


The (Heterogeneous) 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. Postbuyout productivity gains at target firms are large on average and much larger yet for deals executed amid tight credit conditions. A postbuyout tightening of credit conditions or slowing of gross domestic product growth curtails employment growth and intrafirm 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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1. Introduction

Private equity (PE)-backed buyouts are among the most dramatic transformations of corporate structures seen in Western economies. They are also intensely controversial. Although some view buyouts as beneficial in resolving agency problems at target firms, critics point to the prevalence of agency problems within the 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 microdata 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 with 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 after buyout. Second, productivity gains are much larger for deals executed amid tight credit conditions or in recessions. Third, relative employment at targets rises 15%, on average, in firms previously under private ownership, whereas it falls 12% 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.¹ Fifth, rapid upscaling in deal flow at the PE group level brings weaker postbuyout 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. Although 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 et al. (2011), Cohn et al. (2014), Davis et al. (2014), Fracassi et al. (2022), and Cohn et al. (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 heterogeneous effects of PE buyouts. Section 3 discusses the creation of our database. Section 4 sets

forth our empirical approach, and Section 5 reports our results. Section 6 offers concluding remarks. Appendices provide more information about our data and empirical methods plus additional results.

2. 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.²

2.1. 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 trade-offs between publicly traded and privately held ownership. In particular, it has been widely understood since Jensen and Meckling (1976) that being publicly traded can bring greater vulnerability to agency problems. Because of 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 because of 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.³ 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.*

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

2.2. 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 et al. (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, because of 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.⁴ Thus, we hypothesize the following.

Hypothesis 3. *Deals executed amid easy credit conditions exhibit weaker postbuyout 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 et al. (2007) and Andonov et al. (2018) suggest that the sophistication of limited partners varies sharply, because of 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, Harris et al. 2023).⁵ Thus, we hypothesize the following.

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.*

3. Our Sample of Private Equity Buyouts

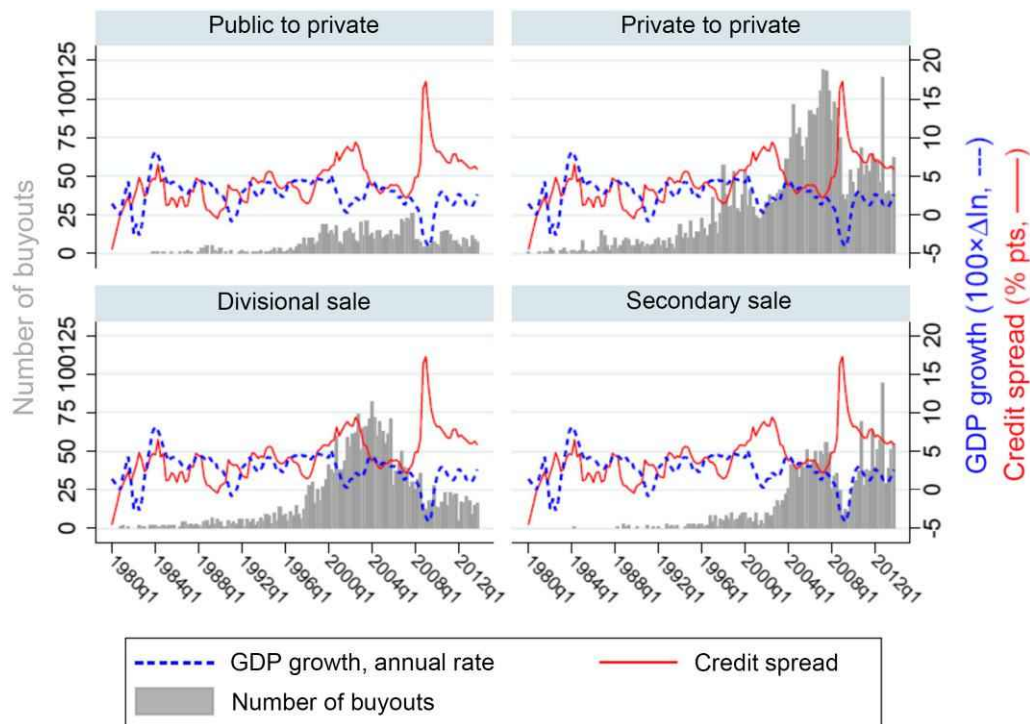
3.1. Identifying Private Equity Buyouts

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.

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 Online 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 Capital IQ and our reviews of other databases, press accounts, and securities filings (Strömberg 2008).

Figure 1 displays quarterly counts of our PE-sponsored buyouts for the four deal types.⁶ 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 2008–2009 recession and maintained a robust pace until the end of our sample period in 2013.

Table D.2 in Online Appendix D 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, whereas divisional deals are relatively prevalent in information technology and utilities. A Pearson chi-squared test rejects the hypothesis that the industry distribution of buyouts is independent of deal type. The distributions of PE buyouts by industry, firm size, and firm age also differ greatly from the corresponding distributions of private sector employment (Davis et al. 2014). Given these patterns, our econometric investigations below compare buyout targets to control firms within cells defined by the cross product of industry, firm size categories, firm age categories, multi-unit status, and buyout year.

Figure 1. (Color online) Quarterly Buyout Counts by Type, 1980 to 2013

Notes. 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 2.1 for an explanation of how we construct our sample of 9,794 leveraged buyouts sponsored by private equity firms.

3.2. Merging Private Equity Buyouts with Census Microdata

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.⁷ 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 or six-digit North American Industrial Classification (NAICS) codes, employer identification numbers (EINs), 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 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.⁸

Table 1 summarizes our sample of PE buyouts matched to Census microdata. 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 with 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

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

Buyout type	Number of matched buyouts (target firms)	Number of target establishments in the buyout year	Employment at target establishments in the buyout year
Panel A: 1980–2013			
All	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
Panel B: 1980–2011			
All	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
Panel C: 1980–2003			
All	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

Notes. 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).

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 microdata using only taxpayer EINs (and not other firm identifiers). As explained in Online 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 at least two years after buyout.⁹ 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 account for the rest.

4. Empirical Approach

4.1. 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 et al. 2022).

In constructing the cross product of control cells, we use about 90 industries, 10 firm size categories, six age categories, a dummy for multiunit firms, and 32 distinct buyout years from 1980 to 2011.¹⁰ Thus, the full cross product entails about 10,000 control cells per buyout

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

4.2. Regression Specifications

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

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

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 percentage change in its labor productivity or average annual compensation per employee. We have $PE_{it} = 1$ if firm i is a buyout target in t and zero 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 prebuyout 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 Table B.2 in Online Appendix B, 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.

4.3. 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, that is, 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.¹¹

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, whereas 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 nonparametric estimate for the weighted average treatment effect on the treated: $(2/3)(5 - 2.5) + (1/3)(5 - 5) = 1.67\%$. 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

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

To satisfy principles TS and SCT, set the weights to one 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 $\hat{\gamma}_{t+s} = 1.67\%$, the same as the simple nonparametric 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 et al. (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 Table B.1 in Online Appendix B, 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^2 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 Online 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. Because 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 postbuyout time frame.

5. Results

5.1. 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 postbuyout 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. Online Appendix D, 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 $t + 2$.

Although 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.¹²

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

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

	All buyouts		Private-to-private		Public-to-private		Divisional		Secondary		<i>p</i> -value, <i>F</i> -test
Dependent variable	Effect	<i>R</i> ²	Effect	<i>R</i> ²	Effect	<i>R</i> ²	Effect	<i>R</i> ²	Effect	<i>R</i> ²	
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		

Notes. 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, multiunit 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 prebuyout growth histories. See Online Appendix B for an explanation of how we weight observations. Results for all margins include the contribution of postbuyout acquisitions and divestitures, whereas 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 are in brackets.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

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 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.¹³ 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 Revenue Enhanced Longitudinal Business Database (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 reweighted RE-LBD is representative of the LBD universe with respect to the size, age, employment growth rate, industry sector, and multiunit status of firms. We apply these weights in our regression analysis of how PE buyouts affect productivity growth.

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 postbuyout 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 Table D.3 in Online Appendix D 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% 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, Fracassi et al. (2022) show that the rapid postbuyout sales growth of retail and consumer products firms reflects the launch of new products and geographic expansion, not markup hikes.

5.2. 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.¹⁴

Online 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 postbuyout 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 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% 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 postbuyout 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.¹⁵

5.3. How Buyout Effects Vary with Market Conditions at Close

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

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

where the new term, $\beta PE_{it} * MktCondition_t$, captures the

interaction between buyout status and market conditions. When using intrayear variation in market conditions, we also include the $MktCondition_t$ main effect. When using only annual variation, we cannot separately identify the main effect, because our cell-level controls encompass annual time effects.

We consider two measures of market conditions at the buyout close: the log change in real gross domestic product (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 (London interbank offered rate) in the closing month.¹⁶ 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 (earnings before interest, taxes, depreciation and amortization) 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.

Turning to the results in Table 3, we find no evidence that the postbuyout 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.¹⁷ Raising the credit spread by one standard deviation corresponds to a postbuyout productivity gain of 26.3% for targets relative to controls and an increase in excess reallocation of 4.2% 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 Total Factor Productivity (TFP) gains at target firms in the manufacturing sector, mainly because of 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.

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

		Interaction variable		
Dependent variable	Statistic	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	

Notes. 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 (first column) or GDP growth variable (second column) 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 the third column, 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 are in brackets.

*** $p < 0.01$; ** $p < 0.05$.

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, for example, by issuing new debt to fund additional dividend payments to equity holders. When credit is costly and tight, financial engineering is less feasible and PE groups may generate returns through operational improvements that raise productivity in portfolio firms.¹⁸

5.4. Robustness Checks on Macroeconomic Conditions Analysis

We now address two potential concerns about the foregoing 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 (GFC).

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 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 National Bureau of Economic Research (NBER)–designated recessions (i.e., 1981–1982, 1990, 2001, and 2008–2009).

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.

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 2008–2009. To assess whether the runup in buyouts before the GFC drives our results, we reestimated 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 amid higher credit spreads show significantly greater productivity growth at targets. The coefficients remain roughly the same size,

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

Dependent variable	Statistic	Credit spread	GDP growth
A. Employment growth			
All margins	Coefficient [St. error]	−0.82 [0.98]	0.44 [1.47]
Organic margins	Coefficient [St. error]	−1.01 [0.77]	0.59 [1.23]
B. Excess reallocation			
All margins	Coefficient [St. error]	0.85 [0.54]	−0.48 [0.71]
C. Labor productivity			
	Coefficient [St. error]	5.70* [3.23]	−8.13 [5.94]

Notes. 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 are in brackets.

* $p < 0.10$.

but the responsiveness of target employment growth to economic conditions is much weaker than before.

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

Recall that public-to-private buyout volume is more procyclical 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 procyclicality 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 procyclical. 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 10,000 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

Online 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 prebuyout 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 postbuyout 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 of Table 5. We can express the decomposition as

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

where \bar{X}_H and \bar{X}_L are vectors whose elements are the

Table 5. Blinder–Oaxaca Decompositions

	Employment growth rate	Excess reallocation rate	Productivity growth
Panel A: Simple regression fit to all buyouts in sample			
Coefficient on a dummy for high GDP growth after buyout—the high–low difference	10.72*** [1.57]	3.3*** [0.75]	
Coefficient on a dummy for high credit spread at close—the high–low difference			13.85** [6.02]
Panel 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 [15.72]	−0.7 [0.6]	12.99*** [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*** [1.77]	−3.25*** [0.79]	15.37* [9.00]
3. Contribution of interactions effects to the high–low difference in panel A	1.41 [1.67]	0.61 [0.7]	−14.52 [8.89]
Observation counts	3,900	3,900	500

Notes. 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, multiunit 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 the text for the full variable list. We use buyouts in the high-value subsample as the reference group when implementing the decomposition.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

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 of Table 5 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 amid 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.

5.6. 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 after 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 et al. (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), Braunet et al. (2017), and Harris et al. (2023).

We now investigate whether there are persistent differences across PE groups in the employment effects of their buyouts.¹⁹ To do so, Table 6 presents a series of regressions inspired by Kaplan and Schoar (2005) and Harris et al. (2023) (and its predecessor Harris et al. 2014). 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 nonoverlapping periods (1980–1984, 1985–1989, ..., 2005–2009, and 2010–2011). We again use the cell-adjusted employment growth rate over the two years after buyout. For each period and PE group, we then compute the mean value of the cell-

Table 6. The Persistence of Buyout Effects on Employment Growth at the Level of Private Equity Groups

Dependent variable	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 five-year period		−0.0387*** [0.0132]					−0.0361*** [0.0117]			
Change in # of buyouts from previous to current five-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

Notes. 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 (1980–1984, 1985–1989, etc.). Results for total employment growth include the contribution of postbuyout acquisitions and divestitures, whereas results for organic employment growth do not. See the text for a full description of the explanatory variables. Huber–White robust standard errors are in brackets.

*** $p < 0.01$.

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.

Although 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) of Table 6 compares to that of 0.17 in the public-market-equivalent buyout analysis of Kaplan and Schoar (2005, table 7, eighth regression). This pattern supports the view that PE groups differ in their approaches to operational improvements at target firms, leading to systematic differences in buyout effects on organic employment growth, whereas 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 (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. Although 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.

5.7. 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 et al. (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 (1980–1984, 1985–1989, 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 Online Appendix C for details.

As shown in Table 7, upscaling in buyout activity at the group level involves lower postbuyout employment growth at target firms (again, relative to controls). The estimated upscaling effects are statistically significant at the 10% 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.²⁰

In summary, upscaling in PE buyout groups is associated with a more negative employment impact on target firms, even after controlling for buyout type, prebuyout 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.

5.8. 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 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.²¹

Table 8 presents the analog of Table 2 but now using firms that we can track over a five-year horizon after 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 Online Appendix A. Despite smaller

Table 7. The Impact of Scaling at the PE Group Level on Employment Growth of Target Firms

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 prebuyout growth history of target firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. 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, multiunit 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 postbuyout 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 the text for a full description of the scaling measures. The mean and standard deviation (S.D.) 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.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

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 with 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 significant 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 Online 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 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 Tables D.7 and D.8 of Online Appendix D, 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% , as compared with 2.95% 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% . 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 Tables 3 and 9, we again find that higher credit spreads at close involve more job reallocation and greater productivity growth at targets (relative to

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

Dependent variable	All buyouts		Private-to-private		Public-to-private		Divisional		Secondary Effect	<i>p</i> -value <i>R</i> ²	<i>F</i> -test
	Effect	<i>R</i> ²	Effect	<i>R</i> ²	Effect	<i>R</i> ²	Effect	<i>R</i> ²			
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		2,183		199		1,593		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		

Notes. 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, multiunit 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 (and 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 prebuyout growth histories. See Online Appendix B for an explanation of how we weight observations. Results for all margins include the contribution of postbuyout 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 are in brackets.

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

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

Dependent variable	Statistic	Interaction variable		
		<i>Credit spread</i>	<i>GDP growth</i>	<i>Recession at close</i>
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	

Notes. 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 (first column) or GDP growth variable (second column) 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 (St.) error, and the coefficient multiplied by the standard deviation (S.D.) 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 the third column, we modify the regression specification (3) 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 are in brackets.

*** $p < 0.01$; ** $p < 0.05$.

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.

6. Concluding Remarks

In his presidential address to the American Finance Association, Zingales (2015, pp. 1343–1344) 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, that is, 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.²²

There is a keen need to better understand the link between PE buyouts and productivity growth. Our evidence that buyouts executed amid 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 leveraging firms and dividending out excess cash (Gompers et al. 2016). In this light, it is unsurprising if PE groups de-emphasize operating improvements when leverage and dividends deliver high private returns. That said, our study provides evidence that buyouts 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 postbuyout productivity gains, and, if so, is the trade-off an acceptable one? Does the procyclical 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 postbuyout 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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Endnotes

¹ Echoing persistent financial performance differences at the group level (e.g., Kaplan and Schoar 2005, 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.

² The main text focuses on productivity, employment and job reallocation effects. Online Appendix D presents estimated buyout effects on firm-level mean wages, which appear to be heavily influenced by buyout-related shifts in management compensation.

³ Among the papers hypothesizing advantages of public ownership such as lower equity costs and relaxed capital constraints are Zingales (1995), Pagano et al. (1998), Chemmanur and Fulghieri (1999), Maksimovic and Pichler (2001), and Brau and Fawcett (2006).

⁴ 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 touch in various ways on market cycles and private equity include Kaplan and Stein (1993), Ivashina and Kovner (2011), Hotchkiss et al. (2021), Harris et al. (2016), and Bernstein et al. (2019). But the impact of these forces on the real-side outcomes at target firms has received little attention to date.

⁵ 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).

⁶ Online Appendix Table D.1 reports average quarterly counts before, during, and after the financial crisis.

⁷ An establishment is a physical location where economic activity occurs. A firm is a legal entity that owns and operates one or more establishments.

⁸ 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.

⁹ Robustness analysis in Section 5.8 explores results over a five-year horizon.

¹⁰ 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, 1,000–2,499, 2,500–4,999, 5,000–9,999, and 10,000 or more employees. Our firm age categories are 0–5, 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.

¹¹ 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.

¹² 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 et al. (1989) and Davis and Haltiwanger (1992, 1999). Here, we apply the same concept to the reallocation of jobs across establishments within the firm.

¹³ 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.

¹⁴ To implement the tests (and those in Table 8), we replace the $\gamma_{PE_{it}}$ 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.

¹⁵ 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.

¹⁶ 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.

¹⁷ From the first quarter of 1980 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 12-month earnings of S&P 500 firms. These alternative measures yielded broadly similar, but somewhat noisier, results.

¹⁸ In Online Appendix D, we also examine how buyout effects vary with the evolution of market conditions after the close of the transaction.

¹⁹ Although it would be interesting to analyze persistence in productivity effects as well, we have too few buyouts with productivity data for an informative analysis.

²⁰ 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).

²¹ 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 2010–2011 buyouts and controls for the productivity analysis. For further discussion, see Online Appendix A.

²² Of course, buyouts may have social consequences well beyond productivity and jobs, as highlighted in papers such as Eaton et al. (2020) and Liu (2021).

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Internet Appendix for The (Heterogenous) Economic Effects of Private Equity Buyouts

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Internet Appendix A: Sample Construction and Matching

1. Overview

We combine information on private equity buyouts from CapitalIQ and other sources with firm-level and establishment-level data held by the U.S. Census Bureau.

Specifically, we undertook a two-part effort, following Strömberg (2008). The first part drew on the CapitalIQ database to create a base sample of PE-sponsored leveraged buyouts. We selected all M&A transactions in CapitalIQ classified as a “leveraged buyout,” “management buyout,” or “JV/LBO” (joint venture/leveraged buyout) that closed between January 1, 1980 and December 31, 2013. To this sample, we added all M&A transactions undertaken by a financial sponsor classified as investing in “buyouts.” We excluded management buyouts not sponsored by a PE firm and startup firms backed by venture capitalists. Although CapitalIQ has back-filled its database using various sources since starting its data service in 1999, its coverage remains incomplete in the early years of our sample. For this reason, the second part of our sample construction efforts relied on other databases,² the business press, and buyout lists for the 1980s compiled by other researchers.

The overlap between our initial sample of PE buyouts and lists of LBOs with a financial sponsor compiled by other researchers is high. For instance, 62 of the 77 buyouts in Kaplan’s (1989) hand-selected sample of LBOs completed between 1980 and 1986 are captured by our CapitalIQ sample, a coverage rate of 81%. We added these 15 missing buyouts to our sample, as we did for other PE buyouts identified using various lists and other sources beyond CapitalIQ.

In the course of our investigations, we discovered that CapitalIQ classifies certain buyout fund transactions as “private placements” rather than acquisitions. In most cases, these private

² These include Dealogic, Preqin, and Thomson Reuters.

placements involve minority stakes or follow-on investments and, hence, are unsuitable for inclusion in our sample. Still, the distinction between buyouts and private placements is not always clear. In addition, some transactions reported as LBO deals were actually venture capital investments, which are not the object of our study. We sought to err on the side of caution by excluding ambiguous transactions and, as a result, may miss some bona fide LBOs.

We also excluded acquisitions not yet completed by the end of 2013, acquisitions of non-control stakes (typically associated with growth and venture deals, not classic buyouts), purchases of firms with foreign headquarters, stakes in public companies that remained publicly traded (PIPES), and other misclassified transactions. We identified these transactions through the careful review of text fields in CapitalIQ records and our own detailed research using other commercial databases, securities filings, and media accounts.

We then match these buyout deals to target firms and their establishments in the Census Bureau's comprehensive Business Register (BR). Our basic approach is as follows. First, we use name and address information to match a particular deal to a specific unit in the BR. Because the matching algorithm relies partly on address information, this step identifies a specific establishment owned by the target firm, which is often but not always a headquarters facility. Second, we use the BR link between that establishment's and its parent firm's identifiers (IDs) to identify the target firm in the BR. In most cases, this method identifies the target firm in the BR and all of its establishments.

We describe our matching process in detail below. The process yields a mapping to one or more firms in the BR for about 7,600 of the 9,794 U.S. buyouts that we identified from CapitalIQ and other sources. Of these 7,600 buyouts, about 4,100 match BR identifiers for a single firm, while the other 3,500 map to identifiers for multiple firms. We resolved about 2,000 of these 3,500

cases to a unique match, leaving about 6,000 buyouts that we confidently match to a unique firm in the BR in the period from 1980 to 2013. The approximately 6,000 matched target firms acquired in PE buyouts from 1980 to 2013 operated about 177,000 establishments as of the buyout year and had nearly 7 million workers on their payrolls as of March in the buyout year.

The main reason we cannot confidently resolve the other 1,500 cases to a unique firm in the BR is because many targets undergo a complex reorganization during the buyout or shortly thereafter. The reorganization can involve the sale of multiple firm components to multiple parties, the emergence of multiple new firm IDs, and the introduction of a complex array of holding company structures. These cases present considerable matching challenges. Rather than include matches of dubious quality, we exclude them from our analysis.

Once matched to the BR, we can identify establishments owned by the target firm as of its buyout year. LBD longitudinal links let us compute employment changes for establishments and firms and track their entry, exit, and ownership changes. We supplement the LBD with firm-level revenue data drawn from the Census BR to obtain a revenue-enhanced version of the LBD (RE-LBD). The revenue data, available from 1996 to 2013, let us study the impact of PE buyouts on labor productivity, defined as real revenue per worker. About 20 percent of LBD firm-year observations cannot be matched to BR revenue data because firms report income under EINs that fall outside the set of EINs that Census considers part of that firm for employment purposes.

Treatment of Timing Matters

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

employment measure. We assign buyouts that close on or after October 1 in calendar year t to the LBD employment value in March of $t+1$. October is the natural cutoff because it lies midway between March-to-March employment changes in the LBD.³

Henceforth, our references to buyout activity in year t refer to deals that closed from October of calendar year $t-1$ through September of calendar year t . In particular, buyouts that closed in October, November or December of 2013 are shifted forward to 2014, beyond the time span covered by our LBD data. As a result, these matched targets are not part of our analysis.

Tracking Firms after the Buyout and Forming Our Analysis Sample

Of necessity, much of our analysis restricts attention to target firms that we can track after the buyout. While we can readily track establishments over time in the LBD, tracking firms is more challenging for two main reasons: the disappearance of firm IDs and irregularities in Census Bureau tracking of PE targets involved in certain divisional sales. We elaborate on these two reasons in turn.

Firm ID Disappearance. The disappearance of a firm ID in the LBD can occur for various reasons. One is the death of a firm and the closure of all of its establishments. Firm death in this sense presents no problem: we capture such events whether they involve target or control firms. A more difficult situation involves a target firm ID that vanishes in the first or second year after the buyout, even though some of its establishments (as of the buyout year) continue to operate. This situation can arise when multiple firms acquire the various components of the original firm. It is inherently difficult to define and measure firm changes when the original legal entity ceases to

³ 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.

exist and has no obvious successor. We exclude these cases from our firm-level longitudinal analyses. To reduce the number of observations lost for this reason and other challenges in tracking firms over time, we restrict our primary longitudinal analyses to the buyout year and the next two years.

Divisional Buyouts. In principle, the annual Company Organization Survey lets Census accurately track the business units involved in divisional sales. However, we discovered divisional sales in which the firm ID of the (new) target firm remained the same as the firm ID of the selling firm. This situation indicates that the new firm created in the course of the divisional buyout did not receive a new firm ID, at least not in a timely manner. This problem does not preclude an establishment-level analysis, because we can often use an alternative identifier – the Employer Identification Number (EIN) – to accurately identify, as of the buyout year, the establishments involved in divisional sales. Unfortunately, EINs are unsuitable for tracking firms through time, because new and acquired establishments may obtain new EINs. Thus, we exclude divisional buyouts from our firm-level longitudinal analyses when the LBD lacks an accurate firm ID for the newly created target firm. We exclude some secondary buyouts for the same reason.

After matching to the BR, we use the Longitudinal Business Database (LBD) – essentially a longitudinal version of the BR – to follow target firms and their establishments over time. We also use the LBD to identify control units (comparable firms and establishments) and to follow them over time as well. In addition, we exploit common alphanumeric identifiers to incorporate other Census micro data for some aspects of our analysis.

The LBD tracks establishments and parent firms using a combination of administrative records and survey collections that include the Company Organization Survey (COS), the Economic Censuses, and the Annual Surveys of Businesses (e.g., the Annual Survey of

Manufactures). Information about company structure is incorporated into the LBD by attaching firm identifiers to records for establishments. Ownership changes are identified when establishments switch parent firms through mergers, acquisitions, and divestitures.

The Census Bureau assigns a unique firm ID to all establishments under common ownership and control in a given year, including establishments that belong to subsidiaries under control of the parent corporation. This firm ID is distinct from a taxpayer ID such as the employer identification number (EIN).⁴ The relationships among the various IDs are as follows. In any given year, an establishment is uniquely associated with a single taxpayer ID and a single firm ID. Moreover, each taxpayer ID is uniquely associated with a firm ID. For multi-establishment firms, a parent firm ID has multiple affiliated establishment IDs and potentially multiple EINs. Put differently, the EIN as a unit of observation is somewhere between an establishment and a firm.

2. Matching Buyout Targets to the Business Register (BR)

From Capital IQ and other sources, we obtain several pieces of information about the acquired entity in a private equity buyout. These pieces include the name of the seller, the name of the acquisition target, the target's address, and the acquisition date. The seller and target are typically the same in whole-firm acquisitions but not in partial-firm acquisitions – for example, when the private equity firm acquires one division of a multi-division company.

We match acquisition targets to firms in the BR using the data matching algorithms that are part of the SAS DQMatch procedure. This is an improved version of the matching algorithm and code we used in Davis et al. (2014). Our DQMatch implementation proceeds through 16 rounds of matching from the strictest criteria (requiring a perfect match on name and address) to

⁴ 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.

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.⁵ Here, we describe our overall matching strategy, explain how we resolve buyout deals that match to multiple target firm candidates in the BR, and discuss issues that arise in tracking firms over time.

A. A Simple Case

Suppose a private equity firm acquires firm A in its entirety during year t and places it under new ownership, possibly with a new name. A simplified version of our matching algorithm in this case works as follows: First, we find an establishment in the BR as of year t located at the target address and owned by a firm with the target name. Second, with this match in hand, we use the firm-establishment links in the BR to identify the full set of establishments operated by the target firm in t . From this point, we can measure the activity of the target firm in t and follow the firm (and its establishments) forward from t using the LBD.

B. Challenges that Arise in the Matching Process

In practice, several challenges arise in the matching process. First, because name and address data are noisy, we may find multiple BR firms that are candidate matches for the acquisition target.⁶ All but one of these candidates, and perhaps all of them, are false positives.

⁵ 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.

⁶We use both physical and mailing address from the Business Register when available to generate matches. There is some noise in the addresses for new units in the Business Register that is typically resolved in an Economic Census. Our use of a multi-year window helps to partly overcome this source of noise. However, we did not find that our match rates peaked in Census years, suggesting that business name clarification in Economic Census years is not a big issue for our purposes.

Second, to cope with timing differences between datasets, we search for matches in the BR over a three-year window centered on the buyout year. While this approach can pick up good matches that we would otherwise miss, it can also introduce additional false positive matches. Whenever we have multiple candidate matches, we need some way to resolve to a unique match. When we cannot do so with sufficient confidence, we drop the acquisition target from our analysis.

Third, it can be hard to distinguish the seller firm from the acquisition target in some cases. For example, suppose a private equity firm acquires establishments e_1 and e_2 from firm A to form a new firm B in year t . In this case, the activity of establishments e_1 and e_2 are associated with both firms A and B in t , because each firm files tax records that cover e_1 and e_2 for part of the year. Thus, when we match the target address to an establishment, that establishment may link to two parent firms in the BR in the buyout year. In this situation as well, we need some way to resolve to a unique match.

Fourth, some private equity buyouts involve complex reorganizations of target entities that lead to the creation of multiple new firms or the piecemeal sale of the target entity to multiple parties. In these cases, even when we successfully match the target address to an establishment and correctly identify that establishment's parent firm, we may identify and track only some of the establishments acquired as part of the buyout. Indeed, there can be multiple true successor firms to the target entity in such cases, and we may capture and track only one of them.

Fifth, another challenge involves divisional buyouts, whereby the private equity firm acquires only part of a multi-division firm. For divisional buyouts, we could not always identify the correct target firm in the BR after matching the deal to a specific establishment. These instances arose because, in some cases, the Census firm ID associated with the matched establishments did not change to reflect the ownership change of the division involved in the buyout deal. We

identified these problematic cases by observing that the matched target establishment remained affiliated with the parent seller firm even after the buyout. It is our understanding that the Census Bureau on occasion had difficulty tracking the new firm in divisional buyouts because of nonresponse on the COS or other survey instruments.

We thus had two types of divisional cases. The first are those where we could accurately identify the target firm using our main method, and the second where we could not. Even in those cases, we were able to link the matched establishment to at least a part of the target firm through the EIN (taxpayer ID). The complete target firm may or may not be identified in such cases, because the divisional business involved in the buyout may have operated with multiple EINs. In the main text and this appendix, we refer to such cases as EIN cases. In these EIN cases, we can accurately identify a part of the target firm in the buyout year and at least some of the corresponding target establishments, but we cannot be confident that we captured the entire target firm. We exclude EIN cases in our firm-level longitudinal analyses, because the EIN is not suitable for tracking firms over time. For example, if a target firm (i.e., an EIN case) creates or acquires a new establishment, it may obtain a new EIN for that establishment for accounting or tax reasons. In such cases, we would not know that the new establishment is part of the target firm.

C. How We Proceed

As explained above, our matching algorithm may initially yield zero, one or multiple candidate matched firms in the BR for a given buyout target. We now provide information about the frequency of these outcomes and describe our process for de-duplicating buyouts that match to multiple Census firm IDs.

No Match

In about 2000 of the 9794 deals in CapitalIQ, no companies within the BR matched even using the loosest matching criteria. Here and below, we provide rounded figures for counts of matched Census firms because of data disclosure restrictions.

Unique Matches

As noted above, we search for candidate matches in the BR over a three-year window centered on the buyout year, t . First, we select a year ($t-1$, t or $t+1$) in the three-year window for the buyout in question. Second, given the year, our algorithm proceeds through 16 rounds using progressively less stringent matching criteria. Third, if we obtain at least one candidate match in a given round, we do not proceed to later rounds for that year. For example, suppose a buyout target matches to a single BR entity in round 4 of our algorithm for year t . Even if the target firm matches with other BR entities in later rounds (which involve less stringent criteria), we stop in round 4 for year t . This process can lead to one or more candidate matches in each of $t-1$, t and $t+1$.

For about 4,000 of the 9,794 buyouts that we identified using CapitalIQ and other sources, the process described in the preceding paragraph yields a single match candidate. That is, the process yields at most one candidate in each of $t-1$, t and $t+1$; and, moreover, when it yields a candidate match in two or three of the years, it is the same firm in each year.

Non-Unique Matches and De-Duplications

The remaining set of about 3500 buyout deals match multiple BR entities. This could happen, for example, if we find an exact match on address, but there are multiple firms in a single building with similar company names in the same year. As another example, Census often redefines the target firm's firm ID after the buyout. When it does, we often detect two match candidates within our three-year window centered on the buyout year: one match to the pre-buyout firm ID, and one to the post-buyout firm ID. We use three methods to arrive at a unique match

between the buyout target and the Census firm ID in these and other cases that yield multiple candidate matches.

The first method for de-duplicating is to check the EINs of the match candidates. For about 25 percent of the duplicates, multiple match candidates have the same EIN. That tells us that each match candidate is owned by the same parent firm, and we proceed on that basis. This method is especially helpful in resolving duplicates that arise when Census changes the firm ID associated with the firm in question within the three-year centered window around the buyout transaction.

The second method for de-duplicating is to exploit the timing pattern of the matches. We consider cases with two candidate matches for the same deal. A common pattern in such cases is that one candidate is the birth of a new firm ID at time t or $t+1$, and the other candidate is a death at time $t-1$ or t . In this context, a “birth” is when a new firm ID appears at time t or $t+1$, one that did not appear earlier (in $t-1$ for births in t , or $t-1$ and t for births in $t+1$). A “death” is when a firm ID disappears in time t or $t+1$. We investigated cases that fit this pattern and determined that they likely reflect PE-precipitated reorganizations. Since these candidate matches satisfy name and address matching criteria, they are unlikely to be spurious. This second step uniquely resolves about 200 additional firm IDs in the BR to a particular target firm in a PE buyout.

If the first and second methods do not yield a unique match, we deploy a third method as follows. First, for the set of candidate matches, rank firm IDs by the strictness of the criteria that generated their inclusion as match candidates. Then create three flags:

- Set Flag 1 to 1 for those firm IDs with the highest rank among the match candidates. If there are two candidate matches, for example, one for year $t+1$ with an exact name and address match and one for year t that matches exactly only on the name, set Flag 1 to 1 for the one that matches exactly on both name and address.

- Among candidate matches with the highest rank, set Flag 2 to 1 for firm IDs that are present in year $t+1$.
- Among candidate matches present in year $t+1$, set Flag 3 to 1 for firm IDs that achieve the highest rank.

If one, and only one, firm ID satisfies Flag 1 = Flag 2 = Flag 3 = 1, we treat that firm as the true match and use it in our analysis. This three-flag method resolves about 1000 additional buyouts to a Census firm ID. Altogether, our three resolution methods yield about 2000 additional matched deals. This gives us the total sample of approximately 6000 matched buyout deals.

3. Tracking Firms and Establishments after the Buyout

As explained in the main text, we cannot always track target firms with confidence in the years after the buyout. Tracking difficulties can arise because (a) a target is broken into many pieces, some or all of which are re-sold to other firms, and (b) errors and ambiguities in Census data prevent us from following the firm with confidence after the buyout. Thus, our econometric analysis in Section III primarily examines the sample of “Two-Year Continuers” that we track with confidence. Our concept of “Continuers” includes firms that die in the sense that all of its establishments in the buyout year t cease to operate by $t+2$.

Tracking establishments in Census data is typically much easier than tracking firms. However, even establishments are challenging to track in certain limited circumstances. Every five years, the Census Bureau obtains a full list of establishments owned by multi-unit firms from the Economic Censuses. It obtains a full list of establishments owned by large multi-unit firms (250 or more employees before 2013) from the annual Company Organization Survey (COS). The COS also samples smaller multi-unit firms in a targeted manner based on information that they underwent rapid growth or organizational change. When this information is incomplete, Census

may not promptly recognize new establishments operated by small, multi-unit firms in intercensal years. To address this matter, the LBD retimes the intercensal entry and exit of some establishments operated by small multi-unit firms. Still, the timing of M&A activity for small multi-units not covered by the COS or other Census surveys exhibits some bunching in Economic Census years. We do not think this limited bunching is a serious concern for our analysis, in part because small units get little weight in our employment-weighted regressions.

In the robustness section IV.H, we extend the analysis to five-year continuers using the same methodology. To make results comparable, the starting point of this analysis is this sample of two-year continuers in Tables 2 and 3. Tracking this set of buyouts and controls for five years using LBD firm identifiers yields a reduction in the sample size, as is evident from Table 8. This is partly because of the issues of tracking firms using the LBD firm identifier over long periods of time. However, it also reflects the changing vintage issues of the LBD across years. The LBD is revised each year. Our matching analysis yielded a set of files matching buyouts to the LBD through 2013. When we turn to the five-year horizon, this implies we track the outcomes of buyouts from the buyout year to $t+5$ for buyouts from 1980-2008. For the productivity growth analysis, we can use buyouts from 1980-2009 since our analytic databases of matched buyouts uses the revenue productivity database through 2014. We decided to include 2009 buyouts in the productivity analysis because as discussed above there is a significant reduction in sample size for the productivity analysis. We also note that productivity growth cannot be measured for pseudo-firms so that analysis is restricted to employment growth and reallocation outcomes.

Internet Appendix B: Empirical Methods and Identification Assumptions

This appendix provides additional details about our empirical methods. The first relates to how we track business outcomes over time. While we focus on firm-level outcomes, we exploit the establishment-level data in the LBD in several ways: to distinguish organic changes at the firm level from acquisitions and divestitures; to capture new facilities opened after the buyout; and to decompose firm-level employment changes into the gross job creation and destruction components associated with growing and shrinking establishments, respectively. The LBD's capacity to isolate each of these adjustment margins is one of its major strengths.

A second aspect relates to aggregation and the measurement of growth rates. Let E_{it} denote employment at establishment or firm i in year t – i.e., the number of workers on payroll in the pay period covering March 12. We measure the employment growth rate of unit i from $t - k$ to t as $g_{it,t-k} = (E_{it} - E_{i,t-k})/X_{it,t-k}$, where $X_{it,t-k} = 0.5(E_{it} + E_{i,t-k})$. This growth rate measure is symmetric about zero and lies in the interval $[-2, 2]$, with endpoints corresponding to death and birth.⁷ Employment growth at higher levels of aggregation is then given by $g_{t,t-k} = \sum_i (X_{it,t-k}/X_{t,t-k})g_{it,t-k}$, where $X_{t,t-k} = \sum_i X_{it,t-k}$. Using these formulas, we can easily and consistently aggregate from establishments to firms, from individual units to industries, and over time periods. This approach to growth rates and aggregation also works for gross job creation and destruction, job reallocation, and employment changes along particular dimensions such as acquisitions and divestitures or continuing establishments.

⁷ This growth rate measure has become standard in analyses of establishment and firm dynamics, because it shares some useful properties of log differences while also handling entry and exit. See Davis, Haltiwanger, and Schuh (1996) and Törnqvist, Vartia, and Vartia (1985).

A third aspect relates to the selection of control units for comparison to buyout targets in our regression models. We need suitable control units because the distribution of PE buyouts across industries and business characteristics is not random. Target firms are larger and older than the average firm and disproportionately concentrated in manufacturing, information technology, accommodations, and food services (Davis et al., 2014). They also differ by deal type, as shown above. Moreover, growth and volatility vary greatly by firm size and age, and workplaces and technologies differ greatly by industry.⁸ Hence, we sort target firms into cells defined by industry, size, age, multi-unit status, and buyout year. We then identify all firms not backed by private equity that fall into the same cell as the given target firm(s) and treat those firms as control units for the target firm(s) in that cell. Specifically, we define our control cells as the full cross product of about 90 industries (at the three-digit NAICS level), ten firm size categories, six firm age categories, a dummy for firms with multiple establishments, and 32 distinct buyout years from 1980 to 2011.

Fourth, we estimate the effects of buyouts using a suitably weighed least-squares regression approach as described in the main text. That is, we compare changes in employment growth rates and productivity growth at target firms in the wake of buyouts to contemporaneous changes at their matched control units.⁹ This approach, together with our control variables, facilitates an apples-to-apples comparison when estimating buyout effects.

A fifth aspect pertains to exactly how we weight observations when implementing our weighted least-square regressions. As an illustration, consider a case with two target firms in two separate control cells and target-control comparisons from t to $t + k$. The targets have activity

⁸ 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).

⁹ 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.

levels $X_{1,t+k,t} = 0.5(E_{1,t+k} + E_{1t})$ and $X_{2,t+k,t} = 0.5(E_{2,t+k} + E_{2t})$, where $E_{i,t}$ denotes the level of employment at firm i in year t . The first target's share of aggregate target activity is $\tilde{\omega}_{1,t+k,t} \equiv X_{1,t+k,t}/(X_{1,t+k,t} + X_{2,t+k,t})$, and the second's share is $\tilde{\omega}_{2,t+k,t} \equiv X_{2,t+k,t}/(X_{1,t+k,t} + X_{2,t+k,t})$. Since each control cell has a single target, these are also the control cell weights in this example.¹⁰

Principle **SCT** requires $\sum_j^{\mathbb{C}=1} \omega_{j,t+k,t} = \tilde{\omega}_{1,t+k,t}$ and $\sum_j^{\mathbb{C}=2} \omega_{j,t+k,t} = \tilde{\omega}_{2,t+k,t}$, where \mathbb{C} indexes control cells, and j indexes control units in the cell. We impose principle **EC** to equalize the weights on each control unit within a given control cell.

We tried other approaches to within-cell weighting of control units but ultimately settled on **EC** for three reasons. 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 scrutinized them carefully. We believe we have identified (and corrected) gross errors in target outcomes. A similarly careful approach for controls is infeasible because they are so numerous. Second, it is often hard to fit very large firms to one industry category, even at the three-digit NAICS level. The classification challenges presented by these 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, we mitigate these concerns.

Recall that we aim to recover the average treatment effect on the treated (buyout) firms under CMI and SUTVA. A standard approach, which we took in Davis et al. (2014), is to fit a regression model with heterogeneous treatment effects, average over the treatment effect

¹⁰ 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.

estimates, and compute the standard error for the average treatment effect by the delta method. (See Chapter 18 in Wooldridge, 2002.) Weighting principles TS and SCT afford a simpler econometric approach that recovers the average treatment effect of interest from a specification with a homogenous treatment effect. Under this simpler approach, we need not resort to the delta method to obtain standard errors. We can instead obtain them directly from the standard output for weighted least squares regressions in STATA and other widely used statistical packages. That is the approach we take in this paper.

Table B.2 provides information about pre-trends based on the $LEST_{it}$ and $LFIRM_{it}$ measures. Recall that $LEST_{it}$ is the pre-buyout growth rate of establishments owned by firm i as of the buyout year t , regardless of who owned them before the buyout. $LFIRM_{it}$ is the firm's pre-buyout growth rate (inclusive of establishment acquisitions and divestitures). As reported in Panel A of Table B.2, $LFIRM_{it}$ is higher, on average, for targets than for the corresponding controls. The difference is marginally significant in a specification with no fixed effects and significant at the one percent level when including a full set of fixed effects. In contrast, Panel B shows little indication that $LEST_{it}$ differs between target and control firms. This pattern suggests that target firms tend to grow faster than control firms in the pre-buyout period (i.e., from $t - 3$ to $t - 1$) via acquisitions. There is, however, no evidence that the set of establishments owned by targets as of the buyout year grew faster in the pre-buyout period.

Table B.1. Robustness Analysis to Inclusion of Saturated Fixed Effects and Lagged Employment Growth Controls

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. This table provides estimates of specification robustness analysis. All specifications use the “matching estimator” weights described in the text. The first column provides the estimate on employment growth (panel A) and productivity growth (panel B) differences over a two-year horizon between buyouts and controls without any controls. The second column includes the fully saturated set of fixed effects (the full cross product of firm age, firm size, industry, multi-unit status, and buyout year effects). The third column is the baseline specification reported in Table 2. Huber-White robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

<i>Dependent Variable</i>	Effect	R ²	Effect	R ²	Effect	R ²
A. Employment Growth,	1.97		1.97		1.78	
		0.0006		0.30		0.30
All Margins	[3.05]		[1.97]		[1.96]	
Observations (000s)	6,425					
B. Labor Productivity	7.80		7.80		7.63	
	[9.04]	0.0048	[4.97]	0.45	[5.13]	0.45
Observations (000s)	910					
Controls						
Fixed Effects	No		Yes		Yes	
Lagged Employment Growth Controls	No		No		Yes	

Table B.2. Pre-Trends in Employment Growth: Buyouts vs. Targets

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. All specifications use the “matching estimator” weights described in the text. Panel A provides the estimated difference between buyouts and controls of the employment growth for the firm in years t-3 to t-1. Panel B provides the estimated difference between the employment-weighted establishment growth rate in years t-3 to t-1 for the establishments that are owned by the firms in year t (the buyout year). Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

<i>Dependent Variable</i>	Effect	Effect
A. Lagged Employment Growth at Firm	5.37* [3.02]	5.37*** [1.75]
B. Lagged Employment Growth at Employment-Weighted Establishments	0.44 [1.79]	0.44 [1.14]
Controls (Fixed Effects)	No	Yes

Internet Appendix C: Creating the Private Equity Group-Transaction Sample

This appendix describes how we supplemented our database to capture information about the PE groups (also referred to as general partners, or GPs) that sponsor buyouts.

We transformed our original data for present purposes from being at the buyout level (as in Tables 2 through 5) to being at the buyout-PE group level. Thus, a single buyout transaction identifier may have multiple entries (with the same CapitalIQ transaction identifier), if there is a “club” (or syndicated) transaction with participation by multiple PE groups. In the Capital IQ database, there are 9809 distinct transactions meeting our criteria. In 89% of the cases, we obtained at least minimal information (organization type and fundraising history) about at least one buyout group active in the transaction. Because some transactions involve multiple PE groups, there are 11,606 distinct observations of PE group-buyout pairs. The most-active PE groups in the sample by deal count include some of the largest and most recognizable PE organizations, as well as leading specialists in middle market deals. (As we document in Table 1, sample sizes shrink once these data are matched to the Census information.)

PE Group Identifier

We assigned each PE group an identification number. That was simple when the group began as and remained an independent entity. Cases involving a change in control (and sometimes a name change), often associated with an acquisition or spin-off, were more complex.

Where there was a spin-out, we considered the spun-out entity to be a new PE group (with a new identifier), unless we were highly confident that it encompassed essentially all the PE investment activity of the predecessor group. In the latter case, a group might change status over time: i.e., from part of a bank or a family office to independent. When an independent group was acquired by another group, it was subsumed into the acquiring group after the acquisition, and its

investments assigned to the acquirer's identifier. In addition to relying on entries in Capital IQ (the "Firm Description" field, a text description of the firm's history, its investment profile, and more), Refinitiv Thomson One, and Preqin databases, we undertook extensive online research to make these determinations. We especially relied here on searches using Factiva and Lexis-Nexis for historical information that was not accessible through searches of the unrestricted Internet.

In many cases, it was helpful to use information about the year the organization was founded in these determinations. Capital IQ often reports the year founded, sometimes in the "Firm Description" field instead of the "Year Founded" field. In cases where this information was missing, we used the start dates reported in Preqin.

Three complexities, however, arose in the determination of founding dates:

- Firms spun off from another institution (typically a bank) sometimes recorded their start date as that of the spin-off, and other times when the predecessor group was established. We standardized these (to the extent possible) to the year the predecessor group was established within the old parent institution. If instead of the spin-off of a clearly delineated group within the old parent institution, the creation of the firm entailed the departure of a few individuals within a larger body, the date of the actual firm formation was used.
- Groups that were still parts of a parent institution sometimes used the year the first PE program was set up at the parent, the year the specific initiative was established, or the year the parent was established. We standardized these to the extent possible to the year the first PE program was set up at the parent institution. If we could not get a start date of the first PE program or the specific investing program's inception, we left this information blank.

- Few corporations, family offices, and institutional investors disclosed when their private investment programs began, simply reporting when the overall entity was founded. If we could not get a date for the program's inception, we left this information blank.

Type of Organization

CapitalIQ was used to classify types of PE groups, particularly the fields "Primary Industry," "Institution Type," and "Firm Description." For groups whose status changed (e.g., due to an acquisition or divestiture), we used the information as it stood at the time of investment. Again, these changes were confirmed and precise dates identified using online searches. Where this information was incomplete, we supplemented it with online searches.

We used the following scheme to classify firms.

0- PE groups or diversified investors where private equity is an important component (e.g., Blackstone, Carlyle). This includes organizations with a "fundless" structure (e.g., who are investing off their balance sheet or on a "deal by deal" basis), as well as those who raised their last fund many years earlier.

1- Investment arms that are subsidiaries of other financial institutions, including investment/commercial banks, insurance companies, mutual funds, and brokerage houses, whether investing through funds or directly from these entities' balance sheets.

2- Investment arms that are subsidiaries of non-financial operating corporations, whether investing through funds or directly from these entities' balance sheets. In some cases, investment groups are identified by their largest holding, making them difficult to distinguish from operating companies. Other entities are unclear whether they are a business or investment company. A key test is whether there is a recognizable "core" business in a single or set of related industries. Berkshire Hathaway and GE are perhaps extreme cases. BH could be regarded as a

(fundless) PE group, an insurer, or an operating company; we classified as a 0. On the other hand, General Electric or Mitsubishi's various financing subsidiaries could be regarded for much of the period as a (funded or fundless) PE group, a financial services firm, or an operating company; we classified them as a 2. Because the sample is limited to PE buyout transactions (i.e., excluding traditional strategically motivated acquisitions), no transactions by Danaher Corp., one of the most active acquirers of U.S. manufacturing firms, are included in the database. Thus, the only corporate transactions are those where the firms are either (a) undertaking their own PE-type transactions, often through a financing arm, or (b) co-investing with a large limited partner.

3- Investment arms that are subsidiaries of institutional and family investors, such as pensions, sovereign wealth funds, university endowments, and the like, whether investing through funds or (more commonly) directly from these entities' balance sheets. Again, we also include co-investments with PE groups; but as we point out elsewhere, the coverage of co-investment by CapitalIQ does not seem comprehensive and indeed biased (Fang et al., 2015).

4- Organizations with the bulk of their assets (90%+) in debt, hedge, and real estate funds or who primarily lend off their balance sheets (excluding commercial banks, who are included in 1), but who do some PE investing on the side. This category does not include diversified investment managers who also own some of these funds.

Prior Fundraising

Fundraising data was bulk-downloaded from Preqin. The Preqin firm names were matched to the firm names in the Capital IQ data. This matching, in many cases, took considerable background research using online sources, due to the plethora of groups with similar sounding names (e.g., Pine Brook, Pine Creek, Pine Street, Pine Tree Equity, Pine Tree Growth, and PineBridge, not to mention various variants of White Pine).

Coverage of funds (amounts raised and performance) in Preqin is imperfect, particularly before 2000. We supplemented the Preqin information with fundraising data from Refinitiv Thomson One for those entities with no fundraising data in the relevant period. (Again, this took considerable research to resolve name matching.) In other cases, we found fundraising material online (e.g., state pension web sites) that summarized the timing and size of a group's funds.

We summed the count and size of the funds closed in the year between the deal year in the original data and four years before ($t-4$ to t), covering PE funds. All fund totals are expressed in millions of current U.S. dollars, converted from foreign currencies (if necessary) using the exchange rate at the mid-point of the year of the investment contained in the U.S. Federal Reserve Bank's H-10 series. We identified which funds to use based on the organization of the firm as of the time of the investment. Thus, for transactions in 2007 and before, we would look only at the funds raised by GSO Capital Partners. After its 2008 acquisition by Blackstone, we would compute the total raised by Blackstone (and GSO) in the five-year period.

We also created a normalized series: the funding divided by total funding raised at the beginning of the deal year in the original data and four years before ($t-4$ to t). These fundraising totals were for the years from 2000 to 2013 from Preqin (for U.S. based buyout and balanced private equity funds only) and from 1980 to 1999 from Thomson Reuters (North American-based buyout, mezzanine and growth funds).¹¹

Prior Fund Performance

For these firms, performance data (as of the end of 2019 or the closest date prior to this point) for any funds from years $t-12$ to $t-5$ were also collected. We captured funds for banks and

¹¹ For years before 1980, we assumed based on press accounts an annual fundraising rate of \$100 million per year in 1978 and 1979, and \$50 million per year in 1976 and 1977.

corporations that raised funds under different divisions and programs (e.g., for General Electric, entities raising funds included GE Capital, GE Commercial Finance, and GE Holdings). These data were primarily taken from Preqin but complemented with information from PitchBook and state public pension disclosures.

We focused on the performance of the most recent U.S. (or global) PE funds in that period with performance data and the two most recent funds. We looked at internal rate of return (IRR) and multiple of invested capital (MoIC), since this information was most readily available in Preqin. (Coverage of public market equivalents was much thinner.) In each case, we subtracted the benchmark performance calculated as the pooled IRR and weighted MoIC. The sources of the benchmark performance information were as follows:

- For vintages 1985, 1987 to 1988, and from 1990 to 2008: Preqin database, using data on North American buyout funds.
- For vintages 1986 and 1989: Cambridge Associates via ThomsonOne, using data on US buyout and growth equity funds.
- For vintage years 1976 to 1984: Venture Economics, 1998 Investment Benchmark Reports, Newark, 1998, using data on US buyout funds (data on the 1976-83 period is consolidated in the report).
- For vintage years 1968 to 1975: Venture Economics, Venture Capital Performance 1989, Waltham, 1989, using data on all US private capital funds (data on the 1970-76 period is consolidated in the report).

In cases where we used multiple funds, we took a fund-weighted average of the fund net performance. Because this averaging process is more correct for TVPIs (an average of two IRRs

may be quite different than the IRR of the combined cash flows), we focused on the TVPI measure in the paper.

This performance information was typically missing for groups that invested off their own balance sheet (which included many financial institutions, family offices, and corporations, and some private equity groups with fundless structures). In some cases, groups invested through both funds and their balance sheets, whether PE groups that have raised outside capital at the management company level (e.g., KKR) or more typically, banks and corporations. To cite one example, the amount raised through its funds was a small fraction of what GE invested, since most was done through its balance sheet. Unfortunately, there is no way to create a fund-like measure for balance sheet assets, since capital designated for investments is typically not segregated in financial reports. We thus computed the total for the formal funds.

We did a variety of diagnostic tests to verify the information and to catch potential errors. These exercises included:

- Looking at all PE investment entities with a start year before 1945. (Some entities did indeed start earlier, but we sought to be extra careful here.)
- Looking at PE investment entities with the same identification number but a different classification. (Again, due to spin-offs, some organizations did change status, but we were extremely careful and conservative here.)
- Looking at PE investment entities with the same identification number but a different start year. (Again, such cases could result as a result of a spin-off—see the rules delineated under Step 1—but we wanted to be sure.)

- Looking at PE investment entities with the same identification number and transaction year, but a different number and volume of funds raised in the prior five years. (These were typically the result of miscoding organizations with complex organizational histories.)
- Looking at PE investment entities with the same identification number, but with substantial discrepancies in the number and volume of funds raised in the prior five years between adjacent years. (These were typically a consequence of miscoding organizations with complex organizational histories.)

Internet Appendix D: Additional Results

Table D.1 tabulates the data presented in Figure 1 for three periods selected to highlight how PE deal flow sank during the financial crisis and recovered afterwards. Table D.2 provides information about the distribution of PE buyouts by industry sector and deal type. It also uses the same sample as Figure 1.

In Table D.3, Panel A breaks down the overall employment change by establishment status. Here, “Continuers” refer to establishments that operate under ownership of the same firm (target or control) throughout the period from t to $t+2$. Continuer employment at target firms shrinks by (a statistically insignificant) .7% relative to control counterparts in the two years after buyout. The rate of employment change at growing continuers is essentially identical for buyouts and controls, as indicated by the “Creation” results. In contrast, contracting continuers shrink more rapidly at targets, as indicated by the “Destruction” results. Target firms experience 4.1% larger employment losses from shuttered establishments (“Deaths”) and 2.6% greater employment gains due to new facilities (“Births”). They also add more jobs through acquisitions to the tune of 5% of base employment. All three of these differences are statistically significant. The difference in job changes from divestitures, however, is statistically significant but small in magnitude.

Because the dependent variables are components of overall net growth, we can sum up the coefficients. Consider first the results for “Continuers” and “Deaths,” which capture all employment changes for establishments owned and operated by targets and controls in the buyout year. Summing these two components yields a two-year employment growth rate differential of -4.8 percentage points (-0.74 – 4.09). That is, establishments operated by target firms as of the buyout year shed 4.8% 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 2.6 points to this sum. That yields a net differential of -2.3 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 4.2 points, yielding an overall buyout effect on firm-level employment of 1.9 percentage points over two years (slightly larger than the overall sum due to rounding error). The other panels in Table D.3 consider various results for job reallocation (overall and excess) and labor productivity.

Table D.4 reports estimated buyout effects on employment by adjustment margin and buyout type. Notably private-to-private has a high contribution from establishment births and acquisitions, public-to-private has a high contribution from continuing establishments job destruction, establishment deaths and divestitures, divisionals also has a high contribution from continuing establishments job destruction, establishment deaths and divestitures, secondary has a high contribution from job creation of continuing establishments, establishment births and acquisitions. It is this variation that underlies the Table 2 result of substantial differences in both overall and organic net employment growth.

Table D.5 presents the results for the adjustment margins for five-year continuers for all buyouts. These results help provide the detail for Table 8 for all buyouts. Turning to results by buyout type, Table D.6 presents the adjustment margins results for five-year continuers by buyout type. These results provide the detail for Table 8 by buyout type. Table D.7 presents employment growth and reallocation differences for all buyouts and by buyout type for pseudo-firms. Table D.8 presents the contribution of continuing establishments and establishment deaths for pseudo-firms for all buyouts and by buyout type.

Table D.9 provides evidence on the wage effects of PE buyouts using a larger, broader sample than previous studies. How buyouts affect wages has long been controversial. Critics argue

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

Our wage measure in Table D.9 is the change from buyout year t to $t+2$ in the firm's gross annual compensation per employee.¹² The wage sample is smaller than in Panels A-C of Table 2 for the same three reasons discussed in regard to productivity in the text. In addition, compensation data are unavailable for some firms in the LBD.

The first column in Table D.9, Panel A reports a statistically significant wage drop of 3.1% at target firms relative to controls over two years post buyout. Because we derive this estimate as a difference-in-difference, it nets out persistent target-control differences in workforce composition. However, it does not control for changes from the buyout year t to $t+2$ in firm-level

¹² Barth et al. (2014) provide a detailed description of the LBD wage measure: "The data follow the definition of salaries and wages used for calculating the federal withholding tax. They report the gross earnings paid in the calendar year to employees at the establishment prior to such deductions as employees' social security contributions, withholding taxes, group insurance premiums, union dues, and savings bonds. Included in gross earnings are all forms of compensation such as salaries, wages, commissions, dismissal pay, paid bonuses, vacation and sick leave pay, and the cash equivalent of compensation paid in kind. Salaries of officers of the establishment, if a corporation, are included. Payments to proprietors or partners, if an unincorporated concern, are excluded. Salaries and wages do not include supplementary labor costs such as employer's Social Security contributions and other legally required expenditures or payments for voluntary programs." Thus, our wage measure includes management compensation except for stock option grants, which are typically constructed to defer tax obligations until exercise or sale. Buyouts often tilt the compensation of senior management toward stock options (Leslie and Oyer, 2008), so we may slightly understate the true wage change at target firms.

workforce composition. Establishment births, deaths, acquisitions, and divestitures are potentially important sources of such changes in firm-level workforce composition.¹³

Panel A suggests that buyout-induced wage effects also differ greatly by type. The drop in compensation per worker is especially large in private-to-private deals with a 4.3% decline and in secondary deals with a 7.5% decline. Divisionals exhibit a mild increase although not statistically significantly different from zero. More positive 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.¹⁴

Panels B and C display the relationship between the differences in wage changes and economic conditions at and after the buyouts. We find that higher credit spread in the two years after the buyout yields a somewhat greater decline in earnings per worker while higher GDP growth in the two years after the buyout yields a mitigating impact on the decline in earnings per worker from a buyout.

¹³ Appendix Table D.3 explores these firm-level adjustment margins and show that they are especially active at target firms in the wake of buyout deals.

¹⁴ The top five personnel of Axalta received compensation in 2013 of \$17.2 million, including the aggregate fair value of stock option awards as of the grant date. While the reporting of option grants may differ for tax purposes (and hence in our data), even the total non-option compensation of the five individuals was \$6.1 million. We cannot directly observe the compensation of the top five employees of DuPont Performance Coatings in 2012, but web sites such as Glassdoor suggest that senior divisional managers at DuPont received contemporaneous compensation packages in the mid-six figures. See Axalta Coating Systems, Schedule 14A, March 23, 2015 and Lerner and Tuzikov (2018). Thus, the compensation of top Axalta personnel in 2013 was much greater than what they, or their counterparts, likely earned as senior divisional managers before the buyout.

We also consider how buyout effects vary with the evolution of market conditions after the close of the deal. We measure post-buyout changes in market conditions from March (or the first quarter) of the buyout year t to March (first quarter) of year $t+2$.¹⁵ Table D.10 focuses on the post-buyout change in credit conditions, and Table D.11 focuses on post-buyout growth in real GDP.

Consider the results for all buyouts. Faster GDP growth in the two-year interval after buyouts brings greater post-buyout employment growth at targets relative to controls and greater excess reallocation. These effects are statistically significant and large: A unit standard deviation rise in the post-buyout GDP growth rate raises employment growth at targets relative to controls by 4.5 percent of base employment, and it raises target excess reallocation by 3.1 percent of base employment. A rise in credit spreads after buyouts brings slower post-buyout employment growth at targets relative to controls and slower excess reallocation. These effects are statistically significant and similar in magnitude to the ones associated with a unit standard deviation change in the GDP growth rate. Productivity growth is especially large (and statistically significant) at public-to-private deals with widening small credit spreads and faster GDP growth in the two years following the buyout.

Figure D.1 illustrates how post-buyout employment growth and excess reallocation at target firms (relative to controls) vary with the evolution of GDP growth and credit spreads. In the top panel, the baseline employment growth effect depicted in the center bar is of modest size, in line with our results in Table 2. However, the relative post-buyout employment performance of targets is highly sensitive to the evolution of market conditions. For example, a post-buyout decline in GDP growth by two standard deviations lowers the relative employment growth of targets by

¹⁵ Similar results obtain when using the change from the buyout closing date in year t to March of year $t+2$.

7%. Changing credit spreads lead to a similar pattern in the lower panel. Excess reallocation rates at target firms are also sensitive to the post-buyout evolution of market conditions.

Tables D.10 and D.11 also report results by deal type. Recall that average buyout effects vary greatly by deal type (Table 2), and the mix of buyouts by deal type varies over the economic and credit cycles (Figure 1). The tables provide evidence that the productivity effects are more sensitive to post-buyout macroeconomic and credit conditions for public-to-private than private-to-private deals, with divisional deals in the middle. In particular, when GDP grows faster or credit spreads narrow, the productivity growth of target firms is even higher (relative to controls) for the targets of public-to-private buyouts. A similar pattern holds for excess reallocation, except divisional buyouts show a greater sensitivity than public-to-private deals to post-buyout macroeconomic and credit conditions.

One explanation is that high leverage in public-to-private deals prevents management and investors from implementing pre-buyout operating plans when market conditions deteriorate or credit tightens, with negative implications for productivity and reallocation. A similar dynamic may hold for divisional buyouts, which are likely to resemble public-to-private deals along important dimensions. Interestingly, the pattern goes the other way in private-to-private deals: deteriorating economic conditions or tighter credit conditions lead to greater productivity gains at targets relative to controls.

We find some differences across buyout types in the responsiveness of target employment levels to post-buyout economic and credit conditions. Only some of these interaction effects on target employment growth are statistically insignificant.

Table D.12 reports these results for specifications and samples that parallel the ones in the “All Buyouts” columns in Tables D.10 and D.11. Again, the relative employment responsiveness

of target firms to recessions is, if anything, stronger than when using continuous metrics. Both overall and organic employment growth at targets worsens (relative to controls) when the economy enters a recession after the buyout. Also, akin to results in Tables D.10 and D.11, deteriorating economic conditions post-buyout involve less reallocation at targets. Coefficients on the recession interaction variable in these cases are roughly equal to a three standard deviation shift in the continuous interaction variables in those tables. The productivity regressions, however, show smaller coefficients for the interaction variables and less statistical significance than obtained with continuous measures of economic conditions.

Table D.13 repeats models considered in Tables D.10 and D.11, 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 widening credit spreads and greater GDP growth after buyouts are associated with more reallocation at targets, but effects are not precisely estimated. As in Table 4, the coefficients remain roughly the same size, but the responsiveness of target employment growth to economic conditions is weaker than before.

Table D.1. Private Equity Deal Flow Before, During, and After the Financial Crisis. The table reports the quarterly flow of private equity buyouts, overall and by deal type, in selected periods. It also reports the average value of the credit spread in the closing month and the annual real GDP growth rate over the four quarters that end in the closing quarter. The table entries are tabulated from the data plotted in Figure 1.

	<i>All PE Buyouts</i>	<i>Private to Private</i>	<i>Public to Private</i>	<i>Divisional Sales</i>	<i>Secondary Sales</i>
<i>A. Pre-Crisis, January 2004 to December 2007</i>					
Buyouts Closed Per Quarter	203	88	15	52	43
Average Credit Spread	3.27%				
Average Real GDP Growth Rate	2.85%				
<i>B. Crisis, October 2008 to June 2010</i>					
Buyouts Closed Per Quarter	87	46	5	17	18
Average Credit Spread	11.79%				
Average Real GDP Growth Rate	-1.40%				
<i>C. Post-Crisis, July 2010 to December 2013</i>					
Buyouts Closed Per Quarter	133	58	9	17	49
Average Credit Spread	6.81%				
Average Real GDP Growth Rate	1.97%				

Table D.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). The sample is the same as in Figure 1.

<i>Sector</i>	<i>GICS code</i>	<i>Buyout Type</i>				<i>Total</i>
		<i>Private-to-Private</i>	<i>Public-to-Private</i>	<i>Divisional</i>	<i>Secondary</i>	
Energy	10	2.9	2.2	2.6	2.2	2.6%
Materials	15	8.1	5.7	9.3	8.6	8.3%
Industrials	20	28.9	19.0	23.4	28.6	26.5%
Consumer staples	25	18.6	24.6	18.8	20.7	19.6%
Consumer discretionary	30	7.4	4.6	4.0	6.2	6.0%
Health care	35	10.1	12.0	8.0	10.3	9.7%
Financials	40	3.9	4.7	4.7	2.7	3.9%
Information technology	45	11.5	15.8	17.7	12.3	13.7%
Communications services	50	7.2	7.5	8.1	7.4	7.5%
Utilities	55	0.6	1.0	2.1	0.8	1.1%
Real estate	60	0.8	3.1	1.3	0.2	1.0%
		100.0%	100.0%	100.0%	100.0%	100.0%

Note: A test of the null hypothesis that the industry distribution of buyouts is independent of deal type yields a Pearson Chi-squared statistic of 260.7 with a p-value of 0.000.

Table D.3. Buyout Effects by Adjustment Margin at Target Relative to Control Firms. The sample contains matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status, and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are (approximate) percentage amounts from the buyout year t to $t+2$, unless otherwise noted. All results in Panel A are expressed as percentages of firm-level base employment. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. A positive coefficient in each case indicates that activity on that dimension is greater for buyouts. See Section II in the main text for an explanation of how we weight observations. Results for “All Margins” include the contribution of post-buyout acquisitions and divestitures, while results for “Organic Margins” exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. Huber-White robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A. Employment Growth	Buyout Effect	Standard Error	R ²
All Margins	1.78	[1.96]	0.30
Organic Margins	-2.29	[1.51]	0.28
By Establishment Status			
Continuers	-0.74	[0.98]	0.28
Creation	0.46	[0.53]	0.34
Destruction	1.20*	[0.66]	0.27
Deaths	4.09***	[0.92]	0.30
Births	2.55***	[0.64]	0.34
Acquisitions	5.00***	[0.85]	0.40
Divestitures	0.82**	[0.37]	0.26
Number of Firm Observations (000s)	6,425		

B. Reallocation (% of Base Employment)	Buyout Effect	Stan. Err.	R ²
Excess Reallocation, All Margins	9.21***	[1.33]	0.40
Excess Reallocation, Organic Margins	3.39**	[1.59]	0.36
Job Reallocation, All Margins	14.12***	[1.70]	0.41
Job Reallocation, Organic Margins	8.30***	[1.53]	0.40
Number of Firm Observations (000s)	6,425		

C. Productivity Change at Targets Relative to Controls, and Separate Contributions of Revenue and Employment Changes

	Buyout Effect	Standard Error	R ²
Revenue Per Employee	7.63	[5.13]	0.45
Revenue Contribution	6.69	[5.01]	0.44
Employment Contribution	-0.94	[2.13]	0.39
Number of Firm Observations (000)	910		

Table D.4. Buyout Effects on Employment by Adjustment Margin and Buyout Type. The sample contains matched two-year continuers that underwent private equity buyouts from 1980 to 2011 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status, and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are employment changes from the buyout year t to $t+2$, expressed as a percentage of firm-level base employment. A positive coefficient in each case indicates that activity on that dimension is greater for buyouts. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a separate weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the main text for an explanation of how we weight observations. Results for “All Margins” include the contribution of post-buyout acquisitions and divestitures, while results for “Organic Margins” exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. Huber-White robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Private-to-private		Public-to-private		Divisional		Secondary	
By Adjustment Margin:	Buyout Effect	R ²	Buyout Effect	R ²	Buyout Effect	R ²	Buyout Effect	R ²
Continuers	1.31 [1.10]	0.30	-3.47** [1.40]	0.33	-3.04 [2.02]	0.27	4.21** [1.81]	0.34
Creation	0.34 [0.72]	0.31	0.32 [0.65]	0.46	-0.4 [1.17]	0.28	2.70* [1.46]	0.28
Destruction	-0.97 [0.80]	0.40	3.79*** [1.03]	0.35	2.64** [1.23]	0.36	-1.51* [0.79]	0.41
Deaths	-1.06 [1.91]	0.45	6.68*** [1.45]	0.40	7.21*** [1.14]	0.44	1.83 [1.34]	0.39
Births	2.32*** [0.83]	0.23	-0.41 [0.58]	0.34	4.96*** [1.60]	0.23	4.20*** [1.17]	0.24
Acquisitions	10.33*** [2.56]	0.37	1.66** [0.85]	0.31	5.29*** [1.11]	0.28	4.28*** [0.84]	0.40
Divestitures	0.12 [2.56]	0.31	2.98*** [0.99]	0.33	-0.1 [0.59]	0.31	-0.17 [0.59]	0.29
Observations (000s)	3,920		426		2,255		588	

Table D.5. Buyout Effects by Adjustment Margin at Target Relative to Control Firms (Five-Year Horizon). The sample for employment (productivity) 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 $t+5$, unless otherwise noted. All results in Panel A are expressed as percentages of firm-level base employment. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the main text for an explanation of how we weight observations. Results for “All Margins” include the contribution of post-buyout acquisitions and divestitures, while results for “Organic Margins” exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. Huber-White robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A. Employment Growth	Buyout Effect	Standard Error	R ²
All Margins	2.95	[3.07]	0.29
Organic Margins	-2.54	[2.08]	0.31
By Establishment Status			
Continuers	-0.35	[0.86]	0.30
Creation	-0.76*	[0.46]	0.36
Destruction	-0.40	[0.60]	0.29
Deaths	6.04***	[1.78]	0.34
Births	3.85***	[1.12]	0.38
Acquisitions	5.72***	[1.15]	0.37
Divestitures	0.08	[1.26]	0.23
Number of Firm Observations (000s)	3787		

B. Reallocation (% of Base Employment)	Buyout Effect	Stan. Err.	R ²
Excess Reallocation, All Margins	13.34***	[2.21]	0.43
Excess Reallocation, Organic Margins	7.54***	[2.92]	0.36
Job Reallocation, All Margins	14.53***	[2.31]	0.39
Job Reallocation, Organic Margins	8.73***	[2.32]	0.41
Number of Firm Observations (000s)	3787		

C. Productivity Change at Targets Relative to Controls, and Separate Contributions of Revenue and Employment Changes

	Buyout Effect	Standard Error	R ²
Revenue Per Employee	10.86*	[6.00]	0.45
Revenue Contribution	11.58**	[5.90]	0.46
Employment Contribution	0.72	[2.10]	0.37
Number of Firm Observations (000)	870		

Table D.6. Buyout Effects on Employment by Adjustment Margin and Buyout Type (Five-Year Horizon). The sample contains matched five-year continuers that underwent private equity buyouts from 1980 to 2008 and control firms in the same cells defined by the full cross product of firm age, firm size, industry, multi-unit status, and buyout year. Some firms serve as controls for more than one buyout type. Outcome measures are employment changes from the buyout year t to $t+5$, expressed as a percentage of firm-level base employment. A positive coefficient in each case indicates that activity on that dimension is greater for buyouts. Each reported effect is the coefficient estimate [standard error] on a buyout indicator in a separate weighted least-squares regression that includes a full set of cell-level fixed effects and controls for pre-buyout growth histories. See Section II in the main text for an explanation of how we weight observations. Results for “All Margins” include the contribution of post-buyout acquisitions and divestitures, while results for “Organic Margins” exclude them. Reallocation measures are computed from establishment-level employment changes at the firm. Huber-White robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	Private-to-private		Public-to-private		Divisional		Secondary	
By Adjustment Margin:	Buyout Effect	R ²	Buyout Effect	R ²	Buyout Effect	R ²	Buyout Effect	R ²
Continuers	0.76 [1.59]	0.32	-0.93 [1.28]	0.38	-2.92** [1.34]	0.26	3.72*** [1.37]	0.36
Creation	-0.88 [0.78]	0.42	0.08 [0.72]	0.38	-2.08*** [0.79]	0.33	0.66 [0.08]	0.38
Destruction	1.64 [1.01]	0.28	1.02 [0.92]	0.39	0.84 [1.08]	0.23	-3.07*** [0.82]	0.35
Deaths	1.48 [3.57]	0.34	3.83 [4.11]	0.42	14.64*** [2.14]	0.36	4.59*** [1.71]	0.29
Births	-2.21 [2.33]	0.40	-0.23 [1.55]	0.41	11.58*** [1.66]	0.45	4.83*** [1.96]	0.40
Acquisitions	12.05*** [2.89]	0.39	3.06 [2.12]	0.34	4.94*** [1.03]	0.40	7.39*** [1.58]	0.42
Divestitures	0.01 [2.95]	0.25	0.52 [1.37]	0.35	-3.77 [3.09]	0.17	3.54*** [0.45]	0.42
Observations (000s)	2183		199		1,593		326	

Table D.7. Estimated Buyout Effects on Employment and Job Reallocation – Five-year Horizon, “Pseudo Firms”

The sample contains matched private equity buyouts from 1980 to 2008 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. This table tracks the outcomes of “pseudo-firms” defined as follows. A pseudo-firm comprises the establishments that are part of the target or control firms in t . Measures of net growth and reallocation reflect the employment-weighted outcomes for these establishments. Then these pseudo-firms are tracked through year $t+5$. 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. Given the definition of pseudo-firms, these results do not reflect any births or acquisitions. Moreover, for the pseudo-firm divestitures are irrelevant as that component of the pseudo-firm is tracked include births or acquisitions. Moreover, establishments that are divested are tracked forward as part of the pseudo-firm. Reallocation measures are computed from establishment-level employment changes at the pseudo-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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Dependent Variable	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary		p-Value, F-test
	Effect	R ²	Effect	R ²	Effect	R ²	Effect	R ²	Effect	R ²	
A. Employment Growth,	-8.3*** [1.9]	0.31	4.0 [4.0]	0.34	-7.2** [3.4]	0.39	-19.4*** [3.0]	0.35	-6.5*** [3.7]	0.32	0
B. Job Reallocation,	7.8*** [1.9]	0.41	-1.1 [4.6]	0.47	8.6*** [3.1]	0.45	14.9*** [3.0]	0.41	7.8** [3.0]	0.34	0.04
C. Excess Reallocation,	2.6*** [0.9]	0.37	2.6 [2.0]	0.46	4.7*** [1.2]	0.45	1.9 [1.3]	0.31	0.78 [1.3]	0.29	0.32
Observations (000s)	4499		2731		220		1769		390		

Table D.8. Estimated Buyout Effects on Adjustment Margin and Buyout Type – Five-year Horizon, “Pseudo Firms”

The sample contains matched private equity buyouts from 1980 to 2008 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. This table tracks the outcomes of “pseudo-firms” defined as follows. A pseudo-firm comprises the establishments that are part of the target or control firms in t . Measures of net growth and reallocation reflect the employment-weighted outcomes for these establishments. Then these pseudo-firms are tracked through year $t+5$. 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. Given the definition of pseudo-firms, these results do not reflect any births or acquisitions. Moreover, for the pseudo-firm divestitures are irrelevant as that component of the pseudo-firm is tracked include births or acquisitions. Moreover, establishments that are divested are tracked forward as part of the pseudo-firm. Reallocation measures are computed from establishment-level employment changes at the pseudo-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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Dependent Variable	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary	
	Effect	R ²	Effect	R ²	Effect	R ²	Effect	R ²	Effect	R ²
A. Continuing Establishments Employment Growth	-0.8		2.9*		-2.1		-2.8*		0.3	
		0.29		0.33		0.37		0.24		0.33
	[0.9]		[1.6]		[1.6]		[1.6]		[0.2]	
B. Establishment Deaths,	7.4***		1.4		5.0		16.4***		7.1***	
		0.34		0.40		0.42		0.37		0.28
	[1.7]		[4.0]		[3.1]		[2.5]		[2.7]	

Table D.9. Buyout Effects on Wages by Establishment-Level Adjustment Margin. The table presents tables identical to those in the paper, but for wages. Panel A replicates Table 2; Panel B, Table 3; and Panel C, Tables D.10 and D.11 (first column only). See tables in the paper for more information.

Panel A: Estimated Buyout Effects.

Dependent Variable	All Buyouts		Private-to-private		Public-to-private		Divisional		Secondary		p-Value SUR test
	Effect	R ²	Effect	R ²	Effect	R ²	Effect	R ²	Effect	R ²	
Annual Compensation Per Employee	-3.14*** [0.81]	0.45	-4.29*** [1.01]	0.47	-1.43 [1.84]	0.62	1.24 [1.65]	0.50	-7.54*** [0.89]	0.38	0.03
Observations (000s)	3,653		2,103		162		1,438		284		

Panel B: How Buyout Effects Vary with Macroeconomic and Credit Conditions at the Close.

Dependent Variable		Interaction Variable	
		Credit Spread	GDP Growth
Annual Compensation Per Employee	Coefficient	0.26	0.93
	[St. Error]	[0.25]	[0.89]
	Unit S.D. Effect	-0.80	1.53

Panel C: How Buyout Effects Vary with the Credit Spread Change and GDP Growth in the Two Years after the Buyout.

Dependent Variable		Credit Spread	GDP Growth
Annual Compensation Per Employee	Coefficient	-0.44***	0.62**
	[St. Error]	[0.11]	[0.27]
	Unit S.D. Effect	-1.92	2.12

Table D.10. How Buyout Effects Vary with the Credit Spread Change in the Two Years after the Buyout

The outcome measures, samples, weighting method, and regression specifications in this table follow Table 2 except for two extra explanatory variables in each regression: the change in the credit spread in the two years after buyout close and its interaction with the buyout indicator. For each outcome measure, table entries report the estimated coefficient on the interaction variable, its estimated standard error, and the coefficient multiplied by the sample standard deviation of the Credit Spread. This standard deviation ranges from 4.3 to 4.9 across the regression samples. The final column presents the p-value from F-tests of the equality of the coefficients of the three reported buyout type variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

<i>Dependent Variable</i>		All Buyouts	Private-to- private	Public- to- private	Divisional	p-Value, F-test
A. Employment Growth, All Margins	Coefficient	-0.62**	-1.22***	-0.54	0.61	0.09
	[St. Error]	[0.30]	[0.46]	[0.35]	[0.71]	
	Unit S.D. Effect	-3.0	-5.8	-2.2	2.1	
Organic Margins	Coefficient	-0.27	0.10	-0.36	0.48	0
	[St. Error]	[0.26]	[0.23]	[0.30]	[0.61]	
	Unit S.D. Effect	-1.3	0.47	-1.5	1.6	
B. Excess Reallocation, All Margins	Coefficient	-0.61***	-0.33	-0.43**	-0.97**	0.75
	[St. Error]	[0.17]	[0.24]	[0.21]	[0.40]	
	Unit S.D. Effect	-3.0	-1.6	-1.8	-3.3	
C. Labor Productivity	Coefficient	-1.94**	0.22	-6.93**	-0.94	0.01
	[St. Error]	[0.87]	[0.37]	[2.81]	[0.91]	
	Unit S.D. Effect	-10.5	1.2	-35.9	-2.4	

Table D.11. How Buyout Effects Vary with the GDP Growth Rate in the Two Years after the Buyout

The outcome measures, samples, weighting method, and regression specifications in this table follow Table 2 except for two extra explanatory variables in each regression: the GDP Growth Rate in the two years after the buyout close and its interaction with the buyout indicator. For each outcome measure, table entries report the estimated coefficient on the buyout-GDP interaction variable, its standard error, and the coefficient multiplied by the sample standard deviation of the GDP Growth Rate, which ranges from 3.4 to 3.6 across the regression samples. The final column presents the p-value from F-tests of the equality of the coefficients of the three reported buyout type variables. Huber-White robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.10.

<i>Dependent Variable</i>		All Buyouts	Private-to- private	Public-to- private	Divisional	p-Value, F-test
A. Employment Growth, All Margins	Coefficient	1.35**	0.56	-0.15	2.6**	0.14
	[St. Error]	[0.54]	[0.71]	[0.73]	[1.20]	
	Unit S.D. Effect	4.5	1.9	-0.4	9.2	
Organic Margins	Coefficient	0.59	-1.06***	-0.10	1.70*	0
	[St. Error]	[0.40]	[0.35]	[0.51]	[0.90]	
	Unit S.D. Effect	2.0	-3.6	-0.3	5.9	
B. Excess Reallocation, All Margins	Coefficient	0.93***	-0.40	0.77**	1.74***	0.09
	[St. Error]	[0.28]	[0.56]	[0.31]	[0.63]	
	Unit S.D. Effect	3.1	-1.4	2.1	6.0	
C. Labor Productivity	Coefficient	2.10*	-0.41	8.49**	1.78	0.27
	[St. Error]	[1.14]	[0.57]	[3.99]	[1.65]	
	Unit S.D. Effect	7.3	-1.8	27.6	6.4	

Table D.12. How Buyout Effects Vary with NBER Recessions

This table considers the same outcome measures, estimation method, and samples as Tables D.10 and D.11, but we modify the regression specification (3) in the paper to instead include interactions between the buyout indicator and the inception of such a recession within two years of the transaction instead of interactions with market conditions using the Credit Spread or GDP Growth variable. For the 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.

<i>Dependent Variable</i>		<i>Interaction Variable Recession Afterwards</i>
A. Employment Growth, All Margins	Coefficient	-13.17***
	[St. Error]	[3.97]
Organic Margins	Coefficient	-8.41**
	[St. Error]	[3.31]
B. Excess Reallocation, All Margins	Coefficient	-13.00***
	[St. Error]	[3.04]
C. Labor Productivity	Coefficient	-11.21
	[St. Error]	[9.26]

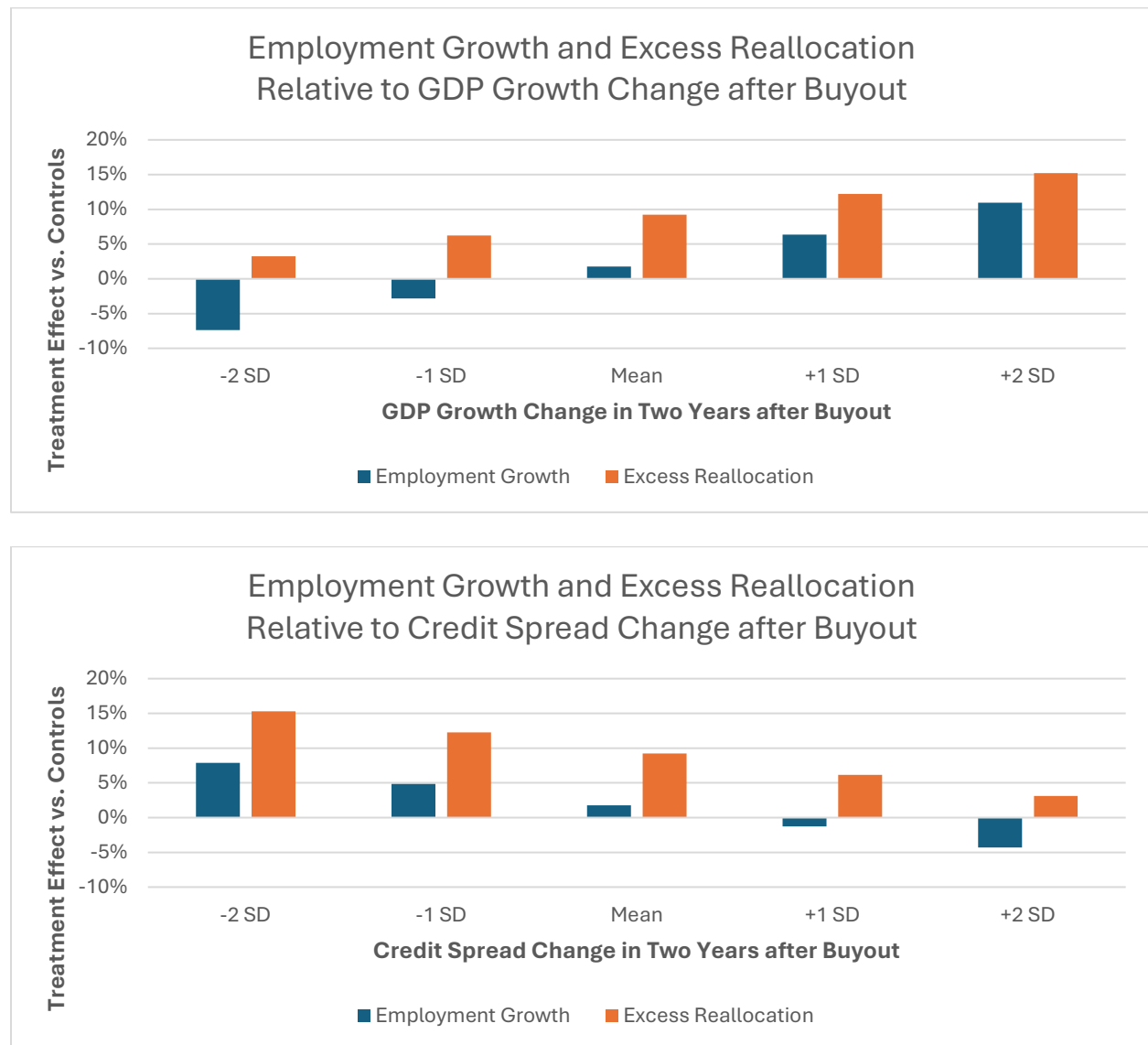
Table D.13. The Impact of Excluding Deals Most Impacted by the Global Financial Crisis

This table considers the same outcome measures, estimation method, and samples as the first column in Tables D.10 and D.11, 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.

<i>Dependent Variable</i>		<i>Table D.10 Credit Spread</i>	<i>Table D.11 GDP Growth</i>
A. Employment Growth, All Margins	Coefficient	0.97	0.86
	[St. Error]	[1.10]	[0.76]
Organic Margins	Coefficient	1.39*	0.02
	[St. Error]	[0.80]	[0.52]
B. Excess Reallocation, All Margins	Coefficient	-0.62	0.54
	[St. Error]	[0.49]	[0.44]
D. Labor Productivity	Coefficient	-1.65	-0.78
	[St. Error]	[2.54]	[1.00]

Figure D.1. How Buyout Effects Vary with the Post-Buyout Evolution of Market Conditions

This figure uses the estimated interaction effects in Table 3 to depict how the post-buyout employment growth rate and excess reallocation rate at targets (relative to controls) vary with the post-buyout evolution of market conditions. The center bars show the estimated target-control differential when evaluating at the sample mean of the market condition measures. The other bars show the target-control differential when evaluating the market condition measures at -2, -1, +1, and +2 standard deviations below or above their respective sample means.



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