NBER WORKING PAPER SERIES

THE ECONOMIC EFFECTS OF PRIVATE EQUITY BUYOUTS

Steven J. Davis
John C. Haltiwanger
Kyle Handley
Ben Lipsius
Josh Lerner
Javier Miranda

Working Paper 26370
http://www.nber.org/papers/w26370

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2019

Davis, Haltiwanger, Handley, and Lerner are affiliates of the National Bureau of Economic Research. Haltiwanger was also a part-time Schedule A employee at the U.S. Census Bureau during the preparation of this paper. We thank Ron Jarmin, Steve Kaplan, Ann Leamon, Antoinette Schoar (discussant), and Kirk White for helpful comments, as well as seminar participants at the American Economic Association 2019 annual meeting, Georgia Tech, Harvard Law School, the Hoover Institution, MIT, and the NBER Productivity Lunch Group. Alex Caracuzzo, Stephen Moon, Cameron Khansarinia, Ayomide Opeyemi, Christine Rivera, Kathleen Ryan, and James Zeitler provided painstaking research assistance. Per Strömberg generously gave permission to use transaction data collected as part of our World Economic Forum project. We thank the Harvard Business School's Division of Research, the Private Capital Research Institute, the Ewing Marion Kauffman Foundation, and especially the Smith Richardson Foundation for generous research support. Opinions and conclusions expressed herein are those of the authors and do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed (DRB-B0109-CDAR-2018718, DRB-B0110-CDAR-2018-0718, DRB-B0020-CED-20181128, DRB-B0018-CED-20181126, and CBDRB-FY19-CMS-8034). Lerner periodically receives compensation for advising institutional investors in private equity funds, private equity groups, corporate venturing groups, and governments designing policies related to private equity. All errors and omissions are our own. 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/w26371.ack

The Economic Effects of Private Equity Buyouts
Steven J. Davis, John C. Haltiwanger, Kyle Handley, Ben Lipsius, Josh Lerner, and Javier Miranda
NBER Working Paper No. 26371
October 2019
JEL No. D22,D24,G24,G34,J63,L25

ABSTRACT

We examine thousands of U.S. private equity (PE) buyouts from 1980 to 2013, a period that saw huge swings in credit market tightness and GDP growth. Our results show striking, systematic differences in the real-side effects of PE buyouts, depending on buyout type and external conditions. Employment at target firms shrinks 13% over two years in buyouts of publicly listed firms but expands 13% in buyouts of privately held firms, both relative to contemporaneous outcomes at control firms. Labor productivity rises 8% at targets over two years post buyout (again, relative to controls), with large gains for both public-to-private and private-to-private buyouts. Target productivity gains are larger yet for deals executed amidst tight credit conditions. A post-buyout widening of credit spreads or slowdown in GDP growth lowers employment growth at targets and sharply curtails productivity gains in public-to-private and divisional buyouts. Average earnings per worker fall by 1.7% at target firms after buyouts, largely erasing a pre-buyout wage premium relative to controls. Wage effects are also heterogeneous. In these and other respects, the economic effects of private equity vary greatly by buyout type and with external conditions.

Steven J. Davis
Booth School of Business
The University of Chicago
5807 South Woodlawn Avenue
Chicago, IL 60637
and NBER
Steven.Davis@ChicagoBooth.edu

John C. Haltiwanger
Department of Economics
University of Maryland
College Park, MD 20742
and NBER
haltiwan@econ.umd.edu

Kyle Handley
Ross School of Business
University of Michigan
701 Tappan Street
Ann Arbor, MI 48109
and NBER
handleyk@umich.edu

Ben Lipsius
University of Michigan
618 South Main Street, Apt 512
Ann Arbor, MI 48104
blipsius@umich.edu

Josh Lerner
Harvard Business School
Rock Center 214
Soldiers Field
Boston, MA 02163
and NBER
jlerner@hbs.edu

Javier Miranda
U.S. Bureau of the Census
Economy-Wide Statistics Division
4600 Silver Hill Road
Washington, DC 20233
javier.miranda@census.gov

Policymakers have enacted and proposed several initiatives in the past decade to address the perceived harms of private equity. For example, the European Union implemented an Alternative Investment Fund Managers Directive to prevent "asset stripping" from private firms after acquisition by private equity or other financial sponsor.[2] As another example, European Central Bank Guidance on Leveraged Transactions requires stringent internal review of "all types of loan or credit exposures where the borrower is owned by one or more financial sponsors."[3] Senator Elizabeth Warren recently introduced the "Stop Wall Street Looting Act" to broadly regulate private equity in the United States.[4] Gregory (2013) argues that buyouts should be monitored for macro-prudential reasons, because their high indebtedness "poses risk to the stability of the financial system." Similar concerns animate U.S. regulatory guidance of leveraged lending to facilitate buyouts and post-buyout activities of target firms.[5] Appelbaum and Batt (2014) and Phalippou (2017), among others, see private equity's heavy reliance on debt financing and intense focus on investor returns as having negative effects on firm performance, employment, and wages.

We speak to these policies, proposals, and concerns by examining the real-side effects of private equity (PE) buyouts of U.S. firms from 1980 to 2013. To carry out our study, we use an improved version of the large-sample methodology in Davis et al. (2014). Specifically, we first

---

[2]Alternative Investment Fund Managers Directive 2011/61/EU, https://eur-lex.europa.eu/legal-content/EN/TXT/PDF/?uri=CELEX:32011L0061&from=EN, Chapters IV and V, especially Chapter V, Section 2, Articles 26–30.
[3]https://www.bankingsupervision.europa.eu/ecb/pub/pdf/ssm.leveraged_transactions_guidance_201705.en.pdf, Section 3. Among other things, this ECB Guidance states that "Underwriting of transactions presenting high levels of leverage … should remain exceptional … and trigger a referral to the highest level of credit committee or similar decision-making level."
[4]https://www.warren.senate.gov/imo/media/doc/2019.7.17%20Stop%20Wall%20Street%20Looting%20Act%20Text.pdf, Section 3 (13).
[5] See, for instance, Office of the Comptroller of the Currency, Board of Governors of the Federal Reserve System, and Federal Deposit Insurance Corporation, "Interagency Guidance on Leveraged Lending," at www.federalreserve.gov/supervisionreg/srletters/sr1303a1.pdf, pp. 1-7.

tap multiple sources to identify and characterize about 9,800 PE buyouts of U.S. firms. For roughly 6,000 of these buyouts, we successfully merge their information with comprehensive Census micro data on firm-level and establishment-level outcomes. Next, we estimate the effects of buyouts on employment, job reallocation, labor productivity, and compensation per worker at target firms relative to contemporaneous developments at comparable firms not backed by private equity. Our large sample, long time period, high-quality data, and ability to track firms and establishments enable a careful look at buyout effects. Because our sample encompasses huge swings in credit market tightness and macroeconomic performance, we can address questions about how these external conditions relate to the performance of target firms.

We find striking and systematic outcome differences depending on buyout type, credit market conditions at the time of buyout, and the evolution of macroeconomic and credit conditions post buyout. Our chief findings pertain to outcomes at buyout targets relative to control firms over the first two years after the buyout:

- Relative to control firms, employment at targets rises 13 percent in firms previously under private ownership (private-to-private buyouts) and 10 percent in secondary buyouts (sale from one PE entity to another). Employment falls by 13 percent in buyouts of publicly listed firms (public-to-private deals) and by 16 percent in divisional buyouts.
- The overall average employment impact of PE buyouts is a statistically insignificant -1.4 percent in our sample. After netting out post-buyout acquisitions and divestitures to isolate organic changes, the overall average impact is -4.4 percentage points.
- The pace of intra-firm job reallocation at target firms rises relative to control firms post buyout. This pattern holds across all buyout types, and much of it reflects greater acquisition and divestiture activity by target firms.

- Labor productivity rises by an average of eight percent at target firms (again, relative to controls), a striking impact given that targets tend to be mature firms in mature industries. Productivity gains are concentrated in private-to-private and public-to-private buyouts.
- Target productivity gains and intra-firm job reallocation increases are larger yet (relative to controls) for deals executed amidst tight credit conditions.
- A post-buyout widening of credit spreads or slowdown in GDP growth lowers employment growth at targets and sharply curtails productivity gains in public-to-private and divisional buyouts.
- Compensation per worker falls by 1.7% at target firms after buyouts, largely erasing a pre-buyout wage premium relative to controls. Wage effects also differ greatly by buyout type.

In short, the impact of private equity is more complex and varied than champions or detractors claim. Proponents such as Jensen (1989) see buyouts as engines of efficiency and value creation, fueled by the concentrated ownership of target firms, highly levered capital structures, and high-powered financial incentives. Critics see these same features as harmful to targets and their workers and as a source of systemic risk.[6] We find strong support for the engines-of-efficiency view in the most prevalent deal types. With respect to employment and wage effects of buyouts, our evidence is mixed and contingent on deal type. The post-buyout performance of target firms also varies with external credit and macroeconomic conditions.

[6] Early studies on the real-side firm-level outcomes associated with PE buyouts include Kaplan (1989) and Lichtenberg and Siegel (1990). More recent work considers larger samples, often by exploiting a combination of proprietary and government databases. See, for instance, 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). Davis et al. (2014) also summarize several case studies.

Previous research also finds differences by buyout type, as we discuss in Section IV. Public-to-private buyouts involve greater leverage and bankruptcy risk but few advantages in financial returns, at least in recent decades. Private-to-private buyouts appear more likely to create value by relaxing financial constraints and improving management practices. While earlier studies lead us to anticipate differences, we offer a more systematic examination of real-side outcomes in a much larger sample of buyouts and one with wide swings in external conditions.

There is also previous work 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, where company-level characteristics were much more influential. Kaplan and Schoar (2005), among others, find that easier credit conditions bring greater inflows into buyout funds and lower fund-level returns.[7] In short, the literature suggests that when economic growth booms and credit spreads narrow, private equity funds attract larger inflows, their deals involve more leverage and higher valuations, and investors ultimately receive lower returns. We add to knowledge about buyout deal flow over time and its sensitivity to external conditions. Our more

[7] 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).

important contribution in this regard, however, is to systematically investigate how the real-side effects of PE buyouts relate to credit market and macroeconomic conditions.

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 from the 1870s to the 2000s (e.g., Reinhart and Rogoff, 2009; Campello, Graham, and Harvey, 2010; and Schularick and Taylor, 2012) and a first-order concern for modern central bankers. We develop new evidence on how target-firm performance relates to credit market conditions at the time of the buyout and afterwards.

Our study also speaks to broader concerns about financialization of the economy. The financial sector's share of U.S. Gross Domestic Product (GDP) rose from less than four percent in 1950 to eight percent in 2010, accelerating after 1980, as documented by Philippon (2015). He also provides evidence that the cost of financial intermediation has changed remarkably little since the nineteenth century, despite dramatic advances in information technology that might be expected to lower the costs of creating, pooling, holding, and trading financial assets. Zingales (2015) argues that the financial sector is prone to agency problems and other inefficiencies that create a range of distortions in the real economy, many of which are poorly understood and neglected by scholars. We cast new light on how one increasingly important form of financialization relates to the performance of the affected firms.

The next section describes the creation of our sample. Section II discusses our empirical methodology. Section III presents our baseline results on the economic effects of PE buyouts, and Section IV considers how the effects vary with cyclical and credit conditions. Section V concludes.

I. 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 later-stage 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 facilitates 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. 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,[8] 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 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

[8] These include Dealogic, Preqin, and Thomson Reuters.

databases, securities filings, and media accounts. The resulting sample contains 9,794 PE-led leveraged buyouts of U.S. companies from January 1, 1980 to December 31, 2013.[9]

We sort the sample buyouts into four main deal types: the buyout of an independent, privately held firm (private-to-private), the buyout of a publicly listed firm (public-to-private), the buyout of part of a firm (divisional), and the sale of portfolio firms from one PE firm to another (secondary). We derive our classifications from the textual descriptions in CapitalIQ, as well as our own reviews of other databases, press accounts, and securities filings.

B. *Inspecting the Full Sample (Before Linking to Census Data)*

Figure 1 displays quarterly counts of PE-sponsored buyouts in our sample for these four deal types.[10] As noted in other studies, PE buyout activity grew enormously in recent decades. The 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 it remained at modest levels through

---

[9] Ayash and Rastad (2017) criticize our approach to distinguishing LBOs from growth equity buyouts, because "data service providers have difficulty differentiating between leverage buyouts and growth equity buyouts." They advocate a transaction value cutoff approach, based on the idea that smaller deals are more likely to be growth equity buyouts. The cutoff approach yields two types of errors: (*a*) in deals larger than the cutoff, the improper inclusion of ***all*** growth equity buyouts, and (*b*) in deals smaller than the cutoff, the improper exclusion of ***all*** leveraged buyouts. While some early papers in the PE literature used size cut-offs, they did so due to a lack of data on smaller transactions (e.g., Kaplan, 1989, footnote 3). Most major papers in the recent literature on PE rely on classification methodologies resembling ours to a greater or lesser extent. Examples include Axelson et al. (2013), Faccio and Hsu (2017), Fang, Ivashina, and Lerner (2013), and Ivashina and Kovner (2011).

[10] Appendix Table B.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.

the end of our sample. Counts for private-to-private deals and secondary sales 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 IV 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 B.2.) Axelson and co-authors (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 B.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.

C. *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 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.

[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.

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. Appendix A describes our matching process in detail. 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. There are other challenges as well, as discussed in Appendix A. 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**.**

D. 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.[12]

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.

E. 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.

[12] 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.

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 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.

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. These compositional changes over time can also be seen in Figure 1.

In our econometric analysis below, we limit attention to matched buyouts that closed from 1980 to 2001, 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.

II. Empirical Methods and Identification Assumptions

This section describes several important 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 $g_{it,t-k} = \left(E_{it} - E_{i,t-k}\right)/X_{it,t-k}$, where $X_{it,t-k} = 0.5\left(E_{it} + E_{i,t-k}\right)$. This growth rate measure is symmetric about zero and lies in the interval [-2, 2], with endpoints corresponding to death and birth.[13] Employment growth at higher levels of aggregation is then given by $g_{t,t-k} = \sum_i \left(X_{it,t-k}/X_{t,t-k}\right) g_{it,t-k}$, 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.

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

[13] 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).

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.[14] 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.[15] 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, wages, and productivity at target firms in the wake of buyouts to contemporaneous changes at their matched control units.[16] This approach, together with our control variables, facilitates an apples-to-apples comparison when estimating buyout effects.

---

[14] 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).

[15] 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.

[16] 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.

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, we find material differences in the effects of buyouts on some of these dimensions, as discussed below. 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.[17]

To achieve our estimation goal, we adopt two principles in weighting the observations:[18]

- **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.

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 $\omega_{2,t+k,t} \equiv$

[17] See Chapter 18 in Wooldridge (2002) for an extended discussion of CMI and SUTVA in panel regression studies of treatment effects.
[18] Neither equal weighting nor simple activity weighting of regression observations recovers the average treatment effect of interest.

$X_{2,t+k,t}/(X_{1,t+k,t} + X_{2,t+k,t})$. Since each control cell has a single target, these are also the control cell weights.[19] 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.[20]

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

[19] 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.

[20] A subtle issue with weighting had to do with divisional buyouts, where one unit was spun out of a larger entity. Here we used the employment in the spun-out entity after the buyout transaction, not that of the corporate parent.

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 here.

III. Estimated Buyout Effects on Jobs, Reallocation, Productivity and Wages

A. *Regression Specification and More on Identification*

Our firm-level regression analysis considers the same type of semi-parametric specifications as our earlier paper. To be precise, we estimate specifications of the following form by least squares, weighting each observation as detailed in Section II:

$$Y_{i,t+2} = \alpha + \sum_c D_{cit}\theta_c + \lambda_1 LEST_{it} + \lambda_2 LFIRM_{it} + \gamma PE_{it} + \varepsilon_{it}, \quad (1)$$

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*. 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 has one or 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 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. Per our discussion of weighting in Section II, the coefficient γ recovers a consistent estimate of the weighted average treatment effect on treated units (i.e., buyout targets) under assumptions CMI and SUTVA. 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. *Average Treatment Effects Over All 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 many times larger, because most target firms (and the corresponding control firms) have multiple facilities. We have fewer usable observations for compensation and labor productivity measures, 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 B.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 are of great interest, our earlier work shows that buyouts have larger effects on the pace of job reallocation. Recall that *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 of its establishments and cuts an equal number of jobs at other establishments, then its excess reallocation equals its overall job reallocation.[21]

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.

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,

[21] 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.

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.

Panel D in Table 3 provides new evidence on the wage effects of PE buyouts using a larger, broader sample than previous studies. Our wage measure is the change from buyout year t to $t+2$ in the firm's gross annual compensation per employee. [22] In computing firm-level wage changes, we restrict attention to establishments owned and operated by the firm in both t and $t+2$. This choice reduces the scope for workforce composition changes to drive our estimated wage effects. The wage sample is smaller than in Panels A-C for three reasons. First, we cannot calculate wage changes for firms that close all establishments by $t+2$. When we drop a target that dies in this sense, we also drop controls in the cell associated with that target. In particular, 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. That results in the loss of additional observations. Third, compensation data are missing for some firms in the LBD.

[22] 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 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.

The first column in Panel D reports a statistically significant wage drop of 1.7% 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. In Table A.2, we also estimate the target-control differential in compensation per worker as of the buyout year *t*, again restricting attention to establishments owned by the firm in *t* and *t+2*. There, we find a wage premium of 2.5% in favor of targets. Because this estimate does *not* net out target-control differences in workforce composition, it is more susceptible to an omitted variables bias. This concern aside, our point estimates imply that buyouts erase 70% of the modest pre-buyout wage premium enjoyed by workers at target firms.

Panel E in Table 3 provides evidence on how PE buyouts affect firm-level labor productivity, measured as the natural log of revenue per worker.[23] Relative to Panels A-C, we lose observations for the same three reasons in Panel E as in Panel D. However, the number of observations lost due to missing revenue data is much larger than the number lost due to missing wage data. In addition, 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 that arise because of errors in the revenue data, errors in linking revenue and employment data 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

[23] 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.

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 D of Appendix Table B.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.

Case studies provide insights into how PE buyouts achieve productivity gains. We summarize two well-documented cases. In late 1987, Berkshire Partners bought out the Lake States Transportation division of the Soo Line, renaming it Wisconsin Central. The new management cut operating employees per train from 4.8 to 2.2 and cut wages by 15%.[24] As a result, labor costs dropped from the historical 50% of revenue to 32% in 1988. In later years, Wisconsin Central

[24] The vast majority 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.

continued to improve labor productivity through the application of better information technology and tight management, with revenue ton miles per hour worked rising from 1376 in 1989 to 2120 in 1995.[25] In another case, Clayton, Dubilier & Rice bought out Hertz in 2006. By the time it sold its last equity stake in 2013, the workforce at Hertz had shrunk by 3.5% and its revenues had grown more than 25%, yielding nearly a 30% gain in real revenue per worker. To achieve these productivity gains, the PE group reduced overhead costs, rationalized rental facilities (particularly those not at airports), and upgraded a management team that presided over "a post office like culture."[26] While not all buyouts yield productivity gains, our evidence says that many do.[27]

C. *Treatment Effects 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. Meanwhile, 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.

Appendix Table B.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

---

[25] Jensen, Burkhardt, and Barry (1989) and SEC filings.

[26] Luehrman and Scott (2007), Louie at al. (2018), and SEC filings.

[27] Blackstone's buyout of Celanese is another well-documented case with large productivity gains. See, for instance, El-Hage and Luehrman (2009).

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.

Perhaps this heterogeneity should not surprise. Public-to-private deals 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. Some divisional sales involve units that fit poorly with the pre-buyout parent firm. In other divisional sales, the parent firm recognizes a need for downsizing but offloads that unpleasant task to shield its public image and preserve employee morale in the rest of the firm. Buyouts of privately held firms may more often be motivated by a desire to professionalize management or improve access to financing. Some secondary sales reflect an incomplete, ongoing effort to improve operations and profitability in the target firm or a hasty, but successful exit by the first PE owner to pave the road to raising a new buyout fund.

Turning to Panels B and C, we see that 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. A similar pattern holds for secondary deals, but the magnitudes are smaller and not always statistically significant. 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.

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 offers a case in point.[28]

Turning to productivity effects, we again find large differences by type of buyout. 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 divisional and secondary buyouts and statistically insignificant.

We see the results in Table 3 as 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

[28] 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.

exhibit high post-buyout employment growth (largely but not entirely via acquisitions), wage reductions, and large productivity gains. These results align with those of Boucly, Sraer, and Thesmar (2011), who analyze a sample of largely private-to-private buyouts of French firms. They conclude that these buyouts eased financing constraints at target firms, enabling their expansion. Large productivity gains also 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 predominate. They find that PE buyouts bring better management practices.

Public-to-private deals exhibit large job losses, often through facility closures, and large (imprecisely estimated) productivity gains. An important role for facility shutdowns is consistent with evidence of higher bankruptcy rates after public-to-private deals (Strömberg, 2008) and high debt burdens (Axelson et al., 2013). The concentration of public-to-private deals in advance of credit-market tightening (Table 1) may contribute to employment losses, a hypothesis we consider in Section IV. Consistent with our productivity results, Kaplan (1989) suggests that buyouts in the 1980s led to sharp improvements in financial performance relative to industry peers. Guo, Hotchkiss, and Song (2011) question this conclusion based on examination of public-to-private buyouts between 1990 and 2006. Cohn, Nestoriak, and Wardlaw (2017) find that workplace injury rates but fell after public-to-private buyouts but not after private-to-private ones.

Secondary deals exhibit high target employment growth, largely organic, high reallocation and few discernible effects otherwise. This pattern is broadly consistent with Degeorge, Martin, and Phalippou (2016), who find positive financial performance in many secondary deals. *Divisional* buyouts involve large employment losses and massive reallocation effects alongside large gains in compensation per worker. As we discussed above, the latter effect may partly reflect mid-level managers who take on more senior roles in the wake of divisional buyouts.

Large job losses after public-to-private and divisional buyouts could 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 even when they undermine the trust needed to sustain efficient contracting. If this hypothesis holds in our setting, we expect to see productivity losses at targets relative to controls in the wake of buyouts. Instead, we find large, though imprecisely estimated, productivity gains at targets in the wake of public-to-private buyouts. The productivity evidence is more consistent with the re-contracting hypothesis for divisional buyouts, but the wage evidence is not. It is possible that higher wages accrue mainly to senior managers, or that working conditions deteriorate in the wake of buyouts. Our evidence does not speak to those possibilities. More broadly, we cannot rule out any role for the type of re-contracting that undermines trust and efficient investment in the wake of PE buyouts. But the re-contracting hypothesis does not fit the main patterns of our evidence.

IV. How the Impact of Buyouts Varies with Market Conditions

A. *A Richer Regression Specification*

We now investigate how the economic effects of PE buyouts vary with market conditions. 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.

B. *How Buyout Effects Vary with Market Conditions at Close*

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 between high-yield U.S. corporate bonds and the one-month U.S. LIBOR in the closing month.[29] 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 with leverage in the buyout transaction and with the EBITDA-multiple paid, and positively with the ultimate financial return on the buyout to PE investors. At the same time, 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

[29] 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.

insignificant for each dependent variable. In contrast, higher credit spreads at close involve large, statistically significant effects on excess reallocation and productivity growth. [30] Raising the credit 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 easy, 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. This substitution between financial engineering and operational improvements may work through the selection of buyout targets, through the way PE

[30] In unreported results, we tried two other measures of external financial conditions: (a) the credit spread measure of Gilchrist and Zakrajšek (2012) and (b) equity market valuations, measured as the ratio of end-of month equity prices to the trailing twelve-month earnings S&P 500 firms. These measures yielded broadly similar, but somewhat noisier, results.

firms and senior managers in portfolio firms allocate their time and attention after the buyout, or through a combination of the two. Survey data in Bernstein, Lerner, and Mezzanotti (2019) 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 2007-09, when spreads were high. While we cannot pin down the mechanism, our results say that the marginal social return to PE buyouts in the form of target productivity gains rises and falls with the credit spread.

C. *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. 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 $t+2$.[31] 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 3.3 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, slower excess reallocation, and higher wage growth. These effects are statistically significant and similar in magnitude to the ones associated with a unit standard deviation change in the GDP growth rate.

[31] Similar results obtain when using the change from the buyout closing date in year t to March of year $t+2$.

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 credit cycles (Figure 1 and Table 1). These earlier results suggest that the sensitivity of targets to the post-buyout evolution of external market conditions may also differ by deal type.

As seen in Table 5, a post-buyout widening of credit spreads brings relative employment drops at target firms in private-to-private and secondary deals. A one standard deviation rise in the spread over two years after the buyout is associated with a relative employment drop of about 5 percentage points in both cases. The drop involves organic employment changes in secondary deals but mainly reflects a decline in acquisitions in private-to-private buyouts. Lower post-buyout GDP growth is also associated with lower employment growth (except for public-to-private deals, where the impact is negligible), but the effects are not statistically significant when cutting the sample by deal type.

In five out of eight reported regressions, we see that a deterioration in external market conditions (slower GDP growth or higher spreads) brings a significant decline in excess reallocation at targets relative to controls. In only one (statistically insignificant) case does the effect go in the opposite direction. Excess reallocation in target firms is especially sensitive to the post-buyout evolution of market conditions for public-to-private and divisional deals.

The wage analysis in Tables 5 and 6 uncovers a puzzling aspect of public-to-private deals. In these deals, a post-buyout deterioration in market conditions brings greater wage growth at target firms compared to control firms. These effects are statistically significant and large. Given the heavy debt loads of target firms in public-to-private deals (Axelson et al., 2013), one might anticipate that slower aggregate growth and costlier credit would lead to wage and benefit concessions by workers (e.g., Matsa, 2010, and Benmelech, Bergman, and Enriquez, 2012). We find just the opposite.

The productivity results in Tables 5 and 6 for public-to-private buyouts are more in line with stories about the downside of heavy debt loads. When GDP grows faster or credit spreads narrow, the relative productivity growth of target firms is appreciably higher. A similar pattern holds for divisional buyouts. Interestingly, the pattern consistently goes the other way in private-to-private deals: deteriorating economic conditions lead to greater productivity gains at targets relative to controls. One possibility is that the high leverage of public-to-private deals preclude management and investors from implementing the detailed operating plans developed in advance of the buyout, with implications for both productivity and wages. Private-to-private buyouts may not be as constrained.

We close the discussion of Tables 5 and 6 with a final remark. Often, we cannot draw sharp inferences about how PE buyout effects relate to the post-buyout evolution of external conditions when we split the sample by deal type. Still, we again find much evidence of heterogeneity in effects across buyout types. In this respect as well, there is little basis for treating private-to-private, public-to-private, divisional and secondary buyouts as homogeneous in their effects on jobs, reallocation, wages, and productivity.

V. 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." 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 by deal type and market conditions. 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. Our results also highlight how buyouts can lead to large productivity gains on the one hand and job and wage losses for incumbent workers on the other. This mix 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 place less emphasis on operating improvements when leverage and dividends deliver high private returns. Policies that harness the power of PE buyouts to drive productivity gains are more likely to bring high social returns along with high private returns.

Our evidence that buyout effects on employment growth are pro-cyclical, particularly for private-to-private and secondary buyouts, also warrants attention in future research. This aspect of our results suggests a "PE multiplier effect" that accentuates cyclical swings in economic activity. It resonates with concerns that private equity magnifies the effects of economic shocks. Our results also reinforce 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 large post-buyout employment losses and poor productivity performance during aggregate downturns.

Our analysis and results point to several other important questions: Do public-to-private and divisional buyouts cause avoidable employment losses? Or were target firms in dire need of restructuring and retrenchment to prevent worse outcomes at a later date? Given the productivity gains at target firms in the wake of public-to-private buyouts, were the matched control firms also in need of major restructuring? More broadly, are job losses and compensation cuts 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 points the way to future research on these issues.

Another set of questions involves whether and how the economic effects of buyouts vary across private equity groups. For example, (how) do real-side buyout effects vary with the experience and size of the PE group? Do certain PE groups consistently create private value by raising productivity at target firms (relative to otherwise comparable firms)? Kaplan and Schoar (2005) and later research find that financial performance varies across private equity groups in a manner that persists from fund to fund. In future work, we hope to explore whether and how the real-side performance of target firms varies across PE groups in a persistent manner.

References

Agrawal, Ashwini, and Prasanna Tambe, 2016. "Private equity and workers' career paths: The role of technological change." *Review of Financial Studies*, 29, 2455-2489.

Appelbaum, Eileen, and Rosemary Batt, 2014. *Private Equity at Work: When Wall Street Manages Main Street*. New York, Russel Sage Foundation.

Axelson, Ulf, Tim Jenkinson, Per Strömberg, and Michael Weisbach. 2013. "Borrow cheap, buy high? The determinants of leverage and pricing in buyouts." *Journal of Finance*, 68, 2223–2267.

Ayash, Brian, and Mahdi Rastad. 2017. "Private equity, jobs, and productivity: A comment." Unpublished working paper, https://ssrn.com/abstract=3050984.

Barth, Erling, Alex Bryson, James C. Davis, and Richard Freeman, 2014. "It's where you work: Increases in earnings dispersion across establishments and individuals in the U.S." National Bureau of Economic Research, Working paper no. 20447.

Benmelech, Efraim, Nittai K. Bergman, and Ricardo J. Enriquez, 2012. "Negotiating with labor under financial distress." *Review of Corporate Finance Studies*, 1, 28–67.

Bernanke, Ben, and Mark Gertler, 1987. "Banking and macroeconomic equilibrium," in *New Approaches to Monetary Economics: Proceedings of the Second International Symposium in Economic Theory and Econometrics,* edited by William A. Barnett and Kenneth J. Singleton. Cambridge: Cambridge University Press, 89-112.

Bernanke, Ben, Mark Gertler and Simon Gilchrist, 1999. "The financial accelerator in a quantitative business cycle framework," in *Handbook of Macroeconomics,* edited by John B. Taylor and Michael Woodford. New York, North-Holland, Volume I, 1341-1393.

Bernstein, Shai, Josh Lerner, and Filippo Mezzanotti, 2019. "Private equity and financial fragility during the crisis." *Review of Financial Studies*, 32, 1309-1373.

Bloom, Nicholas, Raffaella Sadun, and John Van Reenen, 2015. "Do private equity owned firms have better management practices?" *American Economic Review Papers and Proceedings*, 105, 442-446.

Boucly, Quentin, David Sraer, and David Thesmar, 2011. "Growth LBOs." *Journal of Financial Economics*, 102, 432–453.

Campello, Murillo, John R. Graham, and Campbell R. Harvey, 2010. "The Real Effects of Financial Constraints: Evidence from a Financial Crisis." *Journal of Financial Economics,* 97, 470-487.

Cohn, Jonathan B., 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.

Cohn, Jonathan B., Nicole Nestoriak, and Malcolm Wardlaw, 2019. "Private equity buyouts and workplace safety." Unpublished working paper, https://ssrn.com/abstract=2728704.

Davis, Steven J., and John Haltiwanger, 1992. "Gross job creation, gross job destruction, and employment reallocation." *Quarterly Journal of Economics*, 107, 819-863.

Davis, Steven J., and John Haltiwanger, 1999. “Gross job flows,” in *Handbook of Labor Economics,* edited by David Card and Orley Ashenfelter. New York: North-Holland, Volume 3B, 2711-2805.

Davis, Steven J., John Haltiwanger, Kyle Handley, Ron Jarmin, Josh Lerner, and Javier Miranda, 2014. “Private equity, jobs, and productivity.” *American Economic Review*, 104, 3956–3990.

Davis, Steven J., John Haltiwanger, Kyle Handley, Ron Jarmin, Josh Lerner, and Javier Miranda, 2018, “Private equity, jobs, and productivity: Reply to Ayash and Rastad.” Unpublished working paper, https://ssrn.com/abstract=3113272.

Davis, Steven J., John Haltiwanger, and Scott Schuh, 1996. *Job Creation and Destruction*. Cambridge, MA: The MIT Press.

Degeorge, Francois, Jens Martin, and Ludovic Phalippou, 2016. “On secondary buyouts.” *Journal of Financial Economics*, 120, 124-145.

Dunne, Timothy, Mark J. Roberts, and Larry Samuelson, 1989. “The growth and failure of U. S. manufacturing plants.” *Quarterly Journal of Economics*, 104, 671-698.

El-Hage, Nabil N., and Timothy A. Luehrman, 2009, “Blackstone/Celanese,” Harvard Business School simulation no. #3712.

Faccio, Mara, and Hung-Chia Hsu, 2017. “Politically connected private equity and employment.” *Journal of Finance*, 72, 539–574.

Fang, Lily, Victoria Ivashina, and Josh Lerner, 2013. “Combining banking with private equity investing.” *Review of Financial Studies*, 26, 2139-2173.

Farcassi, Cesar, Alessandro Previtero, and Albert Sheen, 2018. “Barbarians at the store? Private equity, products, and consumers.” Kelley School of Business, Indiana University, Research Paper no. 17-12, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2911387.

Gilchrist, Simon, and Egon Zakrajšek, 2012. "Credit spreads and business cycle fluctuations." *American Economic Review*, 102, 1692-1720.

Gompers, Paul, Steven N. Kaplan and Vladimir Mukharlyamov, 2016. “What do private equity firms say they do?” *Journal of Financial Economics*, 121, 449-476.

Gregory, David, 2013. “Private equity and financial stability,” *Bank of England Quarterly Bulletin*, no. 1, 38-47.

Guo, Shourun, Edie Hotchkiss, and Weihong Song, 2011, “Do buyouts (still) create value?,” *Journal of Finance*, 66, 479-517.

Haltiwanger, John, Ron S. Jarmin, Robert Kulick, and Javier Miranda, 2017. “High growth young firms: Contribution to job growth, output, and productivity growth” in *Measuring Entrepreneurial Businesses: Current Knowledge and Challenges*, edited by John Haltiwanger, Erik Hurst, Javier Miranda, and Antoinette Schoar. University of Chicago Press, 11-62.

Haltiwanger, John, Ron S. Jarmin, and Javier Miranda, 2013. "Who creates jobs? Small vs. large vs. young." *Review of Economics and Statistics*, 95, 347-361.

Harris, Robert S., Tim, Jenkinson, and Steven N. Kaplan, 2016. "How do private equity investments perform compared to public equity?" *Journal of Investment Management*, 14 (3), 1-24.

Hotchkiss, Edith S., Per Strömberg, and David C. Smith, 2014. "Private equity and the resolution of financial distress." European Corporate Governance Institute, Finance working paper no. 331, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=1787446.

Ivashina, Victoria, and Anna Kovner, 2011. "The private equity advantage: Leveraged buyout firms and relationship banking." *Review of Financial Studies*, 24, 2462–2498.

Jensen, Michael C., 1989. "The eclipse of the public corporation." *Harvard Business Review,* 67 (5), 61-74.

Jensen, Michael C., Willy Burkhardt, and Brian Barry, 1989. "Wisconsin Central Ltd. Railroad and Berkshire Partners (A) and (B): Leveraged buyouts and financial distress," Harvard Business School case nos. 190062 and 190070.

Kaplan, Steven N., 1989. "The effects of management buyouts on operating performance and value." *Journal of Financial Economics*, 24, 217–254.

Kaplan, Steven N., and Antoinette Schoar, 2005. "Private equity performance: Returns, persistence, and capital flows." *Journal of Finance*, 60, 1791–1823.

Kaplan, Steven N., and Jeremy Stein, 1993. "The evolution of buyout pricing and financial structure in the 1980s." *Quarterly Journal of Economics*, 108, 313–357.

Kindleberger, Charles P., 1978. *Manias, Panics, and Crashes: A History of Financial Crises.* New York: Basic Books.

Lerner, Josh, and Alexey Tuzikov, 2018. "The Carlyle Group and Axalta." Harvard Business School Case 9-818-040.

Leslie, Phillip, and Paul Oyer, 2008. "Managerial incentives and value creation: Evidence from private equity." National Bureau of Economic Research, Working paper no. 14331, https://www.nber.org/papers/w14331.

Lichtenberg, Frank R. and Donald Siegel, 1990. "The effects of leveraged buyouts on productivity and related aspects of firm behavior." *Journal of Financial Economics,* 27, 165–94.

Louie, Dickson L., Claudia Zeisberger, Peter Goodson, Nicholas Shannahan, and Kimberly McGinnis, 2018, "Private equity achieves returns through operating improvements: CD&R's acquisition and turnaround of Hertz," INSEAD Case no. IN1461.

Luehrman, Timothy A., and Douglas C. Scott, 2007, "Hertz Corporation (A) and (B)," Harvard Business School case nos. 208030 and 208031.

Matsa, David A., 2010. "Capital structure as a strategic variable: Evidence from collective bargaining." *Journal of Finance*, 65, 1197-1232.

Phalippou, Ludovic, 2017. *Private Equity Laid Bare*. Independently published, 2017.

Philippon, Thomas, 2009. "The bond market's q." *Quarterly Journal of Economics*, 124, 1011-56.

Philippon, Thomas, 2015. "Has the U.S. finance industry become less efficient? On the theory and measurement of financial intermediation." *American Economic Review*, 105, 1408-38.

Rajan, Raghuram G., 1994. "Why bank credit policies fluctuate: A theory and some evidence." *Quarterly Journal of Economics,* 109, 399-441.

Reinhart, Carmen M. and Kenneth S. Rogoff, 2009. *This Time Is Different: Eight Centuries of Financial Folly.* Princeton: Princeton University Press.

Schularick, Moritz and Alan M. Taylor, 2012. "Credit booms gone bust: Monetary policy, leverage cycles, and financial crises, 1870-2008." *American Economic Review,* 102, 1029-1061.

Shleifer, Andrei, and Lawrence H Summers, 1988. "Breach of trust in hostile takeovers," in *Corporate Takeovers: Causes and Consequences*, edited by Alan J. Auerbach. Chicago: University of Chicago Press, pp. 33-56.

Strömberg, Per, 2008, "The new demography of private equity," in *Globalization of Alternative Investment Working Papers: The Global Economic Impact of Private Equity Report*, edited by Anuradha Gurung and Josh Lerner. Geneva, World Economic Forum, vol. 1, 3-26.

Törnqvist, Leo, Pentti Vartia, and Yrjö Vartia, 1985. "How should relative change be measured?" *American Statistician,* 39, 43-46.

Wooldridge, Jeffrey M., 2002. *Econometric Analysis of Cross Section and Panel Data.* Cambridge: MIT Press.

Zingales, Luigi, 2015. "Presidential address: Does finance benefit society?" *Journal of Finance*, 70, 1327-1363.

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, Reallocation, Compensation, 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 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

	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary	
Dependent Variable	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2
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
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
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
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
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
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
Observations (000s)	6,400		3,900		400		2,300		600	
D. Annual Compensation Per Employee	-1.7*** [0.6]	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
Observations (000s)	3,900		2,100		200		1,500		300	
E. 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
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. Annual Compensation Per Employee	Coefficient	0.66	-0.65
	[St. Error]	[0.62]	[0.78]
	Unit S.D. Effect	2.0	-1.1
D. 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. 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	Secondary
A. Employment Growth, All Margins	Coefficient	-0.57*	-1.04**	-0.64	0.62	-0.75**
	[St. Error]	[0.30]	[0.48]	[0.39]	[0.66]	[0.30]
	Unit S.D. Effect	-2.8	-4.9	-2.6	2.1	-5.6
Organic Margins	Coefficient	-0.30	0.25	-0.51	0.36	-0.70***
	[St. Error]	[0.26]	[0.25]	[0.34]	[0.56]	[0.25]
	Unit S.D. Effect	-1.5	1.2	-2.1	1.2	-5.2
B. Excess Reallocation, All Margins	Coefficient	-0.64***	-0.19	-0.49*	-1.14**	-0.57*
	[St. Error]	[0.18]	[0.22]	[0.25]	[0.46]	[0.29]
	Unit S.D. Effect	-3.1	-0.9	-2.0	-3.9	-4.2
C. Annual Compensation Per Employee	Coefficient	0.33*	0.19	1.13***	-0.41	0.26
	[St. Error]	[0.20]	[0.22]	[0.23]	[0.33]	[0.24]
	Unit S.D. Effect	1.4	0.9	4.4	-1.4	1.6
D. Labor Productivity	Coefficient	-1.43	1.70*	-4.94**	-1.83**	2.02**
	[St. Error]	[0.91]	[1.01]	[2.18]	[0.83]	[0.50]
	Unit S.D. Effect	-6.1	9.2	-25.7	-4.6	9.8

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. 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	Secondary
A. Employment Growth, All Margins	Coefficient	0.96*	0.28	-0.05	1.82	0.82
	[St. Error]	[0.54]	[0.67]	[0.72]	[1.14]	[0.64]
	Unit S.D. Effect	3.2	1.0	-0.1	6.3	3.1
Organic Margins	Coefficient	0.34	-1.21***	-0.04	1.18	0.84
	[St. Error]	[0.40]	[0.34]	[0.53]	[0.84]	[0.52]
	Unit S.D. Effect	1.1	-4.2	-0.1	4.1	3.2
B. Excess Reallocation, All Margins	Coefficient	0.88***	-0.56	1.03***	1.67**	0.60
	[St. Error]	[0.28]	[0.40]	[0.35]	[0.74]	[0.46]
	Unit S.D. Effect	3.0	-1.9	2.8	5.8	2.3
C. Annual Compensation Per Employee	Coefficient	-0.24	0.54	-1.42***	-0.42	0.71
	[St. Error]	[0.41]	[1.26]	[0.53]	[0.52]	[0.72]
	Unit S.D. Effect	-0.8	2.0	-4.2	-1.5	2.4
D. Labor Productivity	Coefficient	0.98	-2.29*	4.86*	2.68*	-0.96
	[St. Error]	[1.17]	[1.23]	[2.65]	[1.55]	[1.13]
	Unit S.D. Effect	3.6	-10.4	16.4	10.0	-3.2

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.

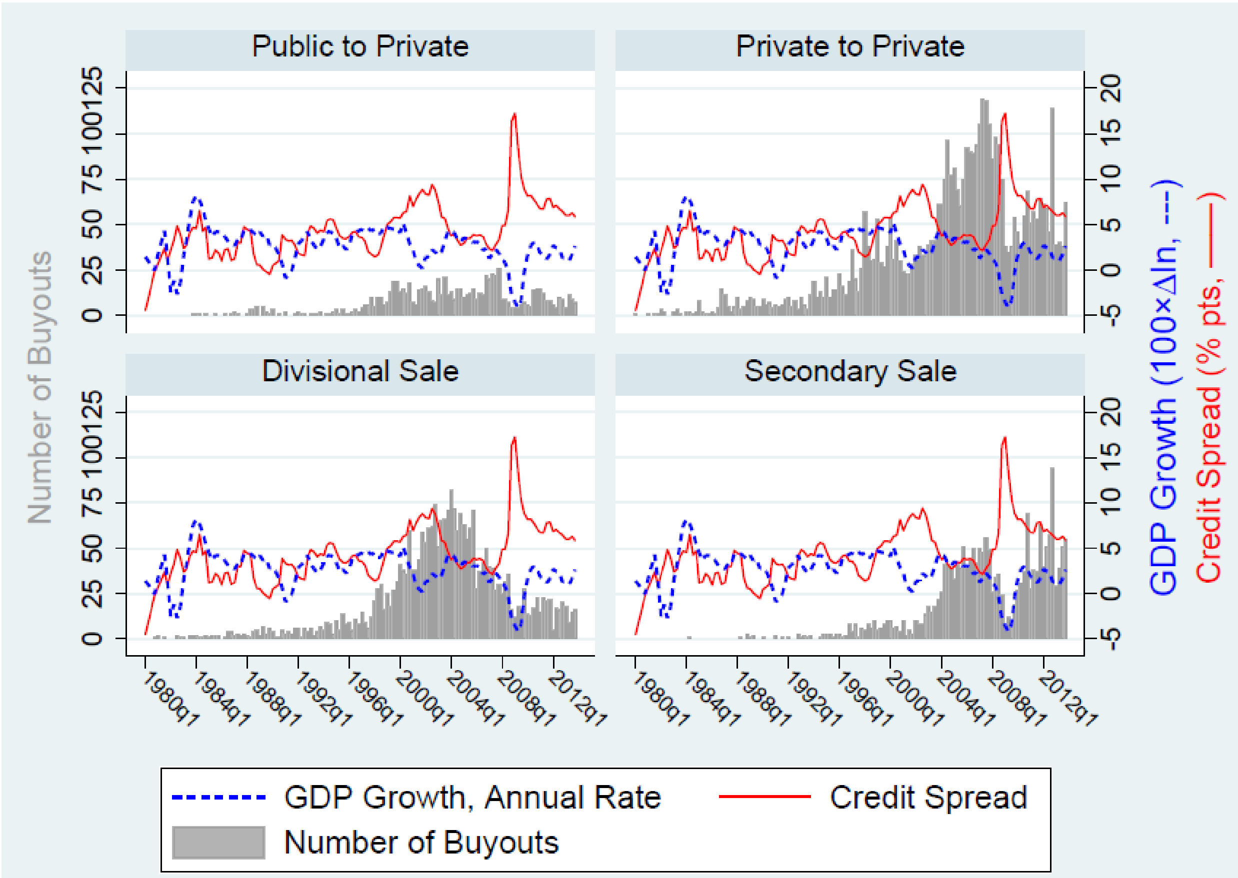
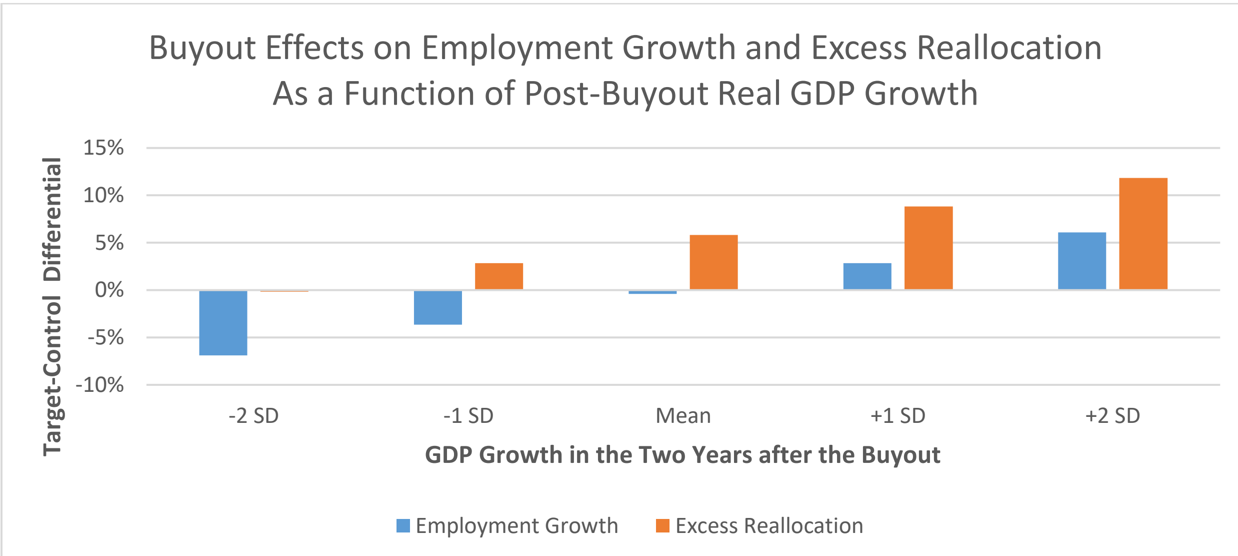
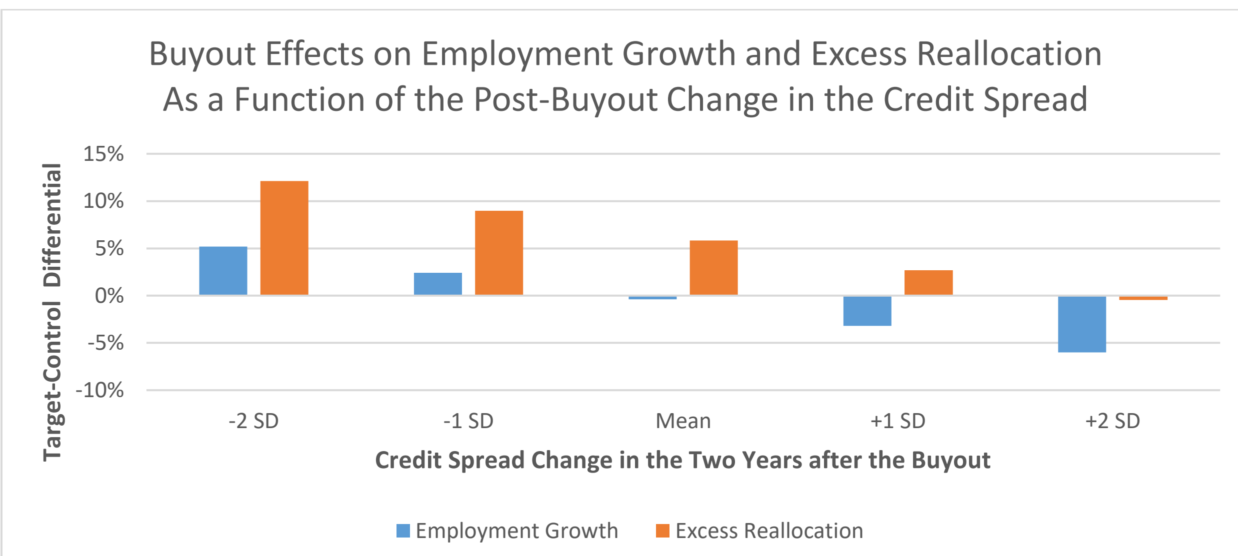


Figure 2. How Buyout Effects Vary with the Post-Buyout Evolution of Market Conditions
This figure uses the estimated interaction effects in Tables 5 and 6 to depict how the post-buyout employment growth rate and excess reallocation rate at targets (relative to controls) vary with the post-buyout evolution 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.





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. We start by matching 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.

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).[32] 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 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

[32] 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.

criteria and the algorithm used to identify matches in detail.[33] 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.[34] All but one of these candidates, and perhaps all of them, are false positives.

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.

[33] 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.

[34] 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.

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 Section I.E of 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 Sections III and IV 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.

Appendix B: Additional Results

Table B.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 B.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 B.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 B.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 B.4 consider various results for job reallocation (overall and excess), compensation per worker, and labor productivity.

Finally, Table B.5 reports estimated buyout effects on employment by adjustment margn and buyout type.

Table B.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 B.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 B.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 B.4. Buyout Effects by Adjustment Margin and Wages at Buyout Targets 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. Annual Compensation Per Employee

Change at Targets Relative to	Buyout Effect	St. Error
Controls from Buyout Year *t* to *t+2*	-1.72***	[0.62]
R^2	0.22	
Difference Relative to Control Continuers in Buyout Year t:	Coefficient Estimate	Standard Error
Target Continuer	2.45**	[1.23]
Control Death	-19.58***	[5.13]
Target Death	-4.03	[6.28]
Control Divestiture	3.99	[5.75]
Target Divestiture	12.74	[13.32]
R^2	0.44	
Difference Relative to Control Continuers in Year t+2:	Coefficient Estimate	Standard Error
Target Continuer	1.13	[1.14]
Control Birth	-6.17***	[1.74]
Target Birth	-7.27***	[2.05]
Control Acquisition	1.96	[4.04]
Target Acquisition	-4.07	[3.08]
R^2	0.47	
Number of Firm Observations (000)	3,900	

D. 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 B.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)	3,900		400		2,300		600	