**The Social Impact of Private Equity Over the Economic Cycle**

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**Abstract:** We study the impact of U.S. private equity buyouts on firm-level employment, job reallocation, earnings per worker, and labor productivity. Our sample covers thousands of buyouts from 1980 to 2013, which we link to Census micro data on the target firms, their establishments, and millions of comparable firms and establishments that serve as controls. Our results uncover striking differences in the real effects of buyouts, depending on the nature of the target firm, GDP growth, and credit market conditions. Employment at target firms shrinks by nearly 13% relative to controls over two years in buyouts of publicly listed firms but expands by 11% in buyouts of privately held firms. Slower GDP growth after the buyout brings lower employment growth at targets (relative to controls), as does a widening of credit spreads. Buyouts lead to productivity gains at target firms relative to controls – nine percentage points, on average, over two years post buyout. Tighter credit conditions at the time of the buyout are associated with much larger post-buyout productivity gains in target firms. A post-buyout widening of credit spreads or slowdown in GDP growth sharply curtails productivity gains in public-to-private deals.

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Excessive financialization in the advanced economies is an enduring concern. 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. One possibility, critics suggest, is that potential cost savings were diverted into rent-seeking activities or dissipated by other inefficiencies. Indeed, Zingales (2015) argues that the financial sector is unusually 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 consider one highly visible form of financialization: private equity (PE) groups that acquire controlling equity stakes in other firms. PE transactions, or buyouts, expanded greatly in number and volume in recent decades (Kaplan and Stromberg, 2009), directly touching a sizable share of U.S. employment (Davis et al., 2014). The pace of buyout activity also fluctuates sharply with economic and credit cycles. Partly motivated by these empirical patterns, we examine employment, job reallocation, wage, and productivity outcomes at firms acquired in PE buyouts, including how their outcomes vary with credit and economic conditions.[2] Our study also finds ample motivation in earlier research and in concerns about the leveraged nature of PE buyouts.

[2] Early studies on the real-side firm-level outcomes associated with private equity buyouts include Kaplan (1989) and Lichtenberg and Siegel (1990). More recent work considers much larger samples, often by exploiting government databases. See Bernstein et al. (2016), Bernstein and Sheen (2016), Boucly, Sraer, and Thesmar (2011), Cohn, Mills, and Towery (2014), Cohn, Nestoriak, and Wardlaw (2017), Davis et al. (2014), and Farcassi et al. (2018). Davis et al. (2014) also summarize several case studies.

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. (2003) look at a broader sample of transactions 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.[3] 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.

These empirical patterns suggest to some (e.g., Appelbaum and Batt, 2014) that private equity activity is too volatile, too sensitive to credit conditions, and too prone to leverage, with harmful consequences for the broader economy. In line with this view, a 2013 policy statement by U.S. banking regulators provides guidance on leveraged lending as follows: "A financial institution should have clear underwriting standards regarding leveraged transactions … as these risks may find their way into a wide variety of investment instruments and exacerbate systemic risks within the general economy."[4] Similarly, European Central Bank guidance on leveraged

[3] Similarly, Gompers and Lerner (2000) show that large inflows into venture capital funds lead to substantially higher valuations in venture investments.

[4] See Office of the Comptroller of the Currency, Board of Governors of the Federal Reserve System, and the Federal Deposit Insurance Corporation, "Interagency Guidance on Leveraged

lending states that "Underwriting of transactions presenting high levels of leverage – defined as the ratio of Total Debt to EBITDA exceeding 6.0 times at deal inception – should remain exceptional … and trigger a referral to the highest level of credit committee or similar decision-making level."[5] Likewise, Gregory (2013) argues that buyouts should be monitored for macro-prudential reasons, because "the increased indebtedness of such companies poses risk to the stability of the financial system."

Notwithstanding these concerns and policy initiatives, claims about excessive cyclicality and leverage in PE deals may be overstated, outdated, or misplaced. Large PE buyouts completed from 2005 to 2008 led to relatively few bankruptcies during or after the global financial crisis (Primack, 2015). Compared to other similarly leveraged firms, PE-backed firms were no more likely to default during the financial crisis, and they tended to resolve financial distress more efficiently (Hotchkiss, Stromberg, and Smith, 2014). According to practitioner accounts, the strong ties that PE firms have developed with the banking industry strengthened the capacity of their portfolio firms to weather financial strains. Close banking ties enabled PE-backed firms to borrow more cheaply, negotiate more favorable "covenant light" agreements,[6] and continue tapping credit during crises (Ivashina and Kovner, 2011). Bernstein, Lerner, and Mezzanotti (2018) show that a sample of approximately 400 British PE-backed firms cut investments less than peers during the

Lending," March 22, 2013 at www.federalreserve.gov/supervisionreg/srletters/sr1303a1.pdf, pages 6-7. EBITDA is Earnings Before Interest, Taxes, Depreciation and Amortization.

[5] See "Guidance on Leveraged Transactions," ECB Banking Supervision Division, May 2017, at https://www.bankingsupervision.europa.eu/ecb/pub/pdf/ssm.leveraged_transactions_guidance_201705.en.pdf. For a useful comparison of U.S. and European regulatory guidance regarding leveraged lending, see Shearman & Sterling LLP, "Leveraged Lending: Summary of ECB Guidance Compared to US Guidance," June 21, 2017, at www.shearman.com/~/media/Files/NewsInsights/Publications/2017/06/Leveraged-Lending-Summary-of-the-ECB-Guidance-compared-to-the-US-Guidance-FN-062117.pdf.

[6] Covenant light is a type of financing that places fewer restrictions on borrowers.

financial crisis and had greater equity and debt inflows. Moreover, buyout funds established before 2006 earned greater returns than publicly listed equities, while funds established after 2005 experienced returns similar to public equities (Harris, Jenkinson, and Kaplan, 2016).

As this summary suggests, previous research has considered how the financial characteristics of and returns to private equity investments vary over time. There has been little investigation, however, into the broader social effects of PE buyouts over the economic cycle. Do the cyclical patterns in transaction structures documented in previous research carry over to changes in the employment, productivity, and wage levels of portfolio firms? In particular, do deals undertaken when GDP growth is high, or credit conditions are easy, have fewer beneficial effects (or more harmful effects) on portfolio firms and their employees? Does a surge of highly leveraged buyout deals set the stage for a painful crunch if and when the economy contracts and credit conditions tighten? Answers to these questions should inform the regulation of PE buyouts and leveraged bank lending.

To address these questions, we examine non-financial outcomes for roughly 5,100 U.S. buyouts undertaken between 1980 and 2011. Using an improved version of the large-sample methodology in Davis et al. (2014), we explore the extent to which PE buyouts affect employment levels, the pace of job reallocation, wages, and productivity, all expressed relative to contemporaneous outcomes at comparable firms not backed by private equity. We focus on how outcomes unfold at PE buyout targets relative to control units over the first two years after the buyout. Our main findings are as follows:

- Employment at target firms shrinks by a statistically insignificant 1.4% (relative to controls), on average, over the first two years after the buyout. Setting aside post-buyout acquisitions and divestitures to focus on organic changes, target-firm employment shrinks by a statistically significant 4.4% (again, relative to controls).

- These "bottom line" results mask huge differences in employment outcomes by type of buyout. In public-to-private deals, target employment contracts nearly 13% relative to controls over two years. Divisional sales involve job losses of about 11%. Private-to-private and secondary deals show a strikingly different pattern: target-firm employment expands by 12% and 8% relative to controls in the first two years after the buyout.[7]
- The reallocation rate of jobs across companies and establishments is significantly higher post buyout. Much of this increase involves extra merger and acquisition activity.
- There is a small, statistically insignificant drop in average earnings per worker of -0.3% at target firms relative to controls. Wage losses are the largest in private-to-private buyouts, while wage changes are actually positive in divisional sales.
- Target firms are more productive than controls before buyouts, and the differential widens by 9 percentage points over the first two years post buyout. Productivity gains are concentrated in private-to-private and public-to-private buyouts.
- Buyouts are more common when GDP grows rapidly and credit spreads are narrow, and they tend to precede an expansion in spreads.
- Buyouts executed amidst wider credit spreads experience more intra-firm job reallocation and greater productivity gains.
- Expanding credit spreads and slow GDP growth post buyout bring slower employment growth for targets relative to controls. A one standard deviation unfavorable shift in these

[7] Secondary deals refer to the acquisition of a portfolio firm from another PE firm.

external conditions reduces the relative employment growth of targets by about three percentage points.

- For public-to-private deals, a post-buyout deterioration in external conditions leads to smaller productivity gains and reduced wage losses.

These findings point to several broader implications:

1. To consider the social impact of private equity in aggregate is highly misleading. There are large differences by deal type, over the economic cycle, and between existing and greenfield facilities. There are also large differences by industry sector (Davis et al., 2014).
2. Our evidence that tighter credit conditions bring greater productivity gains at targets (relative to controls) suggests a degree of substitution in the levers by which PE groups create value for their investors. In particular, when debt is more expensive (or less available), PE groups select buyouts that rely more on operating improvements to create value for their investors and less on financial engineering.
3. The relative impact of PE buyouts on employment at target firms is pro-cyclical. Downturns intensify the job losses associated with PE buyouts (relative to controls), particularly for private-to-private and secondary deals.
4. Our results reinforce concerns about public-to-private deals, which account for 10% of PE buyouts from 1980 to 2013 and 32% of employment in target firms. Public-to-private deals exhibit large post-buyout employment losses, a concentration of deals around market peaks, and poor productivity performance during downturns.

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

## I. Creating Our Samples of Private Equity Buyouts

### *A. Identifying Private Equity Buyouts*

Our study builds on the data work and analysis in Davis et al. (2014).[8] We 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 event typically involves a shift toward greater leverage in the capital structure of the target firm and, sometimes, a change in its management. As indicated by our quotations of U.S. and European regulators above, 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 Stromberg (2008). The first part drew on the CapitalIQ database to create a base sample of PE-sponsored leveraged buyout transactions. We selected all M&A transactions in CapitalIQ classified as a "leveraged buyout," "management buyout," or "JV/LBO" (joint venture/leverage buyout) and 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

[8] This effort originated as part of the World Economic Forum effort to assess the long-term effects of private equity, and also included a study of the demographics of private equity (published as Kaplan and Stromberg, 2009), growth buyouts in France (Boucly, Sraer, and Thesmar, 2011) the relationship between private equity and innovation (Lerner, Sorensen, and Stromberg, 2009), and management practices and private equity (Bloom, Sadun, and van Reenen, 2015).

reason, the second part of our sample construction efforts relied on other databases,[9] the business press, and transaction lists for the 1980s compiled by other researchers.

The largest source of discrepancies between our CapitalIQ sample and these lists are pure "management buyouts": LBO transactions not sponsored by a buyout fund or other financial institution but, instead, undertaken by management itself. Since these management buyouts are not the object of our study, we exclude them from our sample. Naturally, the overlap between our CapitalIQ-based sample and lists compiled by other researchers is greater for LBOs with a financial sponsor. For instance, 62 of the 77 transactions 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 transactions to our sample, as we did for other PE buyouts identified using various 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 that were announced but not yet completed by the end of 2013, acquisitions of non-control stakes (typically associated with growth and venture transactions, not classic buyouts), purchases of firms with foreign headquarters, stakes in public companies that remained publicly traded (PIPES), and other misclassified transactions. We

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

identified these transactions through the careful review of text fields in CapitalIQ records and our own detailed research using other commercial databases, securities filings, and media accounts. The resulting sample contains 9,794 PE-led leveraged buyouts of U.S. companies from January 1, 1980 to December 31, 2013.[10]

We sort the sample transactions 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 buyout of a portfolio firm from another PE firm (secondary). We derive our classifications from the textual descriptions of the transactions 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.[11] As noted in earlier 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 in 2008 as the financial crisis gathered force, credit conditions tightened, and the economy contracted. Interestingly, the flow of new

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[10] 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).

[11] 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 same point applies to Tables 1 and 2 below.

public-to-private buyouts dropped off well before the onset of the financial crisis, and remained at modest levels through the end of our sample. Counts for private-to-private 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. We regress the natural log of the quarterly buyout count on deal-type indicators interacted with market conditions at the buyout close (top panel) and 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.[12] Both regressions include controls for deal type and a linear time trend.

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 (51%) log points higher for divisional sales in periods with above-average GDP growth, conditional on the credit-spread interaction variables and the controls. Buyout counts are 18-26 log points lower when credit spreads are wider than average, conditional on the other regressors. 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

[12] See Section IV for the precise definition of these measures.

spreads. This result says that target firms are more likely than not to face a tightening of credit conditions post buyout, an issue that we explore below.

Table 2 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 in Utilities. A Pearson chi-squared test rejects the null 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, as shown in Davis et al. (2014). Given the patterns in Tables 1 and 2 and our earlier work, our econometric investigations below compare buyout targets to control units defined by buyout period, industry, firm size, and firm age.

*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.[13] 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), employer identification numbers, business names, and location information.

[13] 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 data on buyouts 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. The Online Appendix 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 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 the Online Appendix. Rather than include matches of dubious quality, we exclude them from our analysis.

Once matched to the BR, we can identify all 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 can report income under EINs that may fall outside the set of EINs that Census considers part of that firm for employment purposes**.** Haltiwanger et al. (2017) provide additional information about the revenue data.

### *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 transactions 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.[14]

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 sample. All told, we are left with about 6,000 matched target firms acquired in PE buyouts from 1980 to 2013. These firms 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.

### *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

[14] 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 transaction dates are distributed fairly evenly over the calendar year.

more challenging for two main reasons: the disappearance of firm identifiers (IDs), and irregularities in Census Bureau tracking of PE targets involved in certain divisional sales. We elaborate on these two reasons in turn.

*Firm ID Disappearance*. The disappearance of a firm ID in the LBD can occur for various reasons. One is the death of a firm and the closure of all of its establishments. Firm death in this sense presents no problem: we capture such events whether they involve target or control firms. A more difficult situation involves a target firm ID in the buyout year that disappears in later years, even though some of the establishments owned by the firm (as of the buyout year) continue to operate. This situation can arise when the various components of the original firm are acquired by multiple existing firms. It is inherently difficult to define and measure firm changes when the original legal entity ceases to exist, and we exclude these cases from our firm-level longitudinal analyses.[15] 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 Transactions*. 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

[15] Even establishments are challenging to track in some circumstances. Every five years, the Census Bureau obtains a full list of establishments owned by multi-unit firms from the Economic Censuses. It also 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). However, the COS samples smaller multi-unit firms in a targeted manner based on information that they underwent rapid growth or organizational change. Thus, Census may not promptly recognize the ownership of 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.

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.

Table 3 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. But the 22% share of secondary sales is nearly twice as large as in our earlier work, reflecting a 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.

Panel C compares matched buyouts in our new sample to those in Davis et al. (2014) for the overlapping period from 1980 to 2003. 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 only 10% fewer matched buyouts. The mix of buyout types in our new 1980-2003 sample is similar to that in our earlier work, but our new sample has considerably fewer establishments and less employment.

## 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 post 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.[16] 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 tmo growth rates and aggregation also works for gross job creation and destruction, job reallocation, and employment changes on particular margins such as acquisitions and divestitures or continuing establishments.

[16] 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 Tornqvist, Vartia, and Vartia (1985).

A third aspect relates to the selection of control units for comparison to buyout targets in our regression models. We need suitable control units because the distribution of private equity buyouts across industries and business characteristics is not random. Target firms are larger and older than the average firm and disproportionately concentrated in manufacturing, information technology, accommodations, and food services (Davis et al., 2014). They also differ by deal type, as shown above. Moreover, growth and volatility vary greatly by firm size and age, and the workplace and production process differ greatly by industry.[17] In view of these facts, 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 (3-digit NAICS), 10 firm size categories, 6 firm age categories, a dummy for firms with multiple establishments, and 32 distinct buyout years from 1980 to 2011.[18] 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

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[17] 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).

[18] We define industry for multi-unit firms based on the modal industry of their establishments, computed on an employment-weighted basis. Our firm size categories are 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500-4999, 5000-9999, and 10000 or more employees. Our firm age categories are 0-5 years, 6-10, 11-15, 16-20, and 21 or more years. Following Davis et al. (2014), when a firm first appears in the LBD, we assign it the age of its oldest establishment. We then increment the firm's age by one year for each year it continues as a legal entity in the LBD. In this way, we avoid arbitrary increases or decreases in firm age due to the sale and purchase of establishments.

contemporaneous changes at their matched control units.[19] This approach, in combination with our rich set of controls, facilitates an apples-to-apples comparison when estimating the effects of buyouts.

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.[20]

To achieve our estimation goal, we adopt two principles in weighting the observations:[21]

- **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.

[19] 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.
[20] See Chapter 18 in Wooldridge (2002) for an extended discussion of CMI and SUTVA in panel regression studies of treatment effects.
[21] Neither equal weighting nor simple activity weighting of regression observations recovers the average treatment effect of interest.

To be precise, suppose we have two target firms in two separate control cells, and we are interested in target-control comparisons from $t$ to $t+k$. The targets have activity levels $X_{1,t+k,t} = 0.5(E_{1,t+k} + E_{1t})$ and $X_{2,t+k,t} = 0.5(E_{2,t+k} + E_{2t})$. The first target's share of aggregate target activity is $\omega_{1,t+k,t} \equiv X_{1,t+k,t}/(X_{1,t+k,t} + X_{2,t+k,t})$, and the second's share is $\omega_{2,t+k,t} \equiv X_{2,t+k,t}/(X_{1,t+k,t} + X_{2,t+k,t})$. Since each control cell has a single target, these are also the control cell weights.[22] 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, so 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 then 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.[23] These alternative approaches to weighting control units led to results similar to the ones reported below.

[22] 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 in the main text.
[23] Three concerns motivated our experimentation with alternative schemes that give less weight to larger control units, while still adhering to principle SCT. First, very large employment values for certain control units could reflect measurement error. This concern might apply to targets as well, but since our sample has only a few thousand targets, we scrutinize them carefully. We believe we have identified (and corrected) gross errors in target outcomes. A similarly careful approach for controls is infeasible, since there are so many of them. Second, it is often hard to fit very large

Recall that we aim to recover the average treatment effect on the treated (buyout) firms under CMI and SUTVA. A standard approach 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, e.g., Chapter 18 in Wooldridge (2002).) That is the approach we took in Davis et al. (2014). 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. Analysis of Social Impact

### A. *The Regression Specification and Additional Remarks about 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:

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

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

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.

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 $\gamma$ 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 throw light on the economic role of private equity, and 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 systematically alter market equilibrium outcomes for controls through demand and supply channels or by competitive pressures that stimulate productivity gains at controls. Since buyout targets account for modest activity levels compared to controls, standard market equilibrium effects are unlikely to be important in our setting, especially within our two-year post-buyout time frame.

B. *Average Treatment Effects Over All Buyouts*

Table 4 presents our first set of regression results. The sample contains firms that underwent buyouts from 1980 to 2011 and matched control firms in the same cells. The top row in Panel A says that employment at target firms shrinks by a statistically insignificant 1.4 percentage points relative to control units in the two years after the buyout. The second row says that target-firm employment shrinks by a statistically significant 4.4 points relative to controls when omitting post-buyout acquisitions and divestitures. These "bottom line" effects of PE buyouts on target firm employment are moderately larger than we found in Davis et al. (2014): -0.9 percentage points overall, and -3.7 points for organic growth.

The other rows in Panel A break down the overall employment change into several margins of adjustment. "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, 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 3.7% more jobs through acquisitions.

Because the regressions are employment weighted, we can sum the coefficients over the margins. 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) for targets. 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.

Panel A also provides evidence that buyouts raise job reallocation. Compared to controls, target firms exhibit greater job destruction through establishment shutdowns, more job creation through establishment births, more employment losses through divestitures, and greater employment gains through acquisitions. In short, targets undergo a faster pace of job reallocation after buyouts than controls. We delve more deeply into the reallocation effects of buyouts shortly.

How buyouts affect wages has long been controversial. Critics argue that buyouts lead to lower wages, as formalized by Shleifer and Summers (1988). Indeed, Lichtenberg and Siegel (1990) find that buyouts lead to lower compensation for white-collar workers. More recently, Agrawal and Tambe (2016) suggest that buyouts can enhance human capital in target firms, particularly by developing employee knowledge of information technology. Survey evidence in Gompers, Kaplan, and Mukharlyamov (2016) is consistent with this view.

Panel B in Table 4 provides new evidence on the wage effects of PE buyouts using a much larger sample than previous studies. Our wage measure is the firm's gross annual compensation per employee.[24] We consider the same sample as before, except for dropping firms that close all

[24] 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 obligation until exercise

establishments by t+2, because we cannot calculate wage changes for firms that die. (There are very few such firms among targets.) The first row in Panel B reports a modest, statistically insignificant wage drop of -0.3% at target firms relative to controls over two years post buyout. The next two rows in Panel B show that target firms pay a wage premium of about 3% in the buyout year and two years later. Thus, we find no evidence that PE buyouts have systematic effects on wages – at least when aggregating over deal types and time periods.

Panel C reports results for firm-level revenue productivity, measured as the log of Real Revenue per Worker using the industry-level price deflators described in Haltiwanger et al. (2017). As noted above, the revenue productivity data are available for about 80 percent of the firm-level observations from 1996 onwards. To address the potential selection bias introduced by missing productivity observations, we construct inverse propensity score weights for the observations as in Haltiwanger et al. (2017). 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 in the regression analysis of productivity growth in addition to the activity weights described in Section II.

The second row of Panel C says that target firms are 35 log points more productive than control firms as of the buyout year, a very large gap. The gap widens by 9 log points over the next two years after the buyout, according to the productivity change regression reported in the top row of Panel C, and by 6 log points when comparing the productivity level regression in t+2 to the one in t.[25] Our earlier work in Davis et al (2014) finds that PE buyouts lead to smaller TFP gains at

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or sale. Buyouts often tilt the compensation of senior management toward stock options (Leslie and Oyer, 2008), which means we may slightly understate the true wage change at target firms.

[25] Our propensity score weights that adjust for the missing productivity observations differ across the three regressions in Panel C. That is why the level and change regressions yield somewhat different estimates for the effect of buyouts on productivity.

target firms relative to controls in the manufacturing sector. Here, we find a larger effect of PE buyouts on labor productivity when looking across all industry sectors.[26]

Table 5 reports the estimated impact of PE buyouts on two measures of reallocation activity. The *overall* job reallocation rate 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. A firm's *excess* reallocation rate is the difference between its job reallocation rate and the absolute value of its net growth rate.[27] If a firm changes employment in the same direction at all of its establishments, then 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 the firm adds jobs at some of its establishments and cuts an equal number of jobs at other establishments, then excess reallocation equals overall job reallocation.

According to Table 5, the overall job reallocation rate is 7.1 percentage points higher at targets for organic employment changes over the two years after the buyout and 11.5 points higher when including acquisitions and divestitures, both highly significant. These results confirm our previous inference that PE buyouts accelerate the pace of reallocation at target firms, more so when including acquisitions and divestitures. The excess reallocation rate is 5.0 percentage points higher at target firms for all changes, but (insignificantly) lower for organic changes. The implication is that the faster pace of job reallocation induced by buyouts mainly involves greater reallocation of

[26] Foster, Haltiwanger, and Krizan (2006) show that gross output per worker and TFP are highly correlated within industries, presumably because materials and capital shares are similar across firms within industries. Because our control variables include industry-by-year effects, we effectively perform within-industry comparisons in our productivity growth regressions.

[27] This concept of excess reallocation is often used in the literature on gross job flows to analyze the nature of job reallocation within and between industries or sectors. Examples include Dunne, Roberts, and Samuelson (1989) and Davis and Haltiwanger (1992, 1999). Our approach here applies the concept to the reallocation of jobs across units within firms.

across firms rather than within target firms. That is, PE buyouts lead to net job gains at some target firms (relative to control units) 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 initial employment over the first two years after the buyout.

C. *Treatment Effects by Buyout Type*

There are sound reasons to expect the social impacts of PE buyouts to vary by deal type. Public-to-private deals involve target firms with highly dispersed ownership. These firms may suffer from poor corporate governance and face a strong need for cost cutting. Buyouts of privately held firms may be more often motivated by a desire to professionalize management or gain better access to financing. 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 outsources that unpleasant task to new PE owners in an effort to shield its public image and employee morale in the rest of the firm. Some secondary sales reflect an incomplete effort by the initial PE owner to improve the operations and profitability of the target firm, often truncated by the desire to have a successful exit prior to raising a new fund.

In light of these observations, Table 6 reports regression results by deal type. The outcome variables and specifications parallel the ones in Tables 4 and 5. As seen in the top row of Panel A, the employment effects of PE buyouts differ dramatically by deal type. Target employment contracts nearly 13 percent relative to controls over two years post buyout in public-to-private deals. This result, along with the high visibility and large employment share (31% of target employment from 1980 to 2013) of public-to private deals, helps explain concerns about job losses in PE buyouts. Divisional sales also involve large job losses relative to controls – about 11 percent over two years. The similarities between public-to-private and divisional deals are perhaps unsurprising, given that both typically involve sellers who are publicly traded entities. In sharp contrast, target employment jumps by 13% relative to controls in private-to-private deals (26% of target employment) and by 10% in secondary deals (19%). Buyout effects also differ sharply for organic changes: -10% and -16% for public-to-private and divisional deals versus +4% and +6% for private-to-private and secondary deals.

Turning to Panel B, buyouts bring large upticks in overall job reallocation for all deal types, with magnitudes ranging from 9% of buyout-year employment in secondary sales to 19% in divisional sales. However, the character of the extra buyout-induced reallocation differs among deal types. Job losses in public-to-private and divisional deals largely reflect establishment closures and, for divisional deals, job cuts in continuing establishments. Buyout-induced job gains in private-to-private and secondary deals reflect the important roles of acquisitions and establishment births and, for secondary sales, a boost in job creation at continuers. For public-to-private deals, essentially all of the extra job reallocation reflects a downsizing of some target firms (relative to controls) and an upsizing of others. In other words, targets show virtually no uptick in excess reallocation in public-to-private deals. In contrast, an uptick in excess reallocation at target firms accounts for one-half to two-thirds of the extra buyout-induced job reallocation in the other deal types. For divisional sales, most of the extra excess job reallocation occurs on organic margins.

Panel C focuses on wage differences and effects associated with PE buyouts. At the time of buyouts, employees in public-to-private and divisional targets receive sizable wage premia relative to their counterparts in control firms, while employees in secondary targets receive a discount. More noteworthy for our purposes, earnings per worker rise 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]

Panel D considers productivity changes. Again, we find large differences in buyout effects by deal type. Target firms in private-to-private deals experience a gain in revenue per worker of 14 log points over two years post buyout relative to control counterparts. Targets in public-to-private deals enjoy even larger gains, but the imprecise estimate precludes a strong inference.

In summary, Table 6 says the social impacts of PE buyouts vary greatly by deal type, as anticipated. The pattern of results is broadly consistent with the limited body of evidence compiled in previous research on the real-side effects of PE buyouts. (The literature on private equity is voluminous but mainly speaks to financial characteristics and outcomes.)

[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 the value of the 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.

- *Private-to-private* deals exhibit high post-buyout employment growth (largely but not entirely due to acquisitions), wage reductions, and large productivity gains. These results align with the view that private equity eases financing constraints at target firms, enabling their expansion (Boucly, Sraer and Thesmar, 2011). The large productivity gains align with evidence in Bloom, Sadun and van Reenen (2015) that PE buyouts bring better management practices. Their sample contains buyouts of middle-market firms for which private-to-private deals are likely to predominate.
- *Public-to-private* deals exhibit large job losses, often through facility closures, and large (imprecisely estimated) productivity gains. The large job losses in these deals (and in divisional sales) may partly reflect the workforce recontracting hypothesis of Shleifer and Summers (1988). They may also partly reflect a concentration of these deals in advance of credit-market tightening, a topic we consider in the next section.
- *Divisional* deals also involve large job losses, through both facility closures and cutbacks at continuers, but large gains in compensation per worker.
- *Secondary* deals exhibit high target employment growth, largely organic, and few discernable effects otherwise. This pattern resonates with Degeorge, Martin, and Phalippou (2016), who find positive financial performance in secondary deals. It is reasonable to hypothesize in many cases, that the previous PE owner undertook considerable restructuring, setting the stage for rapid employment growth after the secondary buyout.

## IV. How the Impact of Buyouts Varies with Market Conditions

We now investigate how the social impact of PE buyouts varies with market conditions. To do so, we estimate expanded regression specifications of the form,

$$Y_{i,t+2} = \alpha + \sum_c D_{cit}\theta_c + \lambda_1 LEST_{it} + \lambda_2 LFIRM_{it} + \gamma PE_{it} + \beta\ PE_{it} * MktCondition_t + \varepsilon_{it},$$

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.

Table 7 considers two measures of market conditions when the buyout closed: the log change in real GDP over the four-quarter interval ending in the quarter of the buyout closing (using U.S. Bureau of Economic Analysis data) and the spread in the buyout month between the yield to maturity in the Bank of America Merrill Lynch U.S. High Yield Index for corporate bonds and the one-month LIBOR. Tables 8 and 9 instead consider how market conditions evolve after the close. We measure post-buyout changes in market conditions from March (or the first quarter) of year t to March (first quarter) of year t+2.[29] In short, Table 7 tells us how targets fare post buyout (relative to control firms) as a function of market conditions near the deal close, while Tables 8 and 9 tell us how targets fare as a function of the evolution in market conditions after the buyout.

Turning first to the Table 7 results, 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 buyout close. The $\beta$ coefficients on the interaction term are imprecisely estimated and statistically insignificant for each dependent variable in columns (1) to (5). In contrast, higher credit spreads at the close are associated with large, statistically significant post-buyout increases in excess reallocation and productivity growth at target firms. These increases come on top of the baseline effects seen in the top row. The last row in Table 7 reports the product of the $\beta$ coefficient

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

and a unit standard deviation change in market conditions. Raising the credit spread by one standard deviation corresponds to a post-buyout gain of 21.7 log points for targets relative to controls and an increase in excess reallocation of 4.6 percent of buyout-year employment.

Thus, the credit spread effect on target-firm productivity growth is quite large and is accompanied by a sizable increase in the pace of job reallocation inside target firms. This pattern suggests that PE buyouts foster productivity gains by catalyzing creative destruction within target firms. In unreported results, we directly examine the post-buyout relationship between productivity growth and excess job reallocation and find that targets with higher excess job reallocation enjoy higher productivity growth. These results echo one of the chief findings in Davis et al. (2014) despite our use of a different productivity measure, different empirical methods, and data for a much broader set of industries. Our earlier study finds 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 growth 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.

Figure 2 illustrates how post-buyout productivity growth and excess reallocation at target firms vary with credit spreads at the time of buyout. Evaluated at the sample mean credit spread, Table 7 says that buyouts raise productivity by about 15 log points over two years at targets relative to controls. The buyout-related productivity boost is more than twice as large when the credit spread is one standard deviation about its sample mean. Post-buyout excess reallocation also rises with the credit spread at target firms relative to controls, as discussed above.

One interpretation of these patterns is that PE groups have multiple tools for earning investment returns on their portfolio firms. When credit is cheap and easy, it is 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 attractive and PE groups focus more on generating returns by cultivating 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 firms and senior managers in portfolio firms allocate their time and attention, or through a combination of the two.

Turning to Table 8, faster GDP growth in the two-year interval after buyouts is associated with faster post-buyout employment growth at targets relative to controls and greater excess reallocation. The effects are large, as seen in the last row: A unit standard deviation rise in the post-buyout GDP growth rate comes with a gain in relative employment growth at targets of 3.2 log points and a relative increase of 3.0 percent in excess employment reallocation. A rise in credit spreads after buyouts involves slower employment growth at targets relative to controls, slower organic growth, slower excess reallocation, higher wage growth, and lower productivity growth. These shifts are statistically significant on every margin. While the credit spread results in Table 8 are stronger than the results for GDP growth, the outcome response pattern is the same. In unreported results, we also find a broadly similar pattern when using equity market valuations to measure external conditions.

Figure 3 illustrates how post-buyout employment growth and excess reallocation at target firms vary relative to controls with the post-buyout evolution of external market conditions. The baseline employment growth effects depicted in the center bars are of modest size, in line with our results in Section III. However, the relative post-buyout employment performance of targets is highly sensitive to the evolution of market conditions. For example, a post-buyout widening of credit spreads by two standard deviations lowers the relative employment growth of targets by 5 log points. Excess reallocation rates at target firms are also highly sensitive to the post-buyout evolution of market conditions. While not illustrated in the figure, the results in Table 8 also imply that post-buyout productivity growth at targets rises strongly with an improvement in economic conditions (faster GDP growth or shrinking credit spreads).

Post-buyout wage growth at targets is also sensitive to the evolution of credit conditions. According to Column (9) in Table 8, a unit standard deviation widening of the credit spread (440 basis points) in the two years after the buyout is associated with a relative wage gain at targets of 1.4 log points. Whether this result reflects a compositional shift in the workforce (e.g., layoffs concentrated among low-wage workers) or wage gains for employees at target firms relative to those at control firms is an open question.

As the reader will have noted, high credit spreads when the buyout closes and widening credit spreads after the buyout closes have very different relationships to the post-buyout performance of targets. As we saw in Table 7, deals done during periods of high credit spreads prove to have more productivity gains. But if credit spreads further increase after the buyout, the effect goes the other way, as revealed in Table 8. This contrast might seem anomalous. As noted above, PE groups may react to tight credit conditions by choosing transactions that are conducive to operational improvements. If credit conditions deteriorate post-buyout, however, they appear unable to "switch gears" to improving productivity. Rather, the deteriorating conditions seem to translate into fewer productivity gains (and more job losses). Given the pervasiveness of road-maps for future operational plans prepared by PE groups as part of the due diligence process (e.g., Gompers, Kaplan, and Mukharlyamov, 2016), one possibility is that they get locked into a particular strategy, hampering their ability to promptly shift course later if market conditions change after the purchase. As we saw earlier, a post-buyout widening of credit spreads also brings slower pace of excess reallocation at targets relative to controls.

Given these intriguing results, Table 9 considers the role of post-buyout market conditions by deal type. As we saw in Table 6, the employment response to buyouts differs dramatically across deal types. Moreover, Figure 1 and Table 1 show that the mix of buyouts by deal type varies over the economic cycle. These remarks suggest that the sensitivity of targets to the post-buyout evolution of external market conditions may also differ by deal type.

As seen in Panel B of Table 9, 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 log points. The drop involves organic employment changes in secondary deals. Lower post-buyout GDP growth is also associated with lower employment growth (except for public-to-private deals), but the effects are not statistically significant when cutting the sample by deal type.

The post-buyout evolution of market conditions shows a stronger relationship to excess reallocation rates in target firms. In five out of eight reported regressions, we see that a deterioration in external market conditions 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 wages and productivity results in Table 9 highlight the special character of public-to-private deals. In particular, in these deals a post-buyout deterioration in market conditions brings greater wage growth and slower productivity growth at target firms compared to controls. These effects are statistically significant and quite large, as seen in the last row of each panel. Given the size and heavy debt loads of target firms in public-to-private deals (Axelson et al., 2003), downturns place may place great stress on their restructuring plans. The combination of adverse market conditions and heavy indebtedness may hamper their efforts to undertake productivity-improving changes. Normally, we might anticipate that financial pressures on the firm would also translate into wage and benefit concessions by workers (e.g., Matsa, 2010, and Benmelech, Bergman, and Enriquez, 2012). But in this setting the dynamics may be more complex. For instance, a firm may be unable to sell units with high labor costs when external conditions are weak, and it may lack the resources to finance a shift in operations to new facilities with lower labor costs when debt burdens are too heavy.

## 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 financial enterprise that critics see as dominated by rent-seeking activities with little in the way of societal benefits.

Our results show that it is highly misleading to speak about "the" social impact of private equity. The real-side effects of buyouts vary greatly by deal type and with market conditions. The effects of public-to-private buyouts are especially sensitive to market conditions. Tighter credit market conditions when buyouts take place are associated with greater post-buyout productivity gains at target firms (relative to control firms). This result suggests a degree of substitution in the levers by which private equity groups create value for their investors. Furthermore, our evidence that the relative impact of private equity on employment is pro-cyclical, particularly for private-to-private and secondary investments, suggests a “PE multiplier effect” that may accentuate cyclical swings in economic activity.

Our paper also points to some important unanswered questions. Foremost among these are whether and how the social impact of buyouts varies among private equity groups themselves. In particular, do buyout effects vary with the experience and size of the private equity group? Kaplan and Schoar (2005) find that the financial performance varies across private equity firms in a manner that persists from fund to fund. This pattern suggests that real-side effects are also likely to differ across private equity firms in a persistent manner.

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**Table 1: Market Conditions and Private Equity Buyout Frequency by Deal Type, 1980-2013**

We regress the natural log of the count of PE buyouts 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. 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 above or below their median values. Each regression has 454 observations. *** p<0.01, ** p<0.05, * p<0.1.

**Regression of ln(buyout count) on deal type interacted with measures of market conditions, 1980-2013**

| | 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 | 0.282*** | 0.660*** | 0.412*** | 0.0174 | | |
| | [0.0946] | [0.161] | [0.156] | [0.144] | 0.74 | 0.000 |
| Wide Credit Spread | -0.207** | -0.266* | -0.181 | -0.249* | | |
| | [0.0993] | [0.147] | [0.149] | [0.150] | | 0.019 |
| *B. Over Next 2 Years* | | | | | | |
| High GDP Growth | 0.119 | 0.449*** | 0.523*** | -0.407*** | | |
| | [0.112] | [0.147] | [0.163] | [0.153] | | 0.000 |
| Widening Credit Spread | 0.212* | 0.678*** | 0.325** | 0.200 | 0.75 | |
| | [0.112] | [0.142] | [0.148] | [0.139] | | 0.000 |

**Table 2: 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).

| | | 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% |

NB: 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 3: Summary Statistics for Private Equity Buyouts Matched to Census Micro Data**
Panel A considers all matched targets in our 1980-2013 sample period. Panel B considers buyouts in the 1980-2011 period, for which we can follow targets and their control units for two years post buyout. Panel C considers the same period (1980-2003) covered by the analysis sample in Davis et al. (2014). "Two-year continuers" include target firms that shut down all establishments by the second year after the buyout year.

| | 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 |
| Excluding EIN cases | 4,500 | 144,000 | 5,690,000 |
| Two-year continuers, | 3,600 | 127,000 | 4,970,000 |
| Excluding EIN cases: | | | |
| 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 |
| Excluding EIN cases | 1,500 | 59,000 | 2,630,000 |
| Two-year continuers, | 1,200 | 49,500 | 2,210,000 |
| Excluding EIN cases: | | | |
| 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 4: Buoyout Effects on Employment, Wages, and Productivity**

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each row reports a single weighted least-squares regression. The top row in Panel A reports the estimated buyout effect on the employment growth rate of target firms from the buyout year to two years later. The next row isolates the buyout effect on the organic employment growth rate (i.e., excluding post-buyout acquisitions and divestitures), and the remaining rows break out particular employment adjustment margins. Panels B and C report buyout effects on firm-level wage and revenue productivity growth. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories, as described in the main text. See Section II in the main text for an explanation of how and why we weight observations. The regressions in panels B and C exclude firms that exit within two years after the buyout. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Panel A: Employment

| Dependent Variable: | Buyout Effect | R^2 |
|---|---|---|
| Firm-level Growth Rate from Buyout Year t to t+2 | -0.0135 | 0.32 |
| | [0.0217] | |
| Firm-Level Growth Rate from t to t+2, Organic Changes Only | -0.0438** | 0.29 |
| | [0.0190] | |
| By Adjustment Margin | | |
| Continuers | -0.0153 | 0.28 |
| | [0.0115] | |
| *Creation* | *0.0020* | *0.34* |
| | *[0.0041]* | |
| *Destruction* | *-0.0173** | *0.27* |
| | *[0.0096]* | |
| Deaths | -0.0403*** | 0.30 |
| | [0.0124] | |
| Births | 0.0117** | 0.34 |
| | [0.0051] | |
| Acquisitions | 0.0369*** | 0.38 |
| | [0.0097] | |
| Divestitures | -0.0065 | 0.26 |
| | [0.0041] | |

Panel B: Wages

| Dependent Variable | Buyout Effect | $R^2$ |
|---|---|---|
| Change in Wages from t to t+2 | -0.0028 | 0.41 |
| | [0.0168] | |
| Wage Level in t | 0.0321** | 0.74 |
| | [0.0141] | |
| Wage level in t+2 | 0.0293 | 0.69 |
| | [0.0200] | |

Panel C: Productivity

| Dependent Variable | Buyout Effects | $R^2$ |
|---|---|---|
| Change in Rev Prod from t to t+2 | 0.0937* | 0.46 |
| | [0.0497] | |
| Rev Prod in t | 0.3544*** | 0.64 |
| | [0.1034] | |
| Rev Prod in t+2 | 0.4158*** | 0.60 |
| | [0.0865] | |

**Table 5: Buyout Effects on Firm-Level Job Reallocation and Excess Reallocation**

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each row reports a single weighted least-squares regression. The dependent variable is the excess reallocation rate or job reallocation rate computed from establishment-level employment changes between the buyout year t and t+2. The key independent variable is a dummy equal to one for buyout targets. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories, as described in the text. See Section II in the text for an explanation of how and why we weight observations. "All Margins" captures the "Organic Margins" plus post-buyout acquisitions and divestitures. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Estimated Effect on Targets Relative to Controls from Buyout Year t to t+2

| Dependent Variable | Buyout Effect | R^2 |
|---|---|---|
| Firm-Level Excess Reallocation Rate, All Margins | 0.0495*** [0.0114] | 0.40 |
| Firm-Level Excess Reallocation Rate, Organic Margins (Births, Deaths & Continuers) | 0.0061 [0.0154] | 0.35 |
| Firm-Level Job Reallocation Rate, All Margins | 0.1147*** [0.0182] | 0.39 |
| Firm-Level Job Reallocation Rate, Organic Margins (Births, Deaths & Continuers) | 0.0713*** [0.0176] | 0.39 |

**Table 6. How Buyout Effects Vary by Deal Type**

This table follows Tables 4 and 5 except for reporting results by deal type, as indicated in the column headings. See notes to Tables 4 and 5 for additional information. *** p<0.01, ** p<0.05, * p<0.1.

Panel A: Employment

| Dependent Variable: | Private-to-private | | Public-to-private | | Divisional Sales | | Secondary Sales | |
|---|---|---|---|---|---|---|---|---|
| | Buyout Effect | R^2 | Buyout Effect | R^2 | Buyout Effect | R^2 | Buyout Effect | R^2 |
| Firm-level Employment Growth Rate from Buyout Year t to t+2 | 0.1275*** | 0.37 | -0.1263*** | 0.38 | -0.1145** | 0.32 | 0.0989*** | 0.32 |
| | [0.0250] | | [0.0285] | | [0.0470] | | [0.0254] | |
| Organic Growth | 0.0309** | 0.33 | -0.0997*** | 0.38 | -0.1599*** | 0.29 | 0.0609*** | 0.30 |
| | [0.0152] | | [0.0242] | | [0.0424] | | [0.0227] | |
| By Adjustment Margin | | | | | | | | |
| Continuers | 0.0055 | 0.30 | -0.0159 | 0.33 | -0.0764*** | 0.29 | 0.0263** | 0.36 |
| | [0.0104] | | [0.0120] | | [0.0274] | | [0.0128] | |
| Creation | 0.0027 | 0.36 | 0.0023 | 0.29 | -0.0086 | 0.28 | 0.0210* | 0.43 |
| | [0.0057] | | [0.0056] | | [0.0096] | | [0.0108] | |
| Destruction | 0.0028 | 0.32 | -0.0182* | 0.32 | -0.0678*** | 0.33 | 0.0053 | 0.29 |
| | [0.0077] | | [0.0099] | | [0.0245] | | [0.0102] | |
| Deaths | 0.0003 | 0.34 | -0.0626*** | 0.44 | -0.0976*** | 0.28 | -0.007 | 0.29 |
| | [0.0104] | | [0.0205] | | [0.0200] | | [0.0158] | |
| Births | 0.0251*** | 0.40 | -0.0213*** | 0.33 | 0.0142 | 0.37 | 0.0416*** | 0.42 |
| | [0.0077] | | [0.0071] | | [0.0120] | | [0.0122] | |
| Acquisitions | 0.0953*** | 0.44 | 0.004 | 0.42 | 0.0332** | 0.38 | 0.0329*** | 0.39 |
| | [0.0259] | | [0.0057] | | [0.0154] | | [0.0096] | |
| Divestitures | 0.0027 | 0.20 | -0.0301*** | 0.35 | 0.0102** | 0.23 | 0.0036 | 0.22 |
| | [0.0053] | | [0.0104] | | [0.0049] | | [0.0061] | |
| Observations (000s) | 3,900 | | 400 | | 2,300 | | 600 | |

Panel B: Reallocation

| Dependent Variable | Private-to-Private Buyout Effect | Private-to-Private R^2 | Public-to-private Buyout Effect | Public-to-private R^2 | Divisional Sales Buyout Effect | Divisional Sales R^2 | Secondary Sales Buyout Effect | Secondary Sales R^2 |
|---|---|---|---|---|---|---|---|---|
| Firm-Level Excess Reallocation All Margins | 0.0547** | 0.42 | 0.0171 | 0.39 | 0.0995*** | 0.44 | 0.0714*** | 0.45 |
| | [0.0228] | | [0.0156] | | [0.0185] | | [0.0235] | |
| Firm-Level Excess Reallocation Births, Deaths & Continuers | -0.0378 | 0.40 | -0.017 | 0.36 | 0.0765*** | 0.37 | 0.0422 | 0.40 |
| | [0.0345] | | [0.0185] | | [0.0226] | | [0.0279] | |
| Firm-Level Job Reallocation All Margins | 0.1174*** | 0.39 | 0.0959*** | 0.45 | 0.1940*** | 0.43 | 0.0935*** | 0.39 |
| | [0.0271] | | [0.0233] | | [0.0448] | | [0.0270] | |
| Firm-Level Job Reallocation Births, Deaths & Continuers | 0.0248 | 0.44 | 0.0617*** | 0.44 | 0.1711*** | 0.41 | 0.0643** | 0.41 |
| | [0.0191] | | [0.0203] | | [0.0441] | | [0.0277] | |
| Observations (000s) | 3,900 | | 400 | | 2,300 | | 600 | |

Panel C: Wages

| | Private-to-private | | Public-to-private | | Divisional Sales | | Secondary Sales | |
|---|---|---|---|---|---|---|---|---|
| Dependent Variable: | | | | | | | | |
| | Buyout Effect | R^2 | Buyout Effect | R^2 | Buyout Effect | R^2 | Buyout Effect | R^2 |
| Change in Wages from t to t+2 | -0.0588* | 0.13 | -0.0183 | 0.81 | 0.1102*** | 0.41 | -0.0301 | 0.37 |
| | [0.0335] | | [0.0156] | | [0.0343] | | [0.0250] | |
| Wage Level in t | 0.0387 | 0.77 | 0.0472* | 0.77 | 0.0675** | 0.74 | -0.0700** | 0.62 |
| | [0.0264] | | [0.0252] | | [0.0276] | | [0.0319] | |
| Wage level in t+2 | -0.0201 | 0.56 | 0.0289 | 0.84 | 0.1777*** | 0.72 | -0.1001** | 0.55 |
| | [0.0360] | | [0.0235] | | [0.0373] | | [0.0397] | |
| Observations (000s) | 2,100 | | 200 | | 1,500 | | 300 | |

Panel D: Productivity

| | Private to Private | | Public To Private | | Divisional Sales | | Secondary Sales | |
|---|---|---|---|---|---|---|---|---|
| Dependent Variable | | | | | | | | |
| | Buyout Effect | R^2 | Buyout Effect | R^2 | Buyout Effect | R^2 | Buyout Effect | R^2 |
| Change in Productivity from t to t+2 | 0.1440*** | 0.436 | 0.2674 | 0.5046 | -0.0429 | 0.3762 | 0.0296 | 0.425 |
| | [0.0431] | | [0.1691] | | [0.0584] | | [0.0596] | |

**Table 7: How Buyout Effects Vary with Market Conditions Near the Closing Date**

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each column reports a single weighted least-squares regression. Moving from left to right across columns, the dependent variable is the firm-level employment growth rate from buyout year t to t+2, the organic part of the firm-level employment growth rate over the same interval, the excess job reallocation rate from t to t+2, the log change in the wage (annual earnings per worker) from t to t+2, and the log change in revenue productivity from t to t+2. The key independent variables are a dummy for whether the observation is a buyout target, a measure of market conditions when the buyout closed, and the interaction between the two. Columns (1)-(5) use real GDP growth to measure market conditions, and columns (6)-10) use the credit spread). We adjust the measures of market conditions to mean zero in the regression sample. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the text for an explanation of how and why we weight observations. The final row presents the estimated effect of a unit standard deviation positive shock to market conditions on the dependent variable for buyout targets relative to control units. Huber-White robust standard errors in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0$.

| | Market Conditions Measure: Real GDP Growth Rate over The Four-Quarter Period Ending When the Buyout Closed | | | | | Market Conditions Measure: Credit Spread In the Month the Buyout Deal Closed | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) Emp. Growth | (2) Organic Growth | (3) Excess Reall. | (4) Wage Growth | (5) Prod. Growth | (6) Emp. Growth | (7) Organic Growth | (8) Excess Reall. | (9) Wage Growth | (10) Prod. Growth |
| Buyout | -0.0136 | -0.0435** | 0.0491*** | -0.0047 | 0.0946** | -0.0117 | -0.0435** | 0.0545*** | -0.0028 | 0.1656** |
| | [0.0210] | [0.0178] | [0.0115] | [0.0159] | [0.0449] | [0.0221] | [0.0195] | [0.0115] | [0.0162] | [0.0708] |
| MktConditions | -6.4790 | -9.642 | -1.141 | 10.9300 | -24.70* | 0.0995 | 0.0995 | -0.0131 | 0.0056 | -0.6737** |
| | [9.137] | [8.889] | [4.161] | [9.940] | [14.67] | [0.0611] | [0.0618] | [0.0346] | [0.0713] | [0.2848] |
| Buyout * MktConditions | -0.2394 | 0.1441 | -0.6586 | -0.6516 | -3.67 | 0.0028 | -0.0012 | 0.0132*** | 0.0066 | 0.0627** |
| | [1.279] | [1.081] | [0.6881] | [0.7838] | [4.905] | [0.0077] | [0.0062] | [0.0045] | [0.0062] | [0.0301] |
| Observations (000s) | 6,400 | 6,400 | 6,400 | 3,700 | 910 | 6,400 | 6,400 | 6,400 | 3,700 | 910 |
| R-squared | 0.3177 | 0.2959 | 0.401 | 0.4113 | 0.4622 | 0.3184 | 0.2957 | 0.4043 | 0.41 | 0.4753 |
| MktConditions Mean | 0.0258 | 0.0258 | 0.0258 | 0.0278 | 0.028 | 5.158 | 5.158 | 5.158 | 4.752 | 5.54 |
| MktConditions St. Dev. | 0.0175 | 0.0175 | 0.0175 | 0.0165 | 0.019 | 3.494 | 3.494 | 3.494 | 3.057 | 3.459 |
| Effect of 1 St. Dev. Shock | -0.0042 | 0.0025 | -0.0115 | -0.0108 | -0.0697 | 0.0098 | -0.0042 | 0.0461 | 0.0202 | 0.2169 |

**Table 8: How Buyout Effects Vary with Changes in Market Conditions in the Two Years after Closing.**
The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each column reports a single weighted least-squares regression. Moving from left to right across columns, the dependent variable is the firm-level employment growth rate from buyout year t to t+2, the organic part of the firm-level employment growth rate over the same interval, the excess job reallocation rate from t to t+2, the log change in the wage (annual earnings per worker) from t to t+2, and the log change in revenue productivity from t to t+2. The key independent variables are a dummy for whether the observation is a buyout target and an interaction between the buyout dummy and a measure of the change in macroeconomic conditions from the buyout year t to t+2. after the transaction closing date. Regressions in the first (second) five columns use the GDP growth rate (credit spread) to measure macroeconomic conditions. We adjust the market conditions measures to mean zero in the regression sample. All specifications include a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the text for an explanation of how and why we weight observations. The final row presents the estimated effect of a one standard deviation positive shock to the macroeconomic variable on the dependent variable for buyout targets relative to control units. Huber-White robust standard errors in brackets; *** $p<0.01$, ** $p<0.05$, * $p<0$.

| | Market Conditions Measure: Annualized Real GDP Growth Rate from the Buyout Year to Two Years later | | | | | Market Conditions Measure: Change in the Credit Spread from the Buyout Year to Two Years Later | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) Emp. Growth | (2) Organic Growth | (3) Excess Reall. | (4) Wage Growth | (5) Prod. Growth | (6) Emp. Growth | (7) Organic Growth | (8) Excess Reall. | (9) Wage Growth | (10) Prod. Growth |
| Buyout | -0.0040 | -0.0405** | 0.0583*** | -0.0052 | 0.1248** | -0.0056 | -0.0397** | 0.0584*** | -0.0061 | 0.1468** |
| | [0.0242] | [0.0203] | [0.0110] | [0.0168] | [0.0559] | [0.0233] | [0.0202] | [0.0121] | [0.0176] | [0.0616] |
| Buyout * MktConditions | 0.9560* | 0.3382 | 0.8823*** | -0.2411 | 1.818 | -0.0057* | -0.003 | -0.0064*** | 0.0033* | -0.0175* |
| | [0.5410] | [0.3993] | [0.2806] | [0.4057] | [1.129] | [0.0030] | [0.0026] | [0.0018] | [0.0020] | [0.0089] |
| Observations (000s) | 6,400 | 6,400 | 6,400 | 3,700 | 910 | 6,400 | 6,400 | 6,400 | 3,700 | 910 |
| R-squared | 0.3189 | 0.2942 | 0.4045 | 0.4096 | 0.4613 | 0.3187 | 0.2946 | 0.4058 | 0.4101 | 0.4675 |
| MktConditions Mean | 0.0463 | 0.0463 | 0.0463 | 0.0466 | 0.0533 | 0.3800 | 0.38 | 0.38 | 0.7460 | -0.181 |
| MktConditions St. Dev. | 0.0337 | 0.0337 | 0.0337 | 0.0343 | 0.0363 | 4.9130 | 4.913 | 4.913 | 4.3710 | 4.257 |
| Impact of Unit St. Dev. Positive Shock | 0.0322 | 0.0114 | 0.0297 | -0.0083 | 0.0660 | -0.0280 | -0.0147 | -0.0314 | 0.0144 | -0.0745 |

**Table 9: How Buyout Effects by Deal Type Vary with Changes in Market Conditions in the Two Years after Closing**

The sample contains firms that underwent private equity buyouts from 1980 to 2011 and matching control firms in the same cells. Each column reports a single weighted least-squares regression for the indicated dependent variable, deal type and post-buyout change in market conditions. See notes to Table 8 for additional information. *** p<0.01, ** p<0.05, * p<0.1.

Panel A: Change in Real GDP

| | Employment Growth Rate from Buyout Year t to t+2 | | | | Organic Employment Growth Rate from t to t+2 | | | |
|---|---|---|---|---|---|---|---|---|
| | Private to Private | Public to Private | Divisional Sales | Secondary Sales | Private to Private | Public to Private | Divisional Sales | Secondary Sales |
| Buyout | 0.1284*** | -0.1276*** | -0.0951* | 0.0901*** | 0.0270** | -0.1007*** | -0.1473*** | 0.0519** |
| | [0.0259] | [0.0390] | [0.0533] | [0.0240] | [0.0137] | [0.0285] | [0.0465] | [0.0212] |
| Buyout * MktConditions | 0.2795 | -0.0460 | 1.8230 | 0.8187 | -1.211*** | -0.035 | 1.1760 | 0.8389 |
| | [0.6737] | [0.7233] | [1.139] | [0.6354] | [0.3393] | [0.5330] | [0.8442] | [0.5191] |
| Observations (000s) | 3,900 | 400 | 2,300 | 600 | 3,900 | 400 | 2,300 | 600 |
| R-squared | 0.3737 | 0.3828 | 0.3262 | 0.3254 | 0.3369 | 0.3885 | 0.3034 | 0.3109 |
| MktConditions Mean | 0.0446 | 0.0537 | 0.0526 | 0.0245 | 0.0446 | 0.0537 | 0.0526 | 0.0245 |
| MktConditions St. Dev. | 0.0344 | 0.0275 | 0.0348 | 0.0380 | 0.0344 | 0.0275 | 0.0348 | 0.038 |
| Impact of 1 SD Shock | 0.0096 | -0.0013 | 0.0634 | 0.0311 | -0.0417 | -0.0010 | 0.0618 | 0.0206 |

| | Excess Reallocation Rate from t to t+2 | | | | Wage Growth Rate from t to t+2 | | | |
|---|---|---|---|---|---|---|---|---|
| | Private to Private | Public to Private | Divisional Sales | Secondary Sales | Private to Private | Public to Private | Divisional Sales | Secondary Sales |
| Buyout | 0.0529** | 0.0463*** | 0.1173*** | 0.0650*** | -0.0583* | -<br>0.0511*** | 0.1060*** | -0.0325 |
| | [0.0222] | [0.0179] | [0.0194] | [0.0238] | [0.0326] | [0.0195] | [0.0362] | [0.0234] |
| Buyout * MktConditions | -0.557 | 1.0282*** | 1.6722** | 0.5963 | 0.5433 | -1.418*** | -0.4162 | 0.7131 |
| | [0.3959] | [0.3457] | [0.7420] | [0.4568] | [1.257] | [0.5263] | [0.5169] | [0.7191] |
| Observations (000s) | 3,900 | 400 | 2,300 | 600 | 2,100 | 200 | 1,500 | 300 |
| R-squared | 0.4255 | 0.3946 | 0.4493 | 0.4485 | 0.1286 | 0.8147 | 0.4087 | 0.3662 |
| MktConditions Mean | 0.0446 | 0.0537 | 0.0526 | 0.0245 | 0.0423 | 0.0482 | 0.0522 | 0.0320 |
| MktConditions St. Dev. | 0.0344 | 0.0275 | 0.0348 | 0.038 | 0.0374 | 0.0295 | 0.0354 | 0.0332 |
| Impact of 1 SD Shock | -0.0192 | 0.0283 | 0.0582 | 0.0227 | 0.0203 | -0.0418 | -0.0147 | 0.0237 |

| | Productivity Change from t to t+2 | | | |
|---|---|---|---|---|
| | Private to Private | Public to Private | Divisional Sales | Secondary Sales |
| Buyout | 0.1588*** | 0.4980** | -0.0079 | 0.0245 |
| | [0.0459] | [0.2153] | [0.0713] | [0.0606] |
| Buyout * MktConditions | -2.127 | 8.753** | 1.572 | -1.284 |
| | [1.308] | [3.778] | [1.573] | [1.118] |
| Observations (000s) | 410 | 20 | 620 | 40 |
| R-squared | 0.4446 | 0.5498 | 0.3804 | 0.4275 |
| MktConditions Mean | 0.0406 | 0.0489 | 0.0584 | 0.0367 |
| MktConditions St. Dev. | 0.0456 | 0.0338 | 0.0373 | 0.0332 |
| Impact of One SD Shock | -0.0970 | 0.2959 | 0.0586 | -0.0426 |

Panel B: Change in Credit Spread

| | Employment Growth Rate from Buyout Year t to t+2 | | | | Organic Employment Growth Rate from t to t+2 | | | |
|---|---|---|---|---|---|---|---|---|
| | Private to Private | Public to Private | Divisional Sales | Secondary Sales | Private to Private | Public to Private | Divisional Sales | Secondary Sales |
| Buyout | 0.1362*** | -0.1102*** | -0.1210*** | 0.0788*** | 0.0288* | -0.0868*** | -0.1637*** | 0.0422** |
| | [0.0250] | [0.0304] | [0.0466] | [0.0246] | [0.0156] | [0.0248] | [0.0429] | [0.0211] |
| Buyout * MktConditions | -0.0104** | -0.0064 | 0.0062 | -0.0075** | 0.0025 | -0.0051 | 0.0036 | -0.0070*** |
| | [0.0048] | [0.0039] | [0.0066] | [0.0030] | [0.0025] | [0.0034] | [0.0056] | [0.0025] |
| Observations (000s) | 3,900 | 400 | 2,300 | 600 | 3,900 | 400 | 2,300 | 600 |
| R-squared | 0.3773 | 0.3857 | 0.3221 | 0.3271 | 0.3326 | 0.3913 | 0.3004 | 0.3126 |
| MktConditions Mean | 0.1860 | 0.7200 | -0.0925 | 4.4830 | 0.186 | 0.72 | -0.0925 | 4.483 |
| MktConditions St. Dev. | 4.7130 | 4.1240 | 3.3960 | 7.4070 | 4.713 | 4.124 | 3.396 | 7.407 |
| Impact of One SD Shock | -0.0490 | -0.0264 | 0.0211 | -0.0556 | 0.0118 | -0.0210 | 0.0122 | -0.0518 |

| | Excess Reallocation Rate from t to t+2 | | | | Wage Growth Rate from t to t+2 | | | |
|---|---|---|---|---|---|---|---|---|
| | Private to Private | Public to Private | Divisional Sales | Secondary Sales | Private to Private | Public to Private | Divisional Sales | Secondary Sales |
| Buyout | 0.0563** | 0.0294* | 0.1115*** | 0.0563** | -0.0585* | - 0.0459*** | 0.1134*** | -0.0295 |
| | [0.0232] | [0.0173] | [0.0190] | [0.0221] | [0.0333] | [0.0144] | [0.0338] | [0.0249] |
| Buyout * MktConditions | -0.0019 | -0.0049* | -0.0114** | -0.0057* | 0.0019 | 0.0113*** | -0.0041 | 0.0026 |
| | [0.0022] | [0.0025] | [0.0046] | [0.0029] | [0.0022] | [0.0023] | [0.0033] | [0.0024] |
| Observations (000s) | 3,900 | 400 | 2,300 | 600 | 2,100 | 200 | 1,500 | 300 |
| R-squared | 0.4242 | 0.3922 | 0.4524 | 0.4502 | 0.1282 | 0.8190 | 0.4098 | 0.3657 |
| MktConditions Mean | 0.186 | 0.72 | -0.0925 | 4.483 | 1.1500 | 0.8250 | 0.1830 | 2.1130 |
| MktConditions St. Dev. | 4.713 | 4.124 | 3.396 | 7.407 | 4.5940 | 3.8590 | 3.3930 | 5.9950 |
| Impact of One SD Shock | -0.0090 | -0.0202 | -0.0387 | -0.0422 | 0.0087 | 0.0436 | -0.0139 | 0.0156 |

| | Productivity Growth Rate from t to t+2 | | | |
|---|---|---|---|---|
| | Private to Private | Public to Private | Divisional Sales | Secondary Sales |
| Buyout | 0.1018*** | 0.5028*** | -0.013 | 0.012 |
| | [0.0319] | [0.1872] | [0.0635] | [0.0616] |
| Buyout * MktConditions | 0.0163 | -0.0711*** | -0.0087 | 0.0190*** |
| | [0.0103] | [0.0270] | [0.0083] | [0.0051] |
| | | | | |
| Observations (000s) | 410 | 20 | 620 | 40 |
| R-squared | 0.4463 | 0.5884 | 0.3802 | 0.4413 |
| MktConditions Mean | 0.248 | 0.258 | 0.11 | 1.603 |
| MktConditions St. Dev. | 5.452 | 5.202 | 2.537 | 4.833 |
| Impact of One SD Shock | 0.0889 | -0.3699 | -0.0221 | 0.0918 |

**Figure 1: Quarterly Buyout Counts by Deal 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 1.A for an explanation of how we construct of our sample of 9,794 leveraged buyouts sponsored by private equity firms.

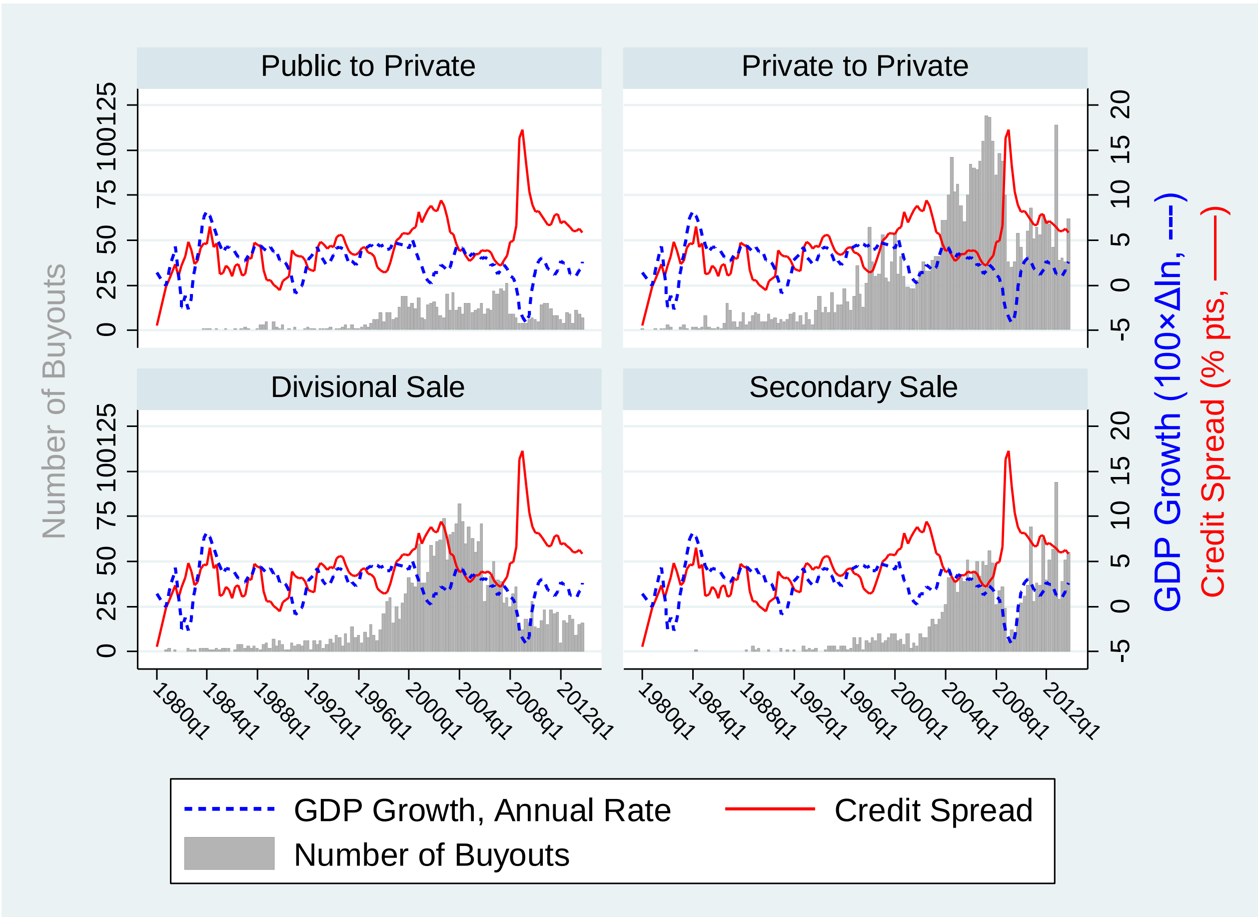


**Figure 2: How the Post-Buyout Rates of Productivity Growth and Excess Reallocation of Targets Vary with the Credit Spread on the Buyout Date**

This chart uses the estimated coefficient on the credit spread interaction effects in Table 7 to depict how target outcomes over the two years after the buyout vary with credit spreads at the time of the buyout. Center bars show baseline effects on rates of productivity growth and excess reallocation when the credit spread equals its sample mean. The other bars show buyout effects on targets for credit spread values -2, -1, 1 and 2 standard deviations about the sample mean.

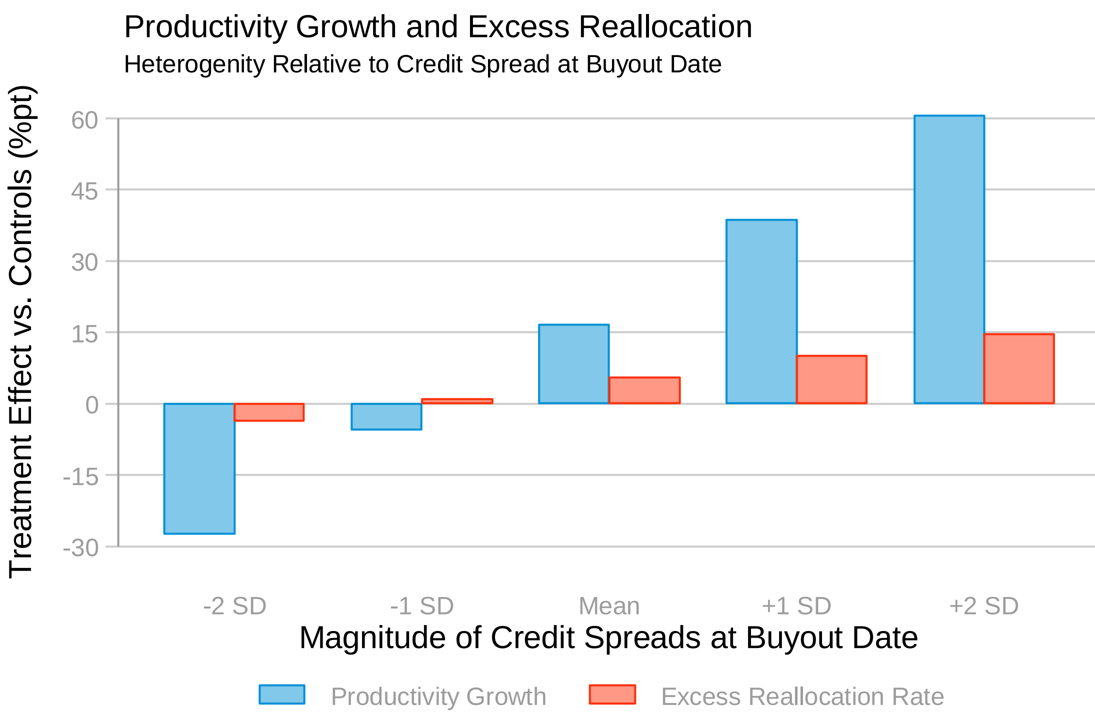
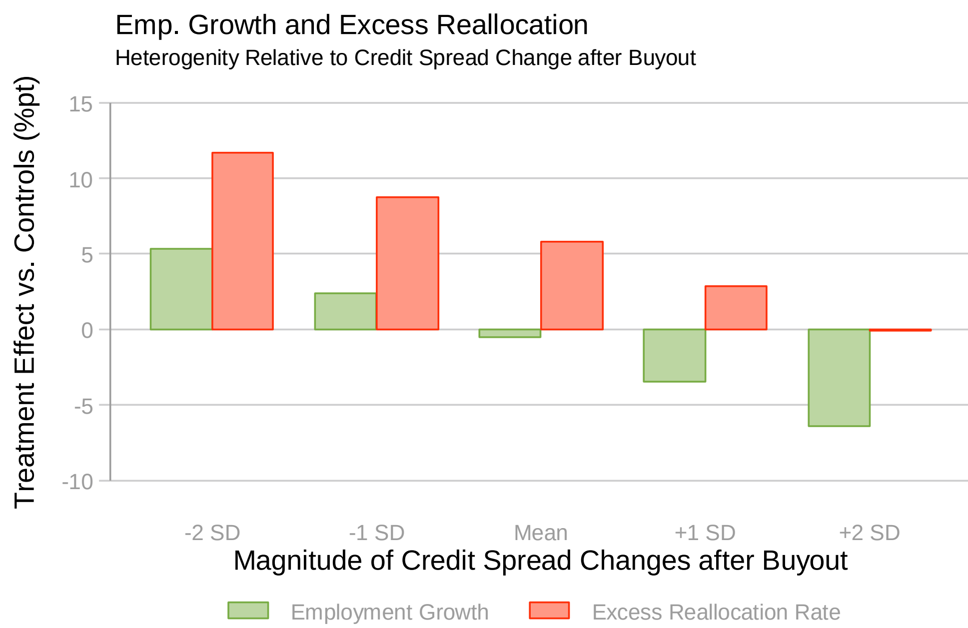
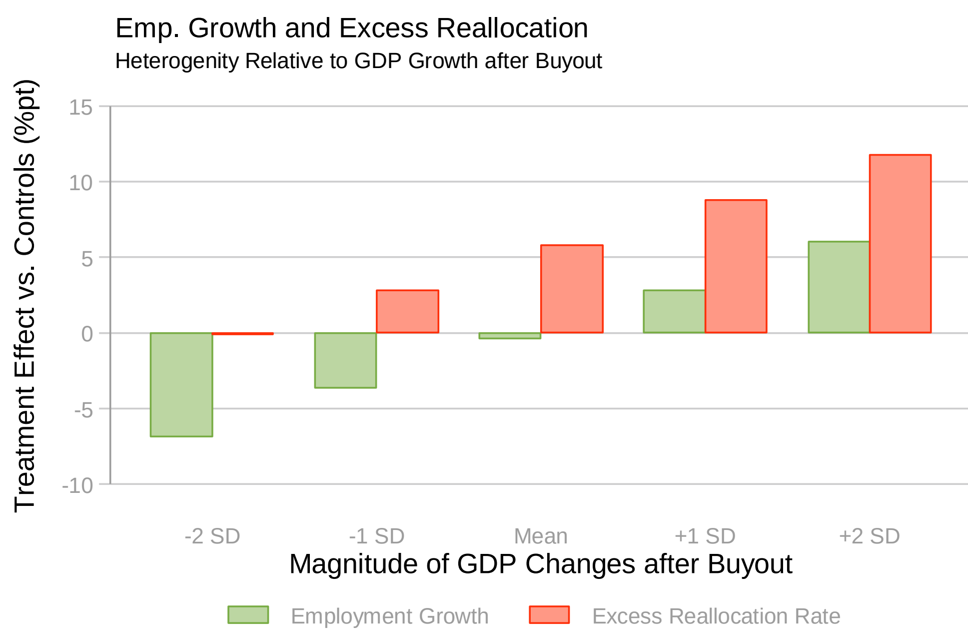


**Figure 3: How the Post-Buyout Rates of Employment Growth and Excess Reallocation of Targets Vary with the Post-Buyout Evolution of Market Condition**

The top panel shows the impact of changes in credit spreads on outcomes. The bottom panel shows the impact of GDP changes. These results are based on Table 8. The bars graphing the effects are centered on the Average Treatment Effect at the mean ± 2 standard deviations.



# Online Appendix: 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 firm 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). 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 code we developed and implemented algorithm 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 criteria and the algorithm used to identify matches in detail.[30] 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.

### 2.1. 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.

[30] 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.

## 2.2. 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.[31] 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. 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

[31]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 should help overcome some of this source of noise. Note that we did not find that our match rates had peaks in Census years suggesting this is not a major issue.

identified these problematic cases by observing that the matched target establishment remained affiliated with the parent seller firm even after the buyout transaction. 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 transaction 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.

We develop a methodology that takes advantage of the timing of the acquisition event and the time series properties of the associated firms to identify the target. Our strategy then requires we match the target name and address to a window of years around the acquisition event. For each target we match their information to the BR at time of acquisition, $t$, and to $t+1$ and $t-1$. In addition to the history of activity, we also exploit the employer tax identifier, the EIN, of the firms associated with the target.[32]

### 2.3. How We Proceed

We describe our process for de-duplicating the buyout transactions that are matched to multiple Census firm IDs by separating them into a set of mutually exclusive cases.

<u>No Matches</u>

In about 2000 of the 9794 deals in CapitalIQ, no companies within the BR matched even using the loosest matching criterion.

[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.

Unique Matches

As previously described, the search algorithm first proceeds through 16 rounds of matching using progressively less strict match criteria. A unique match is a match to a single firm identifier in the strictest match criterion available for that deal. For example, suppose a buyout transaction target from CapitalIQ matches to a single BR entry in round 4 of our algorithm. If it also matches to multiple firms in subsequent, less strict rounds, but had no matches until round 4, then this entry is a unique match. From the initial 9794 deals in CapitalIQ, we find about 4000 unique matches.

Non-Unique Matches and De-Duplications

The remaining set of about 3500 deals from CapitalIQ match to multiple firms within the round where the strictest match criterion is applied. This could happen, for example, if we exact match on address, but there are multiple firms in a single building with similar company names. We use several methods to arrive at a unique match between the CapitalIQ and a Census firm ID.

The first method for de-duplicating our dataset is to check the EINs of the matched firms. In about 25 percent of the duplicate, matches matched to multiple firms with the same EIN. This possibility arises given our use of the three-year window and is an indicator that M&A and/or reorganization activity is underway. This enables us to link the multiple matches and we follow the continuing firmid when calculating employment growth rates, etc.

The second method for de-duplicating or data is to exploit the timing pattern of the matches. We look for cases with at least two firms associated with the same deal. A common pattern is that one of them is a birth of a new firm ID at time t or t+1 and the other is a death at time t-1 or t. In this context, a birth is when a firm ID appears at time t or t+1 and that firm ID did not exist in the preceding years (t-1 if the birth is in t or t-1 and t if the birth is in t+1) and a death is when a firm ID disappears in time t or t+1. We investigated these patterns and determined they are likely to indicate PE-precipitated reorganization. These firm IDs were already matched using name and addressing matching criteria, so that they are not simply spurious patterns in the data. This second step uniquely resolves about 200 additional firm IDs to a transaction.

The third method follows four rules we developed to help resolve duplicate matches.

- Rule 1: Within a set of duplicates, we choose the firm ID that has the strictest match criteria. For example, if we have a duplicate match and one firm identifier has an exact name and address match and a different firm identifier only matches on the name, we resolve the duplicate by keep the highest quality match.

- Rule 2: We choose the firm IDs with the strictest match criteria and condition on survival to period t+1 in the LBD.
- Rule 3: We apply Rule 2, but then also include resolutions from Rule 1 that may not have survived into t+1.
- Rule 4: We change the order of operations. We condition on survival to period t+1, then choose the match that satisfies the strictest matching criteria.

When all four of these rules resolve to the same firm, we consider that firm to be the match and use it in our analysis. These rules uniquely resolve about 1000 additional deals to a Census firm ID. Combined, these resolution criteria yield approximately 2000 additional matched deals. This gives us the total sample of approximately 6000 matched deals.