. We then compute *InjuryRateChange* as the change in average injury rate for the buyout firm from before to after the buyout, minus the change in average injury rate for the control establishments. We compute the explanatory variable *IndAdjInjuryRateChange* similarly, using the residuals from an OLS regression of injuries per employee on industry-year indicators rather than the raw injuries per employee as the input. *$p < .1$; **$p < .05$; ***$p < .01$ (based on a two-tailed t-test).

	(1)	(2)
InjuryRateChange	-5.7863*	
	(3.2184)	
IndAdjInjuryRateChange		-5.5713*
		(3.2088)
Constant	-1.0299***	-1.0290***
	(0.1576)	(0.1580)
Observations	114	114
Pseudo-R^2	.0250	.0230

Table 9: Employment and employee utilization changes after PE buyouts

This table presents difference-in-differences estimates of post-buyout employment and employee utilization changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of public firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $log(Employment)$. The dependent variable in columns 4 through 6 is $HoursWorked/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $EstabInjuryRate$ equals the establishment's $Injuries/Employee$ the last year observed prior to the buyout. $IndustryInjuryRate$ equals the mean four-digit SIC code $Injuries/Employee$ for the full BLS sample. $EmpDecrease$ is an indicator equal to one if an establishment's employment declines from the last year observed prebuyout to the first year observed post-buyout and zero otherwise. Standard errors, clustered at the firm level, are shown below each point estimate. *$p<.1$; **$p<.05$; ***$p<.01$ (based on a two-tailed t-test).

Dep var	log(Empl)	log(Empl)	log(Empl)	HoursWorked/ Employee	HoursWorked/ Employee	HoursWorked/ Employee
PostBuyout	-0.0679	-0.0760	-0.2339	-0.1068	-0.1142	-0.1292
	(0.1210)	(0.1197)	(0.1426)	(0.0676)	(0.0684)	(0.0947)
BuyoutFirm * PostBuyout	-0.1395***	-0.2517***	-0.2035***	0.0366*	0.0836***	0.0071
	(0.0371)	(0.0667)	(0.0502)	(0.0206)	(0.0319)	(0.0303)
PostBuyout * EstabInjuryRate		0.4272			0.0956	
		(0.3207)			(0.1494)	
BuyoutFirm * PostBuyout * EstabInjuryRate		1.710***			-0.7141**	
		(0.4959)			(0.3336)	
PostBuyout * IndustryInjuryRate			5.6282**			0.8179
			(2.4147)			(2.1093)
BuyoutFirm * PostBuyout * IndustryInjuryRate			1.1578**			0.5349
			(0.4752)			(0.3706)
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,942	6,942	6,942	6,942	6,942	6,942
Adjusted R^2	.8770	.8779	.8772	.6734	.6639	.6734

Table 10: Injury rates and post-buyout reobservation of buyout establishments

This table presents estimates from a linear probability model of the likelihood that a public buyout establishment in the BLS data in the 4-year window prior to the buyout is reobserved in the data in each of the 4 years after the buyout. The sample consists of all establishments matched to the public buyout sample in at least one of the 4 years before the buyout. The dependent variable is an indicator for whether the establishment is reobserved in a given post-buyout year. The explanatory variable is $Injuries/Employee$ measured in the last year the establishment is in the BLS data prior to the buyout. The regressions include firm fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

Reobserved at	Year $t+1$	Year $t+2$	Year $t+3$	Year $t+4$
Injuries/employee	0.2661***	0.1900***	0.2122***	0.1823**
	(0.0695)	(0.0720)	(0.0793)	(0.0964)
Observations	1,353	1,353	1,353	1,353
Adjusted R^2	.4583	.4591	.4581	.4572

Table 11: Injury rate changes after PE buyouts: Public- versus private-firm buyouts

This table presents triple difference estimates of the difference in post-buyout injury rate changes at establishments of PE-acquired firms (first difference), relative to control establishments (second difference), between previously public and previously private target firms (third difference). The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $WasPublic$ is an indicator equal to one for public-firm buyout establishments and zero for private-firm buyout establishments. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	-0.0016			-0.0015		
	(0.0044)			(0.0031)		
PostBuyout	0.0075	0.0053	0.0038	0.0066*	0.0042	0.0035
	(0.0073)	(0.0049)	(0.0045)	(0.0039)	(0.0037)	(0.0035)
WasPublic	0.0022			0.0039		
	(0.0057)			(0.0032)		
BuyoutFirm * PostBuyout	0.0011	0.0015	0.0025	0.0027	0.0038	0.0043
	(0.0045)	(0.0046)	(0.0045)	(0.0031)	(0.0030)	(0.0029)
BuyoutFirm * WasPublic	0.0073			0.0009		
	(0.0058)			(0.0036)		
PostBuyout * WasPublic	-0.0018	-0.0061*	-0.0044	-0.0036	-0.0050**	-0.0043**
	(0.0047)	(0.0034)	(0.0035)	(0.0024)	(0.0021)	(0.0021)
BuyoutFirm * PostBuyout * WasPublic	-0.0112**	-0.0092*	-0.0116**	-0.0071**	-0.0066**	-0.0076**
	(0.0052)	(0.0054)	(0.0054)	(0.0035)	(0.0032)	(0.0032)
log(Employees)			-0.0059**			-0.0029*
			(0.0025)			(0.0015)
HoursWorked/Employee			0.0136***			0.0055**
			(0.0026)			(0.0024)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,779	8,779	8,779	8,779	8,779	8,779
Adjusted R^2	.3224	.6751	.6811	.2612	.6339	.6365

Table 12: Injury rate changes after PE buyouts: Variation with firm characteristics

This table presents estimates of cross-sectional differences in post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. All columns show estimates from OLS regressions of the following form:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + \gamma BuyoutFirm_i * PostBuyout_{it} + \theta PostBuyout_t * Characteristic_i \\ + \lambda BuyoutFirm_i * PostBuyout_{it} * Characteristic_i + \epsilon_{it}.$$

In panel A, $InjuryRate$ is $Injuries/Employee$. In panel B, $InjuryRate$ is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. See Appendix A. for definitions of the characteristics. The coefficients for $PostBuyout$ and $PostBuyout * Characteristic$ are not shown for the sake of brevity. All regressions include establishment and industry-year fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

A. All injuries

	BuyoutFirm * PostBuyout		BuyoutFirm * PostBuyout * Characteristic		Obs	Adj R^2
Target characteristics						
(1) log(Assets)	−0.0198	(0.0122)	0.0016	(0.0015)	6,942	0.6623
(2) TangibleAssetRatio	0.0001	(0.0027)	−0.0239**	(0.0117)	6,942	0.6640
(3) RoutineTaskShare	−0.0072	(0.0065)	−0.0013	(0.0016)	6,942	0.6631
(4) PosAbnormalAccruals	−0.0175*	(0.0120)	−0.0090**	(0.0043)	6,360	0.6757
(5) HighAnalystCoverage	−0.0026	(0.0015)	−0.0148**	(0.0068)	6,942	0.6639
(6) TransitoryHoldingPct	−0.0035	(0.0041)	−0.0168**	(0.0081)	6,922	0.6641
Buyer characteristics						
(7) FrequentBuyer	−0.0223	(0.0135)	−0.0086	(0.0059)	6,942	0.6627
(8) ClubDeal	−0.0034	(0.0026)	−0.0077	(0.0056)	6,942	0.6630
(9) IndustrySpecialist	−0.0072**	(0.0030)	0.0038	(0.0079)	6,942	0.6621
(10) MgmtParticipation	−0.0164	(0.0127)	0.0018	(0.0065)	6,942	0.6624
Transaction characteristics						
(11) EmpDecrease	−0.0152**	(0.0068)	0.0134*	(0.0076)	6,942	0.6637
(12) ChairTurnover	−0.0082**	(0.0032)	0.0068	(0.0054)	6,942	0.6621
(13) NewDirector	−0.0065***	(0.0023)	−0.0011	(0.0059)	6,942	0.6619
(14) BuyerExecJoinsBoard	−0.0052***	(0.0017)	−0.0108	(0.0139)	6,942	0.6622
(15) CEOTurnover	−0.0084***	(0.0032)	0.0110**	(0.0045)	6,942	0.6629
(16) LeverageChange	−0.0055	(0.0245)	0.0051	(0.0102)	3,809	0.6344

Table 12: Injury rates around buyouts: Variation with prebuyout firm characteristics (Continued)

B. DART injuries

	BuyoutFirm * PostBuyout		BuyoutFirm * PostBuyout * Characteristic		Obs	Adj R^2
Target characteristics						
(1) log(Assets)	−0.0039	(0.0066)	0.0002	(0.0008)	6,942	0.6232
(2) TangibleAssetRatio	0.0000	(0.0020)	−0.0074	(0.0065)	6,942	0.6235
(3) RoutineTaskShare	−0.0084	(0.0065)	0.0013	(0.0105)	6,815	0.6224
(4) PosAbnormalAccruals	−0.0001	(0.0065)	−0.0031	(0.0031)	6,293	0.6329
(5) HighAnalystCoverage	−0.0065	(0.0075)	−0.0079**	(0.0039)	6,942	0.6240
(6) TransitoryHoldingPct	0.0024	(0.0022)	−0.0172***	(0.0057)	6,922	0.6276
Buyer characteristics						
(7) FrequentBuyer	−0.0030	(0.0073)	−0.0021	(0.0037)	6,942	0.6231
(8) ClubDeal	−0.0039	(0.0062)	−0.0012	(0.0026)	6,942	0.6232
(9) IndustrySpecialist	−0.0070	(0.0070)	−0.0010	(0.0052)	6,942	0.6233
(10) MgmtParticipation	−0.0019	(0.0069)	0.0030	(0.0030)	6,942	0.6236
Transaction characteristics						
(11) EmpDecrease	−0.0033	(0.0026)	0.0017	(0.0035)	6,942	0.6238
(12) ChairTurnover	−0.0031**	(0.0013)	0.0042	(0.0034)	6,942	0.6243
(13) NewDirector	−0.0033**	(0.0015)	0.0018	(0.0025)	6,942	0.6240
(14) BuyerExecJoinsBoard	−0.0030	(0.0013)	0.0037	(0.0039)	6,942	0.6234
(15) CEOTurnover	−0.0009	(0.0063)	0.0046	(0.0038)	6,942	0.6230
(16) LeverageChange	−0.0058	(0.0132)	0.0097*	(0.0051)	3,898	0.5978

Appendix A. Variable Definitions

$Injuries/Employee$: Annual number of injuries divided by the reported average number of employees, measured at the establishment-year level, from the SOII

$DARTInjuries/Employee$: Annual number of "days away, restricted, transfer" injuries (injuries so severe the employee could not return to work at a normal capacity) divided by reported average number of employees, measured at the establishment-year level, from the SOII

$HoursWorked/Employee$: Total hours worked at an establishment scaled by the reported average number of employees, measured at the establishment-year level, from the SOII

$log(Assets)$: The natural log of the Compustat item at for the buyout firm immediately prior to the buyout

$TangibleAssetRatio$: Compustat item $ppent$ divided by at

$RoutineTaskShare$: Percentage of employees in an industry performing "routine task" labor, measured at the industry-year level, based on OES and DOT data and following the procedure of Autor and Dorn (2013) (we thank Ben Zhang for sharing the routine task share data)

$AbnormalAccruals$: Abnormal accruals from a modified Jones' model based on Compustat data. Total annual accruals, $ta = (oancf - ibc)/at_{t-1}$, are regressed on the change in cash sales $((sale-sale_{t-1})-(rect-rect_{t-1}))/l12_{t-1}$ $1/at_{t-1}$, tangible assets $ppent/at_{t-1}$, and inverse assets $1/at_{t-1}$ for each year and two-digit SIC industry. The residual is then calculated as the abnormal accrual.

$PosAbnormalAccruals$: Indicator equal to one if $AbnormalAccruals > 0$ and zero otherwise

$AnalystCoverage$: # of analysts covering each stock, from I/B/E/S

$HighAnalystCoverage$: Indicator equal to one if $AnalystCoverage$ is greater than the sample median (13) and zero otherwise

$TransitoryHoldingPct$: Shares held by institutional investors identified as "transitory" investors by Bushee (1998) divided by all shares held by institutional investors per the Thompson 13(f) holdings data

$FrequentBuyer$: The acquirer group includes a PE firm with at least six buyouts in the full sample

$ClubDeal$: Indicator for whether multiple PE firms are part of a buyer group in a transaction

IndustrySpecialist: Indicator equal to one if at least 50% of buyer's buyout targets in same two-digit SIC industry as target

MgmtParticipation: Indicator equal to one if management was part of acquirer group and zero otherwise

EmpDecrease: Reduction in reported average employment, from the SOII

LeverageChange: The change in leverage $(dlc+dltt)/at$ from before the year before buyout to the year after the buyout, from Compustat

CEOTurnover: CEO changes at time of or within first year after buyout, per Capital IQ People Intelligence database

ChairTurnover: Board chair changes at time of or within first year after buyout, per Capital IQ People Intelligence database

BuyerExecJoinsBoard: PE acquirer executive joins board at time of or within first year after buyout, per Capital IQ People Intelligence database

NewDirector: New director joins board at time of or within first year after buyout, per Capital IQ People Intelligence database

Appendix B. Additional Tables

Table B1: Injury rate changes after PE buyouts: Public firm controls only

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. Each buyout establishment is matched to one control establishment (instead of up to five, as in the main sample) in the same industry and belonging to a publicly traded firm with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0108*			0.0022		
	(0.0062)			(0.0027)		
PostBuyout	-0.0008	0.0081	0.0083*	-0.0049	0.0002	0.0004
	(0.0054)	(0.0056)	(0.0044)	(0.0052)	(0.0046)	(0.0041)
BuyoutFirm * PostBuyout	-0.0121***	-0.0122**	-0.0134***	-0.0062***	-0.0040*	-0.0046**
	(0.0040)	(0.0055)	(0.0052)	(0.0023)	(0.0020)	(0.0019)
log(Employees)			-0.0066			-0.0032
			(0.0041)			(0.0026)
HoursWorked/Employee			0.0214***			0.0118***
			(0.0055)			(0.0031)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,547	2,547	2,547	2,547	2,547	2,547
Adjusted R^2	0.4707	0.7766	0.7852	0.3794	0.7322	0.7384

Table B2: Injury rate changes after PE buyouts: Matching on multiple characteristics

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry using propensity score matching, where $log(Employees)$, $HoursWorked/Employee$, and $Injuries/Employee$ are used to estimate an establishment's propensity to be acquired as part of a buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0068*			0.0017		
	(0.0035)			(0.0015)		
PostBuyout	0.0041	0.0034	0.0038	-0.0007	-0.0009	-0.0009
	(0.0053)	(0.0048)	(0.0042)	(0.0040)	(0.0044)	(0.0040)
BuyoutFirm * PostBuyout	-0.0115***	-0.0083***	-0.0099***	-0.0050***	-0.0026**	-0.0034**
	(0.0025)	(0.0026)	(0.0026)	(0.0014)	(0.0013)	(0.0014)
log(Employees)			-0.0083***			-0.0047***
			(0.0029)			(0.0017)
HoursWorked/Employee			0.0139***			0.0057***
			(0.0032)			(0.0022)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,615	6,615	6,615	6,615	6,615	6,615
Adjusted R^2	.3394	.6633	.6710	.2630	.6257	.6296

Table B3: Injury rate changes after PE buyouts: Matching on name only

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Unlike in Table 4, the sample includes public buyout firms matched only on firm name (i.e., no EIN matches). Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0053			0.0009		
	(0.0040)			(0.0023)		
PostBuyout	0.0034	0.0002	0.0014	0.0037	0.0000	0.0002
	(0.0068)	(0.0033)	(0.0042)	(0.0045)	(0.0038)	(0.0036)
BuyoutFirm * PostBuyout	-0.0096***	-0.0070**	-0.0082***	-0.0042**	-0.0022*	-0.0031*
	(0.0029)	(0.0031)	(0.0026)	(0.0018)	(0.0013)	(0.0017)
log(Employees)			-0.0083***			-0.0046***
			(0.0024)			(0.0017)
HoursWorked/Employee			0.0137***			0.0055**
			(0.0032)			(0.0025)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,860	6,860	6,860	6,860	6,860	6,860
Adjusted R^2	.3140	.6612	.6700	.2498	.6221	.6260

Table B4: Injury rate changes after PE buyouts: Single control establishment

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to one control establishment (instead of up to five, as in the main sample) in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0070			0.0021		
	(0.0040)			(0.0021)		
PostBuyout	-0.0075	-0.0101	-0.0075	-0.0031	-0.0099	-0.0087
	(0.0101)	(0.0075)	(0.0080)	(0.0073)	(0.0062)	(0.0064)
BuyoutFirm * PostBuyout	-0.0099***	-0.0069**	-0.0092***	-0.0035*	-0.0015	-0.0027
	(0.0035)	(0.0032)	(0.0035)	(0.0019)	(0.0016)	(0.0018)
log(Employees)			-0.0103***			-0.0055***
			(0.0027)			(0.0015)
HoursWorked/Employee			0.0161***			0.0077***
			(0.0040)			(0.0019)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,914	2,914	2,914	2,914	2,914	2,914
Adjusted R^2	.3322	.6258	.6401	.2303	.6020	.6105

Table B5: Injury rate changes after PE buyouts: Difference-in-differences estimates with a lower minimum establishment size

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 50 employees at the time of the buyout (instead of 100 as in the main sample) are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0087**			0.0028		
	(0.0042)			(0.0018)		
PostBuyout	-0.0021	0.0014	0.0019	0.0016	0.0032	0.0033
	(0.0060)	(0.0045)	(0.0044)	(0.0042)	(0.0042)	(0.0040)
BuyoutFirm * PostBuyout	-0.0096***	-0.0067**	-0.0076***	-0.0038**	-0.0020	-0.0024*
	(0.0029)	(0.0028)	(0.0029)	(0.0015)	(0.0012)	(0.0013)
log(Employees)			-0.0084***			-0.0034***
			(0.0019)			(0.0012)
HoursWorked/Employee			0.0117***			0.0033
			(0.0038)			(0.0029)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,026	10,026	10,026	10,026	10,026	10,026
Adjusted R^2	.2686	.6012	.6078	.2396	.5949	.5967

Table B6: Injury rate changes after PE buyouts: Estimates from count models

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired firms relative to control establishments based on count models. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. The dependent variable in columns 1 through 4 is $Injuries/Employee$. The dependent variable in columns 5 through 8 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries				DART injuries			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BuyoutFirm	0.0102	0.1766			-0.0256	0.0518		
	(0.2149)	(0.1469)			(0.2293)	(0.1843)		
PostBuyout	0.3610**	0.0676	0.0279	0.0230	0.3000*	0.0219	0.0493	0.0471
	(0.1436)	(0.1153)	(0.0472)	(0.0490)	(0.1572)	(0.1359)	(0.0705)	(0.0721)
BuyoutFirm * PostBuyout	-0.1335	-0.1463*	-0.1390*	-0.1469**	-0.1427	-0.1003	-0.1187*	-0.1225*
	(0.0988)	(0.0843)	(0.0781)	(0.0718)	(0.1281)	(0.0954)	(0.0629)	(0.0638)
log(Employees)				-0.1498***				-0.1007**
				(0.0352)				(0.0435)
HoursWorked/Employee				0.3067***				-0.2400***
				(0.0494)				(0.0780)
Model	Poisson	nbreg	Poisson	Poisson	Poisson	nbreg	Poisson	Poisson
Establishment FE	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,223	14,223	12,376	12,376	13,698	13,698	10,947	10,947

Table B7: Injury rate changes after PE buyouts: Private-to-private buyout sample

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired firms in our auxiliary sample of private-to-private buyouts relative to control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	-0.0014			0.0001		
	(0.0050)			(0.0034)		
PostBuyout	-0.0089	-0.0100	-0.0104	-0.0092	0.0009	-0.0104
	(0.0155)	(0.0191)	(0.0187)	(0.0117)	(0.0149)	(0.0187)
BuyoutFirm * PostBuyout	0.0004	0.0014	0.0022	0.0023	0.0037	0.0022
	(0.0050)	(0.0049)	(0.0049)	(0.0034)	(0.0032)	(0.0049)
log(Employees)			0.0042			0.0042
			(0.0036)			(0.0036)
HoursWorked/Employee			0.0136***			0.0136***
			(0.0040)			(0.0040)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,837	1,837	1,837	1,837	1,837	1,837
Adjusted R^2	.3349	.6975	.7011	.2964	.6603	.6645

AUGUST 2018

Companies Rush to Go Private

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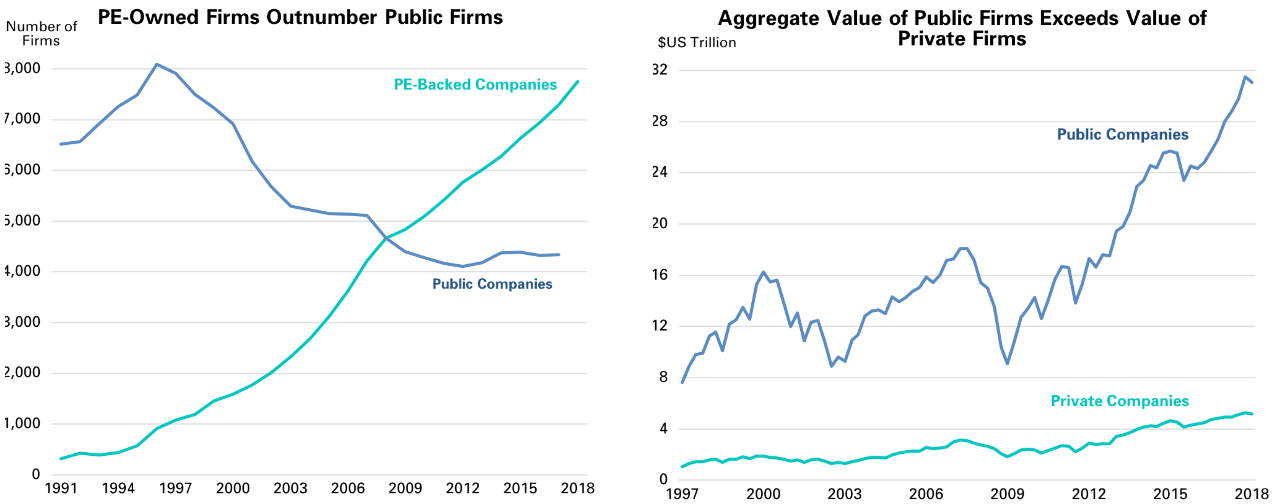
ACCESS TO CAPITAL

MILKEN INSTITUTE

COMPANIES RUSH TO GO PRIVATE

The storm over Elon Musk's awkward attempt to take Tesla private is the latest wave of the continuing tsunami that began in 1995 when firms started their rush to "go private." Even Saudi Arabia has announced intentions to delay the listing of their nationally owned oil company Saudi Aramco until 2020 or beyond. We now have more firms owned by private equity (PE) investors than are listed on all the U.S. stock exchanges, although they are small when compared with their listed counterparts (Figure 1).[1] As market breadth narrows, questions are being raised about the efficacy of listing on U.S. equity markets. More investors and company managers are re-examining the role of public equity markets for financing new companies and innovative investments, which traditionally have helped boost U.S. productivity and sustainable economic growth.[2]

Figure 1. More U.S Companies Are Owned by Private Equity than Are Listed on Public Stock Exchanges



Source: World Federation of Exchanges, Federal Reserve, SEC, Thomson Reuters Eikon, and Milken Institute; March 2018
Note: Public domestic companies, excl. investment trusts

[1] The Federal Reserve estimates the total value of all publicly traded firms to be $31.1T at the end of 2018Q1 compared with $5.2T for all privately held companies.

[2] Demirguc-Kunt and Levine (1997) highlight "how stock markets might boost long-run economic growth" and Levine's (2005) literature review outlines the positive relationship between financial deepening and growth. Unfortunately, there is little empirical research on the implications of capital market broadening for boosting productivity and growth.

COMPANIES RUSH TO GO PRIVATE

Private capital markets have become a favored alternative source of company financing. In addition, private equity firms have become more innovative in developing options for financing the growth of small and emerging firms, and converting many publicly listed companies into private companies. Consequently, more companies are choosing to be privately owned, or are staying private for longer periods before becoming publicly listed.

There is a myriad of reasons to explain why companies may prefer private versus public ownership but none are definitive. One often cited reason for companies going private are burdensome regulatory disclosure and reporting requirements of being a listed company. Yet there is little definitive evidence to substantiate such claims.[3]

The hostile interactions between Elon Musk and company analysts during recent quarterly earnings calls for Tesla illustrate the tension over public disclosure and accountability requirements, and company managers' desire for autonomy. In addition, firms that are publicly listed are vulnerable to managerial interference by activist shareholders who often agitate for disruptive changes in management strategies, especially after reported misses in achieving quarterly earnings expectations.[4] Even President Trump has joined in on this debate by asking the Securities and Exchange Commission to assess potential gains from changing corporate quarterly earnings reporting practices.[5] Certainly, PE firms may also require even more frequent reporting, and may impose severe measures to ensure accountability in the face of poor earnings performance. Indeed, PE owners may choose to make major changes in the management personnel, managerial strategies, and company operations. However, PE company managers are not subject to additional public pressure from stock price gyrations and judgmental company analysts.

[3] More key arguments and findings regarding regulatory burden will be detailed in our upcoming research report.

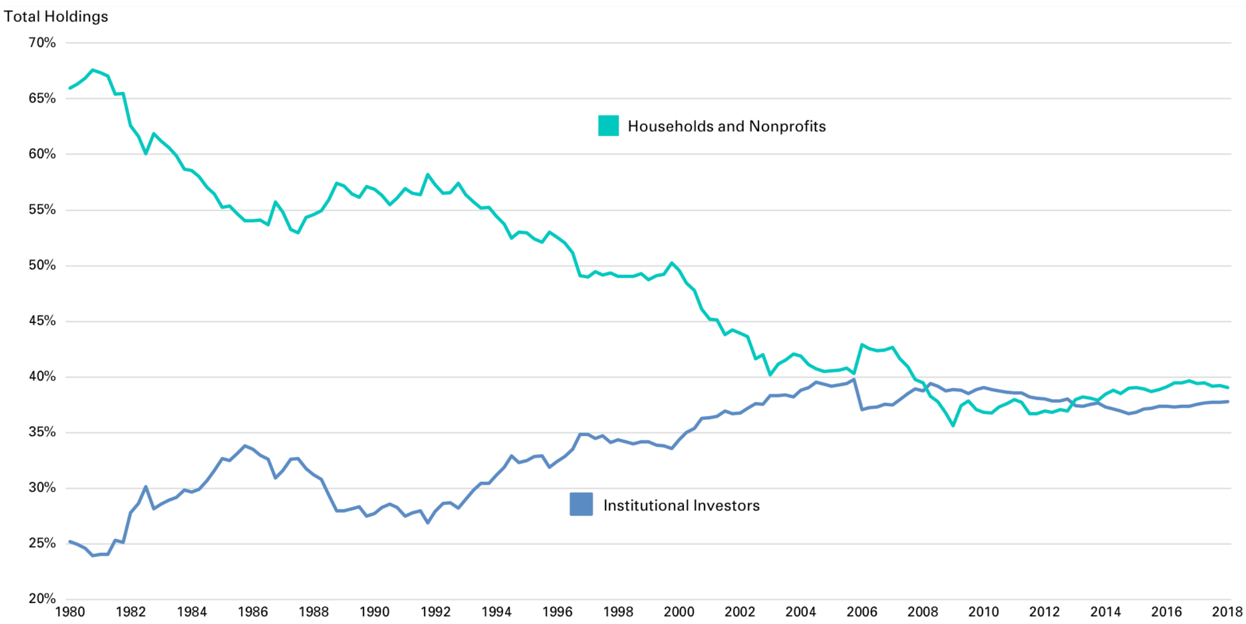
[4] Jamie Dimon and Warren Buffett propose eliminating quarterly earnings reporting for public companies to reduce shareholder pressure and stock market turmoil in response to earnings misses. This issue is discussed here.

[5] President Trump tweeted on August 17, 2017: "In speaking with some of the world's top business leaders I asked what it is that would make business (jobs) even better in the U.S. 'Stop quarterly reporting & go to a six month system,'" said one. That would allow greater flexibility & save money. I have asked the SEC to study!"

PRIVATE CAPITAL FAVORED BY SHIFT IN COMPOSITION OF EQUITY OWNERSHIP

Incentives for companies to favor private ownership increased in the late 1990s when institutional investors began to dominate individual/household equity investors. Following the end of WWII, households owned most of the shares in publicly listed companies directly, and until 1980, they held close to 70 percent of all corporate equity (Figure 2).[6] Since then, the household ownership share has declined steadily until 2003, after which it stabilized at approximately 40 percent. During the same period, institutional investors' share doubled from 20 to 40 percent.

Figure 2. Corporate Equity Investors Shift from Households to Institutions, January 1980-March 2018



Source: Federal Reserve and Milken Institute; as of June 2018
Note: Institutional investors include insurance companies, private pension funds, mutual and closed-end funds, and exchange-traded funds

The growing presence of institutional investors allowed companies to raise funds more efficiently and without many of the reporting and disclosure requirements necessary for publicly traded companies with retail investors. That is because institutional investors tend to rely on other intermediaries (e.g., venture capital and private equity firms) to monitor and manage how companies use investor funds. By comparison, less-sophisticated retail investors generally are presumed to need a higher level of investor protection, which implies stringent regulatory, disclosure, and governance requirements enforced by various government agencies.[7]

[6] Corporate equities in the Federal Reserve's Financial Accounts include both public and private equity.

[7] See "Remembering the Forgotten Investor" by then acting chairman of the SEC Michael S. Piwowar. It questions the sharp delineations in the definition for accredited or qualified investors stemming from Regulation D of the 1933 Act. They are individuals with an annual income of at least $200,000 in each of the past two years ($300,000 for joint income) or a net worth of at least $1 million

PRIVATE CAPITAL BECOMES FAVORED ALTERNATIVE FOR COMPANY FINANCING

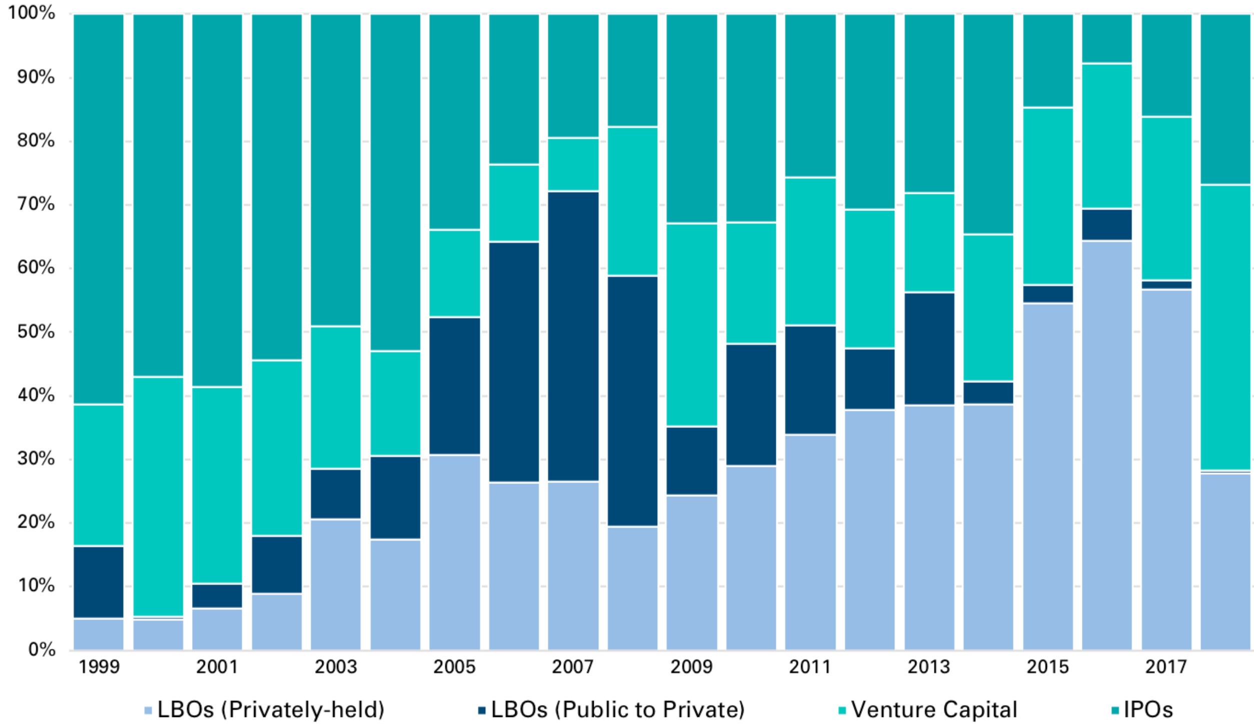
Private equity firms provide financing options that have become the favored alternatives for companies that are eager to raise funds. Companies can raise capital more efficiently (e.g., at a lower administrative cost) with private placements to "qualified" investors than by a public offering.[8] In addition, households may continue to benefit from corporate gains by buying shares in the few publicly traded firms conducting private financing (e.g., listed PE firms), or by allowing their pension funds to do the investing for them. Investing indirectly with institutional investors such as pension funds would allow households to own indirectly a more diversified portfolio containing securities that would otherwise not be available for small investors.

Regulations that segregate investment opportunities, and exclude large groups of investors from profitable investment opportunities have severe consequences that include worsening the distribution of wealth. Such exclusionary practices raise thorny social justice issues regarding whether all investors should have equal access to investment opportunities. Because they are considered "unsophisticated," most households are not even given the opportunity to choose to invest in some opportunities regardless of their level of education or investment experience. They will not meet necessary regulatory requirements for being a "qualified" or "accredited" investor that is necessary to invest in most private equity funds. Indeed, current regulatory policies limit most individual and household investments to a segmented (and shrinking) universe of publicly listed companies.

[8] A private placement is a debt or equity security that does not involve a public offering, and is therefore exempt from registration with the SEC. These investments are limited to accredited investors, see previous sidenote.

COMPANIES RUSH TO GO PRIVATE

Figure 3. IPOs Account for a Diminishing Share of Capital Raised/Invested



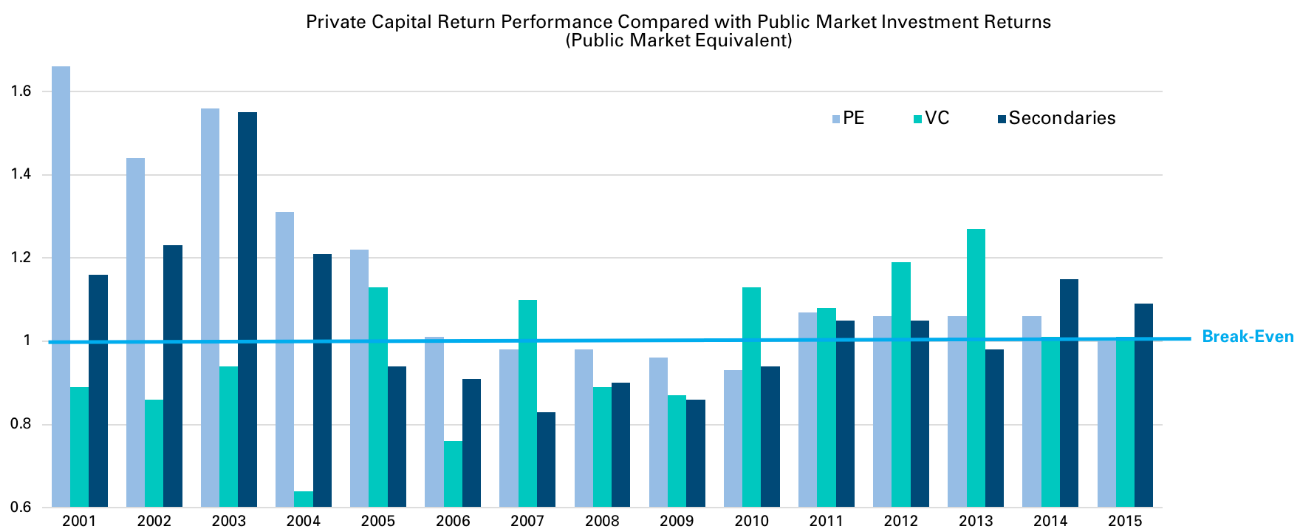
Source: Pitchbook, Thomson Reuters Eikon, and Milken Institute; 2018 reflects the time period through May 2018
Note: Limited to companies headquartered in the U.S. and initial public offerings on NASDAQ or NYSE

Notwithstanding their limited investor base, private equity (PE) firms are becoming a major source of funding for companies that do not wish to list on the public stock exchanges, or for those listed companies desiring to "go private." PE now provides five times more capital than raised with IPOs. By comparison, less than two decades ago PE-supplied capital was only 75 percent of the capital raised by IPOs (Figure 3). The dramatic shift began in 2004, when PE sponsors spent more than $150 billion to take public companies private; a year when IPOs did not account for more than 50 percent of all equity capital raised.

PRIVATE EQUITY OFFERED INVESTORS HIGHER RETURNS THAN LISTED COMPANIES

For some time, private equity has offered investors returns that were substantially higher than those available from investing in listed companies. Prior to the Crisis, the returns on PE funds exceeded the returns offered by publicly listed firms (e.g., the S&P 500) by a wide margin (20 to 60 percent) (Figure 4).[9] However, in the years after the Crisis, the performance of PE funds deteriorated significantly, and barely paced the S&P 500 in 2015.[10] The development of secondary markets for PE assets have allowed PE firms to realize higher returns in the post-Crisis years. However, because secondary PE market transactions are negotiated, pricing and realized returns may not be as efficient as under full price discovery in public equity markets. Secondary PE market valuations may be ephemeral once companies are listed on public stock exchanges.

Figure 4. Private Equity Returns Were Much Higher



Source: Pitchbook and Milken Institute

The increased use of leverage has helped boost apparently sagging returns. PE firms often finance funds participating in the leveraged loan market that helps finance leveraged buyouts (LBOs). Often the companies involved with PE funds already are

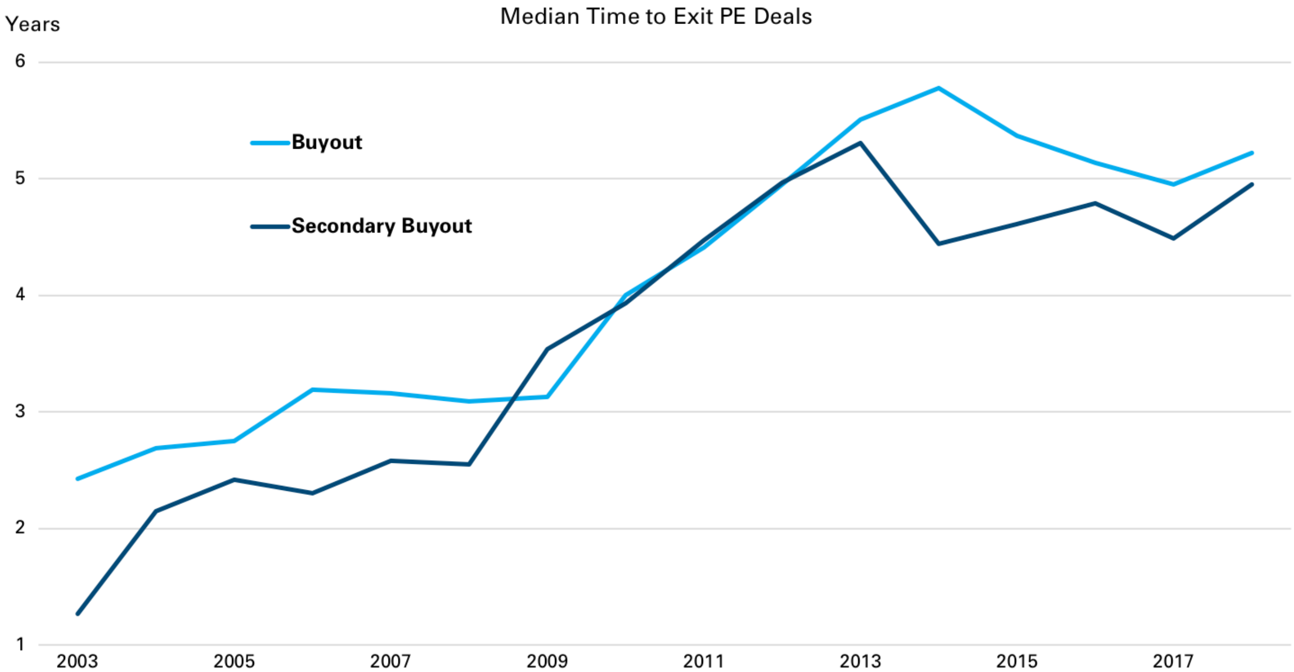
[9] To compare private equity returns we used the "public market equivalent" measure, which compares the return of PE funds relative to the S&P 500; a value greater than 1 implies outperformance. See "PME Introduction" by Henly (2013) for more detail.

[10] The breakeven point is where private capital returns equal the returns from the S&P500 or an alternative stock market index representative of the private capital investment. Venture capital funds (who supplied capital to firms at an earlier stage of development) had slightly better returns than PE funds during the 2010 to 2013 period.

heavily indebted and the loans to fund LBOs have weak lending terms ("covenant lite").[11] Credit rating agencies have recently issued warnings about the risk associated with such corporate borrowing as interest rates rise and the pace of PE deal-making surges.

The larger range of financing options (including secondary PE market sales) utilized by PE firms appears to have lengthened the duration firms remain private. The time for firms to "turn-around," i.e., between a fund's entry and exit from PE ownership, has increased significantly over the last two decades. Indeed, the median time to exit for both initial and secondary buyouts has tripled since 2002 (Figure 5). Providing company managers more time to shape and execute their strategic and operational plans could allow these companies to be more profitable once they do "go public."

Figure 5. Private Equity Deals Take Longer to Exit



Source: Pitchbook and Milken Institute, May 2018

[11] Leveraged loans are debt of companies with below investment grade credit ratings. Leveraged loans are typically senior to the company's other debt and are used mainly to fund leveraged buyouts.

CONCLUDING THOUGHTS

EXPANDED PRIVATE FINANCING IS IMPORTANT FOR ADVANCED CAPITAL MARKETS

The means by which companies raise capital have changed over time. As the headlines for Elon Musk's recent misadventures with Tesla suggest, so have the relative advantages of private over public ownership. While the decline in the number of listed domestic companies is well documented, little attention has been paid to the consequences of the growing private equity market and its institutional investor base. As equity owners shifted from households to institutional investors, the importance of private financing as a means for improving the efficiency and profitability of U.S. companies has amplified.

Investing in PE firms has become more popular among institutional investors, despite fading expectations for earning higher-than-market returns from investments in companies restructured by the PE managements. PE firms make extensive use of a rich menu of securities and leverage to shape capital structures that can incentivize more efficient company operations and provide better stakeholder returns.

In addition, corporate managers have sought PE financing to escape the frequent disclosure requirements and costly internal controls aimed at investor protection that come with public listing. Moreover, we see that nationalized companies, such as Saudi Aramco, are reassessing the balance of benefits from listing on U.S. and global stock exchanges against potential disclosure requirements and legal liabilities that may arise. Some corporate managers want shelter from stock price pressure and interference from shareholder activists. In addition, the development and expansion of secondary private equity markets for trading assets among PE firms has benefited PE investors and company managers. Investors may realize their gains sooner, while corporate managers are able to prolong the time for improving corporate strategies and operations before listing on a public stock exchange.

CONCLUDING THOUGHTS

We believe the severe drop in the volume of domestic IPOs and rise of PE financing reflects an ongoing evolution in U.S. capital markets. Such market broadening is a byproduct of financial innovations that occur when companies and investors continuously re-optimize their capital structures.

However, as more companies are owned by PE funds in which households cannot invest, social policy questions about the fairness of maintaining an unequal distribution of investment opportunities need to be addressed. Moreover, legislation that mandates listed companies to meet more social and wealth distribution objectives that are not directly related to the operations of the company, likely will incentivize even more delistings from stock exchanges and exits into private ownership. This, in turn, likely will exacerbate the unequal distribution of investment opportunities and worsen the already skewed distribution of wealth.

Nevertheless, private capital plays an important and growing role connecting financial resources to investment opportunities. IPOs, publicly listed companies, and private equity are all complementary investment vehicles. Each plays a vital role that allows companies more efficient access to capital for improving productivity, boosting long-term growth, and creating better jobs.

ABOUT US

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Private Equity and the Leverage Myth

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KEY FINDINGS

- Why isn't the appropriately leveraged volatility of public companies a reasonable approximation of private equity volatility? The authors look for clues in the public markets where they find no association between volatility and leverage, counter to what financial theory would suggest.
- The evidence suggests that the relationship between leverage and volatility is hopelessly obscured by a variety of confounding effects in both public and private markets.
- This article arrives at the counterintuitive conclusion that private equity volatility is similar to public equity volatility despite its higher leverage. The likely explanation is that privately held companies are inherently less risky and thus able to bear greater leverage.

ABSTRACT

Investors have traditionally relied on mean–variance analysis to determine a portfolio's optimal asset mix, but they have struggled to incorporate private equity into this framework because they do not know how to estimate its risk. The observed volatility of private equity returns is unrealistically low because the recorded returns of private equity are based on appraised values, which are serially linked to each other. These linked appraisals, therefore, significantly dampen the observed volatility. As an alternative to observed volatility, some investors have argued that private equity volatility should be estimated as leveraged public equity volatility, because private equity companies are more highly levered than publicly traded companies. However, this approach yields unrealistically high values for private equity volatility, which invites the following question: Why isn't the appropriately leveraged volatility of public companies a reasonable approximation of private equity volatility? This article offers an answer to this puzzle.

TOPICS

*Private equity, volatility measures**

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Investors struggle to incorporate private equity into mean–variance analysis because they do not know how to measure its volatility. They recognize that the observed volatility of private equity returns is unrealistically low because returns are estimated from appraised values that are serially dependent.[1] This serial dependence arises

[1] For example, Anson (2013, 2016, 2017) finds that the serial correlation in private equity returns extends back three to four quarters. The observed volatility of private equity is also understated

because appraisals rely, in whole or in part, on stale information from events that occurred prior to the valuation date. For example, appraisers may reflect the values of comparable privately held companies that were bought or sold in the past. Or, they may anchor company valuations to their own prior values. Because each period's appraisal incorporates values from prior periods, the returns of an appraisal-based index are artificially smooth, akin to a moving average.

Some have suggested that private equity volatility should be estimated as leveraged public equity volatility, because private equity firms are more highly levered than public firms.[2] But this approach produces unrealistically high values, which is puzzling. Why isn't the relative volatility of private equity related to its relative leverage? Because the public market is considerably less opaque than the private market, we approach this question by looking for clues from the public equity market.

To begin, we show how leverage should affect volatility theoretically. We then document the extent to which private equity violates this theoretical relationship. We also simulate the effect of leverage on volatility, which underscores the disconnect between leverage and private equity volatility. We then turn to the public market in search of clues about the relationship between leverage and volatility. We first sort public firms by their explicit leverage and compare these differences to differences in volatility. We then regress volatility on leverage both across time and cross-sectionally, controlling for a wide range of confounding factors. These regressions fail to show a relationship between leverage and volatility. So, we examine several individual companies anecdotally to gain a better understanding of the apparent disassociation between leverage and volatility. By induction, we extend our findings to private equity and propose an approach for measuring private equity volatility, which, in our view, yields realistic values.

LITERATURE REVIEW

It is common practice for investors to estimate the volatility of private equity buyouts by applying a leverage multiple to public equity volatility. This approach is also espoused widely in the literature:

- L'Her et al. (2016) estimate the net debt-to-enterprise value ratio for buyouts to be 55% on average versus 25% to 30% for public companies. They extrapolate this finding to recommend that public market returns be scaled up to adjust for this leverage differential to compare with buyout performance.
- Stafford (2015) notes that a typical private equity transaction increases debt-to-enterprise value from roughly 30% to 70% and that an investor in public markets would therefore need to invest about double the capital to generate returns similar to those of private buyouts.
- Chingono and Rasmussen (2015) equate private equity buyouts to investments in levered (public) small-cap value companies.
- L'Her, Karthik, and Desrosiers (2017) state that "1.2 should be viewed as a minimum beta estimate for buyout funds," which they attribute to the observation

because it is based on returns net of performance fees, which attenuate upside deviations but not downside deviations. Because standard deviation does not distinguish between upside and downside deviations, performance fees reduce standard deviation but not downside risk, which is what matters. In our analysis, we ignore this effect because it is not that large; we have found in previous research that it misstates standard deviation by about 1%.

[2] Not all privately equity investments are levered. In this article, we focus on buyout funds because they represent the largest segment of the private equity market overall and are of the greatest interest to large, institutional investors. Buyout investors employ more leverage, on average, than public companies.

EXHIBIT 1

Expected and Actual Volatility, December 1996–September 2018 (annualized)

		Expected		Actual	
	Public Equity	Private Equity	Ratio	Private Equity	Ratio
Quarterly	16.3%	32.6%	2.00	9.9%	0.61
Annual	17.8%	35.5%	2.00	15.4%	0.87
Triennial	17.6%	35.2%	2.00	17.0%	0.97

that buyouts have significantly higher leverage multiples than public firms. Axelson, Sorenson, and Stromberg (2014) and Harris, Jenkinson, and Kaplan (2016) make similar assertions with leverage-adjusted beta estimates for buyouts ranging between 1.5 and 2.7.

THE PRIVATE EQUITY LEVERAGE PUZZLE

Private equity firms are highly levered, yet their observed volatility is lower than that of public equity. Even after adjusting for the smoothing effect introduced by appraisals, private equity volatility is significantly lower than the product of public equity volatility and the relative leverage of private equity to public equity.[3]

In principle, equity volatility should scale directly with leverage absent other effects, as shown in Equation (1).

$$\sigma_E = L\sigma_A \tag{1}$$

where σ_E equals equity volatility, L equals leverage (assets/equity), and σ_A equals asset volatility.

Axelson et al. (2013) show that buyout firms have approximately two times more leverage than their public counterparts. Therefore, based on Equation (1) and assuming similar asset volatility between the two groups, we would expect buyout equity's volatility to be roughly two times that of public equity's volatility. This is not what we observe empirically.

Exhibit 1 compares public and private equity volatility over the period December 1996 through September 2018. We use the S&P 500 Index to represent public equity and the buyout segment of the State Street Private Equity Index to represent private equity.[4] We measure the volatility of quarterly total returns for the S&P 500 and quarterly internal rates of return (IRRs) for private equity. To account for the smoothing effect appraisals have on private equity valuations, we also measure the volatility of overlapping annual and triennial returns.[5] The first column reports public equity's volatility, the second set of columns shows the expected volatility of private equity, assuming it is twice that of public equity (given that it has two times more leverage), and the third set of columns reports private equity's actual volatility.

Contrary to the expectation that leverage should have a one-to-one relationship with volatility, Exhibit 1 shows that leverage has no apparent effect on private equity volatility. Actual private equity volatility does increase over longer measurement intervals, which

[3] Other studies, such as Anson (2016), find that de-smoothed private equity returns have higher volatility than public equities. Estimates vary based on the source of private equity data and the specific de-smoothing methodology. We therefore base our conclusions on the volatility of longer interval private equity returns, which we discuss later in the article.

[4] The State Street Private Equity Index captures the pooled internal rate of return each quarter for 3,097 private equity funds in which State Street clients are limited partners. The IRR calculation incorporates quarterly net asset values (NAVs) for each fund as well as daily net cash flows that occurred throughout the quarter. The index captures a total commitment size of $3.1 trillion as of September 30, 2019.

[5] An alternate approach is to estimate the serial correlation statistics of the return series with a regression and reverse engineer the estimated smoothing effects. The approach is often called de-smoothing. Measuring volatility from longer interval returns has the same effect because multi-period returns contain all of the effects of serial correlation. Longer interval returns have the advantage that they represent an actual investment outcome, and they do not require a model to estimate.

EXHIBIT 2

Time Series and Cross-Sectional Panel Regressions of Firm Volatility (Simulated)

	Beta	t-Statistic	R-Squared
A. Time series panel regression of firm volatility on leverage and market volatility			
Leverage	1.01	95.45	0.63
Market volatility	0.45	5.19	
B. Cross-sectional panel regression of firm volatility on leverage			
Leverage	0.99	45.64	0.43

is a function of its serial dependence. But it falls short of public equity volatility, where we would expect it to be twice as large. Therein lies the puzzle.

SIMULATED EFFECT OF LEVERAGE ON VOLATILITY

To support the validity of our expectation from Equation 1 of a one-to-one relationship between leverage and volatility, we simulate the theoretical association of leverage and volatility using the following steps.

1. We form 500 hypothetical new "companies" that each consist of investment in one of the S&P 500 stocks as the sole "asset."
2. Each year, we introduce random amounts of leverage (measured as the asset-to-equity ratio), modeled from a distribution with a mean of 1.5 and a lower bound of 1, to each hypothetical company and record the monthly returns of these levered companies over time.[6] Thus, we have added leverage mechanically to each position in a way that is uncorrelated to all other variables.
3. We record the annual volatility of each new company.
4. We repeat steps 2–3 to generate results for a 28-year period.
5. We run panel time series and cross-sectional regressions (specified later).

The results of this experiment are shown in Exhibit 2.

The leverage coefficient of 1.01 in Exhibit 2, Panel A, is consistent with a one-to-one relationship between leverage and volatility through time. When a given company's leverage goes up from one year to the next, its volatility increases proportionally. The leverage coefficient of 0.99 in Panel B reveals the analogous one-to-one relationship in the cross-section of companies. In any given year, the synthetic companies with higher leverage have higher volatility than those with lower leverage.

These simulations lend unambiguous credence to the notion that volatility should scale directly with the degree of leverage. We are therefore left with the puzzle that even though private equity has twice the leverage of public equity, its volatility adjusted for smoothing is no greater than public equity volatility. To resolve this puzzle, we turn to the public market to look for clues about the empirical relationship between leverage and volatility.

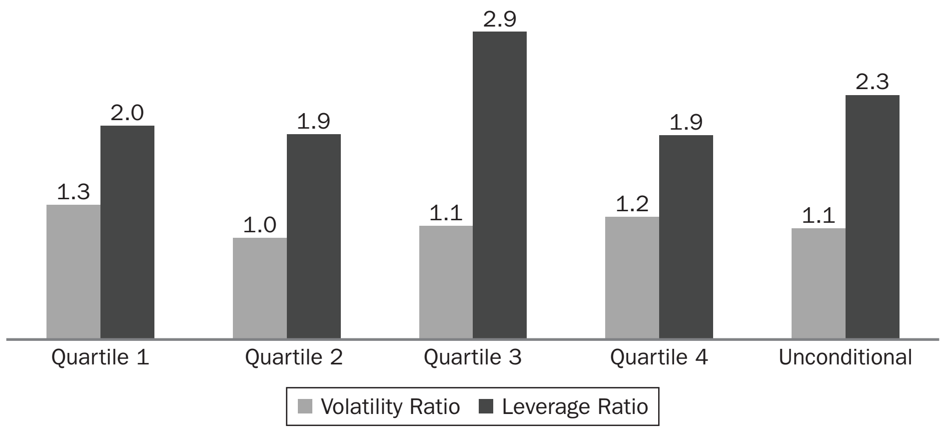
THE EFFECT OF LEVERAGE ON VOLATILITY IN THE PUBLIC EQUITY MARKET

We base our analysis of leverage and volatility within the public equity market on the constituents of the S&P 500 from March 2001 through October 2018. Within groups of stocks with similar levels of fundamental risk, we form capitalization-weighted portfolios according to leverage. In this initial experiment, we use borrowing costs as a proxy for fundamental risk. Specifically, each month we form 16 portfolios by 1) sorting stocks into quartiles according to their borrowing cost[7]

[6] We draw from a log-normal distribution with a mean of 0.50 (representing the debt-to-equity ratio) and a lower bound of zero. Then we add 1 to the simulated values to put them in units of assets-to-equity.

[7] We proxy borrowing costs with Worldscope item 08356, "Interest Rate - Estimated Average," which is computed as interest expense divided by the sum of short-term debt, current long-term debt,

EXHIBIT 3
High-Leverage Portfolios versus Low-Leverage Portfolios with Borrowing Costs as Control Variable



(a proxy for the fundamental risk of their underlying businesses, irrespective of the degree of leverage) and then 2) within each fundamental risk quartile, sorting stocks into quartiles according to their leverage (asset-to-equity ratio). We record the monthly returns of these 16 portfolios over the full sample.

We next evaluate the impact of leverage while controlling for differences in fundamental risk across firms. Specifically, within each risk quartile, we compare the volatility of the portfolio of firms with the highest leverage to the volatility of the portfolio of firms with the lowest leverage. These ratios are reported in Exhibit 3 (light gray bars). For comparison, we also show the ratio of leverage for the same portfolios (dark gray bars). For example, on average, within the universe of firms with the lowest fundamental risk (quartile 1), the most levered firms have two times more leverage than the least levered firms, and the volatility of their returns is 1.3 times greater.

If leverage functions as expected, the volatility ratios should be of similar magnitude to the leverage ratios. However, for every quartile of fundamental risk, they are about half of what we would expect. Moreover, the rightmost bars report results for portfolios that ignore fundamental risk (they are single-sort portfolios formed on leverage alone). If borrowing costs effectively proxy for fundamental risk, we would expect a stronger relationship between leverage and volatility for the double-sort portfolios than for the single-sort portfolios. Again, this is not the case.

We repeat the experiment using two alternative proxies for fundamental risk: level of cash flows and volatility of cash flows.[8] All else equal, we presume that companies with higher and more stable cash flows relative to their book value are inherently less risky. Exhibit 4 summarizes the average volatility and leverage ratios of the double-sort portfolios. Again, we find that the volatility ratio of highly levered firms to firms with low leverage does not come close to matching the ratio of their respective amounts of leverage.

Next, we resort to regression analysis to determine if differences in volatility correspond to differences in leverage. Our data comprise the S&P 500 constituents

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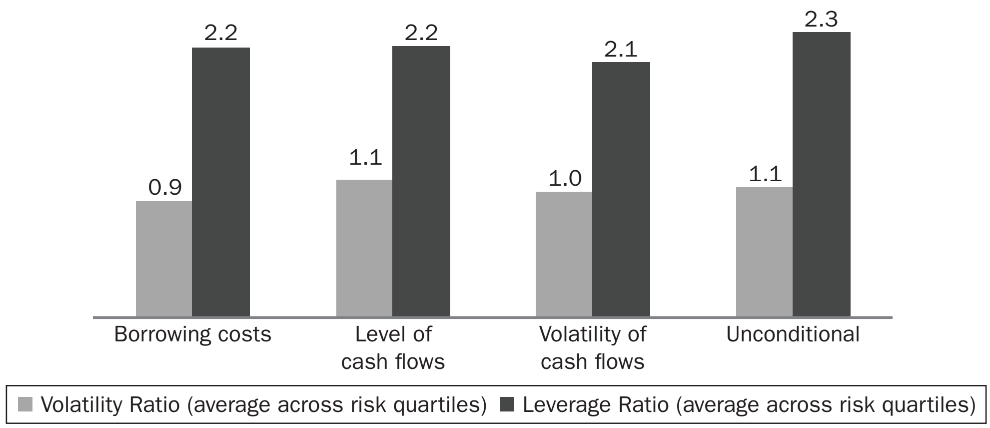
and long-term debt. We exclude observations with zero borrowing costs, negative borrowing costs, and those that reside in the 5% right tail of the distribution. All data are from the Worldscope database obtained via Datastream.

[8]We compute the level of cash flows as cash flow from operations (Worldscope item 18310A) divided by the book value of assets (Worldscope item 03501A). We compute the volatility of cash flows as the rolling five-year volatility of the level. For both variables, we exclude observations that reside in the 5% right tail of their respective distribution. All data are from the Worldscope database obtained via Datastream.

EXHIBIT 4

High-Leverage Portfolios versus Low-Leverage Portfolios with Level and Volatility of Cash Flows as Control Variables



from January 1990 through December 2017. We exclude firms with no leverage as well as annual equity volatilities that reside in the 5% right tail of the distribution.[9] We run two panel regressions: one that captures the time series relationship between equity volatility and leverage and another that captures the cross-sectional relationship. We use the natural log of each variable in order to transform equity volatility into a linear combination of leverage and asset volatility.

For the time series panel regression, we include a constant for each stock (firm fixed effects) and use market volatility to represent asset volatility, as shown in Equation (2).

$$\ln \sigma_{i,t} = \beta_1 \ln L_{i,t-1} + \beta_2 \ln \sigma_{M,t} + \text{firm fixed effects} + u_{i,t} \tag{2}$$

where $\sigma_{i,t}$ equals volatility of firm i's equity over calendar year t, $\sigma_{M,t}$ equals volatility of the market (S&P 500) over calendar year t, and $L_{i,t-1}$ equals leverage (assets/equity) of firm i at end of year $t - 1$.

For the cross-sectional panel regression, shown in Equation (3), we include a constant for each year (year fixed effects).

$$\ln \sigma_{i,t} = \beta_1 \ln L_{i,t-1} + \text{year fixed effects} + u_{i,t} \tag{3}$$

where $\sigma_{i,t}$ equals volatility of firm i's equity over calendar year t and $L_{i,t-1}$ equals leverage (assets/equity) of firm i at end of year $t - 1$. In both regressions, we adjust the standard errors to account for possible correlations across firms.

Exhibit 5 shows results from the time series panel regression. In addition to the baseline regression described earlier, we also report leverage coefficients for three variants. First, we control for changes in the average amount of leverage in the market from year to year. Second, we exclude financial and utility companies, recognizing that their capital structure may differ from other sectors. Third, we exclude the Global Financial Crisis period of 2008–2009, recognizing that extraordinary events during this period may distort our results. Yet, in each case, we are unable to reject the null hypothesis that the leverage coefficient equals zero.

Exhibit 6 shows the cross-sectional results. In this case, we introduce four variants, each designed to control for the underlying volatility of a company's assets.

[9] We remove the tails to prevent firms that are experiencing extreme volatility driven by crisis or extraordinary circumstances from having undue influence on our results.

EXHIBIT 5
Time Series Panel Regression

	Beta	t-Statistic	R-Squared
Leverage	0.11	1.41	0.50
Market volatility	0.39	5.41	

	Leverage Beta	t-Statistic	R-Squared
Controlling for market leverage	0.08	1.01	0.52
Excluding financial and utility companies	0.10	1.51	0.50
Excluding Global Financial Crisis (2008 and 2009)	0.10	1.42	0.50

EXHIBIT 6
Cross-Sectional Panel Regression

	Beta	t-Statistic	R-Squared
Leverage	–0.03	–1.40	0.25

	Leverage Beta	t-Statistic	R-Squared
Excluding financial and utility companies	–0.03	–1.24	0.24
Controlling for firm-specific borrowing costs	–0.02	–0.93	0.26
Controlling for level of firm-specific cash flows	–0.07	–1.55	0.31
Controlling for volatility of firm-specific cash flows	–0.08	–2.49	0.31

First, we again exclude financial and utility companies from the analysis, as we did with the time series regression. We also control for the three measures of fundamental risk introduced in Exhibits 3 and 4.

Again, we see no evidence that volatility scales with leverage. The coefficients are not only insignificant, they are in the wrong direction as well. Given this evidence of the lack of association between leverage and volatility in the public equity market, it is unsurprising that private equity volatility shows no correspondence to leverage. In the next section, we examine two companies in search of anecdotal evidence to explain the lack of correspondence between leverage and volatility.

ANECDOTAL EVIDENCE

The following analysis highlights why it is difficult to capture the expected relationship between leverage and volatility.

Exhibits 7 and 8 depict the relationship between leverage and volatility for two companies: Motorola and Clorox. They illustrate that volatility can be highly time-varying even during periods where leverage is relatively stable. These examples reveal why a time series regression cannot detect a relationship. Even surrounding dramatic changes in the leverage of both firms, we do not observe commensurate changes in volatility. In fact, these two companies both increased their leverage following steep *declines* in volatility. The degree of leverage is an endogenous, firm-specific decision; it stands to reason that low-risk companies incur more leverage, all else equal, precisely *because* they are more able to do so.

Exhibit 7 plots the relationship between the rolling one-year volatility of Motorola shares against its rolling asset-to-equity ratio. The volatility of Motorola stock varied

EXHIBIT 7

Leverage and Volatility: Motorola

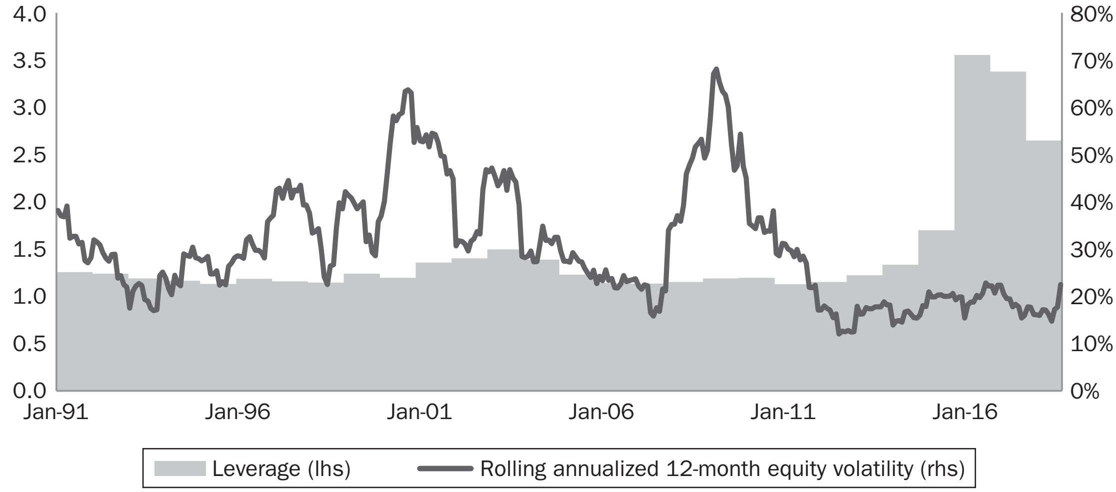
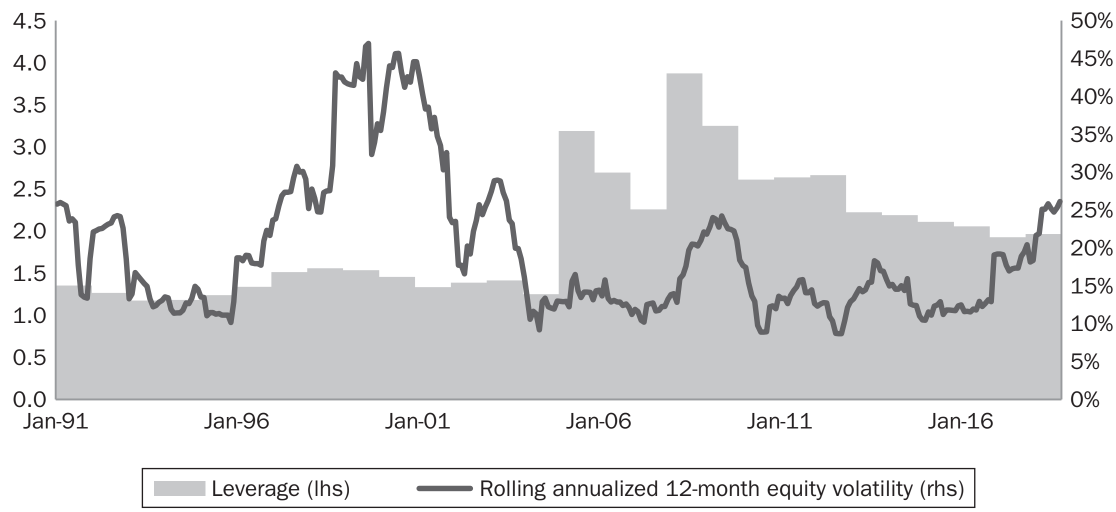


EXHIBIT 8

Leverage and Volatility: Clorox



widely between 1991 and 2013, reaching a high of 70% during the Global Financial Crisis and a low of approximately 20% in recent years. Yet, its leverage hardly changed at all during this period. Furthermore, when the company tripled its leverage between 2015 and 2018, there was no apparent impact on its volatility. There does not appear to be any discernible relationship between the leverage and volatility of Clorox, either, as shown in Exhibit 8. For example, the firm has gradually reduced its leverage since 2011 only to see its share price volatility rise.

Next, we explore some of the fundamental factors that hide the effect of leverage on volatility. We find that financial leverage is a poor indicator of effective leverage for firms with substantial non-debt obligations. Moreover, we find that fundamental risk is unobservable and difficult to proxy. We illustrate these issues in Exhibit 9 by analyzing six companies: Lincoln National, AIG, GM, McDonalds, GE, and Cummins. Our observations are as follows.

EXHIBIT 9

Average Financial Attributes of Select Companies, 1990–2017

	Financials		Industrials		Consumer Discretionary	
	Lincoln National	AIG	Cummins	GE	General Motors[8]	McDonalds
Leverage	1.02	1.17	1.21	2.11	1.49	2.01
Volatility	38%	65%	36%	25%	27%	21%
Fundamental risk						
Credit worthiness						
Borrowing costs[1]	7.1%	6.1%	7.0%	4.3%	7.2%	5.4%
Profitability						
Level of cash flows[2]	26%	14%	32%	33%	39%	25%
Volatility of cash flows[3]	15%	67%	17%	13%	8%	65%
Dividend yield	2.9%	0.6%	2.2%	2.9%	2.5%	1.9%
Other obligations						
Non-debt liabilities						
Deferred income + reserves[4]	40%	52%	1%	0%	1%	0%
Pension obligations[5]						
Plan size[6]	0.01	0.01	0.33	0.09	0.59	n/a
Funding status[7]	89%	73%	91%	82%	80%	n/a

[1]Estimated as interest expense/(ST debt + current LT debt + LT debt).

[2]Measured as cash flow from operations normalized by book value of assets.

[3]Volatility of normalized cash flows (note 2).

[4]Measured as (insurance reserves + deferred income)/book value of assets.

[5]Pension obligation data begin in 2002.

[6]Measured as fair value of plan assets divided by book value of assets.

[7]Measured as fair value of plan assets divided by projected benefit obligation.

[8]Data for General Motors (GM) begin in 2009. If we measure McDonalds' leverage and volatility for the overlapping sample, the comparison is even starker: McDonalds' leverage is 3.33 (compared with GM's 1.49) and its volatility is 13% (compared with GM's 27%).

- Compared with the other companies, Lincoln National and AIG have relatively low leverage but high volatility. This is because they carry a lot of non-debt liabilities in the form of insurance reserves.
- Within the consumer discretionary sector, GM has lower leverage than McDonalds but higher volatility. This may reflect the substantial size of its pension fund.
- Within the industrial sector, GE has higher leverage than Cummins but lower volatility. It could be the case that GE is fundamentally safer. It has significantly lower borrowing costs; however, its profitability measures are similar to Cummins' profitability measures.

Exhibit 9 highlights but a few of the countless factors that can affect a firm's inherent risk and thereby complicate the relationship between its leverage and volatility. We submit that this complexity renders it difficult, if not impossible, to control systematically for these factors in public markets let alone in private markets where the required data are scarce.

CONCLUSION

The observed volatility of private equity is unrealistically low compared with the volatility of public equity. Investors have recognized that this apparent low volatility arises from the fact that valuations are based on appraisals that are anchored to prior period valuations. This practice has the effect of smoothing returns. Investors can offset this effect by estimating volatility from rolling longer-interval returns, such as annual or triennial returns. With this adjustment private equity volatility is similar to public equity volatility. Of course, it will differ by sub-categories within private equity.

Be that as it may, many investors believe that private equity volatility should be much higher than public equity volatility because private equity is much more highly levered than public equity. We showed both theoretically and by simulation that volatility should scale directly with leverage. However, it turns out that private equity volatility, adjusted for smoothing, is approximately equal to public equity volatility despite its much greater leverage, which presents a puzzle. To address this puzzle, we resorted to analyzing the relationship between leverage and volatility in the public market, where data are easier to come by. Using sorts as well as time series and cross-sectional regressions, we were unable to detect any relationship between leverage and volatility that conforms even remotely to the theoretical relationship of leverage and volatility. To put it bluntly, our results were robustly insignificant.

Next, we sought to understand why we could not uncover a statistical relationship between leverage and private equity. First, we reviewed the time series properties of leverage and volatility for two companies. We discovered that leverage is often stable for long periods whereas volatility is highly time varying. This is the proximate cause of why we cannot detect a time series relationship.

We next reviewed the financial conditions of several companies. This analysis revealed that companies have several sources of implicit leverage, which obscures the effect of explicit leverage. We also observed that asset stability differs significantly across companies for a variety of business reasons, which confounds the effect of explicit leverage on volatility. This anecdotal evidence suggests to us that the relationship of leverage and volatility is hopelessly obscured by a variety of confounding effects.

We assert that the volatility we estimate from longer-interval private equity returns (to offset the effect of valuation smoothing) is the correct approximation of private equity volatility because it approximates the actual distribution of outcomes realized by private equity investors over longer horizons. When applied to the data, this approach yields the stubborn conclusion that private equity volatility is similar to public equity volatility despite its higher leverage. Why is this the case? It could be that buyout fund managers prefer to invest in companies whose underlying business activities are inherently less risky and can therefore bear higher leverage, which increases profits. Whatever the reason, our findings debunk the widespread misconception that private equity has higher volatility than public equity volatility as a result of its higher leverage.

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Capital Structure and Leverage in Private Equity Buyouts

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Private equity buyouts depend on debt financing. In fact, the practitioner and academic research literature generally refers to buyouts as *leveraged* buyouts, or LBOs, precisely because of the important role of debt in funding such transactions. But in contrast to the literally thousands of empirical studies over more than half a century that have focused on the capital structure of *public* companies, there are remarkably few large-scale empirical studies of the role that leverage plays in buyouts—its effects on the risk, returns, incentives, and other basic characteristics of LBOs. The relative scarcity of research on PE capital structure is attributable mainly to the lack of widely available financial data on buyout deals or other aspects of PE capital structures. The few studies that we now have rely mainly on comparatively small proprietary datasets or are limited to a subset of more transparent transactions, such as public-to-private buyouts or financings that include publicly traded bonds.

With this in mind, we recently conducted a study designed to shed light on the various kinds of debt now used to finance buyout transactions, and to provide some current insights on the profitability and leverage of such transactions using newly available data. In the pages that follow, we summarize the current state of knowledge on buyout financing from both a theoretical and empirical perspective with the aim of informing academics, practitioners, and policymakers.

We begin by providing an overview of the different ways that debt can enter into the financing of buyout transactions—not only at the level of the portfolio companies, but also at the level of the funds or investors in those funds. Then, following a brief discussion of capital structure theory, we use the existing literature to address several specific questions of importance to both practitioners and policymakers: What accounts for the cyclical nature of private equity? How does leverage affect the alignment of interests and incentives between the general partners (GPs) and the limited partners (LPs) who provide the bulk of the equity capital in buyouts? How has PE performed as an investment and how is that performance linked, if at all, to leverage? We close by presenting the findings of our recent analysis of new deal-level data.

Before turning to the detailed analysis, we provide a summary of our main conclusions:

• Debt enters into the PE buyout ecosystem in a variety of ways. Along with direct borrowing by the individual portfo-

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lio company acquired in the buyout transaction, the buyout funds themselves are increasingly borrowing using either LP commitments or equity interests in the underlying companies as collateral. In addition to these borrowings, private equity GPs and LPs have been raising debt independent of the funds or portfolio companies. In this way, buyout capital structures have been evolving over time to incorporate incremental leverage as the debt markets and PE firms have created new ways to attract risk capital.

• Leverage decisions made as part of PE buyout deals depend, at least to some extent, on the characteristics of those deals. In particular, finance theory predicts that the deal partners (typically employed by the GP) will trade off the benefits of debt with the expected costs. Potential benefits include a greater debt tax shield and stronger management incentives to generate cash flow. Potential costs arise mainly from the increased financial risk, including the risk of and costs associated with bankruptcy, as well as other operating and financial frictions. The leverage-supporting characteristics of deals vary across industry and geography, and over different time periods, though to a lesser extent than both academics and practitioners appear to believe. All of which suggests that many of the same forces that shape capital structure in public companies are at work in PE buyouts.

• Leverage makes possible PE firms' concentration of ownership, which in turn is expected to improve monitoring of operating performance and managerial decision making. Along with more disciplined capital spending, a number of studies suggest that PE has a comparative advantage in managing high leverage and its potential costs—one that effectively enables PE-backed firms to take on higher levels of debt than comparable public companies.

• Although reducing potential agency conflicts between GPs and their operating managers and creditors, the typical PE investment structure introduces conflicts of interest and incentives between GPs and LPs that can, at least in part, be managed by contractual arrangements.

• The most recent and comprehensive research suggests that PE funds generate superior risk-adjusted returns compared to public equity investments. This implies that even after their fees, GPs have created value for LP investors through a number of interrelated sources including better governance, operational engineering, multiple expansion and leverage. While it is difficult to empirically characterize risks in private investments, studies suggest that PE firms have comparative advantages that allow them to mitigate the impact of leverage on financial risks faced by other investors.

• Studies of PE capital structures and return and risk outcomes continue to confirm the highly cyclical nature of private equity activity, suggesting that institutional features combined with macroeconomic cycles are to some degree hardwired into the industry.

• Using a new sample of thousands of individual PE buyouts transacted over more than three decades, we find that, in almost all sectors, the vast majority of deals were profitable when compared to public market returns. We also document that the relationship between leverage and returns depends on the way leverage is measured. When leverage is measured as the ratio of net debt to total enterprise value, we observe a strong positive relationship with returns, which is consistent with a risk-return trade-off. High debt-to-value deals tend to target larger, established companies with low growth rates that can provide predictable cash flows to service debt. Entry EBITDA multiples tend to be lower in such cases, and the companies pay down more debt than average. But when we measure leverage as net debt divided by EBITDA (typically referred to as "the leverage ratio"), we find a weakly *negative* relationship with returns. Deals with high leverage ratios tend to target companies with faster growing earnings and higher operating margins. Deals with high leverage ratios are associated with above-average entry EBITDA multiples, but do not appear to be riskier than deals with low leverage ratios.

"PE firms have comparative advantages that allow them to mitigate the impact of leverage on financial risks faced by other investors."

Overview of Private Equity and the Use of Debt

PE funds are typically structured as closed-end private partnerships with a life span of ten or more years. The partnership is made up of limited partners (LPs) and general partners (GPs), each of which have rights and responsibilities as governed by their partnership agreement. The LPs are institutional and high-net-worth individual investors who provide the majority of the capital to the partnership. The GP manages the capital, deciding when it is called, what it is used for, and how and when it is returned to the LPs subject to provisions in the partnership agreement. The GPs typically charge a management fee on the committed or the invested capital and earn a share of the profits, known as "the carry," though typically only after a preferred return (or hurdle rate) is realized by the LPs. The LPs' liability risk is limited to the capital they contribute. The GP role is typically managed by professional PE fund managers. These managers protect themselves from liability, at least in part, by not serving directly as the GPs, but instead as shareholders of the corporation that serves as the GP.[1]

As the PE industry has evolved over the last half century, so too has the use of debt. Since the earliest days of leveraged buyouts, PE managers have used debt financing, "multiple arbitrage," and operational improvements combined with more effective governance as the primary drivers of value creation. PE typically targets gross equity returns in excess of 20%, which is higher than cost of equity capital for many strategic acquirers who compete with PE to own assets. LP suppliers of equity have illiquid claims and cede control to the GP when capital is called or returned. Moreover, the GP has a much higher concentration of ownership, and much of their annual compensation comes from the returns.[2] The resulting high cost of private equity, together with the perceived incentive benefits of concentrating ownership, pushes GPs to use as much leverage as they can confidently support, with the goal of minimizing their blended cost of capital, and so enabling them to compete more effectively for assets. Typical uses of debt proceeds by PE-backed companies are similar to those of public companies, including the funding of M&A transactions, the refinancing of existing debt, and the recapitalization of a company's balance sheet. Traditional PE financings have most frequently included issuances in both the syndicated bond and bank markets. However, as financing alternatives evolve, PE remains at the vanguard pursuing investment opportunities for which traditional sources of capital may have once been too expensive.

In the 1970s and 1980s, PE-backed companies were among the earliest and most frequent issuers of high-yield bonds, which were used mainly to fund their takeover efforts. To compensate investors for their higher chance of issuer default, high-yield bonds offer higher interest rates and sometimes investor-friendly structural features. Until the 1980s, traded high-yield bonds were simply the outstanding bonds of "fallen angels," once investment grade companies that had experienced credit rating downgrades. Drexel Burnham and other investment banks launched the modern high-yield market in the 1980s by selling new bonds from companies with non-investment grade ratings to fund mergers and leveraged buyouts.

Many of the issuers of high-yield bonds continue to be companies backed by private equity. Today's high-yield bonds typically take the role of junior debt capital—subordinate to senior secured loan debt but senior to the PE fund's equity investment. High-yield bond investors include mutual funds, pension funds, insurance companies and arrangers of instruments that pool debt securities (as in collateralized debt obligations, or CDOs). High-yield bonds offer investors the potential for diversification, higher current income, capital appreciation, and longer duration. The size of the global high-yield corporate bond market was estimated to be in excess of $2.8 trillion at the end of 2019, of which some $2.5 trillion had been issued by U.S. industrials.[3]

With the advent of "market flex" language in the syndicated loan market during the Russian debt crisis of the late 1990s, loan syndications emerged as a full-fledged capital markets alternative for PE financings.[4] Leveraged loans, which are loans with non-investment grade ratings, are typically senior secured debt instruments, either first or second lien. They also typically provide floating-rate coupons, may or may not have covenant provisions, and usually have shorter duration than bonds.

The syndicated leveraged loan market, which developed as an offshoot of the investment grade loan market, provides a way for borrowers to access banks and other institutional capital providers of loans in a less expensive and more efficient form than traditional bilateral credit lines. As a result, by the late 1990s many PE-backed companies were relying heavily on the leveraged loan market to fund their portfolio companies. Leveraged loan investors include banks, finance companies, institutional investors (typically using structured vehicles such as collateralized loan obligations, or CLOs), loan mutual funds, and ETFs. The Bank of England estimates the current size of the global leveraged loan market at more than $2 trillion, a rise of more than 100% since 2007. U.S. leveraged loans outstanding at the end of 2019 amounted to over $1.2 trillion, with the remainder mostly denominated in euros.[5]

Away from the syndicated loan markets, private credit alternatives expanded dramatically during the post-financial crisis period. In the wake of the financial crisis, many financial institutions faced the need to reduce leverage, thanks in part to higher capital reserve requirements and increased regulation that forced many banks to curtail traditional bank

1 See Josh Lerner, Ann Leamon, and Felda Hardymon, (2012), *Venture Capital, Private Equity, and the Financing of Entrepreneurship*, Wiley Press.

2 Anecdotal evidence suggests that while the gross internal rate of return private equity managers typically underwrite varies with changes in the market cycle and dynamics, typical estimates range from 15%-30%, with 20%-25% most frequently sighted. Managers have generally tended toward the lower end of the range in the post-financial crisis period.

3 "U.S. Corporate Debt Market: The State of Play in 2019," *S&P Global Market Intelligence*.

4 "Leveraged Commentary & Data (LCD): Leveraged Loan Primer," *S&P Market Intelligence*.

5 "U.S. Corporate Debt Market: The State of Play in 2019," *S&P Global Market Intelligence.*

loan lending. As a result, alternative sources for risk capital stepped into the void, developing a range of private credit structures to meet the growing capital needs of companies, particularly in the middle market. Faced with an historically low interest rate environment, institutional investors have increased allocations to private credit.[6] Private credit assets under management (AUM) exceeded $767 billion in 2018, more than three times the amount in 2008. Much of that expansion can be attributed to supply-side growth driven by PE-backed borrowers. While typically more expensive than a bank or syndicated loan alternative, private credit capital has certain advantages over traditional market alternatives. Notable among them are quick and efficient access for middle-market companies where banks are lending less; fewer counterparties; less regulation and potentially higher leverage levels; the tendency for lenders to hold the loans until maturity; and less public visibility. Despite the emergence and significant growth of private credit in the post-financial crisis decade, the syndicated bank and bond markets continue to be the largest sources of PE debt financing, particularly for the largest, most complex, and multinational financings in which the relative size, liquidity, and sophistication of the syndicated markets continue to be most important.

As the depth and breadth of credit markets have expanded with investor appetite, innovations have followed. Figure 1 depicts various layers of debt that have emerged and the Appendix provides more detail. The emergence of holding company debt in the early 2000s was one innovation. Holding company (HoldCo) debt, which is issued above the operating company (OpCo) level, is junior in priority of repayment, has a junior collateral claim to all debt at the OpCo, and is typically non-cash pay because it is subject to restricted payment provisions of OpCo debt. The primary role of HoldCo debt has been to provide a mechanism for adding incremental debt in a transaction beyond what is accessible at the OpCo.

From the "bottom-up" perspective of OpCo creditors, HoldCo debt behaves essentially as equity and has minimal impact on the cash flow and creditworthiness of the operating company. While holding company debt is generally riskier than operating company debt, often holding only a pledge against the underlying equity as collateral, it can be priced to meet investor demand for yield in robust markets. At the same time, from the "top-down" perspective of private equity, HoldCo debt behaves very similarly to OpCo debt; it can be used to reduce the size of the equity investment while increasing the risk of the residual equity. Although more expensive and riskier than OpCo debt, it is cheaper than equity capital. Not all market conditions support HoldCo debt financings; it becomes accessible only when investor risk appetites are high and credit markets are robust.

Securitized markets have also developed over the last two decades, spurring further innovation and access to capital for private equity.[7] Securitized debt is a form of financing commonly used by companies to raise debt proceeds with the backing of illiquid assets on their balance sheet. Securitized financing requires the creation of a special purpose vehicle (SPV). Effectively a trust that is separate from the operating company, the SPV provides legal isolation of the assets from the original holder of the assets. After receiving the assets from the operating company, the SPV then issues securities backed by the assets of the trust and delivers the proceeds to the operating company. The interest and principal on the securities are paid from the cash flows that arise from the trust assets; the operating company effectively "rents" the assets back from the SPV.

Because the debt issued by the SPV is nonrecourse to the originator, an important benefit of securitized debt is that the credit rating of the debt is based on the SPV's assets rather than the originator's cash flow and assets. The proceeds raised from the sale of the securitized assets are returned to the operating company, thereby enabling illiquid assets of the originator to be turned into cash.

Although securitized financings are commonplace for financial institutions—which use them to finance mortgages or credit card receivables—one of the first times it was used by private equity was during the buyout of Hertz in 2005 by The Carlyle Group. In the case of buyouts, the PE backer is able to raise more debt at lower cost than a traditional financing structure would allow. The concept of a SPV structure is frequently used in commercial mortgage-backed securitizations as well, and was also co-opted by private equity in the form of an OpCo/PropCo structure to finance buyouts of companies with substantial real estate assets on their balance sheets.[8] Whole business securitization structures have also been used by franchise businesses when financing PE-backed acquisitions.[9]

Fund-Level Debt. In a more recent development, the advent of *fund-level* debt has been adopted by private equity. In the case of fund-level debt, the lenders can look either to the

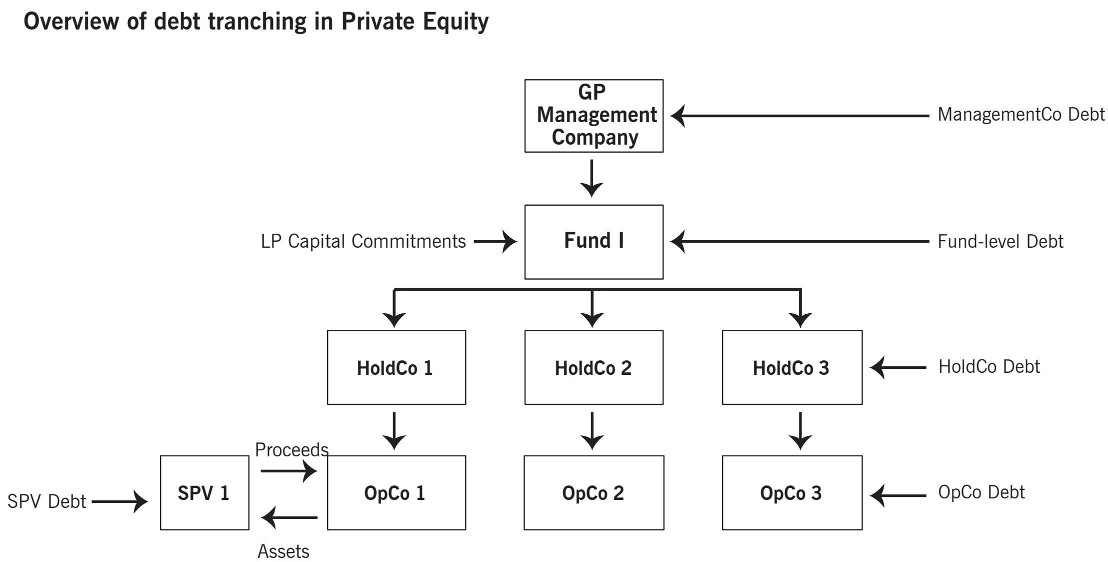
6 Shawn Munday, Wendy Hu, Tobias True, and Jian Zhang, (2018), "Performance of Private Credit Funds: A First Look," *The Journal of Alternative Investments*, 21(2), 31-51.

7 Anil Shivdasani and Yihui Wang, (2011), "Did Structured Credit Fuel the LBO Boom?" *Journal of Finance*, 66(4), 1291-1328.

8 For example, the Toys "R" Us LBO of 2006.

9 For example, the Dunkin' Donuts LBO, 2007.

Figure 1
Overview of Debt in Private Equity



unfunded capital commitments of the LPs or to the underlying equity collateral invested in companies across the fund's portfolio for collateral. In the case of unfunded capital commitment, lenders underwrite the LP credit risk, which in many cases is considered investment grade. In the case of fund-level loans with pledges of collateral from funded commitments, the risk of illiquid equity investments in private companies is often considered non-investment grade and is quite high.

While the adoption of fund-level debt is a relatively new phenomenon in private equity, it has long been used in private credit to enhance LP returns. Business Development Companies (BDCs) have for many years benefited from access to SBIC-guaranteed debt at the fundlevel. Other private credit funds have access to loans at the fund level, often in the form of subscription lines (also referred to as "capital-call" or "wireline" facilities). PE managers can use such subscription lines to facilitate less frequent capital calls from limited partners. These subscription lines typically have to be repaid somewhere in the 30-day to one-year timeframe but can be reborrowed.

Some PE fund managers use fund-level leverage to act as leverage above and beyond what may be efficient (or allowed) at the portfolio company, thereby increasing internal rates of return at the expense of a (modest) reduction in multiples of invested capital. While the effects of fund-level leverage are straightforward when fully disclosed, some ambiguity exists in reporting standards as a result of the less than consistent disclosure of fund-level returns on both a before- and after-fund-level leverage effects basis.[10]

Management Company-Level Debt. More recently, GPs have borrowed loans or issued bonds at the management company level to finance their operations. Management Company (ManagementCo) debt can be used to provide incremental leverage on underlying investments of the fund, seed/acquire new investment strategies, compensate employees, or achieve other general corporate purposes. Lenders and creditors often look to the cash flows of the ManagementCo or personal guarantees of the shareholders of the management company for credit support. Loans at the management company-level are traditionally rated investment grade and funded by large banks and financial institutions. Both secured and unsecured investment-grade bond issuances have been syndicated by the management companies as well. ManagementCo debt effectively acts like any other corporate debt of a financial services company.

A Brief Overview of Capital Structure Theory

The most basic question about leverage in private equity is this: Why do PE buyouts have substantially higher leverage than similar public companies? If the optimal, or value-maximizing, capital structure is indeed a higher level of debt, why

10 See James F. Albertus Matthew Denes (2020), "Private Equity Fund Debt: Capital Flows, Performance, and Agency Costs," SSRN working paper 3410076.

don't public companies operate with more debt? Alternatively, if public company capital structure is on average optimal, doesn't that imply that PE deals are overleveraged and excessively risky?

To provide a framework for answering these and other related questions, we start with an overview of capital structure theory based on the traditional literature that focuses on public companies. This overview will serve as a basis for understanding what may be relevant for private companies and, in particular, the PE buyout transactions that we focus on later. We end this section with an overview of capital structure theory that is related specifically to private equity.

Classic Theory

In the classic trade-off theory of capital structure, companies choose an optimal level of debt based on the tax shield provided by the deductibility of interest payments and the frictions associated with high levels of debt such as higher expected bankruptcy costs. The optimal capital structure is determined in a static equilibrium as the point where the tax benefits of higher debt are just offset by the marginal expected costs of greater frictions.[11]

The trade-off theory predicts that the optimal capital structure decision should be largely the same for private and public companies with similar firm characteristics and financial conditions. As a consequence, the trade-off theory can explain changes in optimal capital structure only to the extent that the difference in ownership structure between public and private companies affects either the tax shields or financial frictions associated with debt.

While higher debt levels result in a greater tax shield, PE-backed companies face much the same tax policies as public companies; and large public companies, thanks to their global operations, often have more sophisticated tax avoidance opportunities that may be unavailable to smaller private companies. If anything, then, corporate tax incentives are likely to work to promote the acquisition of small and mid-sized firms by larger public firms.

In short, even if taxes play an important role in determining optimal capital structure, they are likely to play a relatively modest role in explaining why PE buyouts have more debt. And so if the classic trade-off theory is to explain why buyout deals have high leverage, there must be differences in other frictions that are affected by debt financing. As we discuss later, PE-owned companies have reasons to operate with higher leverage as well as advantages over public companies in managing the expected costs of financial distress that have nothing to do with corporate taxes.

The strongest challenger to the static trade-off theory is the so-called "pecking order" theory, which predicts that companies will choose internal over external funds whenever possible; and when forced to raise outside capital, they will choose debt over equity to minimize the "information costs" arising from information asymmetries between managers and the market. In particular, outside investors in companies proposing new securities offerings worry about a "lemons problem" and price-protect themselves by reducing the value of the firm when the offerings are announced. Because the lemons problem is greater for shareholders than bondholders, issuing equity is generally the most costly and hence least desirable way to raise capital.

Other research has focused on the possible effects, negative as well as positive, of capital structure and leverage ratios on managerial incentives to maximize efficiency and value. Most important for our purposes is Michael Jensen and William Meckling's seminal paper[12] that presented the theory of "agency costs" associated with raising and operating with outside equity. More specifically, Jensen and Meckling showed how the combination of information asymmetry and agency conflicts between managers and outside shareholders over things like the optimal size and diversification of public companies effectively reduces their value. Heavy debt financing, as Jensen and Meckling noted at the end of their article, has significant potential to manage agency conflicts by concentrating ownership and minimizing the need to rely on outside capital.

The theory of agency costs sheds light on an important fundamental difference between private and public ownership. Whereas the PE buyout investors typically take a controlling interest in a company, giving them full control of the board and the power to hire and fire management—which they often exercise—the ability of public shareholders to reform companies that fail to serve their interests generally depends on costly interventions by the market for corporate control, with its threat of takeover, and other forms of shareholder activism.

What's more, especially in large, mature companies, shareholder activists often exert pressure to pay out excess (equity) capital and operate with higher leverage ratios, with the aim of discouraging corporate overinvestment. But in PE-controlled companies, as we just saw in highly lever-

11 Franco Modigliani and Merton H. Miller, (1958), "The Cost of Capital, Corporation Finance and the Theory of Investment," *The American Economic Review*, 48(3), 261-297. See Stewart Myers, (2001), "Capital Structure," *The Journal of Economic Perspectives*, 15, 81-102, for a detailed discussion.

12 Michael C. Jensen and William H. Meckling, "Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure," *Journal of Financial Economics*, 3 (1976) 305-360, Q North-Holland Publishing Company.

aged public companies, the high leverage not only spurs the search for efficiencies and disciplines capital spending, but plays a perhaps still more important role: facilitating the concentration of ownership that enables PE companies to acquire and maintain full control over their portfolio companies.

Nevertheless, as we also discuss below, if this concentration of control in the hands of the firm's largest investor works to minimize the cost arising from the agency conflict between managers and owners, the structure of PE funds generates a new agency relationship between the GPs and their LPs that gives rise to new frictions.

Capital Structure Theory as Applied to Private Equity

In a much cited 1989 *Harvard Business Review* article called "Eclipse of the Public Corporation,"[13] Jensen viewed the rise of "LBO partnerships" like KKR and Clayton & Dubilier as a "new organizational form"—one that, in acquiring and operating companies across a broad range of industries, was competing directly with, and threatening to supplant, public conglomerates. As Jensen put it, "The LBO succeeded by substituting incentives held out by compensation and ownership plans for the direct monitoring and often centralized decision-making of the typical corporate bureaucracy."[14]

The heavy debt financing played a critically important role in consummating the deal we just noted—making possible the concentration of ownership and control by the PE sponsor. But it also played a valuable ongoing corporate governance function, providing what Jensen described as "an automatic internal monitoring-and-control system." That is, if problems were developing, top management would be forced by the pressure of the debt service to intervene quickly and decisively. By contrast, in a largely equity-financed company, management could allow much of the equity cushion to be eaten away before taking the necessary corrective action.

The crux of Jensen's argument, then, is that debt serves as a control mechanism to focus the efforts of managers and owners on increasing efficiency and value. But if this model was appropriate for mature companies with stable lines of business, it was not likely to work for companies requiring significant capital investment or in early stages of development, such as firms backed by venture capital. Nonetheless, venture capital is predicated on much the same concentrated ownership structure as PE buyouts, only for the most part without leverage (though as we will see below, some new forms of VC debt have emerged in recent years).

Viewed within the context of the pecking-order theory, the information gap between managers and shareholders that increases the costs of public companies operating with outside equity is effectively closed by the concentration of ownership and board participation of PE buyout (as well as VC) sponsors. And the potential information gap between lenders and managers in public companies could also be reduced by PE sponsors' greater interaction and pressure to remain on good terms with their bankers and other debt providers. Thanks to their more frequent dealings, greater two-way flow of information, and stronger relations with banks, the most reputable PE sponsors, as studies have reported, have been able to get better lending terms.[15] And to the extent the expected costs of financial distress are significantly lower for PE-backed companies—an argument we present more evidence for below—the classic trade-off theory would also predict much higher optimal levels of debt.

Agency Conflicts within PE: the GP-LP Relationship

But all this begs the question: What other information or agency problems could a private ownership structure create that are not present in a public ownership model? After all, although there are no public shareholders in the PE model, there are LPs who depend on fair treatment by GPs for their net returns in much the same fashion as shareholders depend on managers and boards. Hence there would appear to be similar potential for self-dealing and other abuses.

In a number of important respects, then, the principle-agent relationship between private equity GPs and LPs adds a layer of complexity and friction that could be important for understanding optimal capital structure in PE buyouts. The potential agency conflicts in question arise from the delegated asset management typical in PE fund structures, and the contracts that are designed to manage such conflicts.

As one example, a GP's limited liability and the option-like carried-interest provisions could provide GPs with incentives to invest in even overpriced and overleveraged deals. Recognizing these incentives, LPs presumably choose to invest with GPs that they feel are best suited to meet their investment goals. LPs also sometimes negotiate partnership agreement terms that are deemed to better align the GP's interests with those of the LP.

As another example, fund-level debt could benefit the GP in ways that provide no benefit to, and even additional costs

13 Michael Jensen, (1989), "Eclipse of the Public Corporation," *Harvard Business Review*, 67(5), 61-74.

14 Jensen, "Active Investors, LBOs, and the Privatization of Bankruptcy," cited earlier.

15 The theoretical framework provided by Malenko and Malenko (2015) highlights the impact of reputation in securing capital.

for, the LP. For example, many LPs are not taxable entities and thus derive no benefit from any sort of tax shield provided by borrowing at the fund level. In addition, many LPs have the ability to adjust their own effective fund leverage through their own borrowing (or lending), presumably at a lower cost.

But to come back to the possibility just mentioned, the agency conflict, or imperfect alignment of interests and incentives, between LPs and GPs could result in a predictable pattern of "procyclical" LBO leverage that takes the form of too many overpriced and overleveraged deals in robust economies and loose credit conditions—and to excessive cutbacks in prices, leverage, and LP commitments under recessionary conditions.[16] The excessive retrenchment of capital in such cases is attributable to the agency conflict—the temptation of GPs, when provided excessive capital, to invest even in bad deals. And the net effect of this GP-LP conflict is a predictable scarcity of LP and hence PE investment during economic downturns. The most visible sign of this conflict of incentives is a predictable plummeting of late-cycle returns stemming from the excess of overpriced deals transacted by GPs during boom times.

And like the conflict of incentives in the relationship between GPs and LPs, a similar agency problem is likely to complicate the relationship between banks (lenders) and GPs. During boom periods, the risk of overpriced deals is borne disproportionately by the banks with their fixed claims. This misalignment of interests and outcomes also contributes to the cyclicality of LBO leverage—tending to excess in good times, and too little in bad.

The Evidence on Debt and Leverage in LBO Transactions

We now summarize the findings of studies that bear on several fundamental questions about leverage in buyout transactions. Although we focus mostly on past empirical work, in the last portion of our discussion we supplement these findings with ongoing analysis using a new dataset on *individual portfolio companies*.

Why Is the Leveraged Buyout Market So Cyclical?

We start with one of the most basic questions about private equity and associated credit markets: What drives the historically pronounced cyclical behavior of LBOs? As the literature has expanded, several explanations for the procyclical pattern in LBO leverage levels have emerged. Chief among them are market timing, GP-LP agency conflicts, agency problems between banks and PE investors, fluctuations in aggregate risk premia, and the growing use of subscription lines of credit.

A number of studies have provided evidence that GP-LP agency conflicts play an important role in the procyclical pattern seen in LBO leverage levels. Specifically, the leverage of LBOs responds more to relaxations or contractions of credit market conditions than that of other companies. Since LBO leverage is procyclical, leverage peaks when debt is cheap during "hot" credit markets. In contrast, public companies generally respond to the same market conditions by reducing their *market* leverage and thus exhibiting a "countercyclical" leverage pattern. Pro-cyclical PE investment patterns and countercyclical investment performance have also been documented in venture capital as well as buyouts.[17]

Studies have also confirmed the ability of PE investors to time their debt market issuance in order to "arbitrage" the conditions between debt and equity markets by increasing the leverage of deals in response to cheap credit—and documented the significant contribution of such market timing to the pro-cyclical pattern of buyout activity. A study published in 2010 finds that the more reputable PE firms are less likely to participate in LBO transactions when credit risk spreads are narrow and lending standards relaxed.[18] A 2012 study finds that LBOs have higher leverage when debt market liquidity is high and credit and leveraged loan spreads are low.[19] And a 2019 study comparing PE to strategic buyers concludes that periods of overvalued credit markets lead to increases in the leverage of PE funds and the price-to-earnings ratios paid by strategic buyers.[20] None of these studies, however, finds that hot credit markets are associated with better PE fund performance.

Banks have a unique position as credit experts, providers of access to capital markets investors, and advisors on transac-

16 These and similar results are predicted by theoretical frameworks, including those formulated by Ulf Axelson, Tim Jenkinson, Per Strömberg, and Michael S. Weisbach, (2013), "Borrow Cheap, Buy High? The Determinants of Leverage and Pricing in Buyouts," *Journal of Finance*, 68(6), 2223-2267; and Alexander Ljungqvist, Matthew P. Richardson, and Daniel Wolfenzon, (2019), "The Investment Behavior of Buyout Funds: Theory and Evidence," *Financial Management*, 49(1), 3-32.

17 See Paul Gompers, Josh Lerner, Anna Kovner, and Daniel Scharfstein, 2008, "Venture Capital Investment Cycles: The Impact of Public Markets," *Journal of Financial Economics*, 87, 1-23; and Steven N. Kaplan, and Jeremy C Stein, (1993), "The Evolution of Buyout Pricing and Financial Structure in the 1980s," *Quarterly Journal of Economics* 108 (2): 313-357. Axelson, Jenkinson, Strömberg, and Weisbach (2013) find that debt market conditions predict LBO leverage. Ljungqvist, Richardson, and Wolfenzon (2019) find that PE funds accelerate their investment flows and earn higher returns when investment opportunities improve, competition for deal flow eases, and credit market conditions loosen.

18 Cem Demiroglu and Christopher M. James (2010), "The Role of Private Equity Group Reputation in LBO Financing," *Journal of Financial Economics*, 96(2), 306-330.

19 Wouter De Maeseneire and Samantha Brinkhuis, (2012), "What Drives Leverage in Leveraged Buyouts? An Analysis of European Leveraged Buyouts' Capital Structure," *Accounting & Finance*, 52, 155-182.

20 Marc Martos-Vila, Matthew Rhodes-Kropf, and Jarrad Harford, (2019), "Financial vs. Strategic Buyers," *Journal of Financial and Quantitative Analysis*, 54(6). 2635-2661.

tions. Additionally, banks are compensated on a transaction basis instead of on an hourly or "when value is created" basis. To the extent such banks are in a better position to observe deal prospects than the market as a whole, they are likely to allocate capital and services in a more pro-cyclical manner than other participants and so exacerbate PE credit cycles.

Since the mid-1980s, syndicated loans have been the primary structure for debt financing in PE deals. While these loans originate in a bank, a syndicate of lenders acts as the funders and the originating bank owns only a portion of the loan. A 2013 study[21] of syndicated lending in PE deals investigates the market-timing distortions that might be attributed to it.[22] In addition to the effects on cyclicality, the authors of this study find that banks are no better equity investors than other LPs. When compared to stand-alone, or "parent-financed" deals, bank-affiliated deals had worse financing terms for the borrowers and worse *ex post* outcomes—notably, more debt downgrades and fewer upgrades. At the same time, although parent-financed deals provided significantly better financing terms for borrowers, they failed to exhibit better *ex ante* credit characteristics or deliver better *ex post* outcomes.[23]

As the authors of this study also discuss, this relationship and the involvement of banks in private equity has sparked substantial debate, including the inclusion of the Volcker Rule in the U.S. Dodd-Frank Act of 2010.[24] Furthermore, parent-financing deals pose an additional market risk. Banks, which occupy a unique position as debt market intermediaries, are able to "originate and distribute the debt from their own risky deals during the peak of the market, thereby amplifying the cyclicality of investments and the credit market."[25]

Other studies have demonstrated effects of macroeconomic conditions on LBO leverage levels, as well as investors' demand for a higher liquidity premium during bust periods.[26] As one example, a 2017 study[27] that focuses on the effects of the risk premium finds that 30% of the total variation in PE buyout activity can be attributed to changes in the aggregate equity risk premium while only 10% can be attributed specifically to credit market conditions.[28] In addition, the authors note a number of firm-level differences, including the following: "(1) firms with high market beta or high idiosyncratic volatility (a higher cost of capital and greater illiquidity costs) are less likely to be targets and there are even fewer high-beta firms when the risk premium is high; (2) firms with poor corporate governance and in less competitive industries are more sensitive to changes in the risk premium; (3) more liquid industries (easier for acquirers to exit) are less sensitive to movements in the risk premium.

How Have Buyouts Performed, and How Has Leverage Affected Risk?

The most common way to measure the performance of buyouts in academic studies is to compare the returns of PE investments to comparable public market returns. Most analysis of buyout funds has been conducted at the fund level. For example, a 2016 study by L'Her et al.[29] found that PE buyout funds outperform public equities before making any adjustments for differences in risk, but that such outperformance becomes insignificant after adjusting the benchmark for the systematic risks of buyout portfolio companies.

But in a more recent comprehensive study of buyout fund returns, Steve Kaplan and one of the present authors found that PE returns have exceeded a wide range of public market indexes on average over a variety of horizons and using a number of benchmarks.[30] Reinforcing that finding, another 2019 study undertook an extensive review of risk and return estimates for buyout funds and concluded that, although estimates vary substantially by method, time period, and data source, the most recent and comprehensive studies appear to be converging on PE fund risk estimates that are slightly higher than public markets (beta of around 1.3), and historical risk-adjusted outperformance of around 3% per year.[31]

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21 Lily H. Fang, Victoria Ivashina, and Josh Lerner, (2013), "Combining Banking with Private Equity Investing," *Review of Financial Studies*, 26(9), 2139-2173.

22 Shleifer and Vishny (2010) find that during credit market booms, banks will fund more risky projects when debt securities are mispriced by outside investors and banks hold only a portion of the loan as they receive loan origination fees. This increases the cyclicality of the credit market.

23 "The superior nonpricing terms of parent-financed deals are concentrated entirely in credit market peaks when banks retain the least of the loans, which suggests that the superior financing terms result from favorable credit supply conditions. They also find that bank involvement in private equity—especially their role as lenders—generates significant cross-selling opportunities for banks, which enables them to capture more future revenues (while their risk exposures can be syndicated out)." See Fang, Ivashina, and Lerner (2013), p. 2144.

24 The basis for the Volcker rule is the belief that "equity investments by banks could reflect bank managers' incentives to grow revenues and maximize volatility, which can create systemic risks. Such incentives might arise because banks' own equity values increase with volatility, and large banks enjoy implicit bail-out guarantees". See Fang, Ivashina, and Lerner (2013), p. 2140.

25 Fang, Ivashina, and Lerner (2013), p. 2141.

26 See Francesco Franzoni, Eric Nowak, and Ludovic Phalippou, (2012), "Private Equity Performance and Liquidity Risk," *Journal of Finance*, 67(6), 2341-2373; and Valentin Haddad, Erik Loualiche, and Matthew Plosser, (2017), "Buyout Activity: The Impact of Aggregate Discount Rates," *The Journal of Finance*, 72(1), 371-414.

27 Haddad, Loualiche, and Plosser (2017). https://www.nber.org/system/files/working_papers/w22414/w22414.pdf.

28 Buyout activity is negatively related to the market-wide risk premium after controlling for credit market conditions.

29 Jean-Francois L'Her, Rossitsa Stoyanova, Kathryn Shaw, William Scott, and Charissa Lai, (2016), "A Bottom-Up Approach to the Risk-Adjusted Performance of the Buyout Fund Market," *Financial Analysts Journal*, 72(4), 36-48.

30 Gregory W. Brown and Steven N. Kaplan, (2019), "Have Private Equity Returns Really Declined?" *The Journal of Private Equity*, 22(4), 11-18. See also Robert Harris, Steven N. Kaplan and Tim Jenkinson, (2014), "What Do We Know about Private Equity Performance?" *Journal of Finance*, 69(5).

31 Arthur Korteweg, (2019), "Risk Adjustment in Private Equity Returns," *Annual*

What's more, as Will Goetzmann and colleagues[32] argued in a recent study, PE funds also appear to provide diversification benefits to LPs in the form of "priced risk factors" in illiquid markets that are only partly spanned by public factors.[33] By providing exposures somewhat different from those of public markets, PE markets are effectively providing investors with an additional source of "factor risk premia" and hence value-adding diversification.

While the fund-level analysis discussed above suggests that funds in aggregate generate superior risk-adjusted returns, it is difficult to accurately characterize risk in private investments. For example, one recent study[34] disputes the widespread belief that PE investments have higher volatility than public equity due to higher leverage. The study's results suggest that the volatility of private equity returns is not detectably higher than that of public equity, despite its higher leverage. The authors argue that buyout fund managers prefer to invest in companies whose underlying business activities are inherently less risky and can therefore bear higher leverage, which increases profits without the commensurate expected increase in overall volatility.

Another study[35] provides evidence of a negative relationship between deal leverage and return that is attributed to heightened competition among bidders during periods of easy credit. As an equilibrium outcome of the deal process, good credit market conditions are related to both larger amounts of debt and higher transaction prices. But the higher price translates to a lower deal return upon exiting, an effect that is especially notable for less reputable funds with poor interim performance.[36]

As noted earlier, hot credit markets can lead to high leverage, which could lead to higher default rates.[37] But it is hard to discern this relationship in the data, where estimates of the effects of leverage on the probability and cost of distress vary widely, especially between PE-backed private companies and public companies. For example, a 2010 study by the Private Equity Council concluded that PE-backed firms had a default rate of 2.8%, as compared to a rate of 6.2% for similar public companies during the 2008-2009 recession.[38] Another study published in the same year,[39] after examining over 2,000 public and private companies that obtained leveraged loan financing between 1997 and 2010, reported that PE-backed firms were no more likely to default than similar public companies with comparable leverage, and showed themselves better able to deal with financial distress. In the words of the authors, "When private equity-backed firms do become financially distressed, they are more likely to restructure out of court, take less time to complete a restructuring, and are more likely to survive as an independent going concern than financially distressed peers not backed by a private equity investor." As if to anticipate these findings, a study done 12 years earlier estimated the distress costs of a set of LBOs that entered bankruptcy in the 1990s. When the authors combined their estimates of 10%-20% of total enterprise values with (ex post unconditional) probabilities of bankruptcy for buyouts of around 5%, the expected financial distress costs for LBOs ended up averaging as low as 0.5% to 1% of firm value.[40]

Viewed together, the findings of these studies suggest that PE has a comparative advantage in managing high leverage—one that effectively enables PE-backed firms to take on higher levels of debt without incurring commensurate levels of financial risk that would otherwise reduce their values. Nevertheless, in a sign that public companies can also learn

Review of Financial Economics, 11, 131-152.

32 William N. Goetzmann, Elise Gourier, and Ludovic Phalippou, (2018), "How Alternative Are Private Markets?" SSRN Working Paper 3227020. https://doi.org/10.2139/ssrn.3227020.

33 Goetzmann et al. (2018) provide an eight-factor model that captures 57.2% of the total variance of private market returns. The eight factors are: all European private funds (except those focusing on Venture Capital), non-small (i.e., largest three quartiles) Venture Capital funds, U.S. non-small Real Estate funds, U.S. non-small Distressed Debt funds, energy (oil & oas) funds, funds with a low-risk profile, and the other two factors cannot be easily characterized. Four of their eight private factors are relatively well spanned by a five-factor model that includes the U.S. market equity factor, the size factor [SMB] of Eugene Fama and Ken French, (2015), "Five-Factor Asset Pricing Model," *Journal of Financial Economics*, 116(1), 1-22; the alternative value factor [HMLd] of Clifford S. Asness, Andrea Frazzini, (2013), "The Devil in HML's Details," *The Journal of Portfolio Management*, Volume 39, Number 4, the quality of earnings factor [QMJ] of Clifford S. Asness, Andrea Frazzini, and Lasse H. Pedersen, (2018), "Quality Minus Junk," *Review of Accounting Studies*, 24, 1-79; and the low-beta factor [BAB] of Andrea Frazzini and Lasse Heje Pedersen, (2014), "Betting Against Beta," *Journal of Financial Economics*, 111(1), 1-25.

34 Megan Czasonis, William B. Kinlaw, Mark Kritzman, and David Turkington, (2020), "Private Equity and the Leverage Myth," SSRN *Working Paper* 3540545.

35 Reiner Braun, Nicholas G. Crain, and Anna Gerl, (2017), "The Levered Returns of Leveraged Buyouts: The Impact of Competition," SSRN *Working Paper* 2667870.

36 See Marc Martos-Vila, Matthew Rhodes-Kropf, and Jarrad Harford, (2019), "Financial vs. Strategic Buyers," *Journal of Financial and Quantitative Analysis*, 54(6). 2635-2661; who find that as competition increases among PE funds, gains captured from the overvalued debt market may be captured by the target firms, and thus PE funds may experience lower returns. Two theoretical rationales, the co-insurance effect and the monitoring effect explain this behavior. The co-insurance effect derives from the fact that "strategic buyers are less able than financial buyers to exploit investors' misperceptions because strategic buyer combines projects and the valuation mistake partially offset each other." The monitoring effect derives from the fact that "overvaluation increases the moral hazard problem and enhances the importance of better governance to eliminate misbehavior, which are the strength of PE funds."

37 For supporting evidence, see Steven N. Kaplan, and Jeremy C. Stein, (1993), "The Evolution of Buyout Pricing and Financial Structure in the 1980s," *Quarterly Journal of Economics,* 108(2), 313-357; and Ulf Axelson, Tim Jenkinson, Per Strömberg, and Michael S. Weisbach, (2013), "Borrow Cheap, Buy High? The Determinants of Leverage and Pricing in Buyouts," *Journal of Finance*, 68(6), 2223-2267.

38 See Private Equity Council (2010).

39 Edie Hotchkiss, David C. Smith, and Per Strömberg, (2010), "Private Equity and the Resolution of Financial Distress," NBER Chapters, in *Market Institutions and Financial Market Risk, National Bureau of Economic Research, Inc*.

40 Gregor Andrade and Steven N. Kaplan, (1998), "How Costly is Financial (Not Economic) Distress? Evidence from Highly Leveraged Transactions That Become Distressed," *Journal of Finance,* 53, 1443-1493.

to use high leverage to their advantage (as Jensen suggested they would), Steve Kaplan and Jeremy Stein's 1993 study of large leveraged recaps of public companies in the late '80s provides evidence of management's ability to handle their debt loads by reducing their operating as well as financial risks.[41]

How Do PE Firms Add Value Through Leverage?

As discussed earlier, the potential gains from higher leverage are likely to come through several channels, but can be related back to a fundamental trade-off between the benefits coming from the tax shield and more efficient operations and the costs associated with a higher probability of financial distress. A 2011 study[42] finds that the estimated tax savings associated with the debt in public-to-private LBOs are positively related to acquisition premiums, but the fact that such premiums are roughly twice the size of the tax savings implies that the tax savings from increasing financial leverage effectively accrue to the selling public shareholders rather than the PE fund sponsoring the LBO.[43] On the other hand, a 2014[44] analysis of the confidential corporate tax returns in 317 public-to-private LBOs find more room for value creation from the debt tax shield. Specifically, the authors document that debt levels remain high for several years after acquisitions and that EBITDA growth makes the value of the tax shield more durable than assumed in other analyses. On the other hand, a recent study[45] of the corporate taxes and leverage of a large sample of U.S. public and private companies actually finds a negative relation between tax rates and leverage, which suggests that the tax shield is not a primary driver of leverage decisions.[46]

Public companies are acutely aware of the effects of financial distress and the importance of maintaining financial flexibility. For PE-backed companies, however, the PE sponsors' access to credit effectively works to "relax the financial constraints of portfolio companies."[47] And along with the strong ties between GPs and the banking industry that give PE investors preferred access to credit, their capital commitments by LPs with long-term holding periods provide PE investors with another source of capital during economic downturns. And as we mentioned earlier, buyouts sponsored by more reputable PEs with strong track records are less likely to experience financial distress during their operating lives.

Leverage trade-offs have been studied in other asset classes as well. For example, a 2011 study of the optimal fund-level leverage in real estate finds that the advantages include tax shield, ability to purchase more properties, liquidity and flexibility, and increase in return on invested equity.[48] Among the drawbacks of such fund-level leverage is loss of the benefits of the investor's bond exposure and incurring double transaction costs in the bond market, interest rate volatility risk, additional fees and management alignment difficulties, and high cost of distress.

In his 1989 *Harvard Business Review* article cited earlier, Jensen proclaimed the superiority of the corporate governance structure of PE-owned firms over that of public companies. Jensen argued that together "with active boards, high-powered management compensation, and concentrated ownership," the leverage component plays a critical role in the success of PE buyouts, first by making possible the concentration of equity ownership, and then by exerting pressure on management to operate more efficiently and pay out excess capital.

In support of Jensen's argument, a growing literature has investigated the effects of private equity ownership on firm productivity, product quality, employment, and related dimensions; and during normal times, these studies have found substantial positive effects on the operations of the firms in which they invest.[49] In addition to direct value creation,

41 Steven N. Kaplan and Jeremy C Stein, (1993), "The Evolution of Buyout Pricing and Financial Structure in the 1980s," *Quarterly Journal of Economics,* 108 (2): 313-357.

42 Tim Jenkinson and Rüdiger Stucke, (2011), "Who Benefits from the Leverage in LBOs?" SSRN Working Paper 1777266.

43 A result that is confirmed empirically by Braun, Crain, and Gerl (2017).

44 Jonathan B. Cohn, Lillian F. Mills, and Erin M. Towery (2014), "The Evolution of Capital Structure and Operating Performance after Leveraged Buyouts: Evidence from U.S. Corporate Tax Returns," *Journal of Financial Economics*, 111, 469-494.

45 Ivan Ivanov, Luke Pettit, and Toni M. Whited, "Taxes Depress Corporate Borrowing: Evidence from Private Firms," (September 18, 2020). Available at SSRN: https://ssrn.com/abstract=3694869 or http://dx.doi.org/10.2139/ssrn.3694869.

46 The effect is stronger for private companies. The authors show that the value benefits from a decline in credit spreads associated with lower taxes more than offset the decline in value of the tax shield. Consequently, lower taxes incentivize higher debt levels.

47 During the financial crisis, PE-backed companies decreased investments less than non-PE-backed companies. PE-backed companies have been less bound by financial constraints: higher debt issuance and equity issuance, a relative decrease in the cost of debt, greater growth in their stock of assets in the years after the crisis, increased their market share in the industry during the crisis, more likely to be sold through nondistressed merger and acquisition (M&A) transactions. See Shai Bernstein, Josh Lerner, and Filippo Mezzanotti, (2019), "Private Equity and Financial Fragility during the Crisis," *Review of Financial Studies*, 32(4), 1309-1373.

48 Maarten van der Spek, and Chris Hoorenman, (2011), "Leverage: Please Use Responsibly," *Journal of Real Estate Portfolio Management,* 17(2), 75-88.

49 See Greg Brown, Robert Harris, Tim Jenkinson, Steve Kaplan, and David Robinson, (2020a), "Private Equity: Accomplishments and Challenges" *Journal of Applied Corporate Finance*, 32(3). Examples include Shai Bernstein and Albert Sheen, (2016), "The Operational Consequences of Private Equity Buyouts: Evidence from the Restaurant Industry," *Review of Financial Studies*, 29, 2387-418; Shai Bernstein, Josh Lerner, Morten Sorensen, and Per Strömberg, (2016), "Private Equity and Industry Performance," *Management Science*, 63(4), 1198-213; Quentin Boucly, David Sraer, and David Thesmar, (2011), "Growth LBOs," *Journal of Financial Economics* 102, 432-453; Steven J. Davis, John Haltiwanger, Kyle Handley, Ron Jarmin, Josh Lerner, Javier Miranda, (2014), "Private Equity, Jobs, and Productivity," *American Economic Review*, 104(12), 3956-3990; Kose John, Larry Lang, and Jeffrey Netter, (1992), "The Voluntary Restructuring of Large Firms in Response to Performance Decline," *Journal of Finance* 47, 891-917; Steven Kaplan, S. (1989), "The effects of Management Buyouts on Operating Performance and Value," *Journal of Financial Economics*, 24(2), 217-254; Frank R. Lichtenberg and Donald Siegel, (1990), "The Effects of Leveraged Buyouts on

the anticipation of these improvements by itself allows for higher leverage at the time of the buyout, which in turn generates value from the debt tax shield. And consistent with this argument, a 2011 study has demonstrated a clear link between post-buyout performance and the level of bank financing.[50]

What Do Studies Tell Us About Collateralized Loan Obligations, Direct Lending, and Venture Debt?

Thus far we have focused on evidence from the perspective of the portfolio company, GP, or LP—that is, essentially from the borrower's perspective. Of course, for every borrower there is a lender, and a body of studies has examined the effects of such borrowing on the efficiency of debt markets that provide capital to the private equity industry.

After the global financial crisis, for example, several studies examined the market for collateralized loan obligations (CLOs), which are effectively collateralized debt obligations backed by corporate debt. A 2012 study[51] provides evidence that adverse selection is not, as many observers have assumed (because originators are not keeping the loans they originate), an inevitable consequence of the securitization of corporate loans.[52] The authors find no consistent evidence that securitized corporate loans are riskier than similar non-securitized loans, neither during the 2005-2007 period lead-up to the financial crisis nor for the subset of loans purchased by the CLO from its underwriters.[53] The authors argue that the larger loan size and the syndication process itself make corporate loans less vulnerable to adverse selection than the securitized mortgages to which they are regularly compared. Corporate loans, at origination, are funded by a group of banks and institutional investors whose concern about their reputations lead them to screen the quality of such loans.[54]

Loan covenants also play a role in allocating control rights between PE-backed issuers and their investors. A 2016 study[55] investigates the possible negative effects of the rising number of covenant-light (cov-lite) leveraged loans, including the higher costs of resolving financial distress stemming from higher coordination costs borne by dispersed lender groups. Contrary to what their name might suggest, cov-lite loans do not have fewer covenants, but weaker enforcement mechanisms, which has at least the potential to make them riskier.

The increasing use of cov-lite loans is especially relevant for leveraged loans, since they are widely syndicated to a diverse group of institutional investors.[56] With the bulk of leveraged loans funded by CLOs, loan mutual funds, hedge funds, securities firms, insurance companies. and pension funds, any renegotiation triggered by financial covenants requires multiple-party coordination. But contradicting the widespread view that the rising use of cov-lite loans reflects the demands of the borrowing companies, the authors present evidence that as cov-lite volumes have expanded, the yields on cov-lite loans—and thus the effective cost of cov-lite financing for issuers—have actually fallen over time, reflecting increases in investor demand.[57]

Recently, academic interest has expanded to direct lending by nonbank creditors. In a 2018 study[58] that provided a first look at the risks and returns of private credit funds, the authors found positive returns for the top three quartiles in terms of IRR and excess returns relative to leveraged-loan, high-yield and BDC indexes. Direct lending funds, which undertake a "bilateral" origination of a loan between a single borrower and a small group of lenders, are shown to have a relatively low beta and positive alpha compared to the leveraged loan and high-yield indices, which is viewed as evidence of diversification benefits relative to other credit strategies.

A 2019 study[59] examined the effect of changes in bank regulatory capital positions on the entry of nonbank lenders. The study showed that undercapitalized banks were especially likely to remove loans with higher capital requirements from their balance sheets when bank capital is scarce, and that a significant portion of these loans was reallocated to

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Productivity and Related Aspects of Firm Behavior," *Journal of Financial Economics*, 27(1), 165-194; and Erik Stafford, (2017), "Replicating Private Equity with Value Investing, Homemade Leverage, and Hold-to-Maturity Accounting," SSRN Working Paper 2720479.

50 Shourun Guo, Edie S. Hotchkiss, and Weihong Song, (2011), "Do Buyouts (Still) Create Value?" *Journal of Finance*, 66(2), 479-517.

51 Effi Benmelech, Jennifer Dlugosz, and Victoria Ivashina, (2012), "Securitization without Adverse Selection: The Case of CLOs," *Journal of Financial Economics*, 106(1), 91-113. https://doi.org/10.1016/j.jfineco.2012.05.006.

52 Several studies provide evidence that securitization resulted in lower lending standards, which led to adverse selection in the collateral pools underlying these products. See Benmelch et al. (2012) and Benjamin J. Keys, Tanmoy Mukherjee, Amit Seru, and Vikrant Vig, (2010), "Did Securitization Lead to Lax Screening? Evidence from Subprime Loans," *Quarterly Journal of Economics*, 125, 307-362.

53 Fundamental agency tensions can plague this subset. The CLO underwriter is typically a bank and is responsible for loan screening and interacting with the rating agencies. However, these underwriting banks "may use this channel to sell fractions of their own riskier loans to CLOs" (Benmelch et al. (2012).

54 "Fractions of the same underlying loan are simultaneously held by multiple CLOs as well as by other institutional investors and banks. In addition, the bank that originated the loan (the lead bank) typically retains a fraction of the loan on its balance sheet and each underlying loan is rated" (Benmelch et al. (2012). These all provide incentives of the investors for better screening process and risk retention by the originator.

55 Bo Becker and Victoria Ivashina, (2016), "Covenant-Light Contracts and Creditor Coordination." https://www.hbs.edu/faculty/pages/item.aspx?num=50952.

56 Traditional lenders like banks and finance companies account for about 10%-15% of loan origination.

57 If the rising of cov-lite loans is driven by demand shock from the borrowers, the price would be expected to rise.

58 Shawn Munday, Wendy Hu, Tobias True, and Jian Zhang, (2018), "Performance of Private Credit Funds: A first look," *The Journal of Alternative Investments*, 21(2), 31-51.

59 Rustom M. Irani, Rajkamal Iyer, Ralf R. Meisenzahl, and Jose-Luis Peydro, (2020), "The Rise of Shadow Banking: Evidence from Capital Regulation," SSRN Working Paper 3166219.

nonbanks. Such credit reallocation was viewed as a capital market response to the negative impacts of the 2008 crisis, when loans funded by nonbanks experienced both a sizable reduction in credit availability and greater price volatility in the secondary market.

Another 2019 study[60] looked at the post-crisis lending of non-bank financial institutions, including finance companies (FCOs), private equity/venture capital (PE/VC) firms, hedge funds, bank-affiliated finance companies (bank FCOs), investment banks, insurance companies, business development companies (BDCs), and investment managers. While most studies examine a syndicate led by a commercial bank, this study focused on the direct negotiation process between non-bank financial institutions and borrowers. Based on a randomly collected sample of publicly traded middle-market firms during the post-crisis period (2010-2015), the authors find that non-bank lending was widespread—accounting for almost a third of the market—and that these institutions fund less profitable, more leveraged, and more risky and volatile firms. In particular, the study showed that PE (including venture capital) firms were especially likely to lend to faster growing, R&D-intensive firms.

What's more, to address the agency problem between the borrowers and lenders, non-bank lenders are less likely to monitor borrowers by including financial covenants, but more likely to align incentives using warrants and engage in more intensive ex-ante screening. The authors also find that nonbank loans have 1.9% higher interest rates, but that the difference between bank and non-bank loans are due to the market segmentation and differences in funding costs rather than difference in loan risks.

Yet another 2019 study[61] examines the growth in direct lending during the period 2003-2016, and the potential extent of adverse selection costs. The analysis documents how institutional investors have aggressively entered the market, accounting for about 80% of the direct loan volume in 2016. Direct lending tends to become more active when banks face tighter capital and regulatory constraints, and is more prevalent among borrowers with limited credit history. But even so, the study finds direct loans to be of similar credit quality to bank-originated loans. In addition, and more tellingly, direct loans issued by PE or investment management firms exhibit significantly better performance than other institutional loans.

Increasingly, young firms backed by venture capital are entering debt markets as a source of external capital. Because such firms are unlikely to possess tangible assets and positive cash flows, it is difficult for them to secure traditional bank lending. To fill the gap, so-called "venture debt" has become increasingly popular as start-up financing intended to "extend the runway" between venture rounds and reduce equity dilution.

A 2016 study[62] of venture debt financing that conducted a "discrete choice" experiment using 55 senior U.S. venture debt lenders concluded that (1) patents are as important as tangible assets as collateral to lenders; (2) venture debt lenders show a preference for start-ups that offer warrants that can help overcome the agency problems; and (3) VC backing can substitute for positive cash flow, but only for early-stage ventures. In addition, a 2018 study[63] also finds that venture debt can create firm value by reducing dilution, aligning the entrepreneur's incentives with the firm's, and inducing entrepreneur's risk-taking behavior.

New Evidence on Buyout Leverage (and Performance) at the Deal Level

As part of our ongoing research, we analyzed a new proprietary dataset that has leverage information for individual buyout deals provided by the StepStone Group. Our sample consisted of 6,248 buyout transactions from the period 1984 through 2020 with sufficient performance and financial accounting data for our analysis. Although this is only a subset of total transactions, they are among the largest and together represent about $1.3 trillion in combined equity investments and about 4.5 trillion in total enterprise value (TEV). By our estimates, these transactions cover about half of the value of all (global) historical buyouts with PE fund sponsors. As one would expect, most deals in the first half of the sample are fully exited, but as we move closer to the present, an increasing proportion are not fully exited. We now summarize the main results of the analysis (and refer the reader to the white paper for a more thorough presentation of the methods and results).[64]

The typical PE deal in our sample was held for 4.6 years and part of a fund with an average size of about $2.6 billion, though there is of course a wide range of fund sizes. Although the mean deal's TEV is $718 million, the median

60 Sergey Chernenko, Isil Erel, and Robert Prilmeier, (2019), "Nonbank Lending," National Bureau of Economic Research Working Paper, no. 26458. https://doi.org/10.3386/w26458.

61 Maria Loumioti, (2019), "Direct Lending: The Determinants, Characteristics and Performance of Direct Loans," SSRN Working Paper 3450841.

62 Gaétan de Rassenfosse and Timo Fischer, (2016), "Venture Debt Financing: Determinants of The Lending Decision," *Strategic Entrepreneurship Journal*, 10(3), 235-256.

63 Jesse Davis, Adair Morse, and Xinxin Wang, (2018), "The Leveraging of Silicon Valley: Venture Debt in the Innovation Economy," SSRN Working Paper 3222385.

64 The paper, "Debt and Leverage in Private Equity: A Survey of Existing Results and New Findings," can be downloaded from the Institute for Private Capital website.

TEV is only $195 million. Thus, as expected, the size of the deals is heavily skewed, with a relatively large number of small and mid-sized transactions, and a few much larger deals. Although deal size dropped during the global financial crisis of 2008-2009, it has grown significantly in the last decade to the point that by 2019 the median deal had returned to its previous peak reached in 2007. The mean entry EBITDA multiple paid by the PE sponsor was 10.8 times.

As measures of leverage, we use two metrics representing different ways of viewing capital structure at the deal level. One is a "flow" measure that can be used to assess debt-servicing capability, which is defined as entry Net Debt divided by entry EBITDA. The average leverage ratio was 4.2 times, with an interquartile range of 2.8 to 5.4. Over the life of a deal, the leverage ratio declined slightly for the typical firm, though more than a quarter of the firms experienced increasing leverage ratios.

The second measure of leverage is a "stock measure"—defined as entry Debt-to-TEV or (D/V)—which measures the fraction of total firm value financed with debt. The average D/V was 0.49, with an interquartile range of 0.37 to 0.62. D/V values tended to decline relatively more than the flow leverage ratios over the life of a deal—and rarely increased.

The large majority of firms increased in value while owned by PE firms, although such growth has proved very cyclical, with deals done in the 1997-2001 and 2006-2008 periods growing much less than average. Nevertheless, the growth in the TEV of buyouts has become much more pronounced since the GFC. Such TEV growth derives from two general sources: increases in operating performance and increases in valuation multiples. Annual growth rates in revenue and EBITDA, which both averaged about 12%, were considerably lower than the average TEV growth rate of 19%, which suggests that much of recent TEV growth is attributable to expanding valuation ratios as well as increased growth and profitability.[65] For the large majority of deals, we found that the EBITDA multiple not only increases, but that multiple expansion has reached a record high in recent years.

In sum, our findings show that PE buyouts in recent years have produced larger deals, and higher growth rates and enterprise values.

Our flow measures of leverage (Net Debt divided by EBITDA) at the deal level have exhibited considerable cyclicality, with values well above average during the years leading up to the GFC and then plummeting in 2008 and 2009. Nevertheless, by 2018, leverage ratios had returned to pre-GFC levels. Despite the cyclicality, there has generally been more variation within years than across years—and a wide range of values not only across industries, but even within every industry. What's more, regardless of deal year or industry, we find that leverage ratios decline on average during a deal's life; but there is a wide range of outcomes, and for more than a quarter of transactions, leverage ratios increase.

"For the large majority of deals, we found that the EBITDA multiple not only increases, but that multiple expansion has reached a record high in recent years."

But unlike leverage ratios based on EBITDA, we found that the average D/V ratio in PE buyouts declined sharply during the GFC and has not increased since then. During the financial crisis, moreover, the typical deal shifts from being financed with a majority of debt to a majority equity. And the average D/V ratio since 2015 has been lower than at any other time during our sample period. The average D/V ratio varies by industry, but the majority of transactions in recent years have been financed with 40% to 60% debt for all industries. And over the life of the deals, D/V declines significantly in the vast majority of cases.

In sum, the growth in leverage ratios and decline in D/V ratios post-GFC has been driven by a confluence of trends. First, higher expected revenue and profitability growth have attracted higher EBITDA multiples. Higher entry multiples, by definition, increase both the value of a transaction and the leverage ratio for a given level of debt. Nevertheless, a modest decline in D/V ratios post-GFC has tempered the increase in leverage ratios slightly. Realized high growth in EBITDA, combined with record multiple expansion, has resulted in more rapid declines in both the leverage ratio and D/V ratio over a typical deal's lifetime.

Buyout Performance Measured at the Portfolio Company Level

Measuring performance at the deal level is typically done on gross returns since fees and carry depend on the overall perfor-

65 See, for example, Figures 1.6 and 1.7 in Bain & Company's *Global Private Equity Report 2020*.

mance of a fund. A recent study using portfolio company data from Burgiss[66] shows that buyout deals are generally profitable in all time periods, across all industry sectors, and in all major geographies. Unfortunately, that study did not have detailed information on leverage.

When using the StepStone sample, we find similar, but somewhat stronger performance than in the Burgiss data. The median gross money multiple was 1.84 (as compared to 1.55 in the Burgiss data) with an interquartile range of 1.07 to 3.07. The median deal gross IRR was 21.0%, with an interquartile range of 4% to 43%. Median gross PMEs showed that deals typically outperformed public market returns, though the lowest quartile gross performance of buyout deals is generally inferior to market returns.

Gross deal-level performance has been quite cyclical, with high returns from deals closed in the mid-1990s, early 2000s, and post-GFC. Conversely, gross returns were relatively weak for deals closed in the late 1990s and leading up to the GFC. And when we examined deal returns by sector, we found remarkably consistent results, with surprisingly small differences across sectors. In almost all sectors, the vast majority of deals were profitable (before fees) on both an absolute and market-adjusted basis.

The StepStone data allowed us to look at the relationship between leverage and performance at the deal level. Summarized at a high level, the findings show that deals with high D/V ratios tend to be larger companies with lower EBITDA and TEV growth as well as lower operating margins than low D/V deals. In addition, high D/V deals have higher entry leverage ratios than low D/V deals, but over the life of the deal, high D/V deals experience significant drops in net debt outstanding accompanied by large declines in both D/V *and* leverage ratios. In contrast, low D/V deals experience substantial growth in net debt, no change in D/V ratios, and large increases in leverage ratios. Exit EBITDA multiples expand less in high D/V deals than in low D/V deals. In terms of deal performance, the top quartile of D/V deals generate much higher returns than the other three quartiles, though returns increase monotonically with D/V.

Like high D/V deals, deals with high leverage ratios are also larger and have lower TEV growth over the life of the deal. However, deals with high leverage ratios have higher operating margins and experience higher EBITDA growth. Perhaps the biggest contrast with high D/V deals is that the entry EBITDA multiples are much higher for deals with high leverage ratios than those with low leverage ratios. Over the life of the typical high leverage deal, net debt expands but both the leverage ratio and D/V contract. Upon exit, deals with high leverage ratios also experience weak multiple expansion and the variation in multiple expansion is much greater than for low leverage ratio deals—the opposite of what is observed for D/V.

Viewed together, our results suggest that deals with high leverage ratios are expected to grow revenues and profits to service the higher leverage. But in contrast to high D/V transactions, the performance of high leverage ratio deals is inferior to that of low leverage ratio deals. And regardless of the level of D/V, deals with high leverage ratios are larger, have higher entry EBITDA multiples, and less expansion in EBITDA multiples over the life of the deal. In contrast, there is a strong positive relation between D/V and performance regardless of the entry leverage ratio.

Overall, our findings echo the conclusions of others about the cyclicality of leverage found in other studies. Moreover, they show the deal performance is linked to the use of debt, but that such linkage depends on how leverage is measured relative to the cash flow and value of the underlying company.

Conclusions

Although private equity has grown dramatically as an asset class in the last few decades, there have been relatively few large-scale empirical studies of the role that leverage plays in buyouts—its effects on the risk, returns, incentives, and other basic characteristics of LBOs. In these pages, we provide an overview of the evolution of PE capital structures, the types of leverage used, the theories offered to explain capital structure choices, and the recent empirical studies that shed light on leverage in PE deals.

Buyout capital structures have evolved over time as the debt markets and PE firms have created and adopted new ways to attract debt capital. Debt enters into the PE buyout ecosystem in a variety of layers and structures. PE firms continue to use innovative capital structures, adding layers of debt at the fund and investor level on top of those at the portfolio company. Moreover, the forms and sources of debt vary, widely introducing an array of incentive and risk-sharing elements that are more complex than the simple, "stylized" view of leverage as borrowings that work to increase equity returns on an underlying asset with an exogenous set of operating returns and risks.

Leverage decisions in PE are shaped by many of the same forces and considerations that influence public companies. That said, a number of studies suggest that PE has a comparative advantage in managing high leverage and its potential

66 Gregory W. Brown, Robert S. Harris, Wendy Hu, Tim Jenkinson, Steven N. Kaplan, and David Robinson, (2020b), "Private Equity Portfolio Companies: A First Look at Burgiss Holdings Data," SSRN Working Paper 3532444.

costs—one that effectively enables PE-backed firms to take on higher levels of debt than comparable public companies. Leverage also plays a role in facilitating the concentrated ownership of firms (by a PE fund), which itself is expected to lead to better governance, and increases in the operating efficiency and value of the business. And consistent with this thinking, research continues to show PE buyouts providing net returns to LPs that exceed the returns to public market investors. But on the negative side, the structure of PE deals also continues to raise concerns about possible conflicts of interests and incentives between GPs and their LPs and creditors that may be managed only with partial success by PE's contractual arrangements.

The capital structure decisions in PE vary considerably across the cycle, with rises and falls in leverage with fluctuations in credit market conditions and PE investment and returns. A number of studies offer explanations of the highly cyclical nature of private equity activity, suggesting that institutional features combined with macroeconomic cycles are to some degree hardwired into the industry. Several explanations for the procyclical pattern in LBO leverage levels have emerged, including market timing, GP-LP agency conflicts, agency problems between banks and PE investors, fluctuations in aggregate risk premia, and the growing use of subscription lines of credit.

Finally, our exploration of individual buyouts using a new large dataset provides more evidence of PE outperformance. It also shows that the relationship between debt and performance depends on how leverage is measured. When debt is measured as a percentage of deal value, we find the expected positive relationship with average returns—consistent with a simple model of financial leverage generating a risk-return trade-off. But when leverage is measured as a multiple of EBITDA, we find only a weak negative relationship with performance. The data suggest that firms with high debt-to-value ratios are more likely to be mature "value" firms whereas firms with high leverage ratios tilt towards growth—and these differences explain the results related to performance.

Looking forward, there is of course much more to learn about the effects of PE leverage and capital structure choices. It is difficult to measure and characterize the risk of PE investments and how it is affected by leverage. In fact, even the choice of an appropriate measure of leverage—whether in relation to value or operating cash flow—is important for understanding the links between leverage and PE investments. The rich field for research is increasingly fueled by new innovations in financing as investors are exposed to risks stemming from debt of many forms and at many layers in PE structures. We look forward to considerable progress in our understanding of these issues as more comprehensive, including portfolio company, data become available to researchers.

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Appendix I: A Brief Summary of the Structural Variations and Uses of PE-Related Debt:

Management Company Debt: Debt issued or borrowed at the management company level backed by the partners' interest in the management company and/or personal guarantees. This can be either secured or unsecured and can be in the form of a loan or bond. Large global PEs (several of which are publicly listed) have borrowed in the form of term loans and issued bonds. The term loans have been senior secured first lien, typically with 7-year tenors. The bonds have been both secured and unsecured obligations with long-dated maturities (including 30 years). Most of these issuances have been investment grade rated with effective yields in the low single digits. Use of proceeds includes M&A, seed new business lines, fund dividends to partners, and general corporate purposes

Fund-Level Debt: Debt borrowed at the fund-level, backed by undrawn LP capital commitments and/or pledges of equity collateral of the underlying portfolio companies (HoldCo's and OpCo's).

- **Subscription Lines:** One common form of fund-level debt is typically referred to as a "wire line" facility or "subscription line." These instruments enable the borrower to use proceeds instead of LP capital to make early investments or pay fees and expenses. Typical features include:
  - limited as a percentage of the LPs' capital commitments (commitments from the most creditworthy LPs earn a 90% advance rate, and commitments from lesser credits earn lower advance rates or, in some cases, zero),
  - are secured by the LPs' capital commitments,
  - generally must be repaid in the early or middle part of the fund's life (unless extended), although terms are beginning to lengthen.

Because subscription lines are backed by either undrawn capital commitments or a pledge of underlying illiquid equity collateral, they do not lever funds in the sense of allowing funds to invest more than committed capital.

- **SBIC Loan:** SBA-guaranteed debt provided at the fund level to private capital funds that are designated participants in the SBIC program. Features include:
  - leverage at 2:1 debt/equity up to a cap of $175mm,
  - senior in right of repayment to all other LP & GP capital,
  - act as a form of low-cost incremental capital to invest in small businesses,
  - typically priced in the very low single digits.

SBIC loans effectively allow funds to invest more than LP committed capital at a specified 2:1 ratio up to a size constraint.

- **"Other" Fund-Level Debt**: There are a variety of other sources of debt that can provide incremental leverage at the fund level to meet borrowers' needs. Often these facilities are structured to meet fund investment needs that are constrained by the operating agreement or LPA. For example, a fund past its draw-down period may seek to invest incremental capital into a portfolio company to preserve or enhance value of the investment. The loan could be collateralized at a low LTV via a pledge of the underlying illiquid equity investments across the existing portfolio. The lender is effectively stepping in front of the LPs and GPs in right of repayment. These loans are typically priced in the mid-to-high teens or higher. Another example includes combination facilities that include characteristics of a subscription facility with a loan backed by portfolio company equity pledges.

Holding Company (HoldCo) Debt: Debt issued or borrowed at the holding company level that is structurally subordinate to all claims at the OpCo level. The debt is typically backed by a pledge of the equity collateral in the underlying portfolio company and guaranteed by relevant subsidiaries. Holding company debt is utilized to provide incremental leverage in a transaction when existing debt covenants preclude the addition of incremental debt at the operating company level. When viewed at the operating company level, all debt above the operating company is junior in all respects; effectively, ManagementCo/Fund-level/HoldCo debt behaves as if it were equity from the perspective of OpCo lenders. Pricing is typically in the very high single digits to double-digit range.

Operating Company (OpCo) Debt: Debt issued or borrowed at the operating company level. It can be structured as senior or junior, secured or unsecured, loan or bond, etc. What is typically recognized as the LBO debt in a leveraged buyout.

SPV Debt: Some operating companies will utilize SPV structures to finance their operations. These structures typically involve creating a SPV then transferring a specified set of collateral to the SPV, which is then borrowed against by the SPV. The OpCo makes a recurring "rent" payment to the SPV in exchange for use of the underlying collateral. The SPV structure is used to achieve more efficient forms of financing for the company in lieu of traditional OpCo financing structures. Examples include airlines, rental car companies, finance companies, etc.

Many of the specific channels for debt financing remain hard to study because of a lack of transparency, but a comprehensive knowledge of the landscape facilitates an understanding of how various stakeholders are impacted by leverage.

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Does Private Equity Over-Lever Portfolio Companies? *

Sharjil M. Haque †

This Version: November 2022

Abstract

Detractors have warned that Private Equity (PE) funds tend to over-lever their portfolio companies because of an option-like payoff, building up default risk and debt overhang. This paper argues PE-ownership leads to substantially higher levels of *optimal* (value-maximizing) leverage, by reducing the expected cost of financial distress. Using data from a large sample of PE buyouts, I estimate a dynamic trade-off model where leverage is chosen by the PE investor. The model is able to explain both the level and change in leverage documented empirically following buyouts. The increase in optimal leverage is driven primarily by a reduction in the portfolio company's asset volatility and, to a lesser extent, an increase in asset return. Counterfactual analysis shows significant loss in firm value if PE sub-optimally chose lower leverage. Consistent with lower asset volatility, additional tests show PE-backed firms experience lower volatility of sales and receive greater equity injections for distress resolution, compared to non PE-backed firms. Overall, my findings broaden our understanding of factors that drive buyout leverage.

Keywords: Private Equity; Capital Structure; Default Risk; Trade-off Theory

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1 Introduction

It is well-known that Private Equity (PE) funds acquire companies in leveraged buyouts (LBO) using substantial amount of debt (Kaplan and Stromberg, 2009). The sharp increase in portfolio company leverage following a PE-sponsored buyout has generated conflicting views.[1] One well-known view is that PE fund managers over-leverage their portfolio companies (Kaplan and Stein, 1990; Andrade and Kaplan, 1998), and buyout capital structure is primarily driven by credit market conditions instead of debt capacity of a given firm (Axelson, Jenkinson, Strömberg, and Weisbach, 2013). The alternate view is that high leverage is efficient since PE leads to lower debt-equity conflicts for a given debt ratio relative to public firms (Malenko and Malenko, 2015), thereby allowing PE-backed firms to trade off the benefits of higher debt against potentially lower expected cost of financial distress. Which effect dominates is thus an empirical question.

In this paper, I examine if PE investors systematically over-lever portfolio companies and estimate the *optimal* (value-maximizing) leverage of PE-backed firms. If PE sponsors over-leverage and overpay for deals as suggested by Axelson et al. (2013) and Axelson, Strömberg, and Weisbach (2009), we would expect optimal leverage of portfolio firms to be meaningfully lower than what we see in the data, which can lead to significant aggregate costs (Faria-e Castro, Paul, and Sánchez, 2021).

Examining this mechanism is challenging since we do not readily observe optimal leverage. Existing papers empirically examining leverage in PE rely on standard regressions of leverage on a number of factors that proxy costs and benefits of debt.[2] However, this approach cannot detect if firms have too much debt or too little debt on average and implicitly assumes firms are always optimally levered (Korteweg, 2010). Moreover, it cannot incorporate the endogeneity of the bankruptcy decision, which is jointly determined with leverage. The alternate approach is to structurally estimate optimal leverage.

[1] Throughout the text, I use the terms PE-backed, PE-sponsored, and PE-owned interchangeably to refer to portfolio firms.

[2] See for example Axelson et al. (2013); Guo, Hotchkiss, and Song (2011); Kaplan and Stein (1993).

As Ivashina and Kovner (2011) suggest, PE managers are effectively shadow-borrowers since they control the borrower's equity, management, capital structure and strategic direction. Consistent with this observation, if we relabel the PE General Partner (GP) as a CEO who chooses capital structure to maximize equity value taking into consideration a potentially different expected cost of financial distress, then a standard trade-off model with endogenous leverage could explain high buyout debt. Consequently, I estimate a dynamic trade-off model using data from a large, international sample of PE-owned portfolio companies covering pre- (post-) buyout financial information. Since the data allows me to estimate optimal pre- (post-) buyout leverage ratios, I can identify the underlying mechanism that explains a change in the optimal leverage. Additionally, the model allows me to examine tax benefits of debt and default risk following PE-intervention.

Using the post-buyout sample, I begin with my benchmark case and estimate the Leland (1994) structural model that considers trade-off theory with endogenous debt and default. However, a key hurdle I encounter is the need for market prices of PE-backed firms since estimation of Leland-type models involves recovering a firm's unobserved asset value and volatility such that they deliver its empirically observed equity value and volatility (Elkamhi, Ericsson, and Parsons, 2012; Bharath and Shumway, 2008; Nagel and Purnanandam, 2020). Given the absence of equity prices of private companies, I design a similar yet even more conservative matching methodology relative to Bernstein, Lerner, and Mezzanotti (2019) to identify nearly-identical public companies. Specifically, I match each PE-backed firm in my sample to public companies in terms of profitability, leverage, total assets and volatility of return on assets in the same country-industry-year. These variables are chosen to condition on factors that typically differentiate PE-backed firms from public companies.

My key finding is that private equity leads to substantially higher levels of optimal leverage. The estimation predicts mean and median post-buyout leverage ratios of 47.7 and 52.6 percent respectively, which matches the data quite well. Specifically, mean and median leverage in the data is around 50 percent, consistent with previous studies (Brown, 2021;

Gornall, Gredil, Howell, Liu, and Sockin, 2021). Re-estimating the model with pre-buyout data generates much lower optimal leverage ratio of around 33 percent, which is also close to pre-buyout levels. The model thus explains both the level and *change* in post-buyout leverage. In a counterfactual exercise, I find that the median firm in my sample stands to lose approximately 4.0 percent of value if they chose to stay at debt levels close to what is observed pre-buyout. The model predicts substantial cross-sectional heterogeneity in the cost of sub-optimal leverage with an inter-quartile range of 2.4 to 5.7 percent of value.

Next, I inspect the channel driving the results. The primary reason for the increase in optimal leverage is a sizeable reduction in estimated asset volatility, and to a lesser extent, an increase in asset return. I find that mean asset volatility declines from 0.309 pre-buyout to 0.177 post-buyout. Lower asset volatility reduces the firm's time-weighted probability of default and, by extension, the expected present value of bankruptcy costs (for a given default cost), thus raising optimal leverage.[3] The benchmark finding is consistent with the theory proposed by Malenko and Malenko (2015), who argue risk-shifting incentives are lower under PE-ownership in a setting where debt brings the standard tax and agency benefits as well as bankruptcy costs.

To support this finding, I also provide reduced-form evidence consistent with lower asset risk. I provide evidence on two (not necessarily mutually exclusive) channels: operational engineering and financial distress resolution through equity injections (Gompers, Kaplan, and Mukharlyamov, 2022a; Gryglewicz and Mayer, 2020). Using a matched difference-in-differences strategy with firm and year fixed effects to alleviate concerns related to selection, unobserved firm-specific factors and aggregate credit conditions, I uncover two key empirical facts. First, PE-firms receive greater equity infusion when default risk is high, relative to matched public companies consistent with Bernstein et al. (2019), Hotchkiss, Smith, and Strömberg (2021) and Haque, Jang, and Mayer (2022). This capital infusion comes from

[3] Prior research shows that in endogenous default models, the higher volatility and resulting lower-coupon effect dominates the opposing effect of higher coupon due to the likelihood of a firm finding itself in a very good state when it raises risk. See for example, Strebulaev, Whited, et al. (2012).

sponsoring funds with so-called dry powder since committed capital is typically invested over a series of years, rather than all at once. The key implication is that equity injection reduces distress risk, hence diminishes incentives to shift risk to lenders. This is the distress resolution channel.

Second, I show that PE-sponsored firms experience a reduction in the volatility of sales following PE-takeover: the operational engineering channel. Lower volatility in sales is consistent with findings in Fracassi, Previtero, and Sheen (2022), who use granular store-level data to show firms diversify both their product basket as well as geographic product market after PE-takeover, thus lowering risk. One concern with this finding, may be that PE managers may be manipulating accounting data to maximize fund-level risk-adjusted return, consistent with findings from Brown, Gredil, and Kaplan (2019). To alleviate this concern I show that my results are unchanged with a restricted sample of just a few advanced European countries where firms are required to disclose their financial statements and have them regularly audited (unlike firms in the US).

For completeness and robustness purposes, I extend the standard model in three additional directions to capture issues often associated with PE. First, Hotchkiss et al. (2021) show that PE-backed firms tend to negotiate out-of-court with lenders more often that similar non-PE firms, if they are in distress. Following Strebulaev et al. (2012), I estimate a simple version of a trade-off model where firms issue private bank debt and can negotiate its coupon payments in low-profitability states of the world instead of filing for a relatively more traditional chapter 11 bankruptcy. Second, PE-backed firms have also been accused of so-called asset-stripping (e.g. Reuters, 2010; The Gaurdian, 2021). The Leland (1994) model can capture liquidation of assets to fund higher payouts, allowing me to compute optimal leverage at different liquidation rates as a share of asset value. Third, as Ivashina and Kovner (2011) suggest, close relationships between banks and PE funds may loosen covenant violation thresholds, allowing for higher leverage. I introduce an Interest Coverage covenant in a parsimonious manner into the Leland (1994) model to examine this channel. In general, I find that these extensions are not as successful in explaining post-buyout leverage ratios as the benchmark model, indicating these factors may not be the primary driver of higher optimal leverage.

Finally, one might worry that higher optimal leverage arises from PE sponsor reputation (Ivashina and Kovner, 2011). The benchmark model can capture this effect through changes in loss given default. For example, higher PE sponsor reputation could potentially reduce loss of customers or limit fire sales in highly levered firms or firms with high default risk. In a comparative static exercise, I show that changes in dead-weight default costs do not generate the substantial change in leverage as the change in asset volatility does, and thus cannot explain the observed change in the data.

Related Literature. This paper contributes to a large literature on debt and leverage in private equity-sponsored leveraged buyouts. The extant literature on capital structure in PE has primarily focused on the role of aggregate market conditions (Malenko and Malenko, 2015; Bernstein et al., 2019; Axelson et al., 2013), reputational concerns (Malenko and Malenko, 2015; Huang, Ritter, and Zhang, 2016), deal returns (Brown et al., 2019; Brown, 2021), mechanisms in the initial year of operation (Robb and Robinson, 2014), agency conflicts between general and limited partners (Axelson et al., 2009; Gryglewicz and Mayer, 2020), and PE sponsor-lender relationships (Ivashina and Kovner, 2011; Jang, 2022). My paper is conceptually closest to Hotchkiss et al. (2021) who argue the expected cost of financial distress under PE-ownership is lower, given the standard trade-offs associated with choosing leverage. My paper differs from these by proposing and directly estimating the *optimal* leverage in buyouts, while previous papers have primarily theorized that optimal leverage could be different under PE, such as Malenko and Malenko (2015). To the best of my knowledge, this is the first paper to quantitatively examine optimal leverage in PE taking into account the endogenous nature of default and corporate debt policy.

I also contribute to the large literature on the effects of private equity buyouts. As suggested by Kaplan and Stromberg (2009), recent theories (Malenko and Malenko, 2015;

Gryglewicz and Mayer, 2020) or survey evidence (Gompers et al., 2022a), PE owners affect firm value and outcomes through operational, governance, and financial engineering. In this context, several papers study whether and how PE owners affect firm outcomes, managerial incentives, stakeholders, and/or create value (see, among others, Boucly, Sraer, and Thesmar (2011); Cronqvist and Fahlenbrach (2013); Cohn, Mills, and Towery (2014); Bernstein and Sheen (2016); Antoni, Maug, and Obernberger (2019); Gupta, Howell, Yannelis, and Gupta (2021); Gornall et al. (2021); Cassel (2021); Ewens, Gupta, and Howell (2022); Fracassi et al. (2022); Cohn, Hotchkiss, and Towery (2022); Haque et al. (2022)).I complement these efforts by empirically examining the effect of PE-ownership on underlying asset volatility, default risk as well as the tax and incentive benefits of debt.

Finally, this paper also contributes to the structural corporate finance literature. Prior studies which estimated structural leverage models have focused on cost of default (Glover, 2016), pre-default costs (Elkamhi et al., 2012; Elkamhi and Salerno, 2020), the effect of changes in tax rates on small firms (Ivanov, Pettit, and Whited, 2020) and collateral (Li, Whited, and Wu, 2016). Unlike these papers, I provide an examination of the quantitative effect of changes in asset volatility on capital structure, as well as the (counterfactual) effect choosing lower leverage. Unlike papers which have typically calibrated Leland-type models, I structurally estimate the model for proper inference.

2 Structural Model of Optimal Leverage

The key assumption I make in this paper is that the PE manager behaves similar to a profit-maximizing equity-holder. This is a reasonable assumption as Jensen (1986), and more recently Ivashina and Kovner (2011) and Gompers, Kaplan, and Mukharlyamov (2022b) argue, PE managers usually own a majority of the equity in the companies within their portfolio, take active roles in governance and operations, and seek to maximize the value of their investments because they are usually compensated with a large share of the profits of these investments. In this section, I begin by outlining a model of the leveraged firm following Leland (1994). Since the model is well known, I do not repeat detailed theoretical derivations here and only present key equations.[4]

Consider a firm in a continuous-time infinite horizon framework, whose manager maximizes shareholder value. At all times in which the firm is operating, its assets in place produce cash flows at a rate of δ_t, implying cash flows of $\delta_t dt$ are produced in each time interval $[t, t+dt]$. Assume there exists a risk-neutral measure with risk-free rate r under which cash flow rate follows a geometric brownian motion

$$d\delta_t = \delta_t \mu dt + \delta_t \sigma dB_t \tag{1}$$

where $\mu < r$, $\sigma > 0$ are constants representing risk-neutral drift and volatility of δ_t. In Eq. (1), B_t is a standard Brownian motion, which we can think of as random shocks to a firm's fundamental value. Since all value is generated by assets in place in perpetuity, and assuming the firm's capital structure only consists of equity, we can write the value of the unlevered firm as:

$$E_U(\delta) = \mathbb{E}\left(\int_t^{\infty} e^{-r(s-t)} \delta_s ds \right) = \frac{(1-\tau)\delta_t}{r-\mu} \tag{2}$$

where τ represents a constant proportional corporate tax rate.[5]

Now suppose the firm issues debt to take advantage of tax shields. Debt takes the form of a consol bond with constant coupon rate C. I follow the literature in assuming a full loss offset provision, so the firm subsequently pays taxes $\tau(\delta_t - C)dt$ per unit in time.[6]

[4] Readers interested in the theory can also see Leland and Toft (1996), Leland (1998), He (2011), Strebulaev et al. (2012) and Glover (2016), among others.

[5] One concern could be that the model cannot capture time-varying macroeconomic risk. Recall that the goal of this paper is to explain leverage ratios in the cross-section pre-(post-) buyout, as opposed to the time-series. Thus time-varying credit conditions should be less of a concern, however, I also provide tests in Section 6 that tackle this issue directly.

[6] Strebulaev et al. (2012) argue taxes are asymmetric in the real world, so that profits are taxed at a higher rate than losses. While I abstract away from this for simplicity in the benchmark model, unreported results confirm carry-forward or carry-back provisions of tax code does not change the main result of this paper.

2.1 Equity Value and Endogenous Default

Now, I outline the value process for the equity-holder's payoffs. In the context of this paper, I assume the PE fund manager acts as the owner-manager or equivalently is the equity-holder following an LBO. Equity value can be computed through the following ordinary differential equation that equates the required rate of return for the equity-holder with the expected rate of return on equity, which is the sum of the terms on the right hand side.

$$rE(\delta) = \delta_t - (1-\tau)C + \mu\delta\frac{\partial E}{\partial \delta} + \frac{1}{2}\sigma^2\delta^2\frac{\partial^2 E(\delta,t)}{\partial \delta^2} \tag{3}$$

The left hand side is the required equity return. The first term on the right-hand side captures the cash flow generated by the firm per unit of time. The second term is the after-tax coupon payment per unit of time. The third and fourth term capture the expected change in equity value caused by a fluctuation in the firm's asset value. Following a series of unexpected negative shocks that deteriorates the firm's financial status, the equity-holder may choose to default. Standard smooth-pasting condition yields the endogenous default-triggering asset level

$$\delta_B = (1-\tau)C\frac{r-\mu}{r}\frac{\gamma}{1+\gamma} \tag{4}$$

where γ is the root of the fundamental quadratic equation, defined below.

$$\gamma = -\frac{\mu - 0.5\sigma + \sqrt{(0.5\sigma^2-\mu)^2 + 2\sigma r}}{\sigma^2} \tag{5}$$

2.2 Debt Value and Optimal Leverage

The value of debt is given by Eq. (6) below. The first term on the right-hand side is the constant coupon flow to debt-holders if the firm is solvent. The second term is equal to 0 since debt takes the form of a perpetual bond, thus time-independent. The last two terms on the right hand side are defined similar to the equity-holder's value.

$$rD(\delta) = c - \frac{\partial D(\delta)}{\partial t} + (\mu)\delta_t \frac{\partial D(\delta)}{\partial \delta} + \frac{1}{2}\sigma^2 \delta_t^2 \frac{\partial^2 D(\delta)}{\partial \delta^2} \tag{6}$$

The value of the leveraged firm is the sum of debt and equity values. Simplification yields the following standard equation which effectively is the sum of the value of the unlevered firm, the tax benefits of debt less bankruptcy costs, thus capturing trade-off theory.

$$V_L(\delta) = \frac{\delta_0}{r - \mu} + \frac{\tau C}{r}\big(1 - (\frac{\delta}{\delta_B})^{-\gamma}\big) - \frac{\alpha \delta_B}{r - \mu}(\frac{\delta}{\delta_B})^{-\gamma} \tag{7}$$

We obtain the optimal coupon C^* by maximizing the levered firm value in Eq. (7). This is then used to compute optimal leverage shown in Eq. (8).

$$L_i = \frac{D(\delta, \delta_B, C^*)}{D(\delta, \delta_B, C^*) + E(\delta, \delta_B, C^*)} \tag{8}$$

3 Estimation Method

To find out if a trade-off model of optimal leverage can, on average, explain leverage ratios we see in PE-backed firms, I now estimate the model with empirical data from a large sample of PE firms. In this section, I describe the empirical strategy and sample construction.

3.1 Estimation

To estimate the model, I first set some parameters to typical values seen in the literature. Specifically, I set the risk-free rate to 5 percent Strebulaev et al. (2012). Following Leland (1998), Strebulaev et al. (2012) and He (2011), I set the corporate tax rate to 20 percent, which is appropriate given the international nature of the sample, described subsequently. Bankruptcy cost is set to 23 percent following Andrade and Kaplan (1998).

Estimation strategy is standard. The advantage of the Leland (1994) model is that it provides closed-form solutions for debt value, equity values and volatilities. The two key inputs to the model - asset value (δ_t) and asset volatility (σ) - cannot be observed. Instead they are inferred by requiring the model to fit observable data. Specifically, I calibrate the model for each firm-quarter by simultaneously solving Eq. (9) and (10) for δ_t and σ that deliver the observed values of a firm's quarterly equity and stock return volatility. This procedure has been widely used in prior research (Nagel and Purnanandam, 2020; Elkamhi et al., 2012; Bharath and Shumway, 2008; Vassalou and Xing, 2004).

$$E_{mkt} = E_{mod}(\delta_t; \sigma) \tag{9}$$

$$\sigma_e = \frac{\delta_t}{E_{mod}(\delta_t; \sigma)} \frac{\partial E_{mod}(\delta_t; \sigma)}{\partial \delta_t} \sigma \tag{10}$$

In Eq. (9) and Eq. (10), *mod* and *mkt* denote model and market values respectively. I estimate δ and σ using straightforward numerical solution, and compute bootstrap standard errors using 5,000 replications.[7]

The necessity of market prices presents a challenge unique to this paper since we are examining private firms. In an ideal experiment, one would proxy for market prices using identical public companies. My empirical design follows similar thinking. I develop a more conservative matching procedure relative to Bernstein et al. (2019) and estimate the model using market price of this sample of matched public companies. As I will argue below, selection on unobservable dimensions is likely less of a concern given my choice of matching covariates and the conservative nature of my match.

3.2 Data

The data collection process is divided into three parts. First, I collect private equity deal-level data from Bureau Van Dijk's (BvD) Zephyr database. Zephyr has been increasingly

[7] I use Matlab's built-in Levenberg–Marquardt algorithm to iteratively solve the model, using a convergence tolerance criterion of 10^{-3}. Computation of bootstrapped standard errors is carried out through a Linux-based computing system, which substantially reduces estimation time.

utilized among PE researchers and has been verified as a comprehensive and representative sample of PE transactions compared with other PE databases (Jenkinson and Stucke, 2011; Bansraj, Smit, and Volosovych, 2020). Zephyr includes information on deal confirmation date, industry classification, country of the portfolio company and sponsoring fund.[8] I retrieve all Private Equity transactions labelled Institutional Buyout or deals where financing is labelled Leveraged Buyout or Private Equity from 2000 to 2019. In doing so, I exluded all Growth Capital and Venture Capital deals.

Second, I match target firms with their annual company-level accounting data from Orbis, which has also been used in previous studies such as Bernstein et al. (2019). One advantage of Orbis relative to other firm-level BvD datasets (e.g. Amadeus) is that Orbis does not remove firms from the sample after a few years of inactivity. This is important since it minimizes selection concerns arising from a substantial number of firms exiting after the financial crisis. I use information on deal-confirmation date from Zephyr to identify the pre-buyout and post-buyout years. I exclude firms in the utilities, financials and public sectors. I restrict the sample to firms with data on book assets, short-term debt, long-term debt, sales and cash and cash-like assets for the sample period. In addition, I require firms to have accounting data in at least the two years immediately preceding a buyout. Excluding firms that did not meet this minimum data criterion led to an initial sample of 1,383 PE-backed firms in the post-buyout sample. Next, I exclude firms that does not meet the requirements for the matching algorithm described below, leading to a final sample of 731 firms. As will be described below, variations in the matching criterion lead to higher or lower samples but does not change the main results of this paper.

All variables are defined in Table A1 in the Appendix. To minimize the effect of outliers, all variables are winsorized at the 1 and 99 percent level. Table A2 in the Appendix shows key moments such as asset value, net leverage and industry composition are quite comparable in the full and the matched sample. I focus on net leverage (henceforth, leverage) in this

[8]When sponsoring fund information is missing, Zephyr includes the name of the acquiring company which I use to pin down the sponsoring fund from public sources.

paper because PE buyout managers typically consider debt minus cash and cash-like assets when considering companies for leveraged buyout targets.

I also verify that key moments are comparable to other studies. For example, Brown (2021) report net leverage, measured as debt minus cash and cash-like assets over enterprise value, of 51 percent immediately following a buyout. First, as Table A2 shows, leverage ratio in my sample is quite similar and stands at 49.2 and 49.5 percent respectively in the full and matched samples respectively. Second, The sample is also consistent with the literature in the time series. Brown (2021) document that leverage nearly doubles following a buyout. As Figure 1 shows, leverage in this sample displays a similar pattern in the year following a buyout.

[Insert Figure 1 Here]

A key part of the empirical strategy relies on data of comparable (matched) public companies that have observable market prices. Hence in the third step, I retrieve financial data on non-PE backed public companies from Compustat (North America and Global). I restrict the Compustat data to the same sample period and data availability requirements mentioned above for the PE-backed firms. Since my PE-sample is at the firm-year level, I obtain accounting data for public companies at the annual frequency. However, for equity price and shares outstanding, I retrieve daily data. Further details on non-PE companies are provided below in section 3.3.

3.3 Matching Procedure and Sample Characteristics

PE-backed companies are not a random sample of the population. For instance, they are like to be larger and more leveraged than the average firm. Following Bernstein et al. (2019), I find a suitable sample of comparable public companies using a matching algorithm. For each year a firm is under PE-ownership, I find at most 5 non-PE owned public companies, if available, in Compustat that (a) was in the same country, (b) belonged to the same

2-digit NAICS industry, (c) had return on asset (ROA), leverage, total book assets and volatility of ROA within a *10* percent bracket around a PE firm-year. The key difference from Bernstein et al. (2019) is that I include the volatility (standard deviation) of profits as a matching covariate and I require a much tighter match relative to their 30 percent bracket. Reasonable variations of this matching procedure with fewer or additional variables leads to moderate changes in matched sample size, but does not change the key result of the paper.[9]

The key concern with using market prices of the matched sample is selection of PE-backed firms based on dimensions we cannot observe in the data. For example, the traded price of a PE-backed company may be influenced by whether or not it is backed by a reputed private equity sponsor.[10] Demiroglu and James (2010) suggest reputation can be proxied by performance, which in this context is captured by ROA. Specifically, since performance is persistent in PE (Kaplan and Schoar, 2005), firms backed by more reputable sponsors are likely to be relatively more profitable. Similarly differences in risk-shifting or incentives due to PE-ownership is likely to be captured by matching on volatility of ROA.[11] Nevertheless, in section 6, I provide reduced-form evidence using PE-firm data consistent with the main mechanism that will drive the key result in my benchmark structural estimation.

[Insert Table 1 here]

Table 1 compares firms backed by PE and matched non-PE backed public firms. My matching algorithm leads to a match of 731 PE-owned firms with around 2,900 firm-year observations for key matching covariates in the post-buyout sample. The matched public firm sample has around 6,500 firm-year observations. We can see that the matching is quite effective in ensuring the two samples are very similar. There is no statistically significant difference in means across the two samples, and standardized percentage bias is less than or

[9] For instance, I introduced sales growth as a matching covariate which decreased our matched sample, but left our main results unchanged.

[10] In other words, investors may value a firm owned by Kohlberg Kravis Roberts Co. more highly than an identical firm owned by a less reputable sponsor.

[11] It is also plausible that any remaining confounding effect that is not captured by any of the four matching covariates are likely to have relatively smaller effect on equity prices since investors are likely to put more weight on observable financial data when determining market price.

equal to 5 percent for all matching covariates. Further inspection shows mean leverage ratio for the two samples is around 48 percent. This is also consistent with prior literature.[12]

We can also see both the median and standard deviations of matching covariates in the public company sample are quite similar. For example, median ROA volatility in the PE and matched samples are 0.055 and 0.054 respectively. Mean leverage ratios are quite close as well. Mean ROA is 3.4 and 3.2 percent for the PE and matched public sample, while the median ROA is somewhat higher for the matched public firms.

How does the matched PE sample used in the analysis compare with the unmatched full sample? Table A2 in the Appendix shows the samples are quite comparable both in terms of portfolio company characteristics and industry characteristics. For example both book assets and leverage ratios are quite similar. Manufacturing companies dominate both samples, although they are somewhat more frequent in the sample used in the analysis. Overall, the sectoral composition is qualitatively similar across both samples.

4 Benchmark Results

Because we are primarily interested in deriving an optimal leverage value consistent with trade-off theory, we need only two model inputs: market value and volatility of equity. Armed with the matched sample of public firms, I estimate the model for each firm-quarter by matching model-implied equity value and volatility with the observed market capitalization and historical equity volatility of the matched sample. Market capitalization is simply share price times number of shares outstanding while equity volatility is the standard deviation of daily (historical) price return. I set the drift rate, μ=1.78%, which is estimated directly from the data using mean historical equity return.

[12] For example, Gornall et al. (2021) use data from Stepstone SPI and Pitchbook respectively and find their sample has leverage ratio of approximately 50 percent, where leverage ratios are defined similarly.

4.1 Post-Buyout Optimal Leverage

Table 2 reports benchmark estimation results using over 32,000 firm-quarter observations. First, Panel A reports model inputs. The median equity value is USD 68.5 mn while the mean is USD 235 mn. Median equity volatility is estimated at 18.5 percent while the mean is higher at 26.2 percent.

[Place Table 2 Here]

Panel B reports my key results. I begin by tabulating the estimated mean, median, 25*th* and 75*th* percentile asset volatility, along with bootstrapped standard errors. The recovered asset volatility moments are marginally lower than their respective equity volatility. For example, mean asset volatility is 23.8 percent while the median is 18.4 percent. Using estimated asset volatility I derive model-implied optimal leverage using an initial asset value of 100. It is worth recalling the the Leland (1994) model is scale invariant so initial asset value choice does not affect leverage ratios.

Row 2 in Table 2 reports mean, median, 25*th* and 75*th* percentiles of optimal leverage. For example, using the median estimated asset volatility, the model predicts optimal leverage ratio of 50 percent. Similarly, using the 75*th* percentile estimated asset volatility, I find a much lower optimal leverage ratio of 32.2 percent. The 25*th* percentile asset volatility predicts a much higher optimal leverage ratio of 59.4 percent. The negative relationship between asset volatility and optimal leverage is well-known in the literature, but the key question is how these predicted leverage moments match with the real data. Row 3 tabulates mean, median, 25*th* and 75*th* percentiles of leverage ratios in the actual post-buyout sample. As can be seen, the model is quite consistent with the data. Median leverage ratio in the post-buyout data is 49.6 percent, while the mean is 47.8 percent. The model also is consistent with the first and third quartile predicted leverage ratios. For example, the 25*th* percentile leverage ratio in the data is 32.4 compared to 32.2 predicted by the model. Mean leverage in the data and the model are also quite comparable.

The next row reports the ratio of the default boundary to the cash flow generated in each time period, $\frac{\delta_B}{\delta_t}$. We can see the mean boundary to value ratio is 0.361, which is quite comparable to Elkamhi et al. (2012) who estimate the Leland and Toft (1996) model and find a mean ratio of 0.29. That being said, I find the median boundary to value ratio is much higher compared to their estimation, reflecting the skewed nature of the asset value distribution and higher leverage in PE-backed firms. Next, median and mean distance-to-default, computed following Bharath and Shumway (2008), is 2.2 and 2.5 respectively. A natural question is if distance-to-default is exceptionally low for firms closer to the tail of the distribution. To shed light on this question, I plot distance to default for the entire sample in Figure 3. I do not find evidence of a non-trivial share of firms with distance to default lower than 1.

[Insert Figure 3 Here]

Finally, I compute the tax benefit of debt scaled by the un-levered value of the firm as follows:

$$Tax\ Benefit = \frac{\frac{\tau C}{r}\left(1 - (\frac{\delta}{\delta_B})^{-\gamma}\right)}{\delta_t/(r-\mu)} \tag{11}$$

where the numerator is simply the variables which capture discounted tax benefits of the levered firm value from Eq. (6). The model predicts high leverage ratios do indeed generate significant tax benefits. Median and mean tax benefit of debt is approximately 20 percent of unlevered value.

As will be shown subsequently, changes in asset volatility and resulting optimal leverage effectively capture agency benefits of debt as well. While the Leland (1994) model does not explicitly model agency costs, subsequent analysis in this paper using the pre-buyout sample will reveal part of the value from debt is consistent with agency benefits Jensen (1986), in addition to tax benefits.

4.1.1 Parameter Sensitivity

One concern with the benchmark estimation is choice of value of calibrated parameters, which were set according to the literature. Since researchers have used a range of values in the past, it is worth examining how sensitive optimal leverage ratios are to the choice of r, τ, and α which were not estimated (unlike δ and σ). In addition, since the previous literature has used several proxies to approximate μ, I also check the robustness of the result with respect to changes in mean drift. In this section, I examine the change in optimal leverage ratio due to a 20 percent increase in one of these four parameters, while setting the remaining parameters to values used in the benchmark estimation. Since the effects on leverage could be asymmetric, given the highly non-linear nature of the model, I also repeat the exercise using a 20 percent decrease.

[Insert Table 3 Here]

Table 3 reports these results. Overall, the directional change is consistent with Leland (1994). Increase in risk-free rates raises the tax benefits of debt. However, if we examine the first row we can see that the effect of a 20 percent increase in risk-free rate on optimal leverage is quite small, given the estimated asset volatility and asset value. The median leverage in this case is 54.1 percent relative to 50 percent in the benchmark case. We note similar patterns in the first and third quartiles. Similarly, the effect of different choice of bankruptcy cost value is also relatively small. The model's predictions on asymmetric effect of an increase relative to a decrease in parameter value is also quite small. Change in μ also does not change optimal leverage drastically. A 20 percent increase in mean estimated drift leads to only a 1.5 percent point increase in leverage (at the median) as shown in row 3. In an additional exercise, I ask what level of drift would be required to lower model-implied leverage closer to standard public company debt levels, conditional on the estimated volatility level. Figure A1 in the Appendix reports results from this exercise through a scatter plot of asset return and leverage. We find at the estimated asset volatility level, drift would have to be significantly negative to match public company leverage ratio.

Relative to risk-free rates, tax rates tend to have a larger effect on optimal leverage. Median optimal leverage rises to 57.7 percent following a 20 percent increase in tax rate, and declines to 47.7 percent following an equivalent decrease in the tax rate, as presented in the last row of Table 3. Nevertheless, these changes are not significantly different from the benchmark model predictions and the overall takeaway from this exercise is that the choice of calibrated parameter values cannot explain the large change in leverage we observe in the data following buyouts.

4.2 Pre-Buyout Optimal Leverage

In this section, I investigate if the benchmark results documented thus far are due to selection effects, that is PE funds select companies with relatively higher levels of optimal leverage, or due to changes brought about by PE-ownership. Two key parameters can significantly shift optimal leverage when asset value follows the standard log-normal diffusion process: (i) asset return/drift, and (ii) asset volatility. On drift rate, there is extensive literature that shows PE-owned firms are more efficient and profitable given better management and more aligned incentives (Bernstein and Sheen, 2016; Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda, 2014). On asset risk, Malenko and Malenko (2015) show that for a given leverage ratio, debt-equity conflicts could be less severe when a firm is owned by a PE sponsor relative to a non-PE owned public company with dispersed shareholders. They argue because PE-owned firms borrow against both its own assets and the sponsor's reputational capital, debt-equity conflicts stemming from risk-shifting are lower relative to non-PE or independent firms with similar leverage ratios. Their theory is consistent with Ivashina and Kovner (2011) who find PE investors have close relationships with banks and lenders. It follows that PE-backed firms experienced a reduction in asset volatility from risk-shifting activities following an LBO.

Using the benchmark model and pre-buyout financial data, I estimate asset volatility, drift and corresponding optimal leverage *before* companies in my sample are taken over by PE funds. My estimation strategy is the same as that described in the previous section. I find similar matched public companies and draw on equity value and volatility from the pre-buyout matched sample to estimate the model. I restrict the match to years $t-1$ and $t-2$ where t is the year that a company undergoes an LBO. Table A3 in the Appendix provides descriptive statistics of the PE-backed firms and their matched counterparts in the two years before buyout. First, as can be seen from examining the mean and median of the treated and control group, both samples are quite comparable. For instance, the mean leverage ratio in the PE-owned firms before the buyout is 29.9 percent and 28.7 percent in the matched group. The standard deviations are also quite comparable.

Crucially, we also note that median profitability and profit volatility are markedly differently in the pre-buyout sample. Comparing with corresponding moments in Table 1, we observe the median pre-buyout firm's ROA is approximately 2 percentage points lower than the median post-buyout firm, while volatility of ROA is higher. While, these univariate figures cannot be used to interpret any PE-effect, they are qualitatively both consistent with the literature on private equity's effect on value and risk as well as indicative of the underlying mechanism at play in the benchmark model.

4.2.1 Results and Discussion

The results of the pre-buyout estimation are reported in Table 4. Using over 9,000 firm-quarter observations, I estimate asset volatility and bootstrapped standard errors using 5,000 re-samplings.[13] I find the Leland (1994) model predicts substantially higher asset volatility distribution for the pre-buyout sample. For example, median and mean recovered asset volatility stands at 0.309 and 0.303, which is approximately 50 percent higher than the estimated σ_v post-buyout. Using these estimated asset volatilities, I again generate a distribution of optimal leverage ratio. The model predicts median optimal leverage of 33

[13] The pre-buyout estimation sample is relatively smaller since I match on only the two years before buyout.

percent, which is nearly identical to the data as can be seen in Panel B. Mean predicted leverage ratio is also quite similar at just under 30 percent. Counterfactual analysis, discussed in Section 4.3, will show much of the increase in optimal leverage is driven by lower asset volatility.

[Insert Table 4 Here]

Looking at the $25th$ and $75th$ percentile, we find the model struggles somewhat to explain the data. One interpretation is that firms for example in the first quartile are systematically under-leveraged pre-buyout. Alternatively, one might infer that the Leland (1994) model is particularly successful in explaining PE leverage ratios in the sample median (mean). Not surprisingly, we find the boundary-to-value ratio is much lower pre-buyout given less debt. On the other hand, the tax benefit of debt as a consequence of lower leverage is also lower: mean tax benefit to unlevered value is approximately half of what we observed in the post-buyout sample.

The reduction in asset volatility is consistent with the theory proposed by Malenko and Malenko (2015) and empirical evidence shown by Haque et al. (2022) on lower earnings volatility in a large sample of bank-dependant U.S. PE-backed firms. PE managers have lower incentive to shift risk to debt-holders relative to managers in a public firm due to repeated deals and, consequently, greater reliance on lenders for continued deal flow. Another, not necessarily mutually exclusive view is that higher risk pre-buyout is driven by inefficient management of free cash flows, first proposed by Jensen (1986). While the model is not intended to differentiate between these mechanisms, my estimation strategy uncovers a change in asset volatility consistent with these views. Moreover, Section 6 provides reduced-form empirical evidence consistent with the reduction in σ_v. A key implication is that, by reducing underlying risk, PE managers reduce the chances of bankruptcy which reduces the expected cost of financial distress consistent with trade-off theory.

4.3 Counterfactual Analysis

Since much of the criticism of PE centers around high leverage ratios, a natural counterfactual analysis is to quantify the difference in firm value if firms deviated from optimal leverage. In particular, I examine the cost of choosing sub-optimal leverage. Specifically, I examine the loss in leveraged firm value if a PE-backed firm chose lower leverage despite lower asset volatility and higher asset return post-buyout. Put differently, what is the cost to the firm if it does not behave according to the trade-off theory?

I proceed by re-running the model such that each PE firm chooses half the amount of the optimal model-implied coupon, given their estimated asset volatility and asset return. While at first pass this coupon choice may appear arbitrary, it mirrors the typical difference in leverage between PE-backed and public companies. For each firm, I estimate levered firm value V_L^{sub} corresponding to this sub-optimal leverage ratio. Letting, V_L^* denote levered firm value at the PE firm's optimal leverage ratio given the estimated asset volatility, I compute cost of deviating from optimal leverage as the difference between V_L^* and V_L^{sub}.

[Insert Figure 2 Here]

The results are plotted in Figure 2. The blue bar quantifies levered firm value when the firm chooses half the model predicted optimal coupon. The red bar reports levered firm value at the optimal coupon. All parameterizations are otherwise identical to the baseline post-buyout estimation. Thus, I interpret the difference as the cost of choosing optimal leverage. Not surprisingly, I find there is indeed a cost of deviating from optimal leverage. Beginning with the bars on the extreme left which reports the levered firm value given the $75th$ percentile asset volatility estimate post-buyout, we note firms stand to gain by reaching the higher optimal leverage. We also note that the cost of deviating from optimal leverage rises as asset volatility declines. For example, difference in levered firm value at the $75th$ percentile (estimated from asset volatility at the $25th$ percentile) is higher relative to the estimates at the median. While this is not particularly surprisingly given that Leland-type models predict higher value from lower volatility on average, the question worth examining is how large is this cost.

[Insert Table 5 Here]

Table 5 reports the difference in the two values as percentage of the sub-optimal leveraged firm value. We find the cost is non-trivial. For firms in the median of the cost distribution, deviating from optimal leverage can cost up to 4.0 percent of value, and can go upto 5.7 percent for those in the lower end of the risk distribution. In other words, for firms that achieve substantial reductions in risk, choosing lower leverage is much more costly than those with higher risk. It is interesting to note that the cost of deviation estimated from the model is quite comparable to the literature. Specifically, Korteweg (2010) finds that the net benefits to leverage is approximately 5.5 percent for a representative firm.

5 Model Extensions

Thus far, we have shown optimal leverage ratios estimated from a trade-off model is, on average, consistent with PE-firm leverage. In this section, I study simple extensions to the standard trade-off model to examine two issues often associated with PE-sponsored buyouts.

5.1 Debt Covenant

One alternate explanation behind high buyout leverage could be a weaker covenant setting due to close relationships between lenders and private equity sponsors (Ivashina and Kovner, 2011; Achleitner, Braun, Hinterramskogler, and Tappeiner, 2012; Demiroglu and James, 2010). For instance, it could be that banks set looser covenant violation thresholds for PE-sponsored deals, thus raising covenant slack relative to public firms and allowing higher leverage. We would thus expect a trade-off model with covenants to explain the data. To examine this possibility, I extend the Leland (1994) framework to incorporate covenants and re-estimate the model.

Specifically, I model an interest coverage threshold which the literature has shown is one of the most prevalent types of covenants (Greenwald et al., 2019). For parsimony, I follow Strebulaev et al. (2012) who propose simple extensions to the Leland (1994) model to capture an exogenous default threshold because the firm violates a net-worth covenant. The starting point is the observation that $\frac{\delta_t}{C}$ effectively captures a coverage ratio (EBITDA/Interest Expense) and thus replacing the optimal default barrier with a covenant violation threshold can incorporate coverage ratio covenants in a simple and parsimonious way.

$$\delta_t = \theta C \tag{12}$$

Setting $\theta C = \delta_B$, I derive the optimal coupon by maximizing Eq. (7). Details of the derivation are presented in Appendix A9. I re-estimate post-buyout asset volatility and leverage similar to the baseline. All calibrated parameters are the same as before except I need to set a value for the covenant violation threshold, θ. I set $\theta = 2.63$ following findings in Bräuning, Ivashina, and Ozdagli (2022), whose sample of 119 maintenance covenants require borrowers to maintain the coverage ratio at that level.

[Insert Table 6 Here]

Table 6 presents results from this estimation. The asset volatility estimates are largely unchanged in the post-buyout sample. To wit, compared to our benchmark estimation, this version of the model predicts median asset volatility of 0.192, marginally higher than the 0.177 estimate reported in Table 2. However, when I estimate optimal leverage with this new asset volatility, the model does a below par job of matching the data. Median leverage predicted by the model post-buyout is less than 10 percent, given the calibrated value of θ and other parameters.

Why is optimal leverage so low with interest coverage covenants? The key reason is that setting an exogenous default threshold which is equivalent to relaxing the deep-pockets assumption leads the agent to declare default much earlier and lowers optimal leverage. As long as $\theta < 1$, the agent can inject equity. The greater the default threshold in a covenant, the earlier an agent declares default. In this case, we set $\theta = 2.63$, thus forcing default much earlier.

In the context of the key hypothesis motivating this exercise - PE sponsorship can loosen covenant threshold - we can now use this version of the model to ask what level of violation threshold would lead to observed PE leverage, conditional on the estimated asset volatility levels? This question is plausible since the Bräuning et al. (2022) sample likely captures both sponsored and non-sponsored firms. In other words, would a much lower covenant violation threshold lead to observed PE buyout leverage?

[Insert Figure 4 Here]

We plot the sensitivity of optimal leverage to covenant violation threshold, θ, in Figure 4. We observe a highly non-linear relationship between θ and optimal leverage. θ close to 1 leads to leverage ratios around 20 percent, which is somewhat comparable to many public company leverage ratios. On the other hand, we note that it would require violation thresholds significantly lower than 1 to match observed PE leverage, given all other parameter values. This would imply equity issuance cost for PE-backed firms is lower than public firms, which is plausible given PE funds' deep-pockets (Bernstein et al., 2019; Hotchkiss et al., 2021). Crucially, at $\theta < 0.4$, this version of the trade-off model also does a reasonable job of matching buyout leverage, conditional on the estimated asset volatility parameter.

5.2 Renegotiable Bank Debt

Second, previous studies have shown that PE-backed firms tend to avoid bankruptcy court more often, and liquidate less often compared to non-PE backed, highly leveraged firms experiencing financial distress (Hotchkiss et al., 2021). One potential explanation for why higher leverage is optimal in PE-backed firms could be PE sponsors' ability to negotiate out-of-court with lenders. However, a standard property of the Leland (1994) model, as well as its variants such as Leland and Toft (1996) or Leland (1998), is that debt-holders take control of the company if equity-holders default and crucially, there is no scope for renegotiation. Put differently, debt in the benchmark model is defaultable public debt and does not take into account private debt contracts, such as bank debt. In this section, I estimate a simple extension of the standard trade-off model with bank debt to examine if post-buyout leverage can be explained by the ability of borrowers to renegotiate debt.

To keep my analysis parsimonious, I follow Strebulaev et al. (2012) and assume the firm only has bank debt outstanding and has full bargaining power with the lender. Since the model is well-known, I only discuss its key features in this section and refer readers to Appendix A8 for further details. In this model without defaultable debt, the firm simply negotiates its coupon payments in low-profitability states of the world. This negotiation occurs at an asset level which the equity holder chooses by maximizing equity-value payoff, similar conceptually to the endogenous default point discussed earlier. Crucially, due to the assumption that the firm has full bargaining power, the firm renegotiates to keep the value of bank debt at its reservation value. The value of the leveraged firm is the sum of the unleveraged value in perpetuity and the tax benefits of debt. Similar to Leland (1994), the firm chooses an optimal coupon by maximizing levered firm value.

I report mean and various percentiles of leverage, keeping all the parameter values fixed at their benchmark quantities. I report the results in Figure 6 for two bankruptcy cost values: the baseline case of 0.23 and a lower bankruptcy cost of 0.1. My results indicate that the extended model with bank debt produces much lower leverage ratios when bankruptcy cost is held at the benchmark value. As can be seen from the bottom four rows in Figure 6, mean and median optimal leverage is much lower relative to the baseline estimation, and by extension, the data. One interpretation is that this model predicts PE-backed firms are over-levered. On the other hand, when I lower default cost to 10 percent, not implausible given the arguments in Hotchkiss et al. (2021), optimal leverage is much closer to the baseline.

5.3 Asset Liquidation and Dividend Payout

Third, in the main analysis, net cash outflows associated with the leverage decisions must be financed by selling additional equity, consistent with bond covenants restricting firms from selling assets. In other words, there are no net cash outflows resulting from payments to debt or equity-holders. However, PE investors are often accused of asset stripping, to the point where the EU has implemented a directive to stop this type of activity (e.g. Reuters, 2010; The Gaurdian, 2021). Asset stripping typically involves selling off individual assets to generate dividend payouts for investors.

To capture liquidation of assets to fund higher payouts, I follow Leland (1994) and consider the case of cash outflows that are proportional to asset value. This leads to a lower effective drift rate of $\mu^{'} = \mu - d$, where d is the payout rate as a share of asset value, δ. The only key change is that $\mu^{'}$ replaces μ in the root of the fundamental quadratic equation, γ outlined in Eq. (5). I consider two cases where $d = 0.01$, similar to Leland (1994), as well as $d = 0.02$. These liquidation rates are equivalent to approximately 2 percent and 4 percent payout on equity value respectively, based on the median leverage ratio predicted in the baseline case.

The rest of the estimation procedure is unchanged. I report the results in Figure 5 comparing optimal leverage ratios from the baseline post-buyout results with the extended version capturing asset liquidation in order to meet higher payouts. The top four moments capture the exercise where $d = 0.01$, and the bottom four moments are the ones with $d = 0.02$. Not surprisingly, we observe a decline in optimal leverage ratio in both cases and a higher decline when the asset liquidation rate is higher.

The key question is does asset liquidation substantially lower optimal leverage ratios? The answer appears to depend on what we consider as an appropriate liquidation rate. When the payout rate is 1 percent on asset value, consistent with Leland (1994), the change is quite small. For example, leverage ratios decline by approximately 2 percentage points at the mean and median. In fact, median leverage ratio of 50 percent with $d = 0.01$ is nearly identical to median leverage ratio in the data (49.8 percent) as reported in Table 1.

When I raise asset liquidation rate to 2 percent, median and mean optimal leverage ratio declines to 47.6 and 42.9 percent respectively. While the median is still quite close to the data, the mean optimal leverage is now much lower relative to mean leverage in the data. One interpretation of this result is that if PE investors exercise high asset liquidation rate in order to pay themselves dividend, then there is some moderate evidence of over-leveraging since actual mean leverage is higher. It is also worth observing that when $d = 0.02$, the disagreement between the baseline and the extended model appears to be more much pronounced at the first quartile, but is much smaller at the third quartile.

6 Reduced-form Evidence

The results so far are consistent with the idea that private equity can lower expected cost of financial distress by lowering underlying asset volatility of portfolio companies. Admittedly, one limitation of the benchmark model is that asset volatility is not endogenous.

In this section, I provide reduced-form evidence consistent with the idea that private equity can lower asset volatility. First, I show PE-ownership leads to a reduction in the volatility of sales consistent with an operational engineering channel (Gryglewicz and Mayer, 2020). Second, consistent with better distress resolution (Hotchkiss et al., 2021), I show PE-backed firms receive additional equity injection (relative to matched controls) whenever they are in financial distress. Equity injection during financial distress implies a reduction in incentives to engage in asset substitution or risk-shifting.

Operational Engineering Channel: Lower Sales Volatility. Fracassi et al. (2022) use store-level data to show PE-backed firms launch new products and expand their geographic reach relative to comparable controls. This diversification is consistent with a reduction in the volatility of sales and a reduction in volatility of the unlevered value of a firm in capital structure models. Thus, as a first exercise, I show the volatility of sales declines under PE-ownership relative to matched controls. I create a matched control group using the methodology described in Section 3.3, to address selection concerns, and run the following difference-in-differences regression specification.

$$Y_{it} = \alpha_i + \delta_t + Post_{it} + Post_t \times LBO_i + X_{it} + \epsilon_{it} \tag{13}$$

where the outcome variable is the *standard deviation* of a firm's Sales, scaled by Earnings Before and Interest Taxes, computed separately in the pre-(post-) buyout samples. LBO_i is a dummy taking value 1 if a company was ever owned by PE investors, and $Post$ is a dummy for the period following a PE-sponsored buyout. If the observation is a matched control firm, $Post_{it}$ equals 1 when the PE portfolio company matched to i has undergone an LBO, and 0 before. Furthermore I augment our specification with a set of firm covariates, firm (α_i) and year (δ_t) fixed effects. My estimation strategy thus controls for channels that have been documented as important drivers of buyout leverage: (i) economy-wide credit conditions (Axelson et al., 2013), the rise of structured credit (Shivdasani and Wang, 2011), (iii) fund managers non-randomly targeting specific firms and (iv) unobserved time-invariant factors.

[Insert Table 7a Here]

I report the results in Table 7a. We present results that iterate between various combinations of fixed effects and firm-controls. In column 1 for example, we include only firm fixed effects to capture time-invariant unobservable firm-level factors that can effect our outcome of interest. We observe a large and negative coefficient on the difference-in-differences estimator, $Post \times LBO$, indicating a reduction in sales volatility under PE-ownership. We also note that the $Post$ variable is positive and highly significant, suggesting these firms were on track to experience higher volatility but PE-ownership reduced this effect. In column (2) we drop firm fixed effects but include year fixed effects and immediately see a large drop in R^2, implying a lot of the variation does indeed come from time-invariant firm-level factors. Crucially, in column (3) we include both fixed effects and find that our coefficient of interest is still highly significant and negative. The estimate barely changes when we include time-varying firm-level controls including the natural log of total assets, leverage and ROA, signifying that the result is quite robust.

However, one concern with a reduction in sales volatility is a possibility that PE fund managers may manipulate accounting data to maximize risk-adjusted return at the fund-level, which in turn can lock-in greater capital from marginal investors in the future. Indeed, Brown et al. (2019) show PE fund managers can inflate fund returns during fund-raising, especially if the manager is under-performing. While my analysis is at the portfolio-company level, one could plausibly have similar concerns, since the location of private firms substantially affects its financial reporting environment. For example, private firms in the United States and Canada, are not required to make their financial reports public nor have them audited (Minnis and Shroff, 2017). On the other hand, most middle-market and larger European firms are required to both disclose their financial statements and have them regularly audited.

I thus repeat my analysis on sales volatility by restricting the estimation sample to the following European countries: Spain, Italy, France, Germany and UK. The rationale is that the need for auditing will lower systematic manipulation. Table A4a in the Appendix provides estimates with this sub-sample. As can be seen, although the estimates are somewhat smaller, they are still economically meaningful and highly significant. The only specification where the estimate is not significantly different from zero is where I do not include firm-level fixed effects or time fixed effects. Including firm and year FE, as well as firm-level time-varying control in column (4) yields an estimate of -0.308, which is significant at the 1 percent level.

Distress Resolution Channel: Equity Injection. One mechanism that can explain lower asset volatility and expected cost of financial distress is deployment of fresh capital into a distressed firm. Because PE groups raise funds that are drawn down and invested over multiple years—commitments that are very rarely abrogated—they may have "deep pockets" during downturns (Bernstein et al., 2019). These capital commitments may allow them to make equity investments in their firms when accessing other sources of equity, or financing in general, is challenging. Equity injection during financial distress can explain why debt overhang is less for a given leverage ratio, and reduces the equity-holder's incentive to shift risk to debt-holders when in financial distress. Put differently, capital infusion resolves financial distress more quickly and thus reduces a classic asset substitution problem at high leverage ratios.[14]

Following prior studies, I show that PE-backed firms in my sample receive greater capital injection relative to comparable non-PE companies. I proceed as follows. I define an indicator variable *Distress* as follows:

$$Distress = \begin{cases} 1 & \text{if Altman Z-Score} < x \\ 0 & \text{otherwise} \end{cases}$$

where the Altman Z-score is computed at the *company-year* level and x is a positive constant.[15] Using this *Distress* variable, I estimate Eq. (14) below where the dependant variable is Net Equity Contribution/Asset at the firm-year level. Equity Contribution is defined as the difference in total Book Equity over the past year, minus profit following Bernstein et al. (2019).[16] I introduce a triple interaction between *LBO*, *Post* and *Distress*. All second-order interactions are also included, unless they are absorbed by fixed effects. A positive coefficient is indicative of PE-backed firms receiving additional equity contribution compared to a matched control group when they are in financial distress. To summarize, I

[14] The literature argues the motivation to inject fresh equity comes from PE sponsors being repeat players in the buyout market; recurrent episodes of costly financial distress could harm reputations with lenders, fund investors, and other stakeholders

[15] Since I do not observe data on Retained Earnings, I proxy with Cash flows which Orbis (2007) defines as Profit for the Period plus Depreciation.

[16] For profit I proxy with Cash Flows in period t. I also verify that my results are not affected if I used other measures of Profit such as Profit Before Taxes. These are available upon request.

estimate variants of the following triple-interaction equation:

$$Y_{it} = \beta_1 Post_{it} + \beta_1 LBO_i \times Post_{it} + \beta_2 LBO_i \times Post_{it} \times Distress_{it} + \gamma^{'}\mathbf{X}_{it} + \alpha_i + \delta_t + \epsilon_{it} \quad (14)$$

[Insert Table 8a Here]

I report the results in Table 8a. The sample size is somewhat smaller relative to the regressions in Table 7a since our outcome variable is measured in changes. The key coefficient of interest is that on the triple interaction term $LBO \times Post \times Distress$. x is set to 1 in columns (1) and (2) and 1.5 in columns (3) and (4). In column (1) we include both firm and year fixed effects, thus our estimated coefficient is identified from within-firm variation over time.

We observe that the triple interaction coefficient is positive and highly significant. The estimate implies PE-backed firms receive 94.4 percent greater capital infusion relative to matched non-PE firms, conditional on severe financial distress. The estimate rises to 1.13 if we drop time fixed effects, as shown in column (2). In column (3) we use a higher threshold, which intuitively captures a relatively lower severity of financial distress. We find that our coefficient of interest is significant at the 10 percent level, and the point estimate is much smaller at 0.461. This is consistent with the idea that relatively greater financial distress leads to higher equity injection by sponsoring funds.

7 Conclusion

Private Equity is often accused of over-leveraging their investments. Prior studies argue PE sponsors primarily look at credit market conditions when choosing buyout debt, and buyout capital structure is unrelated with cross-sectional factors. This standard view implicitly assumes optimal leverage does not change post-buyout. However, we do not know whether higher buyout leverage is optimal without a structural model that endogenizes default, leverage and the key benefits and costs of debt. This paper argues a PE-manager behaves much like a standard equity-holder and chooses capital structure by balancing the benefits of debt with the expected cost of financial distress. The model's key result is that PE managers are able to achieve a higher level of optimal leverage, which are on average, consistent with the data. The model also predicts higher optimal leverage results from a significant reduction in asset risk, and to some extent, an increase in asset return. Consistent with higher optimal leverage, I show that PE's contribution to corporate distress and financial fragility is lower than previously argued.

To support results from the structural estimation, I provide additional empirical evidence consistent with key factors that drive higher optimal leverage. Specifically, using a set of matched difference-in-differences regressions, I show that PE backed firms reduce sales volatility and also receive greater equity injections when in financial distress relative to comparable non PE-backed companies. These mechanisms support the notion of lower agency costs and reduced incentives to shift risk to debt-holders, which reduces the expected present value of bankruptcy costs and raises the optimal level of leverage.

How can we reconcile the empirical evidence on credit market conditions and initial buyout structure, in prior studies? One possible explanation is prior studies primarily examine firm characteristics at deal entry, while my empirical strategy internalized the effects of post-buyout changes in the portfolio company's characteristics. Overall, this paper broadens our understanding of what drives buyout leverage, and highlights the need to examine the value-maximizing leverage ratio when firms are backed by financial sponsors.

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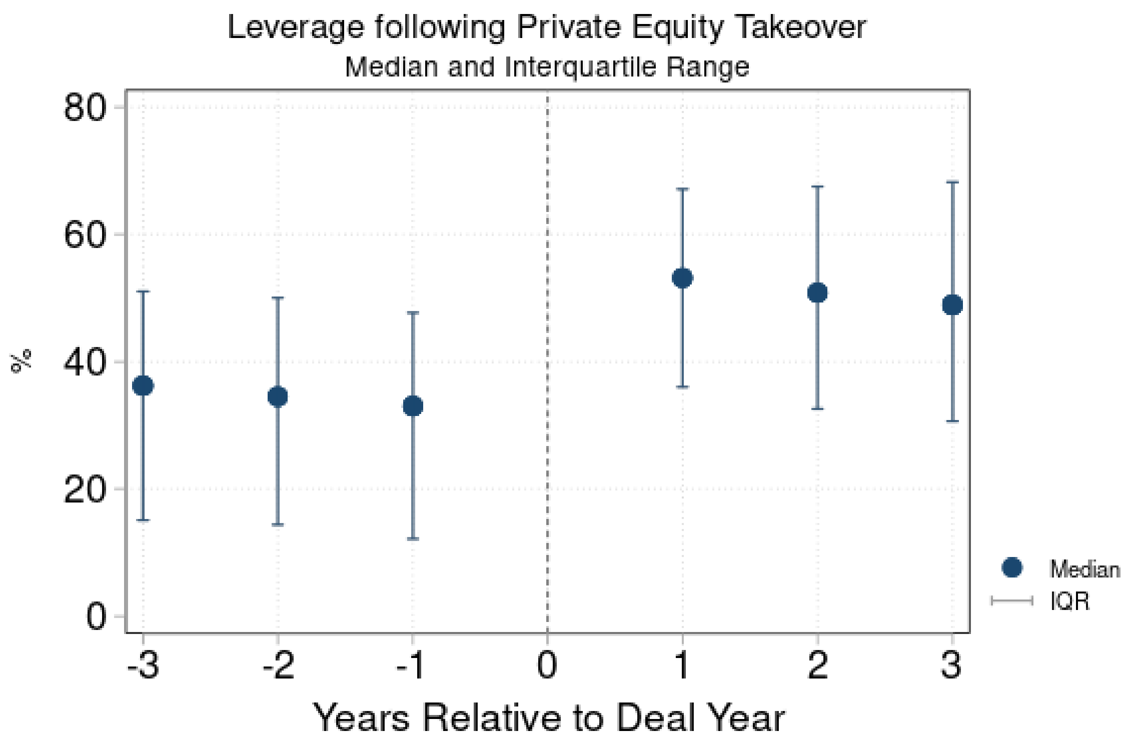
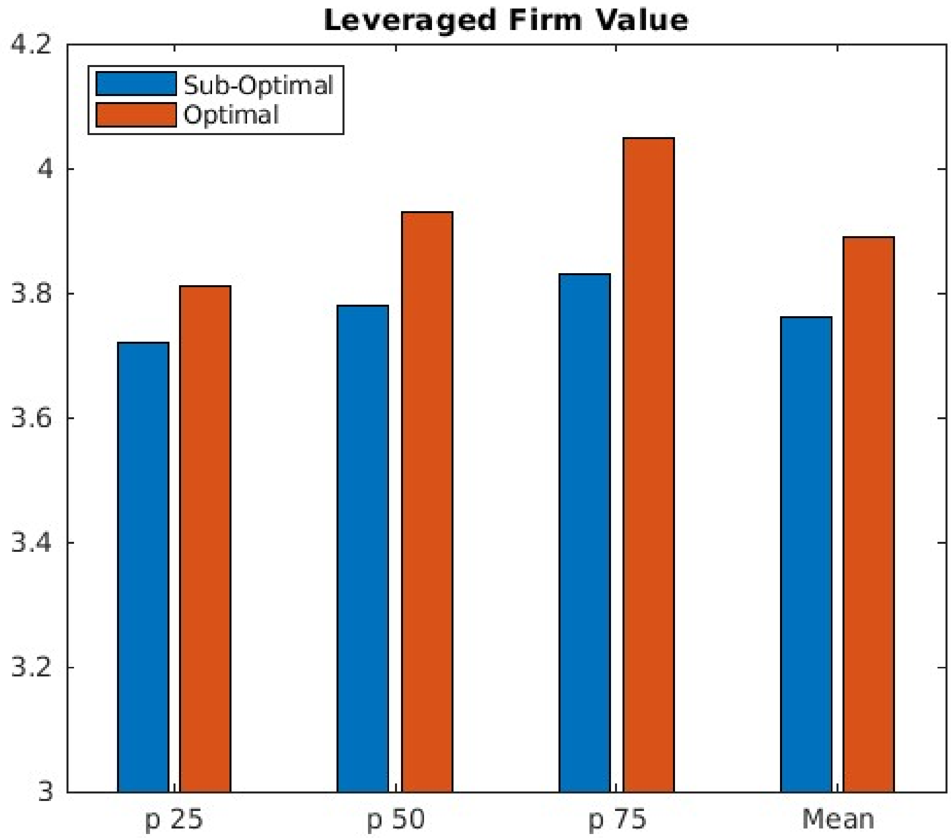
Figure 1: Leverage Dynamics



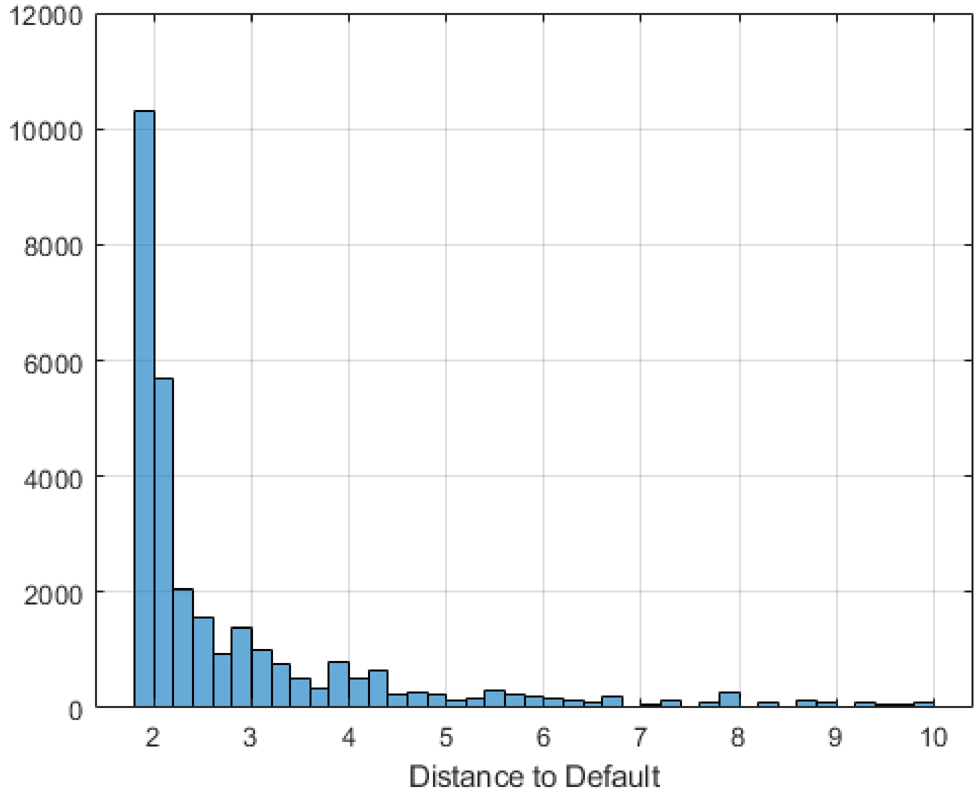
(a) *This chart plots leverage (Net Debt/Asset) in PE-backed firms in the pre-(post-) buyout periods. The x-axis plots years relative to the PE deal-year. The dot plots the median quantity, and the bands plot the interquartile range (IQR).*

Figure 2: Counterfactual Policy: Cost of Choosing Lower (sub-optimal) Leverage



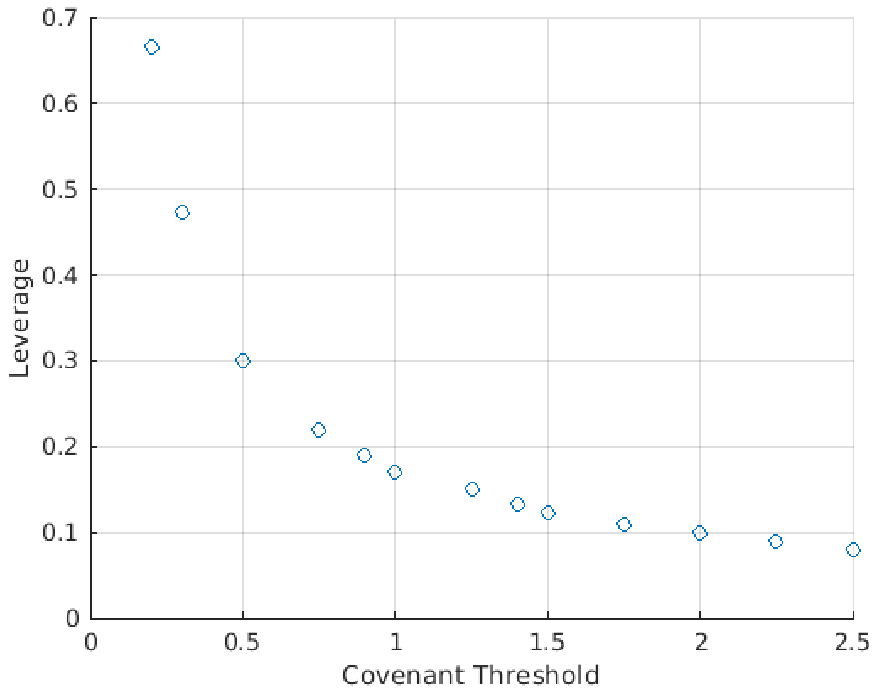
(a) *Notes: The chart above reports results from a counterfactual analysis on the cost of deviating from optimal leverage for a PE-backed company. The y-axis plots leveraged firm value and the difference between the two bars captures the cost of choosing sub-optimal leverage. Both charts plots the difference in firm value at the optimal* C^* *and a sub-optimal* C_{sub}*, where* $C_{sub} = 0.5 * C^*$*. This particular formulation of sub-optimal capital structure was chosen to match leverage ratios of standard non-PE companies.* δ_0 *value was set to 100. All values were multiplied by 0.001 to simplify visual exposition.*

Figure 3: Distance to Default Post-Buyout



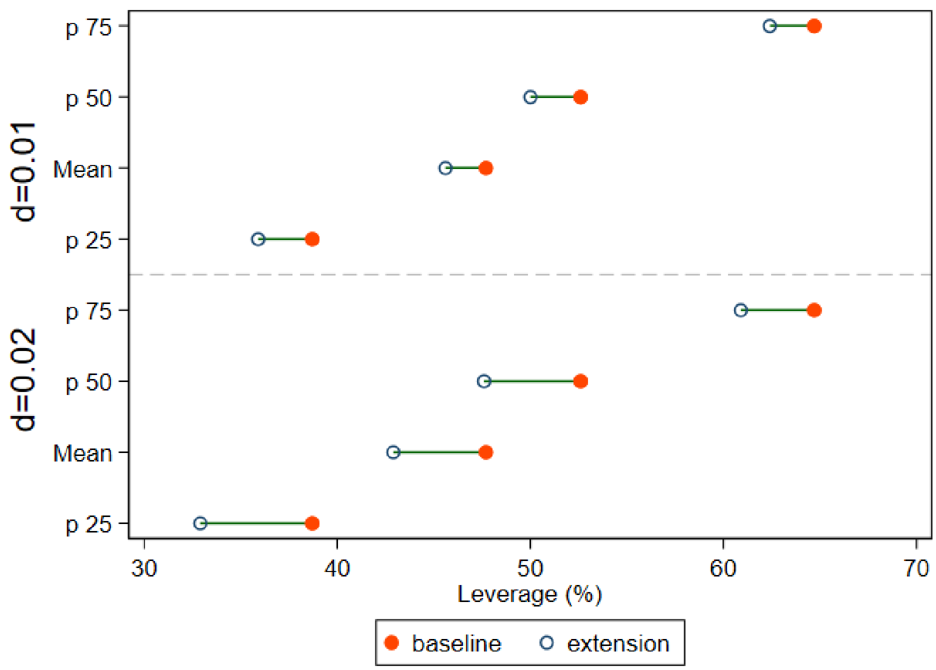
(a) *Notes: The chart above reports distance to default estimates from the benchmark Leland (1994) estimation. The model is estimated by solving for unobserved asset value and volatility that matches observed equity value and volatility. Market equity is computed as outlined in Section 3.3 and Equity volatility is computed as the standard deviation of (historical) daily stock price return for each firm and aggregated to the quarterly level to facilitate model calibration at the firm-quarter level. To calibrate the model, we set the risk-free rate to the drift rate,* $T = 1$ *and we approximate the default barrier with* V_B *which is derived endogenously.*

Figure 4: Model Extension: Optimal Leverage with Binding Covenant Threshold



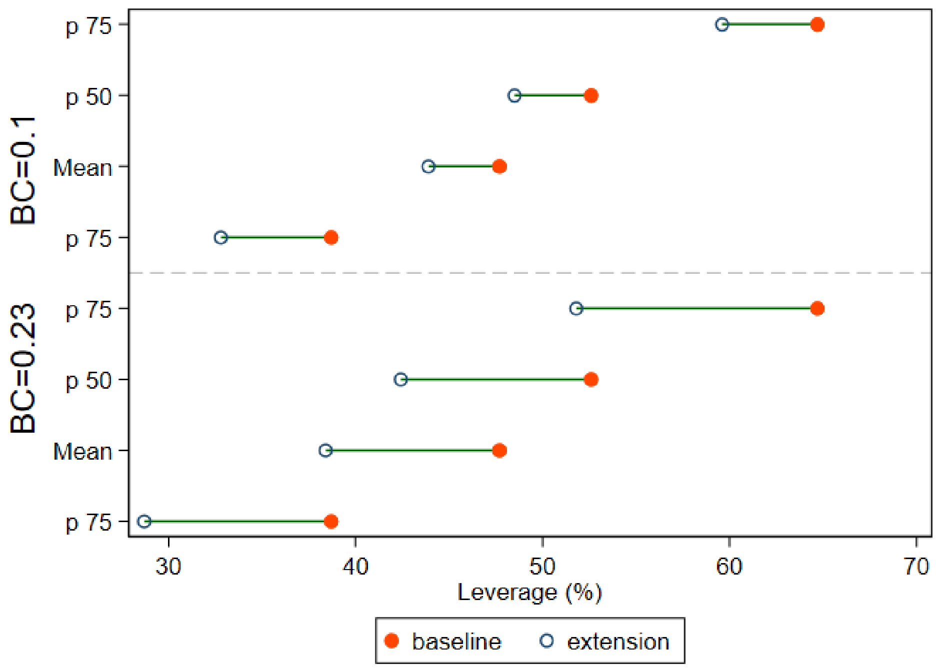
(a) *Notes: This chart reports leverage estimates from a trade-off model extended to capture an interest coverage covenant. The key difference of the model relative to the standard Leland (1994) model is explained in Section 5. Estimation methodology is described in Section 3.3. The x-axis plots θ, the covenant violation threshold, that links firm earnings to interest expenses and the y axis plots model predicted optimal leverage.*

Figure 5: Model Extension: Optimal Leverage under Asset Liquidation



(a) *Notes: This chart reports leverage estimates from a trade-off model extended to capture asset liquiditation. The key difference of the model relative to the standard Leland (1994) model is explained in Section 5. I use asset volatility quantities and all other calibrations from the benchmark estimation.*

Figure 6: Model Extension: Optimal Leverage under Bank Debt



(a) *Notes: This chart reports leverage estimates from a trade-off model extended to capture bank debt. The key difference of the model relative to the standard Leland (1994) model is explained in Section 5. I use asset volatility quantities and all other calibrations from the benchmark estimation. BC is abbreviation for bankruptcy cost. I use both the baseline bankruptcy cost as well as an alternative value of 0.1, presented in the top 4 rows.*

Table 1: Covariate Balance

Variable	PE Sample				Matched Sample					
	N	Mean	Median	SD	N	Mean	Median	SD	Mean diff.	%bias
Log Size	3020	18.2	17.89	1.57	6706	18.3	18.7	1.72	-0.1	-5.0
Leverage (%)	2875	48.8	49.8	28.9	6580	48.1	44.1	26.9	0.7	2.6
ROA (%)	3012	3.4	3.3	0.179	6585	3.2	4.2	0.07	0.2	1.7
Volatility	3028	0.091	0.055	0.34	6715	0.102	0.0539	0.32	-0.01	-3.0

(a) *Notes: This table reports summary statistics of sample firms across PE-backed and non-PE backed comparable public companies. The last column reports mean difference across the two groups.*

Table 2: Benchmark Results: Post-Buyout Leverage

A. Model Input	N	Q1	Q2	Q3	Mean
Market Equity ($ Mn)	32229	19.2	69.9	237	348
Equity Volatility	32229	0.135	0.191	0.357	0.242

B: Results	Q1	Q2 (Median)	Q3	Mean
Asset Volatility	0.122	0.177	0.277	0.203
	(0.054)	(0.046)	(0.032)	(0.058)
Leverage (Model)	38.7	52.6	64.7	47.7
Leverage (Data)	32.3	49.8	65.6	49.5
Boundary to Value ($\frac{\delta_B}{\delta_t}$)	0.188	0.331	0.475	0.361
Distance-to-Default	1.956	2.207	3.603	2.588
Tax/Unlevered Value	0.137	0.197	0.252	0.205

(a) *Notes: The columns are ordered by quartiles, after which the mean is reported.* δ_t *is recovered asset value in a firm-quarter,* δ_B *is the model-predicted endogenous default barrier. For the estimation, I set* r*=0.05,* τ*=0.2 and* α*=0.23.* μ*=0.0178, which is estimated directly from mean historical equity return data. See Appendix Table A5 for a summary of calibrated parameters and their sources. The formula for Tax Benefit to unlevered value is provided in Eq.* (10)*. Bootstrapped standard errors are reported in parenthesis computed from 5,000 re-samplings with replacement.*

Table 3: Sensitivity of Optimal Leverage to Calibrated Parameters

	Q2 (Median)	Q1	Q3
Benchmark Estimation	52.6	38.7	64.7
A. 20 % increase in calibrated parameter			
Risk-free rate, r	54.1	40.7	65.3
Bankruptcy cost, ρ	51.8	38.6	63.7
Drift, μ	54.1	39.6	66.0
Tax rate, τ	57.7	32.4	67.4
B. 20 % decrease in calibrated parameter			
Risk-free rate, r	50.7	36.4	62.9
Bankruptcy cost, ρ	53.4	38.9	64.8
Drift, μ	51.1	37.9	62.3
Tax rate, τ	47.7	44.5	60.3

(a) *Notes: This table reports sensitivity tests of model-implied optimal leverage with respect to the calibrated parameters, which were set according to previous studies. The benchmark estimation reports the same results from Table 2 as reference where leverage ratios are derived from estimated asset volatilities. For example, 'Q2 Median' in the first row reports the optimal leverage ratio at the median estimated asset volatility using the benchmark calibration. For each of the three calibrated parameters, Panel A reports optimal leverage from a 20 percent increase in the value of one calibrated parameter, while keeping the others at their benchmark value. Panel B reports the same for a 20 percent decrease.*

Table 4: Model Predicted Pre-Buyout Leverage

	N	Q1	Q2 (Median)	Q3	Mean
A. Model Inputs					
Market Equity ($ Mn)	9312	8.65	28.8	104	289
Equity Volatility	9312	0.329	0.424	0.924	1.3
B. Estimation Results					
Asset Volatility		0.272	0.331	0.406	0.320
		(0.003)	(0.01)	(0.006)	(0.002)
Leverage (Model)		25.3	29.1	33.0	22.2
Leverage (Data)		5.90	27.5	46.8	24.6
Boundary to Value		0.09	0.11	0.14	0.14
Distance-to-Default		2.22	2.57	2.91	2.56
Tax/Unlevered Value		0.04	0.04	0.051	0.049

(a) *Notes: The columns are ordered by quartiles, after which the mean is reported.* δ_t *is recovered asset value in a firm-quarter,* δ_B *is the model-predicted endogenous default barrier. For the estimation, I set* r*=0.05,* τ*=0.2 and* α*=0.23.* μ*=-0.013, which is estimated directly from mean historical equity return data for the pre-buyout sample. The formula for Tax Benefit to unlevered value is provided in Eq. (10). Bootstrapped standard errors are reported in parenthesis obtained from 5,000 re-samplings with replacement.*

Table 5: How Large is the Cost of Deviating from Optimal Leverage?

	p25	p50	p75	Mean
Levered Value: Sub-optimal	3.72	3.78	3.83	3.76
Levered Value: Optimal	3.81	3.93	4.05	3.89
Difference	0.09	0.15	0.22	0.13
Cost of Sub-Optimal Leverage	2.4%	4.0%	5.7%	3.5%

(a) *Notes: This table reports simulated cost of deviating from optimal leverage as outlined in Section 4.3. The columns are ordered by quartiles, after which the mean is reported. The first two rows report levered firm value (divded by 1000), given the estimated optimal leverage at different percentiles. All parameterizations are the same as the benchmark post-buyout. Sub-optimal firm value is estimated by setting optimal coupon to half of that predicted by the benchmark model. The last row reports the difference in two values as a percentage of the sub-optimal value, quantifying the cost of deviating from optimal leverage.*

Table 6: Extended Model with Debt Covenants

Results	Q1	Q2 (Median)	Q3	Mean
Asset Volatility	0.136***	0.192***	0.358***	0.244***
Leverage - Model (%)	11.68	8.3	2.61	6.1
Leverage - Data (%)	32.3	49.8	65.6	49.5
Boundary to Value ($\frac{\delta_B}{\delta_t}$)	0.074	0.296	0.443	0.28
Distance-to-Default	2.249	2.700	3.660	2.860
Tax/Unlevered Value	0.005	0.020	0.031	0.019

(a) *Notes: This table reports results of the benchmark model that incorporates coverage ratio covenant described in Section 5. The columns are ordered by quartiles, after which the mean is reported. δ_t is recovered asset value in a firm-quarter, δ_B is the model-predicted endogenous default barrier. For the estimation, I set r=0.05, τ=0.2 and α=0.23. μ=0.0178, which is estimated directly from mean historical equity return data. See Appendix Table A5 for a summary of calibrated parameters and their sources. The formula for Tax Benefit to unlevered value is provided in Eq. (10). For each asset volatility quartile estimate, bootstrapped standard errors are computed from 5,000 re-samplings with replacement.*

Table 7: Reduced Sales Volatility under PE-ownership

Y_{jt} : *Sales Volatility*	(1)	(2)	(3)	(4)
Post × *LBO*	-0.713***	-1.074***	-0.835***	-0.825***
	(0.182)	(0.405)	(0.191)	(0.202)
Post	0.682***	0.179	0.709***	0.710***
	(0.131)	(0.303)	(0.132)	(0.134)
R^2	0.931	0.010	0.932	0.929
Firm FE	Y	N	Y	Y
Year FE	N	Y	Y	Y
Controls	N	N	N	Y
N	2,538	2,537	2,537	2,465

Standard errors in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

(a) *Notes: This table reports difference-in-differences regression estimates at the firm-year level. The dependant variable is each firm's standard deviation of Sales, scaled by the firm's Earnings Before Interest and Taxes. Post takes value 1 for each year after a buyout, defined similarly for matched controls. LBO takes value 1 if a firm was actually acquired by a PE-sponsored leveraged buyout, 0 for matched controls. Controls include the log of book assets, leverage and return on assets. All variables are defined in Table A1 in the Appendix.*

Table 8: Equity Injection during Financial Distress

$Y: Equity\ Injection$	$Altman < 1$		$Altman < 1.5$	
	(1)	(2)	(3)	(4)
$Post \times Distress \times LBO$	0.944***	1.130**	0.461*	0.926***
	(0.211)	(0.462)	(0.229)	(0.207)
$Post \times Distress$	-1.241***	-1.207***	-0.622*	-0.803***
	(0.395)	(0.262)	(0.298)	(0.186)
R-squared	0.320	0.313	0.320	0.314
Firm FE	Y	Y	Y	Y
Year FE	Y	N	Y	N
Controls	Y	Y	Y	Y
N	1,965	1,965	1,965	1,965

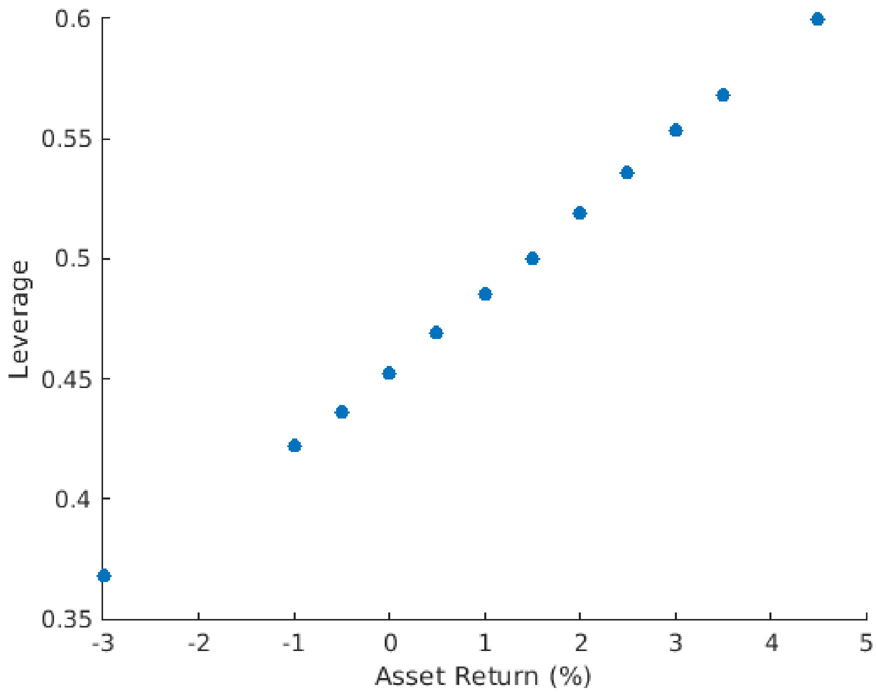
Standard errors in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

(a) *Notes: This table reports results of matched difference-in-differences regressions of outcomes of PE-backed companies relative to public controls. Following Bernstein et al. (2019),* Y_{jt} *is Net Equity Contribution/Asset. Equity Contribution is measured as the difference in total equity (shareholder value) over the past year, minus profit. Specifications vary by fixed effects and definition of Distress. Post takes value 1 in years after a buyout. Distress takes value 1 if the computed Altman Z-score is less than 1 in a given company-year in Columns (1) and (2) and less than 1.5 in columns (3) and (4). I also control for confounding pairwise interactions if they are not absorbed by firm fixed effects.*

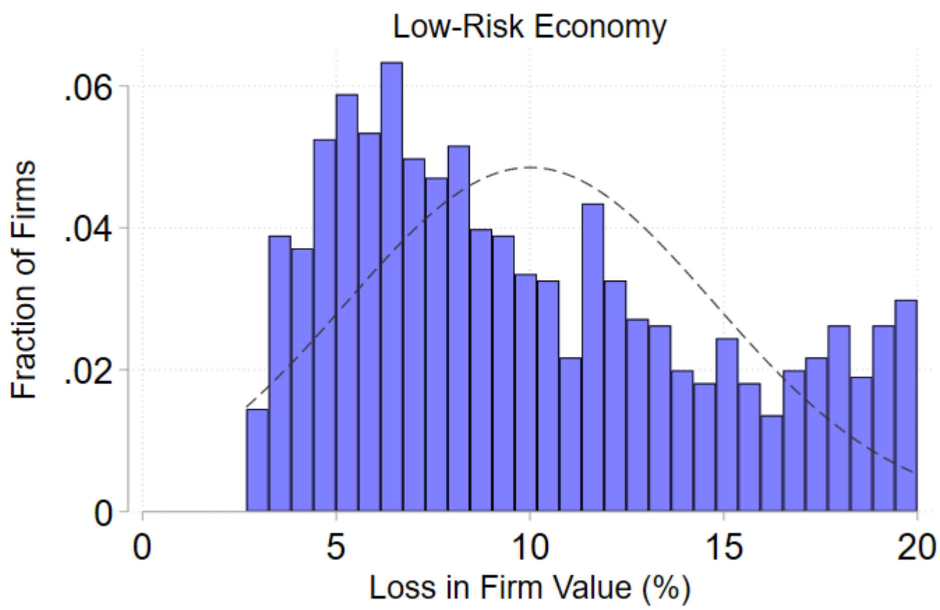
Internet Appendix

Figure A1: Sensitivity: Asset Return and Optimal Leverage



(a) *Notes: This chart reports leverage estimates from a trade-off model at different values of asset return, μ. σ_v is set to the value estimated from the benchmark analysis. All other parameter values are set to their benchmark calibrations.*

Figure A2: Counterfactual Policy: Sub-optimal Leverage and Low-risk economy



(a) *Notes: This chart reports the cost of choosing sub-optimal leverage in a low-risk economy using the benchmark estimation. To simulate a low-risk economy, I introduce a common (negative) shock of* $\sigma = 0.1$ *to the distribution of estimated firm risk.*

Table A1: Variable Definition

Variable	Description
Sales	Net Sales. BvD Code (TURN)
Size	Total Book Assets. BvD Code (TOAS)/Compustat Code (AT)
Debt	Total Book Debt. BvD Code (CULI -OCLI + LTDB); Compustat Code(dlc+dltt)
Cash and Cash Equivalents	Total Cash and Cash-like assets. BvD Code (Cash); Compustat Code (che)
Leverage	(Debt- Cash and Cash Equivalents)/Size
EBIT	Earnings Before Interest and Taxes. BvD Code (OPPL); Compustat ()
Return on Asset	EBIT/Size
Profit Volatility (Volatility)	Standard Deviation of EBIT
Sales Volatility	Standard deviation of (Sales/EBIT)
Shares Outstanding	Compustat (cshod)
Market Price	Compustat (prccd)
Net Equity Injection	Change in Book Equity (BvD Code SHFD)- Profit (PL)

Table A2: Sample Comparison

	All PE-backed Firms			PE Sample Used in Analysis		
	N	Mean	SD	N	Mean	SD
A. By Firm Characteristics						
Asset Size ($ Mn)	6576	18.3	1.6	3020	18.2	1.6
Leverage	6576	49.2	31.7	2875	49.5	28.9
	Share of Sample			Share of Sample		
B. By 1-digit NAICS Industry						
Agricultural, Forestry and Fisheries		0.8%			0.5%	
Mining, Utilities and Construction		6.1%			3.8%	
Manufacturing		42.8%			55.7%	
Wholesale and Retail Trade		13.1%			10.9%	
Information, Financials, Admins		32.9%			26.3%	
Other		4.2%			2.8%	

(a) *Notes: This table compares the PE sample retrieved from Orbis after standard cleaning procedures with the PE sample used in the analysis. The PE sample used in the analysis are those that could be matched to one or more comparable public companies using the methodology described in 3.3.*

Table A3: Matched Pre-Buyout Sample

A. Covariate Balance	PE Sample				Matched Sample				
	N	Mean	Median	SD	N	Mean	Median	SD	Mean diff.
Log Size	1551	18.79	18.6	1.586	9439	18.73	18.96	1.39	0.06
Leverage	1535	0.299	0.34	0.25	9389	0.287	0.25	0.25	0.012
Profitability	1552	0.011	0.01	0.6	9447	0.031	0.015	0.32	-0.02
Volatility	1539	0.31	0.29	0.13	9400	0..26	0..33	0.08	0.05

(a) *Notes: This table reports summary statistics of sample firms across PE-backed and non-PE backed comparable public companies using the pre-buyout sample only. The last column reports mean difference across the two groups.*

Table A4: Reduced Sales Volatility under PE-ownership: Restricted Sample

Y_{jt} : *Sales Volatility*	(1)	(2)	(3)	(4)
Post × *LBO*	-0.297***	-0.523	-0.316***	-0.308***
	(0.085)	(0.597)	(0.091)	(0.095)
Post	0.242***	-0.134	0.240***	0.242***
	(0.067)	(0.513)	(0.068)	(0.070)
R-squared	0.991	0.024	0.992	0.992
Firm FE	Y	N	Y	Y
Year FE	N	Y	Y	Y
Controls	N	N	N	Y
N	872	870	870	849

Standard errors in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

(a) *Notes: This table reports difference-in-differences regression estimates at the firm-year level; the estimation is restricted to 5 large european economies: UK, France, Italy, Spain and Germany. The dependant variable is each firm's standard deviation of Sales, scaled by the firm's Earnings Before Interest and Taxes and computed separately in the pre-(post-) buyout samples. Post takes value 1 for each year after a buyout, defined similarly for matched controls. LBO takes value 1 if a firm was actually acquired by a PE-sponsored leveraged buyout, 0 for matched controls. Controls include the log of book assets, leverage and return on assets. All variables are defined in Table A1 in the Appendix.*

Table A5: Model Parameters

Parameter	Value	Source
Risk-free rate	0.05	He (2011); Strebulaev and Whited (2012)
Tax Rate	0.2	Leland (1998); He (2011)
Bankruptcy Cost	0.23	Andrade and Kaplan (1998)
Drift, Post	0.017	Estimated
Drift, Pre	0.05	Estimated

(a) *Notes: This table reports key parameters required to initialize the benchmark model, and tabulates their sources. Drift is computed directly from historical (daily) equity return and aggregated to the quarterly level to facilitate the estimation.*

A8 Trade-off Model with Bank Debt

Trade-off model with Bank debt follows Strebulaev et al. (2012). I only outline the key equations and refer readers to the original paper for further details. The firm has only bank debt D_B outstanding, with a strong bargaining position with the bank. Thus, the firm can make a take-it-or-leave-it offer which the bank can reject. In that case the firm is liquidated. The bank's payoff can be denoted by:

$$R_B(\delta) = min\big[c_{bank}/r, (1-\alpha)(1-\tau)\frac{\delta}{r-\mu}\big] \tag{15}$$

where $\frac{c_{bank}}{r}$ is the promised coupon if the firm is solvent. Due to the firm's bargaining power, it will keep the bank debt at its reservation value if renegotiation were to occur, which we can denote as:

$$R_B(\delta) = (1-\alpha)(1-\tau)\frac{\delta}{r-\mu} \tag{16}$$

Importantly, total levered firm value is now the sum of only the un-levered value post-tax and tax benefits of (bank) debt, and can be given by the equation below.

$$V_L(\delta) = (1-\tau)\frac{\delta}{r-\mu} + \tau D_B(\delta) \tag{17}$$

The renegotiation point is conceptually similar to the default point in the standard model. Equity-holders maximize their payoff by choosing the renegotiation point. The mathematical derivations related to an optimal coupon are outlined in Strebulaev et al. (2012) and follow the same value-pasting strategy as Leland (1994).

A9 Optimal Coupon in Trade-off Model with Debt Covenant

As outlined in Section 5, I set the default barrier to a multiple of required coupon payment to capture an interest coverage covenant in a parsimonious manner. Thus, the borrower is forced to relinquish control to creditors if asset value falls to this exogenously specified level. Taking a derivative of the value of the levered firm in Eq. (7) with respect to C with $V_B = \theta C$ and setting the derivative equal to 0 yields the following expression:

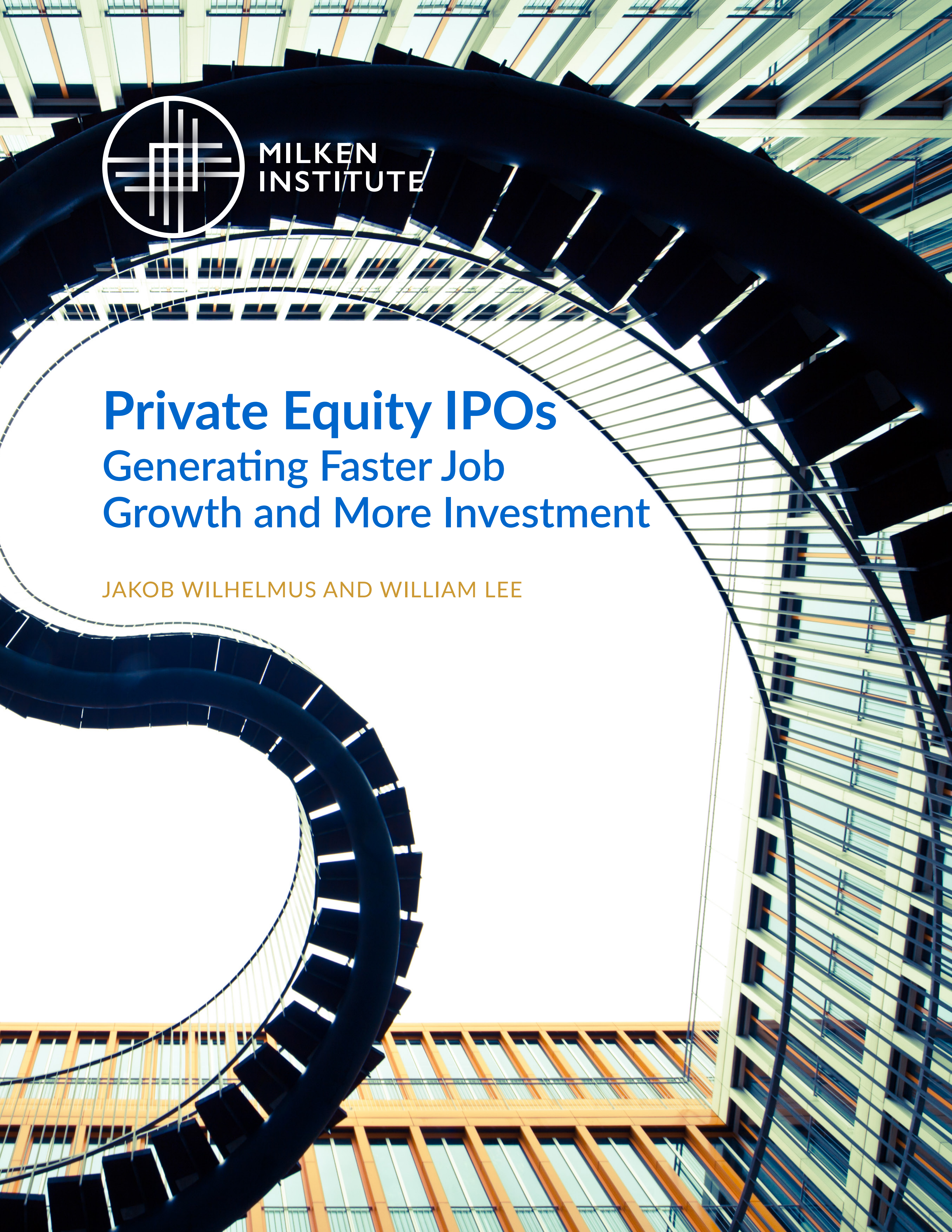
$$\frac{\tau}{r} - (1+\gamma)\frac{\tau}{r}\theta C^{\gamma}\delta^{-\gamma} - (1+\gamma)\frac{\alpha}{r-\mu}\theta C^{\gamma}\delta^{\gamma} = 0 \tag{18}$$

Define:

$$X = (1+\gamma)\delta^{-\gamma}\big(\frac{\tau}{r} + \frac{\alpha}{r-\mu}\big) \tag{19}$$

Simplifying Eq. (18) using the definition in Eq. (19) yields:

$$C^{*} = \big(\frac{\tau}{r}\frac{1}{X}\big) * \frac{1}{\gamma} * \frac{1}{\theta} \tag{20}$$



Private Equity IPOs

Generating Faster Job Growth and More Investment

JAKOB WILHELMUS AND WILLIAM LEE

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PRIVATE EQUITY IPOS

Generating Faster Job Growth and More Investment

Jakob Wilhelmus and William Lee

We present evidence of stronger job creation, revenue generation, and investment spending by listed companies previously backed by private equity (PE) than peers that listed without PE backing.[1] The profile of PE-backed companies we develop challenges popular narratives that emphasize job destruction and cost cutting as the primary source of PE value creation. There is evidence suggesting that private equity firms have become an important source for corporate funding. In addition, we present data for a limited group of companies that PE firms had owned previously, which suggest that these companies play a significant and growing role in creating new jobs and generating more productivity-enhancing investments.

1 Because of the paucity of data for companies under private equity ownership, we have chosen to evaluate the aftermath of private equity involvement. If PE restructuring leaves lasting efficiency gains, then such a successful IPO may grow faster than its peers, including faster job creation as the firm expands its scale.

INTRODUCTION

Private equity (PE) firms have become an increasingly important source of funds for financing US companies.[2] PE firms make investment decisions that can brighten or darken the economic outlook of the communities in which their companies operate. When PE firms initially take over struggling companies, alarms sound over factory and store closings and laid off workers. Politicians, such as Senator Elizabeth Warren, have reportedly targeted PE firms as "vampires—bleeding the company dry and walking away enriched even as the company succumbs."

However, because PE firms are so opaque, there is a paucity of data about the operating history of companies owned by PE firms. Few outsiders know much about the aftermath of PE involvement. There are few sources of data that can show whether the rejuvenated company would create new and better jobs, modernize factories, and generate more store openings that ultimately leave the community much better off than before the PE firm arrived.

Our research has uncovered information about the impressive performance of a small group of companies after PE firms sold them via an initial public offering (IPO). In this report, we provide evidence that these companies that were formerly backed by private equity on average created more jobs, generated larger revenues, and spent more in capital expenditures than did their non-PE-owned peers after their initial listing.

Academic and industry studies are replete with theories purporting to explain why companies with PE investors may have advantages over companies relying on other sources of capital (e.g., banks).[3] Owing to regulatory, governance, and risk-management considerations, banks will often limit funding to asset-poor companies. Banks are especially reluctant to lend to borrowers with a large proportion of intangible-to-tangible assets, a substantial need to incur R&D expenses, and/or an absence of steady cash flows. Under such circumstances, banks are either reluctant or prohibited from funding such companies. By comparison, PE firms are more willing to finance such companies and either accept or manage many of these risks in return for higher prospective returns.

PE firms usually search for companies with high growth potential and develop flexible funding and management strategies to overcome assorted risks and agency

2 Our previous report, Companies Rush to Go Private, discussed the influences behind the rise in private equity financing, its growing importance for corporate capital raising, and potential policy issues raised by these developments.

3 For example, an influential cross-country study finds that "...difficulties in dealing with banks, such as bank paperwork and bureaucracies, and the need to have special connections with banks, do constrain firm growth. Collateral requirements and certain access issues—such as financing for leasing equipment—also turn out to be significantly constraining [on company growth and financing]." (Thorsten Beck, and others. "Financial and Legal Constraints to Growth: Does Firm Size Matter?" Journal of Finance, (July 2005).)

costs. Such risks and agency costs usually stem from information asymmetries between managers and owners, and divergent interests of creditors versus shareholders. PE firms manage such discouraging influences in the companies they own with staged financing to control risks and contractual incentives (e.g., stock options), to deter employee departures and the loss of R&D benefits to other firms, and to restructure management to improve active monitoring that reduces other agency costs.[4]

PRIVATE EQUITY FIRMS' IMAGE AS VISIGOTHS MAY NOT BE ACCURATE

PE investors are facing a growing social and political onslaught because of highly publicized layoffs associated with company downsizing and financial hollowing of companies under PE ownership.[5] Unfortunately, most PE firms have not collected or made available adequate data to change such perceptions or document the net impact of their activities for non-investors and those who influence public policy. For example, there are widespread and deeply held concerns that PE firms engage mainly in "financial engineering" activities that saddle target companies with high debt burdens and do not generate economic or social benefits for non-investors. PE firms often are associated with plant closings and layoffs that contribute to creating "ghost towns" and the hollowing out of mid-America.

Notwithstanding the potential for heightened political pressures to gain more transparency through additional regulation or legislation, PE firms have scant regard for, and hardly acknowledge, the need to collect and standardize data to evaluate such claims. Most PE firms have not collected nor made available data on employment, investment expenditures, and other financial information for the companies they own and/or manage. Exceptions are rare but notable (e.g., New Mountain Capital has been reporting key job growth and investment data for the companies in their portfolio for the last 10 years as part of its "Social Dashboard" report).[6]

In the absence of direct data, some researchers have resorted to indirect data to infer the impact of PE activities on job growth and investment. One successful series of studies and data "mash-ups" began with a prominent study showing that while PE

4 Steven N. Kaplan and Per Strömberg, "Leveraged Buyouts and Private Equity," Journal of Economic Perspectives, Volume 22, Number 4—Season 2008 provides an excellent survey of the academic literature explaining these influences.

5 Negative reports about PE activities dominate the popular press. Recent reviews in the New York Times, Forbes, and the Center for Economic and Policy Research show there is much "damage control" work ahead. Senator Warren has proposed legislation limiting PE tax benefits and increasing accountability for the social consequences of their activities.

6 The New Mountain Capital press release for its 2018 "Social Dashboard" report can be found here.

buyouts may initially destroy jobs relative to their peers (sometimes for years under PE ownership), such companies do ultimately demonstrate some net job creation.[7] Admittedly, it is difficult to identify whether job creation results from company acquisitions or organic growth.

However, from a social policy perspective, there is some merit to considering that "a job is a job," regardless of whether it comes through acquisitions or from organic growth. It is especially important to note that some PE acquisitions may have precluded companies from going out of business and laying off even more of their employees. Indeed, our evidence suggests that PE-backed IPOs created more jobs than their non-PE peers in typically high-wage sectors (e.g., industrials and telecom) as well as lower-wage sectors (e.g., consumer/retail). Nevertheless, we cannot be confident about this inference because of our limited data.

Insufficient data availability remains the core obstacle to providing transparency into how PE ownership influences company performance. It also limits our ability to assess the impact of PE influence on the employment, revenue generating, and investment behavior of companies during and after exiting PE ownership.

There seems to be some indirect evidence, including from a study encompassing the United States and many European countries, that companies tend to show greater employment and productivity growth in sectors with relatively high PE participation.[8] Although deal volume in the United States and the United Kingdom dominate that of the other countries, the authors noted that PE activities also boosted growth in companies (and sectors) located in continental Europe.[9] This finding implies that the aftermath of PE involvement may have beneficial influences on job and productivity growth that transcends the more market-based labor market and investment practices of US and UK companies. To investigate this latter possibility, the researchers focused much of their analysis on companies operating in continental Europe. Unfortunately, they had no direct data indicating whether PE-owned companies invested more in productivity-enhancing capital equipment, staff training, or research and development of new or disruptive technologies that may boost macroeconomic productivity.

7 Steven J. Davis, John Haltiwanger, Kyle Handley, Ron Jarmin, Josh Lerner, and Javier Miranda, "Private Equity, Jobs, and Productivity," *American Economic Review*, 104 (12): 3956-90, (2014). These researchers merge US Census data containing US businesses with paid employees with databases (e.g., Capital IQ) containing firm-level transactions where PE firms can be associated with acquisitions, buyouts, and similar deals.

8 Shai Bernstein, Josh Lerner, Morten Sorensen, and Per Strömberg, "Private Equity and Industry Performance," (2017); also earlier version "NBER Working paper 15632," January 2010.

9 Deals in the United States and United Kingdom accounted for 67 percent of the number of deals in the sample. The authors also resorted to various statistical tests to rule out the possibility of reverse-causation: PE firms are more active in sectors that are already fast growing.

Our study examines the robustness of the inferences made by these earlier studies that suggest there may be some positive impact from PE involvement on subsequent company employment, revenue growth, and investment spending. We focus on the impact of PE ownership of a select population of companies that later become listed on public stock exchanges. The advantage of our approach is that it allows direct "apples-to-apples" comparison of the behavior of PE- and non-PE-owned companies. Our data leverage the strict data reporting requirements for listing on stock exchanges and avoid some of the potential errors that may arise from matching data from disparate sources.

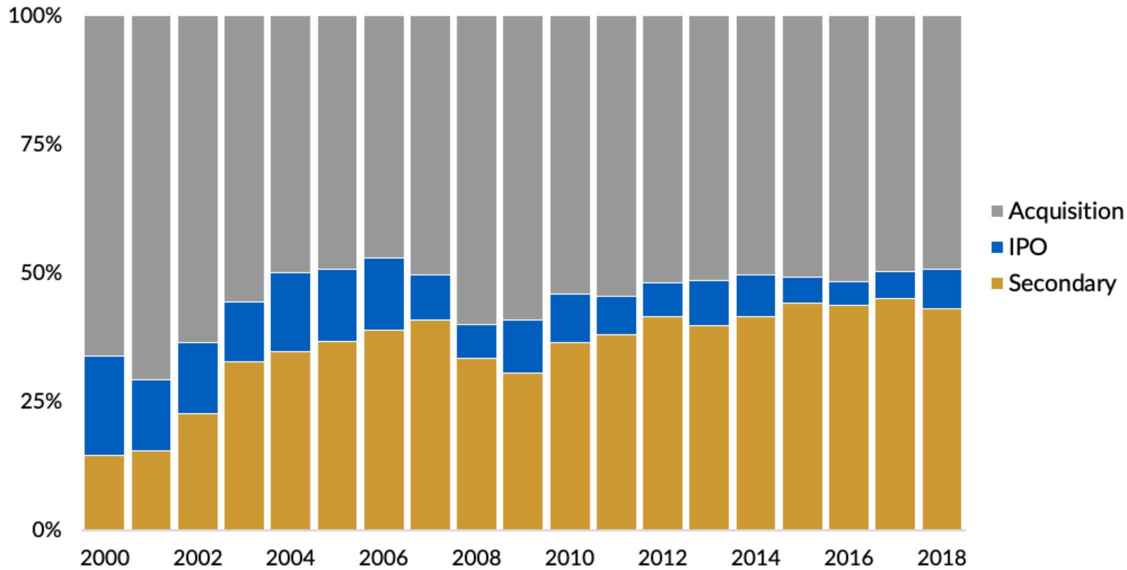
Our study evaluates company-level performance against a tighter control group than earlier studies; we compare the behavior of PE- and non-PE-backed IPOs collectively and separately by sector and company size. We specifically examine whether PE-backed alumni companies show persistently better performance, such as faster employment and revenue growth, and more investment spending compared with companies in the same sectors and of the same size.

We are mindful of the special nature of this group of companies we have chosen to study—they represent a small sliver of companies that leave private equity ownership (approximately 10 to 20 percent). IPOs are not the typical way for a company to exit from private equity ownership. Since early 2000, there has been huge growth in sponsor-to-sponsor deals, where both the buyer and seller are PE firms. Such "secondary" exits now comprise 40 percent of all instances where companies leave a private equity owner (Figure 1). Such alternatives for companies to develop outside the IPO limelight have also extended the tenure of companies under PE ownership. Indeed, some estimates imply that "53 percent of all 2004-vintage buyout funds are still active."[10] Therefore, most of the companies that went private before the Financial Crisis likely have not returned to public markets.

Consequently, the companies we study are a small share of the companies reshaped by PE firms. Nevertheless, we show that the transformation of these companies to provide high returns for their investors may also have a positive influence on aggregate employment and investment trends. Unfortunately, we do not have relevant information about how PE activities influence company-level changes beyond this group of listed companies. Nevertheless, we believe the lessons learned about how PE influences company behavior after public listing contributes to the ongoing discussion about the PE industry. At a minimum, we believe our evidence may help reshape some inaccurate stereotypes about the effects of PE activities and contribute to ongoing discussions about regulatory and other policy changes to manage or limit the PE industry's investment activities.

10 "For the longest time," Pitchbook (2018).

Figure 1: US Private Equity Exits



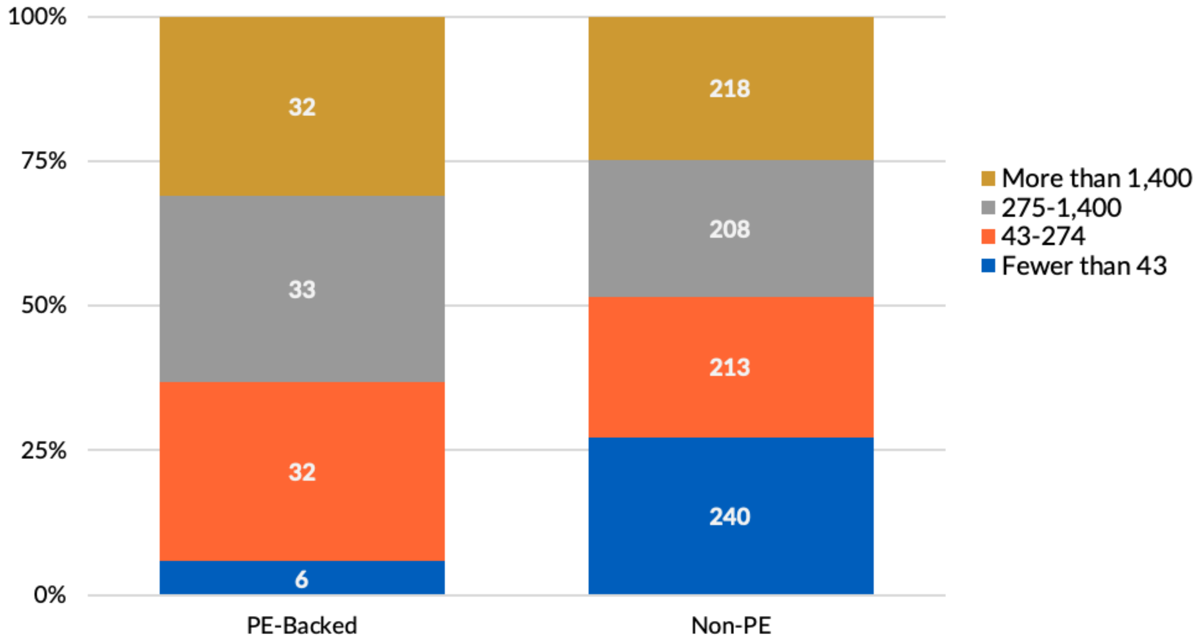
Source: Pitchbook, Thomson Reuters Eikon, and Milken Institute research (as of May 2018).

We divide our study into four analytical sections to differentiate and assess how PE-backed and non-PE-backed IPO behavior may differ concerning job creation, revenue growth, and capital expenditures. The next section shows that for the companies in our data, PE-backed IPOs outperform non-PE-backed IPOs in all these categories, and while PE-backed IPOs of all sizes generate more jobs, it is the smaller to mid-sized companies that invest more (on average). We then take a sector-by-sector approach to show that our findings remain robust even when IPOs are compared to peers within the same sector. The fourth section proposes a template for PE firms to disclose data systematically about their portfolio of companies to help assuage public concerns about detrimental economic and social externalities stemming from their investment activities. The final section provides some concluding thoughts.

PRIVATE EQUITY-OWNED COMPANIES THAT LIST ON STOCK EXCHANGES OUTPERFORM NON-PE-BACKED IPOS

We study the influence of PE ownership on the behavior of a sample of companies that have listed on a US stock exchange from 2002 to 2017. Out of 982 companies that reported data compiled by Capital IQ and Pitchbook, PE firms owned approximately 10 percent of these companies (103) before they listed. These PE-backed companies were of varying sizes but clustered mostly among the largest three quartiles of companies (by employment). They were disproportionately less represented among the group of smallest employers (Figure 2). Indeed, two-thirds of the PE-backed IPOs had more than 275 employees and were in the upper half of the distribution of all IPO companies when ranked by employment.

Figure 2: PE-Backed IPOs Are Evenly Distributed Among the Larger Employees



	Company Distribution by Employment	
	PE-Backed	Non-PE
More than 1,400	32	218
275-1,400	33	208
43-274	32	213
Fewer than 43	6	240
TOTAL	**103**	**879**

	Percentage of PE and Non-PE	
	PE-Backed	Non-PE
More than 1,400	31%	25%
275-1,400	32%	24%
43-274	31%	24%
Fewer than 43	6%	27%
TOTAL	**100%**	**100%**

Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

PE-BACKED COMPANIES SHOW LARGEST POST-IPO EMPLOYMENT GAINS

Our data show that PE-backed IPOs created more jobs and at a faster pace (on average) than non-PE-backed companies during the 2002-2016 period (Figures 3a and 3b). Two years after their IPO, the PE-backed companies created an average of 421 jobs per year compared with an average of 290 jobs for non-PE-backed companies each year from 2002 to 2016.[11] Apart from producing more jobs, our data also show that PE-backed IPOs generated jobs at a faster pace. Average annual employment growth among PE-backed companies for two years after the IPO was 8.7 percentage points faster than for the IPOs without PE involvement (i.e., PE-backed IPOs averaged 71.2 percent average annual employment growth compared with 62.9 percent for non-PE backed companies).[12]

Following the Great Recession of 2008, all companies created jobs at a faster pace, but the rate of PE-backed IPO job creation continued to exceed that for non-PE-backed companies, albeit by only 2.1 percentage points.[13] Although the rate of job creation for PE- and non-PE-backed IPOs converged after 2008, the absolute number of jobs created by the former (on average) continued to exceed by 40 percent (e.g., 417 versus 297) the number produced by non-PE companies. Also, PE-backed IPOs consistently created jobs on average at a faster pace across companies of all sizes. Sorting all the companies by employment as a proxy for company size, the data continue to show that PE-backed IPOs created jobs at a faster pace than non-PE-backed companies (Figure 4). It is clear that among the smallest quartile of companies (those employing fewer than 43 workers), PE-owned companies averaged the fastest employment growth, although for this smallest-company quartile, there were disproportionately fewer of them than non-PE-backed IPOs.[14] However, unlike the other size groupings, in the group of smallest companies (those employing fewer than 43 employees), the average non-PE-backed company created more jobs than the average PE-backed company.

11 In this report, when we refer to "average" employment, revenue, and investment (or capital expenditures), we calculate this value by dividing the total employment, revenue, or investment by the number of companies in the relevant category (e.g., PE-backed company or quartile). Consequently, the rows or columns in some of the tables below may not add to the total because of rounding.

12 The higher average employment growth for PE-backed firms is statistically significant at the 5 percent confidence level.

13 We calculated the average pace of job creation by companies in the PE and non-PE groups by dividing the number of jobs produced by each company for the two-year period after the IPO by the number of IPOs.

14 It is evident that these out-sized job gains are not an arithmetic artifact (where growth is generally more rapid for companies with a small number of employees). A company with five employees will experience 100 percent growth if it adds another five workers, whereas a company with 100 employees would experience only 5 percent growth.

Figure 3a: PE-Associated IPOs Average Faster Growth in Jobs than Non-PE IPOs

Average differential for 2002-2009: +12.1 percentage points
Average differential for 2010-2016: +2.1 percentage points

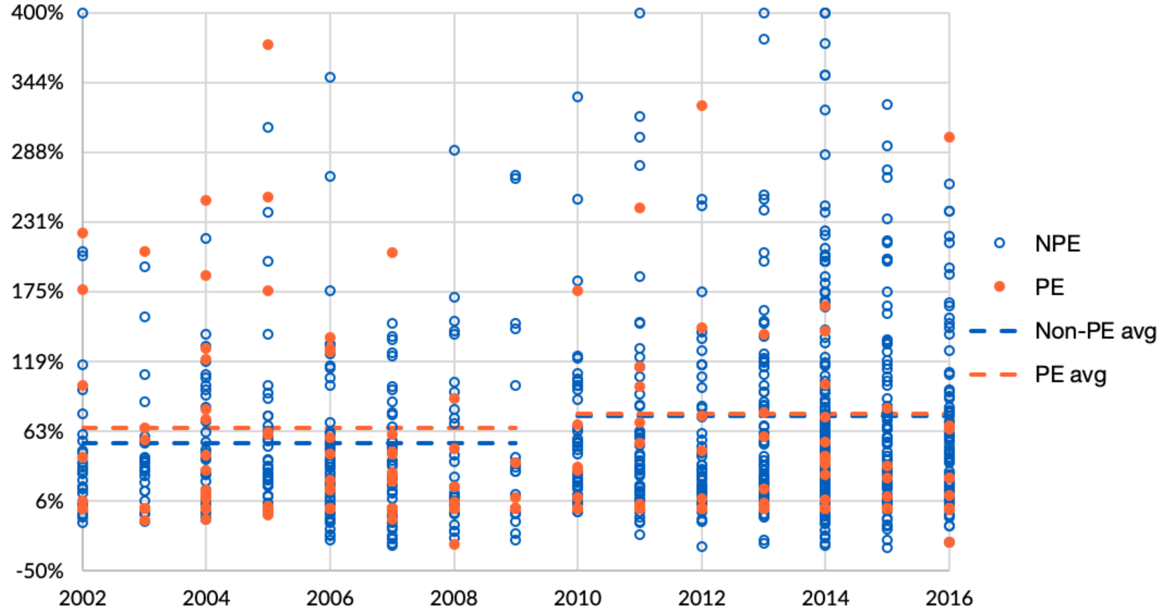
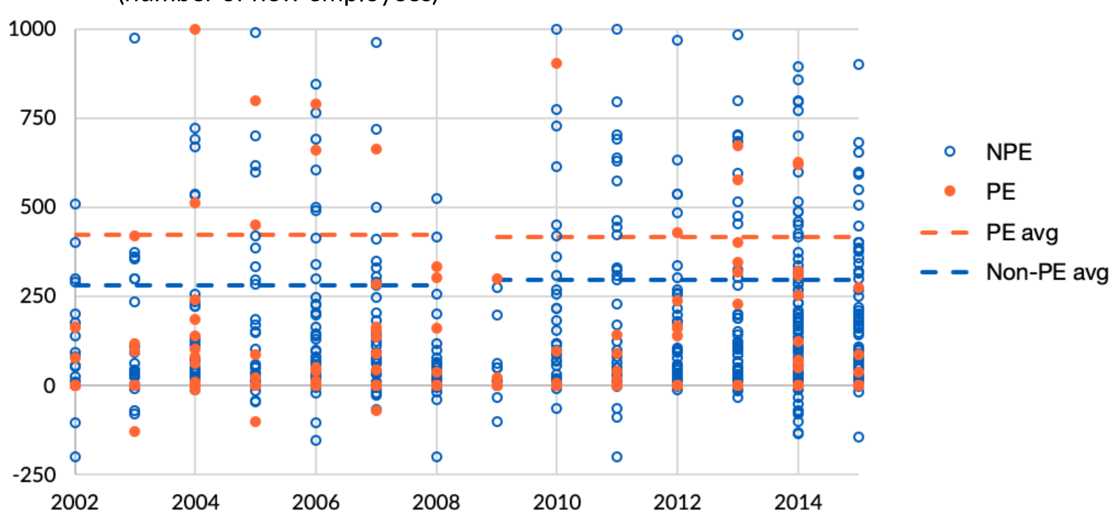


Figure 3b: PE-Associated IPOs Average More Job Creation than Non-PE IPOs

Two-Year Job Creation by PE-Backed and Non-PE-Backed IPOs
(number of new employees)



Two-Year Job Creation After PE-Backed and Non-PE-Backed IPOs
(number of new employees)

Average 2002-2008		Average 2009-2015		Average 2002-2015	
PE	424	**PE**	417	**PE**	421
Non-PE	282	**Non-PE**	297	**Non-PE**	290

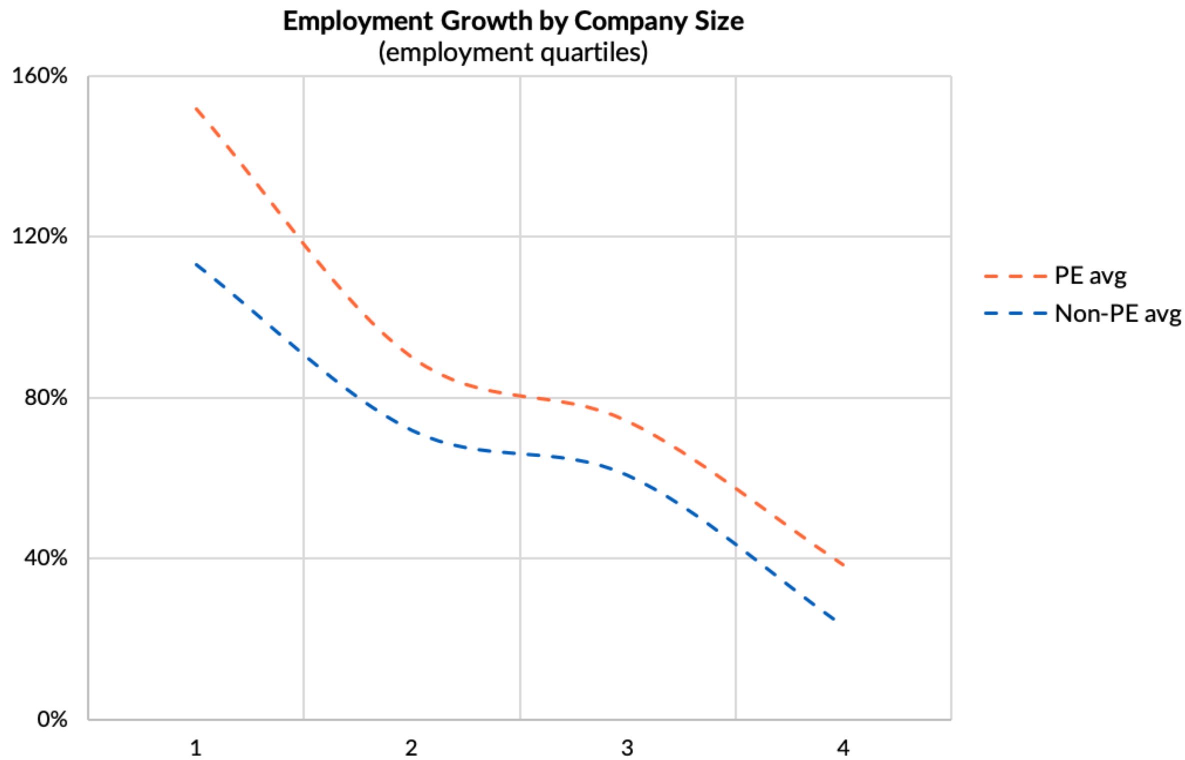
Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

While companies that conducted IPOs tended to be smaller than the typical company in their respective sector, PE-backed IPOs in the largest three employment quartiles consistently created more jobs (on average) than non-PE-backed companies. Indeed, the larger PE-backed companies (those in the largest quartile) generated more than twice as many jobs as the larger non-PE-backed IPOs. Moreover, the table under Figure 4 shows that employment gains for the median PE-backed IPO exceeded the number of jobs created by the median non-PE IPO in that same quartile.[15]

Measuring company size by revenue instead of employment continues to show that PE-backed IPOs generate faster employment growth and more jobs on average (Figure 5). Almost half (45 percent) of all PE-backed IPOs are mid-sized companies, with revenues in the $33 million to $243 million range. On average, these (third quartile) mid-sized companies, representing only 14 percent of all PE-backed IPOs, are the ones that produced jobs at the fastest pace (table accompanying Figure 5). Moreover, the median PE-backed IPO within each quartile produced more jobs than the median non-PE-backed IPO in the same size grouping.

15 At the time of its IPO, the median PE-backed company employed 642 workers, compared with 222 workers for those that were not PE-backed. For all companies in our sample (including those that did not IPO), the median company employed 1,840 workers.

Figure 4a: Faster PE-Backed IPO Employment Growth Evident Across Companies of All Sizes



Employment Quartiles
(number of new employees)

1st	2nd	3rd	4th
42	274	1,400	>1,400

Number of IPOs

	1st	2nd	3rd	4th	Total	Percent
PE average	6	32	33	32	**103**	**10%**
Non-PE average	240	213	208	218	**879**	**90%**

Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

Figure 4b: Faster PE-Backed IPO Employment Growth Evident Across Companies of All Sizes

Average Two-Year Job Creation
(jobs created divided by number of companies in each quartile)

Employment Quartile	1st	2nd	3rd	4th
PE average	26.5	140.1	454.9	1,596
Non-PE average	37.1	122.2	401.4	719
Difference	-10.6	17.9	53.5	877

Two-Year Net Job Creation for Median Company
(number of jobs for median company in quartile)

Employment Quartile	1st	2nd	3rd	4th
PE average	13	86	302	563
Non-PE average	12	45	219	400
Difference	1	41	84	163

Note: Among the 982 firms that conducted IPOs from 2002 to 2017 in our data, 103 were associated with PE firms. Two-thirds of the PE-related firms were in the largest two employment quartiles, employing 275 or more employees. Employment gains for the median company backed by private equity in each employment quartile exceeded job creation by the median non-PE-backed company.
Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

Figure 5a: Mid-Sized PE-Backed IPOs Display Fastest Employment Growth Gains

Revenue Quartiles
($ million)

1st	2nd	3rd	4th
<3	32	243	> 243

Number of IPOs

Revenue Quartile	1st	2nd	3rd	4th	Total	Percent
PE average	7	25	45	26	**103**	**10%**
Non-PE average	239	220	200	220	**879**	**90%**

Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

Figure 5b: Mid-Sized PE-Backed IPOs Display Fastest Employment Growth Gains

Average Two-Year Job Creation
(jobs created divided by number of companies in each quartile)

Revenue Quartile	1st	2nd	3rd	4th
PE average	322	186	604	1,410
Non-PE average	97	238	369	572
Difference	225	-52	235	837

Two-Year Net Job Creation for Median Company
(number of jobs for median company in quartile)

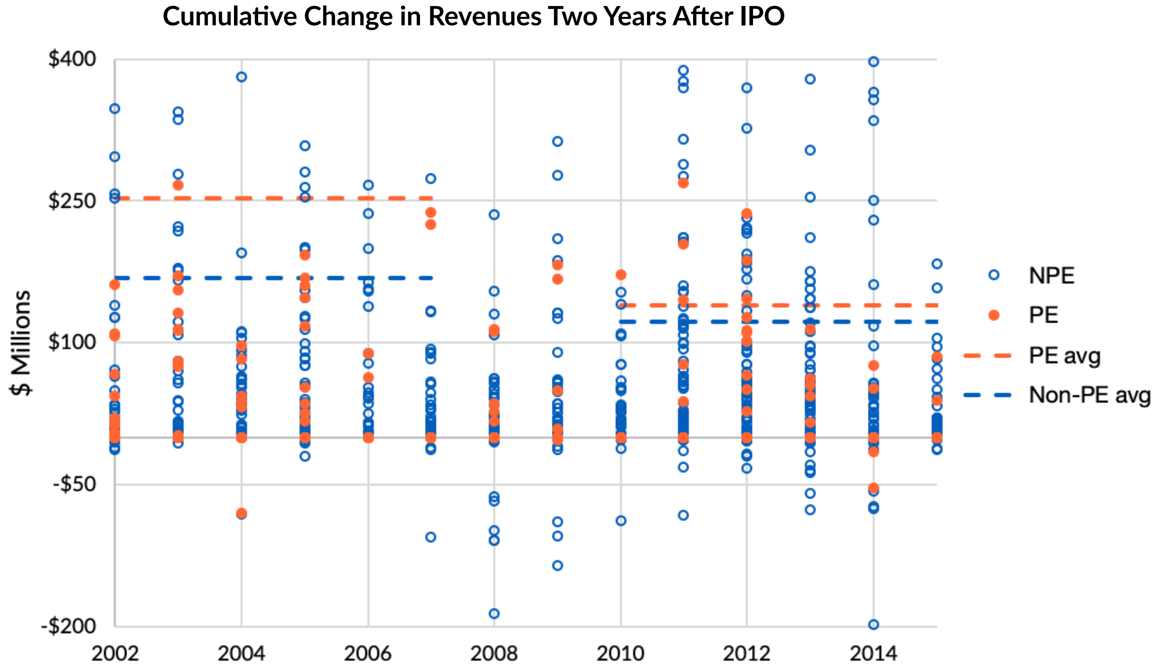
Revenue Quartile	1st	2nd	3rd	4th
PE average	61	86	185	361
Non-PE average	15	49	180	238
PE-Difference (Non-PE base)	46	37	5	123

Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

PE-BACKED IPOS DISPLAY MORE REVENUE GROWTH AND INVESTMENT SPENDING

Comparing their revenue-generating capabilities, PE-backed IPOs, on average, have consistently produced more revenues cumulatively during the two years after their IPO than non-PE-backed companies (Figure 6). Excluding the dampening effects from the 2008 recession, PE-backed IPOs earned an average of $196.3 million per year compared with $145.3 million for non-PE-backed companies. For the pre-recession period 2002-2007, PE-backed IPOs earned 50.3 percent more revenues than non-PE-backed companies (e.g., $252.9 million compared with $168.3 million).

Figure 6: PE-Backed IPOs Generated More Revenues After IPO



Cumulative Revenue Change Two Years After IPO
($ million)

Average 2002-2007		Average 2010-2015		Average 2002-2007, 2010-2015	
PE	252.9	**PE**	139.7	**PE**	196.3
Non-PE	168.3	**Non-PE**	122.2	**Non-PE**	145.3

Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

For the period after the recession (2010-2015), IPO revenue growth slowed, as would be expected during a time when the economy grew at the slowest pace in post-WWII history. However, the revenue falloff in this period was sharpest for PE-backed IPOs, which earned, on average, $139.7 million compared with $122 million for non-PE-backed IPOs.

Although the larger companies generally produced more revenues than did smaller companies, the size of the average revenue gain two years after the IPO was similar across all company sizes except those in the smallest revenue quartile (Figure 7 second panel and accompanying table). Nevertheless, the average PE-backed IPO produced more than twice the revenues of non-PE-backed companies (an average of $133 million compared with $58 million).

The largest difference in revenue generation capacity was among the companies in the small-to-mid-sized (2nd) and largest (4th) quartiles (revenues with $3 million to

$32 million and over $243 million). The earnings of former PE-owned companies in these categories exceeded that of their non-PE counterparts by the largest amounts (Figure 6 and accompanying table). Also, it is notable that these two mid-sized quartiles contained 71 percent of all PE-backed IPOs. However, in comparing the median companies for each quartile, PE-backed IPOs earned more revenues than their non-IPO peers did in all but the largest grouping (companies with revenues greater than $243 million). Revenues of this large PE-backed IPO fell short of those of the non-PE median company by $8 million.

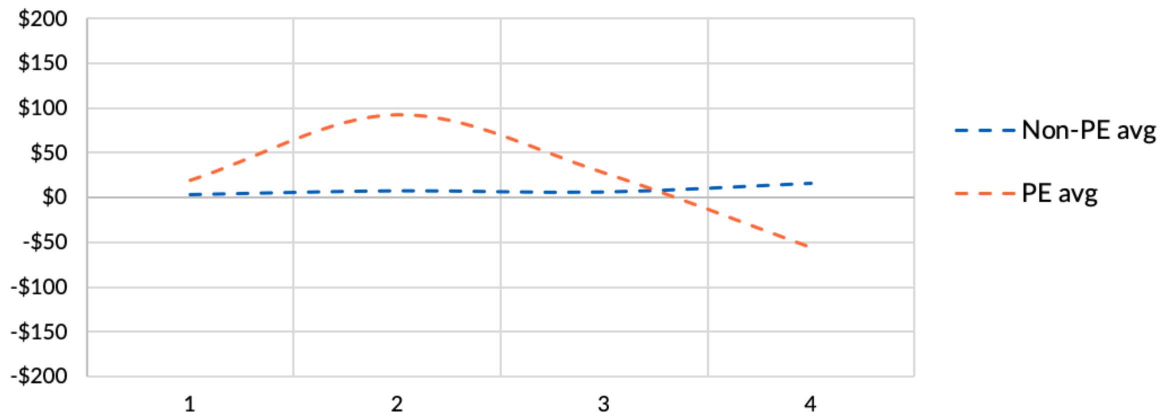
PE-BACKED IPOS INVESTED MORE THAN NON-PE-BACKED IPOS

In addition to generating more revenues, our data show that PE-backed companies, on average, invested more than the average non-PE-backed IPO (Figure 7). During 2002-2015 (excluding the recession years 2008 and 2009), PE-backed IPOs invested an average of $45 million a year, compared with $12.3 million for non-PE-backed companies. Indeed, before the 2008 recession, during the period 2002-2007, PE-backed IPO investment spending (on average) was 4.8 times that of non-PE-backed IPOs ($64 million compared with $13.4 million). As expected, with the drag on revenue growth during the post-recession period, capital expenditures declined for all companies, but much more so for PE-backed IPOs, whose investment spending on average dropped to $26.1 million, which was still more than double the pace of non-PE-backed IPOs.

Company size distinguishes and differentiates investment spending by PE- and non-PE-backed IPOs (Figure 7 and table). On average, the PE IPOs in the small- and mid-sized ($3–32 million revenue) group (second revenue quartile) spent the most on capital expenditures. Their spending exceeded the average spending by their non-PE-backed counterparts by more than 13 times ($93 million compared with $7 million). By comparison, the largest PE-backed IPOs reduced their investment spending by an average of $56 million during the two years after their IPO. Such behavior contrasts sharply with non-PE-backed IPOs of all sizes: They all increased investment spending (on average). Indeed, the non-PE-backed IPOs in the largest revenue quartile also had the largest average increase in investment spending ($16 million). However, the amount of this increase was smaller than the smallest increase in capital expenditures by PE-backed companies ($19 million).

Figure 7: PE-Backed IPOs Invest More than Non-PE-Backed Companies After IPO

Cumulative Two-Year Change in Capital Expenditure
($ millions; revenue quartiles)



Company Distribution by Revenue Quartile
($ million for quartile designation and number of companies)

	1st < $3	2nd $3 to $32	3rd $33 to $243	4th > $243
PE-Backed	7	25	45	26
Non-PE	239	220	200	220

Cumulative Two-Year Capital Expenditure
($ million)

Revenue Quartile	1st	2nd	3rd	4th	Total
PE net	$135	$2,325	$1,241	$(1,467)	$2,233
PE average	$19	$93	$28	$(56)	$22
Non-PE net	$728	$1,646	$1,210	$3,572	$7,157
Non-PE average	$3	$7	$6	$16	$8
Average difference	$16	$86	$22	$(73)	$51
Quartile share of average PE difference	32%	169%	43%	-144%	100%

Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

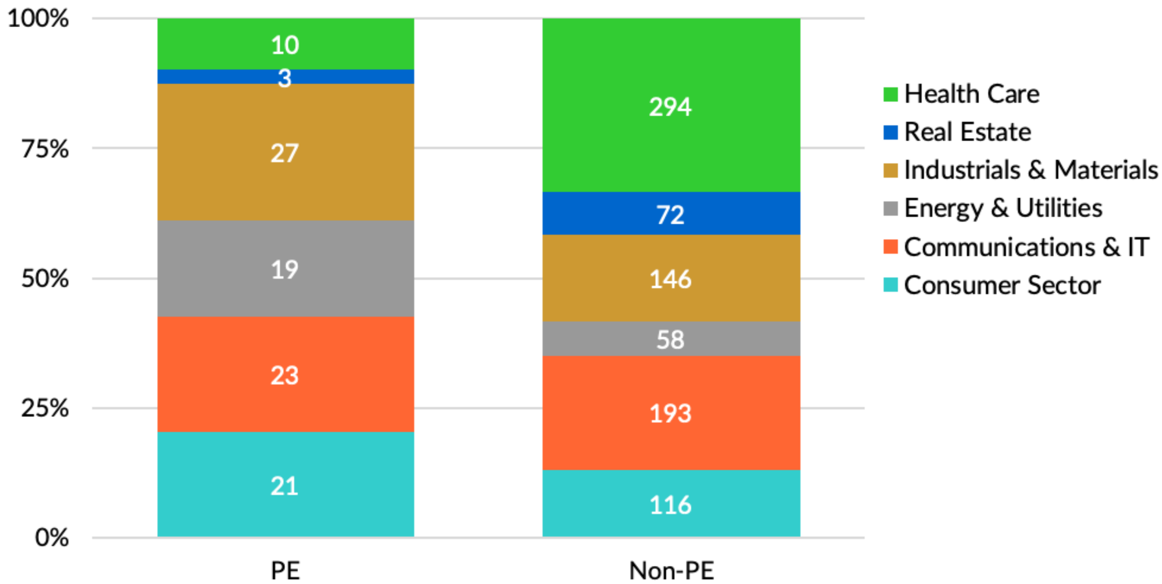
SECTOR-BY-SECTOR ANALYSIS VERIFIES PE-BACKED IPOS EMPLOY, EARN, AND INVEST MORE THAN PEERS

Differences in the sectoral composition of PE-backed versus non-PE-backed IPOs do not explain fully why the PE-backed companies appear to outperform in post-IPO job creation, revenue generation, and investment spending. Certainly, there are many examples where some post-IPO employment or investment spending by PE-alumni companies was larger than for companies in other sectors. For example, almost half of all PE-backed IPOs occurred in two relatively capital-intensive sectors (Figure 8): industrials and materials (26 percent) and communications and information technology (22 percent). By comparison, non-PE IPOs were more heavily concentrated (33 percent) in the labor-intensive health-care sector. Not surprisingly, total health-care capital expenditures after the IPO by all companies (on average) in our sample summed to less than 8.7 percent of the average capital expenditures by IPOs in the Industrial sector, where PE-backed IPOs were the most concentrated.

When considered on a sector-by-sector basis, our data show the following (on average) for PE-backed IPOs compared with their non-PE-backed IPO peers in that same sector (Table 1):

- They employ more workers.
- They generate more revenues (except for companies in the real estate sector).
- They spend more on capital expenditures (except companies in the health-care and real estate sectors).

Figure 8: Sectoral Distribution of PE and Non-PE Companies Differ



Sector Distribution of PE and Non-PE IPOs
(number of companies)

	Health Care	Real Estate	Industrials & Materials	Energy & Utilities	Comms & IT	Consumer Sector	Total
PE-Backed	10	3	27	19	23	21	**103**
Non-PE	294	72	146	58	193	116	**879**
Total	304	75	173	77	216	137	**982**

Percentage of PE and Non-PE Companies in Each Sector

	Health Care	Real Estate	Industrials & Materials	Energy & Utilities	Comms & IT	Consumer Sector	Total
PE-Backed	10%	3%	26%	18%	22%	20%	**100%**
Non-PE	33%	8%	17%	7%	22%	13%	**100%**

Source: Pitchbook and S&P Capital IQ, Refinitiv, and Milken Institute research (as of June 2018).

Industrials & Materials Sector

IPOs in the industrials and materials (IM) sector illustrate well the apparent positive influence of previous PE ownership on company performance. In our data, the IM sector contained the largest number of PE-backed IPOs (27 out of 103 PE-backed IPOs in our data), and PE-backed IPOs comprise 16 percent of the IPOs in this sector. PE-backed IPOs are concentrated among the largest companies (grouped by number of employees) (Table 2). Consequently, it is not surprising that most of the outperformance in job creation over companies that were not PE-backed originated from companies among the largest three quartiles of companies (those employing more than 269 workers).

What is surprising is that only the smaller PE-backed IPOs (i.e., companies in the first two quartiles with revenues below $348 million) demonstrated higher average (per company) revenue generation and investment spending compared with their non-PE-backed peers. In addition, comparing the median c3mpany in each revenue quartile further supports the relationship between company size and performance. Here again, the PE-backed IPOs in the smaller two quartiles produced more revenues and invested more than did non-PE backed companies. However, the reverse is true for the larger median companies. Interestingly, in the largest employment quartile of this sector, the PE-backed median IPO did not produce more jobs than did the non-PE-backed median company.

Communications & Information Technology Sector

The communications and information technology (CIT) sector in our data contained the second highest number of PE-backed IPOs (23 out of 103) and comprised 11 percent of all IPOs in the sector (Table 3). These few IPOs generated more than twice the number of jobs as the other IPOs within this sector combined. Company size did not matter for creating jobs in this instance. The PE-backed IPOs of all sizes generated more jobs than did their peers. This is also evident by comparing the median company in each size quartile.

Unlike the IPOs in the IM sector, it was the larger CIT companies that generated more of the revenues and invested more, on average, than did their non-PE-backed peers.

Consumer Sector

The consumer sector contained the third highest number (21 out of 103) of PE-backed IPOs, comprising 11 percent of all IPOs in the sector (Table 4). As with the previous two sectors, the few PE-backed IPOs produced more jobs, on average, than all other IPOs combined. In this case, companies in the largest grouping (those employing more than 5,209 workers) created most of the incremental jobs (on average).

The larger, PE-backed IPOs also generated most of the incremental revenues for the two years after the IPO, but it was the smallest group of companies (those with revenues below $41 million) that accounted for most of the incremental capital expenditures (of the PE-backed IPOs). One might speculate that in this relatively low-capital intensity sector, the smaller PE-backed IPOs sought to raise productivity by investing more than their competitors invest.

Health-Care Sector

In contrast to the preceding sectors, there were relatively few PE-backed IPOs in the health-care sector; PE-backed health-care IPOs (10 out of 103) represented only 3 percent of all PE-backed IPOs (Table 5). Nevertheless, these few companies created more than twice the number of jobs for the two years after the IPO than all other companies combined. Most of the jobs created stemmed from the seven largest PE-backed IPOs (with revenues greater than $17 million and employing more than 134 workers).

These 10 companies managed to generate revenues (on average) that exceeded the average revenues of non-PE-backed IPOs. However, this outperformance was mainly due to two companies in the third revenue quartile ($2–17 million), and such outperformance may be unusual.

Other Sectors with Few PE IPOs: Real Estate and Energy

Tables 6 and 7 display employment, revenue, and capital expenditure patterns in the energy/utility sectors and real estate sectors, respectively. The energy sector had more employment gains (on average) among PE-backed IPOs but did not have similar performance for revenue growth and capital expenditures. With only three IPOs in our data for the real estate sector, we hesitate to discuss the differences between PE and non-PE-backed IPOs but include the data for completeness.

A PROPOSED TEMPLATE FOR DISSEMINATING DATA FOR PE-OWNED COMPANIES

We understand the reluctance of PE firms to disclose information that may reveal trade secrets and operational improvements underlying company turnarounds that contribute to their out-sized returns. However, more institutional investors, especially public pension funds, reportedly are adding “sustainable” and “responsible” investing goals (e.g., Environmental, Social, and Governance, or ESG, targets) to their usual performance criteria.[16] This implies that there will be more pressure on PE firms to show that they are part of the “investable universe” generating positive social contributions as well as high returns.

16 Investing with a focus on environmental, social, and governance issues—known as ESG—now amounts to $12 trillion in the United States, according to a new report from the nonprofit US SIF: The Forum for Sustainable and Responsible Investment.

To dispel its negative image, critics demand that the PE industry demonstrates that PE interventions into acquiring businesses will leave local communities better off than alternatives without PE intervention. This requires that PE firms provide evidence of ultimate job gains and store and factory openings that offset the initially disruptive changes directed by PE managers.

PE data disclosure can be limited to "macro" variables that do not reveal proprietary information about the PE firm's value-enhancing techniques.

1. **Job creation and destruction:** At a minimum, PE firms should provide a time series detailing job losses and gains (or more precisely, full-time equivalent employees for each year a company is under PE ownership). Certainly, it would be important to distinguish between job changes due to organic growth or through a merger with other companies acquired by the PE firm.

2. **Income generation and distribution:** To satisfy the growing concern about rising income disparities, reporting the median income of company employees and its relationship to CEO compensation would help evaluate the degree to which PE firm activities meet ESG objectives.

3. **Productivity-improving capital expenditures**: Capital expenditures at the company level are vital incremental contributions to boosting aggregate productivity and creating better higher-paying jobs in the future. Disclosing annual capital expenditures and revenues (to scale the capital spending and compare across companies) would provide evidence that PE activities not only profit investors but also aim toward raising the community's standard of living. Additional details about the share of capital expenditures devoted to R&D, equipment, and property would be helpful, too.

We believe this template is a minimal start toward bringing transparency and accountability to PE activities that critics describe as "...vampires bleeding the company dry and walking away enriched as the company succumbs."

CONCLUDING THOUGHTS: PE-BACKED IPOS PUNCH WELL ABOVE THEIR WEIGHT CLASS

Our research uncovered evidence that challenges the negative cost-cutting, plant-closing, layoff-producing stereotype of private equity. While this image may be accurate for companies that remain under PE management, we find that companies that have graduated from PE ownership and listed on stock exchanges behaved differently. They tended to create more jobs, generate more revenues, and invest more than other companies that list.

Nevertheless, our investigation into the working of private equity is limited, and we would be the first to raise caution flags about generalizing our findings owing to our limited data. We do not know whether the post-IPO gains in creating more jobs, revenues, and capital expenditures outweigh the costs of potentially more severe efficiency-improving measures taken by PE firms while they owned the company. After exiting PE ownership, presumably viable companies should be able to grow their payrolls and expand their investment spending at a healthy pace. Nevertheless, PE ownership may not be the primary factor behind the over-performance of PE-backed IPOs. Indeed, we are not able to compare IPOs with the performance of companies that leave PE ownership through a merger with other companies or are sold to other PE firms. Indeed, we cannot assess net job creation, revenue generation, and investment spending even for IPOs because PE firms do not disclose performance data for the companies they own.

If PE firms do not make available relevant data to the public, the uncertainty caused by such opaqueness about the "dark" inner workings of private companies owned by private equity will likely amplify public fears and worsen their already negative image. The lack of transparency and a continued stream of negative anecdotal reports will likely increase concerns about the potentially harsh social and economic impact that the activities of private equity investors may have on target companies and local economies. Consequently, such rising social pressure may restrain and limit some institutional investors (e.g., public pension funds) from allocating investments to private equity. The possibility of such adverse reactions should make the collection and public release of relevant and standardized data a top priority for the PE industry.

Our analysis has focused on those companies that have left private equity ownership and listed on a stock exchange. Consequently, we benefited from having relevant data collected in a standardized and audited manner, as required by regulations for listed companies. Making more data available about how companies fare under PE ownership may ease concerns about their negative social impact and reduce the number of calls for regulating and limiting investment fund flows into private equity. Indeed, the small crack of transparency that allowed us to investigate the aftermath of private equity influence on their IPO alumni suggests more bright rainbows than dark clouds ahead.

Table 1: Overall Sector Distribution

Sector Distribution of Average Two-Year Employment Gains of PE and Non-PE IPOs
(number of workers per company)

	Health Care	Real Estate	Industrials & Materials	Energy & Utilities	Comms & IT	Consumer Sector
PE-Backed	266	38	913	359	596	1,084
Non-PE	126	90	373	107	268	1,048
Difference	140	-52	540	252	328	36

Sector Distribution of Average Two-Year Revenue Gains of PE and Non-PE IPOs
($ million per company)

	Health Care	Real Estate	Industrials & Materials	Energy & Utilities	Comms & IT	Consumer Sector
PE-Backed	72	13	158	73	149	185
Non-PE	42	97	133	-316	127	54
Difference	30	-84	25	389	22	131

Sector Distribution of Average Two-Year Capital Expenditures of PE and Non-PE IPOs
($ million per company)

	Health Care	Real Estate	Industrials & Materials	Energy & Utilities	Comms & IT	Consumer Sector
PE-Backed	0	0	31	18	12	35
Non-PE	1	22	12	27	7	7
Difference	-1	-22	19	-9	5	28

Table 2: Industrials & Materials Sector

Industrials & Materials Employment
(net jobs and per-company average)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	-2,670	9,137	2,754	15,434	24,655
PE Avg	-667	761	551	2572	913
Non-PE Net	8,316	11,770	18,350	16,092	54,527
Non-PE Avg	213	380	483	423	373
PE Difference	-880	381	68	2,149	540
PE Difference (of total difference)	-51%	22%	4%	125%	

Industrials & Materials Number of IPOs

Quartile	1st	2nd	3rd	4th	Total	Percent
PE-Backed	4	12	5	6	27	16%
Non-PE	39	31	38	38	146	84%
					173	

Industrials & Materials Net Revenue
(average)

Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	2,433	1,783	870	-829	4,258
PE Avg	608.4	148.6	173.9	-138.2	157.7
Non-PE Net	1,589	2,246	6,815	8,702	19,352
Non-PE Avg	40.7	72.5	179.3	229	132.6
PE Difference	567.6	76.2	-5.4	-367.2	25.1
PE Difference (of total difference)	209%	28%	-2%	-135%	

Table 2: Industrials & Materials Sector (Continued)

Industrials & Materials Capital Expenditure
(per-company average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	197	799	43	-205	835
PE Avg	49.3	66.6	8.7	-34.2	30.9
Non-PE Net	65	456	367	913	1,800
Non-PE Avg	1.7	14.7	9.7	24.0	12.3
PE Difference	47.6	51.9	-1.0	-58.2	19
PE Difference (of total difference)	81	88%	-2%	-99%	

Table 3: Communications & IT

Communications & IT Employment
(average; net jobs)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	1,575	4,127	7,280	722	13,704
PE Avg	525	825	728	144	596
Non-PE Net	22,339	17,291	20,010	-7,914	51,727
Non-PE Avg	438	353	455	-162	268
PE Difference	87	472	273	306	328
PE Difference (of total difference)	8%	41%	24%	27%	

Communications & IT Number of IPOs

Quartile	1st	2nd	3rd	4th	Total	Percent
PE-Backed	3	5	10	5	23	11%
Non-PE	51	49	44	49	193	89%
					216	

Table 3: Communications & IT (Continued)

Communications & IT Net Revenue
(average)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	125	220	2,025	1,057	3,426
PE Avg	41.6	44.0	202.5	211.3	149
Non-PE Net	6,992.7	2,867.6	5,960.8	8,783.4	24,604
Non-PE Avg	137.1	58.5	135.5	179.3	127
PE Difference	-95.5	-14.5	67.0	32.1	-11
PE Difference (of total difference)	873%	133%	-612%	-293%	

Communications & IT Capital Expenditure
(average)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	-49	5	186	143	285
PE Avg	-16.3	1.0	18.6	28.6	1.4
Non-PE Net	223	162	290	589	1,265
Non-PE Avg	4.4	3.3	6.6	12.0	0.1
PE Difference	-20.7	-2.3	12.0	16.6	1.3
PE Difference (of total difference)	-369%	-40%	214%	295%	

Communications & IT Net Revenue
(median; $ million)

Revenue Quartile	1st	2nd	3rd	4th
PE	43.8	37.5	66.1	117.0
Non-PE	18.3	33.0	58.7	174.8
PE Difference (Non-PE base)	25.5	4.5	7.4	-57.8

Table 4: Consumer Sector

Consumer Sector Employment
(average; net jobs)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	2,839	3,034	12,498	4,400	22,771
PE Avg	473	337	2,500	4,400	367
Non-PE Net	14,919	16,044	27,293	63,283	121,539
Non-PE Avg	533	642	941	1,861	34
PE Difference	-60	-305	1,559	2,539	333
PE Difference (of total difference)	-2%	-8%	42%	68%	

Consumer Sector Net Revenue
(average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	492	773	1,935	682	3,882
PE Avg	82.0	85.9	386.9	682.0	59
Non-PE Net	731.3	1,787.7	2,512.4	1,181.6	6,213
Non-PE Avg	26.1	71.5	86.6	34.8	2
PE Difference	55.9	14.4	300.3	647.2	57
PE Difference (of total difference)	5%	1%	30%	64%	

Table 4: Consumer Sector (Continued)

Consumer Sector Capital Expenditure
(average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	590	30	80	32	731
PE Avg	98.3	3.3	15.9	32	7.1
Non-PE Net	65	105	141	483	795
Non-PE Avg	2.3	4.2	4.9	14.2	0.2
PE Difference	96	-0.9	11	17.8	6.9
	77%	-1%	9%	14%	

Table 5: Health Care

Health-Care Employment
(average; net jobs)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	3	0	111	2,550	2,664
PE Avg	3	0	56	364	266
Non-PE Net	3,006	2,889	6,357	24,873	37,124
Non-PE Avg	40	38	86	360	126
PE Difference	-37	-38	-30	4	

Table 5: Health Care (Continued)

Health-Care Net Revenue
(average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	0	0	52	666	717
PE Avg	0.0	0.0	25.8	95.1	71.7
Non-PE Net	471.1	465.6	1,146.8	10,177.6	12,261.1
Non-PE Avg	6.3	6.1	15.5	147.5	41.7
PE Difference	-6.3	-6.1	10.3	-52.4	

Health-Care Capital Expenditure
(average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	0	2	0	-6	-5
PE Avg	0.0	0.3	-0.1	-3.1	-0.3
Non-PE Net	-2.5	15.0	61.4	155.1	229.0
Non-PE Avg	0.0	0.2	0.8	2.1	0.8
PE Difference	0.00	0.05	-0.93	-5.20	

Table 6: Energy & Utility

Energy & Utility Employment
(average; net jobs)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	290	887	3,008	2,639	6,824
PE Avg	145	99	501	1,320	109
Non-PE Net	843	1,413	227	3,712	6,195
Non-PE Avg	50	141	17	206	7
PE Difference	95	-43	484	1,114	107
	6%	-3%	29%	67%	

Energy & Utility Net Revenue
(average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	303	788	550	-263	1,379
PE Avg	151.7	87.6	91.7	-131.3	11
Non-PE Net	686.8	633.6	3,180.6	-22,851.0	-18,350
Non-PE Avg	40.4	63.4	244.7	-1,269.5	-16
PE Difference	111.3	24.2	-153.0	1,138.2	26
	10%	2%	-14%	102%	

Table 6: Energy & Utility (Continued)

Energy & Utility Capital Expenditure
(average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	405	1,483	-228	-1,318	343
PE Avg	202.7	164.8	-38	-659	-17
Non-PE Net	931.6	1,541	99.3	-1,030.4	1,542
Non-PE Avg	54.8	154.1	7.6	-57.2	3
PE Difference	147.9	10.7	-45.6	-601.8	-20
	-30%	-2%	9%	123%	

Table 7: Real Estate

Real Estate Employment
(average; net jobs)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	0	93	20	0	113
PE Avg	0	47	20	0	67
Non-PE Net	1,331	1,918	-2,314	5,555	6,490
Non-PE Avg	74	113	-129	292	351
PE Difference	-	-66	149	-	82
	-	-80%	149%	-	

Real Estate Number of IPOs

Quartile	1st	2nd	3rd	4th	Total	Percent
PE-Backed	0	2	1	0	3	4%
Non-PE	18	17	18	19	72	96%
					75	

Table 7: Real Estate (Continued)

Real Estate Net Revenue
(average)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	0	92	-54	0	38
PE Avg	0	46	-54	0	
Non-PE Net	1,387	774	1,416	3,411	6,988
Non-PE Avg	77	46	79	180	381
PE Difference	-	0	-133	-	-133
PE Difference (of total difference)	-	0%	100%	0%	

Real Estate Capital Expenditure
(average; $ million)

Revenue Quartile	1st	2nd	3rd	4th	Total/Average
PE Net	0	-2	1	0	-2
PE Avg	-	-1	1	-	0
Non-PE Net	-446	-51	-13	2,082	1,571
Non-PE Avg	-25	-3	-1	110	81
PE Difference	-	2	1	-	3
PE Difference (of total difference)	-	60%	40%	-	

Real Estate Net Revenue
(median; $ million)

Revenue Quartile	1st	2nd	3rd	4th
PE	-	22	-7	-
Non-PE	12	12	33	52
PE Difference (Non-PE base)	-	10	-40	-

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THE EFFECTS OF PRIVATE EQUITY AND VENTURE CAPITAL ON SALES AND EMPLOYMENT GROWTH IN SMALL AND MEDIUM-SIZED BUSINESSES

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ABSTRACT

We study the effects private equity (PE) and venture capital (VC) financing have on small and mid-sized single entity business establishments from 1995-2009. We focus on single entity establishments to cleanly examine the impact of PE and VC financing on establishments' organic growth. This study reveals that PE and VC financing have positive impacts on single entity business establishments' net sales and employment growth. The impact of PE financing on establishments' growth is slower and smaller than VC financing. However, we find that the benefit of PE financing lasts longer than VC financing. We also find that ethnic minority, female, and foreign business owners are less likely to receive PE and VC financing. Finally, we find evidence that although establishments with government contracts are more likely to receive PE and VC financing, those contracts fail to produce marginal post-funding growth and employment benefits.

JEL Classifications: G24; J23; J15; J16; L25
Keywords: private equity; venture capital; growth; employment; sales

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I. Introduction

Private equity, and to a larger extent, venture capital funds are often criticized for not producing sufficient returns to limited partners. Venture capital has returned just a 6.07% average annual return for the 10-year period ended September 30, 2012, while private equity averaged 13.71%.[2] Meanwhile, over the same time frame, the NASDAQ averaged 10.27% per year. Despite the weak performance, the number of private equity funds has grown considerably. Given the generally weak returns, one may wonder if private equity and venture capital investing play significant roles to provide capital to privately owned businesses to grow. Do private equity and venture capital provide capital for diverse groups of business owners? Do they contribute to increased revenue and employment? Recent studies have examined the relationships between private equity leveraged buyouts and job creation/destruction (Davis, Haltiwanger, Jarmin, Lerner, and Miranda, 2011; Amess and Wright, 2012) and offer somewhat mixed views. However, these studies are focused on large firms where the opportunities for cost cutting are significant and access to capital, mostly via public sources where costs are relatively cheap, is almost guaranteed. Existing studies also fail to examine organic growth as they do not utilize single entity establishment level data.

This study takes a closer look at two main roles of private equity (PE) and venture capital (VC) financing: (1) What types of business owners' characteristics are more likely to receive PE and VC financing? (2) What is the differential impact of private equity (PE) and venture capital (VC) financing on small and medium-sized business establishments' net sales and employment growth? We focus on single entity establishments to closely examine the impact of PE and VC

[2] Cambridge Associates LLC U.S. Venture Capital Index and Selected Benchmark Statistics, September 30, 2012. http://www.cambridgeassociates.com/pdf/Venture%20Capital%20Index.pdf. And Cambridge Associates LLC U.S. Private Equity Index and Selected Benchmark Statistics, September 30, 2012. http://www.cambridgeassociates.com/pdf/Private%20Equity%20Index.pdf.

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financing on business establishments' organic growth. By examining data at the single establishment level, we are better able to isolate the impacts of receiving capital on the single business entity that is the beneficiary of such financing. This approach reduces the confounding noise associated with investigating acquisition and divestiture activities within a corporate entity with multiple business units and aggregated firm level data. This differentiates our study from other research in this area. For example, compared to Davis, Haltiwanger, Jarmin, Lerner, and Miranda (2011), our study examines the impact of both PE and VC on single entity establishment sales growth in addition to employment growth. Compare to Puri and Zarutskie (2012) as well as Chemmanur, Krishnan and Nandy (2011), our study covers smaller business entities with lower numbers of employees and smaller revenues. Relative to Guo, Hotchkiss and Song (2011), our business establishments are significantly smaller than $100 million. Furthermore, our sample covers more recent private equity transactions--those occurring after 2006. More importantly, our study compares the timing and long lasting impact of PE and VC financing on net sales and employment growth at the single entity establishments level.

Neumark, Wall and Zhang (2011) find that small businesses create more jobs than the rest of their sample. Still, the benefits of private equity investment in small and mid-sized businesses are not completely understood, in part because access to private equity capital for most small and mid-sized businesses is elusive and, as a result, data are sparse.[3] In fact, according to a recent report by the *Pepperdine Private Capital Markets Project*, just 15% of businesses that attempted to tap private equity in the second quarter of 2012 were successful.[4] For small and mid-sized companies, obtaining capital from private equity and venture capital

[3] The Small Business Association defines small and mid-sized businesses as businesses with the average annual sales of $12 million with less than 500 employees. http://www.sba.gov/content/table-small-business-size-standards.
[4] Private Capital Access Survey Responses, Q2 2012.
http://bschool.pepperdine.edu/appliedresearch/research/pcmsurvey/content/Q2_2012_PCA.pdf.

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often determines whether or not these business establishments survive. The consequences to those not successful raising capital are often severe. Citing the same Pepperdine research, for those businesses seeking capital, a failed attempt is expected to yield the following results: slower revenue growth (71%); hiring fewer employees than planned (54%); and reducing the number of employees (23%). These data suggest that private equity and venture capital play more important roles regarding growth and job creation in the small and medium-sized business space than for large businesses.

Several studies report on the impacts of venture capital (VC) financing on firms' growth and efficiency. Engel and Keilbach (2002), Davila, Foster, and Gupta (2003), and Alemany and Marti (2005) empirically show that VC-backed firms have significantly higher revenues and employment growth rates than non-VC-backed firms. Chemmanur, Krishnan and Nandy (2011) also find that VC-backed firms have higher operating efficiency than non-VC-backed firms due to screening and monitoring. Puri and Zarutskie (2012) report a performance gap between VC and non-VC financed firms. However, little is known about the scarce allocation of venture capital among various types of business owners. According to the *Pepperdine Private Capital Markets Project*, just 9% of businesses that attempted to tap into venture capital in the second quarter of 2012 were successful.[5]

In this study, we utilize the Institute for Exceptional Growth Companies (IEGC) or National Establishment Time-Series (NETS) database, which includes employment time series data on over 44 million business establishments during 1990-2009. The NETS database is coupled with private equity and venture capital transaction information from the Pitchbook database, as well as financial data from Dun & Bradstreet from 1995-2009. Because our research

[5] Private Capital Access Survey Responses, Q2 2012. http://bschool.pepperdine.edu/appliedresearch/research/pcmsurvey/content/Q2_2012_PCA.pdf.

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focuses on the establishment level, these databases allow us to clearly investigate the impacts of PE and VC on organic growth of small and mid-sized businesses, which are vital to the economy.[6] To better understand their roles, our study investigates two relationships: 1) The owners' characteristics displayed that result in increased rates of successfully securing PE or VC financing; and 2) The revenue and employment growth (or destruction) that occurs with PE versus VC financing at these establishments.

In order to investigate, we begin by constructing matched pair samples between single entity establishments that received PE or VC financing with those that never received PE or VC financing (control group). We further refine our sample by selecting single entity establishments that have only grown organically. That is, our sample excludes those businesses that have engaged in acquisitions or divestitures. We also analyze and present the results for establishments that received only one round of PE or VC financing instead of those with multiple rounds of financing. We find consistent evidence that minority (non-Caucasian), women, and foreign business owners' establishments are significantly less likely to receive PE and VC financing. This finding is consistent with the existing literature (Carter and Allen, 1997; Robb and Fairlie, 2007; Cole and Mehran, 2011).

We also find that PE financing is not immediately impactful, either negatively or positively, in affecting the establishments' sales and employment growth rates in the year of financing. However, we do find that PE financing increases establishments' sales and employment growth rates for three consecutive years after funding. This finding suggests it takes some time to develop and execute on new strategies. By contrast, we find that VC financing immediately increases establishments' sales and employment growth rates indicating that a VC

[6] Small businesses represent 63% of net new private-sector jobs, 48.5% of private sector employment, and 46% of private-sector output. SBA Office of Advocacy, Frequently Asked Question, March 2014. http://www.sba.gov/sites/default/files/FAQ_March_2014_0.pdf..

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capital infusion is crucial for these businesses to execute their strategies. These findings indicate that PE and VC financing provide different impacts in terms of timing and sustainability of growth for small and mid-sized single entity business establishments. Our analysis also shows that business establishments with the government contracts are more likely to secure PE or VC financing. However, establishments with government contracts do not necessarily have higher sales and employment growth. While government contracts provide certifications and stable cash flows that allow business owners to secure funding from PE or VC, government contracts themselves do not provide growth. Our findings are robust throughout all additional tests.

The rest of this paper is organized as follows. In section II, we discuss existing literature that is relevant to our study. Section III describes the database comprised of IEGC (NETS), Dun & Bradstreet (D&B), and Pitchbook data, matching process, sample distribution, and univariate analysis. Section IV explains the methodology of regression estimations, hypotheses, and structural regression models. Section V discusses the first stage and second stage regression results. We examine the results from additional tests and robustness checks in section VI. Finally, section VII concludes with a summary of the main contributions of this study.

II. Literature review

Several studies have examined the impact of business owners' access to capital and demographics on firms' growth. Although the growth of women- and minority-owned businesses are increasing at a rapid rate, it has been shown that both demographics are less likely to access venture capital. Each demographic group experiences their own set of complications that has fostered varying ideologies on their competency, affected their firms' leverage, and has further altered their confidence in their ability to secure external financing.

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Bates and Bradford (2008) report that minority-owned firms are capital constrained, which could be attributed to their differential treatment in financial markets. Robb (2012) reports minority-owned businesses experience higher loan denial probabilities and pay higher interest rates than non-minority-owned businesses. Hedge and Tumlinson (2011) identifies that VCs on average are more likely to invest in a startup when the VC and company have top level personnel of the same ethnicity, and co-ethnicity's predictive power is highest for early-round investments. Interestingly, Hedge and Tumlinson (2011) also finds that VCs tend to invest in geographically close companies, because collocation, like co-ethnicity, arguably facilitates superior monitoring and management of investments (Lerner 1995; Sorenson and Stuart 2005). The tendency of individuals to associate with others based on similar ascriptive characteristics is frequently referred to as homophily (Becker-Blease and Sohl 2007). These facts further reveal the disadvantage that minorities experience when seeking external financing. However, these financial restraints are not restricted to minorities only, but affect women entrepreneurs as well.

Women-owned businesses faced greater credit constraints than did similar startups owned by men and were slightly less likely to have high credit scores, compared with men (Robb 2012). Cole and Mehran (2011) further explain this in their findings that female business owners' firms are more likely to be credit constrained because they are more likely to be discouraged from applying for credit and more likely to be denied credit when they do apply. After conducting a study on the availability of credit to entrepreneurs of both genders, Marlow and Patton (2005) determined that women reported fewer problems with bank finance because they were less inclined to apply for such funding in the first instance as they presupposed failure.

Becker, Blease, and Sohl (2007) determine that women business owners are more likely to use angel capital financing rather than venture capital, but still receive a smaller amount of

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external financing than their male colleagues. Unlike venture capital transactions, PE backed buyouts are much less likely to involve multiple rounds of financing (Valkama et al., 2013). This could further explain why women and minorities receive less capital to start and manage their ventures.

Studies have correlated the potential success of a start-up with the amount of equity financing it secures during the early stages of the process (Becker-Blease and Sohl, 2007). Robb (2012) establishes that Blacks and Hispanics start their firms with about half the capital that Whites use and women start their firms with a little over half of what men invest. Fairlie and Robb (2009) find that women-owned businesses prove to be less successful because they have less startup capital, less business human capital, and less prior work experience. If women are actively discriminated against or, due to lack of business experience or bargaining acumen, are in inherently weaker bargaining positions, women-owned businesses may receive capital investments at relatively unattractive rates compared to male-owned businesses (Becker-Blease and Sohl, 2007). Carter elaborates upon this argument to suggest that female-owned firms underperform in almost every respect in comparison to those owned by men and this can be linked directly to the issue of undercapitalization (Marlow and Patton 2005). This implies that the demand for external capital is higher for women and ethnic minority business owners. However, they are facing greater constraints to obtain external financing.

Lower levels of access to start-up capital frequently results in lower sales and profits, less employment, and higher business failure rates. In the first several years after receiving VC, VC-financed firms typically grow rapidly in terms of employment and sales relative to non-VC-financed firms and have lower failure rates relative to matched non-VC-financed firms (Puri and Zarutskie 2012). Carter and Allen (1997) find that the focus on the financial aspects of the

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business amount and effort required to obtain financial resources overwhelms the women entrepreneurs' lifestyle intentions and, thus, their chances for growth.

Based on the strand of these existing studies, we expect that owners' demographic characteristics significantly influence the likelihood of a business establishment to secure funding from VC and PE. In the first stage, our study examines the likelihood of female, ethnic minority, and business owners with foreign status[7] to successfully obtain PE or VC financing. Unlike other studies, however, we investigate at the establishment level.

The literature on the role of private equity continues to evolve with growth in the industry. Much of the research concerns performance, governance and ownership structure, operations, and value; however, more recently there has been increased focus on the intersection of jobs and financing, in part because of more plentiful data for analysis. Guo, Hotchkiss and Song (2011) examine 192 leveraged buyouts (LBOs) transactions with at least $100 million from 1990 to 2006 and compare it with the buyouts in the 1980s. They find that recent LBOs are more conservatively priced and use less leverage. They also find that LBOs provide significantly higher pre- and post-buyout returns while the impact on firms' operating performance is somewhat positive. Amess and Wright (2012) examine a data set of 533 LBOs from 1993-2004 and conclude that LBOs have no net employment effects. However, these LBO studies mostly focus on large firms and do not examine the impact of PE financing at the establishment level. A recent study by Davis, Haltiwanger, Jarmin, Lerner, and Miranda (2011) (DHJLM 2011 hereafter) examined this topic more thoroughly by analyzing 3,200 targets and their 150,000 establishments from Capital IQ, Dealogic, Thomson Reuters SDC, VentureXpert, and the Longitudinal Business Database (LBD) at the U.S. Census Bureau. They conclude that LBOs

[7] Existing literature has been salient about the ability of small business owners with foreign status to raise capital. Because business owners with foreign status face greater regulatory scrutiny, we believe that foreign business owners also face constraints when raising capital from PE or VC.

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result in significant job creation and destruction, which ultimately creates a loss of less than one percent of initial employment. However, their study does not examine the impact of LBOs on establishments' net sales growth.

Boucly, Sraer, Thesmar (2011) examine the impacts of LBOs on French firms and find that corporate behavior is affected. Targets become more profitable and grow faster than their peer group. They also increase capital expenditures. This research contrasts with previous studies that report less investment and/or downsizing. Tykvova and Borell (2012) examine a sample of European companies and report that LBO targets operate at reasonable debt limits, suggesting capacity for increased capital expenditures and growth opportunities. Lerner, Sorensen, and Stromberg (2011) investigate whether LBOs affect the firm's focus on long-term innovations. They find that patents applied for by firms in private equity transactions are more cited and show no significant shifts in the fundamental growth of innovations.

The literature on venture capital (VC) is largely concentrated on the role of the VC to generate information and to act as an intermediary between business owners and external investors. Puri and Zarutskie (2012) discern that venture capitalists might push their companies hard to grow quickly, deciding relatively rapidly which firms have the best chance of achieving a successful exit and terminating those that do not in the interest of allocating more capital to the likely winners in their portfolios. Gompers and Lerner (1999a) examine the role of venture capital firms on certifying initial public offerings (IPOs) of firms in which they invest. The role of venture capitalists is to generate information about these privately held firms prior to going public.[8] Existing studies also examine the role of VC on corporate governance of the firms

[8] Gompers and Lerner (1999b) and Metrick (2007) provide complete coverage of characteristics, investment behavior, and roles venture capitalists play in private firms. Lerner and Schoar (2004) investigate the liquidity of private equity and venture capital investments. Phalippou and Gottschalg (2009) point out that private equity funds underperform the S&P 500 by 3%. Metrick and Yasuda (2010) contrast the performance and fee structure in private

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beyond its traditional financial intermediary role. Hellmann and Puri (2002) indicate that VCs play an important role in firms' management including replacing founder CEOs with external CEOs. Kaplan and Stromberg (2003) show that VCs set extensive corporate governance and incentive structures at the time of their initial investments. Recent studies on venture capital (VC) financing focus on the impact of VCs on firms' growth and operating performance. Chemanur, Krishnan, and Nandy (2011) find that venture capitalists contribute to firms' efficiency by screening the firms with higher efficiency prior to financing and by monitoring the firms during VC financing. They find that efficiency gains come from both increase in sales and lower production costs. Puri and Zarutskie (2012) find that VC-financed firms have lower failure rates and are larger but not more profitable than non-VC firms. However, these studies have not examined the impact of VC financing at the establishments level.

A few studies examine the role of VC on firms' sales and employment growth. Engel and Keilbach (2002) find that German firms that receive venture capital (VC) financing display higher sales growth rates. They find that VC helps business owners commercialize their products rather than to foster new innovations. Davila, Foster, and Gupta (2003) examine 193 VC-backed firms and compare them with 301 non-VC-backed U.S. firms and discover the positive impact of VC financing on firms' subsequent valuation and employment growth. Alemany and Marti (2005) examine the role of VC on small businesses in Spain and find that employment, sales, gross margin, total assets, intangible assets, and corporate taxes grow faster in VC-backed firms than non-VC-backed firms over three consecutive years. Puri and Zarutskie (2012) studied this further and found that after VC financing, companies saw a very rapid growth in the employment

equity funds from buyouts versus venture capital. Lerner (2011) indicates a declining trend of private equity in recent years. Ivashina and Kovner (2011) find that firms that received private equity financing also receive favorable loan terms. Demiroglu and James (2010) find that the reputation of the private equity group determines the success of LBO transactions.

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of VC-financed firms relative to non-VC-financed firms. While VC-financed and non-VC-financed firms are matched at an average of 26 employees each, three years later VC-financed firms have on average 55 employees while non-VC-financed firms have 38 employees (Puri and Zarutskie 2012). Therefore, growth in variables such as sales, gross margin, and employment should be related to the increase in assets that results from both VC funding and an easier access to other external sources of funds (Martí, Menéndez-Requejo, and Rottke 2013).

Beck, Demirgüç-Kunt, and Maksimovic (2008) report that firms that undergo more financial obstacles tend to use more external financing. Commonly, this results in a cycle of more financial obstacles and the need for more external financing. With a study conducted in 48 countries, Beck et al. (2008) concluded that firm size, financial development, and property rights protection were important factors in explaining the observed variation in financing patterns. By comparison, larger firms are able to rely on different sources of external financing in order to increase capital with more ease than small firms.

Overall, the existing literature indicates that the impact of both PE and VC on firms' growth and operating performance is still mixed. More importantly, the literature has not made a direct comparison between the timing and long lasting impact of PE versus VC financing on the single entity establishment level for small and mid-sized businesses. Therefore, there still exists a significant knowledge gap with regard to understanding the role of private equity and venture capital on small and mid-sized establishments' growth and employment where access to capital is unlikely for most. Our research fills this gap.

III. Sample data

This study utilizes the Institute for Exceptional Growth Companies (IEGC) database, which includes the National Establishment Time-Series (NETS) data provided from Walls &

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Associates.[9] Walls & Associates in collaboration with Dun & Bradstreet (D&B) marketing information created the entire NETS database, which contains 350 longitudinal data variables such as annual net sales, employment, business owners' demographic, and geographic locations for 44,241,504 business establishments between January 1990 and January 2010.[10] Several studies have utilized and have validated the accuracy of the NETS database (Neumark, Wall, and Zhang, 2011; Toffel and Short, 2011; Levine and Toffel, 2010).[11] We compare the NETS database with U.S. Census data. Panel A of Appendix A presents a comparison of total employment from the Business Dynamic Statistics data from the U.S. Census with the NETS database. We find that NETS contains a higher number of establishments and therefore reports larger employment numbers from 1995 to 2010.[12] Neumark, Wall, and Zhang (2011) explain that employment from the NETS database is larger than U.S. Census data because NETS counts each job in each business establishment and the NETS has better coverage of small business owners than the U.S. Census.

We also compare the total net sales receipts between the Statistics of U.S. Businesses and NETS for 1997, 2002, and 2007.[13] Panel B of Appendix A shows that the total sales receipts from NETS is smaller than sales receipts from the Statistics of U.S. Businesses despite NETS containing more establishments. These findings suggest that the NETS database may

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[9] Information for the NETS database variables is available online from the Institute for Exceptional Growth Companies (IEGC) at http://143.235.14.134/downloads/NETSDatabaseDescription2013.pdf.

[10] Walls & Associates estimates establishment sales by using the firm-level reported sales (when available) and employment to allocate sales to all of the firm's establishments (even though some may be "intermediate production and distribution facilities"). The point is that these establishments will not directly have sales; but the estimates are intended to capture their overall contribution to revenue of the firm. Employment for each establishment in the NETS database is an actual number of employees rather than an estimated number of employees. January 1990 represents 1989 calendar year data and January 2010 represents 2009 calendar year data.

[11] See http://143.235.14.134/insights.iegc for a complete list of existing studies that utilize the NETS and D&B database.

[12] The Business Dynamic Statistics data from the U.S. Census is compiled every mid-March while the NETS database is compiled every January.

[13] The Statistics of U.S. Businesses collects total sales receipts every 5 years. The first year collected relevant to our study is 1992.

overestimate the numbers of employment and/or it may underestimate the net sales receipt per establishment. To address these concerns, we conduct two additional robustness tests to verify our results in Section IV.

The IEGC merged the NETS database with data from Pitchbook, which contains information on whether these establishments received private equity (PE) or venture capital (VC) investment, was acquired by other firms, or is in the process of going public.[14] The Pitchbook data consists of private financing deals on over 35,000 establishments during 1995 to 2009 and it indicates whether a business establishment receives PE or VC financing (see Appendix B).

The Pitchbook and NETS merged ("POF" data) is provided directly from the Institute for Exceptional Growth Companies (IEGC).[15] It consists of 26,838 observations across 16,482 establishments because some establishments received multiple rounds of financing (see Panel A of Table 1). We find 16,802 observations are financed from private equity and 7,555 observations are financed from venture capital from 1995 to 2009. The rest of the 2,481 observations are either acquired or are in the process of going public. Panel B of Table 1 indicates that over 57% of these establishments are privately held companies and 23% were acquired or merged with other firms.

3.1 Matching process

We merge the POF data back to the IEGC data to find matched establishments (control establishments) for these 16,802 establishments that received PE financing and 7,555

[14] Information for the Pitchbook data is available at http://pitchbook.com/PitchBook_Research.html.
[15] Walls and Associates merged NETS and Pitchbook data based on the establishment name, location, and HQDUNS (headquarter DUNS number). They also matched based on the timing of the NETS and Pitchbook data (i.e. January 1996 NETS data is merged with 1995 year-end Pitchbook data since the NETS data is updated every January and the Pitchbook data is updated at the end of the calendar year). The merging process is explained and available at http://growtheconomy.org/data.lasso and http://growtheconomy.org/faq.lasso.

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establishments that received VC financing. We select single entity business establishments that never engaged in acquisitions and/or sales or purchases of business entities, over the entire sample periods. We define single entity business establishments as establishments with no branches, subsidiaries, or establishments in other locations.[16] To be included, the control (matching) establishments must not have received PE or VC financing during the entire period of 1995 to 2009. Therefore, the control establishments are not found in the Pitchbook database. The control establishments also never engaged in acquisitions, sale, or purchase of business entities, and also meet our criteria as single entity establishments. The matching process is conducted each year at the establishment level rather than at the parent companies level given both the NETS and Pitchbook data are at the establishment level. DHJLM (2011) indicate that the establishment level data provides a clean analysis for organic job creation or destruction at each business establishment by separating it from the acquisitions and sale of operating units. The matching process is conducted with replacements because the control establishments have similar opportunities to obtain PE or VC financing as the PE or VC-financed establishments.

We create matches for the PE-financed establishments with the control establishments based on the 2-digit Standard Industrial Classification (SIC) code, annual net sales, and number of employees during the same corresponding years when the establishments received PE financing. We match-pair the VC financing establishments with non-VC financing (control) establishments based on the 2-digit SIC code, annual net sales, number of employees, and state where establishments are located during the same corresponding years when establishments received VC financing. We include states as one of the matching criteria for VC because VC-investment portfolio companies and similar technologies are usually regionally confined while

[16] The NETS (IEGC) database contains information regarding subsidiaries (Subsidiary) and number of establishments (Kids). We define single entity establishments as establishments with zero Subsidiary and zero Kids.

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PE portfolio companies are more likely to be distributed nationwide. We require both PE and VC control establishments to have different D-U-N-S headquarters numbers indicating that the control establishments are different from the PE and VC-financed establishments. This produces our match-pair sample.

[Insert Table 1 here]

Table 1 indicates that we find 13,538 (80%) matches for PE financing and 6,800 (90%) for VC financing. However, 40% of PE matches and 53% of VC matches have missing data such as net sales, number of employees, and other important variables. We also applied a 1% right tail truncation due to outliers from annual sales growth and employment growth. There are 4,138 of PE matches with multiple establishments and 811 VC matches with multiple establishments. Since we restrict our sample based on our definition of single entity establishments to cleanly examine the impact of PE and VC financing on establishments' organic growth and other sample selection criteria stated above, the final sample consists of 3,874 establishments that received PE financing and 3,074 of these establishments received only one round of PE financing. These establishments that received PE financing also never received VC financing. Similarly, we find 2,291 establishments received VC financing and 756 of these establishments received VC financing once. These establishments never received PE financing. In panel D of Table 1, we show that over 31% of establishments received multiple rounds of PE and over 69% of establishments received multiple rounds of VC financing. This implies that VC tends to provide more rounds of financing to these establishments than PE.

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[Insert Table 2 here]

3.2 Sample distribution

Table 2 provides a description of our final match-pair sample and 44,241,504 business establishments from the whole IEGC (NETS) database across 48 Fama-French industry classifications (Fama and French, 1997). The majority of establishments that received PE and VC financing are classified under business services (SIC 73)[17] and wholesale (SIC 50) industries, which is consistent with the entire IEGC database. Private equity tends to finance wholesale, retail, transportation, and other establishments that generate consistent cash flows and produce machinery while venture capital tends to finance establishments that produce new innovations such as computers, computer chips, and medical equipment. Business establishments from the IEGC (NETS) database are also highly concentrated in business services (SIC 73).

Table 3 indicates that there is geographic clustering for most establishments that received PE or VC financing. The highest concentrations of establishments that received PE financing are located in California (12.85%), Texas (9.19%), New York (7.49%), and Florida (5.63%). Similarly, establishments from the entire IEGC (NETS) database are also concentrated in California, Texas, Florida, and New York. Most establishments that received VC financing reside in California (43.13%), Massachusetts (12.53%), Texas (4.89%), and New York (5.06%). Overall, the match-pair sample for both PE and VC-financed are consistent with the IEGC (NETS) database.

[17] SIC 73 is defined as establishments that primarily engaged in rendering services to business establishments on a contract or fee basis, such as advertising, credit reporting, collection of claims, mailing, reproduction, stenographic, news syndicates, computer programming, photocopying, duplicating, data processing, services to buildings, and supply services.

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[Insert Table 3 here]

Table 4 presents the Pearson correlation coefficients among variables that are relevant in this study for the match-pair sample. The correlation coefficients are examined for both establishments that received financing and their corresponding control establishments that never received financing. Panel A of Table 4 presents the correlations for PE financing establishments relative to their corresponding control establishments. We find that there is positive and significant correlations between receiving PE financing (PEFUNDED) and annual employment growth on the corresponding year (EMPGR0) and sales growth (SALEGR0).We find business owners who are considered minority (non-Caucasian), female gender, and foreign status are negatively correlated with PE financing. We also find that a decrease in Dun & Bradstreet credit rating (CHGDBR-) increases the likelihood of PE financing and vice versa. This evidence suggests that there may be a substitution effect between bank loans and PE financing. The Dun & Bradstreet change in maximum Paydex scores (CHGPAYDEX) are positively related with receiving PE financing indicating that establishments with slower payments are less likely to obtain PE financing. We also find that higher levels of unemployment rates in the county (UNEMP) where the establishment resides is negatively related to PE financing. We find establishments with government contracts (GCONTRACT) are positively related with PE financing while establishments with a legal status of a corporation (CORP) and older establishments (FIRMAGE) are negatively related with PE financing. We find no significant correlations for PE financing across different major states, except Texas.

[Insert Table 4 here]

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Panel B of Table 4 presents the correlations for VC financing establishments relative to their corresponding control establishments. We find a positive and significant correlation between receiving VC financing (VCFUNDED) and annual sales (SALEGR0) and employment growth (EMPGR0) on the corresponding year. We also find that business owners who are considered minority (non-Caucasian), female gender, and foreign status are negatively correlated with obtaining VC financing. We find that the previous year changes in establishments' net sales and employment are positively related with receiving VC financing. This indicates that VCs are searching for establishments with high growth in the prior year. We also find that the higher level of unemployment rate in the county where the establishment resides is negatively related with VC financing. We find establishment with government contracts and corporations are positively related to VC financing while older establishments are negatively related with VC funding. VC financing is positively correlated with California and Massachusetts (CA and MA) and negatively related with New York and Texas (NY and TX). We also do not find significantly high correlations among the independent variables that are used in our regressions for both PE and VC. Therefore, we do not expect multicollinearity issues on our analysis.

[Insert Table 5 here]

3.3 Univariate analysis

Table 5 provides the univariate analysis for establishments that received PE or VC financing compared to their corresponding control group that never received PE or VC financing. Panel A of Table 5 indicates that PE financing is less likely to be accessed by minority owners,

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female owners, and owners with foreign status. Establishments with PE financing have a larger reduction in their Paydex score than their control group. We also find establishments with PE financing have the change in their D&B rating toward lower ratings suggesting PE financing may act as a substitute for bank loans. We find that establishments with PE financing reside in the counties with lower unemployment rates than their control group. Corporations and older establishments have a lower likelihood of obtaining PE financing while establishments with government contracts tend to have a higher likelihood of PE funding.

Prior to a financing event, the annual net sales and number of employees are not statistically significant, which indicates that our matching process yields a very close control entity for each establishment that received PE financing. On average, the annual net sales of our sample companies with PE financing are $8.96 million and the average number of employees is 95.[18] Comparing our sample with DHJLM (2011), we find that our sample firms have a significantly lower numbers of employees.[19] This difference in firms' sizes between our sample and DHJLM (2011) yields different results when we compare our results with theirs.

Panel B of Table 5 presents the univariate analysis for VC financing versus establishments that never received any financing from PE or VC. VC financing is less likely to be given to minority owners, female owners, and foreign owners. We also find establishments with VC financing have a change in their D&B rating toward lower rating suggesting VC financing may act as a substitute for bank loans. We find that the change in annual net sales and the change in number of employees in one year prior to VC financing are higher than the control group. This indicates that VCs are funding establishments with higher growth in the year prior to their financing. We find that most establishments with VC financing are corporations, younger,

[18] The untabulated median annual net sales is only $3.5 million and the median for number of employees is only 40 employees.
[19] Figure 4 of Davis et al. (2011) shows that over 90% of private equity target firms' buyouts have 500+ employees.

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and those with government contracts. The untabulated average annual sales on the VC sample are $6 million (median $2.9 million) and the average number of employees is 45 (median 30) employees.

We also compare the samples of PE and VC-funded establishments with all establishments in the NETS (IEGC) database. The third column of Table 5 presents the summary statistics for all establishments in the NETS database. We find that owners' demographics of our PE-funded sample are not statistically different from the entire NETS database, except for the percentage of women CEOs (WCEO). We find that NETS has a higher average of WCEO than our PE-funded sample. We find the VC-funded sample has significantly lower percentages of foreign owners (FOREIGN) and women CEOs (WCEO) compared to NETS. We also find that the VC-funded sample has a larger change in employment (CHGEMP) than NETS. We find that there are significant differences in the percentage of corporations (CORP), firm age (FIRMAGE), and percentage of government contracts (GCONTRACT) between PE and VC-funded samples within the entire NETS database. Therefore, we advise readers to interpret and to generalize our results with caution.

[Insert Table 6 here]

Table 6 presents the univariate analysis for differences-in-differences to examine the impact of PE and VC financing on establishments' annual sales and employment during one year prior versus one year after financing and compares those establishments with the control establishments that never received PE or VC financing during the same periods. We find that the change in annual net sales for establishments with PE financing is $1.443 million higher than

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those without PE financing during the year prior to one year after financing. We also find that establishments with PE financing create five more employees during one year prior to one year after financing compared to their control group. Compared to PE financing, VC financing has a smaller impact on the establishment change in net sales ($1.167 million), but a higher impact on change in employment (15 employees). Overall, we find that the change in net sales and the change in employment for those establishments with PE or VC financing are significantly higher than their control group during post-PE or VC financing relative to the pre-financing period.

[Insert Figure 1 here]

We trace the impact of PE financing on the level of annual net sales (inflation adjusted to 1984 dollars) and number of employees starting from five years *prior* to five years *after* receiving financing. Figure 1 presents the average annual net sales for establishments that received PE or VC financing relative to their control establishments. The average net sales for establishments that receive PE financing are lower than their control establishments during five years *prior* to receiving financing. However, net sales for establishments that receive PE financing surpass their control establishments during the period over which they are PE-backed. The average increase in net sales for establishments with PE financing during the entire five years after financing is approximately $8.4 million compared to a $6.4 million increase in sales for control establishments without PE financing. This implies that establishments with PE financing achieve 31% more net sales growth than their control establishments over the 5-year period following a PE investment.

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The average net sales for establishments with VC financing for five years prior to financing is lower than their control establishments. However, net sales for establishments that receive VC financing surpass their control establishments during the VC financing period. During the five-year period after a financing event, establishments with VC financing experience an average increase of $11.5 million in their net sales compared to an average increase of $5.2 million for their control establishments. Establishments with VC financing have approximately $6.3 million higher annual net sales per establishment relative to their control establishments five years after their financing event. The result for VC is as expected as the growth trajectories of smaller firms, particularly with funding, are higher than other firms. VC funds typically target start-up or early-stage businesses that are engaged in the development and production of new technologies and medical advances. New investments in these establishments often accelerate commercialization and growth opportunities. These aggressive ramp-ups, which often involve significant jobs and revenue increases, build on relatively small asset bases as compared to the targets of PE firms. Thus, VC investment is more impactful than PE investment. Overall, this highlights the role of PE and VC financing for small and medium-sized establishments to generate higher annual net sales.

Figure 2 presents the number of employees for establishments that received PE or VC financing relative to their control establishments from five years prior to five years after financing. We find that the number of employees for both PE and VC-funded samples during 5 to 3 years prior to PE and VC financing (-5 to -3 periods) stay relatively constant. Therefore, we focus our analysis to the period beginning 2 years prior to the financing event. Similar to annual net sales, the average employment for establishments that receive PE financing is approximately the same as their control establishments during one and two years prior to receiving financing.

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However, employment for establishments that received PE financing surpassed their control establishments during the PE financing inception period. Five years after a PE financing event, establishments with PE financing have 48 more employees on average as compared to 42 more employees for their control establishments. This implies that establishments with PE financing have 14% more jobs growth than their control establishments over the five years after a PE financing event. At the end of five years following the financing event, establishments with PE financing employ 6 more employees per establishment than their control establishments.

[Insert Figure 2 here]

The average number of employees for establishments with VC financing during two years and one year prior to financing is lower than their control establishments. However, employment for establishments that received VC financing surpassed their control establishments in three years after the VC financing inception period. Establishments with VC financing have over 57 more employees per establishment compared to their control establishments at the end of five years after the financing event. Consistent with the result for revenues, a financing event accelerates the growth prospects in greater magnitude for VC-backed firms than for those PE-backed firms. Figure 2 displays the critical role PE and VC financing plays to provide significantly higher employment opportunities in the economy for small and medium-sized single entity establishments.

IV. Multivariate regressions

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There is a potential self-selection bias inherent for establishments with certain business owners' characteristics such as non-minority, domestic, and male owners that may affect the likelihood of receiving funding from PE or VC. There are also some potential unobservable factors such as the amount of competing business proposals received by PE and VC funds, owners' initial capital, owners' family support, and so forth. In order to examine the impact of PE and VC financing on establishments growth, first, we examine the impact of business owners' demographics on the likelihood of a business establishment to receive PE or VC funding using the probit regression. Then, in the second stage, we examine the impact of PE and VC funding on the establishments' subsequent growth rates, measured by inflation adjusted annual sales and employment, using the differences-on-differences regression between the pre- and the post-financing periods. We also correct for a potential self-selection bias using the Heckman correction technique by including the inverse-Mills ratio obtained from the first stage probit regression into the second stage differences-on-differences regression (Heckman 1979; Heckman and Robb, 1985).

4.1. Hypothesis and structural models

Several existing studies have found that owners' demographics significantly influence the likelihood of securing external funding successfully. Becker, Blease, and Sohl (2007) find that women business owners receive significantly smaller funding from angel capital than male owners. Robb (2012) finds that women business owners face greater credit constraints due to lower credit scores than men. Fairlie and Robb (2009) show that women business owners are capital constrained due to less startup capital, less human capital, and less prior work experience. Carter and Allen (1997) find that women's efforts to obtain external capital are constrained by

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their lifestyle intentions. Bates and Bradford (2008) also demonstrate that ethnic minority business owners are also capital constrained. Robb (2012) reports that minority-owned businesses encounter higher rejection rates on their loan applications and pay higher interest rates than non-minority groups. Furthermore, she reports that minority business owners have lower initial capital that hinders them from raising external capital. Based on these prior studies, we hypothesize that owner characteristics, namely gender and ethnicity, significantly influence the likelihood of establishments to secure funding from PE or VC. Additionally, we also believe that business owners with foreign status face similar funding prospects as women and ethnic minorities. Therefore, we also include foreign status as one of the factors that influences the likelihood of securing PE or VC financing. Thus, our first hypothesis is stated as the following:

H1: The likelihood of a business establishment to receive PE or VC funding is dependent on the owners' demographics (i.e. minority, women, and foreign owners) of the corresponding establishment.

We control for gender of establishments' CEOs (WCEO) because establishments' leadership gender may also affect the likelihood of PE or VC financing. We control for changes in the Dun & Bradstreet's Paydex scores (CHGPAYDEX) and credit rating decreases (CHGDBR-) and increases ($CHGDBR_+$) as a measure of the ability to pay their short-term obligations and credit worthiness to obtain bank loans. Previous studies have indicated that PE and VC are able to select private businesses that exhibit higher growth prior to funding decisions (Gompers and Lerner, 1999b). Therefore, we control for the change in business establishments' net sales (CHGSALE) and change in employment (CHGEMP) during one year prior to PE or VC

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funding. We also control for the establishments' ages (FIRMAGE), business form (CORP), and whether the establishments have existing government contracts or not (GCONTRACT). Because we do not have a measure of business owners' wealth and local employment from the NETS database, we use the county level unemployment rate (UNEMP) from the Local Area Unemployment Statistics published by the Bureau of Labor Statistics as a proxy of business owners' wealth and employment in the county at which a business establishment is currently located. We include indicator variables for state, industry, and year. The structural models for the first stage regression are described as the following:

$$\begin{aligned} Probability\ (PE\ financing)_{it} &= \alpha_0 + \alpha_1\, MINORITY_{it} + \alpha_2\, WOWNER_{it} + \alpha_3\, FOREIGN_{it} + \alpha_4\, WCEO_{it} \\ &+ \alpha_5\, CHGPAYDEX_{it} + \alpha_6\, CHGDBR\text{-}_{it} + \alpha_7\, CHGDBR_{+it} + \alpha_8\, CHGSALE_{it-1} + \alpha_9\, CHGEMP_{it-1} \\ &+ \alpha_{10}\, FIRMAGE_{it} + \alpha_{11}\, CORP_{it} + \alpha_{12}\, GCONTRACT_{it} + \alpha_{12}\, UNEMP_{it} + \sum \beta_k\, States\ Dummies_{it} \\ &+ \sum \gamma_m\, Industries\ Dummies_{it} + \sum \delta_n\, Year\ Dummies_{it} + \varepsilon_{it} \qquad (1) \end{aligned}$$

$$\begin{aligned} Probability\ (VC\ financing)_{it} &= \alpha_0 + \alpha_1\, MINORITY_{it} + \alpha_2\, WOWNER_{it} + \alpha_3\, FOREIGN_{it} + \alpha_4\, WCEO_{it} \\ &+ \alpha_5\, CHGPAYDEX_{it} + \alpha_6\, CHGDBR\text{-}_{it} + \alpha_7\, CHGDBR_{+it} + \alpha_8\, CHGSALE_{it-1} + \alpha_9\, CHGEMP_{it-1} \\ &+ \alpha_{10}\, FIRMAGE_{it} + \alpha_{11}\, CORP_{it} + \alpha_{12}\, GCONTRACT_{it} + \alpha_{12}\, UNEMP_{it} + \sum \beta_k\, States\ Dummies_{it} \\ &+ \sum \gamma_m\, Industries\ Dummies_{it} + \sum \delta_n\, Year\ Dummies_{it} + \varepsilon_{it} \qquad (2) \end{aligned}$$

where ε_{it} is the probability regression error term. We estimate the first stage regression using the probit regression with heteroskedasticity correction and we estimate the standard errors from the establishment level clustering.

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Guo, Hotchkiss, and Song (2011) show that PE buyouts create gains in operating performance, in terms of profitability and net cash flows, during post buyout periods. Chemmanur, Krishnan, and Nandy (2011) find that VC-backed firms experience greater improvement in their total factor productivity during post-VC periods. Similarly, Puri and Zarutskie (2012) also find that VC-backed firms experience larger increases in their net sales and employment during post-VC period. Thus, on the second stage, we hypothesize that the establishment's subsequent growth rates, measured by annual sales and annual employment growth rates, are affected by the establishment's ability to secure funding from PE or VC after controlling for the endogeneity of the likelihood for PE or VC financing. Thus, our second hypothesis is stated as the following:

H2: PE or VC funding has positive impacts on business establishment sales and employment growth during the post financing period.

We measure the impact of PE or VC funding on establishment growth using differences-in-differences (Card, Katz, and Krueger, 1994; Bertrand, Duflo, and Mullainathan, 2004). We set up three dummy variables to indicate: (1) the establishments that received PE or VC financing (PE/VC FUNDED), (2) the establishments after the post-financing period for both those that received funding and their control group (POST PE/VC), (3) the establishment that received PE or VC financing during the post-funding period (PE/VC FUNDED x POST PE/VC). Our H2 hypothesis specifically tests whether the establishment that received PE or VC financing during

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the post funding period (PE/VC FUNDED x POST PE/VC) has significantly higher net sales and higher employment growth.[20]

In this second stage regression, we also include the one year lag of sales and employment growth. We include establishment leadership gender (WCEO) as a proxy for risk taking behavior. We include the change in Paydex score (CHGPAYDEX) and credit score decrease (DBR-) and increase (DBR_+) as measures of establishments' ability to secure funding from their creditors that may affect their growth. We also include firm age (FIRMAGE), business form (CORP), and whether establishments have existing government contracts or not (GCONTRACT). Bates and Bradford (2008) find that VCs that focus on minority business enterprises (MBEs) earn returns that are consistent with mainstream funds. This indicates that MBEs are not inferior compared to the non-MBEs even though they are capital constrained. Therefore, we do not include the owners' demographics on our second stage regression model. We include indicator variables for state, industry, and year. The structural models for the second stage regression for establishment annual sales growth (SALEGR) and employment growth (EMPGR) are described as the following:

$$SALEGR_{it} \text{ or } EMPGRW_{it} = \beta_0 + \beta_1 \, PEFUNDED_{it} + \beta_2 \, POSTPE_{it} + \beta_3 \, PEFUNDED_{it} \times POSTPE_{it} + \beta_4 \, LAGSALEGRW_{it-1} + \beta_5 \, WCEO_{it} + \beta_5 \, CHGPAYDEX_{it} + \beta_5 \, CHGDBR\text{-}_{it} + \beta_5 \, CHGDBR_{+it} + \beta_5 \, FIRMAGE_{it} + \beta_5 \, CORP_{it} + \beta_5 \, GCONTRACT_{it} + \lambda \, INVERSE\text{-}MILL_{it} + \sum \gamma_k \, States\ Dummies_{it} + \sum \delta_m \, Industries\ Dummies_{it} + \sum \theta_n \, Year\ Dummies_{it} + \varepsilon_{it} \quad (3)$$

[20] We also regress all the control variables on net sales and employment growth, obtain the residuals from 3 years before and 3 years after PE or VC funding, and run the regression of PE/VC FUNDED, POST PE/VC and PE/VC FUNDED x POST PE/VC on these residuals. The results are consistent with reported results.

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$$SALEGR_{it} \text{ or } EMPGRW_{it} = \beta_0 + \beta_1\, VCFUNDED_{it} + \beta_2\, POSTVC_{it} + \beta_3\, VCFUNDED_{it} \times POSTVC_{it} + \beta_4\, LAGSALEGRW_{it-1} + \beta_5\, WCEO_{it} + \beta_5\, CHGPAYDEX_{it} + \beta_5\, CHGDBR\text{-}_{it} + \beta_5\, CHGDBR_{+it} + \beta_5\, FIRMAGE_{it} + \beta_5\, CORP_{it} + \beta_5\, GCONTRACT_{it} + \lambda\, INVERSE\text{-}MILL_{it} + \sum \gamma_k\, States\ Dummies_{it} + \sum \delta_m\, Industries\ Dummies_{it} + \sum \theta_n\, Year\ Dummies_{it} + \varepsilon_{it} \quad (4)$$

where, λ is the slope of inverse-Mill's ratio and ε_{it} is the regression error term. We estimate the second stage regression using the ordinary least square (OLS) regression with a heteroskedasticity correction and we estimate the standard errors from both establishment and year clustering.

V. Regression results

5.1 First stage probit regression

Table 7 presents the probit regression results for the first stage regression to examine characteristics that influence business establishments' likelihood of receiving PE or VC funding.[21] The reported slope coefficients are stated as the marginal impact for each corresponding independent variable and the robust and establishment clustered z-ratios are presented in parenthesis under the slope coefficients.

[Insert Table 7 here]

[21] The NETS (IEGC) database contains the establishments' owners' demographics in the most recent year only. We verified that there is no change in establishment ownership for our sample and control group and also confirmed with the NETS data provider to ensure that there is no change in ownership. Because there is no change in ownership and we choose single establishments that never experience a sale or combination of assets, mergers or acquisitions, then our owners' demographics from the NETS represent the owners' demographics for the entire period of our study.

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The first two columns of Table 7 present the probit regression results for PE funding and the last 2 columns present the results for VC funding.[22] In the first column we include single entity establishments with multiple rounds of PE financing, and in the second column we only examine single entity establishments that receive PE financing once (single round). We focus our discussions on establishments with a single round of PE financing (second column) and find that owners who are considered as minority, female, and foreign are 21.7%, 2.6%, and 8.8% less likely to receive PE funding, respectively. These results are statistically and economically significant. Thus we find evidence to support our first hypothesis *H1*. This is also consistent with existing literature that finds ethnic minorities, women, and foreign business owners are facing capital constraints.

We do not find evidence that woman CEO status has a significant impact on the likelihood of obtaining PE financing. Additionally, we do not find strong evidence that the change in Paydex score affects the likelihood of PE financing. Establishments that experience a decrease in their Dun & Bradstreet credit ratings are 6% more likely to get PE financing while establishments with increases are 4% less likely to get PE financing. This indicates that PE financing is acting as a substitute for bank loans when business establishments are experiencing changes in their credit ratings. We do not find evidence that the change in net sales and the change in employment during one year prior to financing affect the likelihood of PE financing. This implies that private equity does not necessarily select their investments based on establishments' recent past growth differentials.

We find that older establishments demonstrate less likelihood of receiving PE financing. We believe that older establishments exhibit better reputations and transparency, and therefore

[22] The first stage probit regressions for both PE and VC funded are conducted using the original cross sectional data from NETS (IEGC). We conduct a robustness check by estimating the probit regressions in a panel data procedure (cross sectional and time series) and the results remain robust.

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have better access to less expensive capital such as bank loans. We find that establishments with corporation status are also less likely to receive PE financing. We find establishments with government contracts are 10% more likely to receive PE financing. Government contracts are likely to produce stable cash flows and provide a certification benefit. We find that establishments located in higher unemployment counties are 2% less likely to receive PE financing. We believe that our findings support the existing literature that owners' wealth, measured by local unemployment rate, has significant impacts on the likelihood of PE financing. There is evidence that Florida and New York states are less likely to receive PE financing.

We find similar evidence for the likelihood of VC financing. Again we focus our discussion on the single round of VC sample (fourth column) since it represents the cleanest comparison. We find that minority, women, and foreign business owners are 22.2%, 18.7%, and 17.9% less likely to receive VC financing, respectively. This supports our hypothesis *H1* and is also consistent with the literature. We also find that for establishments that experience a decline in their Dun & Bradstreet credit rating VC financing is 8.6% more likely while establishments with increases are 14% less likely to get VC financing. Thus VC financing and access to credit may serve as substitutes. We find that those establishments with government contracts are 15.3% more likely to receive VC financing. Thus, government contracts provide certification and stable cash flows that are attractive to VC. We find that establishments located in higher unemployment counties are 4% less likely to receive VC financing. We believe that owners' wealth, measured by local unemployment rates, also has a significant impact on the likelihood of VC financing. We find that establishments in California and Massachusetts are more likely to get VC financing. This implies that VC funding tends to agglomerate in certain states where new innovations are more likely to occur.

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[Insert Table 8 here]

5.2 Second stage growth rates regression

In the second stage regressions, we examine the impacts of receiving PE or VC financing on establishments' annual net sales and employment growth. We examine during three years prior to and three years after financing using the differences-in-differences method. First, we examine whether establishments that received PE or VC financing once in any year during 1995 to 2009 have significantly higher growth than their control groups in both prior to and after financing events. This is represented by the PE/VC FUNDED variable.

Table 8 shows that establishments with PE funding have over 2% (2.63% to 2.9%) higher sales growth than their control group. Second, we test whether establishments with PE and their control group are experiencing higher growth during the year PE financing events occurred and thereafter. This is represented by POSTPE variable.[23] We do not find that establishments' growth is significantly higher during post-PE periods compared to pre-PE periods. More importantly, we find that establishments with PE financing are experiencing an additional 2% (2.08% to 2.48%) higher sales growth than their control group during the post-financing period (PEFUNDED x POSTPE), thus supports our second hypothesis *H2.*[24]

We do not find that the lag of sales growth is significantly related to current period sales growth. This implies that there is no serial correlation between past growth and current growth during three years prior to and three years after financing. We find that women CEOs tend to

[23] POSTPE is a dummy variable equal to one if an establishment receives PE funding during the year of funding and thereafter or zero otherwise.
[24] PEFUNDED x POSTPE is a dummy interaction between PEFUNDED and POSTPE. It captures the structural difference between establishments that received PE funding relative to their control group during the year of PE funding and thereafter.

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have 2% lower sales growth. This is consistent with existing literature that women executives tend to be more conservative and less overconfident than male executives (Huang and Kisgen, 2012). We also find that older establishments tend to have lower sales growth rates. This implies that older firms have less opportunity to grow since they are reaching their mature stage. We do not find any evidence that government contracts alone contributes to business establishments' growth.

We find similar results for the impact of VC financing on establishment growth during three years after relative to three years prior to financing events. However, the magnitude of slope coefficients of VC funding on establishments' sales growth is significantly larger than PE funding. We find VC-funded establishments (VCFUNDED) generally have higher sales growth than their control group.[25] More importantly, we find evidence that these establishments with VC funding are experiencing over 22% additional sales growth after they received VC financing (POSTVC).[26]

[Insert Table 9 here]

Next, we examine the impact of PE and VC financing on establishments' employment growth during three years after relative to three years prior to financing. Table 9 presents the results of this analysis. We find that PE financing has positive and significant impact on establishment employment growth during the post period relative to pre-financing period. On average, the establishments with PE financing are experiencing 3% (2.94% to 3.03%) increase in

[25] VCFUNDED is a dummy variable equal to one if an establishment receives VC funding once in any year during 1995 to 2009 or zero otherwise.
[26] POSTVC is a dummy variable equal to one if an establishment receives VC funding during the year of funding and thereafter or zero otherwise.

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employment growth during three years after relative to three years prior to financing events. Again, this evidence supports our hypothesis *H2*. We find that older establishments have lower employment growth since older firms have less opportunity to grow as they reach their mature stage.

We find establishments with VC financing (VC FUNDED) generally have higher employment growth relative to their control group. Moreover, we still find strong evidence that establishments with VC financing are still experiencing over 32% further employment growth during three years after their financing events (VCFUNDED x POSTVC).[27] This supports our hypothesis *H2* that VC financing has significant and positive impact on establishments' employment growth during post-financing periods.

We find evidence that the one-year lag of employment growth significantly affects current employment growth for VC financing. This implies that the employment growth is serially correlated from one period to the next. Again, we do not find any evidence that government contracts alone contributes to business establishments' growth for the VC sample. Overall, we find that both PE and VC financing have significant and positive impact on establishments' net sales and employment growth relative to their control groups. Furthermore, the magnitudes of VC financing on establishment growth are larger than the magnitudes of PE financing.

[Insert Table 10 here]

[27] VCFUNDED x POSTVC is a dummy interaction between VCFUNDED and POSTVC. It captures the structural difference between establishments that received VC funding relative to their control group during the year of VC funding and thereafter.

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Finally, we examine the long-lasting impact of PE or VC financing on establishments net sales and employment growth during the contemporaneous period until 3 years after the financing events. We use the growth during two years prior to financing events as a reference point to examine the impact of PE or VC financing on establishments' net sales and employment growth during the contemporaneous period until three years after the financing events. We include one-year prior to financing event in our sample to represent the pre-financing period. Panel A of Table 10 shows that the impact of PE financing on both net sales and employment growth during the contemporaneous period of financing is insignificant.[28] This indicates that it takes some time for PE to execute their strategies to enhance establishments' growth since PE financing involves changes in ownership and management. Once changes take place, the impact on establishment growth is significantly large in the first year after financing and it persists for three consecutive years.

Panel B of Table 10 presents the impact of VC financing on business establishments' sales and employment growth during the contemporaneous year and three years after financing. We find that the impact of VC financing on both net sales and employment growth is immediate. This implies that venture capital is able to capitalize the business establishments' growth immediately after they deploy their capital into the establishments. However, the impact of VC financing on establishments' growth only lasts for two consecutive years after the financing events. Thus, the impact of VC financing is shorter than the impact of PE financing. Overall, we

[28] Sales and employment growth rates in year 0 are measured as the percentage change of annual sales and employment from the beginning of the year to the end of the year when an establishment just receives PE or VC financing (year 0). The beginning of the year sales and employment in period 0 are basically the end of year sales and employment in one year prior to receiving PE/VC financing (period -1). We use the same method to calculate sales growth and employment growth rates for years 1, 2, and 3. Then we calculate the difference in sales and employment growth rates in years 1, 2, and 3 relative to sales and employment growth rates two years prior to PE or VC financing year (year -2) as our measures of SALEGR1, EMPGR1, SALEGR2, EMPGR2, SALEGR3, and EMPGR3 on Table 10.

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find evidence to support our hypothesis *H2* that both PE and VC financing significantly increase the establishment growth during post financing periods.

VI. Additional robustness tests

We conduct additional robustness tests for our results by examining the impact of PE and VC financing on establishment growth using the propensity matching method (Rosenbaum and Rubin, 1983). We focus on the single round of PE or VC-financed establishments by deleting the establishments that received multiple rounds of PE or VC financing to clearly examine the impact of PC or VC financing on establishments' organic growth.

The propensity scoring method has been used in finance and accounting literature (Tucker, 2010; Lennox, Francis, and Wang, 2012) and is appropriate for our analysis since we only observe establishments that successfully obtained PE or VC financing. The goal of propensity scoring is to construct probabilities of successfully obtaining PE or VC financing for establishments that did not receive PE or VC financing. First, we conduct the probit regression for the entire IEGC sample to estimate the probability of each establishment of receiving PE or VC financing. This probit regression is similar to the first stage regression that is reported in Table 7 for the entire IEGC (NETS). Then, we construct matched-pair establishments that receive PE financing with establishments that never received PE or VC financing based on the closest estimated probabilities (propensity scores) of receiving PE financing in each year. We construct similar match-pair establishments based on the propensity scores for receiving VC financing and name it as the propensity scoring VC sample.

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Our untabulated results from the propensity matching samples are similar to our matched-pair results. PE financing does not have an immediate impact on establishments' growth rates. However, it significantly and positively affects their net sales and employment growth rates for three consecutive years after financing. We also find similar results that VC financing immediately and positively increases establishments' growth rates. We find that the impact of VC financing on net sales and employment growth rates remains positive and significant during two consecutive years after financing. Overall, our results remain robust using the samples from the propensity score method.

The NETS (IEGC) database is presented as cross sectional data. We reshape the original NETS data into a panel (cross sectional and time series) data and conduct robustness checks on our results presented in Tables 7, 8, 9, and 10. Our unreported regression results using the panel data are consistent with the results presented in our Tables 7, 8, 9, and 10. Therefore, we believe that our results are robust.

VII. Conclusions

Academics, business owners, and policy makers have put a significant amount of attention on the topic of impact of private equity (PE) and venture capital (VC) financing on firms' revenue and employment growth. While most of the existing studies focus on the impact of leveraged buyouts (LBOs) by private equity firms on job creation and destructions, the literature on the impacts of PE and VC financing on subsequent growth for small to mid-sized single entity establishments is still underdeveloped. Furthermore, examining the clean impact of PE or VC financing on firms' organic growth is challenging in that data are often aggregated across business operating units or influenced by corporate combinations. Our study directly

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compares the impact of PE and VC financing on single entity establishments' subsequent net sales and employment growth rates for small and mid-sized establishments that are free from acquisitions, sale of business divisions, and combinations. Our study also focuses on establishments that receive a singular round of PE or VC financing. The first contribution of our study is made by examining the pure impact of a single round of PE or VC financing on single entity business establishments sales and employment organic growth rates. Our second contribution is yielded by comparing the impact of PE financing with VC financing during the post financing periods. Most of the existing studies only examine either PE financing or VC financing but not both.

Using the Institute for Exceptional Growth Companies (IEGC or NETS) database, this study is able to cleanly examine the impact of a single round of PE or VC financing on business establishments' net sales and employment organic growth rates. Because we focus on single entity business establishments, the sizes of these establishments are significantly smaller than the sizes of firms that are examined in the previous studies. Using NETS, D&B, and Pitchbook data during 1995 to 2009, we construct matched-pair samples for establishments that received funding from PE or VC with those who never received financing from both PE and VC (control groups). We also carefully select single entity business establishments for the control group such that we can precisely compare the establishments with PE or VC financing with their corresponding control groups.

Our results indicate that minorities, women, and foreign owned establishments are significantly less likely to receive private equity (PE) funding. These groups are even less likely to receive funding from venture capital (VC). Policy makers put forth significant efforts to foster equal opportunity for both minorities and women to have equal access to capital (Hinson, 2010).

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Our paper provides evidence that the likelihood of successfully obtaining funding from PE and VC for minorities, women, and foreign owned establishments is still lower than the white-male group.

After controlling for endogeneity and self-selection biases for probabilities of obtaining capital from PE or VC, we find that PE or VC financing significantly and positively affect the establishments' net sales and employment growth rates. Furthermore, we find that immediate impact of PE financing on establishments' growth is insignificant. This is likely the result of a potentially considerable gap in time between implementing strategic changes and realizing the results. We find that PE financing increases establishments' growth rates for three years after their PE financing event, however. In contrast, we find that the impact of VC financing on establishments' growth is immediate and larger than PE financing. However, the impact of VC financing on establishments growth only lasts for one additional year after the financing year. Thus it is shorter than the impact of PE financing.

Our findings are relevant for policymakers, capital providers, and business owners. First, these magnitudes of demographics on the likelihood of receiving PE and VC funding indicate that minority, women, and foreign-owned establishments are still facing significant challenges to obtain PE and VC funding to grow their businesses. Second, both PE and VC financing sources are very important for these establishments to grow their businesses and to create employment opportunities. These financing events therefore have a positive impact on economic growth.

We also find that there are significant benefits to having government contracts in place when seeking PE or VC financing. The probabilities of successfully raising capital when government contracts are in place are sizable and significant. However, these contracts appear to

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lack any significant influence on sales and employment growth after the financing event occurs. Further work needs to be done in this area to understand why.

The NETS database has limited information regarding business owners' wealth, education, and experience which are important factors that influence the demand for PE and VC financing. We augment the NETS data with the county level unemployment rate at which these business establishments reside as a proxy for owners' wealth and education. We also recognize that the NETS database may overestimate the employment numbers and underestimate the net sales receipts. However, because both the establishments with PE and VC funding and their control establishments are drawn from the same database, we believe that both the funded establishments and their control groups exhibit the same biases. We conduct robustness checks using the propensity matching and reshaping the NETS data into a panel data and we find that our results remain robust.

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Appendix A

Comparison between NETS database and U.S. Census data

Panel A. Aggregate employment from Business Dynamics Statistics and NETS

	Business Dynamics Statistics		National Establishment Time-Series (NETS)	
Year	# Establishments	# Employees	# Establishments	# Employees
1995	5,839,774	98,519,864	12,179,705	144,895,620
1996	5,933,926	100,380,503	12,385,686	145,260,373
1997	6,043,242	103,203,936	13,090,106	149,713,844
1998	6,108,927	106,268,299	13,573,482	154,267,365
1999	6,174,381	109,060,036	13,699,191	156,118,765
2000	6,219,280	112,624,575	13,658,564	160,220,069
2001	6,348,830	114,349,926	14,267,011	167,211,842
2002	6,399,351	112,123,655	16,071,016	173,173,276
2003	6,460,594	112,720,028	17,192,608	169,037,299
2004	6,542,356	114,002,472	17,365,470	165,715,082
2005	6,679,753	115,520,906	18,054,411	164,486,072
2006	6,781,915	118,921,117	19,710,914	167,984,002
2007	6,888,393	119,913,218	20,550,939	169,757,863
2008	6,862,476	120,083,046	22,325,361	169,478,700
2009	6,678,469	113,900,772	22,617,871	171,922,743
2010	6,619,139	111,175,010	22,015,210	161,957,103

Note: Business Dynamics Statistics is updated every mid-March while NETS is updated every January. The Business Dynamics Statistics is downloaded directly from: http://www2.census.gov/ces/bds/estab/bds_e_all_release.xls.

Panel B. Aggregate net sales receipts from Statistics of U.S. Businesses and NETS

	Statistics of U.S. Businesses		National Establishment Time-Series (NETS)	
Year	# Establishments	Receipts (in $1000)	# Establishments	Receipts (in $1000)
1997	6,894,869	18,242,632,687	12,931,953	15,646,277,989
2002	7,200,770	22,062,528,196	15,849,268	19,601,571,421
2007	7,705,018	29,746,741,904	20,311,659	19,433,716,504

Note: Statistics of U.S. Businesses is updated every mid-March while NETS is updated every January. The Statistics of U.S. Businesses is downloaded directly from:
http://www2.census.gov/econ/susb/data/1997/us_4digitsic_receipt_1997.xls,
http://www2.census.gov/econ/susb/data/2002/us_6digitnaics_receipt_2002.xls, and
http://www2.census.gov/econ/susb/data/2007/us_6digitnaics_receipt_2007.xls.

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Appendix B

Examples of the Pitchbook financing database

The Pitchbook financing data indicates whether a particular establishment receives private equity (PE) or venture capital (VC) financing (without dollar amount of PE or VC investments) and its type of ownership. Yeid is the establishment unique identifier from the NETS database. Financing95 implies whether an establishment receives Private Equity (PE) Backed or Venture Capital (VC) Backed financing during year 1995, Financing96 implies whether an establishment receives PE or VC financing during year 1996, etc. Ownership02 implies types of ownership for each establishment during year 2002. Ownership03 implies types of ownership for each establishment during year 2003, etc. The Pitchbook financing and ownership data is available from 1995 to 2009.

Yeid	Financing95	Financing96	Financing98	Financing99
1362			Private Equity Backed	
2846		Private Equity Backed		
3502				Private Equity Backed
15757			Private Equity Backed	
68629				VC Backed
75231	Private Equity Backed			
80424			VC Backed	

Yeid	Ownership02	Ownership03	Ownership04	Ownership05	Ownership06	Ownership07
10000332				Privately Held		
10001797						Privately Held
10001826						Publicly Held
10002734		Privately Held	Privately Held		Acquired/Merged	
10003352						
10012789				Privately Held	Publicly Held	
10014872	Privately Held					

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Appendix C

Variables Definitions

Variables	NETS Field Name	Definitions
PEFUNDED	Financing (Pitchbook)	An indicator variable that takes on a value = 1 if the establishment receives funding from Private Equity during 1995 to 2009
VCFUNDED	Financing (Pitchbook)	An indicator variable that takes on a value = 1 if the establishment receives funding from Venture Capital during 1995 to 2009
EMPGR#	Emp	Percentage change of employment in current year upon receiving funding relative to previous year (in decimal)
SALEGR#	Sales	Percentage change of Sales in current year upon receiving funding relative to previous year (in decimal)
MINORITY	Minority	Minority Owned Indicator-Last (Y = Minority or non-Caucasian Owned, N = Non-Minority or Caucasian Owned)
FOREIGN	ForeignOwn	Foreign Owned-Last (Y = Yes, Space = No)
WCEO	GenderCEO	An indicator variable that takes on a value = 1 if the CEO is a woman or 0 otherwise
WOWNER	WomanOwned	Controlling interest in establishment held by woman-Last (Y = Yes, N = No)
CHGPAYDEX	PayDexMax	Change in D&B Maximum PayDex score. PayDex score 80 indicates that, on average, the business pays its bills in a "Prompt" manner.
CHGDNB-	D&Brating	Change in the first digit of Duns & Bradstreet credit rating toward a worse credit rating (i.e. 3A is worse than 4A rating)
CHGDNB+	D&Brating	Change in first digit of Duns & Bradstreet credit rating toward a better credit rating (i.e. 5A is better than 4A rating)
CHGSALE	Sales	Change in inflation adjusted annual net sales ($). Inflation adjusted annual net sales based on the CPI index (ftp://ftp.bls.gov/pub/special.requests/cpi/cpiai.txt)
CHGEMP	Emp	Change in number of employees
FIRMAGE	Age	Number of years since the establishment was founded
CORP	LegalStat	Legal Status-Last (G = Proprietorship, H = Partnership, I = Corporation, J = Non-Profit, Blank = NA). CORP is equal to one if an establishment is a Corporation.
GCONTRACT	GovtContra	Government Contracts/Grants Indicator-Last (Y=Yes, N= No)
UNEMP	-	County level unemployment rate (%) from the Bureau of Labor Statistics (source: http://www.bls.gov/lau/).
CA	State	An indicator variable that takes on a value = 1 if the state is California (CA) or 0 otherwise
FL	State	An indicator variable that takes on a value = 1 if the state is Florida (FL) or 0 otherwise
MA	State	An indicator variable that takes on a value = 1 if the state is Massachusetts (MA) or 0 otherwise
NY	State	An indicator variable that takes on a value = 1 if the state is New York (NY) or 0 otherwise
TX	State	An indicator variable that takes on a value = 1 if the state is Texas (TX) or 0 otherwise

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Figure 1

Establishments Inflation Adjusted Sales (Revenue) for PE and VC-Backed and their Matching Groups

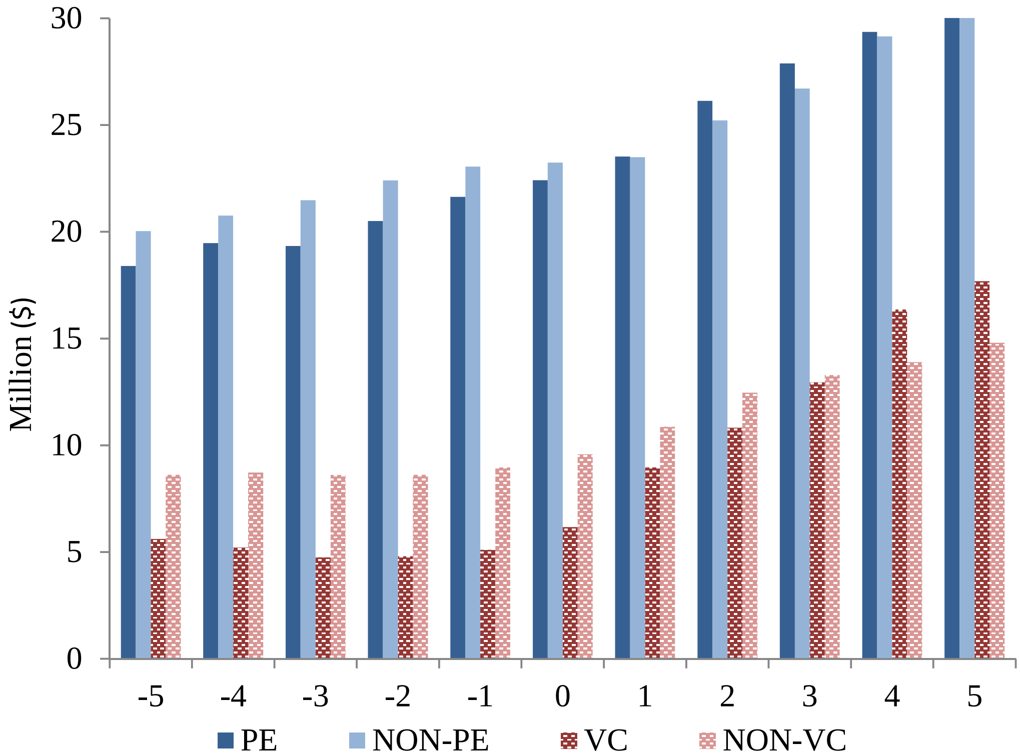

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Figure 2

Establishments Employment for PE and VCBacked and their Matching Groups

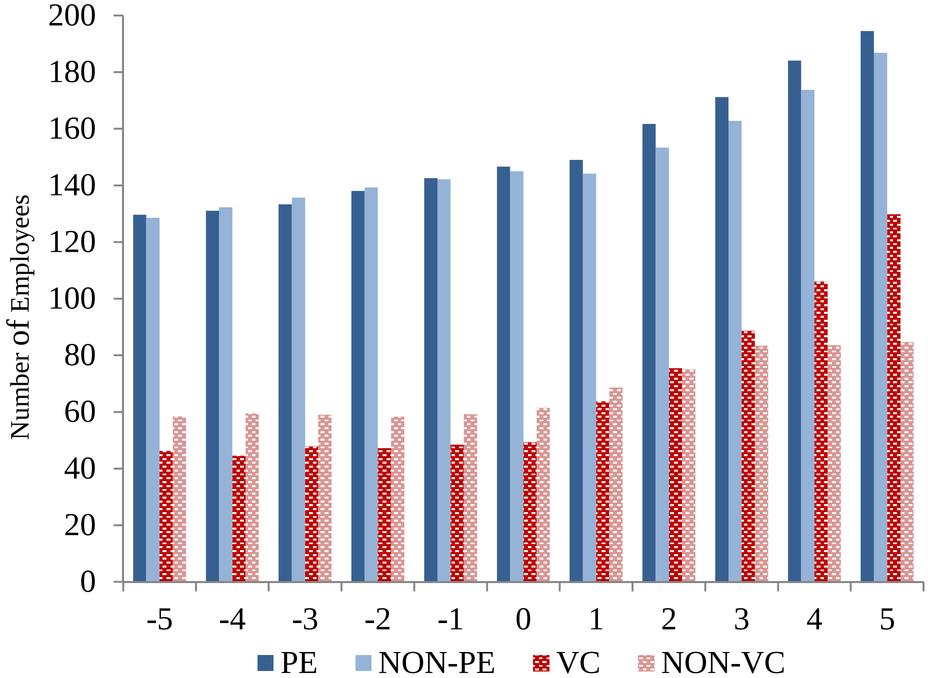

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Table 1

Data distribution and sample formation

This table presents sample selection processes from the original Pitchbook and IEGC (NETS) merged (POF data) to our final samples.

A. Financing	**Observations**	**Percentage**
Received PE Funding	16,802	62.6%
Received VC Funding	7,555	28.2%
Others*	2,481	9.2%
B. Ownership	**Observations**	**Percentage**
Privately Held	15,508	57.8%
Acquired/Merged	6,232	23.2%
Publicly Held	1,149	4.3%
Others**	3,949	14.7%
Total observations	26,838	100%
Number of establishments	16,482	
C. Sample formation	**PE Sample**	**VC Sample**
Initial data	16,802	7,555
Match pair results	13,538	6,800
Missing values	5,445	3,666
Sample prior to 1% truncation	8,093	3,134
Final match-pair sample with multiple establishments	4,138	811
Final match-pair sample with single establishment	3,874	2,291
Final match-pair with single round financing	3,074	756
D. Rounds of financing	**PE Sample**	**VC Sample**
One round	5,521	979
Two rounds	1,530	971
Three rounds	605	628
Four rounds	214	334
Five rounds	72	134
More than five rounds	71	57

*Others in financing imply acquired by other firms or in the process of going public. ** Others in ownership imply the establishments cease to exist.

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Table 2

Sample distribution across Fama-French 48 industries

	PE Match-Pair Sample		VC Match-Pair Sample		IEGC (NETS) Sample	
Industries	Obs	Pct	Obs	Pct	Obs	Pct
Agriculture	22	0.57%	1	0.04%	1,542,504	3.49%
Food	66	1.70%	0	0.00%	49,000	0.11%
Soda	18	0.46%	1	0.04%	15,268	0.03%
Beer	5	0.13%	0	0.00%	9,236	0.02%
Smoke	1	0.03%	0	0.00%	1,422	0.00%
Toys	35	0.90%	7	0.31%	74,526	0.17%
Fun/Entertainment	41	1.06%	11	0.48%	944,726	2.14%
Books	84	2.17%	13	0.57%	188,488	0.43%
Household	56	1.45%	12	0.52%	123,324	0.28%
Clothes	23	0.59%	0	0.00%	58,336	0.13%
Health	125	3.23%	32	1.40%	2,026,970	4.58%
Med. Equipment	106	2.74%	105	4.58%	29,624	0.07%
Drugs	49	1.26%	64	2.79%	14,518	0.03%
Chemical	42	1.08%	11	0.48%	41,922	0.09%
Rubber	83	2.14%	5	0.22%	44,439	0.10%
Textiles	28	0.72%	2	0.09%	66,707	0.15%
Build. Material	106	2.74%	7	0.31%	287,098	0.65%
Construction	103	2.66%	11	0.48%	3,979,342	8.99%
Steel	40	1.03%	4	0.17%	29,113	0.07%
Fab. Prod	68	1.76%	3	0.13%	50,751	0.11%
Machine	169	4.36%	25	1.09%	198,394	0.45%
Elec. Equipment	52	1.34%	33	1.44%	40,227	0.09%
Autos	72	1.86%	1	0.04%	44,396	0.10%
Aero	41	1.06%	1	0.04%	10,382	0.02%
Ships	28	0.72%	0	0.00%	3,642	0.01%
Guns	8	0.21%	5	0.22%	3,899	0.01%
Gold	0	0.00%	0	0.00%	1,689	0.00%
Mines	9	0.23%	0	0.00%	16,040	0.04%
Coal	4	0.10%	0	0.00%	7,008	0.02%
Oil	73	1.88%	3	0.13%	90,910	0.21%
Utility	53	1.37%	8	0.35%	141,177	0.32%
Telecom	126	3.25%	128	5.59%	324,494	0.73%
Personal Service	93	2.40%	31	1.35%	5,311,252	12.01%
Business Service	756	19.51%	1,149	50.15%	11,238,461	25.40%
Computer	56	1.45%	115	5.02%	90,948	0.21%
Chips	145	3.74%	164	7.16%	47,264	0.11%
Lab. Equipment	80	2.07%	47	2.05%	28,263	0.06%
Paper	40	1.03%	0	0.00%	46,584	0.11%
Boxes	42	1.08%	0	0.00%	19,826	0.04%
Transport	11	0.28%	8	0.35%	1,263,342	2.86%
Wholesale	423	10.92%	151	6.59%	6,024,325	13.62%
Retail	163	4.21%	72	3.14%	2,305,084	5.21%
Meals	99	2.56%	3	0.13%	1,375,659	3.11%
Banks	68	1.76%	8	0.35%	533,852	1.21%
Insurance	50	1.29%	9	0.39%	679,215	1.54%
Real Estate	34	0.88%	3	0.13%	1,880,311	4.25%
Security Trading	30	0.77%	13	0.57%	816599	1.85%
Others	48	1.24%	25	1.09%	2,120,947	4.79%
TOTAL	3,874	100%	2,291	100%	44,241,504	100%

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Table 3

Sample distribution across states

State	PE Match-Pair Sample		VC Match-Pair Sample		IEGC (NETS) Sample	
	Obs	Pct	Obs	Pct	Obs	Pct
AK	6	0.15%	0	0.00%	102,369	0.23%
AL	46	1.19%	3	0.13%	586,615	1.33%
AR	14	0.36%	1	0.04%	387,834	0.88%
AZ	88	2.27%	18	0.79%	797,076	1.80%
CA	498	12.85%	988	43.13%	5,446,061	12.31%
CO	96	2.48%	69	3.01%	888,817	2.01%
CT	67	1.73%	28	1.22%	580,122	1.31%
DC	10	0.26%	5	0.22%	122,076	0.28%
DE	7	0.18%	1	0.04%	114,652	0.26%
FL	218	5.63%	51	2.23%	3,748,447	8.47%
GA	121	3.12%	48	2.10%	1,474,127	3.33%
HI	6	0.15%	0	0.00%	144,420	0.33%
IA	15	0.39%	0	0.00%	514,544	1.16%
ID	16	0.41%	8	0.35%	262,907	0.59%
IL	185	4.78%	37	1.62%	1,573,483	3.56%
IN	73	1.88%	7	0.31%	771,531	1.74%
KS	28	0.72%	8	0.35%	428,538	0.97%
KY	33	0.85%	3	0.13%	541,637	1.22%
LA	28	0.72%	4	0.17%	660,716	1.49%
MA	159	4.10%	287	12.53%	919,728	2.08%
MD	62	1.60%	34	1.48%	843,879	1.91%
ME	17	0.44%	1	0.04%	197,229	0.45%
MI	81	2.09%	7	0.31%	1,355,604	3.06%
MN	97	2.50%	27	1.18%	850,169	1.92%
MO	70	1.81%	7	0.31%	784,270	1.77%
MS	23	0.59%	4	0.17%	444,808	1.01%
MT	17	0.44%	1	0.04%	171,942	0.39%
NC	82	2.12%	57	2.49%	1,184,547	2.68%
ND	15	0.39%	0	0.00%	123,605	0.28%
NE	25	0.65%	2	0.09%	275,494	0.62%
NH	35	0.90%	8	0.35%	225,248	0.51%
NJ	139	3.59%	46	2.01%	1,192,497	2.70%
NM	12	0.31%	6	0.26%	248,623	0.56%
NV	35	0.90%	6	0.26%	346,506	0.78%
NY	290	7.49%	116	5.06%	2,747,781	6.21%
OH	120	3.10%	31	1.35%	1,392,733	3.15%
OK	40	1.03%	0	0.00%	497,207	1.12%
OR	53	1.37%	15	0.65%	632,558	1.43%
PA	122	3.15%	60	2.62%	1,652,734	3.74%
PR	5	0.13%	0	0.00%	78,656	0.18%
RI	10	0.26%	2	0.09%	134,535	0.30%
SC	47	1.21%	1	0.04%	530,805	1.20%
SD	10	0.26%	1	0.04%	135,338	0.31%
TN	61	1.57%	13	0.57%	836,547	1.89%
TX	356	9.19%	112	4.89%	3,722,027	8.41%
UT	64	1.65%	26	1.13%	434,731	0.98%
VA	86	2.22%	42	1.83%	1,044,544	2.36%
VI	0	0.00%	0	0.00%	4,916	0.01%
VT	16	0.41%	3	0.13%	109,283	0.25%
WA	69	1.78%	89	3.88%	974,621	2.20%
WI	86	2.22%	8	0.35%	714,349	1.61%
WV	8	0.21%	0	0.00%	188,281	0.43%
WY	7	0.18%	0	0.00%	99,737	0.23%
Total	3,874	100%	2,291	100%	44,241,504	100%

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Table 4
Correlation coefficients
Panel A. PE Match-Pair Sample

PEFUNDED takes on a value = 1 if the establishment receives funding from Private Equity (PE). SALEGR0 is the annual inflation adjusted sales growth during the period at which the establishment received PE financing. EMPGR0 is annual employment growth during the period at which the establishment received PE financing. MINORITY is equal to 1 if the establishment is owned by an ethnic minority. FOREIGN is equal to 1 if the owner of establishment has foreign status. WOWNER is equal to 1 if the establishment is owned by a woman. WCEO is equal to 1 if the establishment CEO is a woman. CHGPAYDEX indicates the annual change of maximum PayDex score CHGDBR- is the change in Duns & Bradstreet credit rating toward a worse credit rating. CHGDBR+ is the change in Duns & Bradstreet credit rating toward a better credit rating. CHGSALE is the annual change in inflation adjusted sales of an establishment during one year prior to financing. CHGEMP is the annual change in employment of an establishment during one year prior to financing. UNEMP is a county level unemployment rate at which the establishment resides. CORP is equal to 1 if the establishment is a corporation. FIRMAGE is the number of years since the establishment is founded. GCONTRACT is equal to one if the establishment has a government contract. CA, FL, NY, TX are state dummy variables to represent California, Florida, New York and Texas at which represent the top four states with the highest percentage of establishment receiving PE financing. * indicates statistically significant at 1% level.

No	Variables	1	2	3	4	5	6	7	8	9	10	11	12	13
1	PEFUNDED	1												
2	SALEGR0	0.0178*	1											
3	EMPGR0	0.0223*	0.7847*	1										
4	MINORITY	-0.0786*	-0.0067	-0.0082	1									
5	FOREIGN	-0.0556*	0.0004	0.004	-0.0523*	1								
6	WOWNER	-0.0644*	-0.0039	-0.0063	0.1927*	-0.0481*	1							
7	WCEO	-0.0144	-0.0028	-0.0026	-0.0101	0.0015	0.0833*	1						
8	CHGPAYDEX	0.0288*	-0.0036	-0.0024	-0.0025	0.0062	-0.0004	-0.0101	1					
9	CHGDBR-	0.0203*	-0.0004	0.0017	0.0178	0.0072	0.0219*	0.0078	-0.0430*	1				
10	CHGDNB+	-0.0319*	0.0034	0.0019	0.0008	-0.0113	-0.0004	0.0054	-0.0580*	0.1594*	1			
11	CHGSALE	0.0128	-0.0156	-0.0118	-0.0099	-0.0046	-0.0022	0.0038	-0.0005	0.0129	-0.0013	1		
12	CHGEMP	0.0075	-0.0117	-0.0165	-0.0026	0.013	-0.0027	0.011	-0.0074	0.0157	-0.0012	0.6900*	1	
13	UNEMP	-0.0417*	-0.0093	-0.0091	0.0194	-0.0210*	0.008	-0.0251*	-0.0203*	0.1351*	0.0841*	-0.0055	0.005	1
14	CORP	-0.0656*	-0.017	-0.0228*	0.0177	0.0555*	0.0211*	-0.0057	0.0171	0.0043	-0.0049	-0.0119	-0.0017	-0.0504*
15	FIRMAGE	-0.1049*	-0.0128	-0.0206*	-0.0546*	-0.0014	-0.0299*	-0.0236*	0.02	0.0148	0.0166	-0.0214*	-0.0142	-0.0098
16	GCONTRACT	0.0701*	0.0212*	0.019	0.0315*	0.0313*	-0.002	-0.0129	0.0047	0.0177	-0.0352*	-0.0032	-0.0052	-0.0744*
17	CA	-0.0108	0.0067	-0.0076	0.0321*	0.0086	0.0274*	0.0056	-0.0077	0.0072	-0.0046	0.0006	0.0029	0.1360*
18	FL	-0.0042	-0.0045	-0.0038	0.0192	-0.0240*	0.0053	0.0089	0.0039	-0.0019	0.0079	-0.0034	0.0032	0.0326*
19	NY	-0.0031	-0.0057	-0.0029	-0.0038	0.0114	-0.0125	0.0099	-0.0152	0.0014	0.0013	-0.0069	-0.0042	-0.015
20	TX	0.0347*	0.0013	0.0006	0.017	-0.0165	-0.0074	0.002	0.0087	0.001	-0.0012	0.0210*	0.0145	-0.0317*

No	Variables		15	16	17	18	19
14	CORP	1					
15	FIRMAGE	0.1260*	1				
16	GCONTRACT	0.0671*	0.1137*	1			
17	CA	0.0179	-0.0726*	0.0103	1		
18	FL	-0.0089	-0.0601*	-0.0322*	-0.0928*	1	
19	NY	0.0036	0.0184	-0.0220*	-0.1049*	-0.0656*	1
20	TX	-0.0438*	-0.0635*	-0.0327*	-0.1170*	-0.0731*	-0.0826*

Panel B. VC Match-Pair Sample

VCFUNDED takes on a value = 1 if the establishment receives funding from Venture Capital (VC). SALEGR0 is the annual inflation adjusted sales growth during the period at which the establishment received VC financing. EMPGR0 is annual employment growth during the period at which the establishment received VC financing. MINORITY is equal to 1 if the establishment is owned by an ethnic minority. FOREIGN is equal to 1 if the owner of establishment has foreign status. WOWNER is equal to 1 if the establishment is owned by a woman. WCEO is equal to 1 if the establishment CEO is a woman. CHGPAYDEX indicates the annual change of maximum PayDex score. CHGDBR- is the change in Duns & Bradstreet credit rating toward a worse credit rating. CHGDBR+ is the change in Duns & Bradstreet credit rating toward a better credit rating. CHGSALE is the annual change in inflation adjusted sales of an establishment during one year prior to financing. CHGEMP is the annual change in employment of an establishment during one year prior to financing. UNEMP is a county level unemployment rate at which the establishment resides. CORP is equal to 1 if the establishment is a corporation. FIRMAGE is the number of years since the establishment is founded. GCONTRACT is equal to one if the establishment has a government contract. CA, MA, NY, TX are state dummy variables to represent California, Massachusetts, New York and Texas at which represent the top four states with the highest percentage of establishment receiving VC financing. * indicates statistically significant at 1% level.

No	Variables	1	2	3	4	5	6	7	8	9	10	11	12	13
1	VCFUNDED	1												
2	SALEGR0	0.0206*	1											
3	EMPGR0	0.0359*	0.0899*	1										
4	MINORITY	-0.1367*	-0.0047	-0.0074	1									
5	FOREIGN	-0.0808*	-0.0017	-0.0028	-0.0443*	1								
6	WOWNER	-0.1279*	-0.0055	-0.007	0.1931*	-0.0315	1							
7	WCEO	-0.0268	-0.0015	-0.0053	0.0038	0.0284	0.0904*	1						
8	CHGPAYDEX	-0.0132	-0.0163	-0.0053	-0.0195	-0.0013	-0.0274	-0.0062	1					
9	CHGDBR-	0.0167	-0.0063	-0.0126	0.018	0.0162	0.0048	0.0025	-0.0268	1				
10	CHGDNB+	-0.0975*	0.0026	-0.0321	0.0163	-0.0157	-0.0051	0.0167	-0.0404*	0.1748*	1			
11	CHGSALES	0.0368*	-0.0027	-0.0065	0.0301	-0.0351*	-0.01	-0.0004	0.0085	-0.0078	-0.0033	1		
12	CHGEMP	0.0830*	-0.0033	-0.0169	-0.001	-0.0199	-0.0155	0.0045	0.0164	0.01	-0.0034	0.3297*	1	
13	UNEMP	-0.0377*	0.0192	-0.0102	0.0245	-0.0357*	0.0065	-0.0366*	-0.019	0.1797*	0.1257*	-0.0196	0.0016	1
14	CORP	0.2127*	0.0054	-0.0012	0.008	0.0484*	0.014	-0.0011	0.004	0.0416*	-0.0570*	0.0074	0.0348*	-0.1021*
15	FIRMAGE	-0.4281*	-0.0093	-0.0539*	0.0122	0.0102	0.0505*	0.0082	0.0282	0.0255	0.0667*	0.0087	-0.0618*	0.0045
16	GCONTRACT	0.0918*	-0.0059	-0.0106	0.0533*	0.0022	0.0068	-0.0224	0.0144	0.0438*	-0.0227	0.0115	0.0114	-0.0969*
17	CA	0.2847*	0.0212	0.0119	-0.0369*	0.0201	-0.031	-0.0164	-0.0045	0.0141	-0.0311	-0.0051	0.0105	0.1808*
18	MA	0.1406*	-0.0023	-0.0072	-0.0458*	0.0134	-0.0527*	0.0103	-0.0316	-0.0173	-0.0171	-0.0099	0.0124	-0.1120*
19	NY	-0.0745*	-0.0014	0.0138	-0.0119	0.0115	0.0166	0.0617*	-0.0109	-0.0065	0.0175	-0.007	-0.0144	-0.0238
20	TX	-0.0589*	-0.0027	-0.0087	0.0367*	-0.0087	0.0294	0.0017	-0.0128	0.016	-0.0244	-0.0058	-0.0014	-0.0493*

No	Variables	14	15	16	17	18	19
14	CORP	1					
15	FIRMAGE	0.0274	1				
16	GCONTRACT	0.1265*	0.0314	1			
17	CA	0.1016*	-0.1808*	-0.021	1		
18	MA	0.0569*	-0.0590*	0.0365*	-0.1982*	1	
19	NY	-0.0281	0.0579*	-0.0282	-0.1603*	-0.0728*	1
20	TX	-0.0400*	0.0001	-0.0021	-0.1706*	-0.0775*	-0.0627*

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Table 5

Univariate analysis for match-pair sample

MINORITY is equal to 1 if the establishment is owned by an ethnic minority. FOREIGN is equal to 1 if the owner of establishment has foreign status. WOWNER is equal to 1 if the establishment is owned by a woman. WCEO is equal to 1 if the establishment CEO is a woman. CHGPAYDEX indicates the annual change of maximum PayDex score. CHGDBR+ is the change in Duns & Bradstreet credit rating toward a better credit rating. CHGDBR- is the change in Duns & Bradstreet credit rating toward a worse credit rating. CHGSALE is the annual change in inflation adjusted sales of an establishment. CHGEMP is the annual change in employment of an establishment. UNEMP is a county level unemployment rate at which the establishment resides. CORP is equal to 1 if the establishment is a corporation. FIRMAGE is the number of years since the establishment is founded. GCONTRACT is equal to one if the establishment has a government contract. * indicates that the means are statistically different from the PE or VC funding sample at 1% level of significance.

Panel A. PE Funding Variable	PE funding	No funding (Control Sample)	IEGC (NETS) Sample
MINORITY	0.023	0.054*	0.018
FOREIGN	0.050	0.078*	0.057
WOWNER	0.061	0.096*	0.076
WCEO	0.002	0.003	0.023*
CHGPAYDEX	-0.474	-0.143	-0.180
CHGDBR-	0.187	0.171	0.282
CHGDNB+	0.886	0.905	0.718
CHGSALES	0.580	0.123	0.355
CHGEMP	5.721	3.401	2.412
UNEMP	5.845	6.050	7.981
CORP	0.781	0.833*	0.519*
FIRMAGE	26.320	31.881*	13.867*
GCONTRACT	0.225	0.169*	0.006*
Panel B. VC Funding Variable	**VC funding**	**No funding (Control Sample)**	**IEGC (NETS) Sample**
MINORITY	0.023	0.084*	0.018
FOREIGN	0.019	0.048*	0.057*
WOWNER	0.054	0.127*	0.076
WCEO	0.001	0.003	0.023*
CHGPAYDEX	-0.477	-0.314	-0.180
CHGDBR-	0.200	0.187	0.282
CHGDNB+	0.856	0.918*	0.718
CHGSALES	0.552	0.148*	0.355
CHGEMP	7.122	1.678*	2.412*
UNEMP	6.349	6.544	7.981
CORP	0.938	0.792*	0.519*
FIRMAGE	7.346	21.278*	13.867*
GCONTRACT	0.222	0.150*	0.006*

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Table 6
Univariate analysis between one year prior and one year after financing

This table represents univariate t-tests for the differences-in-differences between establishments that received PE or VC financing and their control (matching) establishment that never received PE or VC financing. Sales is the establishment inflation adjusted annual sales during one year prior to PE or VC financing and one year after PE or VC financing. Employment is the number of employees in the establishment during one year prior to PE or VC financing and one year after PE or VC financing. *, ** and *** are statistically significant at 10%, 5% and 1% levels.

Variables	PE	NON-PE	(PE)-(NON-PE)
Sales before PE financing (in $Million)	21.638	23.051	-1.413
Sales after PE financing (in $Million)	23.527	23.497	0.03
Change in Sales (in $Million)	1.889	0.446	1.443**
Employment before PE financing	142.183	142.585	-0.402
Employment after PE financing	149.01	144.138	4.872
Change in Employment	6.827	1.553	5.274***
Variables	VC	NON-VC	(VC)-(NON-VC)
Sales before VC financing (in $Million)	5.103	8.963	-3.86*
Sales after VC financing (in $Million)	8.179	10.872	-2.693
Change in Sales (in $Million)	3.076	1.909	1.167*
Employment before VC financing	39.44	59.168	-19.728**
Employment after VC financing	63.756	68.598	-4.842
Change in Employment	24.316	9.43	14.886***

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Table 7
First Stage: Dynamic model for the probability of receiving PE or VC funding

PEFUNDED takes on a value = 1 if the establishment receives funding from Private Equity (PE). VCFUNDED takes on a value = 1 if the establishment receives funding from Venture Capital (VC). MINORITY is equal to 1 if the establishment is owned by an ethnic minority. FOREIGN is equal to 1 if the owner of establishment has foreign status. WOWNER is equal to 1 if the establishment is owned by a woman. WCEO is equal to 1 if the establishment CEO is a woman. CHGPAYDEX indicates the annual change of maximum PayDex score. CHGDBR- is the change in Duns & Bradstreet credit rating toward a worse credit rating. CHGDBR+ is the change in Duns & Bradstreet credit rating toward a better credit rating. CHGSALE is the annual change in inflation adjusted sales of an establishment during one year prior to receiving PE or VC financing. CHGEMP is the annual change in employment of an establishment during one year prior to receiving PE or VC financing. UNEMP is a county level unemployment rate at which the establishment resides. CORP is equal to 1 if the establishment is a corporation. FIRMAGE is the number of years since the establishment is founded. GCONTRACT is equal to one if the establishment has a government contract. CA, MA, FL, NY, TX are state dummy variables to represent California, Massachusetts, Florida, New York and Texas. Other states dummies, Fama-French 48 industry dummies, and year dummies are included in the regressions but not reported to conserve the space. The standard errors are clustered by establishment level. *, ** and *** indicate statistically significant at 10%, 5% and 1% levels.

	PEFUNDED	PEFUNDED	VCFUNDED	VCFUNDED
MINORITY	-0.2108	-0.2170	-0.2912	-0.2218
	(6.73)***	(6.54)***	(6.45)***	(3.53)***
WOWNER	-0.0388	-0.0261	-0.2162	-0.1867
	(1.83)*	(1.76)*	(5.12)***	(5.71)***
FOREIGN	-0.1107	-0.0885	-0.1973	-0.1793
	(3.90)***	(2.88)***	(3.46)***	(2.48)**
WCEO	-0.1391	-0.1074	0.0168	-0.1281
	(1.31)	(0.91)	(0.11)	(0.50)
CHGPAYDEX	0.0016	0.0013	0.0005	0.0027
	(1.70)*	(1.33)	(0.36)	(1.08)
CHGDBR-	0.0551	0.0601	0.0817	0.0862
	(3.49)***	(3.36)***	(3.49)***	(2.19)**
CHGDBR+	-0.0466	-0.0417	-0.1218	-0.1399
	(2.34)**	(1.81)*	(4.20)***	(2.41)**
CHGSALE	0.0012	0.0008	0.0067	0.0018
	(1.52)	(0.96)	(1.53)	(0.24)
CHGEMP	0.0001	0.0001	0.0006	0.0011
	(1.06)	(1.21)	(1.81)*	(0.98)
FIRMAGE	-0.0024	-0.0028	-0.0412	-0.0366
	(7.64)***	(8.25)***	(24.28)***	(14.34)***
CORP	-0.1030	-0.1015	0.4244	0.3196
	(6.96)***	(6.48)***	(13.85)***	(8.22)***
GCONTRACT	0.1179	0.1112	0.1610	0.1533
	(6.50)***	(5.68)***	(5.29)***	(3.00)***
UNEMP	-0.0184	-0.0199	-0.0480	-0.0395
	(5.03)***	(4.99)***	(6.95)***	(3.77)***
CA	0.0104	-0.0039	0.2978	0.2401
	(0.57)	(0.20)	(11.51)***	(6.38)***
MA			0.2332	0.2574
			(6.31)***	(3.94)***
FL	-0.0180	-0.0707		
	(0.67)	(2.63)***		
NY	-0.0236	-0.0625	-0.0522	0.0182
	(0.93)	(2.32)**	(1.18)	(0.27)

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TX	0.0165	0.0100	-0.0524	-0.0279
	(0.73)	(0.41)	(1.19)	(0.43)
INTERCEPT	0.0581	0.1989	0.0940	0.1687
	(0.89)	(2.77)***	(0.46)	(0.81)
Observations	7748	6148	4582	1512
EST. with PE or VC	3874	3074	2291	756
Pseudo R-square	0.0638	0.0609	0.4060	0.3607
State dummies	Yes	Yes	Yes	Yes
Establishment Level Clustering	Yes	Yes	Yes	Yes

*, ** and *** are statistically significant at 10%, 5% and 1% levels.

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Table 8
Second Stage: Differences-in-differences regressions for sales growth during 3 years prior and 3 years after financing

This table presents the second stage regression of differences-in-differences (Bertrand et al., 2004; Card, Katz, and Krueger, 1994) during three year prior to PE or VC financing (period -3, -2 and -1) and three years after PE or VC financing (period 1, 2, and 3). SALEGR is the annual inflation adjusted sales growth in the establishments during 3 years prior to PE or VC financing and 3 years after PE or VC financing. PE/VC FUNDED takes on a value = 1 if the establishment receives funding from Private Equity (PE) or Venture Capital (VC). POST PEVCFUNDED takes on a value = 1 during the periods after establishments received PE or VC financing. PE/VC FUNDED*POST PE/VC is the interaction variable that represents the differences-in-differences between establishments that received PE or VC financing and their control (matching) group during periods prior versus after receiving financing. LAGSALEGR is one period lag of sales growth. MINORITY is equal to 1 if the establishment is owned by an ethnic minority. FOREIGN is equal to 1 if the owner of establishment has foreign status. WOWNER is equal to 1 if the establishment is owned by a woman. WCEO is equal to 1 if the establishment CEO is a woman. CHGPAYDEX indicates the annual change of maximum PayDex score. CHGDBR- is the change in Duns & Bradstreet credit rating toward a worse credit rating. CHGDBR+ is the change in Duns & Bradstreet credit rating toward a better credit rating. CHGSALE is the annual change in inflation adjusted sales of an establishment during one year prior to receiving PE or VC financing. CHGEMP is the annual change in employment of an establishment during one year prior to receiving PE or VC financing. UNEMP is a county level unemployment rate at which the establishment resides. CORP is equal to 1 if the establishment is a corporation. FIRMAGE is the number of years since the establishment is founded. GCONTRACT is equal to one if the establishment has a government contract. CA, MA, FL, NY, TX are state dummy variables to represent California, Massachusetts, Florida, New York and Texas. Other states dummies, Fama-French 48 industry dummies, and year dummies are including in the regressions but not reported to conserve the space. The standard errors are clustered by establishment and year. *, ** and *** indicate statistically significant at 10%, 5% and 1% levels.

	PE	PE	VC	VC
	SALESGR	SALESGR	SALESGR	SALESGR
PE/VC FUNDED	0.0290	0.0263	0.2433	0.2720
	(2.07)**	(2.49)**	(2.98)***	(3.01)***
POST PE/VC	0.0041	0.0083	0.1009	0.1034
	(0.19)	(0.34)	(1.41)	(1.56)
PE/VC FUNDED x POST PE/VC	0.0248	0.0208	0.2518	0.2285
	(2.00)**	(2.11)**	(1.79)*	(1.68)*
LAGSALEGR	-0.0001	-0.0002	-0.0002	-0.0002
	(0.98)	(0.95)	(0.30)	(0.23)
WCEO	-0.2241	-0.2463	-0.7943	-0.6918
	(2.12)**	(2.26)**	(1.85)*	(1.60)
CHGPAYDEX	0.0006	0.0011	0.0005	0.0005
	(1.40)	(1.27)	(0.61)	(0.52)
CHGDBR-	0.0087	0.0215	0.0514	0.0511
	(1.04)	(1.42)	(1.14)	(0.96)
CHGDBR+	-0.0158	-0.0281	-0.1324	-0.1647
	(1.23)	(1.40)	(1.52)	(1.64)
FIRMAGE	-0.0020	-0.0026	-0.0136	-0.0143
	(5.49)***	(5.96)***	(1.53)	(1.50)
CORP	-0.0487	-0.0627	-0.0007	0.0077
	(1.69)*	(1.82)*	(0.00)	(0.05)
GCONTRACT	0.0368	0.0452	0.1880	0.2193
	(1.29)	(1.39)	(1.35)	(1.36)
CA	-0.0239	-0.0206	-0.1979	-0.2505
	(0.70)	(0.55)	(1.69)*	(1.87)*
MA			0.0323	0.0172
			(0.12)	(0.06)
FL	-0.0803	-0.0852		
	(2.49)**	(2.19)**		
NY	-0.0046	-0.0086	-0.2458	-0.3059
	(0.09)	(0.16)	(1.75)*	(1.99)**

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TX	-0.0383	-0.0290	-0.0800	-0.0933
	(1.17)	(0.67)	(0.51)	(0.53)
INVERSEMILL	0.1373	0.1601	0.1038	0.0949
	(1.68)*	(1.99)**	(0.79)	(0.67)
INTERCEPT	0.1134	0.1686	-0.0487	-0.0201
	(1.27)	(1.53)	(0.21)	(0.08)
Observations	46,488	36,888	27,492	9,072
R-squared	0.0128	0.0123	0.0646	0.0643
EST. with PE or VC	3874	3074	2291	756
State dummies	Yes	Yes	Yes	Yes
Establishment and Year Clustering	Yes	Yes	Yes	Yes

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Table 9
Second Stage: Differences-in-differences regressions for employment growth during 3 years prior and 3 years after financing

EMPGR is the annual employment growth in the establishments during 3 years prior to PE or VC financing and 3 years after PE or VC financing. PE/VC FUNDED takes on a value = 1 if the establishment receives funding from Private Equity (PE) or Venture Capital (VC). POST PEVCFUNDED takes on a value = 1 during the periods after establishments received PE or VC financing. PE/VC FUNDED*POST PE/VC is the interaction variable that represents the differences-in-differences between establishments that received PE or VC financing and their control (matching) group during periods prior versus after receiving financing. LAGSALEGR is one period lag of sales growth. MINORITY is equal to 1 if the establishment is owned by an ethnic minority. FOREIGN is equal to 1 if the owner of establishment has foreign status. WOWNER is equal to 1 if the establishment is owned by a woman. WCEO is equal to 1 if the establishment CEO is a woman. CHGPAYDEX indicates the annual change of maximum PayDex score. CHGDBR- is the change in Duns & Bradstreet credit rating toward a worse credit rating. CHGDBR+ is the change in Duns & Bradstreet credit rating toward a better credit rating. CHGSALE is the annual change in inflation adjusted sales of an establishment during one year prior to receiving PE or VC financing. CHGEMP is the annual change in employment of an establishment during one year prior to receiving PE or VC financing. UNEMP is a county level unemployment rate at which the establishment resides. CORP is equal to 1 if the establishment is a corporation. FIRMAGE is the number of years since the establishment is founded. GCONTRACT is equal to one if the establishment has a government contract. CA, MA, FL, NY, TX are state dummy variables to represent California, Massachusetts, Florida, New York and Texas. Other states dummies, Fama-French 48 industry dummies, and year dummies are including in the regressions but not reported to conserve the space. The standard errors are clustered by establishment and year. *, ** and *** indicate statistically significant at 10%, 5% and 1% levels.

	PE	PE	VC	VC
	EMPGRW	EMPGRW	EMPGRW	EMPGRW
PE/VC FUNDED	0.0145	0.0184	0.1207	0.1405
	(0.88)	(0.91)	(1.82)*	(1.89)*
POST PE/VC	0.0195	0.0076	0.0478	0.0570
	(0.88)	(0.44)	(0.74)	(0.97)
PE/VCFUNDED x POST PE/VC	0.0303	0.0294	0.3403	0.3239
	(2.33)**	(2.76)***	(2.65)***	(2.61)***
LAGEMPGR	-0.0264	-0.0303	-0.0108	-0.0105
	(0.85)	(1.11)	(2.01)**	(2.01)**
WCEO	-0.1022	-0.0965	-0.6626	-0.5738
	(1.11)	(0.99)	(1.76)*	(1.53)
CHGPAYDEX	0.0001	0.00004	0.0006	0.0009
	(0.36)	(0.06)	(0.86)	(0.97)
CHGDBR-	0.0013	0.0039	0.0030	0.0017
	(0.19)	(0.33)	(0.08)	(0.04)
CHGDBR+	-0.0032	-0.0188	-0.0506	-0.0776
	(0.32)	(1.08)	(0.78)	(1.03)
FIRMAGE	-0.0011	-0.0015	-0.0080	-0.0071
	(3.74)***	(4.17)***	(0.99)	(0.83)
CORP	-0.0309	-0.0583	-0.1028	-0.1127
	(1.33)	(1.80)*	(0.70)	(0.79)
GCONTRACT	-0.0026	-0.0009	0.2198	0.2355
	(0.10)	(0.03)	(1.69)*	(1.57)
CA	0.0009	0.0019	-0.1348	-0.1774
	(0.03)	(0.05)	(1.33)	(1.56)
MA			-0.1088	-0.1504
			(0.95)	(1.17)
FL	-0.0467	-0.0587		
	(2.93)***	(2.78)***		
NY	0.0377	0.0161	-0.1503	-0.1945
	(0.77)	(0.32)	(1.16)	(1.39)
TX	-0.0528	-0.0504	-0.1091	-0.1056

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	(2.16)**	(1.43)	(0.78)	(0.65)
INVERSEMILL	-0.0375	-0.0399	0.0349	0.0054
	(0.90)	(0.83)	(0.29)	(0.04)
INTERCEPT	0.1641	0.2492	0.2428	0.2551
	(2.46)**	(2.57)**	(1.00)	(1.07)
Observations	46,488	36,888	27,492	9,072
R-squared	0.0134	0.0119	0.0667	0.0650
EST. with PE or VC	3874	3074	2291	756
State dummies	Yes	Yes	Yes	Yes
Establishment and Year Clustering	Yes	Yes	Yes	Yes

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Table 10
Second Stage: The impact of PE and VC financing on establishment annual growth during three years after financing

SALEGR0, SALEGR1, SALEGR2, and SALEGR3 are the annual employment growth in the establishments during period 0, 1, 2, and 3 years after PE or VC financing. EMPGR0, EMPGR1, EMPGR2, and EMPGR3 are the annual employment growth in the establishments during period 0, 1, 2, and 3 years after PE or VC financing. PE/VC FUNDED takes on a value = 1 if the establishment receives funding from Private Equity (PE) or Venture Capital (VC). POST PEVCFUNDED takes on a value = 1 during the periods after establishments received PE or VC financing. PE/VC FUNDED*POST PE/VC is the interaction variable that represents the differences-in-differences between establishments that received PE or VC financing and their control (matching) group during periods prior versus after receiving financing. All control variables from Table 9 including states dummies, Fama-French 48 industry dummies, and year dummies are included in the regressions but not reported to conserve space. The standard errors are clustered by establishment and year. *, ** and *** indicate statistically significant at 10%, 5% and 1% levels.

Panel A. PE Funding	SALEGR0	SALEGR1	SALEGR2	SALEGR3	EMPGR0	EMPGR1	EMPGR2	EMPGR3
PEFUNDED	0.0047	0.1831	0.0251	-0.0441	0.0149	0.1722	-0.0017	0.0665
	(0.18)	(1.40)	(0.16)	(0.41)	(0.68)	(1.29)	(0.01)	(1.08)
POSTPE	0.0188	0.0864	-0.0111	0.0066	0.0198	0.0816	0.0019	0.0099
	(1.07)	(1.16)	(0.37)	(0.38)	(1.94)*	(1.15)	(0.06)	(0.70)
PEFUNDED x POSTPE	0.0146	0.1972	0.0324	0.0297	0.0058	0.1764	0.0279	0.0165
	(0.91)	(3.32)***	(2.40)**	(2.57)***	(0.37)	(3.16)***	(2.21)**	(2.56)***
INTERCEPT	0.1929	1.9092	-0.2331	0.1372	0.3040	1.8548	-0.3409	-0.0260
	(1.69)*	(1.38)	(0.49)	(0.60)	(1.94)*	(1.28)	(0.76)	(0.13)
Observations	35091	32982	26106	23746	34753	32982	26106	23746
R-squared	0.0067	0.0123	0.0082	0.0170	0.0064	0.0117	0.0079	0.0104
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B. VC Funding	SALEGR0	SALEGR1	SALEGR2	SALEGR3	EMPGR0	EMPGR1	EMPGR2	EMPGR3
VCFUNDED	0.1949	0.2264	0.2623	0.0107	0.1469	0.1580	0.0371	-0.0910
	(1.43)	(1.49)	(0.76)	(0.08)	(1.87)*	(1.62)	(0.86)	(0.72)
POSTVC	0.1023	0.0344	0.0238	0.0564	0.0687	0.0692	0.0631	0.0303
	(1.57)	(0.25)	(0.46)	(0.94)	(1.19)	(0.53)	(1.23)	(0.73)
VCFUNDED x POSTVC	0.2856	0.3033	0.0642	0.03483	0.2830	0.2778	0.0268	0.0732
	(2.98)***	(2.38)**	(0.96)	(1.09)	(2.35)**	(2.34)**	(0.44)	(1.45)
INTERCEPT	-0.0118	-1.0202	-2.5033	0.8416	0.2544	-1.0829	0.4838	0.7363
	(0.05)	(1.30)	(0.58)	(1.15)	(1.08)	(1.43)	(1.80)*	(1.03)
Observations	9026	7869	6152	4884	9026	7869	6152	4884
R-squared	0.0624	0.1567	0.1078	0.0524	0.0614	0.1638	0.0565	0.0591
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Family Business and Private Equity: *Conflict or Collaboration? The Case of Messer Griesheim*

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Management scholars have long been interested in understanding value creation in buyouts. In addition to financial and operational engineering, changes in corporate governance provide a main explanation for an increase in company value after a buyout of publicly listed companies. Jensen [1989] argues that due to the concentrated ownership in the hands of the private equity firm, management is monitored more closely than in public corporations. Furthermore, management receives a higher ownership share post-buyout and hence is encouraged to maximize firm value. Compared to public markets with their expectation of steady growth in quarterly profits, private equity firms have a longer perspective, potentially leading to more effective investments to foster long-term growth (Lerner, Sørensen, and Strömberg [2010]).

These arguments can not be applied to buyouts of privately held companies in which the ownership is already concentrated on a few block-holders prior to the buyout. Opportunities for governance engineering may be particularly limited in the case of family firms where certain members of the family have dual roles as owners and managers of the firm. Through the personal involvement, the interests of growth and risk of owners and managers are naturally aligned (Schulze, Lubatkin, and Dino [2002]). Furthermore, special relations between family members allow for informal, self-reinforcing governance mechanisms (Mustakallio, Autio, and Zahra [2002]). The family's emotional attachment to the company, their commitment, and their long investment horizon may be sources of competitive advantage for family firms (Villalonga and Amit [2010]). Family firms may therefore be characterized by governance benefits, and opportunities to create value through changes in corporate governance may be limited in family-firm buyouts. The advantages generated by a private equity involvement may be insufficient to make up for the costs (e.g., the substantial transaction costs associated with purchasing and selling the firm, the distortions associated with the need to raise a follow-on fund, and the like).

But it can also be argued that family ownership and management exposes the firm to the risk of increased governance costs. Family owners are likely to favor family members for key management positions even if more suitable non-family managers exist, which can be interpreted as a form of private benefits appropriation (Villalonga and Amit [2010]). The involvement of family members in both management and control of the company poses a problem of self-control that could make family firms more exposed to managerial entrenchment and maximization of the family's wealth rather than of the company's value. Furthermore, as it is difficult for non-family members to get promoted to senior management positions, highly talented professionals may not choose to work for the family firm and employed personnel may get frustrated by their limited career opportunities, which increases the need for monitoring (Schulze, Lubatkin, Dino, and Buchholtz [2001]). These governance issues could offer the potential for private equity firms to create value through changing management positions, actively monitoring the management, and incentivizing key personnel.

The two different views on the governance benefits and costs in family firms are not mutually exclusive and could both exist in a family firm. Accordingly, recent studies have shown that the effects of family ownership are particularly favorable in family firms controlled by the founding generation, whereas they are more detrimental in family firms owned by later generations (Villalonga and Amit [2010], Wang, Ahmed, and Farquhar [2007], and Villalonga and Amit [2005]). For private equity firms, it is relevant to understand circumstances under which they can create value through governance engineering in family firms.

In the last decade, private equity investments in privately held family firms were a common phenomenon in Germany (Scholes et al. [2009]). Some of these investments suffered severely from the economic downturn in recent years, which even led to bankruptcies of prominent family firms. These failures triggered a negative description of the role of private equity groups in family firms (DGB [2009]). This article follows the history of the family firm Messer Griesheim, from being part of a large conglomerate, through being owned by private equity firms, until ultimately the family regained full control over some of the businesses. The examination of the Messer Griesheim case provides insights into a typical restructuring case and highlights the changes in corporate governance through the interplay between family owners, industrial companies, and private equity firms. Our in-depth analysis allows us to identify important levers for value creation in the context of a privately held family firm. Furthermore, we explore potential conflicts that can stem from the different investment horizons of external investors and the family, in particular the differences between the longer investment horizon of family firms and medium-term outlook of private equity firms.

In April 2001, Allianz Capital Partners (ACP) and Goldman Sachs acquired two thirds of the shares in Messer Griesheim from the conglomerate Aventis for €2.1 billion in a divisional buyout. The Messer family kept the remaining minority stake. At that time, the deal was one of the largest buyouts in Germany as well as the largest industrial buyout in Europe. Post-buyout, an ambitious restructuring plan enabled the company to divest non-core entities. The deal successfully navigated the delicate nature of specific corporate governance aspects of a private equity-backed family firm with global operations. Despite reductions in employment, employee development remained a critical issue for management throughout the deal, as the team provided incentives to encourage key employees to stay with the core businesses. With the exit of ACP and Goldman Sachs in 2004, Messer Griesheim's German, U.K., and U.S. businesses were sold to Air Liquide. The Messer family acquired the shares of the private equity firms in the remaining entities, enabling them to gain control over some of the original businesses. The exit brought a healthy but smaller entity back under family control and culminated the long term effort by the Messer family to stabilize its influence in the business (see Exhibit 1 for a timeline of important events at Messer Griesheim).

The Messer Griesheim case offers an interesting context to analyze value creation post-buyout. The case demonstrates that despite minor conflicts due to different time horizons and priorities, a valuable relationship between the family and the private equity firms was built up. Changes in corporate governance under private equity ownership alleviated governance benefits in the company through intensive monitoring, the inclusion of valuable outside board members, and incentivizing upper management levels to work towards increasing company value. The case provides evidence that the family owner whose decision power was restricted prior to the buyout became more entrepreneurial under private equity ownership, which laid the ground for resuming full control over parts of the original company.

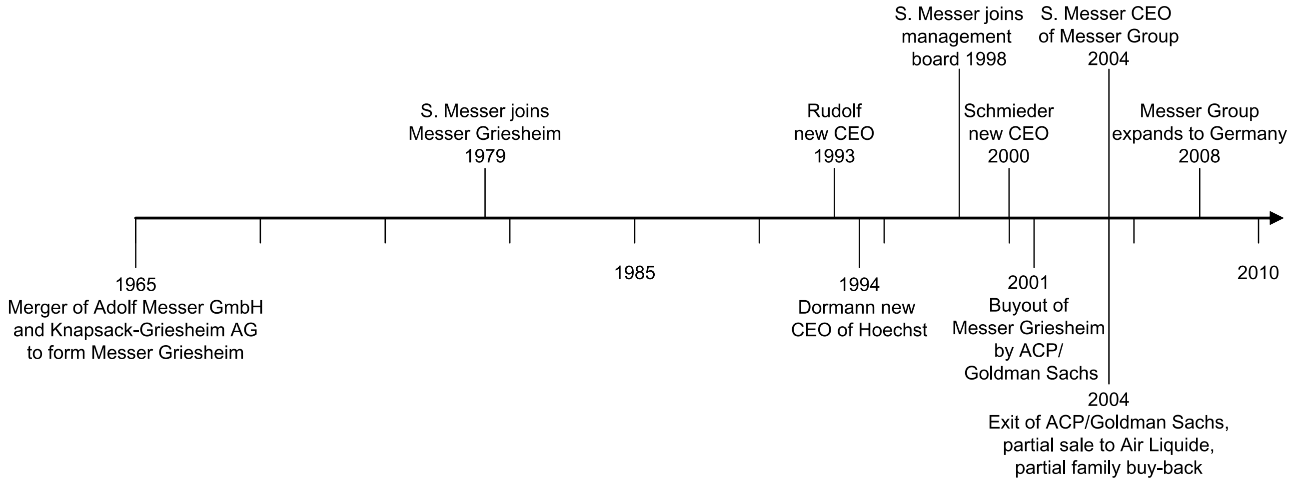
1898: FOUNDING OF A FAMILY FIRM

In 1898, Adolf Messer founded Frankfurter Acetylen-Gas-Gesellschaft Messer & Cie, manufacturing acetylene generators and lighting fixtures. By 1908, the company's product range had expanded and included oxy-acetylene cutters, welding and cutting torches, acetylene generators and pressure regulators, and oxygen systems, including those used in oxy-fuel

EXHIBIT 1

Timeline of Important Events in Messer Griesheim's Company History

The timeline shows important events at Messer Griesheim from 1965, when Messer Griesheim was formed after the merger of Adolf Messer GmbH and Knapsack-Griesheim AG.



technology. The company continued to grow and established an international presence with its first air separation plant in Madrid, its first international office in Oslo, and a U.S. branch in Philadelphia. After World War II, international expansion continued with the establishment of holdings and cooperative partnerships in Europe and the United States.

BEING PART OF THE HOECHST PORTFOLIO

In 1953, the founder passed the reins over to his son, Dr. Hans Messer, with the company at 1,100 employees. The new CEO realized it was not possible to grow the company organically. Messer had specialized in building production plants, but his aim was to expand into producing gases. He searched for a strong partner, and started discussions with Hoechst and BASF. Chemicals and pharmaceutical giant Hoechst showed interest in a merger of Messer and Knapsack-Griesheim, a Hoechst subsidiary. Karl Winnacker, the CEO of Hoechst at that time, had a persuasive nature, and an increasingly cordial relationship developed between him and Hans Messer. However, while negotiations were underway, BASF, a global chemicals company, entered the picture and proposed a 50-50 share split, with each party getting 50% voting rights (Lesczenski [2007]).

But Hans Messer saw a greater fit in Hoechst, which like Messer was Frankfurt based, because their production programs complemented each other well and they were able to pool their resources in research and development. In addition, the friendly relationship between Hans Messer and Winnacker played an important role in the negotiations. In 1965, Messer and Knapsack-Griesheim merged, resulting in Messer Griesheim becoming a subsidiary of Hoechst with business activities in three main areas: welding technology, cryogenics, and industrial gases. The Hoechst team agreed to some of the proposed BASF acquisition structure in order to close the deal. The Messer family expected to retain the same voting rights they would have received in the BASF acquisition, even though their share of equity—only one-third in the merger with Knapsack-Griesheim—was lower than what had been envisioned with BASF.

Until the mid-1990s, Messer Griesheim was considered one of the pearls of Hoechst's portfolio. Relations between Messer Griesheim and the management of Hoechst were good, and collaboration was problem-free. Messer Griesheim investments were financed by ongoing cash flow, making the company relatively independent from Hoechst. But as conditions shifted across Hoechst, Messer Griesheim's glow began to fade.

In 1994, Jürgen Dormann took over as CEO at Hoechst and initiated a strategy shift to focus on core activities with the ultimate aim of turning the company into a "pure" life sciences company. Hoechst's low-profit basic- and specialty-chemical divisions as well as the cosmetics unit were divested. The strategy proved to be successful, and the Hoechst share price doubled between 1994 and the end of 1998 (Tomlinson [2002]). Dormann's aim was to focus Hoechst's activities only on entities in which they were able to hold a lead or second market position. In December 1999, Hoechst merged with Rhone-Poulenc to form Aventis, the world's sixth-largest pharmaceutical group, with Dormann becoming CEO.

At that time, Messer Griesheim did not hold a lead market position in many of its activities, and Dormann's eventual goal was to sell the shareholdings in the firm, also because it did not fit in the new primary focus on life-science activities. Messer Griesheim's management felt that Dormann's criteria to assess the lead position could not be readily applied to the industrial gas markets. In this regional business, it is more important to take into account the leadership within a region rather than on a national or even global scale.

With the decision taken to divest its Messer Griesheim shares, Hoechst worked to grow Messer Griesheim's business to make it more attractive to potential buyers. Herbert Rudolf, member of the management board and post-1993 CEO of Messer Griesheim, followed an aggressive expansion strategy. Rudolf had the support of Dormann and was able to act independently from Hoechst. Hoechst believed it would be easier to find a buyer for a larger business with a greater number of global entities. Therefore, Rudolf established a stronger presence in Latin America, Africa, Asia, and Eastern Europe through the acquisition of existing companies and the construction of new air separation plants.

STRUGGLES FOR THE THIRD GENERATION

Succession is widely recognized as one of the major challenges for family businesses (Chrisman, Chua, and Sharma [2005] and Chua, Chrisman, and Sharma [2003]). In order to preserve and extend competitive advantage, the transfer of explicit as well as tacit knowledge from one generation to the other is essential. It is difficult to express or formalize tacit knowledge, because it is context-specific and develops in an individual situation. Therefore, the transfer of tacit knowledge is challenging and possible only by its application in practice (Cabrera-Suárez, Saá-Pérez, and García-Almeida [2001]).

Messer Griesheim exemplifies a case where the transition from one generation to the other was prepared over a long period of time during which the descendant, Stefan Messer, was able to gain important experience inside the family business. After the death of Hans Messer in 1997, son Stefan Messer—as the third generation active in the family firm—joined the management board in January 1998. At that time, he already had nearly 20 years of experience working at Messer Griesheim. Initially, he had positions in sales at Messer Griesheim in Austria and Germany from 1979 to 1985. Subsequently he worked for Hoechst for two years before he became CEO of the Messer subsidiaries, first in the Netherlands and then in France. Finally, he returned to Messer Griesheim headquarters in 1998.

The long-term involvement of Stefan Messer in the family firm demonstrates his commitment to the family business, which is crucial for a successful transition from one generation to the next (Sharma and Irving [2005] and Chrisman, Chua, and Sharma [1998]). The family followed a long-term process to pass on valuable knowledge to the third generation by giving Stefan Messer expanding roles across different business functions within the family firm while Hans Messer was still the CEO. Following Steier [2001], the succession process can be described as natural immersion, where family members follow a gradual transfer of social capital. However, research has shown that family succession can be threatened by relational factors such as conflicts between the successor and non-family managers (De Massis, Chua, and Chrisman [2008] and Bruce and Picard [2006]). This can also be seen in the case of Messer Griesheim where, in his initial years on the management board, Stefan Messer was faced with an environment where he was not able to fully develop his entrepreneurial potential.

The year Stefan Messer joined the management board, Messer Griesheim had 24 new plants under construction and had negotiated new contracts to build 20 cryogenic and 65 non-cryogenic plants. One year later, Messer Griesheim announced plans to build a 3,000-tons-per-day plant for Thyssen/Krupp, requiring an investment of €50 million (Goldman Sachs [2007]). The Messer Griesheim expansion came at a cost. Messer Griesheim spent €2 billion between 1995 and 2000 on capital investments, representing 25%–30% of its sales. Industry consolidation and market conditions made it clear that the expansion activities had been ill-conceived. After the late entry into several key markets where competitors had already cherry-picked desirable holdings, the company held several questionable investments around the globe and the financials painted a poor picture. Operating margins and returns on capital were lower than the industry average. Even the employee section of the supervisory board criticized Rudolf's expansion strategy, accusing the management of using overly optimistic assumptions or even false investment analysis and planning. Rudolf's aim was to increase the market share in the onsite market, and he offered low-priced, long-term contracts. All in all, Messer Griesheim's leverage increased from €423 million in 1996 to €1.6 billion in 2000 (Alperowicz [2002]).

It was a difficult time for Stefan Messer. The relationship to Rudolf was highly problematic, because Rudolf systematically did not inform him about important business decisions in order to prevent him from having an operational influence. Rudolf did not invite Stefan Messer to meetings of the management board because he claimed that the management would not be able to openly discuss business issues with Stefan Messer present as a "leaky spot" or "spy for the family" (Lesczenski [2007], p. 146). In addition, Rudolf defamed Stefan Messer openly by accusing him of causing damage to the business. At the same time, the family was aware that Rudolf's expansion strategy was threatening to undermine the company's future. Stefan Messer, as well as his mother Ria Messer, voiced concerns about the over heated expansion course and the increasing leverage as well as the decreasing performance that came with it. They tried to alert the supervisory board of Messer Griesheim as well as the management board at Hoechst and later Aventis, but their alerts were not followed up. Without real entrepreneurial influence in the company, Stefan Messer was increasingly frustrated with his role. Stefan Messer recalled: "I really wanted to stay in operational affairs, where I'd gathered experience and had a record of success. But Herbert Rudolf would not let me. So that was how I spent the first two or three years in the senior management team—or rather, had somehow to put up with." (Lesczenski [2007], p. 124.) The conflict between Rudolf and Stefan Messer prevented the family from effectively monitoring the management. The parent company Hoechst (and later Aventis) did not intervene either, as they believed Rudolf's strategy would put them in a good position to divest their shares. So despite concentrated ownership in the hands of the parent company and the family, the management was able to act independently and uncontrolled.

As Aventis continued to focus on becoming a life sciences company and sold its agricultural and chemical concerns, it reviewed different options to divest its shares in Messer Griesheim. Rudolf wanted to take the company public, but with its critical financial situation an initial public offering was not an option. With ongoing consolidation in the market, a merger with a competitor seemed to be an alternative. After the suggestion of a supervisory board member, discussions with AGA were initiated, but without including the Messer family. Management then asked the Messer family to sign confidentiality agreements, although they had not been involved in the negotiations. AGA was not informed about the family's special voting rights or of their veto rights. The deal fell apart, with the family boycotting negotiations. It became clear that it was not possible to close a deal without the agreement of the Messer family, which wanted to keep their shareholdings.

Stefan Messer proposed a merger with Linde and initiated discussions with Linde's management. They agreed in principal that the family would keep only 10% of the shares, but would retain their special voting rights. Hoechst management acquiesced to the discussions and the subsequent due diligence, but they were tentative towards a deal and delayed the negotiation process. The two sides held fundamentally different views: Hoechst sought the highest price it could get for the Messer Griesheim shares, and the Messer family was committed to keeping their special rights. In the summer of 1999, Linde and AGA merged, which made a merger with Messer Griesheim more difficult due to antitrust issues. However, some felt these issues could have been resolved through Linde-AGA divesting certain entities in question to another player such as Air Liquide. The family felt that a merger with Linde-AGA failed largely due to Hoechst's behavior. It became obvious that it was increasingly difficult to agree on an exit strategy.

In January 2000, former Hoechst CFO Dr. Klaus-Jürgen Schmieder took over from Rudolph as CEO at Messer Griesheim. Schmieder had gained the reputation of consolidator within Hoechst and he was expected to follow a similar strategy at Messer Griesheim. Hoechst—now Aventis—publicly stated its intentions to sell its Messer Griesheim shareholdings within one year. Negotiations with the U.S firm Praxair had started, but ended quickly due to antitrust issues and the family's desire to retain its position in the company's management. With the support of strategy consultants Roland

Berger, Schmieder and his team had already developed a detailed restructuring plan including the divestiture of several non-core businesses and a restructuring plan for the core activities, which among other things called for a reduction of 850 employees. The restructuring plan was supposed to help the company regain its financial flexibility.

In preparation for its exit, Aventis proposed to have a beauty contest with a range of private equity investors, but the Messer family refused to have an auction. Again, the conflict between Aventis and the Messer family became apparent. In March 2000, Aventis announced that it would divest its Messer Griesheim stake via a limited auction including financial investors. They selected a number of potential investors and ended up negotiating with ACP and Goldman Sachs, among others.

NEW START: THE DEAL WITH ACP AND GOLDMAN SACHS

ACP was started in 1998 as a wholly owned subsidiary of the Germany-based Allianz Group, a leading global insurance, banking, and asset management company. ACP was among the leading companies in the European direct private equity market in 2001. Goldman Sachs Capital Partners, the venerable Wall Street firm's private equity funds business, was active in Europe since 1993 and had built up a strong reputation in Germany.

ACP was considered an interesting partner for Messer Griesheim due to its long investment horizon as it received long-term capital directly from Allianz. In addition, Messer Griesheim already had an established business relationship with Allianz through a number of insurance contracts. This helped in establishing trust with the family. It gave the family, in particular Ria Messer, confidence that Allianz, a stable, blue-chip German company, was involved. As ACP already owned 10% of Linde, it was not able to do the deal on its own. Therefore, it brought in Goldman Sachs early in the negotiations. The deal was not going to be realized without the consent of the entire family. The private equity firms had to put in considerable effort to build up a trusting relationship. Ria Messer, the most senior member of the family, insisted on meeting Dr. Stephan Eilers and the CFO of Allianz Group to discuss the deal. Eilers played an important role in the deal negotiations, as he had already worked with Messer Griesheim on other transactions and, hence, had already built a trusting relationship with the family. Finally, the deal was closed and a compromise between Aventis and the family was reached. Aventis was able to sell its shares for €2.1 billion to ACP and Goldman Sachs. The family had private equity partners by their side who were aware of their long-term vision to keep control over the company and who supported the company in its restructuring efforts.

Under some of the deal terms, the goal of the Messer family to keep control over the company in the long term was apparent. They requested a call option to buy back the ACP and Goldman Sachs shares after three years. The parties agreed that the family would have to pay off the private equity partners with a 30% internal rate of return. The family was able to exercise the option in the window between April and September 2003. Afterwards, the private equity firms had the right to pursue an IPO or sell their shares to the highest bidder after January 2004. Through a drag-along right, the family would have had to sell their shares to the same bidder.

The negotiations for the by-laws, which included the special voting rights and the veto right, took much longer than the negotiations for the basic arrangement of the call option. Prior to the buyout, the family had 50% of the voting rights; after, the investors would gain majority control with their voting rights representing their share of ownership of 66.2%. However, in order to provide the family with the ability to exert influence, the family received a right to veto key strategic decisions. ACP, Goldman Sachs, and the family jointly agreed to a restructuring plan that was closely based on Schmieder's original work with Roland Berger. Any change of strategy from this plan required the consent of all three parties.

As required in Germany, Messer Griesheim was governed by a two-tier system consisting of the management board and the supervisory board. Furthermore, an additional committee to the supervisory board, a shareholder committee, was formed. Schmieder, Stefan Messer, Jürgen Schöttler, and Peter Stocks had been members of the management board prior to the deal and remained there post-buyout. In contrast, both the supervisory board and the shareholder committee was changed. In line with German co-determination law, the supervisory board was comprised of a shareholder section and an employee section, with equal representation.

ACP was not allowed to have representatives on the supervisory board or the shareholder committee because of antitrust issues related to its ownership of Linde. The shareholder section of the supervisory board was therefore divided equally between the family and Goldman Sachs, each with three representatives. The Messer family selected Dr. Jürgen Heraeus, Wilhelm von Storm, and Dr. Gerhard Rüschen, while Goldman Sachs chose Dr. Alexander Dibelius, Udo Stark, and Stephen Trevor. In addition, six employee representatives were members of the supervisory board, of which two were union representatives. The shareholder committee only included representatives of the family and representatives of Goldman Sachs. The same representatives who were members of the supervisory board for the family and Goldman Sachs were also members of the shareholder committee. In addition, the Messer family appointed Eilers and Goldman Sachs selected Wesley Clark to be representatives on the shareholder committee. Exhibit 2 gives an overview of the corporate governance structure at Messer Griesheim post-buyout.

Stefan Messer selected Heraeus because he also ran a major family business with global operations. Heraeus became chairman of the supervisory board. Von Storm, who had been with Messer Griesheim for over 40 years, was selected because he knew the company and the Eastern European business very well. Rüschen had close ties with the family. Eilers had been involved in the deal from the start, recommending ACP and Goldman Sachs early on to the Messer Griesheim management, and became the lead lawyer for the transaction.

The supervisory board played an important role in maintaining cohesion between management and employees. Fritz Klingelhöfer, one of the employee representatives, became co-chairman of the supervisory board. The employee section of the supervisory board was made up of long-term Messer Griesheim employees. The employee representatives were well regarded by the management due to their industry experience and knowledge of the business model and everyday business issues.

The shareholder committee focused on high-level strategic decisions and did not have much influence on operational issues. Operational issues were discussed in monthly meetings of the supervisory board and the management board, which usually lasted a full day. In these meetings, important decisions on the details of the restructuring and divestiture plan were made. The management reported on the status of the undertakings, and the details of the financial plans were monitored closely throughout.

Exhibit 2

Corporate Governance Structure at Messer Griesheim Post-Buyout

This exhibit shows the corporate governance structure post-buyout from 2001 to 2004 at Messer Griesheim. Due to antitrust reasons, ACP was not allowed to appoint members to the supervisory board or the shareholder committee.

Management Board	• Klaus-Jürgen Schmieder (CEO) • Stefan Messer		• Jürgen Schöttler • Peter Stocks
Supervisory Board Operational decisions in monthly meetings	**Shareholder Representatives**		**Employee Representatives**
	Family • Jürgen Heraeus (Chairman) • Wilhelm von Storm • Gerhard Rüschen	**Goldman Sachs** • Alexander Dibelius • Udo Stark • Stephen Trevor	• Fritz Klingelhöfer (Co-chairman) • Five other employees, including two union representatives
Shareholder Committee Strategic decisions	**Family** • Same three as in supervisory board • Stephan Eilers		**Goldman Sachs** • Same three as in supervisory board • Wesley Clark

Stefan Messer gained influence post-buyout. After the problematic role under Hoechst (and later Aventis) ownership, he was now more accepted by the private equity firms and his voice became stronger. He was present in all meetings of the management board, the supervisory board, and the shareholder committee, and thereby he automatically had more influence on operational decisions. He experienced a totally different management style from a subsidiary of a large conglomerate to a private equity financed company. He gained entrepreneurial experience in managing and restructuring Messer Griesheim as an independent company. However, minor conflicts arose between Stefan Messer and the private equity firms. While the private equity firms based their divestment decisions purely on financial aspects, Stefan Messer also took non-financial aspects into account. Furthermore, Stefan Messer saw future value potential in some of the entities that ACP and Goldman Sachs proposed to divest. For example, Stefan Messer fought against closing down a production site in Hanau, close to Messer Griesheim's headquarters, which would have put 100 people out of work. It became apparent that Stefan Messer felt more emotionally attached to the Messer Griesheim employees and more responsible for them.

The appointment of Clark as compliance director after the initiative of Dibelius, co-chairman of Goldman Sachs Germany at the time, also led to some discussions between Stefan Messer and the private equity firms. In 1999, Messer Griesheim had faced an instance of fraud and compliance problems in South America. A manager originally hired by Hoechst and not from within the ranks of Messer Griesheim was sent to South America by Dormann to build up Messer Griesheim's business there. During his stay, he embezzled several million dollars and was later prosecuted. Clark institutionalized strict rules of compliance and prepared detailed reports to educate employees on ethics and morale values. His initiatives led to a change in corporate culture, and compliance was now regarded as an important company policy. However, Clark also wanted Messer Griesheim to adhere to American standards and, for instance, not to do business in certain countries. Stefan Messer felt that it was inappropriate for a German company to have to adhere to these standards.

The deal with ACP and Goldman Sachs introduced a management incentive program that was innovative by German standards. An exit bonus was offered to the first and second management level of each department. Managers had the chance to buy shares in the holding company for the same price as ACP and Goldman Sachs. However, the minimum amount for participation was one-third of the manager's annual salary. Some of Messer Griesheim's management was under pressure to participate in the program, as it was clear that ACP and Goldman Sachs expected them to do so. As the holding period of the private equity investors could not be foreseen, the program acted as a long-term incentive for managers to stay with Messer Griesheim.

Even though the internet hype had made stock-option schemes more common, there were still very few programs of this kind in Germany, leading to a natural distrust in Messer Griesheim's German and European businesses. The program had higher acceptance in the U.S., where such programs were not unfamiliar; more managers took part, and invested on average a higher amount, in the U.S. compared to other regions. The difficult situation at Messer Griesheim was not a secret, and one-third of the annual salary was a large investment for most of these managers. Many were afraid to risk such a large portion of their savings. Messer Griesheim's top management put in a lot of effort and time in one-on-one meetings with the managers to explain the program, and that its level of risk was in fact less than perceived given the participation of the private equity investors. Eventually, about 85%–90% of Messer Griesheim's managers participated.

Long before the deal with ACP and Goldman Sachs, the employees were aware of the difficult circumstances at Messer Griesheim. In 2000, Schmieder had been open about the difficult situation and emphasized the need for restructuring and divestitures in order to sustain the business. Therefore, employees felt the deal with ACP and Goldman Sachs was a logical step towards improving Messer Griesheim's situation. Post-buyout, the private equity investors had little contact with the employees, and nothing actually changed in their everyday operations. ACP and Goldman Sachs were directly visible only to higher management ranks.

Roland Berger's restructuring plans had always included employment reductions in order to restructure the company's core activities and as part of divesting non-core entities. The divestitures, except two, were sold to strategic buyers; the team sought out entities with a pre-existing presence in the market. Between 2000 and 2003, Messer Griesheim's full-time equivalent headcount decreased by 11.2% per annum from 10,200

Exhibit 3

Messer Griesheim Headcount and Employee Productivity Analysis

This exhibit presents the development of the number of employees (full-time equivalents) and employee productivity at Messer Griesheim per region between December 2000 and December 2003. CAGR = compound annual growth rate.

Number of Employees	2000	2001 (Buyout in April)	2002	2003	Total Change	CAGR 2000 to 2003
Germany	2,600	2,244	2,208	2,134	(466)	(6.4%)
North America	1,400	1,163	996	1,093	(307)	(7.9%)
Western Europe (excluding Germany)	1,100	1,017	997	916	(184)	(5.9%)
Eastern Europe	2,600	2,369	2,283	2,130	(470)	(6.4%)
Asia, Africa, and Latin America	2,500	1,555	741	871	(1,629)	(29.6%)
Total Employees	10,200	8,348	7,225	7,144	(3,056)	(11.2%)

Employee Productivity (in € thousands)	2000	2001 (Buyout in April)	2002	2003	CAGR 2000 to 2003
Revenue per employee	166	194	211	210	8.0%
EBITDA per employee	35	44	56	56	16.9%
Capex per employee	32	15	19	18	(17.4%)

Source: Goldman Sachs [2007].

to 7,144 (see Exhibit 3). According to Goldman Sachs, approximately 80% of these employment reductions came from divestitures and, hence, did not represent actual job losses, as the strategic buyers employed many of the former Messer Griesheim employees. This increase was mirrored in the increasing number of employees in certain regions at major competitors in the same period (see Exhibit 4).

For restructuring the core activities, the original plan called for a reduction of 850 employees, amounting to an overall target of €100 million in savings mainly on employment costs. Roland Berger had analyzed the savings potential in each department of every entity. Post-buyout, these recommendations were followed but in a less aggressive way. The number of actual reductions was less than what was envisioned in the original plan. For ACP and Goldman Sachs, the headcount was an important indicator in monitoring the status of the restructuring activities. They considered employment costs a variable that could be easily reduced. But it took a long time before the number of employees in the core businesses was substantially reduced, as it required discussions with the employee representatives in the work council, who in general understood the necessity of the initiatives. The HR group involved each department's management in the reduction decisions, enabling them to explain the rationales behind them.

About 350 employees, mostly located in Germany, lost their contracts due to restructuring initiatives in the core entities. A portion of these layoffs was due to the outsourcing of Messer Griesheim's German haulage operations, which was seen as a necessary step prior to the exit of ACP and Goldman Sachs and affected about 100 employees. All of them received offers from another German haulage company, but the proposed contract carried standard conditions for the haulage industry that differed from those in their prior Messer Griesheim contract.

The company was able to divest its less successful entities, thereby substantially increasing productivity. Between 2000 and 2003, revenues per employee increased by 8% per annum, EBITDA per employee increased dramatically by 16.9% per annum, and capex per employee decreased substantially by 17.4% per annum (see Exhibit 5).

In 2000, a new strategy was started in order to improve the development of management and employees at all levels and across all regions. The program, which also aimed to improve upper management succession, included a new database that tracked positions as they

EXHIBIT 4

Headcount Development at Major Competitors

This exhibit presents the development of the number of employees (full-time equivalents) at major competitors of Messer Griesheim per region between fiscal-year 2000 and 2003. CAGR = compound annual growth rate.

Number of Employees	2000	2001	2002	2003	Total Change	CAGR 2000 to 2003
Linde						
Germany	18,475	18,380	18,154	17,807	(668)	(1.2%)
Europe (excl. Germany)	20,436	19,693	19,637	19,921	(515)	(0.8%)
North/South America	6,371	6,256	6,340	6,292	(79)	(0.4%)
Asia	1,409	1,577	1,885	2,139	730	14.9%
Australia/Africa	435	494	505	503	68	5.0%
Total Employees	47,126	46,400	46,521	46,662	(464)	(0.3%)
Thereof Gas	18,661	17,689	17,500	17,420	(1,241)	(2.3%)
Air Liquide						
France	9,393	9,856	9,856	10,208	815	2.8%
Europe (excl. France)	8,787	8,932	8,932	8,932	145	0.5%
Americas	8,181	8,008	7,392	7,337	(844)	(3.6%)
Asia Pacific	3,030	3,080	3,388	4,147	1,117	11.0%
Africa	909	924	1,232	1,276	367	12.0%
Total Employees	30,300	30,800	30,800	31,900	1,600	1.7%
BOC						
Europe	11,398	12,173	13,213	12,353	955	2.7%
Americas	6,969	7,305	7,243	7,451	482	2.3%
Africa	17,137	16,120	17,435	17,138	1	0.0%
Asia/Pacific	7,205	7,573	8,389	7,565	360	1.6%
Total Employees	42,709	43,171	46,280	44,507	1,798	1.4%
Praxair	23,430	24,271	25,010	25,438	2,008	2.8%
Airgas	8,000	N/A	>8,500	N/A		

Source: Annual reports of Linde, Air Liquide, BOC, Praxair, and Airgas.

EXHIBIT 5

Key Financials of Messer Griesheim Pre- and Post-Buyout

This exhibit presents consolidated revenues, EBITDA, and capex at Messer Griesheim between December 1996 and December 2003. CAGR = compound annual growth rate.

In € Million

Key Financials	1996	1997	1998	1999	2000	2001 (Buyout in April)	2002	2003	CAGR 1996–1999	CAGR 2000–2003
Revenues	1,148	1,269	1,477	1,492	1,696	1,621	1,526	1,498	9.1%	(4.1%)
% Growth		10.5%	16.4%	1.0%	13.7%	(4.4%)	(5.9%)	(1.8%)		
EBITDA	287	307	336	308	359	371	403	402	2.4%	3.8%
% of Sales	25.0%	24.2%	22.7%	20.6%	21.2%	22.9%	26.4%	26.8%	(6.2%)	8.2%
Capex	299	425	561	557	322	129	136	127	23.0%	(26.7%)
% of Sales	26.0%	33.5%	38.0%	37.3%	19.0%	8.0%	8.9%	8.5%	12.8%	(23.6%)
Net Debt	423	689	1,007	1,356	1,627	1,393	1,240	1,117	47.4%	(11.8%)

Source: Goldman Sachs [2007].

were likely to become vacant and matched them to the most appropriate successors. When ACP and Goldman Sachs entered the picture, they did not change the program but monitored it closely and requested detailed information on its status. The program had always been targeted towards improving management quality, but the interest from ACP and Goldman Sachs gave it additional emphasis and helped to push it forward.

EMPOWERMENT OF THE FAMILY: EXIT OF ACP AND GOLDMAN SACHS

In the second half of 2003, the pressure on the Messer family to exercise their call option increased as the window for the option closed at the end of September. The family did not have the required funds to buy back the whole entity and, hence, their goal was to gain control over at least a portion of their business. At first they hoped to include half of the German operations and additional entities in the buyback while divesting the remaining parts of the business to a strategic buyer. Stefan Messer felt very much attached to the German entities, as this was the home country and seen as the heart of the company.

Stefan Messer had proposed Air Liquide as a possible buyer, but ACP and Goldman Sachs considered a public auction. As Goldman Sachs initiated the auction for half of the Messer Griesheim German concerns and additional entities in the U.K. and the U.S., incoming bids were not high enough. It became obvious that the German concerns were the core business of Messer Griesheim and, therefore, the most attractive part for any strategic buyer. They realized that they had to sell the entire German business, and a second auction, including all German operations, was started. Only a few bids came in, but in May 2004 with an additional €100 million added to its initial offer, Air Liquide bought Messer Griesheim's operations in Germany, the U.K., and the U.S. for €2.7 billion. In terms of the sales multiple, the price paid by Air Liquide was in line with comparable deals at that time (Goldman Sachs [2007]). The remaining parts of the business, including businesses in Western Europe, Eastern Europe, China, and Peru, were acquired by the Messer family with the agreed 30% internal rate of return for the private equity firms. The exit was closed after the window for the call option had already elapsed, but the investors decided to extend the call period, as they saw the company's positive development and a successful exit seemed on the horizon. Shortly after the exit of ACP and Goldman Sachs, Schmieder joined the management board of Air Li–quide. Stefan Messer became CEO of the newly formed Messer Group, which acted as a holding company for the bought-back entities.

The Messer Griesheim case represents a unique private equity deal where the eventual breakup of a company at the exit was not initiated by investors, but rather was motivated by the family in order to regain control over attractive parts of the business, albeit not the favored German operations. The strategy did not necessarily maximize the investors' return, because a higher return may have been realized by selling the entire company to the highest bidder. There were three main rationales behind this. First, the partial buyback of the family implied a high flexibility in structuring the deal with Air Liquide. All parts of the business that might have led to antitrust issues were separated from the deal with Air Liquide and sold to the family. Second, with an exit multiple of over 10 times EBITDA, the sale to Air Liquide was highly profitable, so ACP and Goldman Sachs already achieved a high return through that part of the exit. Third, it underlined the trusting relationship between the family and the private equity firms. Rather than emphasizing the effort to maximize returns, both ACP and Goldman Sachs enhanced their reputation by allowing the Messer family to regain control over various parts of the businesses. As *The Economist* noted, "Goldman Sachs and Allianz Capital [Partners] would love it to be known, from this example, that although they might have made more money if they had found an industrial buyer [for the whole entity], they can be fairy godmothers to family firms who might be wary of using private equity." (*The Economist* [2004].)

2008: RETURNING TO GERMANY

After the buyout, Stefan Messer as CEO of Messer Group had the full responsibility for the company. His multi-decade effort to regain family control over the business and to increase its own operational responsibility came to a successful end. He initiated a course of cautious expansion and, despite an increase in energy prices and raw materials over the next few years, he was able to grow the business both in terms of total sales, EBITDA, and number of employees (see Exhibit 6).

EXHIBIT 6

Key Financials of Messer Group Post-Exit of ACP/Goldman Sachs

This exhibit presents consolidated revenues, EBITDA, capex, and employee productivity at Messer Group post-exit of ACP/Goldman Sachs between December 2004 and December 2008. CAGR = compound annual growth rate.

In € Million

Key Financials	2004	2005	2006	2007	2008	CAGR 2004–2008
Revenue	521	575	630	705	795	11.1%
% Growth		10.4%	9.6%	11.9%	12.8%	
EBITDA	130	138	144	154	172	7.2%
% of Revenues	25.0%	24.0%	22.9%	21.8%	21.6%	(3.5%)
Capex	92	108	153	173	194	20.5%
% of Revenues	17.7%	18.8%	24.3%	24.5%	24.4%	8.4%
Employees	3,762	4,005	4,247	4,380	4,696	5.7%
Employee Productivity (in € thousands)						
Revenue per employee	138	144	148	161	169	5.1%
EBITDA per employee	35	34	34	35	37	1.5%
Capex per employee	24	27	36	39	41	14.0%

Source: Messer Group.

In 2006, the combined sales of all businesses under the full control of the Messer family exceeded €1 billion for the first time in company history (Lesczenski [2007]).

The deal with Air Liquide stipulated a non-compete clause for the Messer Group in Germany, the U.K., and the U.S. for three years until May 2007. Furthermore, the Messer Group was not allowed to use the Messer brand name in these markets for yet another year. After these competitive restrictions elapsed in May 2008, the Messer Group returned to Germany and invested in new German plants. For Stefan Messer, it was a personal success to be able to rebuild operations in Germany, the home country of the family firm.

The turbulence in international capital markets as well as the global economic crisis had an impact on the Messer Group. In the first half of 2009, overall sales decreased for the first time, though in some regions, such as China, the company was able to generate a substantial sales growth despite unfavorable circumstances. In fact, in 2009 Messer achieved the same level of sales and profit as in the booming year 2008. After difficult years under Hoechst (later Aventis) ownership and challenging years of restructuring the business with support of private equity, Stefan Messer evolved to become the new entrepreneurial head of the German family firm, now 112 years old and successfully developing.

CONCLUSION

While value creation in buyouts of publicly held companies can be explained through changes in corporate governance due to more concentrated ownership, it is unclear whether buyouts of privately held family firms also offer opportunities for governance engineering. Ownership is usually equally concentrated prior to the transaction. Financial economics offer arguments both for the existence of benefits as well as costs in such transitions in privately held family firms:

- On the one hand, the natural alignment of the interests of owners and managers in family-led businesses, the personal involvement of family managers, and special family relations between decision makers can lead to effective governance. The high transaction costs and the limited time frame over which private equity funds have to work may prove detrimental.

- On the other hand, problems of private benefit appropriation, self-control, and adverse selection of professionals working in the family firm may imply governance costs from family ownership, which private equity groups can address.

The aim of this article was to analyze how private equity firms may influence governance benefits and/or governance costs in family firms. Furthermore, we set out to investigate the impact of differences in the investment horizons of private equity firms that are focused on realizing a profitable exit in the medium term and the family, which wants to keep long-term control over the business.

Our examination of the Messer Griesheim case as an example of a private equity investment in a family business provides insights on how the private equity firms influenced different business areas. The private equity firms had an influence on operational as well as strategic decisions through their presence on the supervisory board and in the shareholder committee. The deal introduced a management incentive program in the form of an exit bonus, which by German standards of that time was innovative and acted as a long-term incentive for the management. The restructuring efforts and divestitures undertaken under private equity ownership implied reductions in employment. However, they were less severe than envisioned prior to the buyout. Here, differences in the time horizons and priorities of the private equity firm and the family became apparent. The case shows that even though ownership was equally concentrated before and after the buyout, the deal initiated important changes in how the management was supported, monitored, and incentivized, leading to operational improvements in the firm.

Stefan Messer, representing the family, gained influence in the firm through his operational role and his presence in all strategic meetings post-buyout. While we cannot generalize from one case, our evidence reveals that the family owner developed to become more entrepreneurial due to his increased influence on firm strategy under the new corporate governance system and the opportunity to experience a management style focused on restructuring under private equity ownership. The relevance of the Messer Griesheim case goes beyond understanding the circumstances of one firm, as we shed light on how contractual arrangements as well as governance engineering can lay the groundwork for a family to regain control after the exit of private equity firms.

Overall, this article may help to explain how private equity firms create value through changes in corporate governance that are not rooted in more concentrated ownership. However, we only offered detailed insights into one particular case, and an unbiased evaluation of the consequences of private equity investments in family firms is still required to support and generalize our findings. There remains a need for further research based on a large sample in order to find general mechanisms of value creation in private-to-private transactions in the context of family firms.

ENDNOTE

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Barbarians at the Store?
Private Equity, Products, and Consumers*

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Abstract

We investigate the effects of private equity on product markets using price and sales data for an extensive number of consumer products. Following a buyout, target firms increase sales 50% more than matched control firms. Price increases—roughly 1% on existing products—do not drive this growth. The launch of new products and geographic expansion do. Competitors lose shelf space and marginally raise prices. Results for public vs. private targets, during and after the financial crisis, and in industries that vary in structure suggest private equity tailors strategies to the environment, eases financial constraints, and provides expertise to manage growth.

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I. Introduction

Private equity firms have raised more than $3 trillion in capital over the last five years, exercising a growing influence on the day-to-day purchases of millions of consumers.[1] Private equity (PE) firms have a simple goal: acquire businesses, and exit with gains. How they achieve gains, however, is an open question. Do PE firms simply transfer wealth from stakeholders,[2] or do they create value by improving firm operations? Studies show that PE firms improve total factor productivity (Davis et al., 2014a) and managerial practices (Bloom et al., 2015, Bernstein and Sheen, 2016), focus patenting activity (Lerner et al., 2011), increase employee safety (Cohn et al., 2016), and reduce agency problems (Edgerton, 2012).

Firms, however, exist to sell goods and services. Despite this, the effects of private equity on target firm products has received little academic attention. Thus in this paper, we use micro-level retail scanner data to study private equity's strategies in the consumer product market. We answer the following basic questions: When PE acquires a manufacturer of consumer goods, what happens to product prices and sales? Does the product mix change? And does product availability expand or contract? Answering these questions helps reveal whether and how PE firms attempt to create wealth. It also provides insight into how private equity impacts consumers, a topic under constant scrutiny by policy makers and the media. We find that, in the years following the buyout, target firms increase sales by 50% on average compared to matched control firms. Price increases do not drive this sales growth.

[1]Bain and Company (2018) reports that private equity firms have raised $701 billion globally in 2017, reaching a total level of over $3 trillion in the 2012-2017 period. A series of articles published by the New York Times, titled "This is Your Life, Brought to You by Private Equity" 12/24/16, highlights the extensive influence of private equity on consumers.

[2]For example, by exploiting tax rules, extracting dividends, charging monopoly prices, or repackaging assets.

The launch of new products and geographic expansion do.

We compile monthly store-level prices and unit sales for nearly two million unique consumer products sold in nearly 43,000 locations in the United States between 2006 and 2016. This sample covers over 50% of grocery and drug store sales and over 30% of mass merchandiser sales in the United States. The data is remarkably detailed. For example, we can see that in the first week of August 2008, twenty-four cans of Del Monte French style green beans were sold in a particular store in Chicago at an average price of $1.15 per can. We link each product to its parent company. Private equity firms acquired 236 of these companies over our sample period. Most of these firms (222) were privately owned at the time of the acquisition. These companies are the manufacturers of goods sold within retailers; we do not study buyouts of retail chains themselves as, for example, in Chevalier (1995a) and Chevalier (1995b).

We test for changes in product prices and sales, innovation, and availability after a PE buyout. Specifically, we first match each private equity target with a similar counterfactual at the time of the private equity event. We go beyond the firm-level match commonly used in the literature; the granularity of our data allows us to compare product lines and even products within the same store. Each of these different treatment-control pairs represents a cohort. We stack cohort-level observations and run a generalized difference-in-differences estimation.

We begin by documenting that in the five years post-buyout private equity targets increase revenues by 50% on average compared to matched control firms. Price increases do not drive this growth. The average price of the products in a firm's product line increases by about 5% relative to competitors. Further, this increase is primarily a composition effect from either the introduction of new products or expansion into richer areas, as the price of an existing product in a particular store increases by only about 1% relative to its direct competitors sharing shelf space.

Volume growth, therefore, drives revenue growth. How do firms increase units sold? First, PE targets increase the number of products offered by 11% more than matched untreated firms by introducing more new products. Second, PE firms also innovate more into new consumer categories, such as a green bean seller branching to cauliflower. Finally, PE products expand to new stores (+25%), retail chains (+10%), and zip codes (+14%).

Firms that compete with PE targets are affected by the deals. They marginally increase prices following the buyout—less than half of one percent. This evidence is consistent with typical oligopoly models of rivals' behavior when one firm raises prices (e.g., Hotelling, 1929). Competing firms' product variety falls slightly, likely crowded out by the new offerings from PE firms given finite shelf space.

How do private equity firms enable this growth? Why weren't target firms undertaking these actions on their own? To address these questions, we further investigate our results by target firm type, time period, and industry (product category) structure. First, we study the effects of PE for public vs. private firms. PE firms achieve high growth, innovation, and geographic expansion only in private targets. In contrast, we find that public targets raise prices, reducing sales for existing products. This evidence is consistent with PE firms providing access to capital or managerial expertise for private firms (Boucly et al., 2011, Bloom et al., 2015) and taming agency costs for public firms (Jensen, 1986). Second, we examine PE deals separately during and after the late-2000s financial crisis. PE firms achieve growth in both periods and adjusts prices to economic conditions more than non-PE firms. Third, we find PE buyout targets introduce more products in categories that are more fragmented, and achieve higher growth in product categories where they have a stronger market share and in categories that are popular among high-income consumers. Last, we document that PE firms alter target company strategy by increasing corporate acquisitions and advertising expenses. Overall, this evidence suggests that PE achieves growth by pulling several operational levers: by strategically adjusting prices to economic conditions, by focusing innovation and geographic expansion in more appealing product categories, and by promoting investments.

A caveat in interpreting our results is that we cannot unambiguously conclude that private equity firms cause target firms to increase sales, product innovation, and geographic expansion, as "private equity treatment" is not randomly assigned. The standard approach used in the literature to deal with this endogeneity concern is to match treated firms with similar (in the pre-buyout period) untreated firms. A problem with this approach is that firms might differ across a multitude of characteristics, leading to poor matches. Industry codes are coarse; firms in the same broad industry are unlikely to have the same product lineup. The granularity of our data helps mitigate this concern: we match not only similar firms, but also similar product categories and products themselves, using store shelf neighbors as counterfactuals. For example, we compare a can of green beans sold by a target firm with a can of green beans sold by an untreated firm in the same store. This specificity curtails—though does not eliminate—the role that unobservables could play in explaining our results.

Our work contributes to the empirical literature on the effects of private equity on corporate performance and behavior. Chevalier (1995a) and Chevalier (1995b) study the pricing and market expansion behavior of supermarket leveraged buyouts and their competitors. These papers differ from ours along several dimensions. We do not study retail chains themselves; instead, our buyouts are of firms that manufacture products that are then sold within supermarkets, drug stores, and mass merchandisers. Our price and sales data are thus at the individual product level, not overall store level, and we are able to investigate product innovation and geographic expansion. Moreover, we provide evidence on PE deals completed in the 2000s in contrast to the supermarket deals of the 1980s, an important comparison given the evidence that PE strategies have evolved significantly over the past few decades (see, e.g Guo et al., 2011). Our results that PE firms spur growth complement the evidence in Boucly et al. (2011) that French target firms increase profitability, sales, debt issuance, and capital expenditures compared to control firms. Our evidence that PE deals do not seem to significantly harm consumers nicely dovetails with findings that private equity could affect firm stakeholders by, for example, reducing work-related injuries (Cohn et al., 2016), increasing employee technological human capital (Agrawal and Tambe, 2016), improving sanitation and food-safety (Bernstein and Sheen, 2016), and impacting student outcomes in for-profit higher education (Eaton et al., 2019). Last, other studies have documented that PE creates value for its investors (Robinson and Sensoy, 2013 and Harris et al., 2014). Our results on the mechanisms (section VII) shed light on how PE firms might create this value: by promoting investments and by tailoring their strategies to private vs. public target firms, to economic conditions, and to industry (product category) structures.

II. Hypotheses Development

What happens in the product market after private equity buyouts? A popular view in the media is that businesses suffer under PE ownership. To generate cash flows, "you can expand the company, but more likely you slash costs, close divisions, cut staff, curtail marketing, eliminate research and development and more. In other words, cutting to the bone."[3] If PE

[3] *Wall Street Journal*, 3/29/15.

firms follow such a strategy, target companies could trim product offerings and raise prices to boost short term cash flow.[4] Scaling back investment could also be optimal for some target firms. Agency theory (e.g., Jensen, 1986) predicts that managers might engage in empire building. The added leverage and incentive alignment typical in PE buyouts might, therefore, impose discipline. If lower prices stem from an overinvestment in market share, then private equity firms could raise prices. Analogously, if firms are selling too many products in too many places, private equity could prune product offerings and distribution. Last, liquidity constraints imposed by increased leverage could also lead to higher prices (Chevalier and Scharfstein, 1996).

An alternative and more recent stance on the role of private equity would predict, instead, post-buyout product market expansion. Surveying PE firms, Gompers et al. (2016) find that in target firms revenue growth is pursued more aggressively than cost cutting. Analyzing data from 839 French PE deals, Boucly et al. (2011) indeed find that buyouts appear to infuse capital and relax credit constraints, as target firms grow faster and become more profitable than their peers, particularly when capital might be most dear ex ante. Bloom et al. (2015) find that private equity may bring better management practices to target firms. If these mechanisms are at play, we expect to see growth. Implications for pricing, however, are unclear. New or better products might be more expensive. On the contrary, leaner manufacturing or more skillful bargaining with retailers could lead to lower prices.

These contrasting predictions can co-exist in the cross-section of target firms. For example, agency theories might better describe dynamics in more mature industries and for publicly traded firms (Jensen, 1986). Capital constraints may be more relevant for private or small firms (Farre-Mensa and Ljungqvist, 2016). Bloom et al. (2015) find that private

[4] Kosman (2009) devotes an entire chapter to "Lifting Prices" in his book "The Buyout of America."

firms are more in need of managerial expertise than public firms. Davis et al. (2014a) document employment growth following private firm buyouts but contraction after public deals. Boucly et al. (2011) find stronger growth results for private-to-private buyouts. To test these different cross-sectional predictions, we repeat our main analyses separately for private and public target firms (section VII.B).

We also test if the effects of PE vary with economic conditions (section VII.C). Bernstein et al. (2019) study UK PE-backed companies during the financial crisis. Compared to control firms, PE targets decreased investments less and increased market share more. They attribute this findings to the ability of PE firms to raise capital or to provide strategic and operational guidance in difficult times.

How do competitors react to the entry of PE firms? Chevalier (1995b) finds that, following the LBO of a supermarket chain, prices in a local market rise if rival firms are also highly leveraged. Prices, instead, decline in local markets where competitors have low leverage and are concentrated. Similarly, Goolsbee and Syverson (2008) study the airline industry and find that incumbents cut fares when facing potential entry. Gerardi and Shapiro (2009) find that competition has a negative effect on price dispersion in the airline industry. We investigate competitor reaction in prices and product innovation in section VI.

III. Data Description

A. Nielsen Retail Scanner Data

We combine private equity buyouts and retail store scanner data in our analyses. Product market data comes from the Nielsen Retail Scanner database from the Kilts Center for Marketing - Chicago Booth. This database tracks all purchases made in the United States from January 2006 to December 2016 at 42,928 stores from 91 U.S. retail chains (see Table II). Almost all major chains are present in our data, but their identities are anonymized. The largest chain in the sample has 10,129 stores. The sample covers roughly 50% of total U.S. grocery and drug store sales and 30% of U.S. mass merchandiser sales. The stores are spread across the United States, covering 98% of media designated market areas (DMAs). Nielsen tracks weekly average prices and units sold at each store for close to two million unique consumer products.

The Nielsen data identifies products by name and Universal Product Code (UPC). The data are very specific. For example, Table I lists all products available under the category "Canned Green Beans" in a specific grocery store in Austin, Texas, in December 2007. Seventeen green bean products are sold in the store differing in brand (e.g. Del Monte, General Mills), type (e.g. organic, French style), and size (e.g. 8oz, 14.5oz). We exclude UPCs that do not identify unique products (e.g., private label products, products temporarily sold in different size). For each product, each week, in each store, we know the average price, units sold, and total revenue. Table II provides summary statistics. The average product is sold in 571 stores and an average store carries about 19,000 products. Nielsen groups items into mutually exclusive groups such as "Vegetables-Beans-Green-Canned," "Fabric Softeners-Liquid," or "Vacuum and Carpet Cleaner Appliance." These are called "product categories" and should be thought of as highly-specific industry definitions. Panel B of Table II shows that there are 1,127 different product categories, and each one includes on average 21 items belonging to four firms.

We match each UPC to its parent firm. The GS1 organization oversees the management of UPCs. Manufacturers buy from GS1 the usage right to a UPC company prefix that corresponds to the first six to nine digits of the UPCs of its products. Firms are required to disclose their name and address when buying a company prefix. Using the GS1 Data Hub, we exactly match 82% of the UPCs in the data to a GS1 company prefix. We map the remaining UPCs to companies by assuming that UPCs in the same firm share the first eight digits. In Panel C of Table II, we present the characteristics of the sample's over 52,000 firms. The average firm sells 10.2 products in 2.9 product categories through nine retail chains spanning 1,346 stores.

The data allows us to precisely define competitors, market structure, and plausible counterfactuals. We aggregate the data at the monthly level to make the dataset more manageable and to smooth consumption peaks (e.g. Black Friday).[5] The monthly frequency allows us to accurately capture when firms introduce new products, discontinue products, and expand into new markets. Despite the richness of the data, we miss two important pieces of information. First, we observe the prices paid by consumers–the sum of the wholesale price and retailer markup. We cannot say with certainty which of these two price components drives our results. That said, whether PE firms are changing wholesale prices or influencing retailers to change margins, the ultimate effect on the consumer is the same. Second, we do not observe manufacturing costs and markups and, thus, we cannot draw direct conclusions about the profitability or optimality of firms' decisions before or after the private equity deal.

B. Private Equity data

We obtain data on private equity deals from Capital IQ and Preqin. From Capital IQ, we select all "closed," North American, majority stake transactions classified as "Leveraged Buyout", "Management Buyout", "Secondary Buyout", or "Going Private Transaction". We

[5]The Nielsen data records weekly sales from Sunday morning to Saturday night. If the beginning or the end of the month is not on a Sunday, we assign a pro-rata of the weekly units sold and sales to each corresponding month.

do not include venture capital deals. From Preqin, we collect all North American private equity portfolio companies. We keep only deals closed between 2007 and 2015 as we require at least one year of product market data before and after each deal, and the Nielsen data spans 2006-2016. To link PE targets with firms in the Nielsen/GS1 database, we begin with fuzzy match algorithms based on company name and state, and then manually check each deal to make sure the firms are correctly identified. We also buttress this process with a "top-down" approach, collecting the largest PE deals from Capital IQ and manually checking if any belong in the sample. This makes sure we do not miss any large, obvious deals[6]. We end up with 236 private equity deals, of which 222 are buyouts of private firms and 14 are public.

To address the representativeness of our sample, we compare in the appendix our deals with the universe of PE deals in Capital IQ during our sample period and with the PE deals in consumer products (see Appendix Table A1). We find that our deals appear to be larger in size and involve older firms compared to the average PE deal in Capital IQ and in consumer goods. We provide more details on this comparison in the Appendix section II.

Figure 1 shows the number of buyouts over time. Deals are more frequent during the private equity boom of the mid-2000s to 2007 and less frequent during the financial crisis starting in 2008. Online appendix Table A3 lists the most frequent PE buyers in our sample, identified using the category *Buyers* in Capital IQ and *Investors* in Preqin. Table A4 lists the private equity targets with the highest average sales in our sample. The three largest are Del Monte, The Nature's Bounty, and the Pabst Brewing Company. These are not necessarily the targets with the greatest deal value, just those with greatest presence in the consumer product categories we analyze.

[6]Expanded details on how the sample is formed are in the online appendix, section I.

IV. Empirical Methodology

A. Research Design

Private equity firms do not randomly select companies. As shown in Table A5 in the online appendix, they are more likely to target product categories that are less concentrated and more popular among high-income consumers, firms that are larger, and products that are cheaper than competitors.[7] While a comprehensive study of the characteristics of firms and products taken over by private equity is beyond the scope of this study, we use a matching strategy that controls for relevant observable trends. An advantage of our setting is that our detailed data allows us to match each treated unit with a very similar counterfactual.

Our matching strategy does not completely solve endogeneity problems. There are two outstanding concerns. First, while we control for pre-deal observable characteristics, there could be unobserved characteristics that explain differences in post-event outcomes. Second, even if we could match on all pre-deal characteristics, a firm could still be targeted because it is expected to change in the future. We find evidence that alleviates the first concern: after the match, treated and control groups are similar also on observable variables that we do not use in the matching procedure (see Table A6). The granularity of the data helps with the second concern. We are able to compare, for example, two cans of green beans on the same store shelf. While it is possible that one brand has a different future trajectory than another (e.g., buzz from an advertising campaign), matching with such specificity certainly reduces the scope of variation (e.g., we control for a sudden increase in green bean popularity).

An additional concern related to our empirical strategy is that both the treated firms/ product categories/ products and their control units could react to the treatment (the PE

[7]We provide more details on how we identify category concentration and popularity among high-income consumers in section VII.D.

deal). In other words, if competitors react to the entry of PE, then our comparison of treated vs. control units does not capture the full effects of PE entry. To address this concern, in section VI we examine whether competitors change behavior when facing a PE competitor. For example, we compare the prices of the same competitor product in stores where it faces PE vs. stores in which it does not.

B. Matching Procedure

We match each private equity acquired firm, firm-product line, or product with a close competitor chosen based on observable characteristics at the time of the private equity deal. We define each resulting treated-control pair as a cohort and then stack all cohorts. Finally, we run a difference-in-differences regression specification on this stack of cohorts.

We match each of the 236 treated firms and 1,835 treated firm-categories with a similar counterfactual based on four variables measured at the time of the private equity deal: monthly sales, number of unique UPCs sold, number of stores in which they sell, and growth in monthly sales. The first three variables are measured in the most recent pre-buyout month, while growth in sales is computed from 12 months before the deal to the most recent pre-buyout month. In the firm-level analyses, 220 control firms are matched to one treated firm, six to two treated firms, and one to four treated firms.

We also perform analyses at the individual product level. For each product-store—e.g., Del Monte 14.5 oz. French Style Green Beans sold in a particular store in Austin, Texas—we select a matched product in that same store, in the same product category at the time of the private equity deal. We choose the particular green bean item (UPC) with the closest distance based on average price and units sold during the most recent month pre-buyout, and growth in price and units sold from 12 months ago to the most recent month pre-buyout.

We match with replacement each treated unit with the closest control using the Abadie and Imbens (2006) distance metric[8]. Both treated and control units must be in the sample for at least one year before and one year after the buyout event. In the Online Appendix, we investigate if treated or control firms are more likely to disappear post-buyout. We thus focus on deals from 2008 to 2011, so that we have the full two years before and five years after the buyout. Figure A1 shows that the drop-out rate of PE targets and matched controls is very low. Furthermore, PE targets are less likely to drop compared to control firms, with this difference becoming larger especially in the years three to five post-deal.[9]

The matched control product categories and control individual products become the object of our analyses when we investigate the response of competitors in section VI.

C. Econometric Specification

Our main empirical analysis employs a stacked cohort generalized difference-in-differences strategy. Essentially, we take the difference in outcome for each treated unit i (firm, product-category, or product) after the private equity deal relative to before and compare it with the difference in outcome of its matched control unit within the same cohort c.

$$y_{i,c,t} = \beta(d_{i,c} \times p_{t,c}) + \alpha_{i,c} + \delta_{t,c} + u_{i,c,t} \tag{1}$$

All regressions are estimated from 24 months before the event to 60 months afterwards. We choose the pre-window to have enough periods to test the parallel pre-trend assumption

[8]For each of the four matching variables, we compute the difference between treated and control and then divide this difference by the variable's standard deviation in order to normalize the scale. We then compute the overall distance by summing the four scaled differences.

[9]To the extent that PE targets that are more successful than their control firms are dropped from our analyses because their match disappears, then this evidence would suggest that we are potentially understating the effects of PE, especially in the three to five years post-buyout.

and the post-window to allow enough time for any PE effects to emerge. The unit-cohort fixed effect $\alpha_{i,c}$ ensures that we compare the outcome within the same unit in the period before vs. after the deal. The time-cohort fixed effect $\delta_{t,c}$ ensures that the treatment unit is compared only with the matched control at each point in time. $d_{i,c}$ is a dummy variable identifying treated units. $p_{t,c}$ is a dummy variable equal to one if the time period is after the private equity buyout. The coefficient β represents the diff-in-diff effect of the private equity deal on the outcome variable relative to a matched counterfactual. The standard errors are double-clustered at the firm and month level to adjust for heteroskedasticity, serial correlation, and cross-sectional correlation in the error term (Bertrand et al., 2004).

To test the parallel pre-trend assumption and learn how quickly private equity firms implement change, we also estimate the impact of private equity month-by-month, using the equation below:

$$y_{i,c,t} = \sum_{k=-24}^{60} \beta_k (d_{i,c} \times \lambda_{t,k,c}) + \alpha_{i,c} + \delta_{t,c} + u_{i,c,t} \tag{2}$$

$\lambda_{t,k,c}$ is a dummy equal to one if time t is equal to k and zero otherwise. Standard errors are also double clustered at the firm and month level. Given the large number of fixed effects and observations, all regressions in the paper are estimated using the fixed point iteration procedure implemented by Correia (2014).

V. The Effect of Private Equity on Target Firms

A. Sales and Prices

What happens to the sales and pricing of goods sold by consumer products firms acquired by private equity? We start by analyzing these variables at the firm level. Each target firm is matched to an untreated firm as described in section IV.B. Panel A of Table III shows estimated coefficients of regressions of each firm's log sales, sales-weighted average log price, and log units sold on *After*, a dummy variable that equals one for firm-month observations after the private equity deal close date for target firms. We find that revenues relative to a matched firm increase dramatically. The coefficient on *After* is 0.406, translating to a 50% increase in sales in the years following the deal[10]. This result is consistent with papers that document growth following PE buyouts (e.g. Boucly et al., 2011). This growth is primarily driven by a 43% increase in units sold. The average price per firm increases by 5%. We compute average product prices by dividing total revenues by units sold for each firm in each month. This is a very rough price measure—it blends all categories, products, and stores into a single number for each firm and will thus be influenced heavily by composition effects. While it could capture well overall trends in pricing for single category firms, the average price per firm is not likely informative for firms that sell both cheap and expensive items.

To better understand price dynamics and what ultimately drives changes in sales and units, we begin "peeling the onion". We break the unit of analysis down from the firm to the firm-category. In other words, now instead of treating Del Monte as a single entity, we analyze separately their green bean, canned peach, and spaghetti sauce businesses. This sharpens the analysis in two ways. First, it increases the quality of the match, as individual

[10]Throughout the text, we exponentiate the coefficients for regressions with logged dependent variables when reporting magnitudes.

product lines can be matched more precisely than entire firms; Del Monte and General Mills do not participate in exactly the same product categories. Second, it allows us to separate changes in existing product categories from changes in the category mix. The 236 PE treated firms in our sample range from operating in a single Nielsen-defined product category (e.g., Noosa Yoghurt, LLC only sells products in the ”Yogurt-Refrigerated” category in our sample) up to 101 categories for American Roland Food Corp.

In Panel B of Table III, we regress the logs of nationwide revenues, units sold, and average prices for a firm in a particular product category on the *After* variable. This breakdown at the product category level mimics the firm-level results. With the added precision of only comparing product categories, not entire firms, we find that average prices of private equity-owned firms increase by 3% relative to matched firms. Sales increase by 23% and units sold increase by 18%. All are statistically significant at 1%. These point estimates for units and revenues at the category level are a little smaller than at the firm level. This could be a sign that either PE targets' larger categories are growing the most, or that they are expanding to new categories. We explore this in the next section.

Figure 2 plots the trend in log sales and average log prices over time with a 90% confidence interval. The graphs show no obvious pre-trend in sales or price before the PE buyout. This provides comfort that we are comparing similar firms and firm-categories. After the event, at the firm and product-category level, there is a gradual increase in both sales and prices over the next three to five years.

After a PE buyout, we find small price increases and large unit sold increases at the category level. Multiple paths can generate these results; distinguishing between them is important for understanding PE growth strategies. The relative increase in average nationwide category-level prices could be because existing products have been marked up. Alternatively, the composition of goods sold within a category might have shifted towards more expensive varieties (e.g., premium organic products), or the firm might be growing share in markets or retailers that simply charge more (e.g., New York City).

Similarly, there are different paths to the increase in firm-category units sold; PE targets could be gaining share within a store or expanding to new stores.

To peel the onion further, we zoom in to the individual product and store level. Instead of comparing a PE target and control firm's green bean sales nationally, we now compare a PE target's 16 ounce can of Italian-style green beans in a particular supermarket in Austin, Texas with a non-PE can of Italian-style green beans in the same store. In other words, we use literal store shelf neighbors as counterfactuals. This allows us to tease apart changes to existing products from composition and location effects.

The unit of observation is a specific UPC in a specific store in a month. A cohort is defined as a treated-untreated pair of products within the same store and category. We regress the logs of sales, average price, and units on *After*, product-cohort fixed effects, and cohort-time fixed effects.

In Panel C of Table III, we find a 1% increase in the price post-PE for a given treated product relative to a competing product in the same store over the next five years. This 1% increase for existing products implies that the average category price increase of 3% shown in Panel B is mostly due to a composition effect: adding or shifting consumer tastes to products that are more expensive or expanding to locations with higher prevailing prices or cost of living. Results on revenues and units sold differ substantially from the results in Panels A and B; both *After* coefficients are essentially zero. This means that existing products are not gaining share within their current stores. Some combination of selling new products or selling in new places must, therefore, drive unit and revenue increases at the firm and category level. We explore product innovation and geographic availability next.

B. *New Product Development*

Do private equity firms change the pace of new product introduction? Do they expand into new industries? Lerner et al. (2011) and Amess et al. (2015) find that after a leveraged buyout, firms increase their patenting activity and produce more influential patents, suggesting either a relaxation of financial constraints or reduced agency problems. While patents capture the early stages of innovation, our data allows us to study the end result with the release of new products.

Mimicking the price and sales analyses, we first answer these questions at the overall firm level. We match each of the 236 firms acquired by private equity with a non-private equity-owned firm with the closest sales, number of products, number of stores, and growth in sales. The unit of analysis is a firm-month. Table IV illustrates the effect of PE on product innovation. *Number of Products* is the log of the number of unique UPCs a firm sells nationwide in month t. *New products* is the number of products introduced by the firm in month t. A new product is a UPC that appears for the first time in the Nielsen database. *Discontinued Products* is the number of products dropped by the firm in month t, meaning the UPC never reappears again in the sample. To better ensure that we accurately measure introductions and discontinuations in product lines, we exclude from our analyses products that appear in the first six months of a firm's appearance in our sample. Analogously, we exclude products that disappear in the last six months of a firm's presence in our data. The reason for this is if a product (UPC) is sold in November 2016, but not December 2016 (the end of our sample), it may not have been permanently discontinued. It is possible the product simply did not sell any units in December but returned to stores later in 2017. A six-month buffer on both ends gives us more confidence that a product is truly discontinued or new. Last, *Number of Categories* is the log of the number of categories in which a firm sells products at time *t*. Nielsen defines 1,127 total categories.

In Panel A of Table IV we compare the product portfolios of PE and non-PE firms. Column 1 shows that, relative to matched firms, PE-treated firms expand their number of distinct UPCs by 11% after the deal. How is this achieved? Columns 2 and 3 show greater churn—more frequent introduction and discontinuation of products. However, the coefficient on *New Products* is significant and more than double the coefficient on *Discontinued Products*, resulting in increased product variety. We also find treated firms more likely to expand into new product categories. In column 4, the coefficient on $After$ is 5% and it is statistically significant. It appears that PE targets both create new varieties in existing product categories and enter into new ones.

To confirm this interpretation, in Panel B we run analyses at the firm-category level. We compare each treated firm-category with the same category of an untreated competitor. Within a category, PE controlled firms increase their product portfolio by 2.5% relative to their pre-PE ownership days. Both new product introductions and discontinuations increase at a faster rate. Given that existing products do not decline in sales (see Table III), these new products do not cannibalize existing goods. Figure 3 shows that product innovation happens gradually over the years following the PE buyout.

Overall, private equity firms appear to engage in more creative destruction within their product lines, with introductions of new products outpacing discontinuations, resulting in greater product variety. We also find evidence of expansion into new product categories. Since average category-level prices increase for treated firms, the new products must be slightly more expensive. The higher number of products for sale helps explain why overall units sold grow for treated firms despite no change in existing product units sold at the store level.

C. *Product Availability*

Private equity targets increase units sold and revenues more than competitors. In the previous section, we show that introduction of new products contributes to this result. In addition, PE may facilitate geographic expansion.

We report results at the firm-level in Table V, panel A, and at the firm-category-level in panel B. *After* is an indicator variable indicating a post-buyout firm-month or firm-category-month for target firms. Column 1 shows that treated firms increase the number of physical stores in which they sell their products by 25% after the deal, relative to matched untreated firms. This result can happen by selling to more stores within the same retail chain or by entering new retail chains. Column 2 shows that PE firms increase the number of retail chains by 10% post-buyout. How widespread geographically is this expansion? Column 3 shows that PE firms expand to 14% more 3-digit ZIP codes. We obtain similar unreported results for counties, DMAs, and states (see figure A2 in the Appendix for a graphical illustration of these results). The results at the firm-category level (in Panel B) are similar. Figure 4 shows that this expansion occurs steadily over the years following the deal.

In the Appendix Table A7, we investigate more formally the timing of the PE effects in all our major analyses, by interacting our treatment variable with each of the four years following the buyout. We find that most of the results are significant starting from the first year post-buyout and that the effects of PE linearly increase over time.

VI. Competitor Response

The results thus far show what happens to private equity treated goods relative to matched competitors. Competitors, however, do not necessarily stand still. In this section, we investigate how competition responds to PE entry. Combined with the relative changes documented in section V, these results paint a more comprehensive picture of the effects of PE on products and, ultimately, consumers.

A. Competitor Response: Prices

Prices on existing products taken over by PE increase by about 1% relative to matched products (Table III, Panel C). This result is consistent with private equity firms keeping prices constant while competitors lower prices to run highly leveraged targets out of business. Alternatively, the price effects could be bigger if competitors also increase prices. It is ultimately an empirical question whether rivals match PE price increase behavior—as typical oligopoly models would predict—or seize an opportunity for predation.

To identify the pricing response of competitors to private equity entry, we exploit geographic variation in a given competitor's exposure to a PE buyout. As an example, assume that Del Monte, a private equity takeover target, sells green beans in store A but not in store B. General Mills, who is not private equity owned, sells green beans in both stores. We compare the price response of General Mills in store A, which faces PE competition, to its response in store B, which does not. We attribute a differential price response following the buyout to the PE deal. The identifying assumption is that absent the deal, the price of this particular green bean product of General Mills would have moved similarly in both stores.

The control firms in previous regressions now become the objects of interest. We first extract from the same-store analysis of Table III the same non-PE products and store locations that face a PE competitor. We then identify the stores where these non-PE products are sold absent the PE competitor. Given that each product is sold in thousands of stores, we randomly select ten stores, and among these we select the closest match in terms of price level and growth to the non-PE product which does face a PE rival. These two product-stores form a cohort.

In Table VI, *After* is an indicator variable equal to one for non-PE products after their competitors' PE deals, in stores where that newly PE-owned product is sold. As in the previous same-store product analysis, we include product-cohort fixed effects and time-cohort fixed effects. In Panel A, Column 1, the coefficient on *After* is 0.4% and significant, suggesting that private equity leads direct store competitors to marginally raise prices.

A problem for our identifying assumption would be if pricing trends in stores with PE competition are systematically different from trends in stores without PE. For example, PE products could be sold in chains or in geographic areas experiencing differential price changes. We address these possibilities in Column 2 and 3. In Column 2, we require that all eleven stores (ten which sell only the non-PE product, one which also sells the PE entrant) from which the product-store cohorts are drawn are part of the same retail chain. In Column 3 we require that all the stores used to define the cohorts are in the same DMA. The coefficients on *After* in these regressions are 0.4% and 0.3% and still significant. Private equity entry thus leads competitors to marginally raise prices in stores where they directly compete[11].

Figure 5 plots the price response over time from Column 1. Price responses for Columns 2 and 3 are in the Appendix, Figure A3. Interestingly, the price change happens very quickly.

[11] Price changes could be driven by the manufacturer (General Mills in our example) or the individual retail store manager; Levy et al. (1997) notes that both impact final retail pricing. Whether the manufacturer or the retailer is responsible for higher competitor prices when PE is present, however, it is still ultimately the PE buyout that instigated the change.

Added to the relative price increase of approximately 1% for PE-owned goods, the results in panel A suggest the overall PE price increase experienced by consumers could be 1.3 to 1.4%.

B. Competitor Response: Product Mix and Availability

Private equity targets boost product introduction and thus increase variety. How do competitors respond? To address this question we analyze if, after the buyout, there is a change in the number of products these competitors sell in stores where they compete with the PE firms vs. stores where they do not. As an illustrative example, General Mills, which is not PE-owned, sells 10 varieties of green beans in stores A and B prior to the PE buyout of its competitor, Del Monte. Del Monte sells green beans in store A but not store B. What happens to General Mills' green bean variety in store A vs. store B after the PE deal? Our identifying assumption is that any difference in General Mills' store A variety is due to the presence of private equity. The unit of analysis is now a firm's entire product category within a store, not a specific product, since we want to count the number of products in the category. For each store in which a non-PE firm competes with a PE in a given category, we select ten random stores where the non-PE firm does not compete with PE. We form cohorts using all eleven firm-category stores, one treated by a PE entrant and ten untreated. We use all ten control stores because it is not obvious how to identify the best match and because we want to reduce the noise in the measurement of product variety using one single store.

We present these results in Table VI, Panel B. In Column 1, we find that a PE-buyout competitor reduces the number of product offerings by 1.5%. We find similar results in Column 2 where all 11 stores in each cohort are from the same retail chain, and Column 3 where all cohort members are from the same DMA. Unlike with prices, where competitors respond (marginally) in the same direction as their PE shelf neighbors, product variety responds in the opposite direction. Given that shelf space is finite, more aggressive PE product introduction appears to crowd out competitors.

Our findings are at odds with evidence in Chevalier (1995b) that competitors enter and expand into the LBO grocery chain's markets after the deal. There are key differences between the papers that could help explain the different results. First, Chevalier investigates retail chains, while we focus on manufacturers that sell in these chains. Second, Chevalier' sample is heavily influenced by publicly-traded firms, whereas most of our firms are private. In section VII.B, we split our analyses by public and private firms and find results for public firms at the product-store level that are more consistent with evidence in Chevalier (1995b). Last, supermarket LBOs from the 1980's were undertaken as a takeover defense[12]. Decades later, the drivers of PE deals appear starkly different (see our evidence from press releases in section VII.A).

VII. Mechanisms

Private equity deals result in marginally higher prices but significantly higher sales, primarily through aggressive introduction of new products in new locations. How do private equity firms achieve these results? Why is private equity needed? In this section we investigate the potential mechanisms in play. We start by examining cross-sectional and time-series variation in PE impact. Knowing where and when PE is most effective can provide clues to their particular skills and strategy. We study the effects of PE: i) on public versus private

[12]"The vast majority of the leveraged buyouts were not the result of unconstrained decisions by managers and shareholders. Instead, most of them were undertaken in response to unwanted takeover attempts. In fact, all four of the biggest deals (and many of the smaller ones) were undertaken to thwart the unwanted takeover attempts of the Haft family" (Chevalier, 1995b).

targets; ii) around and after the financial crisis; iii) in product categories where target firms have high vs. low market power; iv) in product categories with low vs. high barriers to entry; and v) in categories popular among high-income vs. low-income consumers. We also directly test whether buyout target firms become more acquisitive or increase advertising expenditures. Last, we examine acquisitions of firms by operating companies (i.e., traditional M&As) to test if our results are specific to PE acquisitions or occur whenever there is a change in ownership.

A. *Private Equity Deal Press Releases*

A starting point for understanding how private equity firms achieve results is to investigate their stated plans and strategies. Gompers et al. (2016) survey PE firms to understand how they might create value. In the same spirit, we collect and analyze the press release announcements for the deals in our sample. With the caveats that PE firms might strategically handle their press and likely overstate positive outcomes (e.g. growth) and downplay negative ones (e.g. layoffs), announcements can still offer insights into the range of strategies employed.

We were able to find press releases for 237 deals.[13] We categorize the stated reasons for the deals in Table VII. Reasons are not mutually exclusive. Most press releases (69%) generically mention growth; some specifically detail new product development, acquisitions, or access to distribution. Capital infusion and human capital are mentioned as well. Motivations pertaining to cost cutting and financial engineering are hardly present. There is no mention of PE as a takeover defense, as, for example, in the case of supermarket LBOs in Chevalier

[13]This 237 is out of 297 total firms. The sample here is larger than the final sample in our paper (236 firms) because we include firms here for which we do not have at least one year of data before and after the deal. We have press releases for 184 out of 236 deals in our final sample.

(1995a). Overall, the stated strategies are consistent with our growth results.

B. Public versus Private Targets

Public and private firms may be at different points in their life cycles. They could also have different needs and face different challenges. Private firms are more likely to be small and financially constrained (Farre-Mensa and Ljungqvist, 2016), while public firms are usually larger and more mature and could be more subject to agency and overinvestment problems (Jensen, 1986). In Table VIII, we run our sales and price, product innovation, and product availability tests separately on public and private PE target firms. Of the 236 treated firms, 222 are private and 14 are public. We classify as public to PE those deals where an entire public firm is sold to PE. We do not include in this category the sales of divisions of public firms. We find the impact of private equity is not the same for public and private targets.

In Panel A, the results for private targets match those for the pooled sample (Table III) at the firm level: post-PE prices increase by 5% while sales and units dramatically increase by 52% and 45%. For public firms, however, although the coefficients have the same sign, the magnitudes on sales and units increases are much smaller and not statistically significant. At the firm-category level, the results for private firms are again consistent with the full sample results—significant growth in sales and units and a 4% increase in prices. Directionally, public firm sales and units within a product category fall post-buyout relative to a control. These coefficient are not statistically significant. Public firm buyouts thus do not appear to generate the same growth results.

The within-product-store analyses for the full sample (Table III) document no change in existing product sales and units and a marginal 1% increase in prices. These results mask significant differences between public and private firms. Panel A finishes by showing that for private firms, existing products increase their sales post-buyout by 6%—a result statistically significant at the 1% level. An increase in units sold, not in price, drives this result. This is consistent with the fact that private targets spend more on advertising after the buyout (see section VII.E). Public firms, instead, raise prices by 2% and see revenues fall by 6%.

In Table IV we find that, in the full sample, product offerings expand within existing categories and into new ones after a private equity buyout. In Table VIII, Panel B, we split these innovation results by public vs. private firms. For private firms, post-buyout behavior mimics the full sample findings: the number of products grows by 11% and categories grows by 6%. There is scant evidence, however, that public firms introduce more new products or enter more new product categories relative to controls in the post-buyout period. The coefficient signs are mixed, and the results are not statistically significant.

In Panel C, we revisit geographic expansion. Private firms drive the strong growth in market penetration in the overall sample (Table V), registering higher growth rates across stores, ZIP codes, and chains relative to matched firms post-buyout. The results hold both at the firm and firm-category level. Public firms again show mixed results with no statistical significance.

This divergence in results between public and private firms suggests the existence of both growth and agency motives for private equity deals. Access to financing, managerial expertise, or business connections can help younger, private firms to expand their product lines. *The New York Times* notes that "business owners with a product to sell often dream of winning shelf space in the Wal-Marts and Targets of the world. But...it is a challenge to get shelf space in any store."[14] Public firms, in contrast, may be overinvesting in market share by charging prices that are too low. Our results of growth for private targets and

[14]"Getting Your Product Onto Retail Shelves" *The New York Times* 10/20/2010

higher prices for pubic firms are consistent with other studies. For example, Davis et al. (2014a) document that employment grows following private firm buyouts, while it declines after public deals. Boucly et al. (2011) similarly find stronger growth for private target firms. This variation in deal outcomes can also perhaps explain the negative portrayal of private equity in the media: layoffs and contraction are associated with the most visible, well-known targets.

C. *Financial Crisis*

The financial crisis of the late–2000's provides a setting to investigate how PE treated firms operate when growth is low and capital is scarce, precisely when financial resources and managerial expertise are likely to be important. In Table IX, we split the PE deals into those that close between 2007 and 2010 (during the crisis) and those that close between 2011 and 2015 (after the crisis). Consistent with the full sample results, we find in Panel A that prices, units, and sales increase for PE firm targets in the two time periods, both at the firm and at the firm-category levels. Results at the store level diverge. During the crisis, existing PE products do not gain or lose share relative to shelf neighbors, while their prices fall by 1%. Post-crisis, however, existing products gain share in a given store, even as relative prices increase by 3%. This evidence on prices suggests that PE treated firms could be more responsive to economic conditions in their price setting policies, decreasing prices during the crisis and increasing prices afterward. For product innovation in Panel B, we find that there is more product turnover for PE treated product categories. The product availability results in Panel C show that expansion to new locations is generally similar during the two periods.

There are two main takeaways from these results. First, PE-driven growth occurs in all economic conditions, including during the financial crisis when capital is scarce. This evidence is consistent with Bernstein et al. (2019). They find that during the crisis UK PE-backed companies decreased investments less and increased their market share more, compared to control firms. They attribute this evidence to the ability of PE firms to raise capital, to assist with operating problems, and to provide strategic guidance. Second, we find evidence that PE strategies change based on general economic conditions. During the crisis greater innovation and product turnover drive sales. After the crisis—in better economic times—PE targets are also able to successfully raise prices and gain market share with their existing products.

D. Industry Structure

In which industries/ product categories are PE firms more successful? We examine: i) the PE target's market power within an industry; ii) the industry's overall competitiveness and concentration; and iii) the popularity of an industry among high-income consumers. These cross-sectional tests can provide insights into how PE firms achieve growth.

Lerner et al. (2011) document that, following a buyout, new patent activity becomes more concentrated in "core innovation" areas, i.e., those where there was more patenting prior to the PE deal. Do PE targets in our sample focus their efforts analogously in product categories where they are well-established, or do they direct attention to categories where they have lower penetration and more room to grow? In table X, Panel A, we repeat our main analyses at the product-category level but split the sample by market share. For each firm, each month, we calculate its market share in each product category.[15] A firm's product category is "high market share" if it is above the median firm market share in that

[15]For example, if in a month there are 30 firms nationwide that sell green beans, we divide each firm's green bean sales by total green bean sales that month. We then categorize these 30 firms into those that are above or below the median green bean market share.

category and "low" otherwise. Growth in sales and units sold and higher average prices all happen in the product-categories where target firms have higher market share. We also find more product churn—introductions and discontinuations—and higher geographic expansion in these higher share categories.

We next analyze whether PE strategies vary based on industry concentration. Low concentration industries are traditionally considered more competitive, but they are also less likely to be dominated by a small number of firms. Do PE-treated firms expand where there are many small sellers and, possibly, lower barriers to entry? Or do they pursue growth in categories where few dominant players (e.g., Coke and Pepsi) have the lion's share of the market? For each of the 1,127 product categories, each month, we calculate the nationwide Hirfindahl-Hirschman Index value (HHI). Specifically, we compute the revenue market share by firm and then square and sum these shares, resulting in a value between zero and one. Lower HHI values correspond to lower industry concentration. We split categories into those above and below the median HHI each month, labeled respectively "high HHI" and "low HHI". In Panel B of Table X we run our main specifications separately for these two groups. Many of the results are similar across high vs. low HHI categories. A notable difference is that innovation seems to be concentrated in low HHI categories. Here, target firms introduce more new products and have greater variety.

There is growing evidence that in the past decade product introductions have favored high-income consumers (e.g., Argente and Lee, 2019 and Jaravel, 2018). Do PE-treated firms concentrate their growth efforts in product categories popular among consumers with higher income? We integrate our retail-scanner dataset with the Nielsen Consumer Panel data to address this question. The Consumer Panel Data includes a representative panel of households that provide information about their purchases and, important for our analysis, demographic information including income. We first classify each product category as high-income consumer appealing if high—that is, above median—income consumers are more likely to buy products in the category. In practice, for each product-category, we compute the average income of the consumers that buy products in the category. We define a category as "high-income" if the average income in the category is above the median income among all categories. In Panel C of Table X we separately run our main specifications for high vs low-income categories. All our results are stronger, and statistically significant, for the high-income categories.

Overall, the evidence in this section provides insight into where PE finds positive NPV projects. PE firms are more successful when target firms have higher market power and more popularity among high-income consumers. Innovation efforts seem also more pronounced in categories with lower concentration and potentially lower barriers to entry. These results nicely complement our previous evidence on PE deal selection (Table A5). PE selects categories that are less concentrated and more popular among high-income consumers. In these same categories—as shown in Table X—PE is able to achieve more innovation and higher growth.

E. Company Strategy and Investments

What specific levers do PE firms pull to spur growth? We examine two specific actions: corporate acquisitions and product advertising. In Table XI, Panel A, we investigate if private equity targets become more acquisitive after the buyout. For the years in our sample period,[16] we collect from Capital IQ all M&A transactions where the buyer is one of the 236 firms in our sample or their respective control firms. We find 651 such deals, 361 by

[16]Following our empirical specification from equation 1, we limit our data collection to two years before and five years after the deal.

target firms and the remaining 290 by control firms. Our outcome variable is the number of monthly acquisitions closed by the firm. We keep in the sample only firms that have made at least one acquisition in Capital IQ. The regression follows equation 1. We find that target firms indeed become more active buyers post-buyout, increasing the number of acquisitions per month by 0.016, which translates roughly to one additional deal over the next five years. This result holds whether targets are public or private and during or after the financial crisis. This evidence is consistent with the finding in Davis et al. (2014b) that acquisitions are a driver of growth in buyout deals. We thus investigate further if external growth drives our results. This is an important test, as the growth in sales, innovation, and geographic expansion could be simply driven by redrawing the boundary of the firm rather than by creating new products and markets. In Tables A9, A10, and A11, we repeat our main analyses for price and sales, product innovation, and product availability, excluding the top decile of the most acquisitive target firms. These results are not materially different from those using the entire sample (see Tables III, IV, V). Similar magnitudes for the effects of PE after excluding the most acquisitive firms suggests that organic, internal expansion is a substantial contributor to PE target growth.

Another channel through which firms can achieve sales growth is investing in advertising. We compile data from Ad$pender by Kantar Media, which records the dollar value of monthly advertising expenses for over 3 million brands across 18 major communication media (e.g., television, magazines, radio, newspapers). Ad$pender aggregates these brands to the firm level. The data reported by Kantar Media is sparse, with many missing observations for advertising expenditure. To smooth the data, we thus take the average monthly advertising expenditure when reported and annualize it. We keep only firm-year data where the advertising expenditure is reported for at least one month of the year. Overall we are able to identify monthly spending for 203 out of our 236 treated firms.

We then run a generalized diff-in-diff regression between the treated firms and the matched control firms where the dependent variable is the log of one plus the annualized monthly advertising expenditure. After the buyouts, treated firms increase advertising expenses by roughly 49% compared to their matched control firms. This result is similar across public and private firms, and it is stronger in the years following the financial crisis (2011 to 2015).

PE target firms are more likely to acquire other firms and ramp up advertising following the buyouts. We admittedly cannot disentangle whether PE firms provide managerial expertise or financial resources to make these activities possible. We also cannot comment on the cost-benefit trade-offs of these activities. Nonetheless, these activities are concrete examples of changes to the firm strategy implemented by PE firms.

F. Non-PE Ownership Changes

Are the changes that follow PE buyouts unique to PE buyers, or do acquisitions by operating firms have the same effect? To test if non-PE acquisitions also lead to growth, we repeat our main analyses on sales and prices, product innovation, and product availability, replacing PE buyout targets with merger targets.

We collect from Capital IQ all the target firms of M&A deals during our sample period. Mimicking our process for PE targets, we match these firms first with the GS1 database and then with the Nielsen data. Our final sample of M&A targets consist of 126 firms. For each M&A target firm, we find the closest match using the process described in section IV.B.

Appendix Table A8 mimics Tables III, IV, and V, examining what happens to targets following an acquisition by an operating firm. The results in this setting are quite different compared to PE deals. Most coefficients on the *After* variable are not significantly different from zero.

In stark contrast to PE buyouts, operational M&As do not seem to lead to growth in our sample. Some M&A deals could happen to eliminate competition. For example, Cunningham et al. (2019) find that pharmaceutical firms discontinue acquired drugs that directly compete with their existing products. One caveat in interpreting these M&A results is that some of the growth prospects that the target would have pursued as a standalone firm could instead be implemented under the acquiring firm brand names. With this caveat in mind, our results suggest that PE firms—and not any change in ownership—spur growth.

VIII. Conclusion

Private equity buyouts often elicit strong negative reactions: a common view is that PE firms try to increase corporate profitability by laying off workers and increasing prices, hurting stakeholders such as workers and consumers. Private equity is undoubtedly exercising a growing influence on consumer products and the purchases of millions of people. Using price and sales data for nearly two million unique UPCs sold in over 41,000 stores, we formally investigate the effects of PE on consumer products.

Retail scanner data has several nice features. First, we are able to study the evolution of pricing strategies, product innovation, and geographic availability following a buyout. Second, we can more precisely identify treated units and their counterfactuals in our empirical analyses. In our difference-in-differences estimations, we analyze firms but also decompose them into product categories and products sold within a particular store. This granularity in the data helps mitigate concerns that selection, not actions of private equity firms, drive our results. Last, thanks to the geographic richness of the data, we can also investigate how competitors react by comparing price changes in locations with and without a PE brand.

Contrary to the critics' view, we find that target firms raise prices only marginally. Compared to similar products sold in the same store, target firms raise price by about 1.0%. Competitors respond by also marginally raising prices—by roughly 0.4%—only in those stores where they face direct PE competition. An overall potential price increase of 1.4% in the five years following a buyout does not support the view that private equity firms significantly harm consumers on this dimension. Despite the marginal increase in the price of existing products, target firms experience a significant boom in their overall sales of about 50% in the years post-buyout. Compared to matched firms, target firms launch more products, expand more geographically, and enter more retail chains. Target firms become more acquisitive following buyouts, but organic growth is also strong. PE-driven growth is concentrated in product categories popular among high-income consumers. To the extent that consumers value higher product variety and availability (Lancaster (1990), Kahn and Lehmann (1991), Petrin (2002), Brynjolfsson et al. (2003), and Broda and Weinstein (2006)), PE deals appear to benefit consumers. Overall, our evidence is consistent with private equity being an avenue of wealth creation and not simply wealth transfer. How does PE spur growth? To find clues, we explore different PE target types, economic environments, and industry characteristics. Growth is stronger for private targets, firms that likely demand more access to capital and management expertise. PE product strategies vary with the economic environment: there is more product turnover during the financial crisis; normal times bring higher prices. PE firms are particularly successful in product categories where they hold a strong position in a fragmented market. Our findings are limited to one single "industry" and might not necessarily generalize outside of the consumer product space. Nonetheless, households spend a significant fraction of their monthly budget to buy the products in our study.

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Table I. Example of Product Category: Canned Green Beans

List of canned green bean products available in a specific grocery store in Austin, TX, for the month of December 2007.

UPC	**Product Details**	**Firm Name**	**Size (Oz.)**	**Units Sold**	**Sales**	**Av. Price**
2400016286	Cut Green Beans	Del Monte Foods Inc.	14.5	109.43	101.88	0.92
2400016287	Cut Green Beans (No Salt)	Del Monte Foods Inc.	14.5	86.14	81.68	0.92
2400016289	French Style Green Beans	Del Monte Foods Inc.	14.5	51.00	49.89	0.94
2400016293	Whole Green Beans	Del Monte Foods Inc.	14.5	37.29	39.15	1.05
2000011197	Cut Green Beans	General Mills, Inc.	14.5	30.43	30.12	0.99
2400001546	French Style Green Beans	Del Monte Foods Inc.	28.0	16.71	21.90	1.31
3470001219	Cut Italian Green Beans	Sager Creek Vegetable Co.	28.0	11.29	18.96	1.68
3470001211	Cut Italian Green Beans	Sager Creek Vegetable Co.	16.0	21.57	18.34	0.85
3470001211	Cut Italian Green Beans	Sager Creek Vegetable Co.	14.5	21.57	18.34	0.85
2400039364	Pickled Green Beans with Dill Flavor	Del Monte Foods Inc.	14.5	15.29	18.05	1.13
2000011196	French Style Green Beans	General Mills, Inc.	14.5	17.29	17.11	0.99
2400001830	Cut Green Beans	Del Monte Foods Inc.	28.0	5.57	7.30	1.31
2400016290	French Style Green Beans (No Salt)	Del Monte Foods Inc.	14.5	7.14	7.04	0.95
2400001393	Cut Green Beans	Del Monte Foods Inc.	8.0	8.14	5.94	0.73
2400000087	Cut Green Beans (No Salt)	Del Monte Foods Inc.	8.0	3.71	2.71	0.73
2400016292	French Style Green Beans with Onions	Del Monte Foods Inc.	14.5	1.00	1.05	1.05
2400039201	Organic Cut Green Beans	Del Monte Foods Inc.	14.5	0.29	0.49	1.73

Table II. Summary Statistics

This table presents summary statistics for all variables and data used in the paper. Panel A introduces an overview of the number of products, stores, firms, and private equity deals in the overall Nielsen data. Panel B shows the characteristics of the product categories in Nielsen data. We calculate the Hirfindahl-Hirschman Index (HHI) for each of the 1,123 product categories, each month. Panel C presents firm characteristics in the overall Nielsen data. Panels D focuses on product characteristics split by treatment status.

PANEL A: Overall Nielsen Data

	N.
Products	1,977,481
Stores per Product	571
Products per Store	18,909
Firms	52,205
PE Deals	236
Private Target Deals	222
Public Target Deals	14

	N.
Stores	42,928
Chains	91
3-Digit ZIP	877
Counties	276
Designated Market Areas	206
States	49

PANEL B - Product Category Characteristics

	Mean	Median	S.D.
N. Categories	1,127	-	-
N. Products per Category	20.80	8.07	38.04
N. Stores per Category	30,123	36,762	12,821
N. Firms per Category-Store	4.43	2.00	5.94
Herfindahl-Hirschman Index (HHI)	0.60	0.57	0.34

PANEL C - Firm Characteristics

	Mean	Median	S.D.
N. Products per Firm	10.22	3.00	41.22
N. Stores per Firm	1,345.82	62.00	4,177.03
N. Chains per Firm	8.83	3.00	14.78
N. Categories per Firm	2.87	1.00	6.42

PANEL D - Product Characteristics in Our Sample by Treatment

	Control Group			Treated Group		
	Mean	Median	S.D.	Mean	Median	S.D.
Price	5.33	3.99	5.16	5.19	3.76	5.34
Monthly Units Sold per Store	8.51	1.00	42.26	8.62	1.00	39.40
Monthly Sales per Store	20.42	4.96	106.36	19.64	4.99	81.67

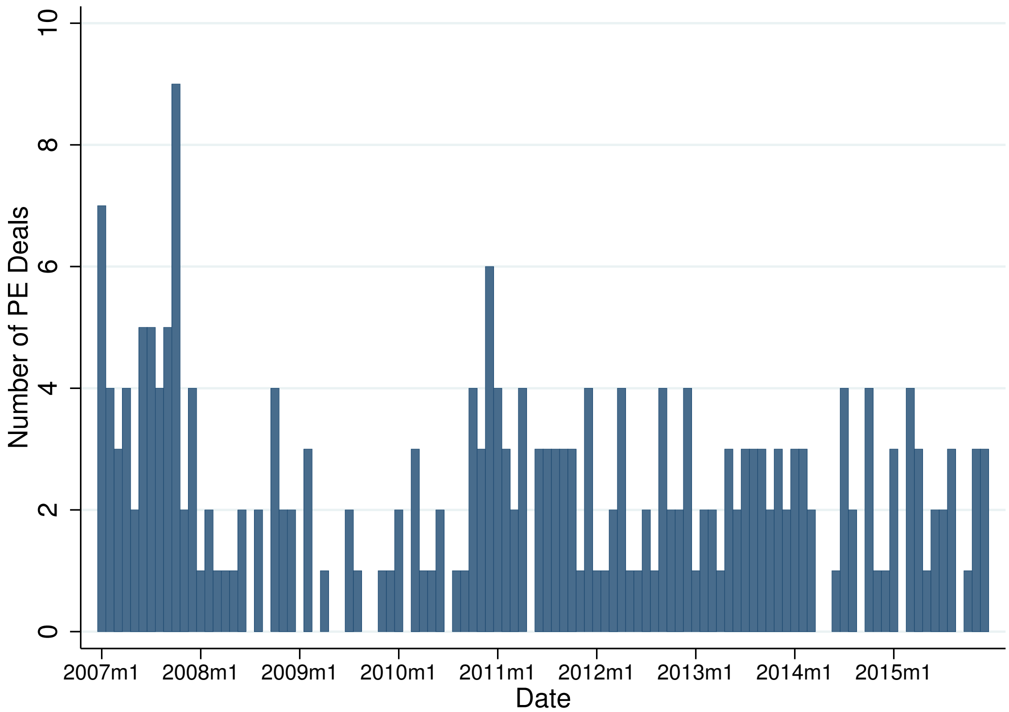


Figure 1. Private Equity Deals over Time

This figure shows the monthly number of private equity deals in our sample from January 2007 to December 2015.

Table III. Private Equity, Sales, and Prices

This table presents OLS coefficient estimates from regressing log of sales, log of average monthly prices, and log of units sold on *After*, a dummy variable equal to one for post-buyout months for firms (Panel A), firm-categories (Panel B), or product-stores (Panel C) that underwent a buyout during our sample period. We use the Abadie and Imbens (2006) distance metric to pair each treated unit with the closest untreated unit. In Panels A and B, we match on sales, unique UPCs sold, and store locations, all during the most recent pre-buyout month, and growth in monthly sales from 12 months before the deal to the most recent pre-buyout month. In Panel C, we match store-products using average price and units sold during the most recent pre-buyout month, and growth in price and units sold from 12 months ago to the most recent pre-buyout month. The unit of analysis is unique at the firm-month-cohort level in panel A, at the firm-product category-month-cohort level in panel B, and at the product-store-month-cohort level in panel C. The estimation period goes from -24 months to +60 months around the private equity deal closing date. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and are double-clustered by firm and month. $^*p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Within Firm

	Sales	Average Prices	Number of Units Sold
After	0.406***	0.053***	0.355***
	(3.59)	(2.86)	(3.43)
Adj. R-Square	0.876	0.933	0.893
N. Obs.	31,596	31,596	31,596
Firm-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes

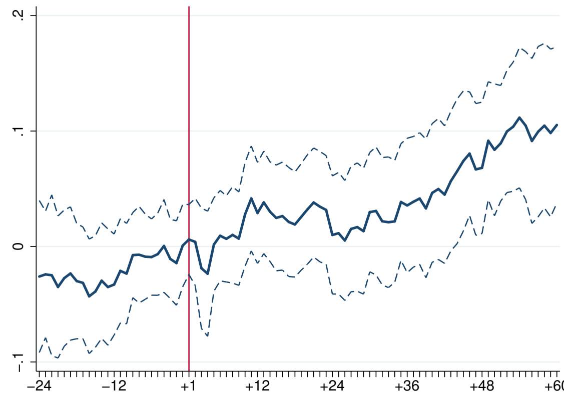
Panel B: Within Firm-Category

	Sales	Average Prices	Number of Units Sold
After	0.211***	0.032***	0.169***
	(3.58)	(3.76)	(3.14)
Adj. R-Square	0.868	0.918	0.884
N. Obs.	224,454	224,454	224,454
Firm-Cat.-Cohort FE	Yes	Yes	Yes
Date-Cat.-Cohort FE	Yes	Yes	Yes

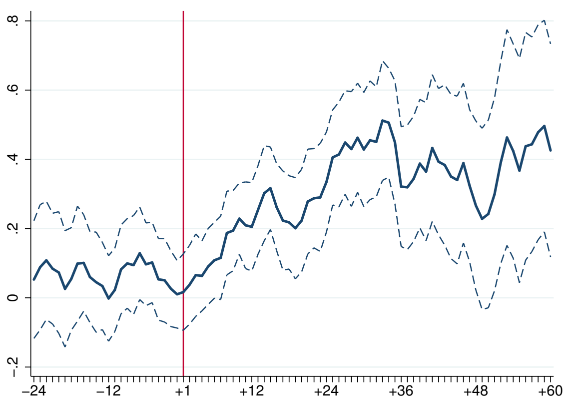
Panel C: Within Product-Store

	Sales	Price	Number of Units Sold
After	0.01332	0.01084**	0.00213
	(0.76)	(2.35)	(0.15)
Adj. R-Square	0.637	0.797	0.773
N. Obs.	880,331,932	880,331,932	880,331,932
Product-Store-Cohort FE	Yes	Yes	Yes
Date-Store-Cohort FE	Yes	Yes	Yes

(a) Sales - Within Firm



(b) Price - Within Firm



(c) Sales - Within Firm-Category

(d) Price - Within Firm-Category

Figure 2. Time Trend of Total Sales and Average Price

These graphs plot the coefficient estimates of regressions following equation 2, where the dependent variables are total sales for panels (a) and (c) and average price for panels (b) and (d). The unit of analysis is a firm-month-cohort for panels (a) and (b) and a firm-category-month-cohort for panels (c) and (d). The coefficient estimate at time t represents the difference in the outcome variables between private equity firms/firm-categories and matched non-private equity firms/firm categories t months away from the date of closing of the private equity deal. The estimation period goes from -24 months to +60 months around the date of the closing of the private equity deal. The closing date is indicated by the vertical line. The dotted lines show the 90% confidence interval.

Table IV. Private Equity and Product Innovation

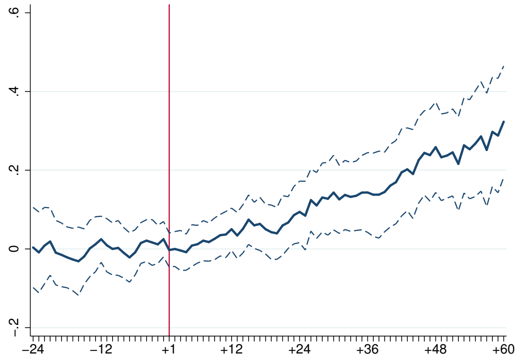
This table presents OLS coefficient estimates from regressing innovation variables on *After*, a dummy variable equal to one for the post-buyout months for firms (Panel A) or firm-categories (Panel B) that underwent a buyout during our sample period. *Number of Products* is the log of the number of unique UPCs a firm or firm-category sells nationwide in month t. *New products* is the number of products introduced by the firm or firm-category in month t, while *Discontinued Products* is the number of products dropped in month t. *Number of Categories* is the log of the number of product categories, out of a total of 1,127 defined by Nielsen, in which a firm sells at time t. Each cohort is a pair of treated-untreated firms (panel A) or firm-categories (panel B). Treated and control are matched as described in Table III. The unit of analysis is unique at the firm-month-cohort level in panel A and at the firm-category-month-cohort level in panel B. The estimation period goes from -24 months to +60 months around private equity deal closing date. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and double-clustered by firm and month. $*p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Panel A: Within Firm

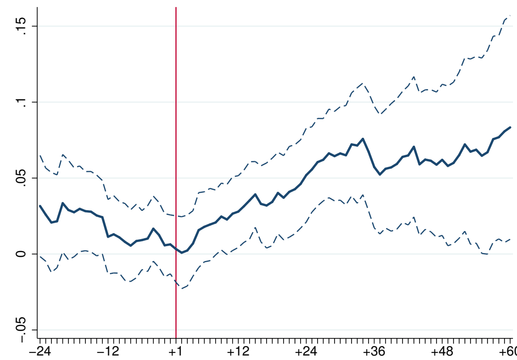
	Number of Products	New Products	Discont. Products	Number of Categories
After	0.104***	0.393**	0.159	0.051**
	(3.12)	(2.06)	(1.11)	(2.22)
Adj. R-Square	0.942	0.514	0.739	0.950
N. Obs.	31,596	31,596	31,596	31,596
Firm-Cohort FE	Yes	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes	Yes

Panel B: Within Firm-Category

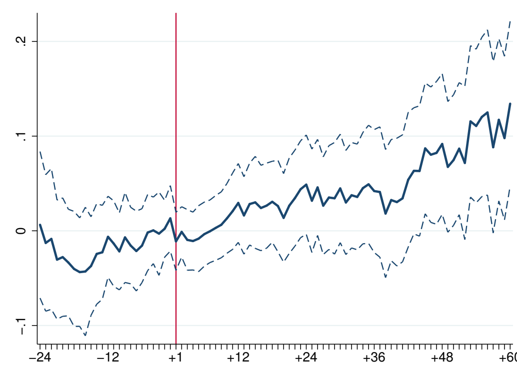
	Number of Products	New Products	Discont. Products
After	0.025**	0.048**	0.034*
	(2.13)	(2.41)	(1.77)
Adj. R-Square	0.920	0.530	0.727
N. Obs.	224,454	224,454	224,454
Firm-Cat.-Cohort FE	Yes	Yes	Yes
Date-Cat.Cohort FE	Yes	Yes	Yes



(a) Number of Products - Within Firm



(b) Number of Products - Within Firm-Category



(c) Number of Product Categories - Within Firm

Figure 3. Time Trend of Product Innovation

These graphs plot the coefficient estimates of regressions following equation 2, where the dependent variables are number of products for panels (a) and (b) and number of product categories for panel (c). The unit of analysis is a firm-month-cohort for panels (a) and (c), and a firm-category-month-cohort for panel (b). The coefficient estimate at time t represents the difference in the outcome variables between private equity firms/firm-categories and matched non-PE firms/firm categories t months away from the date of closing of the private equity deal. The estimation period goes from -24 months to +60 months around the closing date of the private equity deal. The closing date is indicated by the vertical line. The dotted lines show the 90% confidence interval.

Table V. Private Equity and Product Availability

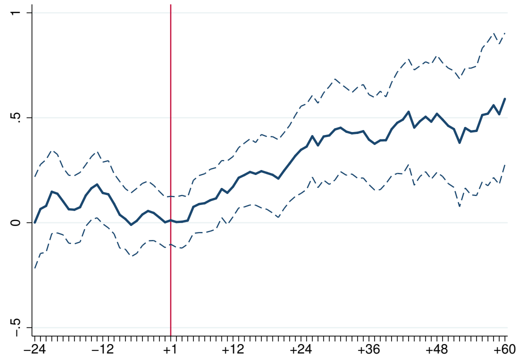
This table presents OLS coefficient estimates from regressing the logs of number of stores, retail chains, and 3-digit ZIP codes where a firm or firm-category is present each month on *After*, a dummy variable equal to one for the post-buyout months for firms (Panel A) or firm-categories (Panel B) that underwent a buyout during our sample period. Each cohort is a pair of treated-untreated firms (Panel A) or firm-categories (Panel B). Treated and control are matched as described in Table III. The unit of analysis is unique at the firm-month-cohort level in panel A and the firm-category-month-cohort level in panel B. The estimation period goes from -24 months to +60 months around the private equity deal closing date. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and double-clustered by firm and month. $^{*}p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Panel A. Within Firm

	N. Stores	N. Chains	N. ZIP Codes
After	0.223***	0.098***	0.129**
	(3.07)	(3.28)	(2.47)
Adj. R-Square	0.907	0.951	0.899
N. Obs.	31,596	31,596	31,596
Firm-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes

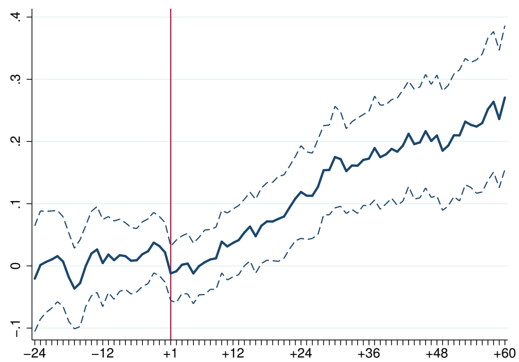
Panel B. Within Firm-Category

	N. Stores	N. Chains	N. ZIP Codes
After	0.130***	0.052***	0.095***
	(2.93)	(2.92)	(2.89)
Adj. R-Square	0.889	0.920	0.882
N. Obs.	224,454	224,454	224,454
Firm-Category-Cohort FE	Yes	Yes	Yes
Date-Category-Cohort FE	Yes	Yes	Yes

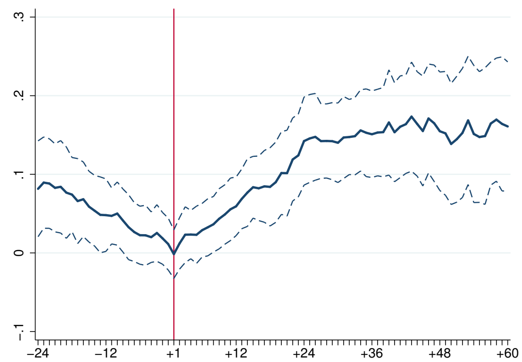


(a) N. Stores - Within Firm

(b) N. Stores - Within Firm-Category



(c) N. Retail Chains - Within Firm



(d) N. Retail Chains - Within Firm-Category

(e) N. 3-digit ZIPs - Within Firm

(f) N. 3-digit ZIPs - Within Firm-Category

Figure 4. Time Trend of Product Availability

These graphs plot the coefficient estimates of regressions following equation 2, where the dependent variables are number of stores for panels (a) and (b), the number of retail chains for panels (c) and (d), and the number of 3-digit ZIPs for panels (e) and (f). The unit of analysis is a firm-month-cohort for panels (a), (c), and (e), and a firm-category-month-cohort for panels (b), (d), and (f). The coefficient estimate at time t represents the difference in the outcome variables between PE firms/firm-categories and matched non-PE firms/firm categories t months away from the closing date of the private equity deal. The estimation period goes from -24 months to +60 months around the date of the closing of the private equity deal. The closing date is indicated by the vertical line. The dotted lines show the 90% confidence interval.

Table VI. Competitor Response

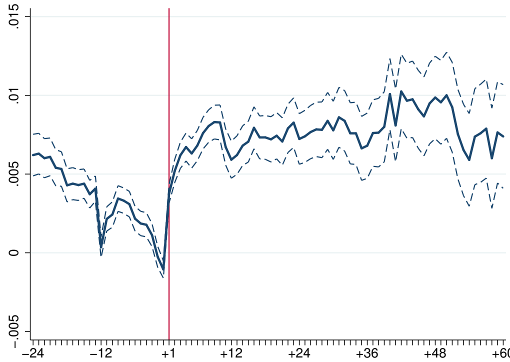
This table presents evidence from product-stores (Panel A) or firm-categories (Panel B) for the competitors of firms that were acquired by a private equity. In Panel A, we present OLS coefficient estimates from regressing the log of average monthly prices on *After*, a dummy variable equal to one in the post-buyout months if the competitor's product was competing in the same store-category with at least one product that underwent a buyout during our sample period. Each cohort is thus made of a treated product sold in a store with PE competition and a matched control product—with the same UPC—sold in different stores without private equity competition. In practice, for each treated product we randomly select ten of these stores without PE competition. Among these ten stores, we then choose the closest match based on the level and growth in the product-store price before the deal, using the Abadie and Imbens (2006) distance metric. In Column 1, we randomly choose ten among all the US stores to select the match. In Column 2, we choose the ten stores within the same retail chain of the treated product. In Column 3, the ten stores are from the same Designated Market Area of the treated product. In Panel B, we present OLS estimates from regressing the log of number of products on *After*, a dummy variable equal to one if the treated firm-category was competing with at least one product in the same category that underwent a buyout during our sample period. Each cohort is thus made of a treated firm-category sold in a store with PE competition and the same firm-category from ten different stores without private equity competition. In Column 1, we randomly choose the ten stores among all the US stores. In Column 2, we choose the ten stores within the same retail chain of the treated firm-category. In Column 3, the ten stores are from the same Designated Market Area of the treated firm-category. The unit of analysis is unique at the product-store-month-cohort level in Panel A and the firm-category-store-month-cohort level in Panel B. The estimation period goes from -24 months to +60 months around the closing date of the private equity deal. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and double-clustered by firm and month. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A. Prices - Within Product-Store

	Full Sample	Same Chain	Same DMA
After	0.004***	0.004***	0.003***
	(6.06)	(8.57)	(5.69)
Adj. R-Square	0.987	0.991	0.988
N. Obs.	6,647,108	5,713,080	5,269,109
Product-Store-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes

Panel B. Number of Products - Within Firm-Category-Store

	Full Sample	Same Chain	Same DMA
After	-0.015***	-0.010***	-0.021***
	(-10.14)	(-4.30)	(-10.19)
Adj. R-Square	0.924	0.957	0.937
N. Obs.	25,200,128	12,724,588	12,191,146
Firm-Category-Store-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes



(a) Competitor price response

(b) Competitor product mix response

Figure 5. Trend in Competitor Response

These figures plot the coefficient estimates of regressions following equation 2, where the dependent variables are average monthly prices for panel (a) and number of products for panel (b). The coefficient estimate at time t represents the difference in the outcome variables between treated product-stores/firm-category-stores and matched controls t months away from the date of closing of the private equity deal. This sample only includes product-stores/ firm-category-stores for control firms that did not go through a private equity deal. In panel (a), each cohort is made of a treated product that is sold in a store-category where a private equity deal occurred, and the best match (with the same UPC) but selected from ten random stores across the US where there is no private equity competitor. In panel (b), each cohort is made of a firm-category where the PE deal occurred, and the average of the same firm-category from ten random stores across the US where there is no private equity competitor. The estimation period goes from -24 months to +60 months around the date of the closing of the private equity deal. The closing date is indicated by the vertical line. The dotted lines show the 90% confidence interval. Regressions are estimated using the fixed point iteration procedure implemented by Correia (2014).

Table VII. Mechanism: Press Releases

This table shows the number (and percentage) of press releases that mention a specific reason for the private equity deal. Out of a total of 297 deals, we were able to find press releases for 237 firms. We compute percentages out of these 237 firms. 44 press releases do not mention any specific reason for the deal. Reason are not mutually exclusive and one press release could mention multiple reasons. The total sample of deals used here (297) is larger that the final sample in our analyses (236), because we do not require to have one year of sales data before and after the deal.

Reason	N. Deals	(%)
Expansion Plans/General Growth	163	(69%)
Financial Capital for Growth	63	(27%)
Industry Experience/Expertise	58	(25%)
New Products	49	(21%)
Acquisitions	29	(12%)
Distribution	26	(11%)
New Management/CEO	24	(10%)
Cost Efficiencies	9	(4%)
Access To Talent	2	(1%)

Table VIII. Mechanisms: Public vs. Private Targets

This table presents OLS coefficient estimates from regressing, in Panel A, logs of sales, average monthly prices, and units sold on *After*, a dummy equal to one in the post-buyout months if the firm, firm-category, or product-store underwent a buyout during our sample period. In Panel B we focus on product innovation, in Panel C product availability. All the outcome variables are either indicator variables or in logs. Public targets are those deals where the target was a public company before the private equity acquisition. Each cohort is a pair of treated-untreated firms, firm-categories, or product-stores where the treated unit is matched to the untreated unit using the same methodologies followed in the previous tables. The unit of analysis is unique at the firm-month-cohort, firm-category-month-cohort, or product-store-month-cohort. The estimation period goes from -24 months to +60 months around the closing date of the private equity deal. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and double-clustered by firm and month. $^*p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Panel A: Sales, Pricing, and Units

		Public Target			Private Target		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
Within Firm	Sales	0.214	(0.53)	2,088	0.420***	(3.54)	29,508
	Average Prices	0.046	(0.94)	2,088	0.053***	(2.73)	29,508
	Units Sold	0.119	(0.36)	2,088	0.372***	(3.41)	29,508
Within Firm-Category	Sales	-0.074	(-0.43)	24,820	0.247***	(4.09)	199,634
	Average Prices	-0.014	(-0.72)	24,820	0.038***	(4.16)	199,634
	Units Sold	-0.059	(-0.40)	24,820	0.198***	(3.55)	199,634
Within Product-Store	Sales	-0.063*	(-1.95)	307,133,126	0.055***	(5.01)	554,415,032
	Prices	0.020**	(2.27)	307,133,126	0.007	(1.39)	554,415,032
	Units Sold	-0.059**	(-2.09)	307,133,126	0.035***	(4.67)	554,415,032

Panel B: Product Innovation

		Public Target			Private Target		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
Within Firm	N. of Products	0.060	(0.47)	2,088	0.107***	(3.09)	29,508
	New Products	1.766	(1.12)	2,088	0.296*	(1.78)	29,508
	Discontinued Products	-0.424	(-0.43)	2,088	0.201	(1.48)	29,508
	Number of Categories	-0.078	(-0.90)	2,088	0.060**	(2.53)	29,508
Within Firm-Category	N. of Products	-0.008	(-0.22)	24,820	0.029**	(2.36)	199,634
	New Products	0.181	(1.51)	24,820	0.032**	(1.98)	199,634
	Discontinued Products	0.043	(0.65)	24,820	0.032*	(1.69)	199,634

Panel C: Product Availability

		Public Target			Private Target		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
Within	N. Stores	0.205	(0.98)	2,088	0.224***	(2.93)	29,508
Firm	N. Chains	-0.080	(-1.43)	2,088	0.110***	(3.49)	29,508
	N. Zip	0.057	(0.37)	2,088	0.134***	(2.44)	29,508
Within	N. Stores	-0.116	(-0.97)	24,820	0.161***	(3.52)	199,634
Firm-Category	N. Chains	-0.086	(-1.61)	24,820	0.069***	(3.96)	199,634
	N. Zip	-0.096	(-1.11)	24,820	0.119***	(3.50)	199,634

Table IX. Mechanisms: During (2007-2010) vs. After (2011-2015) the Financial Crisis

This table presents OLS coefficient estimates from regressing, in Panel A, logs of sales, average monthly prices, and units sold on *After*, a dummy equal to one in the post-buyout months if the firm, firm-category, or product-store underwent a buyout during our sample period. In Panel B we focus on product innovation. In Panel C we study product availability. All the outcome variables are either indicator variables or in logs. The columns "2007-2010" and "2011-2015" include results from private equity deals that closed in those years. Each cohort is a pair of treated-untreated firms, firm-categories, or product-stores where the treated unit is matched to the untreated unit using the same methodologies followed in the previous tables. The unit of analysis is unique at the firm-month-cohort, firm-category-month-cohort, or product-store-month-cohort. The estimation period goes from -24 months to +60 months around the closing date of the private equity deal. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and double-clustered by firm and month. $^{*}p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Sales, Pricing, and Units

		2007-2010			2011-2015		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
Within Firm	Sales	0.589***	(2.98)	15,390	0.255**	(2.01)	16,206
	Average Prices	0.057*	(1.84)	15,390	0.049**	(2.23)	16,206
	Units Sold	0.514***	(2.88)	15,390	0.223*	(1.89)	16,206
Within Firm-Category	Sales	0.206*	(1.98)	99,864	0.215***	(3.20)	124,590
	Average Prices	0.035**	(2.25)	99,864	0.030***	(3.11)	124,590
	Units Sold	0.177*	(1.95)	99,864	0.163**	(2.54)	124,590
Within Product-Store	Sales	-0.021	(-0.73)	415,182,486	0.045***	(2.68)	465,149,446
	Prices	-0.011**	(-2.59)	415,182,486	0.031***	(6.04)	465,149,446
	Units Sold	-0.021	(-0.87)	415,182,486	0.024**	(2.22)	465,149,446

Panel B: Product Innovation

		2007-2010			2011-2015		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
Within Firm	N. of Products	0.106*	(1.90)	15,390	0.102**	(2.57)	16,206
	New Products	0.603	(1.59)	15,390	0.220	(1.40)	16,206
	Discontinued Products	0.347	(1.43)	15,390	0.004	(0.02)	16,206
	Number of Categories	0.048	(1.25)	15,390	0.054*	(1.94)	16,206
Within Firm-Category	N. of Products	0.026	(1.35)	99,864	0.024	(1.66)	124,590
	New Products	0.082**	(1.99)	99,864	0.024	(1.34)	124,590
	Discontinued Products	0.087**	(2.14)	99,864	-0.004	(-0.31)	124,590

Panel C: Product Availability

		2007-2010			2011-2015		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
Within	N. Stores	0.308**	(2.50)	15,390	0.153*	(1.82)	16,206
Firm	N. Chains	0.125**	(2.44)	15,390	0.075**	(2.17)	16,206
	N. Zip	0.206**	(2.24)	15,390	0.064	(1.15)	16,206
Within	N. Stores	0.102	(1.43)	99,864	0.150***	(2.73)	124,590
Firm-Category	N. Chains	0.050**	(2.20)	99,864	0.053**	(2.11)	124,590
	N. Zip	0.079	(1.64)	99,864	0.107**	(2.45)	124,590

Table X. Mechanism: Industry Structure

This table presents OLS coefficient estimates from regressing outcome variables on *After*, a dummy equal to one in the post-buyout months if the firm-category underwent a private equity buyout during our sample period. In Panel A, we split results based on the target firm's market share in the product categories. In Panel B, we separately report results based on the concentration (HHI index) in the product categories. In Panel C, we split the evidence based on the popularity of the product categories among high-end consumers. *Market Share* for each firm is its sales divided by total sales, each month, in a particular category. *High* values of *Market Share* are firms above the median in a category-month. *HHI* is the Herfindahl-Hirschman Index of each product category, each month, calculated by squaring and summing the national market shares of each firm in a given category. *High* values of *HHI* are those categories whose *HHI* is above the median that month. Using the Nielsen Consumer Panel, for each product category, we compute the average income of the consumers that buy products in the category. *High-Income Consumers* categories are those categories that have an average income that is above the median income among of all categories. Each cohort is a pair of treated-untreated firm-categories where the treated unit is matched to the untreated unit with the closest distance at the time of the private equity deal as described in Table III. The unit of analysis is unique at the firm-category-month-cohort level. The estimation period goes from -24 months to +60 months around the private equity deal closing date. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and double-clustered by firm and month. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Market Share in the Product Category

		High Market Share			Low Market Share		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
	Sales	0.265***	(3.13)	92,712	0.109	(1.39)	98,920
	Average Prices	0.055***	(5.07)	92,712	0.013	(0.97)	98,920
	Units Sold	0.208***	(2.73)	92,712	0.089	(1.23)	98,920
Within Firm-Category	N. of Products	0.014	(0.80)	92,712	0.043**	(2.57)	98,920
	New Products	0.107**	(2.53)	92,712	0.005	(0.43)	98,920
	Discontinued Products	0.076*	(1.80)	92,712	0.002	(0.26)	98,920
	N. Stores	0.168***	(3.00)	92,712	0.079	(1.27)	98,920
	N. Chains	0.087***	(4.29)	92,712	-0.007	(-0.26)	98,920
	N. Zip	0.128***	(3.53)	92,712	0.058	(1.21	98,920

Panel B: Product Category Concentration

		High HHI			Low HHI		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
	Sales	0.186***	(2.72)	109,800	0.243***	(3.60)	114,490
	Average Prices	0.037***	(3.23)	109,800	0.031***	(3.25)	114,490
	Units Sold	0.152**	(2.41)	109,800	0.193***	(3.14)	114,490
Within Firm-Category	N. of Products	0.010	(0.71)	109,800	0.037**	(2.47)	114,490
	New Products	0.013	(0.69)	109,800	0.075**	(2.52)	114,490
	Discontinued Products	0.041	(1.42)	109,800	0.020	(1.11)	114,490
	N. Stores	0.133**	(2.58)	109,800	0.128**	(2.55)	114,490
	N. Chains	0.041*	(1.89)	109,800	0.066***	(3.10)	114,490
	N. Zip	0.106***	(2.73)	109,800	0.087**	(2.36)	114,490

Panel C: Category Popularity Among High-Income Consumers

		High-Income Consumers			Low-Income Consumers		
		After	T-stat	N. Obs.	After	T-stat	N. Obs.
	Sales	0.274***	(3.72)	147,044	0.093	(1.28)	77,410
	Average Prices	0.034***	(3.10)	147,044	0.030***	(2.67)	77,410
	Units Sold	0.231***	(3.51)	147,044	0.051	(0.74)	77,410
Within	N. of Products	0.026*	(1.86)	147,044	0.023	(1.38)	77,410
Firm-Category	New Products	0.063***	(2.84)	147,044	0.020	(0.65)	77,410
	Discontinued Products	0.055**	(2.17)	147,044	-0.008	(-0.39)	77,410
	N. Stores	0.168***	(3.19)	147,044	0.057	(0.99)	77,410
	N. Chains	0.070***	(3.63)	147,044	0.017	(0.61)	77,410
	N. Zip	0.123***	(3.21)	147,044	0.043	(0.99)	77,410

Table XI. Mechanism: Company Strategy and Investments

This table presents OLS coefficient estimates from regressing outcome variables of interest on *After*, a dummy equal to one in the post-buyout months if the firm underwent a private equity buyout during our sample period. In Panel A, we restrict the sample to firms for which we observe at least one acquisition in Capital IQ. The outcome variable *Acquisitiveness* counts the number of acquisitions closed in a month. In Panel B, the unit of analysis is a firm-year. We restrict the sample to firm-years in which we see at least one month of positive advertising expenditure. The outcome variable is *Advertising Expenditures*, the log of one plus the annualized average monthly advertising expenses for all the brands related to the firm as reported in Ad$pender by Kantar Media. Each cohort is a pair of treated-untreated firms where the treated unit is matched to the untreated unit with the closest distance at the time of the private equity deal as described in Table III. The unit of analysis is unique at the firm-month level. The estimation period goes from -24 months to +60 months around the private equity deal closing date. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and double-clustered by firm and month. $^{*}p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Panel A: Acquisitiveness

	Full	Target		Time Period	
	Sample	Public	Private	2006-2010	2011-2015
After	0.016***	0.017	0.016***	0.014***	0.018***
	(4.71)	(1.21)	(4.47)	(3.25)	(3.48)
Adj. R-Square	0.107	-0.016	0.112	0.081	0.120
N. Obs.	26,334	1,770	24,564	12,662	13,672
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes	Yes	Yes

Panel B: Advertising Expenditures

	Full	Target		Time Period	
	Sample	Public	Private	2006-2010	2011-2015
After	0.396**	0.499	0.376*	0.056	0.330
	(2.26)	(1.46)	(1.84)	(0.14)	(1.65)
Adj. R-Square	0.746	0.880	0.721	0.682	0.787
N. Obs.	708	87	621	331	377
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes

Online Appendix for

"Barbarians at the Store? Private Equity, Products, and Consumers"

by

Cesare Fracassi, Alessandro Previtero, and Albert Sheen

I. Sample Construction

We follow three steps to create the database used in our analyses. First, we identify private equity deals in the period 2005 to 2017. We rely on *Capital IQ* and *Preqin* as deal sources. Second, we link the products in the Nielsen database to their selling firm names, using their universal product codes (UPCs), and then match these firm names with the PE target firm names. Last, we run robustness checks to ensure that our final sample does not omit major deals and to remove misclassified deals. In the following sub-sections, we review each of these steps in detail.

A. *Identifying the Universe of Potential Deals*

We first collect information on PE deals from Capital IQ, using the following screens:

1. *Merger/Acquisition Features*: Going Private Transaction OR Leveraged Buy Out (LBO) OR Management Buyout OR Secondary LBO
2. *M&A Announced Date*: [1/1/2005-12/31/2017]
3. *Geographic Locations (Target/Issuer)*: United States and Canada (Primary)
4. *Merger/Acquisition Features*: NOT (Acquisition of Minority Stake).

In particular, we rely on the following fields:

- *TargetName*: the target company name used to match Capital IQ data with GS1 data.
- *State*: the target company state also used in the match.
- *DealCompleted*: the deal date used to create the "After" variable in our main analyses.

- *Buyers*: the name of the PE firms involved in the deal.

We complement the deal information from Capital IQ with deals from the Preqin database. We download private equity buyout deals in North America with deal dates from 2005 to 2017. We use the following fields:

- *Firm*: the target company name used to match Capital IQ data with GS1 data.
- *State*: the target company state also used in the match.
- *Deal Date*: the date used to create the "After" variable in our main analyses.
- *Investors*: the name of the PE firms involved in the deal.

B. *Finding Database Matches*

The most challenging and time-consuming part of our data set construction is to match PE target firms to products in the Nielsen scanner database. We first retrieve from GS1—the organization that assigns UPC codes—the link between UPC numbers and the firms that sell the products associated with these UPCs. We then match these firms to the PE targets from Capital IQ and Preqin. We match across the datasets using *company names* and *States*. In practice, we follow these six steps:

1. We modify the fields *"Target/Issuer"* in Capital IQ and *"CompanyName"* provided by GS1 to remove capital letters.
2. We match these fields (*"Target/Issuer"* and *"CompanyName"*) using the Stata user-written command *"reclink"*. "Reclink" uses a fuzzy matching algorithm that provides a score between 0 and 1 that expresses the goodness of the match. Based on this score

and the state, firms fall into four groups. The next four steps in the process are based on the inspection of each of these groups.

3. *"Perfect match, same state"*: We include in our sample all firms with a matching score equal to one (i.e., the highest score) and same state across the two data sources. We have 517 of these firms. We visually inspect each of these firms to verify that indeed the names exactly match.

4. *"Perfect match, different state"*: If the match score is equal to one, but the matched company is listed from two different states, a research assistant has conducted a web search to verify that the match is correct and that there are not two firms with similar names but from these two different states. In this and the following web searches the research assistant has relied on information on the target firm from CapitalIQ (fields: "Product Description," "Primary Sector," and "Primary Industry") and Nielsen ("product_module_desc" and "brand_descr") to verify the actual match between the two firms. We start with 178 of these firms and, after the manual checking process, we add 98 of these firms to our sample.

5. *"Good match, same state"*: For those firms that have a matched score between 0.90 and 1 from the same state, we conduct a web search as in the previous case. We identify 1,535 of these firms. After the clean-up process, we add 794 of these firms to our sample.

6. *"Good match, different state"*: For those firms that have a matched score between 0.99[17] and 1 but with different state information, we conduct our web search. We manually

[17]We select a cut-off higher than the one chosen for the previous category to keep the number of firms that we need to manually inspect manageable.

check 1,117 firms in this category. We then include 179 of these firms in our sample.

At the end of this process, we have 1,588 matched firms between Capital IQ and GS1. Note that we follow this same process to match firms that are the target of M&A deals from Capital IQ to firms in GS1. We use these M&A targets in Table A1 and A8.

We then repeat steps #1 to #6 for the PE deals in the Prequin database. The relevant variables for the match in Prequin are *"Firm"* and *"State."* At the end of this process we have 2,757 matched firms from Prequin. The breakdown of matched firms across the four groups is as follows: 663 *"Perfect match, same state,"* 256 *"Perfect match, different state,"* 1,479 *"Good match, same state,"* and 359 *"Good match, different state."*

When we consolidate the list of target firms across Capital IQ and Prequin, we obtain 3,563 unique firms. We then merge these firms with Nielsen sales data, using the UPCs that are reported in Nielsen. We are able to match 908 firms. The many firms that drop out sell products with UPCs but not in supermarkets, drug stores, or mass merchandisers.

C. Additional Robustness Checks

We run two additional analyses to complement and verify this list of 908 deals.

If companies are recorded under completely different names in Capital IQ (or Preqin) vs. GS1, we would not be able to match them. To address this concern, we first collect from Capital IQ the largest deals (i.e., top decile by deal size) for each year of our analysis (2007 to 2015). Then, we inspect each of these deals focusing on their "Product Description," "Primary Sector," and "Primary Industry." For the deals that appear to be in the consumer product space, we do a web search to retrieve their most popular brands, potential aliases, and names of subsidiaries or parent companies. Last, we try to match any of the above with the GS1 database following the process previously described. This procedure allows us to identify 24 companies that were missing from our sample. The major reason for missing these deals was that firms were reported in Capital IQ/Prequin with different names compared to GS1. For example, the target firm Yankee Holding Corp. was recorded in GS1 as The Yankee Candle Company, Inc.

The initial screenings to retrieve PE deals from Capital IQ and Preqin generate a comprehensive list of 932, meant to capture any potential private equity deal. At this point, given that we have Nielsen sales data between 2006 and 2016 and that we require firms to have at least one year of sales data before and after the deal, we drop deals that closed before 2007 or after 2015. We also discovered that some target firms did not have any of their UPCs record sales within one year surrounding the deal closing date. We drop these firms. Next, we do a deep dive into the remaining deals to verify that these are PE deals as commonly defined in the literature. We base our investigation on the deal description and web-based searches. We end up eliminating: i) deals that do not actually result in a change in control; ii) deals where the buyer is a person as opposed to a private equity firm; and iii) deals where the PE targeted firm was mistakenly matched with a similarly named firm in GS1/Nielsen. We also remove add-on deals where the PE target company, not the PE firm, is the buyer. Our final sample consists of 236 firms.

II. Sample Representativeness

How representative are the 236 deals in our sample of typical PE transactions? To address this question, we compare across different samples the deal features available from Capital IQ. We report these results in Table A1. In our sample period there are 17,566 total deals in Capital IQ. The screening criteria to select this sample are reported in subsection I.A.

We classify 4,811 of these deals as "Consumer Goods", if their primary sector description is "Consumer Discretionary" or "Consumer Staples". The "Capital IQ–GS1" sample includes those deals whose target firms can be matched to the GS1 database. Details on the matching process are reported in subsection I.B. In this sample, we have 1,588 target firms accounting for 1,839 deals. One target firm could be involved in multiple deals because it is the target of secondary PE buyouts. The "Capital IQ-GS1-Nielsen" sample includes those deals from Capital IQ/ GS1 whose targets have sales data in Nielsen. We identify 536 target firms, accounting for 634 deals. After our manual screening, we are left with a final sample of 216 target firms. Each of these firms appears only in one deal. This sample is different from our final sample of 236 deals, because it only includes deals from Capital IQ. Of these 216 firms, 13 targets were public before the deal. The remaining 203 were private firms. The "M&A Sample" includes firms that were target of M&A deals in our sample period. We collect this deals from Capital IQ and we match them to GS1/ Nielsen data, following the same procedure reported in subsection I.B.

We find that our deals appear to be larger in size and involve older firms compared to the average PE deal in CapitalIQ and, even more so, compared to deals in consumer products. Implied equity valuations and total cash payments are also larger for our sample. There is no significant difference in term of number of PE investors involved. With the caveat that the deal information are not very heavily populated in Capital IQ, our sample seems to represent larger PE deals, between the 75th and the 90th percentile of the overall PE deal size distribution.

Table A1. Deal Characteristics and Sample Selection Process

This table shows descriptive statistics of PE deals across different samples from Capital IQ. Our final sample here includes 216 firms—and not 236 as in the paper—because we include only firms from Capital IQ. We describe these different samples in subsection II. *"Deal Value"* is defined as the total transaction value (in US $ Million). *"Implied Equity Value"* and *"Total Cash"* are the equity value and the total cash payment of the deal as reported in Capital IQ. *"Target Age"* is the age, in years, of the target firm when the deal was completed. *"Buyer Number"* is the number of PE firms involved in each deal.

Variable	Stat.	Capital IQ (17,566)	Consumer Goods (4,811)	Capital IQ-GS1 (1,839)	CIQ/GS1 Nielsen (634)	Final Sample (216)	Final Public (13)	Final Private (203)	M&A Sample (126)
Deal Value	mean	383.9	325.6	573.3	659.5	865.5	1,870.9	521.6	472.9
	sd	1,871.9	1,625.0	1,730.3	1,467.4	1,453.7	1,730.3	1,186.6	1,816.6
	p25	2.4	2.0	9.9	12.6	51.0	702.6	22.0	7.7
	p50	20.0	14.2	78.8	112.6	310.0	1,325.3	149.0	25.5
	p75	161.9	110.1	380.2	415.0	1,009.7	2,239.0	420.0	140.0
	N	4,170	1,136	372	122	51	13	38	49
Implied Equity Value	mean	321.7	276.4	501.6	595.6	823.2	1,510.1	535.2	491.5
	sd	1,463.1	1,300.2	1,440.5	1,344.1	1,374.2	1,318.2	1,312.1	1,586.9
	p25	1.9	1.6	8.0	8.5	52.9	476.1	22.0	8.3
	p50	16.0	10.0	73.0	90.0	280.0	1,293.0	100.0	37.0
	p75	139.0	87.5	397.0	420.0	963.9	1,855.2	410.0	173.7
	N	3,814	1,041	335	111	44	13	31	49
Total Cash	mean	319.3	273.1	494.1	570.3	771.4	1,510.1	497.1	461.4
	sd	1,433.2	1,281.4	1,419.0	1,309.9	1,323.9	1,318.2	1,234.3	1,440.8
	p25	2.0	1.6	9.5	9.5	50.6	476.1	20.0	5.7
	p50	17.7	11.8	75.0	90.0	205.7	1,293.0	96.5	34.5
	p75	140.0	94.3	327.0	415.0	963.9	1,855.2	410.0	173.7
	N	3,875	1,051	347	115	48	13	35	46
Target Age	mean	29.4	34.5	37.3	40.4	43.1	64.9	41.7	33.6
	sd	29.3	33.3	33.3	35.8	40.7	39.5	40.5	28.4
	p25	10	11	15	15	15	33	14	14
	p50	21	25	28	31	31	58	30	23
	p75	38	46	50	57	59	87	57	46
	N	11,146	3,050	1,396	495	205	12	193	114
Buyer Number	mean	1.2	1.2	1.2	1.3	1.3	1.4	1.3	1.4
	sd	0.6	0.6	0.6	0.7	0.7	0.7	0.7	0.5
	p25	1	1	1	1	1	1	1	1
	p50	1	1	1	1	1	1	1	1
	p75	1	1	1	1	1	2	1	2
	N	9,057	2,115	1,191	430	208	12	196	125

Table A2. List of Largest Product Categories

This table shows the largest product categories by monthly sales in the Nielsen dataset, together with the average number of products in that category nationwide.

Product Category	Monthly Sales ($)	Av. N. of Products
CIGARETTES	429,254,112	930
SOFT DRINKS - CARBONATED	269,718,144	2,076
CEREAL - READY TO EAT	227,483,344	535
SOFT DRINKS - LOW CALORIE	221,177,712	804
LIGHT BEER (LOW CALORIE/ALCOHOL)	207,607,984	280
WINE-DOMESTIC DRY TABLE	205,774,640	5,258
BEER	176,359,296	1,433
WATER-BOTTLED	175,339,872	1,347
TOILET TISSUE	171,534,576	152
DETERGENTS - HEAVY DUTY - LIQUID	165,413,312	328

Table A3. List of Most Common Private Equity Partners

This table shows the most frequent private equity partners that are involved in the 236 private equity deals in our sample.

General Partner Name	N. of Deals
Sun Capital Partners Inc	9
Encore Consumer Capital	6
Arbor Private Investment Company	5
Wind Point Partners	4
Brazos Private Equity Partners LLC	4
Mason Wells Inc	4
The Riverside Company	4
Brynwood Partners	4
Vestar Capital Partners Inc	4

Table A4. Largest Private Equity Deals

This table shows the largest private equity deals in our sample, sorted by the average monthly sales in the Nielsen dataset.The deal value, from Capital IQ, includes the value of divisions and subsidiaries that do not sell to supermarkets or mass merchandisers.

Target	**Deal Date**	**Monthly Sales ($)**	**Deal Value ($Mil)**
Del Monte Foods Inc.	8-Mar-11	59,519,200	5,482
The Nature's Bounty Co.	1-Oct-10	17,472,164	4,078
Pabst Brewing Company	7-Jun-10	13,083,578	250
Evenflo Company, Inc.	8-Feb-07	9,514,464	260
Bradshaw International, Inc.	16-Oct-08	9,313,272	N/A
The Sun Products Corporation	30-Apr-07	8,821,161	1,250
Peet's Coffee And Tea, Inc.	29-Oct-12	7,129,344	1,010
Matrixx Initiatives, Inc.	17-Feb-11	5,734,518	82
Parfums De Coeur Ltd.	5-Sep-12	5,591,422	N/A
Armored Autogroup Inc.	5-Nov-10	4,919,370	755

Table A5. Private Equity Deal Selection

This table presents OLS coefficient estimates from regressing a product category selection dummy, a firm selection dummy, and a product selection dummy on explanatory variables to determine the private equity interest in specific product categories, firms, or products. The sample is restricted to months when a private equity deal occurred. The industry selection dummy is equal to one if there was a private equity deal in that product category in that month. Firm selection dummy is equal to one if the firm was acquired by a private equity company in that month. Product selection dummy is equal to one if the product is acquired by a private equity company in that month. We describe how we construct the *"High-Income Category"* indicator and how we compute the *"Herfindal Index"* in section VII.D of the paper. The unit of analysis is unique at the product-category-month for column 1, firm-month for column 2, and product-month for column 3. Standard errors are double-clustered at the firm and time. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	Category Selection	Firm Selection	Product Selection
High-Income Category	0.005***		
	(4.86)		
Herfindal Index	-0.023***		
	(-11.41)		
Price Av. (log)	-0.003***	-0.000	-0.001***
	(-6.96)	(-0.61)	(-21.62)
Sales (log)	0.002***	0.001***	0.000***
	(9.00)	(3.06)	(24.10)
Growth N. Products	-0.002	-0.000	
	(-0.82)	(-1.01)	
Growth Sales	0.002	-0.000	-0.000***
	(1.63)	(-1.55)	(-4.51)
Growth Price Av.	-0.002	0.001*	0.001***
	(-0.66)	(1.75)	(9.32)
Adj. R-Square	0.049	0.019	0.083
N. Obs.	130,053	324,630	2,695,569
Date FE	Yes	No	No
Category-Date FE	No	Yes	Yes

Table A6. Summary Statistics of Matching Procedure

This table presents the summary statistics (Mean and Median) of firm-level characteristics for treated and matched control firms at the time of the private equity buyout. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

	Treated		Matched Control		Difference	
	Mean	Median	Mean	Median	Diff	t-stat
Matching Variables						
Monthly Sales	1,036,508.44	55,065.39	902,937.81	59,015.72	-133,570.63	(-0.36)
Monthly Sales Growth	27.31	-0.02	4.46	-0.00	-22.85	(-1.23)
N. Products	36.58	11.50	35.12	11.00	-1.47	(-0.23)
N. Stores	5,298.33	1,408.50	5,277.92	1,494.00	-20.41	(-0.03)
Non-Matching Variables						
Monthly Units Sold	396,994.41	12,114.79	332,081.96	12,190.14	-64,912.45	(-0.33)
Average Price	7.85	4.76	7.27	4.31	-0.58	(-0.52)
N. Categories	7.81	3.00	7.71	3.00	-0.09	(-0.07)
N. Chains	24.61	14.00	24.47	14.00	-0.14	(-0.06)
N. 3-digit ZIP Codes	383.67	313.00	378.01	308.00	-5.66	(-0.18)
N. Counties	106.76	117.50	107.84	114.00	1.08	(0.16)
N. States	30.18	36.00	29.02	34.50	-1.16	(-0.66)
N. DMAs	106.15	100.50	103.89	100.00	-2.26	(-0.30)

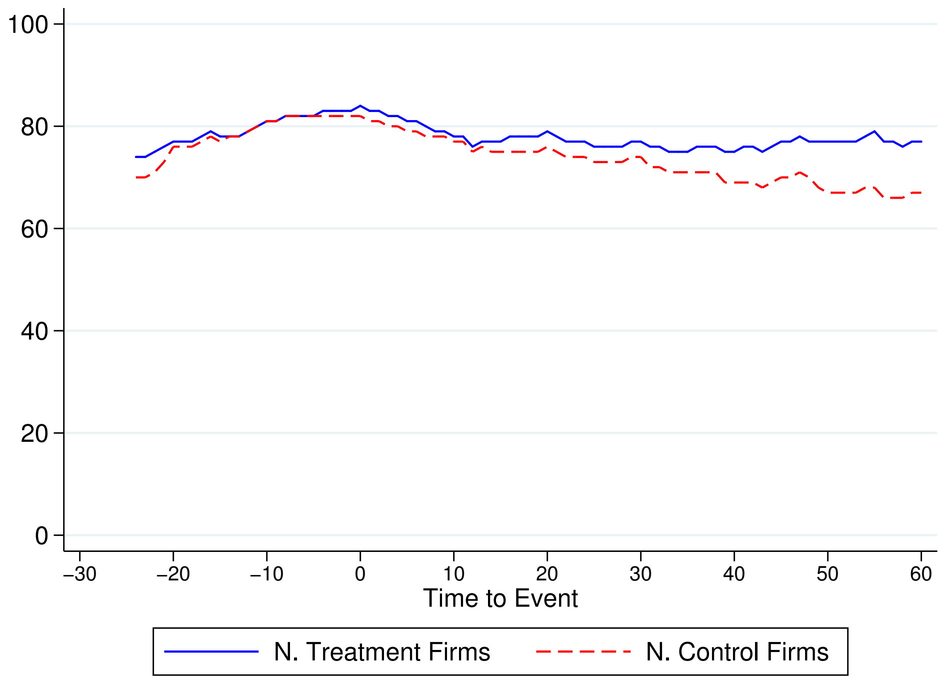
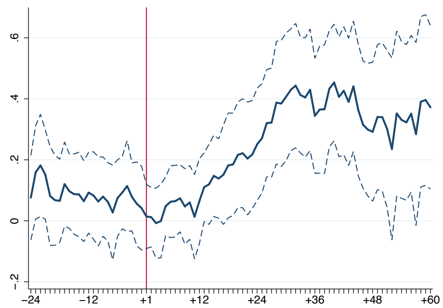


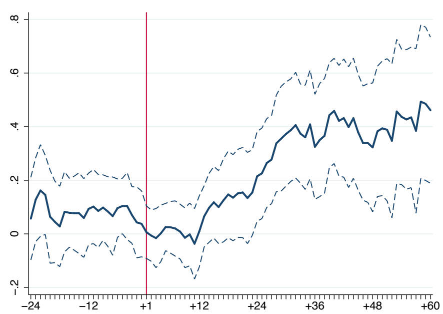
Figure A1. N. of Treated and Control Firms

The figure plot the number of treated and control firms in the sample over time relative to the deal close date, only for deal closed in 2008-2011. We limit our analysis to the years 2008 to 2011 to ensure that we have the five full years of data available for all firms (our sample ends in 2015).



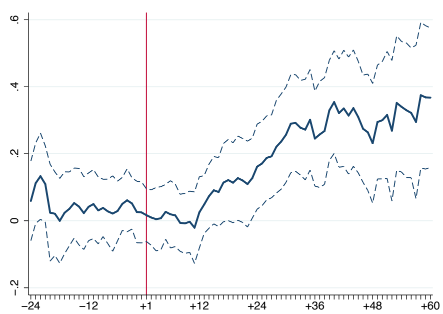
(a) N. of Counties - Within Firm

(b) N. of Counties - Within Firm-Category

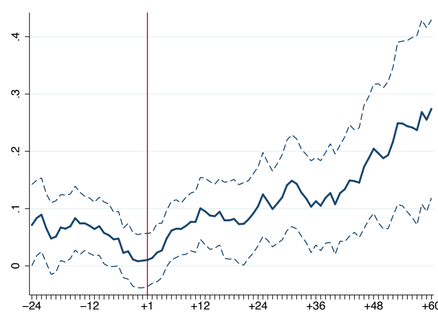


(c) N. of Market Areas - Within Firm

(d) N. of Market Areas - Within Firm-Category



(e) N. of States - Within Firm



(f) N. of States - Within Firm-Category

Figure A2. Time Trend of Product Availability

These graphs plot the coefficient estimates of regressions following equation 2, where the dependent variables are number of counties for panels (a) and (b), the number of designated market areas for panel (c) and (d), and the number of states for panel (e) and (f). The unit of analysis is a firm-month-cohort for panels (a),(c), and (e), and a firm-category-month-cohort for panels (b), (d), and (f). The coefficient estimate at time t represents the difference in the outcome variables between PE firms/firm-categories and matched non-PE firms/firm categories t months away from the date of closing of the private equity deal. The estimation period goes from -24 months to +60 months around the date of the closing of the private equity deal. The closing date is indicated by the vertical line. The dotted lines show the 90% confidence interval.

Table A7. Private Equity and Consumer Goods - Annual Coefficients

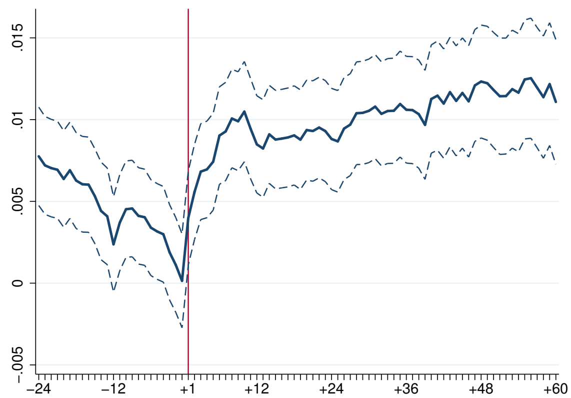
This table presents OLS coefficient estimates from regressing log of sales, log of average monthly prices, log of units sold, number of products, number of stores, number of 3-digit ZIP, and number of categories, on dummies equal to one if the observation month is includes in the year at distance t from the deal close year for firms (Panel A) or firm-categories (Panel B) that underwent a buyout during our sample period. We use the Abadie and Imbens (2006) distance metric to pair each treated unit with the closest untreated unit. We match on sales, unique UPCs sold, and store locations, all during the most recent pre-buyout month, and growth in monthly sales from the past 12 months to the most recent pre-buyout month. The unit of analysis is unique at the firm-month-cohort level in panel A, and at the firm-product category-month-cohort level in panel B. The estimation period goes from -24 months to +60 months around the private equity deal closing date. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are in parentheses and are double-clustered by firm and month. $*p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Panel A: Within Firm

	Sales	Average Price	Units Sold	N. Products	N. Stores	N. Chains	N. ZIP	N. Categories
Year -2	-0.091	-0.037*	-0.059	-0.019	0.038	-0.007	0.064	-0.027
	(-0.90)	(-1.79)	(-0.64)	(-0.60)	(0.54)	(-0.22)	(1.19)	(-1.12)
Year -1	-0.106	-0.017	-0.080	-0.004	-0.017	0.012	0.016	-0.007
	(-1.35)	(-1.25)	(-1.13)	(-0.21)	(-0.39)	(0.69)	(0.44)	(-0.51)
Year +1	0.211**	0.023	0.196**	0.045**	0.170***	0.061***	0.128***	0.028*
	(2.40)	(1.44)	(2.55)	(2.19)	(3.35)	(3.12)	(3.32)	(1.88)
Year +2	0.492***	0.014	0.454***	0.112***	0.343***	0.141***	0.264***	0.039*
	(3.63)	(0.66)	(3.72)	(3.17)	(4.22)	(4.94)	(4.37)	(1.72)
Year +3	0.519***	0.046**	0.460***	0.169***	0.382***	0.185***	0.273***	0.052*
	(3.23)	(2.03)	(3.01)	(3.58)	(3.58)	(5.26)	(3.57)	(1.74)
Year +4	0.548***	0.090***	0.523***	0.250***	0.417***	0.219***	0.267***	0.100***
	(2.87)	(3.64)	(3.00)	(4.50)	(3.27)	(4.89)	(2.88)	(2.78)
Adj. R-Square	0.876	0.933	0.894	0.943	0.909	0.952	0.900	0.950
N. Obs.	31,596	31,596	31,596	31,596	31,596	31,596	31,596	31,596
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Within Firm-Category

	Sales	Average Price	Units Sold	N. Products	N. Stores	N. Chains	N. ZIP
Year -2	-0.040	-0.025***	0.002	0.014	0.058	0.044**	0.057*
	(-0.77)	(-2.74)	(0.05)	(1.26)	(1.42)	(2.44)	(1.88)
Year -1	-0.044	-0.020***	-0.024	-0.003	-0.023	0.003	-0.007
	(-1.36)	(-4.22)	(-0.79)	(-0.47)	(-0.93)	(0.30)	(-0.39)
Year +1	0.146***	0.003	0.139***	0.023***	0.126***	0.061***	0.123***
	(3.76)	(0.51)	(3.91)	(2.97)	(4.06)	(5.31)	(4.89)
Year +2	0.330***	0.021**	0.295***	0.051***	0.251***	0.119***	0.199***
	(5.40)	(2.14)	(5.55)	(4.08)	(5.49)	(6.49)	(5.67)
Year +3	0.252***	0.023*	0.229***	0.047**	0.235***	0.133***	0.169***
	(2.84)	(1.89)	(2.90)	(2.55)	(3.57)	(5.18)	(3.49)
Year +4	0.267**	0.022	0.243**	0.055**	0.221**	0.128***	0.167**
	(2.21)	(1.44)	(2.26)	(2.15)	(2.56)	(3.73)	(2.56)
Adj. R-Square	0.868	0.918	0.884	0.920	0.889	0.921	0.883
N. Obs.	224,454	224,454	224,454	224,454	224,454	224,454	224,454
Firm-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes



(a) Same Retail Chain

(b) Same Designated Market Area

Figure A3. Price Response of Competitors - By Control Type

These figures plot the coefficient estimates of regressions following equation 2, where the dependent variables are product monthly prices. The coefficient estimate at time t represents the difference in the outcome variables between treated products and matched control products, t months away from the date of closing of the private equity deal. This sample only includes products whose firms did not go through a private equity deal. Each cohort is made of a treated product that is sold in a store-category where a private equity deal occurred, and the best match (with the same UPC) but selected from ten random stores where there is no private equity competitor. In Panel (a) we randomly select the ten stores within the same retail chain of the treated product. In Panel (b) we randomly choose the ten stores within the same Designated Market Area of the treated product. The estimation period goes from -24 months to +60 months around the date of the closing of the private equity deal. The closing date is indicated by the vertical line. The dotted lines show the 90% confidence interval. Regressions are estimated using the fixed point iteration procedure implemented by Correia (2014).

Table A8. The Effects of M&A Deals on Consumer Product Firms

This table presents OLS coefficient estimates from regressing, in Panel A, logs of sales (Column 1), average monthly prices (Column 2), and units sold (Column 3) on *After*, a dummy equal to one for the post-M&A months for firms that underwent a M&A during our sample period. In Panel B we focus on product innovation. In Panel C we study product availability. All the outcome variables are either indicator variables or in logs. Each cohort is a pair of treated-untreated firms where the treated unit is matched to the untreated unit with the closest distance at the time of the M&A deal based on sales, unique UPCs sold, and store locations, all during the most recent pre-M&A month, and growth in monthly sales from the past 12 months to the most recent pre-M&A month. For the matching, we use the Abadie and Imbens (2006) distance metric. The unit of analysis is unique at the firm-month-cohort level. The estimation period goes from -24 months to +60 months around the date of the closing of the M&A deal. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are double-clustered by firm and month. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Sales, Pricing, and Units

	Sales	Average Prices	Number of Units Sold
After	-0.167	-0.001	-0.158
	(-0.86)	(-0.04)	(-0.91)
Adj. R-Square	0.852	0.955	0.867
N. Obs.	13,340	13,340	13,340
Firm-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes

Panel B: Product Innovation

	Number of Products	New Products	Discont. Products	Number of Categories
After	-0.025	0.099	0.056	-0.027
	(-0.49)	(1.21)	(1.44)	(-0.82)
Adj. R-Square	0.916	0.381	0.716	0.927
N. Obs.	13,340	13,340	13,340	13,340
Firm-Cohort FE	Yes	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes	Yes

Panel C. Product Availability

	N. Stores	N. Chains	N. ZIP Codes
After	-0.172	-0.133**	-0.144
	(-1.39)	(-2.34)	(-1.56)
Adj. R-Square	0.895	0.924	0.890
N. Obs.	13,340	13,340	13,340
Firm-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes

Table A9. Private Equity, Sales, and Prices - Excluding Acquisitive Firms

This table presents OLS coefficient estimates from regressing logs of sales (Column 1), average monthly prices (Column 2), and units sold (Column 3) on *After*, a dummy equal to one for the post-buyout months for firms (Panel A) or firm-categories (Panel B) that underwent a buyout during our sample period. The sample excludes firms in the top decile of acquisitiveness. Each cohort is a pair of treated-untreated firms (panel A) or firm-categories (panel B) where the treated unit is matched to the untreated unit with the closest distance at the time of the private equity deal based on sales, unique UPCs sold, and store locations, all during the most recent pre-buyout month, and growth in monthly sales from the past 12 months to the most recent pre-buyout month. For the match, we use the Abadie and Imbens (2006) distance metric. The unit of analysis is unique at the firm-month-cohort level in panel A and at the firm-product category-month-cohort level in panel B. The estimation period goes from -24 months to +60 months around the date of the closing of the private equity deal. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are double-clustered by firm and month. $^{*}p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Within Firm

	Sales	Average Prices	Number of Units Sold
After	0.450***	0.058***	0.387***
	(3.81)	(2.98)	(3.58)
Adj. R-Square	0.873	0.932	0.891
N. Obs.	30,016	30,016	30,016
Firm-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes

Panel B: Within Firm-Category

	Sales	Average Prices	Number of Units Sold
After	0.188***	0.036***	0.145**
	(3.06)	(3.90)	(2.59)
Adj. R-Square	0.868	0.916	0.884
N. Obs.	206,730	206,730	206,730
Firm-Cat.-Cohort FE	Yes	Yes	Yes
Date-Cat.-Cohort FE	Yes	Yes	Yes

Panel C: Within Product-Store

	Sales	Price	Number of Units Sold
After	0.01944	0.00733*	0.00542
	(0.95)	(1.67)	(0.32)
Adj. R-Square	0.885	0.785	0.675
N. Obs.	718,937,916	718,937,916	718,937,916
UPC-Store-Cohort FE	Yes	Yes	Yes
Date-Store-Cohort FE	Yes	Yes	Yes

Table A10. Private Equity and Product Innovation - Excluding Acquisitive Firms

This table presents OLS coefficient estimates from regressing the log of number of products (Column 1), a new product dummy (Column 2), a discontinued product dummy (Column 3), and the log of number of product categories (Column 4) on *After*, a dummy equal to one for the post-buyout months for firms (Panel A) or firm-categories (Panel B) that underwent a buyout during our sample period. The sample excludes firms in the top decile of acquisitiveness. We measure the number of products by counting products that a firm or firm-category has on the shelves in at least one store in that month. The New Products variable is the number of products introduced by the firm or firm-category in that month. The Discontinued Products variable is the number of discontinued products by the firm or firm-category in that month. We measure number of categories by counting the categories in which a firm has at least one product on store shelves in that month. Each cohort is a pair of treated-untreated firms (panel A) or firm-categories (panel B) where the treated unit is matched to the untreated unit with the closest distance at the time of the private equity deal based on sales, unique UPCs sold, and store locations, all during the most recent pre-buyout month, and growth in monthly sales from the past 12 months to the most recent pre-buyout month. For the match, we use the Abadie and Imbens (2006) distance metric. The unit of analysis is unique at the firm-month-cohort level in panel A and at the firm-product category-month-cohort level in panel B. The estimation period goes from -24 months to +60 months around the closing date of the private equity deal. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are double-clustered by firm and month. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Within Firm

	Number of Products	New Products	Discont. Products	Number of Categories
After	0.107***	0.404**	0.221	0.053**
	(3.08)	(2.00)	(1.51)	(2.18)
Adj. R-Square	0.940	0.515	0.734	0.948
N. Obs.	30,016	30,016	30,016	30,016
Firm-Cohort FE	Yes	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes	Yes

Panel B: Within Firm-Category

	Number of Products	New Products	Discont. Products
After	0.025**	0.053**	0.044**
	(2.00)	(2.48)	(2.19)
Adj. R-Square	0.918	0.534	0.731
N. Obs.	206,730	206,730	206,730
Firm-Cat.-Cohort FE	Yes	Yes	Yes
Date-Cat.Cohort FE	Yes	Yes	Yes

Table A11. Private Equity and Product Availability - Excluding Acquisitive Firms

This table presents OLS coefficient estimates from regressing the logs of number of stores (Column 1), number of retail chains(Column 2), and number of 3-digit ZIP codes (Column 3) where a firm or firm-category is present on *After*, a dummy equal to one for the post-buyout months for firms (Panel A) or firm-categories (Panel B) that underwent a buyout during our sample period. The sample excludes firms in the top decile of acquisitiveness. Each cohort is a pair of treated-untreated firms (Panel A) or firm-categories (Panel B) where the treated unit is matched to the untreated unit with the closest distance at the time of the private equity deal based on sales, unique UPCs sold, and store locations, all during the most recent pre-buyout month, and growth in monthly sales from the past 12 months to the most recent pre-buyout month. For the match, we use the Abadie and Imbens (2006) distance metric. The unit of analysis is unique at the firm-month-cohort level in panel A and the firm-product category-month-cohort level in panel B. The estimation period goes from -24 months to +60 months around the closing date of the private equity deal. The regressions are estimated using the fixed point iteration procedure implemented by Correia (2014). Standard errors are double-clustered by firm and month. *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Panel A. Within Firm

	N. Stores	N. Chains	N. ZIP Codes
After	0.254***	0.113***	0.152***
	(3.36)	(3.63)	(2.79)
Adj. R-Square	0.906	0.949	0.898
N. Obs.	30,016	30,016	30,016
Firm-Cohort FE	Yes	Yes	Yes
Date-Cohort FE	Yes	Yes	Yes

Panel B. Within Firm-Category

	N. Stores	N. Chains	N. ZIP Codes
After	0.116**	0.048***	0.089**
	(2.50)	(2.62)	(2.59)
Adj. R-Square	0.890	0.921	0.883
N. Obs.	206,730	206,730	206,730
Firm-Category-Cohort FE	Yes	Yes	Yes
Date-Category-Cohort FE	Yes	Yes	Yes

Private Equity in the Global Economy: Evidence on Industry Spillovers

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Private Equity in the Global Economy:

Evidence on Industry Spillovers

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Abstract

Using a novel dataset on global private equity investments in 19 industries across 52 countries, we find that labor productivity, employment, profitability, and capital expenditures increase for publicly-listed companies in the same country and industry as private equity investments. Our results show that positive externalities created by private equity firms are absorbed by other companies within the same industry. Consistent with prior literature on competitive spillovers, these effects are more pronounced in country-industries with higher levels of competition, stronger institutions, and moderate levels of technological development suggesting that the competitive pressures from private equity-backed firms cause industry peers to react.

1 INTRODUCTION

Several studies find evidence of improvement in firm performance following a private equity (PE) transaction (Cumming et al. 2007, Guo et al. 2011, Kaplan and Stromberg 2009). However, little is known about how PE transactions impact competing firms in the same industry. This is surprising given frequent negative publicity about PE transactions adversely spilling over to the broader industry; for example, legislation recently introduced in the United States Senate seeks to prevent private equity firms from "looting companies" and "toppling markets."[1] Using a novel dataset on actual PE investments between 1990 and 2017, this paper explores the dynamic relationship between PE investments and performance characteristics (such as productivity growth, employment growth, capital expenditures, etc.) of public market firms in the same industry over the next several years.

Our analysis builds on the idea of "knowledge spillovers" dating back to Marshall (1890). A large literature has examined how technological advancements and productivity gains at some companies spill over to the other companies within the same industry. In a review of studies related to spillovers from multinational corporations onto local companies, Blomstrom and Kokko (1998) conclude that technology and productivity spillovers take place within an industry as companies compete and directly interact with each other or knowledge is transferred through employees.[2] Similar to FDI, operational and financial changes made by PE firms after a transaction likely create positive and negative spillovers for the industry as a whole. [3] Hence, how overall industry dynamics change following PE investments is an important issue for understanding if documented economic gains from PE investments increase welfare or simply come at

[1] See "Elizabeth Warren Takes Aim at Private-Equity Funds," *The Wall Street Journal*, July 18, 2019, p. A1.
[2] Blomstrom and Persson (1983), Bolmstrom (1986), Blomstrom and Wolff (1994), Caves (1971), Javorcik (2004), Kokko (1994) and Kokko (1996) and are examples of studies providing evidence for the existence of spillovers from foreign multinationals to domestic companies.
[3] One channel of spillovers is that the other firms copy the best practices and new technologies of the private equity-backed firms. It could also be the case that they are forced to come up with their own practices and technologies to become more efficient in order to keep up with the competitive pressure from the more efficient private equity-backed firms.

the expense of comparable losses at other (often publicly traded) firms in the industry.

One exception to the dearth of research on the broader economic impact of PE transactions is Bernstein et al. (2016). In their study of 26 OECD countries, they document that industries with at least one PE transaction in the past five years grow faster in terms of employment and productivity and are less exposed to aggregate shocks. The focus of their paper is aggregate industry performance, rather than peer firms within the same industry, and therefore they are not able to determine if the effects they document are driven by the improvements at PE target firms or competing firms. Thus, our study is the first to directly show that benefits spillover to other companies within the same industry. Additionally, our larger sample of 52 countries provides the first evidence on the impact of PE investments in developing economies as well as novel cross-sectional evidence regarding how the spillover effects are different for countries and country-industries with different institutional characteristics. This variation is an important tool for causal inference as well and allows us to identify specific country and industry characteristics associated with large PE spillovers. The dataset used in the paper is provided by Burgiss and is unique in its detailed coverage of PE investments at the global level. Investment values are aggregated using actual portfolio company investments by both buyout and venture capital funds. The data cover a total PE capital of $1.9 trillion invested in 52 countries across 19 industries from 1990 to 2017 and is the first dataset providing actual dollars of invested PE capital at the industry level across a large number of countries.[4]

As a motivating example for our analysis, consider the buyout of the rental car company Hertz Corporation in 2005. Hertz's performance improved significantly following the transaction, but more to the point of this study, Hertz's two main competitors, Avis-Budget and Dollar-Thrifty, soon implemented new strategies to increase efficiency, perhaps triggered by competitive pressure from the increasingly efficient Hertz. For example, over the two years following the buyout of Hertz, profitability and productivity

[4] Harris, Jenkinson and Kaplan (2014) also use private equity fund flow data supplied by Burgiss in their study of private equity fund performance. Brown et al. (2015) compare Burgiss and other commercially available data sets in terms of what they say about private equity performance.

increased at both Avis-Budget and Dollar-Thrifty.[5] We can illustrate the empirical strategy in this paper by examining a specific observation in our data set: the food and beverage industry in Thailand. In 1999 the Food and Beverage Industry received its largest injection to date of $29 million in buyout capital. Figure 1 depicts how overall industry employment, sales, and capital expenditures changed for Thai public companies in the Food and Beverage Industry. All three measures increased significantly over the three years following the increase in buyout investment. These trends are consistent with positive spillovers from PE onto the public companies within the same industry.

However, many factors may affect country-industry characteristics, so our empirical analysis must separate the impacts from PE investments from other factors. Likewise, it can be a challenge to determine causality due to the potential endogenous nature of discretionary actions (in this case, PE investments). For our analysis, we utilize a panel vector autoregression (panel-VAR) method to alleviate problems of reverse causality. This approach conditions effects on the recent history of industry-country characteristics' and PE investment. We therefore control for past values of employment growth, profitability growth, and labor productivity growth that may be associated with the amount of PE capital invested in an industry overall.

Our results indicate a statistically significant link between PE investments and the real economy. We find that PE capital invested in an industry leads to higher employment growth, profitability growth, and labor productivity growth within the *public firms* in the *same domestic industry* over the next few years. On average, following a one standard deviation increase in the amount of PE capital invested (adjusted by industry sales) employment growth increases by 0.6%, labor productivity growth increases by 0.8%, and profitability growth increases by 2.9% within one year. These effects are economically large – each is on the order of a one standard deviation increase in aggregate growth rates. Private peer firms are also likely to experience spillovers and we are possibly underestimating total industry spillovers. However, we focus on public firms for two reasons. First, we do not have the data on private firm performance at the country-

[5] See the appendix for a more detailed discussion of the buyout of Hertz Corporation.

industry level. Second, as private firms are more likely to receive PE investment and our data does not allow us to observe firm-level PE investments, focusing on public firms helps us to more clearly identify the spillover effects rather than performance improvements at PE targets.

Given the different goals and structures of buyout and venture capital (VC) transactions, we compare how the impact of private equity on the performance of public firms is different after buyout versus VC investments. Overall, our findings are intuitive and indicate that buyout investments are more likely to lead higher profitability through operational spillovers, while venture capital investments create positive industry-wide externalities through the introduction of new technologies and innovation. We also examine the dynamic relationship between industry-wide investment among the public firms and private equity and find that higher levels of private equity investment lead to higher growth in industry-wide capital expenditures. This suggests that private equity companies not only contribute to short-term performance advancement but also facilitate long-run growth through more real investment at the industry-level.

Our large panel of country-industries enables us to provide the first evidence on PE spillovers in developing nations and explore country and industry characteristics that are necessary for spillovers to be realized. These comparisons also enhance our power to test for the existence of a causal effect of PE on industry spillovers. We find that the impacts of PE investments are concentrated in country-industries with higher levels of competition which is consistent with the hypothesis that spillovers come from competitive pressures applied by more efficient PE-backed companies. Strong legal institutions are necessary for PE companies to better implement the governance structures that make their portfolio companies more efficient (Cumming and Walz 2009). Consistent with this hypothesis we find positive spillover effects onto the public companies are stronger in countries with better quality legal institutions and intellectual property rights.[6] The existing evidence on spillovers from foreign direct investments (FDI) shows that productivity

[6] The protection of intellectual property rights is particularly important as it impacts how extensive the private equity companies would introduce new technologies at their portfolio companies. Similarly, Mansfield (1994) finds that technology spillovers are weakest in countries with weak intellectual property protection.

spillovers are strongest for companies in countries with moderate levels of technological advancement. (Kokko 1994, Kokko et al. 1996). We also find that PE investment spillovers are strongest in countries with moderate levels of innovative capacities. Overall, these results provide additional support for a causal effect of PE investments on industry spillovers given that no alternative explanations we are aware of would predict these cross-sectional differences.

While the existing research on PE has utilized mostly data on U.S.-based funds, studying global investments in a cross-country setting is important for two reasons. First, while PE fund formation was primarily a U.S. and U.K. phenomenon pre-1990, by 2017 about 30-40% of the total global PE capital was invested in countries other than the U.S. and the U.K. and yet there is little research on global PE activity.[7] Second, the cross-section of countries allows for the study of the different impacts of PE in countries and industries with different characteristics as noted already.

We conduct a range of tests (discussed below) to gauge the robustness of our results. While it is theoretically possible that PE funds have better foresight than other investors about industry prospects and invest accordingly, this alternative explanation cannot explain our cross-sectional findings. Moreover, even if PE funds have some industry foresight not captured by the panel-VAR, our findings are still important as they provide evidence on PE companies facilitating industry growth by identifying this potential growth and allocating capital accordingly.

This paper contributes to several literatures in finance and economics. First, we build on the growing body of studies that examine how company performance changes after PE transactions (Cao and Lerner 2009, Davis et al. 2009, Kaplan 1989). With the evidence for positive spillover effects at the industry-level, our results support and complement the existing firm-level evidence. Second, our work contributes to the existing spillovers literature by exploring spillover of management practices, knowledge, and technology from PE-backed companies to public companies within the same industry. We provide

[7] See Figure 3 and Table 1, discussed subsequently in the data section.

evidence for a different channel for spillovers other than multinational corporations which is the most discussed channel in the literature.[8] Finally, we also contribute to the large literature of finance and growth that examines the link between financial development and economic growth. Existing studies look at how the development of a country's public and credit markets affects output growth by providing a better allocation of capital (King and Levine 1993, Levine 2004). We consider the impact of a different financial asset class, PE, and show that its entrance into an industry also enhances industry growth by creating positive externalities within the industry.

The remainder of the paper is organized as follows. The next section further discusses the related literatures and how the paper fits in. Section 3 introduces the data and presents some descriptive analysis. Section 4 outlines the empirical strategy using the panel-VAR approach. Section 5 presents our main results and robustness tests. Section 6 concludes.

2 RELATED LITERATURE

Jensen (1989) argues that PE ownership, as compared to public equity ownership, can be a superior ownership structure as it provides a better alignment of incentives between owners and managers as well as a more efficient management of resources.

According to Kaplan and Stromberg (2009), PE companies improve their portfolio companies using practices that can be summarized under three main headings: financial engineering, governance engineering and operational engineering. Financial and governance engineering refer to changes in the structure of ownership and financing that may lead to better monitoring and incentive alignment to overcome agency problems at the portfolio companies. Operational engineering refers to management practices that PE owners use to improve operational efficiencies of their portfolio companies. Firm-level performance after

[8] This is an important contribution as 'the degree to which other modes of international business (besides traditional inward FDI) generate appropriate spillover benefits for the host country is an exceedingly important policy issue for which there is a disappointing amount of evidence.' (Blomstrom et al. 1999, p.15).

PE transactions has been examined in the existing literature in studies looking at transactions in the U.S., the U.K. or European Union countries. Kaplan (1989) tracks large management buyouts of publicly held companies and finds evidence for improved operating performance at these companies as well as increased market values. Similarly, Muscarella and Vetsuypens (1990) study reverse leveraged buyouts (LBO), and find that profitability at target companies increases following the transactions. More recently, Cressy et al. (2007) study U.K. buyouts over the period 1995 to 2002 and find that profitability of PE-backed companies is higher than those of comparable companies over the initial years following the buyout. They further find that industry specialization of PE firms enhances this improvement in profitability. Davis et al. (2009) show that U.S. firms receiving PE investment experience higher subsequent productivity growth. Similarly, studying a sample of PE-backed companies in Western Europe, Acharya et al. (2009) also find evidence for performance gains related to PE investments. Amess et al. (2016) find that LBOs have a positive impact on patent production suggesting that PE firms do not cut long-term investments for the sake of short-term profits. Complementing the existing evidence on operating performance, Cao and Lerner (2009) provide evidence for superior stock market performance for reverse LBOs. On the employment side, Davis et al. (2011) examine establishment-level job creation and destruction at U.S. establishments using data from the U.S. Census Bureau. They find that PE-backed companies have higher job destruction at existing establishments, but at the same time higher job creation at new establishments. Similarly, Popov and Roosenboom (2008) find that venture capital leads to higher new business creation in their study of 21 European countries over the period 1998 to 2008. Agrawal and Tambe (2016) find that employees at PE-backed firms acquire more valuable skills than employees at non-PE firms contributing to better long-run employability. Bernstein and Sheen (2016) also find evidence for gains in human capital to employees of PE-backed companies in their study of PE buyouts in the restaurant industry. On the contrary, Goergen et al. (2016) find evidence for lower employment in their sample of U.K. firms following institutional buyouts as well as no evidence of improvement in productivity or profitability.

Most of the existing studies on PE transactions have found evidence for superior subsequent performance at the firm-level. However, it is still unknown how PE transactions affect the other firms, which do not receive PE capital, within the same country and industry. There is a large established literature that has provided evidence for the existence of productivity spillovers.[9] For example, several studies on different countries, including Caves (1974) on Australia, Globerman (1979) on Canada, and Blomstrom and Persson (1983) on Mexico, demonstrate positive spillover effects from FDI to domestic industries (see Blomstrom and Kokko, 1998 for a detailed review). Similarly, Bernstein and Nadiri (1989) provide evidence for research and development spillovers within an industry and find that overall costs in an industry decline following improvements in technology as knowledge migrates to other firms. On the other hand, Aitken and Harrison (1999) find that the entrance of foreign companies negatively impacts the performance of local firms.

Related to our study, there has been some work on the effects of LBOs on peer firms. Chevalier (1995a, 1995b) studies the impact of LBOs on product market competition and pricing at the rival firms showing that LBOs have broad implications for the industry. Phillips (1995) and Kovenock and Phillips (1997) study the impact of LBOs on rival firms' decisions regarding plant closures and investments. However, all of these studies concern the implications of the capital structure of the LBO firms rather than operational spillovers from the PE target firms. An exception is the recent study by Harford et al. (2015) which finds that peer firms increase real investment and enter into more strategic alliances following an LBO in the industry. On the contrary, Hsu et al. (2010) also study competitive effects of PE investments and conclude that competitors of PE targets experience a decline in stock prices and operating performance. In addition, Cumming, Johan, and Zhang (2014) find evidence of a positive impact of entrepreneurship on

[9] The idea of spillovers was first introduced by Marshall (1890) in the form of knowledge spillovers among firms, and then improved by Arrow (1962), and Romer (1986). Later, Glaeser et al. (1992) put the ideas together and defined the Marshall-Arrow-Romer (MAR) model of knowledge spillovers, which argues that knowledge is industry specific and spills over within an industry once its created.

the real economy (GDP per capita, exports, patents, and unemployment). Our findings complement their country-level results by providing evidence for positive industry-level spillovers from private investments.

This paper also complements a recent study by Bernstein et al. (2016). In their study of 26 OECD countries between 1991 and 2007, they find that industries with at least one PE transaction in the past five years grow faster in terms of employment and productivity. They, however, do not find evidence for differences between industries with high versus low amounts of PE capital. There are several significant differences between our paper and theirs. First, they look at the overall industry performance following a PE transaction, including the companies receiving PE capital and do not specifically explore spillovers, while we focus on aggregate industry measures of publicly listed companies only. This allows us to clearly identify the spillover effects from PE-backed companies to companies that do not receive PE capital within the same industry. Second, their measure of PE is the existence of a PE transaction in an industry, whereas we look at actual dollars of PE capital invested. Third, they study a sample of OECD countries between 1991 and 2007, while we study 52 countries, including both developed and developing nations. This comprehensive sample does not only allow us to provide the first evidence on the impact of PE investments in developing nations but also to compare how the spillover effects are different for countries and country-industries with different institutional characteristics (which can also provide a tool for causal inference). Finally, our analysis includes an additional 10 years from 2008-2017 which has seen substantial growth in PE investment globally.

3 DATA AND DESCRIPTIVE STATISTICS

3.1 Data

The PE investment data come from Burgiss, a services company providing record keeping and performance analysis to the largest institutional investors in the PE universe. The major advantage of this dataset over others is that Burgiss sources its data exclusively from limited partners, as opposed to general partners

(GP); so, the typical biases associated with GP-sourced datasets are not present.[10] Brown et al. (2015) compare different commercial PE datasets in what they say about PE performance. For detailed information about Burgiss and its coverage of the PE universe, see Harris et al. (2012) and Brown et al. (2011).[11]

The primary variable from the Burgiss data is the amount of PE capital measured in U.S. dollars, including both buyout and venture capital, at the country-industry-year level over the period 1990 to 2017. The data cover more than 100 countries though we examine only 52 because of other data limitations and sparse PE investment in some countries. Burgiss provides company-level PE investments aggregated to the industry-country level based on the Industry Classification Benchmark (ICB). So, an example of a unit of observation used in our analysis would be the *U.S. dollar equivalent amount of PE capital invested* in *India* in the *technology* industry in *2003*. This is the first dataset having actual dollar amounts of PE capital invested at this level of detail globally. As the dataset is unique in its coverage of PE investments around the globe, we start with some basic descriptive analysis.

Table 1 presents the distribution of PE capital invested globally among 52 countries in the sample from 1990 to 2017. Panel A ranks the countries based on the total dollar amount of PE capital received, with amounts in million U.S. dollars and inflation adjusted to 2017. Column 1 shows that the U.S. and the U.K. have received the most capital. While venture capital makes up about 25% of total capital invested in the U.S., its share is only about 5% for the U.K. Developed European countries receive large amounts of PE investment – similar to the U.K., around 95% of capital received is associated with buyout activity. China and India rank 3rd and 8th, receiving more than $72 billion and $26 billion of PE investment, respectively. About 55% of the total is venture capital in China, whereas VC makes up 43% of investment in India.

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[10] GP-sourced databases on private equity may have significant biases as GPs strategically stop reporting. In many cases, Burgiss cross-checks data across different investors in the same fund which leads to a high level of data integrity and completeness.

[11] We note that the Burgiss data primarily covers funds of "institutional" quality. However, investments from the large institutional investors constitute the vast majority of the total private equity capital raised around the world.

In Panel B, countries are ranked by the total amount of PE capital received as a percentage of the GDP.[12] The U.S is again on the top of the list, while Luxembourg, Sweden and Denmark rank 2nd, 4th and 5th while they were ranked only 31st, 9th and 17th in Panel A, respectively. Israel, Hong Kong, and Singapore also rank high. China and India, on the other hand, move down the list considerably. Overall, Table 1 shows that PE has become global, and although U.S., U.K., and other big European countries remain big hubs for PE investments, emerging economies such as China, India, and Brazil also get a large portion of the total PE capital invested globally.

Table 2 provides the industry distribution of total PE, buyout and venture capital investment globally from 1990 to 2017. The technology sector receives the highest amount of capital, followed by industrial goods and services, and health care. The technology sector has more venture capital than buyout ($205 versus $176 billion) while a very big portion of PE capital invested in industrial goods and services is buyout (only 7% of total invested capital is venture capital). The health care sector receives $251 billion of PE capital in total, with 37% of it being venture capital. In almost all the other sectors, buyout capital makes up more than 90% of the investments with the exception of telecommunications where the share of venture capital is around 20%. Overall, Table 2 shows that venture capital investment is more concentrated in R&D intensive sectors such as technology and health care, while buyout capital dominates most other sectors.

The data on industry performance variables come from DataStream's Global Equity Indices that provide accounting as well as market price data for different industries in 53 countries using the Industry Classification Benchmark (ICB). DataStream's indices cover over 75,000 securities worldwide. The industry-wide measures are calculated using data from financial statements of publicly listed companies whose stocks cover at least 75% of the total market capitalization in each country-industry. The PE investment data are matched to the industry performance data at the country-industry-year level using the

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[12] The ratio is calculated separately for each year between 1990 and 2017, and then the average is reported for each country.

ICB classification. Additional country-level variables used in the analysis come from World Bank's World Development Indicators (WDI), which are then matched to the other data by country and year. Country-level data on legal environment, such as the quality of institutions and intellectual property rights, and level of innovative capacity come from World Economic Forum's Global Competitiveness Index database. The final matched dataset has around 17,000 country-industry-year observations covering 52 countries and 19 industries.

Table 3 presents summary statistics for the industry- and country-level variables. Variable definitions are provided in Table A1. Over the sample period, industry-wide employment grew an average of 5.9% annually, while median employment growth is 1.8%. Labor productivity growth averages 4.2% annually. These are fast rates for industry-wide employment and productivity growth, but we note that the sample includes developing economies where production in industries outside of agriculture often grows rapidly. Average (median) net profit margin growth is 0.3% (0.8%). Panel B presents summary statistics for country-level variables. Average (median) GDP growth is 2.1% (2.2%). Public market is a measure of the liquidity of a country's stock markets, measured as the total value of stocks traded as a percentage of GDP. The average (median) market value of public equities is 40% (15%) of GDP. Similarly, private credit to GDP is a proxy for the credit market development of a country, measured by the total amount of credit given to the private sector as a percentage of GDP. Private sector credit is on average 78% of a country's GDP.

3.2 Univariate Comparisons of Performance across Country-Industries

In Table 4, we compare average and median employment growth, profitability growth and productivity growth, along with some other variables, in subsamples of country-industry-years. Columns 1 and 2 of Panel A present mean (median) values for the subsamples based on a PE indicator which takes the value of 1 if the country-industry received capital in that year, and 0 otherwise. Column 3 presents *p*-values for the mean (median) difference between these subsamples from a *t*-test (Wilcoxon rank-sum test). Average employment growth and profitability growth are both higher in country-industries with a PE investment, while average labor productivity growth is higher in the subsample of country-industries that did not receive any PE capital. The negative association with productivity growth might reflect PE companies choosing less productive country-industries to invest where there is more room to add value. The average growth in capital expenditures, net debt and industry returns are not significantly different among the subsamples.

Next, we limit the sample to country-industry-years with positive amounts of PE capital invested, and compare means among subsamples of high versus low levels of investment. Results are presented in Columns 4, 5 and 6 in a similar fashion. Several of the results are similar to the earlier comparison. Country-industries that received higher amounts of PE capital have faster growth in employment, profitability, and capital expenditures on average at the time of investment, and the differences are larger in magnitude. In contrast to results reported in Panel A, industries that receive higher amounts of PE capital seem to have higher industry returns and debt growth, not controlling for other differences.

Overall a couple of observations can be made from the univariate comparisons. Industries that receive PE capital have higher employment and profitability growth than industries that do not, and among the industries that received investment, those with higher amounts of capital experience faster growth. Labor productivity growth seems to be lower in industries with PE investments, but among the industries with PE investments, there is no statistically significant difference. These results suggest a possible positive relationship with the level of PE capital invested in an industry and employment growth, profitability growth, and capital expenditures growth. The relationship of PE with productivity growth, on the other hand, seems to be ambiguous.

Although these results are suggestive, it is not possible to draw conclusions about the relationship between PE and industry performance given the host of other factors that determine industry characteristics. For example, the decision of PE companies to invest in a specific industry and country is likely determined by industry growth, efficiency, etc. Thus, we exploit the panel nature of the dataset and utilize a Vector

Autoregression (VAR) model on the panel of country-industry-years to address the issue of jointly determined variables.

4 EMPIRICAL STRATEGY – PANEL-VAR

A VAR is a system consisting of N linear equations with N variables where each variable is explained by its own lagged values together with the current and past values of the remaining $N - 1$ variables in the system. After being introduced by Sims (1980), it has been widely used to explain the dynamic behavior of multivariate economic and financial time series. The main advantage of this estimation methodology is that it treats all the variables in the system as endogenous which leads to a better identification of the dynamic relationships between the variables in the system. In the absence of exogenous instruments, VAR estimation is useful for addressing issues related to endogenous variables.

Although the VAR approach is long-established, it has not been widely used on panel data until more recently. Love and Zicchino (2006) apply a VAR model on firm-level panel data from 36 countries in their study of the dynamic investment behavior of firms in an attempt to isolate the impact of financial factors from fundamental factors that affect firm investment. We follow their empirical methodology and apply a VAR to our panel of country-industry data. In addition to utilizing the time-series component of the data treating the variables in the system as endogenous, the panel-VAR also allows for unobserved individual heterogeneity by including country-industry fixed effects in the estimation. More specifically, following Love and Zicchino (2006), we estimate a panel-VAR system of the following form:

$$X_{ci,t} = \alpha_0 + \alpha_1 X_{i,t} + \mu_{ci} + \tau_t + E_t \text{ , (1)}$$

where

$X_{ci,t}$ is a matrix consisting of industry-level variables and a measure of PE capital invested

$\mu\mu_{ci}$ are country-industry fixed effects

τ_t are time fixed effects.

Country-industry fixed effects are included to control for any unobserved time-invariant individual heterogeneity in the variables. In a single model specification, fixed effects may be removed by demeaning all the variables in the model at the individual observation level (country-industry in this case). However, in this type of VAR specification, where all variables are instrumented by their lagged values, fixed effects introduced by demeaning would be correlated with the regressors violating the exclusion restriction of the instruments. To avoid this problem, we apply a forward-mean differencing method, also known as the "Helmert" procedure (Arellano and Bover, 1995), where only the forward-mean for every country-industry-year is removed. After the Helmert transformation, the model is then estimated using a system GMM where lagged values of the regressors are used as instruments. The specification also includes time-fixed effects to remove the effect of global macro shocks that might affect all the variables in the system.

In a VAR specification, the ordering of the variables in the estimation does matter. VAR methods assume that every variable in the system affects the subsequent variables both contemporaneously and with a lag, while later variables affect the previous ones only with a lag. In other words, variables that appear earlier in the ordering are assumed to be more endogenous. In the estimations throughout the paper, we assume that PE capital invested affects the industry variables both contemporaneously and with a lag, while it is impacted only with a lag. However, all the results in the paper stay the same when we change the ordering of the variables in the system.

The goal of the panel-VAR methodology is to identify the direction of causality between PE capital invested in an industry and industry growth in terms of employment, productivity, and profitability. It should be noted that as the industry measures are aggregated from publicly listed companies in an industry, the effect that will be identified would be a measure of spillovers from PE-backed companies to the public firms in the industry.

5 PE INVESTMENT AND SPILLOVERS WITHIN THE INDUSTRY

5.1 PE and the Real Economy

As discussed above, PE investments into a particular company may incentivize firms not receiving investments to improve efficiency by utilizing new technologies and practices to compete with increasingly efficient PE-backed firms. Thus, there are potential industry-wide externalities from the competitive pressure introduced by PE. If companies are capable of absorbing the spillovers from private-equity backed firms, the industry might experience overall performance gains. On the other hand, if the companies that do not receive PE investment cannot keep up with the new technologies and the competitive pressure, the efficiency gains at the PE-backed companies might drive away demand from their competitors.[13] Hence, the pressure might negatively affect the rest of the industry.[14]

In this section, we estimate a panel-VAR as in equation 1, where the X matrix includes the amount of PE capital invested in an industry, adjusted by industry sales, industry-wide employment growth, profitability growth and labor productivity growth. This method identifies the impact of PE capital on the growth in employment, productivity and profitability of the public firms in an industry so as to measure technology and productivity spillovers from PE backed-companies to the rest of the industry. PE transactions may also lead to integrations, reorganizations, and restructurings, changes in the level of competition, as well as human resource shifts within an industry, which all may impact the performance of peer companies and result in spillovers.[15]

If the PE companies generate spillovers, we should observe a positive impact on industry prospects and thus positive coefficients on the PE variables.[16] Table 5 presents the results of the panel-VAR

[13] Aitken and Harrison (1999), for example, find that the entrance of more efficient foreign companies negatively impacts the performance of local firms because they attract customers away from domestic firms. Djankov and Hoekman (2000), Feinberg and Majumdar (2001), and Kathuria (2002) are other examples of studies providing evidence for negative impacts of spillovers from foreign direct investments.

[14] Even if competitive pressures drive the most inefficient companies out of the market, this may still be beneficial for the economy as a whole since PE-backed firms act as catalysts of a creative destruction process.

[15] We thank an anonymous referee for the suggestion of these alternative channels for spillovers.

[16] Reverse causality could stem from private equity companies' predictions about the industry prospects. However, the cross-sectional evidence presented in Section 5.4 is consistent with a causal effect of private equity on industry spillovers, while an explanation of superior foresight would not have the same cross-sectional predictions.

estimation.[17] The first column shows how the amount of PE capital invested at time *t* is affected by employment growth, profitability growth and productivity growth at time *t* -1. Only the variable's own lag is statistically different from zero suggesting that PE capital invested is not affected by how the industry did a year ago in terms of employment, profitability, and labor productivity growth, after controlling for the amount of capital invested at time *t* -1 and removing country-industry, and time fixed effects. The significant coefficient on the amount of PE capital at time *t -1* suggests that PE capital is persistent, which is not surprising given that many PE (and especially VC) investments are completed in rounds.

The second column presents the results from the part of the estimation where the dependent variable is employment growth. Employment growth at time *t* is significantly affected by productivity growth, and profitability growth at time *t-1*. After an industry experiences faster growth in profitability and higher productivity, it also grows faster in terms of employment subsequently. The main variable of interest for the purpose of this paper is PE capital invested. It also has a positive and statistically significant coefficient indicating that public companies in industries that receive more PE capital experience faster employment growth following the investment. Recall that any unobserved time-invariant heterogeneity is removed by country-industry fixed effects in the estimation. Because employment growth is total employment growth of the public companies in the industry (which do not receive PE investment)[18], the measured effect is the spillover effect from PE-backed targets on to public industry peers. This result is consistent with the

[17] Although the paper provides results with a one-year lag VAR only, results do not change when we estimate VARs with two- or three-year lags. Existing statistical tests for the optimal number of lags cannot be applied to panel data. However, a likelihood ratio test between models with one, two and three lags indicates that the models with two or three lags do not fit significantly better than the model with one lag. Furthermore, Cochrane (2005) suggests that economic theory does not say much about the orders of autoregression terms, and short order autoregressions should be used to approximate for processes.

[18] We thank to an anonymous referee for drawing our attention to the fact that public companies might still be receiving private capital through PIPEs. When we check PIPE volumes though, we see that they represent a very small proportion of the overall public market universe – for instance, in the U.S. $39 billion of PIPE capital was raised in 2010 while the total market capitalization of public companies was above $17 trillion the same year. Hence, the improvements we document at public companies should largely represent spillovers rather than changes at public targets.

hypothesis that as PE-backed companies invest in an industry, other companies within the same industry also improve operations to stay competitive which results in industry-wide employment growth.

The third column shows how labor productivity changes following a PE investment into the industry controlling for changes in employment and profitability together with the growth in labor productivity from the previous year. The coefficient on the amount of PE capital invested is positive and significant at the 5% level indicating that overall industry labor productivity grows faster following the investment of PE capital. So, not only employment, but also labor productivity of public peers grows faster subsequent to PE companies investing in an industry. This result is consistent with PE companies introducing practices and technologies that increase operational efficiency of their portfolio companies, and these efficiency gains spilling over to higher productivity at competing firms.

While the second and third columns show the positive impact of PE on labor productivity and employment growth, they do not provide evidence about the cost effectiveness of these improvements. The higher growth of employment and labor productivity together suggest an increase in output, but do not show that this higher output is more profitable. For example, if output increases because of expensive new investments, profits may deteriorate. This, in turn, may lead to lower firm values. As such, it is important to see how profitability changes in response to the PE investment within the industry (fourth column in Table 5). The amount of PE capital again has a positive and statistically significant relation: after an industry receives PE investment, profitability grows faster in comparison to years with no or low PE capital.[19] This result indicates that the gains in productivity and growth in employment are also reflected in profits. The spillover effects documented here are comparable in magnitude to the effects found in Bernstein et al. (2016), the only other study looking at industry-level changes. [20]

[19] One might be concerned that profit margins increase due to a reduction in sales as profitability is measured as net profits over sales. However, in unreported tests we find that sales growth increases as well indicating that the increase in profitability results from higher sales and lower costs.
[20] However, we note that the Bernstein et al. (2016) industry measures include the companies that receive private equity investments while we examine changes in public companies within the same industry. Thus, we would expect

Overall, the results in Table 5 show that following PE investments into an industry, public companies within the same industry that do not receive investment experience higher employment, productivity and profitability. These results suggest that after some companies in an industry receive PE investment, other companies within the same industry respond in ways that generate value for the industry as a whole. This may result from the competitive pressure from the private-equity backed companies as well as from imitation of better practices implemented by the PE companies. Because the panel-VAR method controls for reverse causality (to the extent that PE companies make their decisions about investing in a particular industry based on what happened in the industry in the past), we can feel more confident that industry performance is not related to the amount of PE capital invested. This suggests a causal effect where PE investments lead to spillovers within industries.[21]

The reported results indicate that PE investments create positive externalities within an industry. As such, the gains at the rest of the industry suggest that private-equity backing leads to efficiency gains at the target company first, which then are also absorbed by the other companies within the same industry. Previous research has shown that PE leads to performance gains at the target companies.[22] The evidence indicates firm-level performance gains ranging from 10% to 40% depending on the study and the measure of operating performance examined. The economic magnitudes of the positive effects presented here are

their results to be larger in magnitude. Also, they use an indicator variable for the existence of a private equity transaction, making it harder to compare their results to the magnitudes documented here.

[21] This is to the extent that private equity investments are exogenous after incorporating the information about how the industry performed in the past. While this finding excludes reverse causality stemming from past values of industry performance affecting private equity investments, there might be other time-varying unobservables that are driving the findings. For example, one could argue that private equity companies have superior foresight and time their investments based on their expectations about the industry prospects going forward. As with any empirical work, it is impossible to perfectly control for expectations and fully exclude all alternative explanations. However, although no single finding would be conclusive by itself, the evidence provided throughout the paper, including the different findings for buyout versus venture capital as well as the cross-sectional findings, altogether are indicative of a causal relationship where private equity investments lead to spillovers. A full discussion of alternative hypotheses is provided in the robustness section.

[22] Kaplan (1989) and recently Guo et al. (2011) both have shown that profitability increases after buyouts. Furthermore, Lichtenberg and Siegel (1990) and Davis et al. (2009) provided evidence for productivity gains following buyouts. The higher growth in industry-level productivity and profitability confirm the findings of the aforementioned studies.

significantly smaller than firm-level effects, which is logical given that what is being captured here are spillover effects only. The effects become larger when looking at a two- or three-year window, but are still significantly smaller than the documented firm-level effects. Nonetheless, implied increases in growth rates are of the same order of magnitude as a one-standard-deviation change in aggregate growth rates for these variables.

5.2 Buyout versus Venture Capital

The previous section shows that PE investments lead to performance gains within the industry; however, we did not differentiate between the two main types of PE investments: buyout transactions and venture capital. In a typical buyout transaction the PE firm acquires a mature target firm by purchasing all of the outstanding equity, often with a significant amount of debt financing. Although buyouts are sometimes criticized for loading-up target companies with debt, cutting jobs, and reducing capital expenditures, the previously cited evidence shows that buyouts on average make their targets more efficient.

On the other hand, a typical venture capital transaction is an investment into a young and growing company, typically made in multiple rounds and without acquiring majority control. Venture capital investments often provide financing for small businesses that otherwise cannot get financing due to high risk and informational asymmetries. As such, venture capital prevents young companies from having to forgo positive investment opportunities. Additionally, venture capital investments are typically associated with an increase in the target company's innovative potential by allowing for more research and development investments and by providing expertise and guidance related to innovation. Kortum and Lerner (2000) provide evidence that venture capital investments indeed spur innovation, and this result is replicated in other studies using both U.S. and European data (see, Bernstein et al. 2016, Popov and Roosenboom 2012).

Given the very different structures of these two transactions and the different stages of financing they provide for the targets, they might also differ in terms of their impact on industry dynamics. To explore if buyout and venture capital have different impacts on industry dynamics, we repeat the panel-VAR analysis for buyout and VC separately. The results in Panel A of Table 6 indicate that the impact of total PE capital invested on industry employment and profitability are replicated when using buyout capital only. However, buyout transactions do not lead to significantly higher productivity at the industry level. We note though that this difference is largely driven by the difference in statistical power of this test as the estimated coefficient is quite similar. Nonetheless, these results suggests that buyout transactions may lead to improvements in profitability through other mechanisms, but the impact via the labor productivity channel is uncertain.[23]

Panel B presents the results with venture capital. The coefficient on lagged PE is larger for venture capital suggesting that VC investments are more persistent than buyout investments (and consistent with VC investments being conducted in multiple rounds). The impact of venture capital investment on employment growth and profitability growth is not statistically different from zero – however, we again note that this is a question of statistical power not a change in coefficient magnitude. So, unlike buyouts, we do not find reliable evidence for profitability gains or faster employment growth resulting from venture capital investments into an industry. Industry-level labor productivity, on the other hand, grows significantly faster following a venture capital investment. The finding that profitability and employment do not increase following venture capital investments, despite the significant increase in productivity, is consistent with costly investments in new technologies and more innovation that limit the need for labor (e.g., automation).

Venture capitalists typically make investments into small companies that are research and development intensive and have higher long-term growth opportunities. Therefore, we might expect little or no employment or profitability spillovers onto the rest of the industry in the short-run. Nonetheless, if

[23] This could be interpreted as buyouts leading to higher profits by cutting costs or through financial engineering, where higher leverage leads to tax benefits as well as disciplines managers with the pressure of making higher interest payments.

venture capital leads to innovation and productivity spillovers, this should lead to higher profitability and employment growth in the long-run. To investigate this possibility, we also estimate panel-VARs with 2- and 3-year lags (results not tabulated) and find some support for this hypothesis. The amount of venture capital invested at time *t-2* leads to significantly higher employment growth as well as higher profitability at time *t*.[24]

To summarize, while buyout investments lead to an increase in employment and profits we do not see an immediate (statistically significant) impact on these from VC. However, VC has an impact on productivity growth in the next year and the evidence suggests a longer time may be needed for impacts to be reflected in higher profits and employment growth after venture capital investments. The lack of significant productivity spillovers from buyouts suggests that the operational and financial improvements introduced by PE companies in buyout transactions are more focused at reducing costs and increasing profits, but do not necessarily lead to higher sales growth. The large impact of venture capital on productivity growth is consistent with the existing evidence showing a positive relation between venture capital and subsequent innovation (Kortum and Lerner, 2000; Mollica and Zingales, 2007; Bernstein et al., 2016). It is also consistent with Gonzales-Uribe (2016) which shows that venture capitalists spur innovation through a better diffusion of knowledge. The impact on industry productivity may also be related to the absorptive capacity of the industries receiving venture capital investment. Venture capital investments are more common in high R&D industries, which have been shown to better absorb spillovers due to higher levels of technical knowledge and human capital in the FDI spillovers literature (Kogut and Chang, 1991).

5.3 PE and Investment

[24] The effect of twice-lagged venture capital invested on profitability growth is larger in magnitude than the effect of lagged buyout capital invested. While a one standard deviation increase in lagged buyout invested leads to a 2% increase in profitability growth, a one standard deviation increase in twice-lagged venture capital invested leads to a 3% increase in profitability growth.

Some evidence documents reductions in capital expenditures at PE-backed companies (Kaplan, 1989). While reduced expenses might increase profitability in the short-run, it raises concerns about future cash-flows being sacrificed for short-term operational gains. In contrast, studies looking at stock market performance of PE-backed companies that are taken public provide evidence for superior returns, which indirectly suggests that long-run prospects are not hurt. To examine this issue directly, we estimate the panel-VAR model with growth in free cash flow, growth in capital expenditures, growth in market-to-book, and the PE measure to detect the dynamic relationship between industry-level investment and PE. Free cash flow is included to control for the sensitivity of investment to the availability of internal financing, while market-to-book is used as a proxy for investment opportunities. Table 7 presents the results.

The first column of Table 7 shows that the amount of PE capital is not related to past values of cash flow, capital expenditure, and market-to-book growth. The second column shows that growth in free cash flows at time *t* is not related to PE investments and capital expenditures at time *t-1* (after controlling for cash flow growth at time *t-1* together with country-industry and year fixed effects). The third column shows that capital expenditures grow faster following PE investments within an industry suggesting that technology spillovers resulting from PE companies lead to faster growth in capital expenditures at other firms.[25] This finding suggests that other companies within the same industry increase capital expenditures to compete with the PE-backed firms. As such, the increased level of investment in the industry will facilitate overall industry growth. This finding is also consistent with Harford and Kolasinski (2012) who find that PE transactions do not lead to underinvestment at the target companies, as well as with Lerner et al. (2008) who provide evidence on increased portfolio company patent productivity as an example of long-run investment after buyout transactions. More closely, our finding is consistent with Harford et al. (2015) who find that LBOs lead to higher R&D investment at a target's industry peers.

[25] As expected, lagged cash flow growth is also found to be positively related to capital expenditure growth suggesting that availability of internal financing facilitates investment as found in previous studies examining cash-flow sensitivity of investment. Furthermore, capital expenditure growth is also related to past values of market-to-book showing that investment increases in response to higher investment opportunities.

5.4 Spillovers and Country-Industry Characteristics

5.4.1 *Competitiveness*

The results so far suggest that there exist productivity spillovers from PE-backed companies in an industry to the public firms within the same industry. How much improvement PE companies provide for their portfolio companies and how well the resulting positive externalities are absorbed by the other firms within the same industry should depend on the characteristics of the country as well as the industry. In this section, we exploit the cross-section of countries and industries to investigate where the spillovers from PE-backed companies are most pronounced.

First, we explore the level of competition within a country-industry. Caves (1974) and Blomstrom and Kokko (1998) suggest that within industry competition leads to more productivity spillovers from FDI. Similarly, as in the example of the Hertz buyout, we expect the spillover effects from PE to be higher in more competitive industries. We investigate this and report results in Table 8 (estimation is identical to the earlier panel-VAR).[26] The table presents the main panel-VAR results on subsamples of country-industries with high versus low levels of competition. We use two proxies for the level of industry competition: industry-level gross margins and industry-level asset turnover ratios. Higher margins can be charged in less competitive industries; hence lower margins would proxy for more competition. Similarly, more competitive industries tend to have more efficient use of assets in generating sales; hence higher asset turnover ratios would proxy for more competition. As predicted, we find that the positive impacts on employment, productivity and profitability are concentrated in country-industries with higher levels of competition (lower margins or higher asset turnover) suggesting that the competitive pressure within an industry is indeed an important factor leading to spillovers.

5.4.2 *Legal Environment*

[26] For brevity, we only present the results for the private equity variable (the first row from Table 5) All the other results are identical: none of the industry variables at time t-1 are related to the amount of private equity capital invested at time t mitigating concerns about reverse causality.

Starting with the seminal work of La Porta et al. (1998) which examines the interaction of law and finance, many studies have examined the relationship between the legal environment, financial development and growth of a country. Lerner and Schoar (2005) show that legal origin and level of law enforcement affect the type and value of PE transactions. In countries with a weaker legal endowment, PE companies can add value by mitigating contractual shortcomings with private contracting; however, Balcarcel et al. (2012) show that reliability in the legal environment limits the flow of PE capital into countries with less developed legal systems. So, although PE might be more beneficial to countries with weaker legal institutions, the weak legal environment discourages PE investment and more importantly limits the implementation of technologies and practices that add value to the portfolio companies. Cumming and Walz (2009) find that PE companies have higher returns in countries with stronger legal conditions and conclude that external corporate governance mechanisms are necessary for PE companies to implement more efficient governance structures at the firm level. Blomstrom and Kokko (1998) discuss that efficient regulations and institutions in a country might lead to higher spillovers from multi-national corporations onto local companies, but they also note that there is not enough evidence to make a clear conclusion about the issue. Mansfield (1994) finds that the strength of a country's intellectual property protection has a significant effect on FDI flows as well as on the extent of technology transferred from U.S. firms to their foreign affiliates. This would suggest that the implementation of new technologies and practices is expected to be stronger in countries with stronger intellectual property rights.

Based on these arguments, we predict that the positive impacts documented earlier should be more pronounced in countries with stronger legal institutions as well as better intellectual property protection. We examine how the legal environment impacts spillovers by splitting the sample based on a measure of the quality of legal institutions and a measure of intellectual property rights from the global competitiveness index published by the World Economic Forum.[27] The results are presented in Table 9. Panel A-B and C-

[27] Institutional quality index combines information on the judicial efficiency, law enforcement, corruption, investor protection, and reporting standards in a country.

D present the results for countries with weak versus strong institutional quality and intellectual property rights, respectively. The results show that the effects are more pronounced for the subsamples of countries with stronger legal institutions and intellectual property rights. These findings underline the importance of a country's legal environment for PE companies to facilitate efficiency gains at their portfolio companies and create spillovers within the industry.

5.4.3 *Innovative Capacity*

Another important facet of spillovers is the ability of local companies to absorb them. A large literature in development economics argues that less developed economies will grow faster because they have lower diminishing returns to capital, and in the long-run, they will catch-up with developed economies (Barro, 1997).[28] In line with the catching-up theory, countries where initial inefficiencies are higher and skills are in short supply will be in greater need of the better practices and technologies potentially introduced by the PE companies. Hence, industries in countries with lower technology levels might benefit more from the entrance of PE capital. On the other hand, productivity spillovers might not take place in countries where starting technology levels are too low, because companies in such countries might be unable to provide a competitive response to PE-backed companies, and PE may lead to a crowding out of existing firms. Several studies have provided evidence on this issue suggesting that too large of a technological gap between the home country of a multinational corporation and the host country leads to smaller spillover effects. For example, Kokko et al. (1996) find that spillovers are only absorbed by companies that have moderate technology gaps with foreign firms.[29] To study the spillovers from PE investments in countries with different levels of technological advancement, we repeat the panel-VAR in subsamples of countries created based on a measure of innovative capacity. The innovative capacity score comes from the global competitiveness index created by the World Economic Forum. Table 10 presents the results. Panel A and B present the results for the subsamples of countries with low and high innovative capacity scores,

[28] For example, Blomstrom and Wolff (1994) show that the entrance of U.S. corporations into Mexico leads to a convergence in productivity levels of local Mexican firms and U.S. firms.
[29] Haddad and Harrison (1991), Cantwell (1989), and Kokko (1994) also find similar results.

respectively. Panel C presents the results for the rest of the countries, which have moderate levels of innovative capacities. As predicted, the positive impact of PE investments on industry growth is most pronounced for the countries with moderate levels of innovative capacities. While the coefficients are mostly positive for countries with the highest or lowest levels of innovative capacities, the results are not statistically different from zero. The results are identical when we use technological readiness score from the global competitiveness index as the proxy for the absorptive capacity of a country.

Overall, the cross-sectional findings in this section indicate that the positive impacts of PE capital on industry dynamics are most pronounced in countries and industries with specific characteristics. Three main conclusions can be drawn. First, the positive impacts of PE investments are concentrated in competitive country-industries suggesting that the competitive pressure is an important channel for spillovers. Second, stronger legal institutions are needed for manifesting positive spillovers. Third, the spillover effects are most effective in countries with moderate levels of technological development as these countries are not only still in need of the new practices and technologies introduced by the PE companies, but also have the sufficient level of technological development that enables them to absorb the spillovers.

Besides providing the first cross-sectional evidence of PE spillovers, the results in this section are also very important as they provide support for a causal effect of PE investments on industry dynamics. All the cross-sectional results are consistent with the argument that PE companies lead to positive externalities and spillovers within the industry, which are reflected in higher employment, profitability and productivity growth. It is hard to identify alternative explanations that would provide the same predictions about the results for the cross-section of countries and industries.

5.5 Private Equity and the Financial Economy

So far, our analysis has focused on how the real side of the economy is affected by PE. In this section, we study the impact of PE capital on two financial variables: leverage and stock returns. In buyout transactions, PE companies typically buy their target companies using high levels of debt which may lead to higher rates of financial distress and bankruptcy.[30] On the other hand, higher leverage can also be a source of value creation at the target companies by providing better incentives for management as well as tax benefits. Jenkinson and Stucke (2011) find that leveraged buyouts generate significant value by higher tax shields. Similarly, Guo et al. (2011) argue that about 30% of returns of PE transactions are due to the tax benefits of higher leverage. Thus, it is important to examine the implication of buyout capital for the overall debt level of an industry.

Additionally, the results so far have provided evidence for industry-wide performance improvements following PE investments. However, it is not shown what the implications are for share values. If these improvements are reflected in investor beliefs, we should observe a positive association between industry returns and the amount of PE capital invested. A thread of the PE literature has provided evidence that PE companies invest into industries/companies that recently had high stock market returns. Our panel-VAR approach allows us to examine two-way causality.

To investigate these questions, we estimate a panel-VAR model with the amount of PE invested, growth in industry-wide debt, and growth in the value of industry return index to examine the dynamic relationship between PE and the two financial variables. Table 11 presents the results. Panel A and B have the results for buyout and venture capital, respectively. The first columns of Panel A (B) show that the amount of buyout (venture) capital invested at time *t* is not related to debt growth at time *t-1*. The insignificant coefficient on lagged industry returns in the first columns of both panels contradicts the existing evidence that PE companies chase returns and further reduces concerns about reverse causality. The second column in Panel A shows that buyout capital is not significantly related to growth in industry-wide debt and there is no evidence of debt causing higher PE investment.[31] The third column in Panel A

[30] The existing evidence on this issue is mixed. Andrade and Kaplan (1998) find that 23% of large public to private transactions defaulted during the 1980s. Kaplan and Stromberg (2009), on the other hand, find that the average default rate of leveraged buyouts is lower than the average default rate for all U.S. corporate bond issuers.
[31] In a previous version of this paper with less up-to-date data, we found evidence for positive spillovers on industry-wide debt growth for earlier years of the sample. This is consistent with PE funds shifting their focus in more recent years toward operational engineering and away from financial engineering.

shows that buyout investments in an industry are not significantly related to stock values within a year. The second column of Panel B shows that there is no significant relationship between the amount of venture capital invested and industry debt. Similarly, the third column of Panel B shows that there is no significant relationship between venture capital and industry stock returns within a one-year time frame.

Overall, two conclusions can be drawn from the results in this section. First, financial structures PE companies are applying in their portfolio companies do not seem to be replicated by public peers. Second, the positive externalities created by the PE companies in an industry on the real side are not reflected in higher stock returns over the time frame we examine.

5.6 Robustness

5.6.1 *The Alternative Explanation of Market-Timing*

An alternative explanation for our findings is "market-timing" by PE funds. If PE companies have superior foresight about an industry's prospects, they could invest in a specific country-industry and this could drive the results we document. The panel-VAR controls for this to the extent that the expectations of the PE companies about the industry growth are shaped by how the industry did in the past. However, it would be impossible to fully exclude an information story where the PE companies have foresight based on additional information. Nevertheless, market-timing cannot be the only driver of the findings of the paper for several reasons.

First, existing evidence suggests that PE companies are not simply timing the market when investing in a portfolio company (see, Ball et al., 2011, and Gredil, 2019). Furthermore, if PE firms time investments in an industry, they would be expected to invest when equities are undervalued to generate superior returns when they exit. This suggests that industry stock returns would be higher following PE investments in contrast to the results we present in Table 11. Second, the cross-sectional findings presented in Section 5.4 are consistent with spillover effects from PE-backed companies to the publicly listed firms within the same industry while they are inconsistent with a market-timing explanation. Specifically, there is no reason why PE companies would have informational advantages in countries with better legal institutions but not in other countries. In contrast, it seems more plausible that informational advantages of PE companies are stronger in countries with weaker legal institutions where informational asymmetries are higher. Likewise, it is not clear why PE companies would be able to time the market in countries with a moderate level of technological capacity, but not in others. Finally, we predict and find that spillovers are stronger in more competitive country-industries, and it is again hard to determine why market-timing would work for competitive country-industries, but not for others. While none of the above explanations may be sufficient to fully exclude a market-timing hypothesis, when put together they support a causal link where PE investments lead to spillovers resulting in superior industry performance. Even if somehow selection could explain all the findings, our results still indicate that PE companies are facilitating economic activity by selecting the best sectors for investment and growth.

5.6.2 *Robustness Checks*

The panel-VAR allows us to utilize the time-series of the data and treat all the variables in the system as endogenous. However, it limits our ability to include additional control variables and a saturated set of fixed effects other than country-industry and time fixed effects that we already include in the panel-VAR. Consequently, to examine the robustness of our results, we estimate our models with different OLS specifications with the industry growth variables as the dependent variable. If the PE companies have a global investment function for investment, it would be important to control for country-level demand and supply shocks as well as industry-level global shocks across time. Hence, we include a rich set of fixed effects in our OLS specifications such as country, industry, and year fixed effects; country-by-industry and year fixed effects; country-by-year fixed effects; industry-by-year fixed effects; country-by-year and industry-by-year fixed effects. None of the fixed effects models change our results.

We also include a set of country-level control variables in our OLS specifications to allow for time-varying macro-economic and institutional factors that could be related to industry performance. Specifically, we control for GDP per capital growth, stock and credit market development, and country-level governance indicators such as rule of law, regulatory quality, shareholder rights and creditor rights together with country, industry, and time fixed effects.[32] Our results are very similar to our panel-VAR results. Additionally, when estimating OLS models we also include a measure of exogenous growth opportunities similar to Bekaert et al. (2007) to control for the PE fund expectations about the country-industry's prospects. Specifically, we utilize the world-wide price-earnings ratio for an industry which should capture growth options including expectations about the future for a specific industry in a country. The results stay the same when this measure is included in the specifications.

Because not all country-industries receive any PE capital in all years, the PE investment data has many values that are exactly zero. To determine if this truncation affects the statistical inference, we repeat our analysis with only the subset of non-zero observations. All of our results get stronger both economically and statistically with this subset. When industry measures and PE investments are aggregated to a broader 9-industry level, the results also stay the same. This provides further evidence against market timing since timing is more difficult at the more aggregated level (see, Gredil, 2019).

One of the shortcomings of the Burgiss data is lower coverage before 1995 (Brown et al., 2012), but all of the results are similar when years before 1995 are dropped from the sample. As the U.S. and the U.K. receive a large portion of PE capital invested, one might be concerned that the results in the paper are driven by these two countries only. We repeat all of the analysis excluding the U.S. and the U.K. and find that all the primary results are the same. Spillovers may take longer than a year or two to occur, so as noted already we repeat the analysis in the paper using VAR's with two- and three-year lags. The main results do not change and the strongest effect is indeed in the first year following the PE investment.[33] Overall, the main results of the paper seem to hold regardless of the estimation method, set of fixed effects, controls, or sample used.

[32] Results are the same when we include country*industry and time fixed effects instead.
[33] It should also be noted that a 1-year VAR will still have responses past 1 year by nature of how the systems are autoregressive, i.e. shocks will continue to propagate.

6 CONCLUSION

PE investments have risen dramatically during the last two decades, not only in developed countries but in developing economies as well. While research has explored how PE firms impact their portfolio companies, it is surprising that there is no evidence on the implications of PE for the global economy as a whole. The well-established spillover literature in economics provides evidence that productivity spillovers exist within industries. Our dataset on global PE investments in 19 industries across 52 countries allows us to study the impact of PE on industry dynamics. By focusing on aggregate industry measures of publicly listed companies, we are able to identify spillovers from PE-backed companies to the other companies within the same industry.

In our analysis of the real economic impact of PE investments, we find that employment growth, profitability growth, and labor productivity growth all increase across the public companies in an industry following PE investments. Additionally, we find that industry-level capital expenditures grow faster as well. Considering the endogenous nature of PE investment into a specific industry, we utilize a panel-VAR. While treating all the variables in the system as endogenous, the model also allows for fixed effects to control for individual heterogeneity at the country-industry level. Concerns about reverse causality are reduced as we do not find evidence that past values of industry dynamics are significantly related to the amount of PE capital a country-industry receives. The improvements in industry-level performance documented in this paper are consistent with an interpretation that the companies receiving PE capital become more efficient and put pressure on the other companies within the same industry, which leads to overall performance gains among the public companies within the industry. As such, while providing novel evidence on industry spillovers from PE onto industries, our findings are also consistent with the existing evidence on the positive impact of PE on firm-level performance.

The spillover effects we document are found to be concentrated in country-industries with higher levels of competition suggesting that competition is an important channel for these spillovers. We further find that the impacts on industry growth are more pronounced in the subsample of countries with stronger institutions and intellectual property rights suggesting that PE companies need a strong legal environment to be able to implement governance practices that lead to efficiency gains at portfolio companies. The positive effects are also concentrated in countries with moderate levels of innovative capacities. These results are largely consistent with the literature that examines spillovers from foreign direct investments onto local industries and finds that companies with moderate levels of technological advancement are better absorbers of productivity spillovers. Overall, the cross-sectional results further indicate a causal effect where PE investments lead to higher industry growth through spillovers.

The findings of the paper are important as they provide evidence on the impact of PE on industry dynamics, rather than individual companies, which is a largely unexplored area. The PE industry has been criticized for their impact on the companies in which they invest. This paper presents a more complete picture of the implications of PE for the global economy. Hopefully, future research will more clearly identify the specific channels which create spillovers from PE-backed companies to the broader set of firms in each industry.

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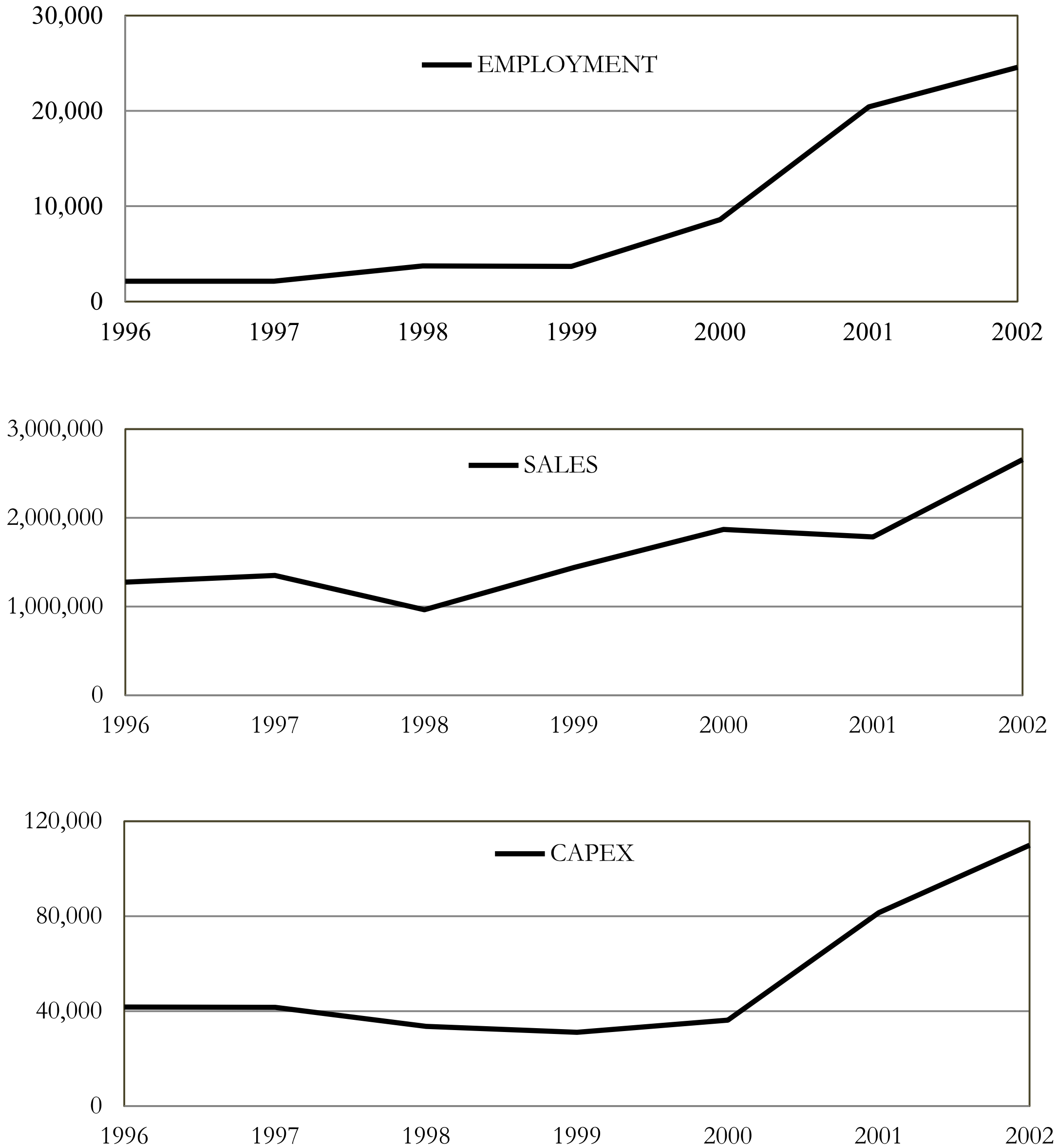
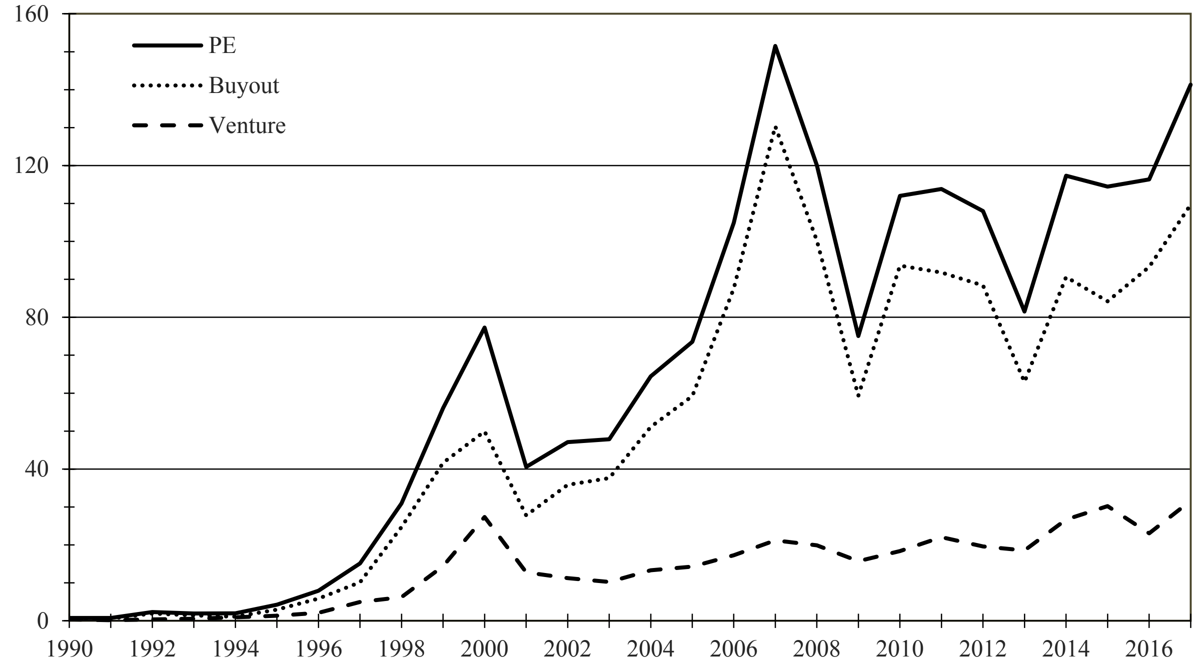
Figure 1: Thailand Food and Beverage Industry around 1999



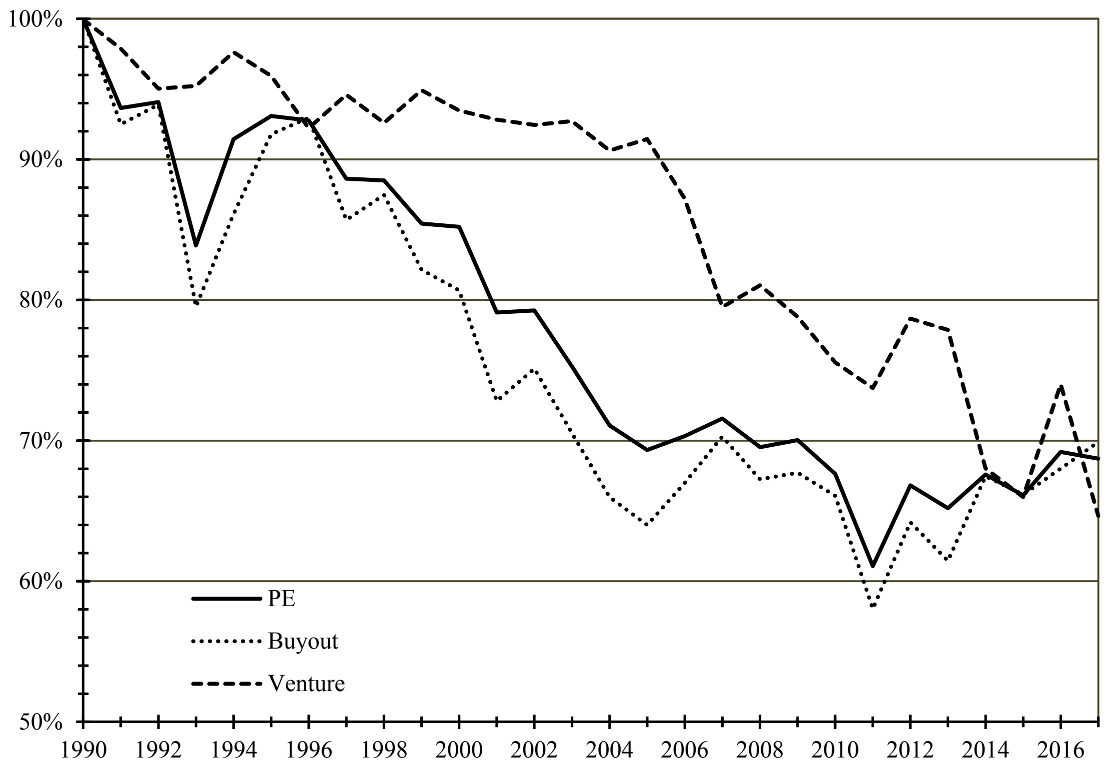
This figure plots industry employment, sales, and capex for the Thailand Food and Beverage Industry around 1999. The industry received $29 million PE capital in 1999. Employment is the total number of employees for all public companies in the industry. Sales is the total sales for all the public companies in the industry. Capex is the total capital expenditures for all public companies in the industry.

Figure 2: Time-series of Total PE Capital Invested Globally between 1990 and 2017



This figure plots the total amount of PE capital invested in 50 countries between 1990 and 2017. The solid line plots the total of buyout and venture capital. The dotted line plots buyout capital and the dashed line plots venture capital. Amounts are in 2017 billion dollars.

Figure 3: U.S. and U.K.'s Share of the Total PE Capital Invested Globally, 1990 – 2017



This figure plots the share across the U.S. and U.K. out of the total amount of global PE capital invested between 1990 and 2017. The solid line plots their share out of the total of buyout and venture capital invested. The dotted line plots their share out of buyout capital and the dashed line plots their share out of venture capital.

TABLE 1: PE Investments around the Globe

This table presents the distribution of PE capital invested among the 52 countries in the sample between 1990 and 2017. The first, second, and third columns show the total amount of PE capital, buyout capital, and venture capital, respectively. Panel A presents the countries ranked by the total amount of capital received and Panel B presents the countries ranked by the total amount of capital received as a percentage of GDP. Amounts in Panel A are in 2017 million dollars. Panel B reports averages of the ratios across years.

Panel A: Distribution of PE Capital Invested Globally

Country	PE Capital Invested	Buyout Capital Invested	Venture Capital Invested
United States	1,193,806	882,597	311,210
United Kingdom	146,686	138,401	8,285
China	72,005	32,698	39,307
Germany	61,084	57,582	3,502
France	46,771	44,498	2,273
Italy	32,383	32,049	334
Australia	28,160	27,251	908
India	26,777	15,194	11,583
Sweden	26,129	24,987	1,142
Canada	25,934	21,310	4,624
Netherlands	22,975	22,296	678
Spain	19,790	19,430	360
Brazil	16,254	15,090	1,164
Japan	15,111	14,499	612
South Korea	14,978	14,232	746
Norway	10,830	10,612	218
Denmark	10,428	9,961	467
Switzerland	9,797	8,567	1,229
Hong Kong	7,951	6,329	1,622
Poland	7,871	7,610	260
Belgium	7,588	7,352	236
Israel	6,899	3,318	3,580
Indonesia	5,915	4,677	1,239
Singapore	5,802	4,760	1,042
Finland	5,082	4,604	478
Argentina	4,488	4,181	307
Ireland	4,295	3,524	771
Turkey	4,112	3,987	125
Austria	3,432	3,180	252
Czech Republic	3,361	3,273	88
Luxembourg	3,233	3,089	145
South Africa	3,086	2,827	259
New Zealand	3,051	2,937	114
Russia	2,905	2,536	369
Mexico	1,953	1,866	87

Greece	1,534	1,534	0
United Arab Emirates	1,373	1,226	148
Malaysia	1,360	1,303	57
Hungary	1,291	1,275	16
Romania	1,105	1,052	53
Peru	1,064	1,048	17
Portugal	1,013	1,010	3
Thailand	1,011	944	67
Colombia	769	707	62
Nigeria	749	702	47
Chile	713	687	26
Egypt	659	642	18
Bulgaria	621	610	12
Slovenia	326	320	6
Philippines	315	237	78
Cyprus	286	277	9
Jordan	120	61	60

Panel B: PE Capital Received as a % of GDP

Country	**PE Capital Invested as a % of GDP**	**Buyout Capital Invested as a % of GDP**	**Venture Capital Invested as a % of GDP**
United States	0.233%	0.172%	0.062%
Luxembourg	0.232%	0.224%	0.008%
United Kingdom	0.168%	0.158%	0.009%
Sweden	0.159%	0.151%	0.007%
Denmark	0.103%	0.099%	0.005%
Israel	0.094%	0.044%	0.050%
Hong Kong	0.094%	0.075%	0.019%
Singapore	0.088%	0.072%	0.016%
Netherlands	0.088%	0.085%	0.003%
Norway	0.080%	0.078%	0.002%
Australia	0.075%	0.073%	0.003%
Ireland	0.066%	0.056%	0.010%
Finland	0.065%	0.059%	0.006%
New Zealand	0.064%	0.062%	0.002%
Canada	0.056%	0.046%	0.010%
France	0.056%	0.053%	0.003%
Switzerland	0.055%	0.049%	0.007%
Germany	0.055%	0.052%	0.003%
Poland	0.053%	0.050%	0.003%
Czech Republic	0.053%	0.051%	0.001%
Belgium	0.050%	0.049%	0.002%

Italy	0.049%	0.049%	0.000%
India	0.048%	0.028%	0.020%
Spain	0.045%	0.044%	0.001%
Cyprus	0.043%	0.042%	0.001%
Bulgaria	0.040%	0.040%	0.001%
Argentina	0.038%	0.036%	0.002%
South Korea	0.038%	0.036%	0.002%
Hungary	0.033%	0.032%	0.000%
South Africa	0.031%	0.029%	0.002%
Slovenia	0.030%	0.030%	0.001%
China	0.030%	0.015%	0.015%
Brazil	0.029%	0.028%	0.002%
Austria	0.028%	0.026%	0.002%
Indonesia	0.026%	0.020%	0.006%
Romania	0.025%	0.023%	0.002%
Greece	0.016%	0.016%	0.000%
Turkey	0.016%	0.015%	0.000%
Jordan	0.015%	0.006%	0.009%
Peru	0.015%	0.015%	0.000%
Malaysia	0.014%	0.013%	0.001%
Portugal	0.014%	0.014%	0.000%
United Arab Emirates	0.012%	0.011%	0.002%
Chile	0.012%	0.012%	0.000%
Thailand	0.011%	0.010%	0.001%
Egypt	0.011%	0.011%	0.000%
Japan	0.009%	0.008%	0.000%
Colombia	0.009%	0.008%	0.000%
Russia	0.007%	0.006%	0.001%
Philippines	0.006%	0.005%	0.002%
Mexico	0.006%	0.006%	0.000%
Nigeria	0.006%	0.005%	0.000%

TABLE 2: Industry Distribution of Total PE Capital Invested Globally, 1990 - 2017

This table presents the industry distribution of total PE capital invested globally between 1990 and 2017. Industry classifications are at Industry Classification Benchmark's super-sector level. Column 1 reports values for total PE, Column 2 for buyout and Column 3 for venture capital. Amounts are in 2017 billion dollars, and provide the total amount of capital invested into a specific industry over the sample period.

	1	2	3
Industry	**PE Capital Invested ($bn)**	**Buyout Capital Invested ($bn)**	**Venture Capital Invested ($bn)**
Technology	380.66	176.08	204.58
Industrial Goods & Services	281.14	260.41	20.74
Health Care	251.12	158.63	92.50
Retail	171.54	146.11	25.43
Media	119.15	110.20	8.95
Travel & Leisure	95.65	91.64	4.01
Personal & HH Goods	88.73	81.48	7.25
Financial Services	83.08	74.09	8.99
Oil & Gas	67.90	63.74	4.16
Telecommunications	63.33	51.85	11.49
Food & Beverage	57.68	54.78	2.90
Insurance	42.24	40.04	2.20
Construction & Materials	38.74	37.71	1.03
Chemicals	34.42	32.37	2.05
Automobiles & Parts	32.30	30.62	1.68
Real Estate	26.67	25.48	1.19
Utilities	21.89	20.40	1.50
Banks	18.98	18.53	0.45
Basic Resources	15.77	15.04	0.73

TABLE 3: Descriptive Statistics

This table presents summary statistics for the industry and country-level variables in Panel A and B, respectively. Variable definitions are in *Table A1*. PE, buyout, and venture capital invested are measured *as a percentage of industry sales*. PE, buyout, and venture capital invested with a plus provide summary statistics for the PE variables excluding the country-industry-years with no investment. Growth variables are measured as log differences. Industry growth variables are winsorized at the bottom and top 5% of the distribution. Summary statistics are in percentages.

Panel A: Industry-level

Variable	N	Mean	Median	Std. Dev.
PE Capital Invested	17,179	0.91	0.00	6.93
Buyout Capital Invested	17,179	0.67	0.00	5.41
Venture Capital Invested	17,179	0.23	0.00	3.52
PE Capital Invested^{+}	5,757	2.72	0.21	11.80
Buyout Capital Invested^{+}	5,007	2.30	0.21	9.84
Venture Capital Invested^{+}	2,740	1.47	0.04	8.71
Employment Growth	17,179	5.89	1.82	14.04
Profit Margin Growth	17,179	0.26	0.83	41.72
Productivity Growth	17,179	4.18	4.54	19.21
CAPEX Growth	16,682	9.10	7.48	44.96
Industry Returns	16,725	8.53	10.13	32.16
Debt Growth	13,036	10.63	5.97	40.98

Panel B: Country-level

Variable	N	Mean	Median	Std. Dev.
GDP Growth	1,686	2.10	2.19	3.55
Public Market	1,449	39.75	15.34	69.99
Credit Market	1,453	77.65	66.59	50.55

TABLE 4: Univariate Comparisons

This table shows that public companies in country-industries with more PE investments on average have higher growth. The table presents mean (median) comparisons. Columns 1 and 2 present means (medians), and Column 3 presents *p*-values for the difference in means (medians) using a *t*-test (Wilcoxon rank-sum test) in both Panels. Panel A compares means (medians) of country-industry-years with and without PE capital. Panel B compares means (medians) for country-industry-years with high versus low amounts of PE capital among the country-industry-years with non-zero PE investments. Employment growth is the log difference in industry-level employment for public firms between time t and t-1. Profit margin growth is the log difference in industry-level net profits over sales for public firms between time t and t-1. Productivity growth is the log difference in industry-level sales per employee for public firms between time t and t-1. CAPEX growth is the log difference in industry-level capital expenditures for public firms between time t and t-1. Industry returns is the log difference in the value of the industry stock return index between time t and t-1. Debt growth is the log difference in industry-level net debt for public firms between time t and t-1. Stocks traded to GDP is the total value of stocks traded in the country as a percentage of GDP. Private credit to GDP is the total credit in the country as a percentage of GDP.

Panel A: PE versus NON-PE Country-Industries-Years

	1	2	3
Variable	**PE**	**NON-PE**	**P-Value Mean (Median) Difference**
Employment Growth (%)	6.34 (3.02)	5.67 (1.11)	0.00 (0.00)
Profit Margin Growth (%)	0.31 (0.00)	-0.03 (0.00)	0.69 (0.85)
Productivity Growth (%)	3.37 (3.39)	4.58 (5.24)	0.00 (0.00)
CAPEX Growth (%)	8.63 (6.94)	9.34 (7.78)	0.33 (0.55)
Industry Returns (%)	8.21 (10.64)	8.69 (9.69)	0.36 (0.66)
Debt Growth (%)	10.55 (5.86)	10.67 (6.04)	0.88 (0.96)
GDP Growth (%)	1.94 (1.70)	2.06 (2.17)	0.01 (0.00)

Panel B: HIGH-PE versus LOW-PE Country-Industry-Years

	1	2	3
Variable	**HIGH PE**	**LOW PE**	**P-Value Mean (Median) Difference**
Employment Growth (%)	7.49 (3.72)	5.19 (2.40)	0.00 (0.00)
Profit Margin Growth (%)	1.19 (0.05)	-0.57 (0.00)	0.22 (0.15)
Productivity Growth (%)	3.32 (3.16)	3.43 (3.55)	0.82 (0.68)
CAPEX Growth (%)	10.12 (7.94)	7.14 (6.29)	0.00 (0.01)
Industry Returns (%)	9.30 (11.67)	7.14 (9.89)	0.01 (0.01)
Debt Growth (%)	12.24 (6.74)	9.05 (4.97)	0.01 (0.03)
GDP Growth (%)	2.16 (1.81)	1.70 (1.57)	0.00 (0.00)

TABLE 5: PE and the Real Economy

This table shows that following PE investments employment, profitability, and labor productivity increase for public companies in the same country and industry. The table presents the results from the panel-VAR estimation of equation 1 from Section 4, where the X vector consists of PE capital invested, industry-level employment growth, labor productivity growth, and profitability growth. The system is estimated with GMM. Employment growth is the log difference in industry-level employment for public firms between time t and t-1. Productivity growth is the log difference in industry-level sales per employee for public firms between time t and t-1. Profitability growth is the log difference in industry-level net profits over sales for public firms between time t and t-1. Country-industry and time fixed effects are included in the estimation as defined in Section 4. Reported numbers show the coefficients of regressing the column variables on the lags of the row variables. Standard errors clustered by country and industry are in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1% respectively.

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.3347***	0.0914**	0.1154**	0.4223***
	(0.0595)	(0.0428)	(0.0557)	(0.1556)
Employment Growth (t-1)	-0.0029	0.1414***	-0.0154	-0.1325
	(0.0024)	(0.0115)	(0.0136)	(0.1359)
Productivity Growth (t-1)	0.0012	0.0737***	-0.0718***	-0.0166
	(0.0016)	(0.0083)	(0.0113)	(0.0274)
Profitability Growth (t-1)	0.0002	0.0047**	0.0026	-0.2329***
	(0.0005)	(0.0021)	(0.0028)	(0.0101)
N Obs.	15,611			

TABLE 6: Buyout versus Venture Capital

This table shows that buyout investments lead to higher profitability and employment, while venture capital leads to higher productivity. The table repeats the estimation presented in Table 5, separately for buyout and venture capital, and results are presented in Panel A and B, respectively. Employment growth is the log difference in industry-level employment for public firms between time t and t-1. Productivity growth is the log difference in industry-level sales per employee for public firms between time t and t-1. Profitability growth is the log difference in industry-level net profits over sales for public firms between time t and t-1. Country-industry and time fixed effects are included in the estimation as defined in Section 4. Reported numbers show the coefficients of regressing the column variables on the lags of the row variables. Standard errors clustered by country and industry are in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1% respectively.

Panel A: Buyout

	Buyout Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
Buyout Capital Invested (t-1)	0.1999***	0.1021**	0.1019	0.3969***
	(0.0641)	(0.0492)	(0.1280)	(0.1465)
Employment Growth (t-1)	-0.0007	0.1415***	-0.0155	-0.1325
	(0.0020)	(0.0115)	(0.0136)	(0.1360)
Productivity Growth (t-1)	-0.0018	0.0737***	-0.0718***	-0.0167
	(0.0014)	(0.0082)	(0.0113)	(0.0275)
Profitability Growth (t-1)	-0.0001	0.0047**	0.0026	-0.2329***
	(0.0004)	(0.0021)	(0.0028)	(0.0101)
N Obs.	15,611			

Panel B: Venture Capital

	Venture Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
Venture Capital Invested (t-1)	0.5407***	0.1064	0.6301***	0.5226
	(0.0788)	(0.1045)	(0.1597)	(0.4454)
Employment Growth (t-1)	0.0015	0.1411***	-0.0149	-0.1311
	(0.0011)	(0.0115)	(0.0136)	(0.1358)
Productivity Growth(t-1)	0.0011	0.0734***	-0.0713***	-0.0155
	(0.0008)	(0.0083)	(0.0113)	(0.0273)
Profitability Growth (t-1)	0.0000	0.0047**	0.0026	-0.2331***
	(0.0002)	(0.0021)	(0.0028)	(0.0101)
N Obs.	15,611			

Table 7: PE and Investment

This table shows that capital expenditures of public firms increase following PE investments into the industry. The table presents the results of the four-variable panel-VAR estimation with GMM as in equation 1, where the X vector consists of PE capital invested, industry-level cash flow growth, capital expenditures growth, and market-to-book growth, similar to Love and Zicchino (2006). Cash flow growth is the log difference in industry-level free cash flows for public firms between time t and t-1. Capex growth is the log difference in industry-level capital expenditures for public firms between time t and t-1. Market-to-book growth is log difference in the price-to-book index of an industry between time t and t-1. Country-industry and time fixed effects are included in the estimation as defined in Section 4. Reported numbers show the coefficients of regressing the column variables on the lags of the row variables. Standard errors clustered by country and industry are in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1% respectively.

	PE Capital Invested	Cash Flow Growth	Capex Growth	Market-to-book Growth
PE Capital Invested (t-1)	0.3567***	0.5101	0.5083***	0.0032
	(0.0615)	(0.3957)	(0.1729)	(0.0512)
Cash Flow Growth (t-1)	0.0003	-0.3062***	0.0017	0.0026
	(0.0004)	(0.0128)	(0.0005)	(0.0019)
Capex Growth (t-1)	-0.0014	0.0747	-0.0005***	-0.0062
	(0.0011)	(0.0794)	(0.0119)	(0.0038)
Market-to-book Growth (t-1)	-0.0001	0.0579	0.1731***	-0.0659***
	(0.0023)	(0.0477)	(0.0267)	(0.0115)
N Obs.	12,310			

Table 8: Industry Spillovers from PE and Industry Competitiveness

This table shows that the positive effects reported in Table 5 are concentrated in more competitive country-industries. The table repeats the analysis presented in Table 5 for subsamples of country-industries created based on the level of competition. Panel A vs. B and C vs. D present the results for the subsamples of country-industries with low versus high levels of competition, measured by the industry-level gross margins and asset turnover ratios, respectively. Low (high) competition country-industries have gross margins above (below) the median of the sample distribution. Low (high) competition country-industries have asset turnover ratios below (above) the median of the sample distribution. The coefficients for the PE variable are presented only, but the estimation is identical to the panel-VAR in Table 5. Variable definitions are in Table A1. Country-industry and time fixed effects are included in the estimation as defined in Section 4. Reported numbers show the coefficients of regressing the column variables on the lag of the row variable. Standard errors clustered by country and industry are in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1% respectively.

Panel A: Low Competition – High Margin

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.3866***	-0.0816	0.1284	-0.0569
	(0.1037)	(0.0888)	(0.1035)	(0.3465)
N Obs.	7,973			

Panel B: High Competition – Low Margin

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.3072***	0.1972**	0.2089***	0.6763***
	(0.0987)	(0.0751)	(0.0623)	(0.2115)
N Obs.	7,638			

Panel C: Low Competition – Low Asset Turnover

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.2467***	-0.0523	0.1203	0.1866
	(0.0726)	(0.0549)	(0.1726)	(0.2448)
N Obs.	7,827			

Panel D: High Competition – High Asset Turnover

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.4548***	0.1466**	0.2115***	0.7457**
	(0.1453)	(0.0659)	(0.0819)	(0.3253)
N Obs.	7,784			

Table 9: Industry Spillovers from PE and Legal Strength

This table shows that the positive effects reported in Table 5 are concentrated in countries with a stronger legal environment. The table repeats the analysis presented in Table 5 for subsamples of countries created based on the level of legal strength. Panel A and B present the results for the subsamples of countries with weak versus strong legal institutions, respectively. Panel C and D present the results for the subsamples of countries with weak versus strong intellectual property rights, respectively. The coefficients for the PE variable are presented only, but the estimation is identical to the panel-VAR in Table 5. Variable definitions are in Table A1. Country-industry and time fixed effects are included in the estimation as defined in Section 4. Reported numbers show the coefficients of regressing the column variables on the lags of the row variables. Standard errors clustered by country and industry are in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1% respectively.

Panel A: Low Institutional Quality

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.2899***	0.0283	0.0636	0.2610
	(0.0858)	(0.0713)	(0.0915)	(0.3496)
N Obs.	7,584			

Panel B: High Institutional Quality

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.3673***	0.1803***	0.1519**	0.4574**
	(0.1135)	(0.0515)	(0.0661)	(0.1985)
N Obs.	8,027			

Panel C: Weak Intellectual Property Rights

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.2901***	0.0288	0.0606	0.2581
	(0.0858)	(0.0713)	(0.0916)	(0.2622)
N Obs.	7,369			

Panel D: Strong Intellectual Property Rights

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.3672***	0.1806***	0.1841**	0.4565**
	(0.1135)	(0.0515)	(0.0660)	(0.1987)
N Obs.	8,242			

Table 10: Industry Spillovers from PE and Innovative Capacity

This table shows that the positive effects reported in Table 5 are concentrated in countries with moderate levels of innovative capacities. The table repeats the analysis presented in Table 5 for three subsamples of countries created based on a measure of innovative capacity. Panel A, B, and C present the results for the subsamples of countries with lowest, highest, and moderate levels of innovative capacities. The subsample of countries with the lowest (highest) innovative capacities includes the countries in the bottom (top) 25th percentile. The subsample of countries with moderate innovative capacities includes the countries that are in between the 25th and 75th percentile of the distribution. The coefficients for the PE variable are presented only, but the estimation is identical to the panel-VAR in Table 5. Variable definitions are in Table A1. Country-industry and time fixed effects are included in the estimation as defined in Section 4. Reported numbers show the coefficients of regressing the column variables on the lags of the row variables. Standard errors clustered by country and industry are in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1% respectively.

Panel A: Lowest Innovative Capacity

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.0377	-0.0211	0.1412	0.0988
	(0.0413)	(0.1121)	(0.1568)	(0.2940)
N Obs.	3,930			

Panel B: Highest Innovative Capacity

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.2151	0.0156	0.0861	0.2913
	(0.1382)	(0.0217)	(0.0676)	(0.4760)
N Obs.	4,113			

Panel C: Moderate Innovative Capacity

	PE Capital Invested	Employment Growth	Productivity Growth	Profitability Growth
PE Capital Invested (t-1)	0.4315***	0.1430***	0.2274**	0.4391**
	(0.0632)	(0.0511)	(0.1104)	(0.1757)
N Obs.	8,307			

Table 11: PE and the Financial Economy

Panel A of this table shows that buyout investments lead to higher debt growth and lower stock returns among the public companies in the same country and industry. Panel B of this table shows that venture capital investments are not significantly related to debt growth or stock returns among the public companies in the same country and industry. The table presents the results from the panel-VAR estimation of equation 1 from Section 4, separately for buyout and venture capital, where the X vector consists of PE capital invested, industry-level debt growth, and industry returns. The system is estimated with GMM. Panel A and B present the results for buyout and venture capital, respectively. Industry returns is the log difference in the value of the industry stock return index between time t and t-1. Debt growth is the log difference in industry-level net debt for public firms between time t and t-1. Country-industry and time fixed effects are included in the estimation as defined in Section 4. Reported numbers show the coefficients of regressing the column variables on the lags of the row variables. Standard errors clustered by country and industry are in parentheses. *, **, and *** denote statistical significance at 10%, 5%, and 1% respectively.

Panel A: Buyout

	Buyout Capital Invested	Debt Growth	Industry Returns
Buyout Capital Invested (t-1)	0.0798**	-0.2476	0.0185
	(0.0308)	(0.2789)	(0.1602)
Debt Growth (t-1)	0.0001	-0.0012	-0.0200***
	(0.0006)	(0.0126)	(0.0064)
Industry Returns (t-1)	0.0005	-0.0255	0.0379***
	(0.0008)	(0.0169)	(0.0135)
N Obs.	10,689		

Panel B: Venture Capital

	Venture Capital Invested	Debt Growth	Industry Returns
Venture Capital Invested (t-1)	0.7409***	-0.4869	-0.6673
	(0.2661)	(1.0080)	(0.5378)
Debt Growth (t-1)	-0.0001	-0.0014	-0.0199***
	(0.0001)	(0.0126)	(0.0064)
Industry Returns (t-1)	-0.0001	-0.0252	0.0382***
	(0.0003)	(0.0169)	(0.0135)
N Obs.	10,691		

APPENDIX

Table A1: Data Sources and Variable Definitions

This table provides descriptions of data sources and variable definitions used in the paper. Panel A defines the data sources, and Panel B presents the variable definitions with the data source for the variable in parenthesis.

Panel A: Data Sources	
Burgiss	The Burgiss Group is a software company that provides data record keeping and performance analysis services to the largest institutional investors in the PE universe. Burgiss data is aggregated at the industry-level using actual fund investments into portfolio companies.
DataStream	DataStream's Global Equity Indices provide industry indices aggregated from financial statements of publicly listed companies across 53 countries and 170 sectors worldwide.
World Development Indicators (WDI)	The development indicators are from World Bank's primary database. It presents the most current and accurate global development data available, and includes national, regional and global estimates.
World Economic Forum's Global Competitiveness Index (GCI)	GCI assesses the competitiveness landscape of 144 economies, providing insight into the drivers of their productivity and prosperity. It provides different indices on a country's legal environment, as well as financial and technological development.
Panel B: Variable Definitions	
PE Capital Invested	$ Amount of PE capital invested, normalized by industry sales, and logged. (BURGISS)
Buyout Capital Invested	$ Amount of buyout capital invested, normalized by industry sales, and logged. (BURGISS)
Venture Capital Invested	$ Amount of venture capital invested, normalized by industry sales, and logged. (BURGISS)
Employment Growth	Log difference in industry employment between time t and t-1. (DATASTREAM)
Profitability Growth	Log difference in industry profit margins, net profit over sales, between time t and t-1. (DATASTREAM)
Labor Productivity Growth	Log difference in industry sales per employee between time t and t-1. (DATASTREAM)
CAPEX Growth	Log difference in industry capital expenditures between time t and t-1. Capital expenditures include, but are not limited to, additions to property, plant and equipment as well as investments in machinery and equipment.
Cash Flow Growth	Log difference in industry free cash flow between time t and t-1. Free cash flow is the sum of funds from operations, funds from/used for other operating activities and extraordinary items.
Industry Returns	Log difference in the value of the industry return index retrieved from DataStream Global Equity Indices between time t and t-1. (DATASTREAM)
Debt Growth	Log difference in industry debt, total debt net of cash and cash equivalents, between time t and t-1. (DATASTREAM)
Market-to-book Growth	Log difference in the price-to-book index of an industry between time t and t-1. (DATASTREAM)
Stocks Traded to GDP	Total value of stocks traded over GDP. (WDI)
Private Credit to GDP	Total amount of private credit over GDP. (WDI)
Institutional Quality Score	Measures the institutional quality of a country. It is a combination of scores on legal institutions, property rights, investor protection as well as judicial efficiency. (GCI)
Intellectual Property Rights	Measures the strength of intellectual property protection in a country. (GCI)
Innovative Capacity	Measures a country's capacity to innovate and adapt to new technologies. (GCI)

The Buyout of Hertz Corporation

The buyout of the car rental company Hertz Corporation in 2005 was one of the biggest buyout transactions in history. The company was acquired for $14 billion by a PE consortium consisting of the Carlyle group, Merrill Lynch's private investments arm, and Clayton Dubilier & Rice. After the buyout, significant changes were made at the company to cut costs and improve operational efficiency. For example, before the buyout a returned car was being cleaned and refueled at different work stations. The new management realized this created unnecessary idle time. To increase efficiency, cleaning stations were moved to where the cars were refueled resulting in a large increase in the number of cars that could be processed every hour. In addition to operational changes, the PE group also changed the governance structure of the company and more closely monitored management.[34]

During the period after the buyout, the two biggest competitors of Hertz, Avis-Budget and Dollar-Thrifty, also experienced significant efficiency gains. For example, in 2006 Avis-Budget introduced a process improvement initiative called "Performance Excellence", designed to make the vehicle rental process easier, cut costs, and enhance the customer rental experience. Similarly, Dollar-Thrifty announced the implementation of several cost-saving initiatives, including some information technology outsourcing and new investment into existing IT systems to increase efficiency. Given the timing of these changes, it is plausible that they were made in response to the competitive pressure from Hertz. During the 2006 to 2007 period, at Avis-Budget and Dollar-Thrifty profit margins increased by 10% and 7%, while labor productivity, measured by sales per employee, also increased by 5% and 6%, respectively. This specific example suggests that practices and technologies causing efficiency gains at a PE-backed company might quickly spill over onto other companies within the same industry.

[34] The New York Times article "Is Private Equity Giving Hertz a Boost?" published on September 23, 2007 discusses the Hertz buyout and talks about the operational changes at Hertz following the buyout.

Private Equity Investment and Local Employment Growth: *A County-Level Analysis*

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KEY FINDINGS

- The study finds a positive association between private equity investment and employment growth. Results indicate that for each $1 million in additional private equity investment, a little more than 1.3 new jobs are created.
- Results imply that private equity investment could create positive externalities. Statistical tests using countywide employment data suggest that company specific private equity investment job-creation effects spill over from the company receiving financing to the local economy.

ABSTRACT: *This study examines the relationship between private equity investment and local employment growth. Using a sample of over 3,000 US counties, we estimate the effect of private equity investment volume and demographic determinants of employment growth (labor supply, labor quality, labor cost, unionization, agglomeration, industry concentration, and regional geography) on employment changes from 2011 to 2014. Controlling for these demographic factors, private equity investment shows a positive correlation with employment changes.*

TOPIC: ***Private equity****

*All articles are now categorized by topics and subtopics. **View at PM-Research.com.**

The size of the private equity industry has exploded since the beginning of the century, with capital invested in private equity funds more than doubling since 2000.[1] As fewer companies opt to list on public exchanges, they have sought private equity investment to expand. Similarly, private equity fund managers often promote their ability to "create value" and "build businesses" by improving operations, expanding distribution networks, optimizing supply chains, or hiring professional management at their portfolio companies. Academic studies (e.g., Kaplan and Stromberg 2009, Cao and Lerner 2009, and Harris et al. 2015) have shown that private equity investment improves the performance of companies, but does better performance translate into job creation?

Analyzing the relationship between private equity investment and company job creation poses many challenges. Previous studies have relied on limited company-level information and have produced conflicting findings. One segment of these studies indicates that private equity stimulates employment

[1] According to the 2016 Preqin Global Private Equity & Venture Capital Report, annual global private capital fundraising increased from $238 billion in 2000 to $551 billion in 2015.

through investment and expanding business growth. Competing studies indicate that private equity fund managers reduce jobs by lowering costs of production at portfolio companies. Because of the scarcity of information about private companies and private equity investments, the robustness of many of these studies comes into question.

This article applies a different methodology to analyze private equity investment's employment impact by analyzing the relationship between investment volumes and job growth in local economies—the US county where the portfolio company is located. By focusing on the geographic impact, this analysis draws on insights from labor economics. It also recognizes that investment in companies often has positive spillover effects in the local economy. We find that private equity investment has a positive relationship with county-level employment change between 2011 and 2014, when controlling for economic, industrial, and demographic characteristics.

RELATED STUDIES

The county-level analysis in the study is motivated by previous research on positive economic impact related to the spillover effects of private investment. A spillover effect is the indirect byproduct that occurs due to an unrelated economic event. They are also called *positive externalities*. For instance, the increase in hiring at the local plant could bring more workers to area, and they would purchase goods and services from local shops and even relocate to be closer to the worksite. In fact, private investment is often seen as a key driver of economic growth. In its analysis of the importance of private investment to the European economy after the global financial crisis, McKinsey and Company (2012) stated, "Private investment holds significant promise as a driver of recovery and sustained medium-term growth."

Private equity investment, specifically, can also produce positive externalities for the economy. Aldatmaz and Brown (2017) showed that private equity investment leads to greater labor productivity, employment, profitability growth, and capital expenditures by publicly-listed companies that are in the same country and industry as the private equity targets. The authors cited the example of the rental car company Hertz Corporation, which improved its operational performance under private equity ownership. Soon after, Hertz's main competitors, Avis-Budget and Dollar-Thrifty, implemented new strategies to increase their respective operational efficiencies. In short, the authors demonstrated that private equity has positive spillovers to the greater economy.

If changes in employment is a proxy for economic health, how does private equity affect employment? A number of studies originating in business and finance have tried to answer the question by analyzing company-level data, but these studies produce conflicting findings. It is likely that data constraints have led to these less robust results.

The earliest studies testing the relationship between jobs and private equity investment at the company level were based on small sample sets, and the findings are mixed. Kaplan (1989) relied on a sample of 76 public-to-private leveraged buyouts (LBOs) completed between 1980 and 1986; these are typically some of the largest acquisitions in private equity. He found a 12% industry-adjusted loss of median employment in the year after a management buyout. Amess and Wright (2007) studied the impact of management buyouts (MBOs) and management buy-ins (MBIs)[2] on wages and employment of 1,350 firms in the United Kingdom between 1999 and 2004, but monitored overall employment in a group of firms instead of matching firms. Their findings indicate that overall LBOs—MBOs and MBIs together—have a statistically insignificant effect on employment growth. However, these results do not differentiate the impact of private equity buyouts compared to management buyout deals.

In a more recent study, Amess et al. (2014) used differences-in-differences methods combined with propensity score matching, as well as a control function approach—a strategy that allows a regression to control for selection bias. Based on this analysis, Amess et al. found that private equity backed-LBOs have a negative effect on employment in the three years following ownership change.

Other studies indicate a positive lagged effect of private equity investment on firm employment. Analyzing British companies, Cressy, Munari, and Malipiero (2011) used a dataset of 48 private equity buyouts and a control group of 84 nonprivate-equity-backed companies in the 1990s and 2000s to find that employment falls

[2] In MBOs, management gains significant ownership of the company. In MBIs, external management and investors acquire the company.

during the first four years of private equity ownership, but it increases in its fifth year. The authors concluded that job losses are offset with job creation.

A more recent US study of buyouts with a substantially larger dataset generated similar findings. Davis et al. (2014) built a sample containing 3,200 US firms acquired in buyouts from 1980 to 2005 and 150,000 US establishments operated by these firms during the buyout year—a substantial improvement in sample size from previous studies.[3] They restricted their sample, which is supplied by Capital IQ, to transactions with a financial sponsor and use of leverage. Transactions not classified as *going private*, *leveraged buyout*, *management buyout*, *platform*, and similar terms are omitted. This restriction may have omitted many private-equity-backed companies, such as those receiving minority investment by growth capital investors and investment without leverage. The study concluded that private equity buyouts lead to greater job loss at establishments in the year of the buyout, shrinking 3% in comparison to controls in the two-year post buyout period and by 6% over five years. However, private equity buyouts create new jobs in newly opened establishments more rapidly than control firms. Once the purchase and sale of establishments are accounted for, the target-control growth differential is less than 1% of initial employment over two years. The authors argue that these findings demonstrate that private equity investment accelerates the process of creative destruction by shutting down less productive establishments and constructing more productive ones.

Examining the effect of private equity and venture capital financing on small and mid-sized single entity business establishments, results from Paglia and Harjoto (2014) support a positive lagged finding. Using a sample of over 16,000 private equity-backed companies, the authors found that although private equity financing does not immediately impact establishments' employment growth rates in the year of financing, it does have a positive impact three years after funding. Similarly, Boucly, Sraer, and Thesmar (2011) used a sample of 839 LBOs between 1994 and 2004 to find that private equity-backed buyouts in France increase employment in the four years after the buyout when compared to a control group.

As noted, the previous studies of the influence of private equity ownership on employment growth analyze company-level data, which poses data constraints. First, an unbiased dataset is not readily available and constructing a sample can be an arduous process. When analyzing changes at the company level, obtaining a sizeable sample of private companies with available employment data is a difficult. Second, tracking changes over time is challenging because firms can change names or become acquired by other companies. These changes convolute the recording of information associated with a single company, including job growth. Establishing a control group of companies without private equity investment is tedious for the same reasons, and the need to match the control firm's industry, size, location, etc., with that of the experimental group (i.e., companies with private equity investment) adds further complications.

The shortcomings of a company-level analyses and the understanding of spillover effects provides an opportunity to examine the effect of private equity ownership on local employment growth. By focusing on employment in a geographic area, rather than at a company, we also draw on insights from previous economic research on factors promoting labor growth.

DATA AND VARIABLES

This study's unit of analysis is counties within the United States. We selected this unit because it is the smallest geographic entity for a range of economic and demographic variables contained in numerous publicly available datasets. Although there is undoubtedly greater availability of state-level data, a state-centric analysis limits us to a much smaller sample size and ignores the economic and social variations that occur within states. The use of counties as a unit of analysis for local economic development, such as employment growth, is not a new practice (see Aldrich and Kusmin 1997; James, Ilvento, and Hastings 2002; Addison Blackburn and Cotti 2008).

The data on 3,141 counties used in this analysis is obtained from a combination of sources.[4] The main independent variable of interest, *private equity investment*, is provided by PitchBook, a comprehensive private

[3] *Firms* refers to the companies. *Establishments* refers to the individual locations or branches operated by the firms.

[4] The number of United States counties varies by source. This article reflects the number of counties (3,141) found in Bureau of Labor Statistics' data.

Exhibit 1
Variable Data Sources

Name	Source
Employment change	US Bureau of Labor Statistics
Employment baseline	US Bureau of Labor Statistics
Private equity investment	PitchBook
Unemployment	US Bureau of Labor Statistics, US Department of Agriculture
Education	US Census Bureau, US Department of Agriculture
Labor force age	US Census Bureau
Annual average pay	US Bureau of Labor Statistics
Minimum wage	US Department of Labor
Right-to-work state	Politifact
Metro	US Census Bureau
Manufacturing	US Bureau of Labor Statistics
Services	US Bureau of Labor Statistics
Finance	US Bureau of Labor Statistics
Regions	Bureau of Economic Analysis

equity database. Information for the dependent variable, *employment growth*, comes from the Bureau of Labor Statistics.

In addition to private equity, our regression models control for other aspects that impact job growth because private equity investment might be a proxy measure of economic prospects in the region. Data for most of the independent variables are pulled from government websites, such as the US Bureau of Labor Statistics, the US Census Bureau, the US Department of Agriculture, and the Bureau of Economic Analysis. Some variables, such as *right-to-work* and *minimum wage*, are state- and city-level characteristics assigned to the respective counties. A complete list of data sources is found in Exhibit 1.

Dependent Variable: Employment Growth

The model in this study is designed to examine the degree to which the key explanatory variable, private equity investment, affects county-level employment growth while controlling for fixed effects at the county level. To perform this analysis, we obtained county-level employment data from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages. With this data, we calculated the dependent variable, employment growth, from the absolute change in employment from 2011 to 2014. Because past company-level studies indicate that private equity investment is unlikely to have the greatest impact on employment growth in the year of financing, we expect that measuring employment impact two or three years after a private equity investment will produce more measurable results (Paglia and Harjoto 2014). Exhibit 2 maps the changes in employment in US counties between 2011 and 2014.

Independent Variables

The analysis in this article is underpinned by socioeconomic literature, which is discussed in the next section. We specified the model's independent variables based on these studies' determinants of employment levels and the availability of data that allows us to operationalize them.

Private equity investment. The independent variable of interest, *private equity investment*, is a measure of the sum of private equity investment (in millions of dollars) in each county during 2011 and 2012. This information was obtained from PitchBook. As previously discussed, the current literature suggests that there is a potential one- to five-year lag in the effect of private equity investment on employment growth at the firm level. [5] As a midpoint of existing literature, we utilize 2011 investment data to allow for a three-year lag to measure employment growth; however, we also included 2012 investment data during the 2011–2014 time period to capture more counties receiving private equity investment. This allows for a two-year lag for 2012 investments. Private equity investment volume data were available for 3,053 counties, just 88 shy of the total counties examined. This sample included 374 counties that received private equity investment. A county map showing the distribution of private equity investment during 2011 and 2012 is found in Exhibit 3.

Labor supply. An abundance of labor supply is hypothesized to generate economic and business growth and, in turn, employment levels, because human capital is a major component of production and

[5] Boucly, Sraer, and Thesmar (2011) found that PE-backed buyouts affect employment in the four years after the buyout. Cressy, Munari, and Malipiero (2011) and Davis et al. (2014) found fluctuations in the following five years. Paglia and Harjoto (2014) found the effect of private equity insignificant in the first year, but positive three years after. Amess et al. (2014) saw a negative impact when examining three years after ownership change.

Exhibit 2
County Employment Change (2011–2014)

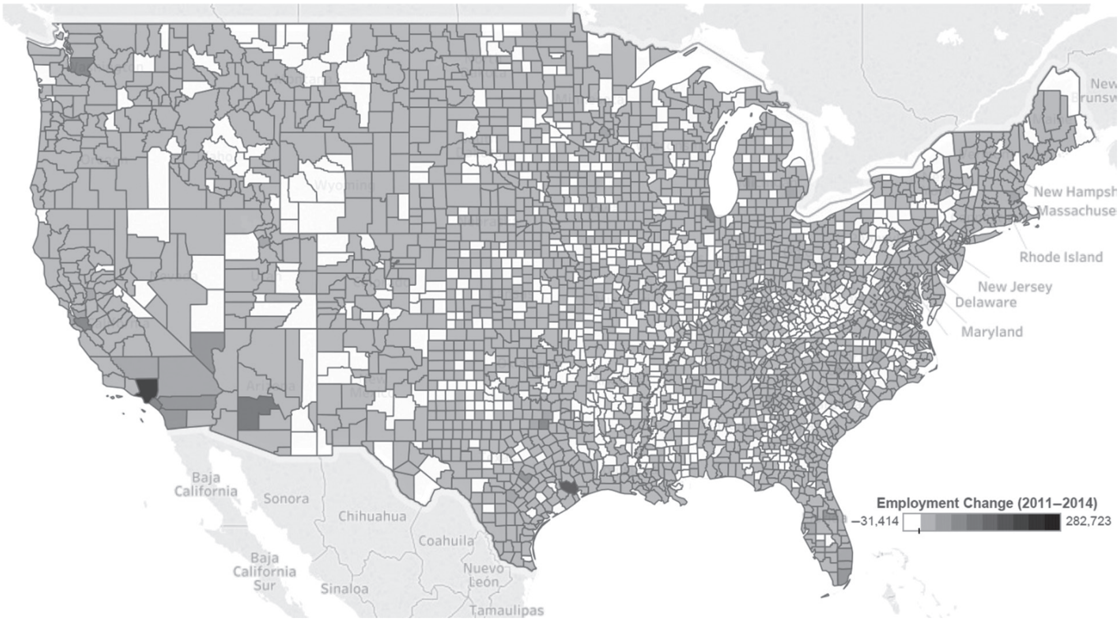
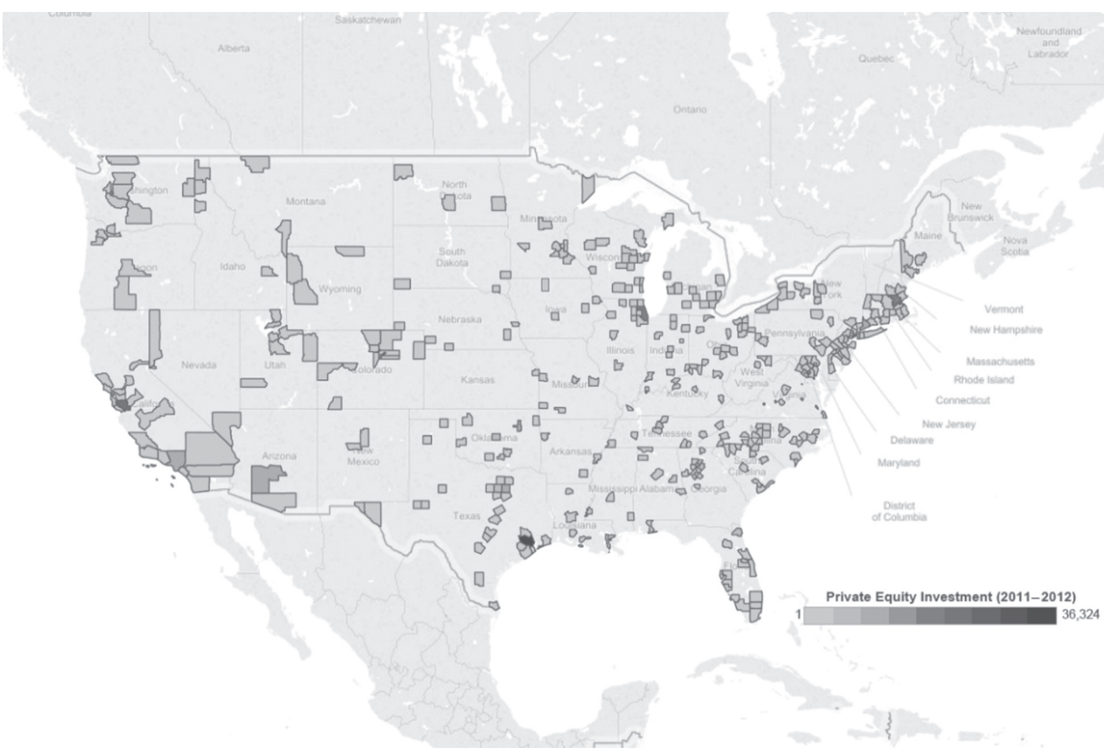


Exhibit 3
County Private Equity Investment (2011–2012)



comprises a greater share of production costs (Blair and Premus 1993). Similar to past studies examining economic and employment growth (James, Ilvento, and Hastings 2002; Addison Blackburn and Cotti 2008), we measure the labor supply with an *unemployment* variable using data from the US Bureau of Labor Statistics. Low unemployment levels are hypothesized to indicate that a county's labor supply is healthy, thus attracting business and increasing employment.

Labor quality. The quality of the labor force has the potential to affect the attractiveness of the local economy to businesses. To measure the quality of county-level labor force, two variables are included in the model. The first, *education*, is a heavily researched determinant of employment and wage growth and a strong indicator of labor force quality (Glaeser, Scheinkman, and Shleifer 1995; Crain and Lee 1999; Blumenthal, Wolman, and Hill 2009). Using US Census Bureau data, we use a variable of education levels measured by the average percentage of adults with a bachelor's degree or higher. The expectation is that counties with more educated populations will experience higher employment growth.

We also hypothesize that age plays a role in predicting employment growth. We expect that countries with aging work forces will see lower employment growth, in part because older workers are expected to leave the labor force and are more costly to hire than younger employees. *Labor force age* is measured by the median age of women and men between the ages of 16 and 64 (US Census Bureau).

Labor cost. It is hypothesized that businesses prefer low labor costs (Aldrich and Kusmin 1997); therefore, the model incorporates controls for county-level labor costs. The US Bureau of Labor Statistics provides county wage data from which we acquired *annual average pay*. We expect countries with higher wage levels (i.e., annual pay) to experience less employment growth when controlling for other factors.

Additionally, there has been much discussion around the effect of minimum wage levels on employment growth, most of which concludes that higher minimum wages does not reduce employment (Card and Krueger 1994; Neumark and Wascher 2007; Addison, Blackburn, and Cotti 2008). To test this, we included a 2011 *minimum wage* variable to estimate a state or city minimum wage rate's impact on local employment. Counties in states without a minimum wage are assigned the federal minimum wage. Consistent with the literature, higher minimum wages are expected to reduce employment growth.

Unionization. There is also a substantial amount of research examining the influence of unionization on employment, but conclusions are varied with some studies (Nickell, Nunziata, and Ochel 2005; Bassanini and Duval 2006) showing a positive correlation and other studies showing the opposite (Blanchard and Wolfers 2000; Baccaro and Rei 2007). To account for the potential effect of union participation of employees on overall employment growth, we assigned counties a *right-to-work* dummy variable indicating whether or not they were located in a right-to-work state in 2011. In states with right-to-work laws, union security agreements are prohibited, which forbids unions and employers from requiring employees to join a union and pay dues.

Agglomeration and urbanization. The theory of agglomeration argues that economies benefit when firms and their potential labor source are located near each other (Glaeser 2010). We also draw on an extensive amount of research showing a positive impact of urbanization and concentrated firm location on economic growth (Glaeser et al. 1992; Simon 1998; Feldman 1999; Rosenthal and Strange 2003). To account for agglomeration and urbanization, we include the variable *metro*, a dummy variable indicating whether or not the county is in a metropolitan area as defined by the US Census Bureau. Agglomeration and urbanization are expected to boost employment growth.

Industry concentration. Past studies have asserted the positive effect of the presence of certain industries on economic growth, particularly *manufacturing*, *financial*, and *services* (Crain and Lee 1999; Blumenthal, Wolman, and Hill 2009). The US Bureau of Labor Statistics provides location quotients, a ratio that describes an area's distribution of employment by industry as compared to a reference or base area's distribution—which we use to operationalize industry concentration. The location quotients used in the model are relative to the United States reference base.

Regions. We include regional dummy variables commonly found in geographical studies concerning economic growth in the United States. The model contains the eight regions defined by the Bureau of Economic Analysis: New England, Midwest, Great Lakes, Plains, Southeast, Southwest, Rock Mountain, and Far West. Southeast is not included in the models as the omitted reference variable.

EXHIBIT 4

Model Variables and Descriptive Statistics

Name	Description	*N*	Mean	Standard Deviation	Min	Max
Employment change	Employment growth (2011 to 2014)	3,125	2,249.61	10,625.98	–5,286.00	282,723.00
Employment baseline	Employment (2011)	3,125	33,828.64	122,164.00	16.00	3,340,009.00
Private equity investment	Private equity investment, $M (2011–2012)	3,053	113.54	1,158.99	0.00	36,324.15
Unemployment	Unemployment rate (2011)	3,140	8.72	3.00	1.40	29.10
Education	% of adults with a bachelor's degree or higher (2010–2014)	3,141	20.09	8.92	2.63	75.09
Labor force age	Median age of women and men between age 16–64	3,141	41.82	3.04	21.80	53.90
Annual average pay	Annual average pay (2011)	3,127	34,365.68	8,837.79	13,990.00	116,904.00
Minimum wage	Minimum wage (2011)	3,141	7.55	0.50	7.25	10.74
Right-to-work state	County in right-to-work state (2011)	3,141	0.56	0.50	0.00	1.00
Metro	Metropolitan area	3,141	0.37	0.48	0.00	1.00
Manufacturing	Manufacturing employment quotient (2011)	3,140	1.24	1.07	0.00	7.38
Services	Services employment quotient (2011)	3,140	0.79	0.19	0.00	1.39
Finance	Finance employment quotient (2011)	3,140	0.64	0.35	0.00	5.65
Regions	Eight BEA regions					

Summary statistics for employment and the models' independent variables are in Exhibit 4.

CHANGE SCORE METHODOLOGY

To test the significance of private equity investment on employment growth, this study uses a change score of employment as the dependent variable. Specifically, employment growth is calculated as $T_2 - T_1$, where T_2 is a county's employment in 2014 and T_1 is its employment in 2011. Using a change score as a dependent variable is not unusual in social science, and studies have argued that it is preferable (Allison 1990; Delecki and Willits 1991). Defining the variable of interest as a change score facilitates interpretation of the direction and nature of the change (Delecki and Willits 1991).

By contrast, the percentage change typically found in financial research is not encouraged when the dependent variable is unit measurement, such as jobs. Delecki and Willits (1991) stated that "such analyses are generally inappropriate and misleading" because correlations to ratios partly depend on the correlations between numerator and denominator terms.

When using change score variables, Delecki and Willits (1991) recommended incorporating T_1 as control variable in the model to generate "regression and partial correlation coefficients that are identical to those obtained from the more commonly accepted models." The change score ($T_1 - T_2$) is often correlated to (T_1), and, for this reason, the model incorporates a baseline (T_1) as a fixed effect variable.[6]

The benefit of using a change score, rather than a percentage, may become clearer in a simple example. Take two counties, Sardine County and Elbow-Room County, where 10,000,000 employees reside in the former and 100,000 employees reside in the latter. Each county experiences 1% growth in net employment during the research period. Employment increased by 100,000 jobs in Sardine County and by 1,000 jobs in Elbow-Room County. We would expect that it would require greater company growth to generate 100,000 new jobs in Sardine County, compared to 1,000 new jobs in Elbow-Room County. A percentage change variable would fail to capture this difference in magnitude.

REGRESSION ANALYSIS

Level-Level Models

Using a progression of five different models, the regressions transition from encompassing only

[6] Delecki and Willits (1991) stated that this correlation is often negative.

Exhibit 5

Determinants of Employment Change Regression Models and Results (robust standard errors)

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	–479.87	–331.88	–5991.32	–4973.25	–1142.21
	(117.75)***	(112.89)***	(2754.99)	(2876.07)*	(3530.26)
Employment baseline	0.08	0.07	0.07	0.07	0.07
	(0.00)***	(0.00)***	(0.01)***	(0.01)***	(0.01)***
Private equity investment		1.36	1.32	1.31	1.31
		(0.61)**	(0.60)**	(0.60)**	(0.58)**
Unemployment			–7.70	–3.97	–33.70
			(21.29)	(22.23)	(23.88)
Education			–19.01	–6.79	–0.45
			(19.66)	(18.06)	(18.29)
Labor force age			–2.67	–2.62	30.84
			(18.45)	(18.50)	(18.67)*
Annual average pay			–0.02	–0.02	–0.02
			(0.02)	(0.02)	(0.02)
Minimum wage			827.65	788.07	52.35
			(364.27)	(371.41)**	(490.00)
Right-to-work state			1117.14	1062.58	280.57
			(181.52)***	(184.33)***	(149.68)*
Metro			–173.52	–113.08	55.90
			(156.45)	(143.52)	(157.34)
Manufacturing				–4.54	127.49
				(33.38)	(40.88)**
Services				–1013.51	–193.89
				(317.24)***	(351.71)
Finance				–216.09	–194.80
				(289.56)	(278.28)
Region—New England					–2680.30
					(870.20)***
Region —Mideast					–2960.68
					(517.72)***
Region—Great Lakes					–412.73
					(238.14)*
Region—Plains					–68.62
					(182.32)
Region—Southwest					740.54
					(277.8)***
Region—Rocky Mountains					695.10
					(275.14)**
Region—Far West					2081.93
					(790.38)***
R^2	0.8605	0.8741	0.8768	0.8768	0.8834
Observations	3,125	3,038	3,037	3,036	3,036

*Notes: *significant at the 90% confidence level. **significant at the 95% confidence level. ***significant at the 99% confidence level.*

the employment baseline to including all available independent variables. These models were initially tested using OLS regression, and we detected heteroscedasticity in all of the models. To correct for this inconsistency, the second round of models estimate standard errors using the Huber-White sandwich estimators. A discussion of the regression results with robust standard errors follows and the details are presented in Exhibit 5.

Model 1 examines the bivariate relationship between employment change and the baseline year. The results of this model show that, on average, counties with higher levels of employment in 2011 gained more employment from 2011 to 2014. Although the coefficient is a small positive number, the level of significance is high and demonstrates the importance of the employment baseline variable in as a control variable.

Model 2 includes our main explanatory variable, private equity investment. We note that the coefficient is positive and significant at the 95 percent confidence level.

Model 3 add controls for fixed effects that would impact the labor market in these counties. This model adds controls for what we consider *core variables*, that is, characteristics of the labor force and county that would impact employment. These variables were chosen to represent what our survey of literature has identified as core factors determining growth. They include variables accounting for labor supply (*unemployment*), labor quality (*education* and *labor force age*), labor cost (*annual average pay and minimum wage*), effect of unionization (*right-to-work state*), and agglomeration and urbanization (*metro*). Of these added variables, surprisingly, only *right-to-work state* is statistically significant and shows a positive correlation to net new jobs. Both Model 2 and Model 3 yield similar interpretations about the impact of private equity investment on employment, that is, private equity investment is correlated with an average increase of 1.36 or 1.32 employees in a county per million dollars invested, respectively.

The last two models add controls for *industry concentration* and *regions*. Because previous studies found significant relationships between certain local industrial composition and employment growth, we first add industry controls in Model 4 to isolate their effects from regional differences. These variables enhance the model and increase the significance of *minimum wage* to a 95% confidence level. Although we would expect a higher minimum wage to negatively impact net new employment, the model shows a positive correlation. In addition, the industry concentration variable, *services*, is found to be significant at the 99% level. Interestingly, a high concentration of services-based companies in a county is negatively correlated with job gains. Finally, *private equity investment* remains positively correlated with *employment change* at the 95% confidence level in this Model.

The final model, Model 5, incorporates dummy variables for US national regions. The results show an increase in the number of significant variables in our model, and particularly the regional dummy variables. We infer that much of the job growth and job loss during the 2011 to 2014 period was concentrated in specific regions, which drives this result. The map of employment change in Exhibit 2 provides support for this inference. When adding regional dummy variables, *labor force age* is positive and significant at the 10% confidence level and *manufacturing* is positive and significant at the 99% confidence level. However, these positive correlations are balanced by the highly negative coefficients associated with the *region—New England*, *region—Mideast*, and *region—Great Lakes*, which are significant at the 99%, 99%, and 90% confidence levels, respectively. By contrast, *region—Southwest*, *region—Rocky Mountain*, and *region—Far West* show significant, positive correlations. Adding the regional variables does not change the results for private equity investment, which shows a significant, positive correlation with a beta coefficient of 1.3.

It is notable that across Models 2–5, the results for the private equity investment variable are stable. *Private equity investment* is positive and significant in these models at the 95% level, and the beta coefficients are remarkably similar. Based on these results, every one million dollars of private equity investment in a county yields an average of 1.31 to 1.36 net new jobs, holding other model variables constant.

Log-Log Regression

The values of the key variables, *employment change* and *private equity investment*, display similar traits. Each variable has a large range of values (see Exhibit 4) with distribution that exhibits right skewness, where the right side of the tail is longer than the left side. For these reasons, this section explores the log transformations of these variables in the model.[7]

Because over 2,000 US counties did not receive any private equity investment during 2011 or 2012, the log-log model drops these observations from the analysis. The log-log model design reduces our sample size to 329 counties, but it has an advantage of elucidating how the size of private equity investment can affect employment.

Substituting log transformations for the dependent and key explanatory variables in the model produces complementary results (Exhibit 6) to the level-level models.

[7] All logarithmic transformations in this study are natural log transformations.

EXHIBIT 6

Determinants of Employment Change Log-Log Regression Models and Results (robust standard errors)

	Model 6
Intercept	3.38
	(1.41)**
Employment baseline	0.00
	(0.00)***
Ln (private equity investment)	0.06
	(0.03)**
Unemployment	0.05
	(0.04)
Education	0.01
	(0.01)
Labor force age	–0.06
	(0.03)**
Annual average pay	0.00
	(0.00)**
Minimum wage	0.08
	(0.13)
Right-to-work state	0.28
	(0.21)
Metro	1.42
	(0.20)***
Manufacturing	0.20
	(0.11)*
Services	2.66
	(0.71)***
Finance	0.19
	(0.19)
Region—New England	0.01
	(0.30)
Region—Mideast	–0.14
	(0.26)
Region—Great Lakes	0.19
	(0.26)
Region—Plains	–0.13
	(0.30)
Region—Southwest	0.80
	(0.26)***
Region—Rocky Mountains	0.38
	(0.24)
Region—Far West	0.71
	(0.29)**
R^2	0.7284
observations	329

*Notes: *significant at the 90% confidence level. **significant at the 95% confidence level. ***significant at the 99% confidence level.*

First, two core explanatory variables now show a positive, significant correlation in the log-log model, *average annual pay* and *metro*. Similar to previous models, *manufacturing*, *region—Southwest*, and *region—Far West* are also positively correlated. However, the direction of correlation flips for *labor force age* and *services*, compared to previous models. The former confirms a predicted negative influence on the log transformation of *employment change*, whereas the latter shows a predicted positive relationship in the log-log model. The direction of these correlations support our original hypotheses. Finally, the model again results in a significant positive correlation between *private equity investment* and *employment change*, albeit with natural log transformations for both variables.

With regard to the impact of private equity investment on employment, the interpretation of these log-log regression results is similar to the level-level model. Exhibit 6 shows that a 1% change in private equity investment yields a 0.06% change in employment, holding all other variables in the model equal.

CONCLUSION

Scholarly research on private equity's effect on job creation has typically been limited by data challenges. This study provides a new approach. It analyzes the employment impact of private equity investment at a level at which more data are available. By analyzing the employment impact at a geographic level—US counties—we are able to draw on labor economics literature to informs which demographic characteristics create predispositions to employment growth. Controlling for these factors, this early study supports the view that private investment improves local employment and therefore has a positive economic impact.

One could critique the analysis because it measures the overall effect of private equity investment on local employment instead of job changes at the specific company receiving private equity investment. However, the results in this study underscore conclusions drawn from past research showing that private equity investment creates positive externalities that may be absorbed by local industry and, thus, create more employment (Aldatmaz and Brown 2017). Indeed, an increase in employment—or even more secure employment—could stimulate consumer spending and business-to-business transactions, which positively affects local companies. Following this hypothesis, we might consider that this study's estimations capture the externalities of private equity investment and resulting local employment growth.

Past research analyzing the determinants of employment growth provided a rich resource for designing the models in our analysis. Indeed, this study is unusual because it marries a financial investment topic with hypotheses more often found in labor economics and socioeconomic studies. Estimations of the impact of private investment on employment include variables controlling for labor supply, labor quality, labor cost, unionization, agglomeration and urbanization, industry concentration, and regional location.

The results of this study show a positive correlation between private equity investment and employment growth at the county level. All six models find the influence of private equity investment on employment to be positive and statistically significant at the 95% level. The level-level models estimate that, on average, private equity investment increases employment growth by 1.31 to 1.36 employees per million dollars of investment. The log-log models find that a 1% increase in private equity investment leads to a 0.06% increase in employment.

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ADDITIONAL READING

Divisional Reverse Leveraged Buyout

Finishing School or Financial Arbitrage?
Michael R. Braun and Anurag Sharma
The Journal of Private Equity
https://jpe.pm-research.com/content/11/1/7

ABSTRACT: *We examine the hypothesis that the leveraged buyout improves the operating performance of units that are divested from corporations. To do so, we focus on an historical sample of Divisional Reverse Leveraged Buyout (D-RLBO) and compare it with a matched sample of spinoffs. We find that prior to divestiture, the D-RLBO units in our sample were healthier than the divested units that were spun off. Furthermore, although the finishing school of leveraged buyout did not improve the operating performance of the D-RLBO units, relative to the matched spinoff sample these units performed well in terms of market returns one-year following the IPO. Our results suggest that buyout specialists selected better performing assets ex-ante and that they did not necessarily enhance their operating performance before bringing them up to the public markets.*

Valuing a Leveraged Buyout

Expansion of the Adjusted Present Value by Means of Real Options Analysis
Francesco Baldi
The Journal of Private Equity
https://jpe.pm-research.com/content/8/4/64

ABSTRACT: *A major flaw in using the adjusted present value (APV) method to value the target firm of a leveraged buyout is that it results in systematic undervaluation of the firm in question. The author proposes to remedy the problem by employing real options analysis to expand the target firm's APV on its equity side. Two real*

options may be identified as being inherent in the leveraged buyout structuring process: a financial default call option and an operating default call option, both of American type. The resulting expanded equity value allows for the flexible management of firm value uncertainty while continuing to incorporate the extra value creation deriving from the exploitation of the tax shield.

The Effects of Leverage, Management Discipline, and Cyclicality on Leveraged Buyout Failure
Harlan D. Platt and Marjorie B. Platt
The Journal of Alternative Investments
https://jai.pm-research.com/content/1/4/28

ABSTRACT: *Despite the impact of market conditions on the popularity and success of leveraged buyouts, research is also needed on conditions leading to the failure of LBOs. In this article, the impacts of leverage, management processes, and the business cycle are analyzed in regard to their association with the failure of LBOs.*

NBER WORKING PAPER SERIES

ECLIPSE OF THE PUBLIC CORPORATION OR ECLIPSE OF THE PUBLIC MARKETS?

Craig Doidge
Kathleen M. Kahle
G. Andrew Karolyi
René M. Stulz

Working Paper 24265
http://www.nber.org/papers/w24265

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Doidge is from the Rotman School of Management at the University of Toronto, Kahle is from the Eller School of Management at the University of Arizona, Karolyi is from the Cornell S.C. Johnson College of Business at Cornell University, and Stulz is from the Fisher College of Business at The Ohio State University, NBER, and ECGI. Parts of this paper update and discuss results from Doidge, Karolyi, and Stulz (2017) and Kahle and Stulz (2017). Other parts are new. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

At least one co-author has disclosed a financial relationship of potential relevance for this research. Further information is available online at http://www.nber.org/papers/w24265.ack

Eclipse of the Public Corporation or Eclipse of the Public Markets?
Craig Doidge, Kathleen M. Kahle, G. Andrew Karolyi, and René M. Stulz
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ABSTRACT

Since reaching a peak in 1997, the number of listed firms in the U.S. has fallen in every year but one. During this same period, public firms have been net purchasers of $3.6 trillion of equity (in 2015 dollars) rather than net issuers. The propensity to be listed is lower across all firm size groups, but more so among firms with less than 5,000 employees. Relative to other countries, the U.S. now has abnormally few listed firms. Because markets have become unattractive to small firms, existing listed firms are larger and older. We argue that the importance of intangible investment has grown but that public markets are not well-suited for young, R&D-intensive companies. Since there is abundant capital available to such firms without going public, they have little incentive to do so until they reach the point in their lifecycle where they focus more on payouts than on raising capital.

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1. Introduction

In 1989, Jensen wrote that "the publicly held corporation has outlived its usefulness in many sectors of the economy." He published in the Harvard Business Review an article titled "The Eclipse of the Public Corporation." There, Jensen argued that the conflict between owners and managers can make public corporations an inefficient form of organization. He made the case that new private organizational forms promoted by private equity firms reduce this conflict and are more efficient for firms in which agency problems are severe. In 1989, there were 5,895 U.S. domiciled listed firms on the U.S. exchanges. This number reached a peak in 1997 at 7,509. As of the end of 2016, the number of U.S. listed firms was down to 3,618. Though the number of listed firms did not initially fall as predicted by Jensen, it eventually did, and dramatically so. Since 1997, the number of listed firms has fallen every year but 2014.

One might easily conclude that this dramatic drop in the number of public corporations represents the eclipse of the public corporation predicted by Jensen in the late 1980s. However, at the same time, hugely profitable and successful public companies such as Google, Apple, Amazon, Microsoft, and Facebook, with market capitalizations that could conceivably reach $1 trillion in the not too distant future, have arisen and flourished. Paradoxically, we seem to have some of the most profitable and successful companies in the history of U.S. capital markets and, at the same time, a collapse in the number of public firms. One common characteristic of Google, Apple, Amazon, Microsoft, and Facebook is that these companies have vastly more intangible than tangible capital. In this article, we argue that U.S. public markets have shown themselves not well-suited to satisfy the financing needs of young firms with mostly intangible capital. In that sense, what we are really witnessing is an eclipse not of public corporations, but of the *public markets* as the place where young successful American companies seek their funding.

In this paper, we first show how the number of listed firms has evolved in the U.S. and abroad. We next show that in the U.S. small firms have left the exchanges and that the propensity of these small firms to list has fallen sharply since 1997. We then show how listed firms have changed in the U.S. In the last section of the paper, we investigate whether the changes that have taken place represent an eclipse of the public corporation in the U.S., an eclipse of the public exchanges, or whether we need a different explanation to make sense of them.

2. The drop in U.S. listings in perspective

Figure 1 shows the evolution of the number of U.S. domiciled firms listed on the NYSE, Amex, and Nasdaq from 1975 to 2016.[1] In 1975, the U.S. had 4,818 listed firms. The figure shows that this number increased rather steadily until 1997, when it reached 7,509 listed firms. From that year onward, the number fell rapidly until 2003 and then at a slower pace. However, the number of listed firms kept falling until 2013, when it reached 3,657. From 2013 to 2014, the number of listed firms increased by 128, but then it fell again, so that in 2016 it was 3,618. As of the end of 2016, the number of listed firms was 25% less than in 1975 and 52% less than its peak in 1997. It is especially striking that the number of firms has fallen so much given that during this time the population of the U.S. increased from 215 million in 1975 to 323 million in 2016. In 1975, the U.S. had 22.4 listed firms per million inhabitants. By 2016, it had just 11.2.

Figure 1 also shows the evolution of the aggregate market capitalization of listed firms, or the sum of the market value of all listed firms. In 2015 dollars, the aggregate market capitalization of listed firms was 7.4 times higher in 2016 compared to 1975. However, in contrast to the evolution of the number of listed firms, the aggregate market capitalization does not evolve smoothly. This is especially true after 1999. In constant dollars, the aggregate market capitalization of listed firms was only $434 billion dollars higher at the end of 2016 than it was at the end of 1999. It is common to look at the aggregate market capitalization of stocks compared to GDP. Many academic studies use this ratio as a measure of financial development.[2] This ratio was 38.3% in 1975. It peaked at 153.5% in 1999, dropped to 69.2% in 2008, and increased back to 124.0% in 2016. The ratio in 2016 is 19% lower than at its peak.

[1] We use two main data sources for our analysis of U.S. firms: CRSP and Compustat. From CRSP, we obtain all U.S. firms listed on the NYSE, AMEX, and Nasdaq, excluding investment companies, mutual funds, REITs, and other collective investment vehicles. When we examine Compustat data, we use the intersection of CRSP and Compustat firms. For non-U.S. firms, we use data from the World Bank's World Development Indicators database and from the World Federation of Exchanges. The construction of the database for foreign exchanges is described in Doidge, Karolyi, and Stulz (2017). To update the database, we follow the approach described in that paper. Note that while it seems easy to figure out the number of listed firms in a country, it is not always so as a number of data choices must be made. For example, updates of public databases such as CRSP and Compustat can make retroactive changes to past counts that can lead to different estimates for the U.S.

[2] See, for example, Levine (1997).

The fact that the market capitalization of the U.S. markets is not higher partly reflects the same phenomenon as the decrease in the number of listed firms. Since the peak in listings in 1997, U.S. firms have been repurchasing dramatically more equity than they have issued. The excess of the amount spent on repurchases over the amount received from equity issuance since 1997 is $3.6 trillion. In other words, U.S. public firms returned significantly more equity capital to shareholders than they raised from the capital markets.

To understand the drop in the number of listed firms since 1997, it is important to understand whether this is a global phenomenon. Another way to frame the question is to ask whether, as a result of this drop, the U.S. now has too few listed firms relative to other countries. Doidge, Karolyi, and Stulz (2017) compile a database of listings across the world since 1990. Figure 2 updates that database and shows the evolution of the number of listed firms in the U.S. compared to the number of listed firms in non-U.S. countries and in non-U.S. developed countries. Neither the number of listed firms in non-U.S. countries nor the number of listed firms in non-U.S. developed countries exhibits a dramatic drop since the late 1990s. In fact, the number of listed firms increases for all non-U.S. countries and even increases among non-U.S. developed countries, but in these latter countries it has been fairly stagnant since 2003. The law and economics literature argues that more prosperous countries, faster growing countries, and countries that protect investor rights better have more listed firms per capita (for example, Djankov, La Porta, Lopez-de-Silanes, and Shleifer, 2008). Using a regression model that relates the number of listed firms to these and other country characteristics, Doidge, Karolyi, and Stulz (2017) confirm that the U.S. indeed has relatively fewer listed firms than other countries with similar characteristics. They refer to this deficit of listed firms in the U.S. as "the U.S. listing gap." Importantly, the existence of a U.S. listing gap does not mean that no other country has a listing gap. What it does mean, however, is that the shortage of listed firms observed in the U.S. is not a global phenomenon. The magnitude of this gap is large and it persists since 2002. Doidge, Karolyi, and Stulz (2017) predict that if the U.S. had as many listed firms per capita as countries with similar GDP per capita, GDP growth, and quality of protection of investor rights, in 2012 it would have had 9,538 listings instead of 4,102.

3. Disappearing small firms

For the number of listed firms to fall, there must be fewer new lists and/or more delists. In other words, firms must be leaving public stock exchanges faster than others enter exchanges. The number of new lists in the U.S. has been extremely low for the last fifteen years and especially so since 2008. The average annual number of new lists from 2009 to 2016 is 179, according to the Center for Research in Security Prices (CRSP). In contrast, the average annual number of new lists from 1995 to 2000 is 683.5. In other words, since the global financial crisis, the average annual number of new lists is less than one third of what it was between 1995 and 2000.

Delisting counts have fallen also, but less than new lists. Firms delist because their performance does not allow them to remain listed, because they voluntarily choose to delist, or because they are acquired. The most important cause of delists since the listing peak is mergers and acquisitions. Since the listing peak, there have been 8,620 delists, according to CRSP. Of these delists, CRSP reports that 5,274, or 61.2% of the total, are due to mergers, 3,060, or 35.5%, are delists due to performance, and only 286, or 3.3%, are voluntary delists. Until the listing peak, both mergers and voluntarily delists were relatively less important as they account for 55.2% and 1.7% of delists, respectively, from 1975 to 1997. Though much has been made of voluntarily delists in the media and popular press, there are simply too few firms that leave the exchanges because they want to, and do so without being acquired, for them to be an important part of the explanation for the overall drop in listed firms.

Everything else equal, research shows new lists are smaller firms and smaller firms are more likely to delist. Hence, a drop in new lists means *relatively* fewer small young firms. As a result of fewer new lists and of delists, the disappearance of small firms from public exchanges has been dramatic. As shown in Figure 3, the percentage of firms with market capitalization below $100 million in 2015 dollars has collapsed over the last forty years. From 1975 to 1991, more than 50% of firms had a market capitalization of less than $100 million. After 1991, this percentage drops steadily. In 1997, it falls below 40% for the first time over our sample period. Since 2003, that percentage never exceeds 30%. In 2016, it is 22%. If there are fewer small firms on public exchanges, the average market capitalization must have increased.

Indeed, it has done so dramatically. In 2015 dollars, the average market capitalization in 1975 was $662 million. At the peak of listings, it was about $2 billion. Since the number of listings started collapsing, the average market capitalization has basically tripled as it now exceeds $6 billion.

Having more delists than new lists implies that small young firms drop off exchanges faster than others enter. It is therefore not surprising that the average age of a listed firm has increased substantially. At the peak of listings, the average age of a listed firm was 12 years. In 2016, the average age was 20 years. Older firms tend to be less dynamic and more set in their ways (see, for example, Loderer, Stulz, and Walchli, 2016).

This disappearance of small firms on U.S. exchanges and the associated increase in the size of listed firms is not accompanied by a disappearance of small firms outside the exchanges. In contrast, however, firms are becoming older both on exchanges and outside exchanges (Hathaway and Litan, 2014). Data on private firms is hard to come by, but there is good data for the distribution of firm size, when size is measured by the number of employees. Doidge, Karolyi, and Stulz (2017) show that the main driver of the drop in listings is not a shift in the population of firms but rather a drop in the propensity of firms to be listed. Their data starts in 1977 and stops in 2012. We update this data through 2015, which is the last year for which it is available from the Longitudinal Business Database (LBD) of the U.S. Census Bureau. Tiny firms with fewer than 20 employees are extremely unlikely to be listed at any point in time. In 2015, the U.S. had almost 4.5 million firms with less than 20 employees and 615,048 firms with more than 20 employees. Since tiny firms are not relevant for our analysis of the overall propensity to list on major exchanges, we exclude tiny firms from our analysis.

In aggregate, the number of firms with more than 20 employees has increased since the listing peak. In 1997, the U.S. had 560,861 firms with more than 20 employees. By 2015, this number increased to 615,048. At the same time, the rate of increase in new firms has been dramatically slower since the listing peak. From 1977 to 1997, the number of firms with 20 employees or more increased at an average annual rate of 3.2% per year. From 1998 to 2015, the average annual rate of increase is half a percent per year. The drop in the average annual rate of increase in firms after the listing peak gives an excessively pessimistic view of the growth in the number of firms because the financial crisis adversely impacted the number of firms. The number of firms with 20 employees or more reached a peak of 636,904 in 2007. It then fell to a trough of 569,569 in 2011. From 2011 to 2015, the number of firms increased at the rate of 2% per year, which is slightly larger than the average rate of increase of 1.5% from the peak to the crisis.

The important takeaway from these counts is that, excluding tiny firms, the propensity to be listed on a major exchange fell by 54% from the listing peak to 2015. Figure 4 shows the drop in the propensity to list across firm size categories. Though the drop in the propensity to list is smaller for the largest firms, the propensity has fallen for all firm sizes since the listing peak. This evolution implies that the distribution of firm size for listed firms has distinctly tilted more towards large firms than before the listing peak. In 1997, 0.23% of the firms with 20 to 99 employees were listed on exchanges. By the end of 2015, that percentage fell by 67% to 0.076%. The percentage of firms that choose to list has fallen by more than 60% for firms with less than 1,000 employees. It has fallen for larger firms as well, but by a slower rate. For instance, by 1997, 58% of firms with more than 10,000 employees were listed. This percentage has fallen to 44% by 2015, or by 24% since 1997.

The same U.S. Census data that we use to estimate the listing propensity across firm size also has information that allows us to estimate the listing propensity by coarse industry categories up to 2014. The propensity to list falls across all industry categories.

4. How listed firms have changed

Listed firms are quite different now compared to listed firms in the 1970s. Looking at averages is helpful to understand how firms have changed. Averaging across all listed U.S. firms covered by both CRSP and Standard & Poor's Compustat, the ratio of capital expenditures to research and development (R&D) expenses was 6-to-1 in 1975. In other words, on average, firms spent 6 times more on capital expenditures than they spent on R&D. Capital expenditures accumulate on a firm's balance sheet as tangible assets. On a balance sheet, fixed assets are assets that are purchased for long-term use, such as land, building, and equipment. In other words, a firm in 1975 had fixed assets corresponding to 34.4% of its assets. If we now look at 2016, on average R&D expenditures were 7.3% of assets in contrast to capital expenditures which were only 3.8% of assets. In other words, capital expenditures were, on average, just 51% of a firm's R&D expenses. On average, fixed assets are now 19.6% of total assets.

As shown in Figure 5, R&D expenditures for the average firm exceeded capital expenditures for the first time in 2002. And, since 2002, R&D expenditures have exceeded capital expenditures every year. The ratio of average capital expenditures to average R&D expenditures was lowest in 2016. In contrast, from 1975 to 2016, the highest ratio was 6.85-to-1 in 1978. The decrease in the ratio of capital expenditures to R&D expenditures can be explained by a decrease in capital expenditures as well as by an increase in R&D expenditures. The ratio of capital expenditures to assets fell sharply starting in 2001. In 2016, average capital expenditures to assets was 3.8% which is the lowest ratio in any year since 1975 except for 2009. In contrast, the average ratio of R&D expenditures to assets was 7.3% in 2016, which is the second highest ratio in any year, but just slightly lower than the 7.4% of 2015.

Though we focused on firm averages, it is important to note that there is large variation across firms in how much they spend on R&D. Many very large firms spend hardly anything on R&D (including Walmart, Berkshire Hathaway, AT&T, Verizon, and Exxon). It follows that looking at averages across firms can overstate the importance of R&D compared to capital expenditures for the economy as a whole, because the ratio of R&D to assets is negatively correlated with size. In dollar terms, R&D expenditures are still less than capital expenditures. What the averages do show is that this is not so for the average firm – there are large numbers of small public firms for which R&D is much more important than capital expenditures.

The evolution of the ratio of capital expenditures to R&D expenditures is indicative of an important transformation of public firms in the U.S. They have become firms for which intangible assets are typically more important than tangible assets. U.S. Generally Accepted Accounting Principles (GAAP) makes it difficult to assess the value of a firm's intangible assets. Firms invest in intangible assets when they train their employees, when they improve their organizational structure, when they develop new systems, when they build their brand, and so on. U.S. GAAP generally requires such transactions to be expensed. Abstracting from taxes, if a firm spends $1 on research that could lead to a profitable new product, its current profitability falls by $1 and its assets fall by $1 because it spent cash. If a firm spends an additional $1 on new machinery, its total assets are unaffected as the decrease in cash is offset by an increase in fixed assets. Further, ignoring tax considerations, spending an additional $1 on capital expenditures has no impact on current profitability as that expenditure is capitalized instead of being treated as an expense. Economists have worked hard to estimate the intangible assets of firms. Falato, Kadyrzhanova, and Sim (2013) estimate that, on average across firms, intangible assets accounted for 10% of net assets (assets minus cash holdings) in 1970, but exceeded 50% in 2010.

When Jensen wrote his article in 1989, he was concerned that managers would hoard and waste resources rather than return cash to shareholders. He called this problem "the agency cost of free cash flow." Back in 1989, U.S. firms held on average 13.6% of their assets in cash. In contrast, in 2016, the average ratio of cash holdings to assets was 21.5%, which was the highest ratio from 1975 to 2016. The increase in cash holdings of U.S. firms is an important change in the composition of assets of these firms, the cause of which has been widely debated. One possible explanation is consistent with Jensen's concerns, namely, CEOs may want to hoard resources rather than pay out profits to shareholders that they cannot reinvest profitably. There are two reasons to be skeptical of this explanation. First, as intangible assets become more important, one would expect firms to hold more cash (Bates, Kahle, and Stulz, 2009). A firm can use tangible assets as collateral to borrow, but it may find it much more difficult, if not impossible, to use intangible assets. With this logic, the increase in the importance of intangible assets predictably leads to an increase in cash holdings.

A second key reason to be skeptical about the importance of agency costs associated with resource hoarding is that U.S. firms have extremely high payout rates in recent years, which represents another important way in which firms have changed. In 1975, 63% of firms paid dividends and on average dividends were 1.3% of assets. In 2000, the percentage of firms paying dividends reached a low point of 30%. Since then, the percentage of firms paying dividends has increased, and it was 42.4% in 2016. Further, while in the early 2000s, average dividend payouts to assets were 0.4%, they are now approximately 1%. In 1975, payouts were almost exclusively in the form of dividends. In 2016, repurchases represented a larger proportion of payouts than dividends. Throughout the 2000s, as shown in Figure 6, repurchases have exceeded dividends as a fraction of assets, typically by a ratio of more than two to one.

Another useful way to see the change in the extent to which U.S. firms pay out their profits to shareholders is to look at payouts relative to net income. Figure 6 also shows the ratio of payouts to net income. In 1975, the average percentage of net income paid out by firms was 26.8%. This percentage reached a low of 20.1% in 1994, only a few years after Jensen's article was published. After 1994, the percentage increased but then fell again to 20.9% in 2001. However, the percentage in 2016 was 44.6%. To put this number in perspective, the first year since 1975 that the payout to net income ratio exceeded 30% was in 2004. Since 2004, this ratio has fallen below the 30% threshold only once (2009). In recent years, this ratio has always been above 40%.

In this analysis, we have focused much on averages across firms and over time. Such an analysis does not give a good understanding of the magnitude of the flows from corporations to shareholders through repurchases in the years since the listing peak. First, in four of the twenty years since 1997, U.S. firms have repurchased more equity than they have issued. The net amount of repurchases over issuance from 1997 to 2016, which represents the net flows going from all corporations to shareholders, amounted to $3.6 trillion in 2015 dollars. In other words, in the typical year since the listing peak, the corporate sector has returned equity capital to shareholders. From 1975 to 1996, the corporate sector issued more equity than it repurchased in 15 years out of 22, so that in a typical year before the listing peak the corporate sector issued more equity than it repurchased. Since 1996, it has repurchased more equity than it issued. Such a shift makes it hard to believe that hoarding of resources by empire-building CEOs is a concern for the corporate sector as a whole and that this hoarding explains the drop in listings. Obviously, while free cash flow concerns do not seem to be a useful explanation for the overall drop in listings, there are many firms where increased ownership concentration or going private transactions were motivated by the existence of important agency costs of free cash flow.

5. Which eclipse is the real one?

In 1975, the top five listed U.S. firms by market capitalization had a total market capitalization of half a trillion in 2015 dollars. In 2016, the top five firms had a total market capitalization of $2.3 trillion. Such evidence is hard to reconcile with a view that the public corporation is in eclipse. The winners in public markets are doing very well indeed. At the same time, however, there are ever fewer public firms and the firms that are public on balance return more equity to shareholders than they invest. This seems to imply that small young firms do not want to use the public markets to obtain funding and believe that they can obtain such funding on better terms elsewhere. It also means that these firms believe that their owners can cash out on better terms by being acquired than going public. As a result, public markets are not attractive for many of these firms and it may be that it is public markets that are in eclipse.

A persistent argument is that firms do not want to be public because of regulation. Those who advance that argument often invoke the Sarbanes-Oxley Act of 2002, Regulation Fair Disclosure (Reg FD), and other restrictions imposed on analysts and the financial services community in the early 2000s. The biggest deficiency of this argument is that the peak for listings takes place in 1997, well before Sarbanes-Oxley and these other major regulatory events. If any regulatory actions played a role in the decrease in listings in the 1990s, it was the *deregulatory* actions that increased the number of investors beyond which a firm has to register its securities.[3] In other words, this deregulation made it easier for firms to raise funds while staying private. Further deregulatory actions took place after the 1990s.

Firms that go public may benefit from having securities registered with the U.S. Securities Exchange Commission (SEC). It allows them to issue more shares, to issue public debt under favorable conditions, and to use their equity as a form of currency to make acquisitions. It allows insiders to reduce their stakes and to diversify their holdings. However, public firms are subject to strict disclosure rules and have to

[3] See de Fontenay (2016). She points out that a 1996 change in section 3(c)(7) of the Investment Company Act effectively removed the 100-investor cap on private investment funds, which in turn made possible the existence of vastly larger funds.

follow U.S. GAAP accounting rules. Both the disclosure rules and GAAP accounting can be problematic for firms that are heavy in intangible assets (see, for example, Leuz and Wysocki, 2016).

If a firm is building a new plant, it is easy for it to disclose that it is doing so. Nobody can steal the plant. The same is not the case if the firm has an intensive R&D program. By disclosing details of that program, a firm gives away some of its ideas. Other firms can build on what they learn. While a firm will try to reveal as little as possible that could be appropriated, it faces the difficult issue that if it discloses too little, outsiders cannot assess its value correctly and are likely to value it at a discount. As a result, the firm is stuck between the proverbial rock and hard place. If it discloses too much, its value falls because outsiders can use what it discloses to enrich themselves, but if it discloses too little, its shares are discounted due to investor uncertainty.

GAAP creates problems of its own. Accounting rules, by definition, are conservative. If a firm acquires a building, it will record it at cost. The belief is that the building was acquired at a market price and could be sold at that market price. However, if a firm spends a lot of money on salaries of researchers, accounting does not treat these salaries as an investment in a research project that is an asset on its balance sheet. Rather, these salaries are treated as a cost that decreases the profitability of the firm. It follows that GAAP may have an inherent bias against intangible assets (Lev and Gu, 2016). Accounting is not as informative for firms with intangible assets as it is for firms with tangible assets. Public investors rely, among other things, on accounting data to assess the value of a firm. If that accounting data is not very informative, these investors will be more skeptical about the value of a firm. Conservative accounting is valuable for firms that want to issue public debt as it provides a better approximation of the collateral available to protect the debtholders. However, firms with large amounts of intangible assets typically do not issue public debt. Intangible assets are usually poor collateral for loans.

Jensen believed that concentrated ownership is valuable in reducing agency costs of free cash flow. Concentrated ownership helps resolve other issues as well. A firm with valuable intangible assets can better convey information about the value of these assets without worrying about expropriation when it can do so for large potential investors in its equity rather than when it has to do so through mandated public disclosures via the SEC. It can do so even better if the potential investors have specialized knowledge about the type of intangible capital the firm is developing, which would generally be the case for venture capitalists and private equity investors. Hence, private forms of equity financing are likely to be preferred by non-public firms that are involved in building intangible assets because they can provide better information to non-public capital providers and these non-public capital providers are in a better position to assess the value of the intangible assets the firm is building. Viewed from this perspective, accessing the public markets to obtain equity capital can only be a second-best solution.

If private funding were not easily available, there would be more public offerings. However, having more public offerings because of a lack of private funding would likely be bad for innovation since public funding involves important frictions that make it less attractive than private funding. Private funding has not been limited in such a way that it has pushed firms to public markets early in their lives. There are at least three reasons for that. First, as already discussed regulatory changes have made it easier to raise funds privately (see, for example, de Fontenay, 2016). Second, technological changes have made it much easier to search for investors and to gather information. Third, young firms do not require as much capital in their build-up phase as they used to (among others, see Davis, 2016). In light of these developments, it is perhaps not surprising that Ewens and Farre-Mensa (2017) document that privately-held startups can now “achieve capital raising (…) historically available only to their public peers.”

The internet has dramatically reduced the costs of search. This applies to finding investors. However, perhaps more importantly, it has made it possible for young firms to find and contract for a wide variety of services that they would have had to build in-house at great expense in the past. A firm with a good idea for a manufacturing product can easily get it produced abroad without having to build a plant. A firm that needs lots of computing power can lease it at low cost. A firm can now more easily rent a back office. All these changes mean that the early stages of the life of a firm require much less capital than they used to. To see this, think of a world where a young firm has to manufacture products on its own. Such a firm would have to raise a large amount of capital to build and outfit a plant.

Gao, Ritter, and Zhang (2013) argue that economies of scope have become more important and that firms have a shorter window to take advantage of them because of the widening threat of greater competition. If this is true, firms may be better off to be acquired by a larger firm rather than to access the public markets to raise capital. The role of economies of scope is closely tied in to the importance of intangible capital. In reviewing the properties of intangible assets, Haskel and Westlake (2017) point out that one key fact is that intangible assets are scalable in a way that tangible assets are not. If a car manufacturer wants to produce twice as many cars, it has to double its manufacturing plant. Doing so requires a large amount of capital. Being acquired by another car company would not make a manufacturing plant available unless that company has an idle manufacturing plant. By contrast, a firm that has developed a new software tool can increase its sales of that tool at a marginal cost that is close to zero. Hence, its main concern is to sell as much of that tool as possible until it is replaced by a better tool. Having access to a platform with broader visibility and distribution ability would be valuable to such a firm.

Exit through acquisition rather than exit through public markets has another important advantage for a firm rich in hard-to-value intangible assets. In accessing public markets, the firm has to convince dispersed shareholders of its value without giving away too much information about its intangible assets. After all, other competitor firms can exploit that public information to gain an advantage. In contrast, in being acquired, a firm has to convince potential acquirers of its value. It can be a setting in which the firm can disclose more with less risk and generally can disclose to potential buyers with specialized knowledge that are in a good position to assess the value of the firm's intangible assets with greater precision than dispersed shareholders.

Other developments have also played a role in the decrease in the number of listed firms. As we saw, mergers are the main factor leading to an increase in delistings. While historically the literature in financial economics has emphasized the role of mergers in improving efficiency by creating synergies, it is not clear how well this view of mergers applies to the kind of mergers that took place in the 2000s. For instance, an important paper by Blonigen and Pierce (2016) uncovers evidence that gains from mergers are due to increased margins, which means the benefits come from a *decrease* in competition. There is increasing evidence of a decrease in competition in many industries in the U.S. (such as, the Council of Economic Advisors, 2016; Grullon, Larkin, and Michaely, 2016). Such a decrease in competition might affect adversely the ability of small firms to succeed on their own.

6. Some speculation about the future of public equity markets

Public markets are better suited for firms with mostly tangible assets than for firms with mostly intangible assets. This is especially true when the usefulness of the intangible assets has yet to be proven on a large scale. Sometimes the market is extremely optimistic about some intangible assets, which confers a window of opportunity on firms with such assets to go public. But otherwise, firms with unproven intangible assets may very well be better off to fund themselves privately. Accounting information conveyed by U.S. GAAP for such firms is of limited use because GAAP treats investments in intangible assets mostly as expenses, so that these assets may very well not show up on firms' balance sheets. Private funding allows firms to convey information about intangible assets more directly to potential investors who often have specialized knowledge, something that they could not convey publicly.

Much of the public debate about the lack of new public offerings has focused on the intensity of capital market regulation. One might be tempted to say that if part of the problem is disclosure, then we should relax mandated disclosure rules. This would be a misreading of our argument. The issue with disclosure of intangible assets is not what firms have to disclose. Rather, it has to do with the nature of the intangible assets they need to disclose. Once an idea is made public it becomes possible for other firms to use it. Deregulation that ends up reducing the trust that investors have in public markets will not lead to more new offerings in the long run.

Investment in intangible assets is highly sensitive to the legal environment in which a firm operates and to the pace of financial development it experiences. A plant is hard to steal. A new idea is not. The U.S. is a country where some firms make massive investments in intangible assets. Empirically, the most R&D intensive firms in U.S. public markets do not have counterparts in foreign public markets (see, among others, Pinkowitz, Stulz, and Williamson, 2015). As intangible assets continue to increase in importance, it should not surprise us to see a further eclipse of public markets. This stalling of public equity market development should be more pronounced in a country like the U.S., where intangible assets are relatively more important for the corporate sector. This evolution has a downside: investors limited to public markets are cut off from investing in high intangible-asset firms. Another downside is that, as fewer firms remain publicly listed on major exchanges, the transparency of public markets applies to fewer firms and more firms are not transparent to society, which may limit overall support for the corporate sector in the long-run. However, this evolution also reflects that U.S. financial development has evolved in such a way that some types of firms can be financed more efficiently through private sources than through public capital markets because the intrinsic properties of intangible assets make it harder for them to be financed in public markets. No deregulatory action is likely to restore the public markets in this case. Instead, we should focus on creating a fertile ground for investment in intangible assets by having appropriate laws, appropriate financing mechanisms, and maybe new types of exchange markets, as these assets appear to be the way of the future for corporations.

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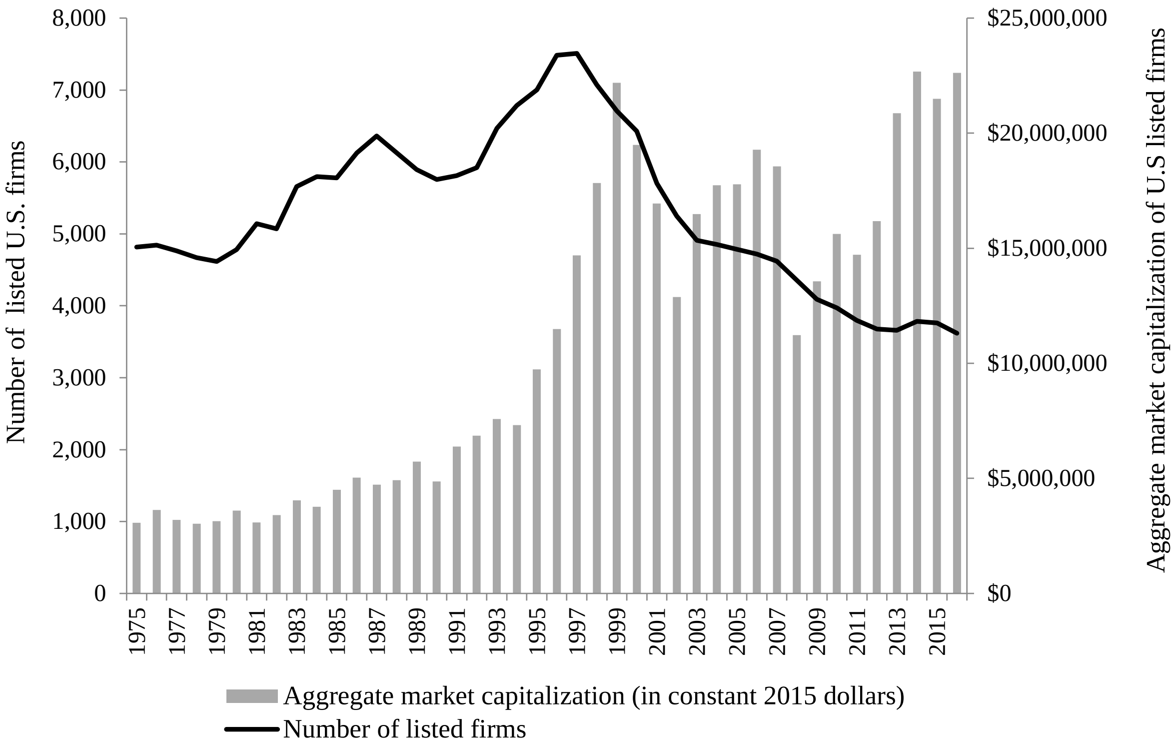
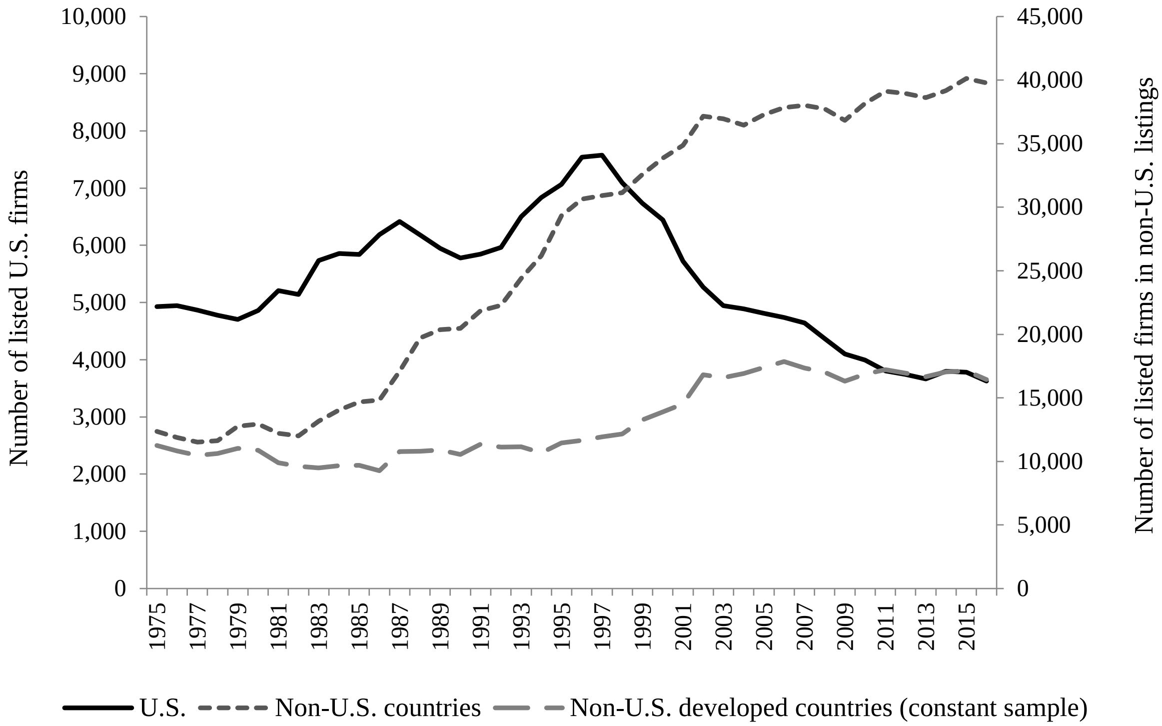
Figure 1. The number of listed U.S. firms and their aggregate market capitalization.



Source: The Center for Research in Security Prices (CRSP).

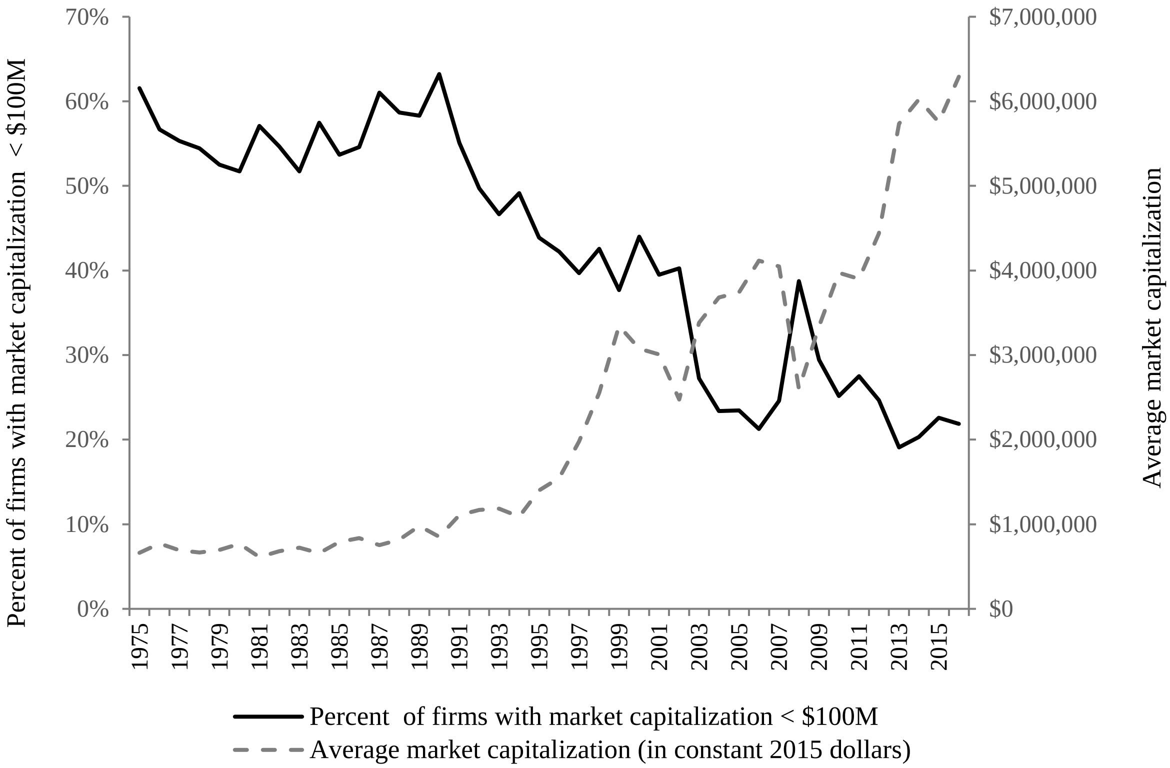
Note: Listed firms include U.S. firms in CRSP that are listed on the NYSE, AMEX, and Nasdaq. Investment companies, mutual funds, REITs, and other collective investment vehicles are excluded. Aggregate market capitalization is in 2015 dollars. The sample period is from 1975 to 2016.

Figure 2. The number of listed firms.



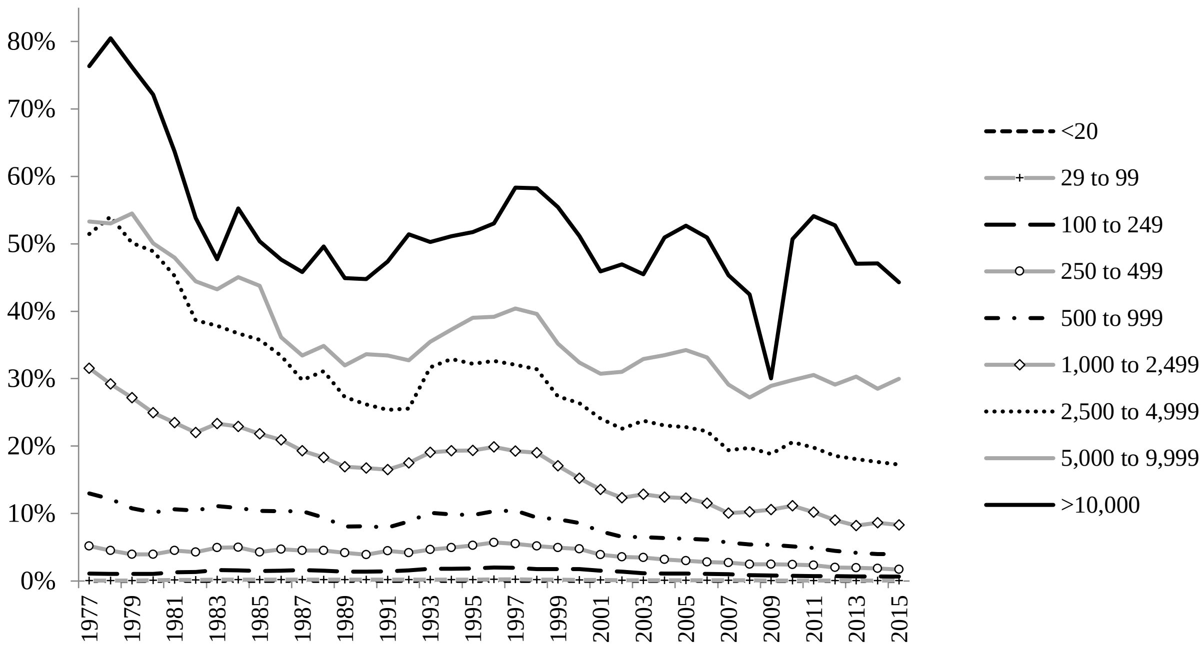
Source: Center for Research in Security Prices (U.S. firms) and the World Bank's World Development Indicators database and the World Federation of Stock Exchanges (non-U.S. firms).
Notes: Listing counts include domestic firms. They exclude investment companies, mutual funds, REITs, and other collective investment vehicles. There are 71 non-U.S. countries. Countries are classified as developed based on the MSCI classification scheme as of 2014. There are 13 non-U.S. developed countries in the constant sample. The sample period is from 1975 to 2016.

Figure 3. The percentage of listed U.S. firms with market capitalization less than $100M and average market capitalization.



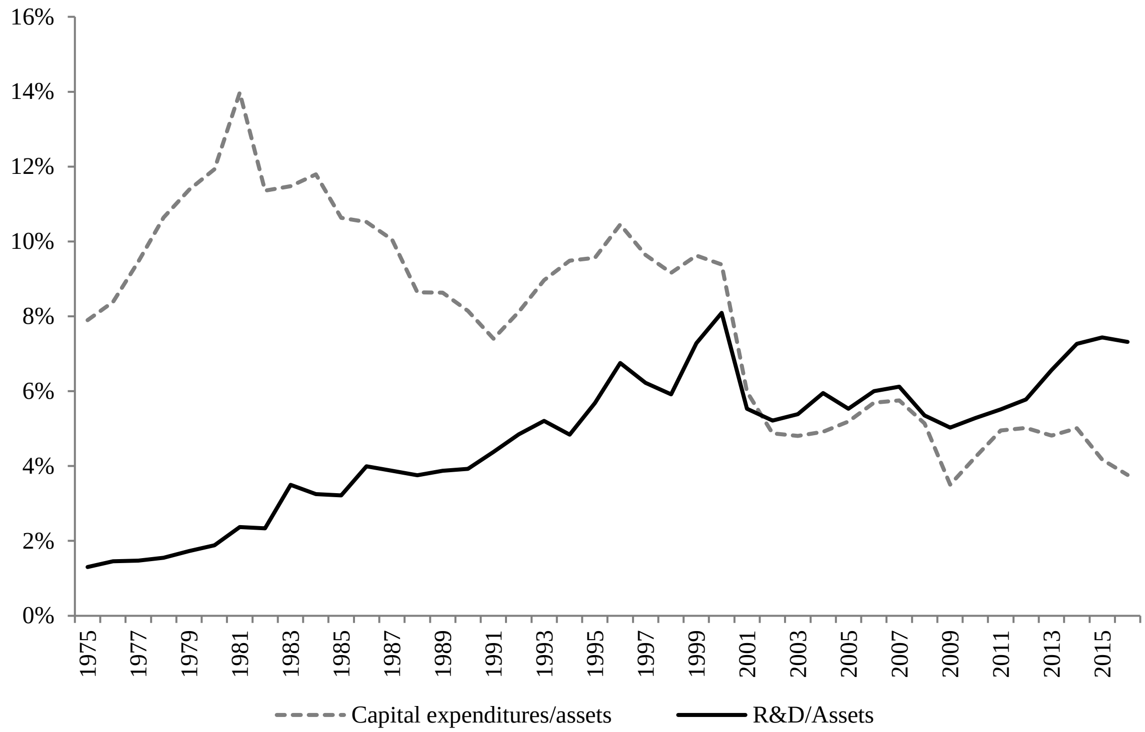
Source: The Center for Research in Security Prices (CRSP).
Note: Listed firms include U.S. firms in CRSP on the NYSE, AMEX, and Nasdaq. Investment companies, mutual funds, REITs, and other collective investment vehicles are excluded. Market capitalization is in 2015 dollars. The sample period is from 1975 to 2016.

Figure 4. Firm size, industry, and listing propensity.



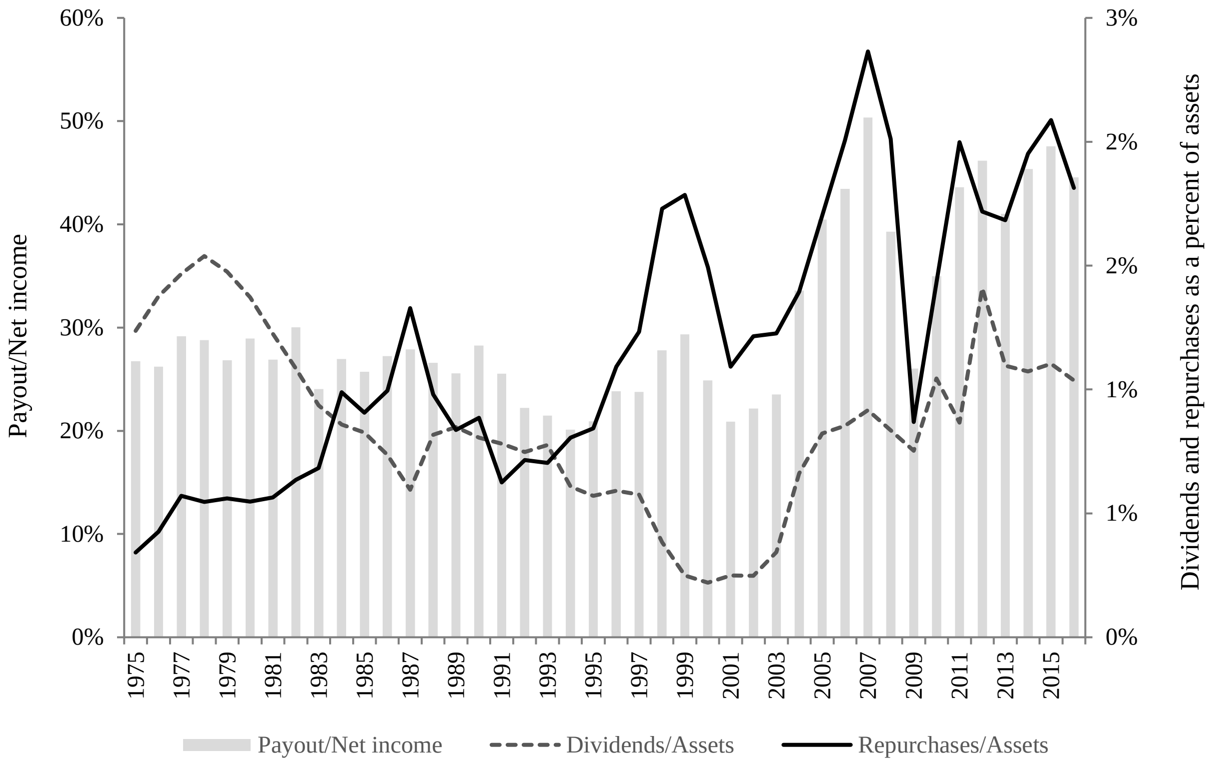
Source: The Center for Research in Security Prices (CRSP), Compustat, and the U.S. Census Bureau's Longitudinal Business Database.
Notes: Listed firms include U.S. firms in CRSP and Compustat on the NYSE, AMEX, and Nasdaq that we can assign to an employee size group. Investment companies, mutual funds, REITs, and other collective investment vehicles are excluded. The percentage of firms that are listed in each employee size group equals listed firms / total firms, where total firms includes public and private firms. The sample period is from 1977 to 2015.

Figure 5. Capital expenditures versus research and development expenditures.



Source: The Center for Research in Security Prices (CRSP) and Compustat.
Note: Listed firms include U.S. firms in CRSP and Compustat on the NYSE, AMEX, and Nasdaq. Investment companies, mutual funds, REITs, and other collective investment vehicles are excluded. Capital expenditures/assets equals capital expenditures divided by lagged assets. R&D/assets equals R&D divided by lagged assets. If R&D is missing, it is set equal to 0. The sample period is from 1975 to 2016.

Figure 6. Firm payout policy.



Source: The Center for Research in Security Prices (CRSP) and Compustat.
Note: Listed firms include U.S. firms in CRSP and Compustat on the NYSE, AMEX, and Nasdaq. Investment companies, mutual funds, REITs, and other collective investment vehicles are excluded. Dividends/Assets equals ordinary cash dividends divided by lagged assets. Repurchases/Assets equals the purchase of stock minus any decrease in preferred stock, divided by lagged assets. Payout/Net income equals dividends plus repurchases, divided by net income. The sample period is from 1975 to 2016.