The (Heterogenous) Economic Effects of Private Equity Buyouts

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Abstract: The effects of private equity buyouts on employment, productivity, and job reallocation vary tremendously with macroeconomic and credit conditions, across private equity groups, and by type of buyout. We reach this conclusion by examining the most extensive database of U.S. buyouts ever compiled, encompassing thousands of buyout targets from 1980 to 2013 and millions of control firms. Employment shrinks 12% over two years after buyouts of publicly listed firms – on average, and relative to control firms – but expands 15% after buyouts of privately held firms. Post-buyout productivity gains at target firms are large on average and much larger yet for deals executed amidst tight credit conditions. A post-buyout tightening of credit conditions or slowing of GDP growth curtails employment growth and intra-firm job reallocation at target firms. We also show that buyout effects differ across the private equity groups that sponsor buyouts, and these differences persist over time at the group level. Rapid upscaling in deal flow at the group level brings lower employment growth at target firms. We relate these findings to theories of private equity that highlight agency problems at portfolio firms and within the private equity industry itself.

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Private equity-backed buyouts are among the most dramatic transformations of corporate structures seen in Western economies. They are also intensely controversial. While some view buyouts as beneficial in resolving agency problems at target firms, critics point to the prevalence of agency problems within the private equity (PE) industry itself. These contrasting perspectives differ in their implications for the economic effects of buyouts on target firms.

We present evidence of a remarkable heterogeneity in the economic effects of buyouts. Specifically, the impacts on employment, job reallocation, and productivity differ markedly by type of buyout, with credit conditions that prevail when the buyout closes, with the evolution of macroeconomic and credit conditions after the buyout, and across the PE groups that sponsor buyouts. We argue that these patterns favor a nuanced view of buyouts, where their impact is shaped both by the portfolio firms and the dynamics of the private equity market.

To carry out our study, we tap multiple sources to identify and characterize about 9,800 PE buyouts of U.S. firms from 1980 to 2013. For roughly 6,000, we successfully merge information about the buyout with comprehensive Census micro data on firm-level and establishment-level outcomes. Armed with this database, we estimate the effects of buyouts on target firms relative to contemporaneous developments at comparable firms not backed by private equity. We focus on outcomes over the first two and five years after the buyout. Our large sample, long time period, high-quality data, and ability to track firms and establishments enable a careful look at the real-side effects of buyouts. We can, for example, investigate how the effects on independent, privately held firms compare to those on publicly listed firms. Because our sample period encompasses huge swings in credit market tightness and macroeconomic performance, we can address recurring questions about how these external conditions affect the relative performance of target firms. By tracking individual PE groups over time, we can assess whether they differ in their impact on target firms, and whether and how much those differences persist over time.

We find, first, that target firms experience large labor productivity gains relative to contemporaneous outcomes at control firms over the first two years post buyout. Second, productivity gains are much larger for deals executed amidst tight credit conditions or in recessions. Third, relative employment at targets rises 15 percent, on average, in firms previously under private ownership, whereas it falls 12 percent in buyouts of publicly listed firms. These results also hold when we look over a five-year (rather than two-year) horizon. Fourth, buyout effects on employment differ among the PE groups that sponsor buyouts, and these differences persist over time at the group level.[2] Fifth, rapid upscaling in deal flow at the PE group level brings weaker post-buyout employment performance at target firms, conditional on the group's performance history, time effects, and a battery of other controls. In short, the real-side effects of buyouts on target firms are more complex and varied than either PE champions or detractors claim. Indeed, the effects are highly circumstance-dependent in a manner that aligns well with theory and with evidence on the financial performance of PE buyouts. While the mix of buyout types and PE sponsor characteristics vary over time, we find little evidence that compositional changes drive our key results.

Our study builds on and draws inspiration from many previous works. Early studies of the real-side outcomes associated with PE buyouts include Kaplan (1989) and Lichtenberg and Siegel (1990). More recent research considers larger samples, often by combining proprietary and government sources. Examples include Boucly, Sraer, and Thesmar (2011), Cohn, Mills, and

[2] Echoing persistent financial performance differences at the group level (e.g., Kaplan and Schoar, 2005, and Harris et al., 2023). Unlike the case of financial performance, however, we see no evidence of a weakening over time in the group-level persistence of real-side effects.

Towery (2014), Davis et al. (2014), , Farcassi, Previtero, and Sheen (2018), and Cohn, Nestoriak, and Wardlaw (2021). Beginning with Bernstein and Sheen (2016), many recent studies consider the impact of private equity in particular industry settings. Relative to Davis et al. (2014), we improve on their empirical methods, extend their sample period to cover the financial crisis and its aftermath, draw on previous research to explain why we anticipate heterogeneity in the real-side effects of PE buyouts, and provide a rich set of new findings on how buyout effects vary with macroeconomic and credit conditions, by type of buyout, across the PE groups that sponsor buyouts, and with the scale of buyout activity at the group level. Below, we offer many additional remarks about how our study and findings relate to previous research.

The next section reviews theoretical perspectives and prior empirical research to motivate several hypotheses about the heterogenous effects of PE buyouts. Section II discusses the creation of our database. Section III sets forth our empirical approach, and Section IV reports our results. Section IV offers concluding remarks. Appendices provide more information about our data and empirical methods plus additional results.

I. Conflicting Views of the Effects of Private Equity Buyouts

Academic and popular critics are sharply divided on the impact of PE buyouts. We review aspects of the literature in this section, with particular attention to potential effects on real-side outcomes at target firms.[3]

A. Private Equity Groups as Value Creators

Buyout funds are associated with positive risk-adjusted returns (Korteweg, 2019). These returns are often seen as rooted in the tradeoffs between publicly traded and privately held

[3] The main text focuses on productivity, employment and job reallocation effects. Appendix D presents estimated buyout effects on firm-level mean wages, which appear to be heavily influenced by buyout-related shifts in management compensation.

ownership. In particular, it has been widely understood since Jensen and Meckling (1976) that being publicly traded can bring greater vulnerability to agency problems. Due to weaknesses in the market for corporate control, difficulties in monitoring by dispersed shareholders, problematic incentives of corporate directors, compensation schemes that reward empire building and myriad other reasons, publicly traded firms can be especially prone to value-destroying activities.

Jensen (1989) proposed that buyouts are optimized to resolve these problems. In particular, he highlights three common features of PE buyouts: high leverage (thereby creating pressure to cut costs to maximize free cash flow), greater use of equity-based compensation to align manager incentives, and governance by a small, focused board of directors. These features incentivize cost-cutting moves, which are likely to boost productivity. While these tools are deployed in all types of buyout transactions, we expect their effects to be stronger on target firms with more serious agency problems.

For privately held targets, these agency problems may be less intense due to a much-reduced separation between ownership and control. But while private firms may face fewer agency problems, they may not benefit from the well-documented benefits of the public market and find it hard to access capital at a reasonable price.[4] As a result, in these contexts, it makes sense for PE groups to devote greater attention to easing capital constraints to drive investment and growth.

The foregoing discussion motivates two hypotheses:

Hypothesis 1: The consequences of buyouts differ between publicly listed and privately held targets, with a greater emphasis on cost-cutting in the former and on investment in the latter.

[4] Among the papers hypothesizing advantages of public ownership such as lower equity costs and relaxed capital constraints are Zingales (1995), Pagano, Panetta, and Zingales (1998), Chemmanur and Fulghieri (1999), Maksimovic and Pichler (2001), and Brau and Fawcett (2006).

Hypothesis 2: Buyouts boost the productivity of target firms, albeit through different mechanisms in different types of buyouts.

B. Private Equity Groups and Internal Agency Issues

Another literature highlights the potential for agency issues with private equity groups. These may again lead to differences in the effects of buyouts, including differences over the economic cycle. The classic paper on these issues is Axelson, Stromberg, and Weisbach (2009). In their setting, privately-informed firms (e.g., the general partners of PE groups) raise funds from less-informed investors. Informational asymmetries create a temptation on the part of general partners to overstate the potential of their investments. Axelson et al. (2013) show that the (second-best) solution ties the compensation of PE investors to the collective performance of a fund, rather than that of individual buyouts. In this way, the general partners have less incentive to invest in bad deals. Moreover, it makes sense for fund managers to invest equity alongside outside debt raised on a deal-by-deal basis, thus providing a further check on the temptation to do lower-quality deals. Even when employing this optimal financing structure, however, Axelson et al. (2013) show that PE groups are tempted to overinvest during hot markets.

This theory suggests that deals done when financing is plentiful may underperform for two reasons. First, due to overfunding, PE groups may move "down their own demand curve" when financing is easy, selecting inferior deals with less scope for value creation in the form of operational improvements. Second, if the supply of experienced PE managers is not fully elastic in the short term (as in Kortum and Lerner, 2000 and Ewens and Rhodes-Kropf, 2015), a larger deal flow may dilute the attention paid to any given portfolio company.[5] Thus:

[5] Axelson et al. (2013) show that credit market conditions influence leverage in buyouts much more than in publicly listed firms. Kaplan and Schoar (2005), among others, find that easier credit conditions bring greater inflows into buyout funds and lower fund-level returns. Other papers that

Hypothesis 3: Deals executed amidst easy credit conditions exhibit weaker post-buyout operating performance. Put another way, the marginal benefits of PE buyouts in the form of productivity gains are countercyclical.

A second set of principal-agent problems can arise in the fund allocation process. Empirical studies such as Lerner, Schoar, and Wongsunwai (2007) and Andonov, Hochberg, and Rauh (2018) suggest that the sophistication of limited partners varies sharply, due to such considerations as the presence of political appointees on some investment committees. This insight finds support in evidence that buyout groups with a track record of poor performance (and poor expected performance) still raise follow-on funds (Kaplan and Schoar, 2005, and Harris et al., 2023).[6] Thus:

Hypothesis 4: The real-side performance of buyout targets differs across PE groups, and these differences persist from one fund to the next at the group level.

II. Our Sample of Private Equity Buyouts

A. Identifying Private Equity Buyouts

We consider transactions that involve changes in the ownership and control of later-stage companies, and that are executed and partly financed by PE firms. In these buyout deals, the (lead) PE firm acquires a controlling equity stake in the target firm and retains significant oversight until it exits by selling its stake. The buyout typically involves a shift toward greater leverage in the capital structure of the target firm and, sometimes, a change in its management. Bank loans are key sources of credit that facilitate the leveraged nature of PE buyouts.

touch in various ways on market cycles and private equity include Kaplan and Stein (1993), Ivashina and Kovner (2011), Hotchkiss, Strömberg, and Smith (2021), Harris, Jenkinson, and Kaplan (2016), and Bernstein, Lerner, and Mezzanotti (2019). But the impact of these forces on the real-side outcomes at target firms has received little attention to date.

[6] These findings also align with evidence that organizational attributes have persistent effects on productivity. See Bloom and van Reenen (2007), Syverson (2011), and Autor et al. (2020).

We made major efforts to construct our sample of these buyouts and to ensure its integrity, expending thousands of research assistant hours, as detailed in Appendix A. The resulting sample contains 9,794 PE-led leveraged buyouts of U.S. companies from January 1, 1980 to December 31, 2013. We sort these buyouts into four main deal types based on descriptions in CapitalIQ and our reviews of other databases, press accounts, and securities filings.

Figure 1 displays quarterly counts of our PE-sponsored buyouts for the four deal types.[7] As also noted in other studies, PE buyout activity grew enormously in recent decades. The expansion is especially striking for private-to-private buyouts, which saw a huge increase in deal flow over time. The flow of new PE buyouts crashed during the financial crisis, as credit conditions tightened and the economy contracted. Interestingly, the flow of new public-to-private buyouts dropped off well before the onset of the financial crisis and remained at modest levels through the end of our sample period. Counts for private-to-private and secondary deals (where PE groups are on both sides of the transaction) rebounded sharply as the economy recovered from the 2008-09 recession and maintained a robust pace until the end of our sample period in 2013.

Appendix Table D.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 Utilities. A Pearson chi-squared test rejects the hypothesis that the industry distribution of buyouts is independent of deal type. The distributions of PE buyouts by industry, firm size, and firm age also differ greatly from the corresponding distributions of private sector employment (Davis et al., 2014). Given these patterns, our econometric investigations below

[7] Appendix Table D.1 reports average quarterly counts before, during and after the financial crisis.

compare buyout targets to control firms within cells defined by the cross product of industry, firm size categories, firm age categories, multi-unit status, and buyout year.

B. Merging Private Equity Buyouts with 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.[8] It draws on a wide range of administrative records and survey sources for data inputs. Firms are defined based on operational control, and all establishments majority owned by a parent firm are included in the parent's activity measures. Core data items include employment, payroll, four-digit Standard Industrial Classification (SIC) or six-digit North American Industrial Classification (NAICS) codes, employer identification numbers, business names, and location information.

To merge our buyouts with 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. Given our interest in employment dynamics, the relationship of the LBD employment measure to the timing of PE buyouts requires careful treatment. The LBD reports total employment in the payroll period containing the week of March 12. Accordingly, for buyouts that close before October 1, LBD employment in March of the same calendar year serves as our contemporaneous employment measure. We assign buyouts that close on or after October 1 in calendar year *t* to the

[8] An establishment is a physical location where economic activity occurs. A firm is a legal entity that owns and operates one or more establishments.

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.[9]

Table 1 summarizes our sample of PE buyouts matched to Census micro data. Panel A reports the number of establishments operated by our 6,000 matched target firms and their employment, with breakdowns by deal type. Panel B considers the 5,100 matched buyouts that closed from 1980 to 2011. Compared to the 1980-2003 sample in Davis et al. (2014), our new 1980-2011 analysis sample has 2.3 times as many matched targets, reflecting high deal flow after 2003. Private-to-private deals account for about half of our 1980-2011 sample, as in our earlier work. The 22% share of secondary sales is nearly twice as large as in our earlier work, reflecting the large flow of these deals in recent years. The share of divisional buyouts is somewhat smaller in our new sample. Panel C compares matched buyouts in our new sample to those in Davis et al. (2014) for their 1980-2003 analysis period. Our new sample has about 20% fewer buyouts in the overlapping period, which reflects the more rigorous matching criteria that we now apply. Our new sample of two-year continuer targets (excluding EIN cases) has 10% fewer matched buyouts. The mix of buyout types in our new 1980-2003 sample is similar to the one in our earlier work.

In our econometric analysis below, we focus on matched buyouts that closed from 1980 to 2011, so we can track their outcomes through 2013 in the LBD. We drop target firms that match to Census micro data using only taxpayer EINs (and not other firm identifiers). As explained in Appendix A, we cannot confidently identify all establishments operated by the target firm in these EIN cases. Finally, we restrict our main regression analyses to firms that we confidently track for

[9] 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. (2014) for additional discussion. As an empirical matter, buyout closing dates are distributed fairly evenly over the calendar year.

at least two years post buyout.[10] That leaves roughly 3,600 target firms, identified as "Two-year continuers" in Panel B of Table 1. Private-to-private deals account for 29% of target employment as of the buyout year in this sample, public-to-private deals account for 36%, divisional deals account for 11%, secondary sales account for 19%, and buyouts of unknown type for the rest.

III. Empirical Approach

A. Creating Samples for Analysis of Buyout Effects

The samples we use to estimate the effects of buyouts contain the target firms designated as "Two-year continuers" in Panel B of Table 1 plus control firms. To obtain control firms, we consider cells defined by the cross product of the buyout year t, the target firm's three-digit NAICS industry classification, the target firm's size category, its age category, and an indicator for whether it owns multiple establishments. The control firms in any given control cell are all firms in that cell that *never* underwent a PE-sponsored buyout in our 34-year sample period. In this way, we sidestep concerns about a "bad comparisons" bias that can arise in staggered difference-in-difference regression applications (Baker, Larcker, and Wang, 2022).

In constructing the cross product of control cells, we use about 90 industries, ten firm size categories, six age categories, a dummy for multi-unit firms, and 32 distinct buyout years from 1980 to 2011.[11] Thus, the full cross product entails about 10,000 control cells per buyout year. Of

[10] Robustness analysis in section IV.H explores results over a five-year horizon.

[11] 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. 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.

course, many cells are unpopulated. Nevertheless, it is clear that our approach matches targets to control firms at a granular level.

B. Regression Specifications

To assess the effects of buyouts on target-firm outcomes, we estimate least-squares regression specifications of the following form:

$$Y_{i,t+s} = \sum_{c} D_{ict}\,\theta_c + \gamma_{t+s}\,PE_{it} + \lambda_1 LEST_{it} + \lambda_2 LFIRM_{it} + \varepsilon_{i,t+s} \qquad (1)$$

where $Y_{i,t+s}$ is, for example, the employment growth rate from t to $t+s$ at firm *i.* Alternatively, $Y_{i,t+s}$ is the change from t to $t+s$ in the pace of job reallocation at the firm or the percent change in its labor productivity or average annual compensation per employee. $PE_{it} = 1$ if firm i is a buyout target in t, and 0 otherwise. The D_{ict} are cell-level dummy variables defined on the cross product described above, where *c* indexes control cells, and the θ_c are cell-level fixed effects.

Specification (1) also includes two controls for pre-buyout firm-level growth, $LEST_{it}$ and $LFIRM_{it}$. 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 $t-1$ (regardless of who owned them during that time period). 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 is 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. As reported in Appendix Table B.2, we find no indication that the establishments acquired in buyouts grew faster (or slower) before the buyout than their control counterparts. There is evidence that $LFIRM_{it}$ is greater for target firms than for the corresponding control firms, which suggests that target firms grew more rapidly than controls before the buyout via acquisitions.

C. Using Appropriate Weights to Obtain the Treatment Effect of Interest

We estimate the average treatment effect of buyouts on targets under two standard identification assumptions. The first is conditional mean independence (CMI): conditional on controls and the treatment indicator, outcomes for the treated and untreated firms are independently distributed. The second is the stable unit treatment value assumption (SUTVA): applying the buyout treatment to one unit has no effect on outcomes at other units. Under these two assumptions, a suitably weighted least-squares regression model of the form (1) yields the average treatment effect of buyouts on the target firms.

We adopt three principles in constructing firm-level weights for the regression analysis:

1. **Target-Share Weighting (TS):** Weight each target (and each target cell) by its share of aggregate target employment, where "aggregate" refers to the sum over all buyouts as of their respective buyout years.
2. **Set Weight of Controls to Weight of Corresponding Targets (SCT):** For controls in a given cell, rescale the sum of their weights to the cell's target employment share.
3. **Equalize Control Weights (EC):** Equalize weights across control units in the same cell.

Principle **TS** helps recover an average treatment effect on the treated that reflects the distribution over cells of target activity levels, i.e., their target employment shares. Even when fitting a regression specification with a homogenous treatment effect, we recognize that the true effect may differ across cells. In this situation, we give more weight to the underlying treatment effect in cells that account for larger shares of target employment, as opposed to a classic difference-in-difference estimator that weights each observation equally.[12]

[12] Recall from Table 1 that private-to-private buyouts account for half of deals but only 29% of target employment. Thus, assigning the same weight to each target (and target cell) would yield a much greater influence of private-to-private buyouts on the estimated treatment effect.

Principle **SCT** ensures that the aggregate weight of the controls in a given cell c is the same as the aggregate weight of targets in the same cell. The motivation is analogous to the motivation for principle **TS.** Principle **EC** ensures that point estimates and standard errors are not unduly influenced by large control units that exhibit extreme outcomes, possibly due to measurement errors. This principle is especially useful for cells that cover the largest firms, because such cells may have few controls and there is no upper bound on unit size.

To illustrate the role of these principles, consider a simple example. Cell A has two targets and eight controls, while Cell B has one target and 10,000 controls. All units have the same employment level in the buyout year (so **EC** holds by construction). Average growth rates from buyout year t to year $t+s$ are as follows: 5% for targets and 2.5% for controls in Cell A, and 5% for the target and controls in Cell B. Given CMI and SUTVA, we can obtain the weighted average treatment effect of buyouts on target-firm growth by calculating the target-control growth differential in each cell and then averaging the differentials over cells. Choosing weights in accordance with **TS** and **SCT** yields a non-parametric estimate for the weighted average treatment effect on the treated: (2/3)(5-2.5) + (1/3)(5-5) = 1.67 percent. This estimator gives twice as much weight to Cell A, because Cell A contains twice as much target activity as Cell B.

We can also recover this estimate using a least-squares regression with suitably weighted observations. Consider a simplified version of (1) given by

$$Y_{i,t+s} = \alpha + \gamma_{t+s}\, PE_{it} + \varepsilon_{i,t+s}. \qquad (2)$$

To satisfy principles **TS** and **SCT**, set the weights to 1 for each target, (2/8) for each control unit in cell A, and (1/10,000) for each control unit in Cell B. With these weights, a weighted least-squares regression yields $\hat{\gamma}_{t+s} = 1.67$ percent, the same as the simple non-parametric estimator. Our regression analysis below works the same way but extends to settings with many cells and many controls. Morgan and Winship (2007, chapters 4 and 5) more fully discuss this type of matching estimator and its properties. See Iacus, King, and Porro (2012) as well.

To see how inference can go awry when neglecting **TS** and **SCT**, suppose we estimate (2) in the manner of a standard difference-in-difference regression, giving equal weight to each observation. Clearly, that approach yields an estimated γ_{t+s} value of essentially zero, which is not informative about the treatment effect of interest. Weighting each firm by its share of *overall* employment does not fix the problem, nor does the inclusion of cell-level fixed effects.

It is worth explaining the role of cell fixed effects when estimating (1) via least squares with our weighting principles. As verified in Appendix Table B.1, we obtain the same estimated value for the treatment effect of interest in specifications with no continuous controls, regardless of whether we include cell-level fixed effects. But adding fixed effects raises the regression R-squared value and yields tighter standard errors on the estimated treatment effect. Thus, we typically include a full set of cell fixed effects. For specifications with continuous covariates, adding cell fixed effects alters the point estimate of the treatment effect – although typically by a modest amount, as we also discuss in Appendix B.

In summary, our empirical approach recognizes that buyout effects can vary with firm characteristics and economic conditions and by industry, deal type, and time period. We will also directly explore multiple sources of heterogeneity in buyout effects. However, there is surely more heterogeneity in treatment effects than we can estimate with precision. Faced with this heterogeneity, our goal is to obtain a consistent estimate for the activity-weighted mean treatment effect on treated units (i.e., buyout targets) under the CMI assumption and SUTVA.

Our estimated treatment effects have a causal interpretation if these assumptions hold. Even if CMI fails, our results provide useful evidence for formulating and evaluating theoretical models of PE behavior and its effects. The SUTVA could fail if treatment effects on targets alter product demand and factor supply conditions facing controls, or if they exert competitive pressures that drive higher productivity at controls. Since targets typically account for modest activity levels relative to controls, these effects are likely to be small in our setting. Another possibility is that buyout targets implement superior technologies or business strategies that controls then emulate. The scope for such imitation effects seems modest within a two-year post-buyout time frame.

IV. Results

A. The Average Economic Effects of Private Equity Buyouts

Table 2 reports the estimated γ coefficients and associated standard errors for specifications of the form (1). Coefficients are approximate percentage point changes from the buyout year *t* to *t*+2. The "All Buyouts" column covers firms that underwent buyouts from 1980 to 2011 and matched control firms in the same cells. There are about 3,600 targets and 6.4 million total firm-level observations in the regressions that consider employment growth and reallocation outcomes. The underlying number of establishments is much larger, because many target firms (and the corresponding control firms) have multiple facilities. We have fewer usable observations for labor productivity, as discussed below.

According to the "All Buyouts" column in Panel A, employment at target firms shrinks (on average) by a statistically insignificant 1.8 percentage points relative to control firms in the first two years after the buyout. Employment shrinks by a statistically insignificant 2.3 percentage points relative to controls when omitting post-buyout acquisitions and divestitures. These "bottom line" effects of PE buyouts on target firm employment are broadly similar to the findings in Davis et al. (2014): -0.9 percentage points overall, and -3.7 points for organic growth. Appendix Table D.3 provides more detail on how target-control employment growth outcomes differ by margin of adjustment. To summarize the largest differences, target firms are more aggressive than control firms in shutting establishments from *t* to *t+2* and in acquiring new establishments from *t* to *t+2.*

While the net employment effects of PE buyouts attract much interest, buyouts have larger effects on the pace of job reallocation. Overall job reallocation for a firm is the sum of its gross job gains due to new, expanding, and acquired establishments and its gross job losses due to exiting, shrinking, and divested establishments. Dividing overall job reallocation by base employment yields the job reallocation rate. A firm's *excess* reallocation rate is the difference between its job reallocation rate and the absolute value of its net employment growth rate. If a firm changes employment in the same direction at all of its establishments, its excess reallocation is zero. To the extent that a firm expands employment at some units and contracts employment at others, it has positive excess reallocation. If a firm adds jobs at some establishments and cuts an equal number at other establishments, its excess reallocation equals its overall job reallocation.[13]

According to Panel B in Table 2, the job reallocation rate is higher by 8.3 percentage points (of base employment) at targets for organic employment changes over two years after the buyout and by 14.1 points when including acquisitions and divestitures, both highly significant. These results confirm that PE buyouts accelerate the pace of reallocation at target firms, more so when including acquisitions and divestitures.

Turning to Panel C, excess reallocation is 9.2 percentage points higher at target firms for all changes, but insignificantly different for organic changes. The implication is that the faster pace of job reallocation induced by buyouts mainly involves greater reallocation of jobs across firms

[13] The excess reallocation concept is often used in the literature on gross job flows to analyze job reallocation within and across regions, industries and other categories. Examples include Dunne, Roberts, and Samuelson (1989) and Davis and Haltiwanger (1992, 1999). Here, we apply the same concept to the reallocation of jobs across establishments within the firm.

rather than within target firms. In other words, PE buyouts lead to net job losses at some target firms (relative to control firms) and net job gains at other target firms. Most of the between-firm reallocation of jobs (about 65%) is due to excess reallocation.

Panel D in Table 2 provides evidence on how PE buyouts affect firm-level labor productivity, measured as the natural log of revenue per worker.[14] Relative to Panels A-C, we lose observations for three reasons in Panel D. First, we cannot calculate productivity changes for firms that close all establishments by *t+2*. When we drop a target that dies in this sense, we also drop controls in the cell associated with that target. If we drop a cell with many controls, we lose many observations. Second, even for targets that survive, some control firms in the cell do not – leading to the loss of additional observations. Third, we drop observations for which firm-level productivity is more than 200 log points from its mean in the same NAICS6-year cell in either the buyout year *t* or in *t+2.* We drop these outliers to guard against large productivity deviations due to errors in the revenue data, errors in linking revenue and employment at the firm level, and errors in the assignment of firms to industries. See Haltiwanger et al. (2017) for a discussion of how these errors can arise in the RE-LBD and why revenue data are unavailable for many firms.

To address the potential selection bias introduced by missing productivity observations, we construct inverse propensity score weights as in Haltiwanger et al. (2017) and similarly to Davis et al. (2014). These weights ensure that the re-weighted RE-LBD is representative of the LBD universe with respect to the size, age, employment growth rate, industry sector, and multi-unit status of firms. We apply these weights in our regression analysis of how PE buyouts affect productivity growth.

[14] RE-LBD labor productivity data are available in real terms using deflators at the NAICS2 and NAICS3 levels. These deflators have no effect on our estimates, which reflect productivity changes at targets relative to contemporaneous changes at controls within the same NAICS3.

Turning to the results, labor productivity rises by 7.6 percentage points at targets relative to controls from buyout year *t* to *t+2* (albeit not estimated precisely). In undisclosed results, we find the largest post-buyout productivity gains at older and larger targets. Davis et al. (2014) estimate that PE buyouts raise total factor productivity by about 2.1 percentage points for target firms in the manufacturing sector. Here, we find a considerably larger effect of PE buyouts on labor productivity when looking across all industry sectors. To help understand this result, Panel C of Appendix Table D.3 decomposes this productivity gain into two pieces: one due to larger workforce reductions at targets, and the other due to greater revenue growth at targets. More than 80 percent of the estimated productivity gain reflects greater revenue growth at targets. We cannot decompose labor productivity gains into markup changes and physical productivity changes, given our data. However, Farcassi, Previtero, and Sheen (2018) show that the rapid post-buyout sales growth of retail and consumer products firms reflects the launch of new products and geographic expansion, not markup hikes.

B. How the Effects Differ by Buyout Type

Table 2 also reports estimated effects by type of buyout. According to Panel A, target employment shrinks by 11.9% (relative to controls) after public-to-private buyouts and by 0.2% after divisional buyouts. It rises by 14.9% after private-to-private buyouts and by 10.6% after secondary buyouts. Isolating organic changes, target employment shrinks by 10.6% after public-to-private buyouts and by 5.3% after divisional buyouts; it rises by 4.7% after private-to-private buyouts and by 6.6% after secondary buyouts. Almost all of these estimates are statistically significant at the 1% or 5% level. Thus, we find strong evidence of buyout-induced employment effects that differ greatly by type of buyout. An F-test rejects the null hypothesis of no differences across buyout types in the estimated effects on target employment growth.[15]

Appendix Table D.4 provides more detail. For example, private-to-private and secondary buyouts create new job positions in new facilities at a faster clip than control firms – to the tune of 2.3% and 4.2% of base employment, respectively. In contrast, job creation at new facilities falls by 0.4% at targets relative to controls in public-to-private deals. Gross job destruction in the wake of divisional targets exceeds that of controls by 10% of base employment, mostly due to jobs lost in facility closures. Similar patterns hold for public-to-private buyouts. Again, the key message is that employment effects of PE buyouts vary greatly by type of buyout.

Perhaps this heterogeneity should not surprise. As discussed above, public-to-private deals (and many divisional deals, which are typically carved out of public firms) involve targets with highly dispersed ownership. These firms may suffer from poor corporate governance before the buyout and face an intense need for cost cutting. Meanwhile, buyouts of privately held firms may more often be motivated by a desire to professionalize management or improve access to financing.

Turning to Panels B and C in Table 2, buyouts bring more reallocation, but the effect again differs greatly by deal type. In divisional deals, overall (excess) target job reallocation rises by 19.8% (16.4%) of base employment relative to controls, 14.4% (11.0%) when netting out the role of acquisitions and divestments. In private-to-private deals, acquisitions and divestments mostly drive the post-buyout reallocation uptick at targets relative to controls. Buyouts bring higher job reallocation at targets in public-to-private deals but less than half of this is due to excess job reallocation. This evidence implies – in line with our earlier discussion – that the extra job

[15] To implement the tests (and those in Table 8), we replace the γPE_{it} in regression specification (2) with a set of four dummy variable terms, one for each buyout type. We then test for equality of the coefficients on these four dummy variables.

reallocation reflects a downsizing of some target firms (relative to controls) and an upsizing of others. By way of contrast, extra excess reallocation at target firms accounts for two-thirds of the extra buyout-induced job reallocation in the other deal types.

Turning to productivity effects, we again find large differences by type of buyout, collectively significant at the 10% confidence level. Target firms in private-to-private deals experience a 10.8 percent productivity gain relative to controls. Targets in public-to-private deals enjoy even larger gains, although the standard errors are large. Estimated productivity effects are smaller for other buyouts and statistically insignificant.

Taken together, the results in Table 2 on differences by buyout type suggest that there is little basis for treating private-to-private, public-to-private, divisional, and secondary buyouts as homogeneous in their effects on jobs, reallocation, and productivity. But they are broadly consistent with the limited evidence in previous research on the real-side effects of PE buyouts. According to our evidence, *private-to-private* deals exhibit high post-buyout employment growth (largely but not entirely via acquisitions) and large productivity gains. Meanwhile, *public-to-private* deals exhibit large job losses, often through facility closures, and large productivity gains. *Divisional* buyouts involve small employment losses but large reallocation effects. Finally, *secondary* deals exhibit high target employment growth, largely organic, high reallocation, and few discernible effects otherwise.[16]

C. How Buyout Effects Vary with Market Conditions at Close

We now investigate how the economic effects of PE buyouts vary with market conditions when the deal closes. To do so, we estimate richer regression specifications of the form,

[16] Secondary deals are somewhat of a grab bag, with PE groups on both sides of the transaction. That makes it hard to interpret the effects of secondary buyouts. Hence, and in the interest of brevity, we do not report breakouts for secondary deals in the rest of the paper.

$$Y_{i,t+2} = \alpha + \sum_c D_{cit}\theta_c + \lambda_1 LEST_{it} + \lambda_2 LFIRM_{it} + \gamma_{t+2} PE_{it} + \beta\, PE_{it} * MktCondition_t + \varepsilon_{it}, \quad (3)$$

where the new term, $\beta\, PE_{it} * MktCondition_t$, captures the interaction between buyout status and market conditions. When using intra-year variation in market conditions, we also include the $MktCondition_t$ main effect. When using only annual variation, we cannot separately identify the main effect, since our cell-level controls encompass annual time effects.

We consider two measures of market conditions at the buyout close: the log change in real GDP over the four quarters leading up to (and including) the closing quarter, and the spread between high-yield U.S. corporate bonds and the one-month U.S. LIBOR in the closing month.[17] Similar spread measures are widely used in the finance literature to characterize debt market conditions. Notably for our analysis, Axelson et al. (2013) show that this spread varies negatively with the extent of leverage in buyout transactions and with the EBITDA-multiple paid. It varies positively with the ultimate financial return on the buyout to PE investors.

The macroeconomics literature offers multiple interpretations for the relationship of spreads to real activity. Viewed through the lens of the q-theory of investment, low bond prices (a high spread) reflect low expected returns to capital (Philippon, 2009). Gilchrist and Zakrajšek (2012) advance a different view. They highlight a major role for movements in "the compensation demanded by investors – above and beyond expected losses – for bearing exposure to corporate credit risk." As they also show, movements in this excess bond premium mirror movements in the equity valuations of financial intermediaries and in their credit default swap premiums. This evidence is broadly in line with our interpretation: a high spread reflects tight credit conditions.

[17] GDP data are from the U.S. Bureau of the Economic Analysis, and the interest rate measures are from Datastream. For the bond rate, we use the yield to maturity on the Bank of America Merrill Lynch U.S. High Yield Index.

Turning to the results in Table 3, we find no evidence that the post-buyout performance of target firms (again, relative to controls) varies with GDP growth in the four quarters leading up to the close. The β coefficients on the interaction term are imprecisely estimated and statistically insignificant for each dependent variable. In contrast, higher credit spreads at close involve large, statistically significant effects on excess reallocation and productivity growth.[18] Raising the credit spread by one standard deviation corresponds to a post-buyout productivity gain of 26.3 percent for targets relative to controls and an increase in excess reallocation of 4.2 percent of base employment. These large effects come on top of the baseline effects reported in Table 2.

The positive association between excess reallocation rates and productivity gains as credit conditions vary suggests that PE buyouts achieve productivity improvements by shifting inputs toward better uses within target firms. In a similar spirit, Davis et al. (2014) find that buyouts lead to TFP gains at target firms in the manufacturing sector, mainly due to the reallocation of activity from less productive plants to more productive ones. Here, we find that high credit spreads at the time of the buyout lead to greater productivity gains and greater reallocation activity in target firms in the two years after the buyout. Both sets of results link buyout-induced productivity gains to an accelerated, purposefully directed reallocation of activity within target firms.

Our credit spread results in Table 3 also suggest that PE groups have multiple tools for earning returns on their investments in portfolio firms. When credit is cheap and readily available, it may be more attractive to rely on financial engineering tools to generate returns, e.g., by issuing

[18] From the first quarter of 1980 Q1 to the fourth quarter of 2013, the correlation between (a) the credit spread at quarter's end and (b) real GDP growth from four quarters previous to the quarter in question is a modest -0.288. In unreported results, we tried two other measures of external financial conditions: the credit spread measure of Gilchrist and Zakrajšek (2012), and equity market valuations, measured as the ratio of end-of month equity prices to the trailing twelve-month earnings S&P 500 firms. These alternative measures yielded broadly similar, but somewhat noisier, results.

new debt to fund additional dividend payments to equity holders. When credit is costly and tight, financial engineering is less feasible and PE groups may generate returns through operational improvements that raise productivity in portfolio firms.[19]

D. Robustness Checks on Macroeconomic Conditions Analysis

We now address two potential concerns about the forgoing analysis of market conditions. First, perhaps the results reflect our particular metrics for market conditions. Second, the results might be largely driven by the many buyouts undertaken in the run-up to the global financial crisis.

The final column of Table 3 addresses the first concern. Rather than looking at how buyout effects vary with a continuous measure of market conditions, we now take a simpler approach. Specifically, we interact the buyout indicator with a dummy for whether the deal was executed during a recession or (b) the U.S. economy entered a recession in the two years after deal execution. Recession years are those for which at least half the months were part of NBER-designated recessions (i.e., 1981-82, 1990, 2001, and 2008-09).

The relative employment responsiveness of target firms to recessions is, if anything, stronger than when using continuous metrics. Relative employment growth at targets is stronger for deals executed during a recession. The reallocation and productivity regressions, however, show smaller coefficients for the interaction variables and less statistical significance than obtained with continuous measures of credit spreads. Nevertheless, the regression indicates that many of our results continue to hold when using the recession indicator of market conditions rather than the continuous measures considered above.

[19] In Appendix D, we also examine how buyout effects vary with the evolution of market conditions after the close of the transaction.

Turning to the second concern, Figure 1 shows a huge surge in buyout activity in the quarters leading up to the GFC. Recall that the economy appeared strong in 2007 but then tumbled into a deep recession in 2008-09. To assess whether the runup in buyouts before the GFC drives our results, we re-estimated our models after dropping buyouts done in 2007. Table 4 repeats models considered in Table 3, but now omitting all targets and controls for buyouts in 2007. By and large, the results are broadly similar to before, but with less precise estimates. We still find that deals done amidst higher credit spreads show significantly greater productivity growth at targets. The coefficients remain roughly the same size, but the responsiveness of target employment growth to economic conditions is much weaker than before.

E. Market Conditions, or Deal Mix Changes over Time?

Recall that public-to-private buyout volume is more pro-cyclical than that of other buyout types, especially private-to-private deals (Figure 1). So, perhaps the greater job losses at target firms in public-to-private buyouts (Table 2) reflect a greater pro-cyclicality in their deal volume.

The sensitivity of our estimated buyout effects to market conditions could also reflect changes in the mix of PE sponsor characteristics over time. Gompers and Lerner (1999) and Kaplan and Schoar (2005), among others, show that the number of first-time funds is especially pro-cyclical. If the targets of young buyout groups have more adverse employment outcomes and are concentrated around market peaks, it could drive a cyclical pattern in our estimated effects of PE buyouts. More generally, a changing mix of active PE funds could drive time variation in the estimated PE effects. If true, that would be an interesting finding, but it would put our earlier results on the sensitivity of buyout effects to market conditions in a somewhat different light.

To explore these matters, we first undertook another large data collection effort to identify and characterize the PE sponsors of our nearly ten thousand buyouts. For 89% of the buyouts, we found information about the PE group in Preqin, Refinitiv, and other public sources. We assigned each PE group an identifier that follows the organization through spinouts and name changes, as explained in Appendix C. We also gathered information about the organization type of the PE group, the number and dollar volume of its previous funds raised, and the group's historical track record (when available). We then merged these new data with our other data.

To analyze whether changes over time in the mix of buyout types and PE sponsor characteristics explain our results on how buyout effects vary with market conditions, we adopt a simple approach that lends itself to a useful decomposition, as we will explain. Specifically, for each buyout we create a "cell-adjusted" performance measure equal to the change from buyout year t to $t+2$ for the target minus the contemporaneous mean change for controls in the same cell (defined as before). We now dispense with controls for pre-buyout growth. We then sort observations by high and low values of a market conditions variable. Then we regress the cell-adjusted outcomes for buyout targets on a constant and the market conditions indicator, weighting buyout observations in the same way as before.

Panel A of Table 5 confirms that this simpler approach yields results very similar to the earlier ones on how target outcomes vary with market conditions. Specifically, relative target employment growth and reallocation rates increase when post-buyout GDP growth is high, and relative target productivity gains are greater for deals that close when credit spreads are high.

Next, we implement a Blinder-Oaxaca decomposition of the difference in relative target performance between high and low values of the market conditions variable. This type of decomposition has a long history in labor economics (Jann, 2008), but it can be readily applied to decompose the estimated difference between any two groups. In our application, the two groups are buyouts associated with high and low values, respectively, of a market conditions variable. For each subsample (i.e., the high-value and low-value observations), we regress the cell-adjusted buyout performance measure on indicator variables for buyout types and four measures of PE sponsor characteristics: the number of funds raised by the sponsor in the five years prior to its buyout of the target firm; the dollar amount it raised in the five years prior to the deal, divided by total U.S. PE fundraising in the same period; a dummy for whether the sponsor was independent, as opposed to a bank or corporate affiliate; and the number of buyouts undertaken by the sponsor in a five-year period around the deal in question. These measures quantify PE sponsor attributes related to the scale of its buyout activity, its fundraising success (a proxy for past performance), and its organization type.

The subsample regressions provide the ingredients of a Blinder-Oaxaca decomposition for the high-low difference in Panel A. We can express the decomposition as

$$(\bar{X}_H - \bar{X}_L)'\hat{\beta}_H + \bar{X}_H'(\hat{\beta}_H - \hat{\beta}_L) + (\bar{X}_H - \bar{X}_L)'(\hat{\beta}_L - \hat{\beta}_H), \qquad (4)$$

where $\bar{X}_H$ and $\bar{X}_L$ are vectors whose elements are the mean values of the explanatory variables in the "high" and "low" regression samples, respectively; and the $\hat{\beta}_H$ and $\hat{\beta}_L$ are the corresponding least squares regression coefficient vectors. The first term of (4) quantifies the contribution of changes in the mix of buyout types and PE sponsor characteristics to the high-low difference, the second term quantifies the contribution of market conditions, and the third term captures the interaction of between-group differences in the $\bar{X}$ and $\bar{\beta}$ vectors.

Panel B reports the decomposition results. The values in row (2) are statistically significant and roughly the same size as the corresponding high-low differences in Panel A. That is, the between-sample differences in the estimated coefficients largely account for the high-low differences in panel A. Moreover, for the employment growth rate and excess reallocation rate, the other two terms in the decomposition are small and statistically insignificant. Thus, for employment growth and excess reallocation, Table 5 confirms that buyout effects vary strongly with market conditions, *and* there is little role for temporal variation in the mix of buyout types and PE sponsor characteristics.

The message for buyout effects on productivity is murkier in two respects: the individual terms on the right side of (4) are imprecisely estimated because of the small sample, and the first and third terms are large and nearly offsetting. Our earlier claim that buyouts executed amidst tight credit conditions yield stronger productivity gains at targets still holds. However, we cannot say with any confidence whether, and to what extent, this result reflects time variation in PE sponsor characteristics or types of buyouts.

F. Do Buyout Effects Differ across Private Equity Groups?

Thus far, we have provided evidence that the real-side effects of PE buyouts differ with market conditions post buyout, with market conditions at close, by type of buyout, and with interactions between market conditions and buyout type. Another potential driver of heterogeneity in buyout effects are systematic differences among the PE sponsors themselves. PE groups are characterized by management stability and distinct investment styles. In addition, persistence in financial performance has often been seen as a distinguishing feature of private equity groups, in contrast to hedge funds and mutual funds. (See Carhart (1997) and Brown, Goetzmann, and Ibbotson (1999) on hedge funds and mutual funds.) Studies that document persistence in the returns of PE groups include Kaplan and Schoar (2005), Ewens and Rhodes-Kropf (2015), Braun, Jenkinson and Stoff (2017), and Harris et al. (2023).

We now investigate whether there are persistent differences across PE groups in the employment effects of their buyouts.[20] To do so, Table 6 presents a series of regressions inspired by Kaplan and Schoar (2005) and Harris et al. (2023, and its 2014 predecessor). They examined the persistence of financial performance at the fund level. Because it is hard for us to associate buyouts with particular funds, we instead aggregate all transactions associated with a given PE group in each of seven non-overlapping periods (1980-84, 1985-89, …, 2005-09, and 2010-11). We again use the cell-adjusted employment growth rate over the two years post buyout. For each period and PE group, we then compute the mean value of the cell-adjusted growth rates. We regress this period-by-PE group mean on its own lagged value (for the previous five-year period), dropping groups with buyouts in only one five-year period. We include time period dummies as well.

While we see relatively few effects with aggregate employment growth, persistence is much stronger, and statistically significant, for organic employment growth. The coefficient of 0.12 on lagged organic growth in regressions (6) through (8) compares to that of 0.17 in the public-market-equivalent buyout analysis of Kaplan-Schoar (2005; 8th regression in Table VII). This pattern supports the view that PE groups differ in their approach to operational improvements at target firms, leading to systematic differences in buyout effects on organic employment growth, while target-specific considerations influence decisions to acquire and divest. These results are robust to adding controls for the PE sponsor's volume of transactions in the five-year period and the change in its volume from the previous five-year period, as seen in columns (7) and (8).

The remaining columns contain two additional results. First, when we add an interaction between the date (expressed here as years since January 1960) and lagged employment growth

[20] While it would be interesting to analyze persistence in productivity effects as well, we have too few buyouts with productivity data for an informative analysis.

(again at the group level), no evidence emerges of falling persistence in the group-specific growth effects. In contrast, the work of Harris et al. (2023) suggests that persistence in the financial performance of buyout funds dropped sharply after 2000. While PE groups may no longer show persistent differences in their ability to monetize their distinct approaches, our results say they continue to show persistent differences in how they affect target firms. Second, when we add fixed effects for PE groups in columns (5) and (10), we obtain results similar to those in Table 9 of Harris et al. (2014): the coefficient on lagged performance turns sharply negative, which says there is regression to the (group-specific) mean in the employment growth rates of buyout targets.

G. How Does Scaling at the Group Level Affect Employment in Portfolio Firms?

Our penultimate analysis investigates how scaling in buyout activity at the group level affects employment outcomes at targets. Previous research finds a detrimental impact of rising fund size on fund manager returns – see, for example, Fung et al. (2008) for hedge funds and Chen et al. (2004) for mutual funds. Similarly, the works of Lopez-de-Silanes, Phalippou, and Gottschalg (2015) and Rossi (2019) suggest a negative relationship between upscaling in buyout activity and the financial performance of PE groups.

Motivated by these earlier works, we investigate how the scaling of buyout activity by PE groups relates to the employment growth of their portfolio companies. To do so, we expand specification (2) to include variables that directly measure aspects of scaling or proxy for it, while also adding controls for buyout type. We consider four measures of scaling: funds raised by the PE group from $t-4$ to t for buyouts in t, divided by total buyout funds raised in the same period; financial performance of the group's last two buyout funds raised in the window from $t-12$ to $t-5$, calculated as returns as a multiple of invested capital (MoIC) minus the benchmark MoIC for the same period; number of buyouts executed by the PE group in the five-year period (1980-

84, 1985-89, and so forth) that contains the buyout year; and the change in number of buyouts from the previous to the current five-year period. See Appendix C for details.

As shown in Table 7, upscaling in buyout activity at the group level involves lower post-buyout employment growth at target firms (again, relative to controls). The estimated upscaling effects are statistically significant at the 10 percent level for all scaling measures except for the change in the number of buyout deals. The estimated magnitudes differ a good deal. For example, a unit standard deviation increase in Adjusted Financial Performance involves a 4.4 percentage point fall in the relative growth of organic employment at target firms, whereas a unit standard deviation increase in Funds Raised in Prior Five Years (Number of Buyouts in Current Five-Year Period) involves a decrease of only 0.8 (0.1) percentage points.[21]

In summary, upscaling in PE buyout groups is associated with a more negative employment impact on target firms, even after controlling for buyout type, pre-buyout growth history, and cell-level fixed effects. Of course, the scaling of PE groups is not exogenous (Rossi, 2019). In particular, past performance has a profound influence on ability to raise new funds (Chung et al., 2012). In this light, the Table 7 results suggest that past success encourages a PE group to scale up its buyout activity, diluting the attention that key group personnel devote to individual portfolio companies. In turn, this dilution of attention leads to slower growth at portfolio companies.

H. Impact over a Longer Horizon

Thus far, our analysis considers the impact of buyouts on employment growth, reallocation and productivity growth over a two-year horizon. We do so because tracking firms over long horizons can be difficult in the face of complex changes in ownership and firm structure, which

[21] The larger implied effect for the Adjusted Financial Performance measure arises, at least in part, because its values are more highly dispersed across PE groups, as reflected in its relatively large coefficient of variation (ratio of standard deviation to mean).

often happens in firms that undergo buyouts. In this subsection, we consider the robustness of our results to tracking firms over a five-year horizon.[22]

Table 8 presents the analog of Table 2 but now using firms that we can track over a five-year horizon post buyout. The smaller sample sizes in Table 8 reflect a loss of both targets and controls as we turn to five-year horizons, as detailed in Appendix A. Despite smaller samples, the core results hold up, for the most part, at the longer horizon. For “All Buyouts” at a five-year horizon, we find no statistically significant impact on employment growth, a large increase in job reallocation within firms, and an increase in productivity growth. The job reallocation and productivity effects are much larger over five years as compared to two years.

Turning to results by buyout type, the results on employment growth, job reallocation, and productivity growth also are broadly similar at two-year and five-year horizons. Again, the magnitudes of the reallocation and productivity effects tend to be larger at the longer horizon. Interestingly, we do not find statistically employment changes at targets (relative to controls) for public-to-private buyouts at the five-year horizon – in contrast to the two-year horizon in Table 2. As we show in Appendix D, the continuing establishments of target firms in public-to-private buyouts shrink less, acquisitions add more, and deaths play a smaller role at a five-year horizon than at a two-year horizon. For private-to-private and secondary deals, buyouts have a large positive net growth differential relative to controls in Table 8, as in Table 2.

Even though results are similar at the two- and five-year horizons, a possible concern is greater selection bias in the five-year results. To address that concern, we also investigated buyout effects on “pseudo-firms” over a five-year horizon. Specifically, we considered all establishments owned

[22] To use our original sample of buyouts and controls over a five-year horizon we needed to exclude 2009-2011 buyouts and controls for the employment outcome analysis and 2010-11 buyouts and controls for the productivity analysis. For further discussion, see Appendix A.

by target and control firms as of the buyout year (for the Table 2 sample) and tracked them over the next five years regardless of ownership. By construction, there are no establishment births at pseudo-firms, and there are no acquisition and divestitures.

As reported in Appendix Tables D.7 and D.8, pseudo-firm buyouts involve greater employment contraction at targets (relative to controls) than in Table 8. For example, the growth rate differential of pseudo-firm buyouts over a five-year horizon is -8.3 percent, as compared to 2.95 percent in Table 8. Recall that the latter figure includes the role of acquisitions, divestitures, and establishment births that are omitted in the pseudo-firm analysis. Removing the contribution of establishment births in our main analysis with actual firms yields a growth differential of -6.5 percent. Hence, it appears that the main reason for the large negative growth effects of buyouts in the pseudo-firm analysis is the omission of post-buyout establishment births.

We also considered the role of macroeconomic conditions in the long-horizon analysis, with focus on conditions that prevailed when the deal closed. Table 9 provides results analogous to Table 3, but now using the five-year horizon. Results are again broadly similar. Comparing Table 3 and Table 9, we again find that higher credit spreads at close involve more job reallocation and greater productivity growth at targets (relative to controls). We also find similar results at the five-year and two-year horizons when using the NBER recession indicator rather than credit spreads, but estimated recession effects are less precise at a five-year horizon.

V. Concluding Remarks

In his presidential address to the American Finance Association, Zingales (2015) makes the case that we "cannot argue deductively that all finance is good [or bad]. To separate the wheat from the chaff, we need to identify the rent-seeking components of finance, i.e., those activities that while profitable from an individual point of view are not so from a societal point of view."

Our study takes up that challenge for private equity buyouts, a major financial enterprise that critics see as dominated by rent-seeking activities with little in the way of societal benefits. We find that the real-side effects of buyouts on target firms vary greatly with market conditions, by type of buyout, across the private equity groups that sponsor buyouts, and with the sponsor's scale of buyout activity. To continue the metaphor, separating wheat from chaff in private equity requires a fine-grained analysis.

This conclusion cast doubts on the efficacy of "one-size-fits-all" policy prescriptions for private equity. Buyouts are associated with large productivity gains in many but not all circumstances. They are associated with large job losses in some circumstances and large job gains in others. This mixture of consequences presents serious challenges for policy design, particularly in an era of slow productivity growth (which ultimately drives living standards) and concerns about economic inequality.[23]

There is a keen need to better understand the link between PE buyouts and productivity growth. Our evidence that buyouts executed amidst easy credit conditions bring smaller productivity gains suggests that PE groups exercise some latitude in how they create value for their investors. When credit is cheap and easy, PE groups may select buyouts – or structure them – to deliver private returns via financial engineering rather than operating improvements. Many PE groups were founded and seeded by investment bankers that historically relied on financial engineering to create private value, employing strategies such as repeatedly re-leveraging firms and dividending out excess cash (Gompers, Kaplan, and Mukharlyamov, 2016). In this light, it is unsurprising if PE groups de-emphasize operating improvements when leverage and dividends

[23] Of course, buyouts may have social consequences well beyond productivity and jobs, as highlighted in papers such as Eaton, Howell, and Yannelis (2020) and Liu (2023).

deliver high private returns. That said, our study provides evidence that buyout can, and often do, drive large productivity improvements in target firms. Policies that harness the power of PE buyouts to drive productivity gains can bring high social returns along with high private returns.

Our results reinforce some concerns about public-to-private deals, which account for 10% of PE buyouts from 1980 to 2013 and 31% of employment in target firms. In particular, public-to-private deals proliferate in advance of credit market tightening, and their targets exhibit poor productivity performance during aggregate downturns and when credit spreads widen.

Our study also points to important outstanding questions: Do public-to-private and divisional buyouts cause avoidable employment losses? Or were targets in dire need of restructuring and retrenchment to prevent worse outcomes at a later date? More broadly, are job losses after some buyouts necessary for post-buyout productivity gains and, if so, is the tradeoff an acceptable one? Does the pro-cyclical employment impact of buyouts reflect socially undesirable risk-taking by private equity or a preferred point on the risk-return frontier? In light of our evidence that buyout performance is sensitive to post-buyout growth and credit conditions, how should expectations and uncertainty about these conditions influence the strategy of PE groups? Resolving these questions is likely to require guidance from theory and new identification strategies. We hope our study helps pave the way for future research on these issues.

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Table 1. Summary Statistics for Private Equity Buyouts Matched to Census Micro Data

Panel A considers all matched targets in our 1980-2013 sample period. The first row in Panel B considers all matched targets in the 1980-2011 period, the second row excludes those matched using EIN numbers only, and the third row further restricts attention to "Two-year continuers," which includes target firms that (i) have the same firm identifier after two years along with positive employment and (ii) firms where the firm identifier no longer exists two years after the buyout and all establishments have shut down. Panel C considers the same 1980-2003 period as the analysis sample in Davis et al. (2014).

	Number of Matched Buyouts (Target Firms)	Number of Target Establishments in the Buyout Year	Employment at Target Establishments in the Buyout Year
A. All, 1980-2013	6,000	177,000	6,890,000
Private-to-private	2,600	42,000	1,800,000
Public-to-private	600	67,000	2,130,000
Divisional Sales	1,300	25,000	1,120,000
Secondary Sales	1,300	31,000	1,280,000
Unknown Type	200	12,000	560,000
B. All, 1980-2011	5,100	164,000	6,400,000
After excluding EIN cases	4,500	144,000	5,690,000
Two-year continuers,	3,600	127,000	4,970,000
Private-to-private	1,800	32,000	1,450,000
Public-to-private	500	58,000	1,800,000
Divisional Sales	400	11,000	470,000
Secondary Sales	800	20,000	920,000
Unknown Type	100	6,000	330,000
C. All, 1980-2003	1,800	69,000	2,990,000
After excluding EIN cases	1,500	59,000	2,630,000
Two-year continuers,	1,200	49,500	2,210,000
Private-to-private	600	21,000	900,000
Public-to-private	200	16,000	690,000
Divisional Sales	200	5,000	210,000
Secondary Sales	150	3,600	180,000
Unknown Type	80	3,900	230,000

Table 2. Estimated Buyout Effects on Employment, Job Reallocation, and Productivity

The sample contains matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status, and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are (approximate) percentage amounts from the buyout year *t* to *t+2*. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a separate weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Appendix B for an explanation of how we weight observations. Results for "All Margins" include the contribution of post-buyout acquisitions and divestitures, while results for "Organic Margins" exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. The final column presents the p-value from F-tests of the equality of the coefficients of the four buyout type variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary		p-Value,
Dependent Variable	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	F-test
A. Employment Growth, All Margins	1.78 [1.96]	0.30	14.93*** [2.80]	0.36	-11.87*** [2.87]	0.38	-0.22 [4.01]	0.30	10.63*** [2.52]	0.31	0.00
Organic Margins	-2.29 [1.51]	0.28	4.69*** [1.75]	0.32	-10.56*** [2.12]	0.40	-5.29* [3.06]	0.27	6.58*** [2.43]	0.30	0.00
B. Job Reallocation, All Margins	14.12*** [1.70]	0.41	10.84*** [3.68]	0.38	15.02*** [1.77]	0.50	19.81*** [3.68]	0.46	11.66*** [2.47]	0.36	0.39
Organic Margins	8.30*** [1.53]	0.40	0.63 [3.10]	0.41	10.38*** [1.49]	0.49	14.42*** [3.10]	0.42	7.21*** [2.46]	0.38	0.18
C. Excess Reallocation, All Margins	9.21*** [1.33]	0.40	5.27 [3.56]	0.41	6.48*** [1.31]	0.42	16.42*** [2.29]	0.43	8.03*** [2.08]	0.42	0.03
Organic Margins	3.39** [1.59]	0.36	-4.94 [4.40]	0.40	1.84 [1.67]	0.37	11.03*** [2.22]	0.36	3.58 [2.38]	0.39	0.01
Observations (000s)	6,425		3,920		426		2,255		588		
D. Labor Productivity	7.63 [5.13]	0.45	10.81*** [2.81]	0.44	28.44* [16.21]	0.51	-6.29 [5.56]	0.41	1.93 [5.85]	0.42	0.03
Observations (000s)	910		411		17		622		40		

Table 3. How Buyout Effects Vary with Macroeconomic and Credit Conditions at the Close

This table considers the same outcome measures, estimation method, and samples as Table 2, but we expand the regression specification to include market conditions at the buyout close and its interaction with the buyout indicator. We measure market conditions using the Credit Spread (in column 1) or GDP Growth variable (in column 2) defined in the text and consider them in separate regressions. For each outcome measure, the table entries report the estimated coefficient on the interaction variable, its standard error, and the coefficient multiplied by the standard deviation of the interaction variable, which ranges from 3.1 to 3.5 for the Credit Spread across samples and from 1.6 to 1.9 for GDP Growth. In column 3, we modify the regression specification (3) in the paper to instead include interactions between the buyout indicator and the presence of an NBER-defined recession during the year of the buyout close instead of interactions with market conditions using the Credit Spread or GDP Growth variable. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

		Interaction Variable		
Dependent Variable		Credit Spread	GDP Growth	Recession at Close
A. Employment Growth, All Margins	Coefficient	-0.14	0.89	7.09**
	[St. Error]	[0.89]	[1.47]	[3.23]
	Unit S.D. Effect	-0.49	1.56	
Organic Margins	Coefficient	-0.53	0.85	7.77***
	[St. Error]	[0.72]	[1.23]	[2.43]
	Unit S.D. Effect	-1.85	1.48	
B. Excess Reallocation, All Margins	Coefficient	1.20**	0.10	4.42
	[St. Error]	[0.49]	[0.74]	[2.89]
	Unit S.D. Effect	4.19	0.17	
C. Labor Productivity	Coefficient	6.59**	-3.41	-4.82
	[St. Error]	[3.00]	[4.94]	[7.59]
	Unit S.D. Effect	25.99	-7.2	

Table 4. Robustness Check: Excluding Deals Most Impacted by the Global Financial Crisis

This table considers the same outcome measures, estimation method, and samples as the first two columns in Table 3, but we modify the sample by excluding observations from 2007. We measure market conditions using the Credit Spread or GDP Growth variable defined in the text and consider them in separate regressions. For each outcome measure, the table entries report the estimated coefficient on the interaction variable and its standard error. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

Dependent Variable		Credit Spread	GDP Growth
A. Employment Growth, All Margins	Coefficient	-0.82	0.44
	[St. Error]	[0.98]	[1.47]
Organic Margins	Coefficient	-1.01	0.59
	[St. Error]	[0.77]	[1.23]
B. Excess Reallocation, All Margins	Coefficient	0.85	-0.48
	[St. Error]	[0.54]	[0.71]
C. Labor Productivity	Coefficient	5.70*	-8.13
	[St. Error]	[3.23]	[5.94]

Table 5. Blinder-Oaxaca Decompositions. The sample contains matched two-year continuers that underwent PE buyouts from 1980 to 2011 with sponsors that can be linked to fund-level commercial data sources. For each buyout, we compute the outcome measure from buyout year *t* to *t+2* for the target firm minus the corresponding average value for control firms in the same cell defined by the full cross product of firm-age category, firm-size category, industry, multi-unit status, and buyout year. Panel A presents coefficients from a regression of the outcome measure on a dummy for being above the median in (a) GDP growth in the two years after the buyout or (b) the credit spread at deal close. Panel B presents Blinder-Oaxaca decompositions of the difference between high and low values of the market conditions variable. The decompositions are based on separate regressions of the outcome measure on buyout type indicators and PE sponsor characteristics in subsamples defined by high and low values of the market conditions variable. See text for the full variable list. We use buyouts in the high-value subsample as the reference group when implementing the decomposition. *** p<0.01, ** p<0.05, * p<0.10.

	Employment Growth Rate	Excess Reallocation Rate	Productivity Growth
A. Simple regression fit to all buyouts in sample			
Coefficient on a Dummy for High GDP Growth Post-Buyout – the "High-Low Difference"	10.72***	3.3***	
	[1.57]	[0.75]	
Coefficient on a Dummy for High Credit Spread at Close – the "High-Low Difference"			13.85**
			[6.02]
B. Blinder-Oaxaca Decompositions			
(1) Contribution of changes in buyout types and PE sponsor characteristics to the High-Low difference in Panel A	-2.8	-0.7	12.99***
	[15.72]	[0.6]	[4.01]
(2) Contribution of changes in coefficients on buyout types and PE sponsor characteristics to the High-Low difference in Panel A	-9.33***	-3.25***	15.37*
	[1.77]	[0.79]	[9.00]
(3) Contribution of interactions effects to the High-Low difference in Panel A	1.41	0.61	-14.52
	[1.67]	[0.7]	[8.89]
Observation Counts	3,900	3,900	500

Table 6. The Persistence of Buyout Effects on Employment Growth at the Level of Private Equity Groups. The unit of observation for the regressions in this table is the average cell-adjusted target outcome for the buyouts of a given private equity sponsor in a particular five-year period (1980-84, 1985-89,…). Results for "Total Employment Growth" include the contribution of post-buyout acquisitions and divestitures, while results for "Organic Employment Growth" do not. See text for a full description of the explanatory variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

	Average Total Employment Growth Rate					**Average Organic Employment Growth Rate**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Lagged Average Employment Growth	0.0747 [0.0461]	0.0744 [0.0462]	0.0753 [0.0462]	-0.0989 [0.3545]	-0.3045*** [0.0747]					
Lagged Average Organic Growth						0.1229*** [0.0465]	0.1226*** [0.0466]	0.1226*** [0.0464]	0.0504 [0.3754]	-0.3174*** [0.0842]
# of Buyouts in Current 5-Year Period		-0.0387*** [0.0132]					-0.0361*** [0.0117]			
Change in # of Buyouts from Previous to Current 5-Year Period			-0.4234 [0.6443]					-1.292 [0.8403]		
Time trend ×Lagged Growth				0.3773 [0.8043]					0.1569 [0.8526]	
R^2	0.0128	0.0132	0.013	0.0131	0.7485	0.0283	0.0288	0.0307	0.0284	0.7725
Period fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
PE group fixed effects					YES					YES

Table 7. The Impact of Scaling at the PE Group Level on Employment Growth of Target Firms. The sample consists of matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status, and buyout year. Some firms serve as controls for more than one buyout type. The dependent variables are percentage changes from the buyout year *t* to *t+2* in "Total Employment Growth," which includes the contribution of post-buyout acquisitions and divestitures, and "Organic Employment Growth," which excludes them. Each reported effect is the coefficient estimate [standard error] on a buyout dummy interacted with the indicated group-level scaling measure. See text for a full description of the scaling measures. The mean and standard deviation values of the scaling measures pertain to the set of all buyouts from 1980 to 2011 for which we could identify the PE sponsor and collect information about the sponsor. This set is larger than the set of buyouts in the regression samples, which also involve matching to Census data on targets and controls. We estimate a separate weighted least-squares regression for each column and report Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

Scaling Measure for the Private Equity Group that Sponsored the Buyout	**Dependent Variable**							
	Total Employment Growth				Organic Employment Growth			
Funds raised in prior five years, normalized (Mean=0.0047, S.D.=0.0138)	-120.30** [60.76]				-56.78* [29.56]			
Adjusted financial performance of prior two funds (Mean=0.25, S.D.=1.12)		-7.11*** [1.75]				-3.96*** [0.93]		
Number of Buyouts in Current Five-Year Period (Mean=6.7, S.D.=8.5)			-0.01* [0.01]				-0.01* [0.01]	
Change in Number of Buyouts from Previous to Current Five-Year Period (Mean=0.43, S.D.=2.11)				-0.11 [0.26]				-0.37 [0.23]
R^2	0.392	0.262	0.389	0.389	0.353	0.243	0.353	0.352
Cell fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Deal type fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Controls for pre-buyout growth history of target firm	YES	YES	YES	YES	YES	YES	YES	YES

Table 8. Estimated Buyout Effects on Employment, Job Reallocation, and Productivity (Five-Year Horizon)

The sample for the employment (productivity) outcomes contains matched five-year continuers that underwent private equity buyouts from 1980 to 2008 (2009) and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status, and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are (approximate) percentage amounts from the buyout year *t* to *t+5*. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a separate weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Appendix B for an explanation of how we weight observations. Results for "All Margins" include the contribution of post-buyout acquisitions and divestitures, while results for "Organic Margins" exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. The final column presents the p-value from F-tests of the equality of the coefficients of the four buyout type variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1

	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary		p-Value,
Dependent Variable	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	Effect	R^2	F-test
A. Employment Growth, All Margins	2.95 [3.07]	0.29	11.81** [5.28]	0.33	-5.07 [7.43]	0.34	2.50 [4.99]	0.25	7.18** [3,34]	0.38	0.11
Organic Margins	-2.54 [2.08]	0.32	0.03 [3.13]	0.36	-5.00 [5.20]	0.39	-5.98* [3.06]	0.26	3.97 [2.53]	0.35	0.08
B. Job Reallocation, All Margins	14.53*** [2.31]	0.41	5.85 [6.07]	0.35	10.89*** [2.70]	0.45	26.13*** [3.73]	0.47	18.52*** [3.47]	0.36	0.01
Organic Margins	8.73*** [2.32]	0.39	-6.20 [4.91]	0.43	4.69 [3.93]	0.42	24.97*** [3.06]	0.51	7.02** [3.13]	0.35	0.00
C. Excess Reallocation, All Margins	13.34*** [2.21]	0.43	0.47 [3.83]	0.46	6.44** [2.51]	0.38	35.69*** [4.10]	0.54	4.39 [2.95]	0.41	0.00
Organic Margins	7.54*** [2.92	0.36	-11.58** [4.87]	0.38	0.25 [3.37]	0.36	34.52*** [5.57]	0.43	-7.11** [3.09]	0.44	0.00
Observations (000s)	3,787		2183		199		1593		326		
D. Labor Productivity	10.63* [6.00]	0.45	16.67** [6.81]	0.44	31.68* [17.38]	0.51	-4.39 [5.27]	0.31	7.01* [4.20]	0.43	0.01
Observations (000s)	870		395		11		601		40		

Table 9. How Buyout Effects Vary with Macroeconomic and Credit Conditions at the Close (Five-Year Horizon)

This table considers the same outcome measures, estimation method, and samples as Table 8 (five-year horizon), but we expand the regression specification to include market conditions at the buyout close and its interaction with the buyout indicator. We measure market conditions using the Credit Spread (in column 1) or GDP Growth variable (in column 2) defined in the text and consider them in separate regressions. For each outcome measure, the table entries report the estimated coefficient on the interaction variable, its standard error, and the coefficient multiplied by the standard deviation of the interaction variable, which ranges from 3.1 to 3.5 for the Credit Spread across samples and from 1.6 to 1.9 for GDP Growth. In column 3, we modify the regression specification (3) in the paper to instead include interactions between the buyout indicator and the presence of an NBER-defined recession during the year of the buyout close instead of interactions with market conditions using the Credit Spread or GDP Growth variable. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

		Interaction Variable		
Dependent Variable		Credit Spread	GDP Growth	Recession At Close
A. Employment Growth, All Margins	Coefficient	1.9	-2.1	9.03
	[St. Error]	[1.6]	[2.4]	[10.41]
	Unit S.D. Effect	4.4	-2.5	
Organic Margins	Coefficient	-0.57	-0.17	13.27**
	[St. Error]	[1.1]	[1.7]	[6.39]
	Unit S.D. Effect	-1.3	-0.2	
B. Excess Reallocation, All Margins	Coefficient	4.0***	-2.1	2.98
	[St. Error]	[1.1]	[1.8]	[4.95]
	Unit S.D. Effect	9.1	-2.5	
C. Labor Productivity	Coefficient	6.9**	-0.5	-7.92
	[St. Error]	[3.0]	[4.1]	[8.51]
	Unit S.D. Effect	24.5	-0.9	

Figure 1. Quarterly Buyout Counts by Type, 1980 to 2013

Each panel shows buyout closings for the indicated deal type in quarter t, overlaid with the contemporaneous credit spread and the log change in real GDP from t-4 to t. We exclude about 300 buyouts that we cannot classify as to deal type. See Section I.A for an explanation of how we construct our sample of 9,794 leveraged buyouts sponsored by private equity firms.