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PRIVATE EQUITY, JOBS, AND PRODUCTIVITY

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ABSTRACT

Private equity critics claim that leveraged buyouts bring huge job losses and few gains in operating performance. To evaluate these claims, we construct and analyze a new dataset that covers U.S. buyouts from 1980 to 2005. We track 3,200 target firms and their 150,000 establishments before and after acquisition, comparing them to controls defined by industry, size, age, and prior growth. Relative to controls, employment at target establishments falls 3 percent over two years post buyout and 6 percent over five years. However, target firms also create more new jobs at new establishments, and they acquire and divest establishments more rapidly. Considering all adjustment margins, relative net job loss at target firms is a modest one percent of employment over two years post buyout. In contrast, the sum of gross job creation and destruction at target firms exceeds that of controls by 14 percent of employment over two years. Buyouts also bring TFP gains at target firms and reductions in earnings per worker. Productivity gains arise mainly from an accelerated exit of less productive establishments and greater entry of more productive ones – that is, from a directed reallocation of jobs within target firms.

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1. Introduction

Leveraged buyouts by private equity firms arouse intense concern and strongly held views. For instance, former Danish Prime Minister Poul Rasmussen – architect of the European Commission's Alternative Investment Fund Managers Directive – contends that "'leveraged buy-outs' leave the company saddled with debt and interest payments, its workers are laid off, and its assets are sold, … benefiting neither workers nor the real economy." The Service Employees International Union, prominent critic of private equity on both sides of the Atlantic, offers this assessment: "Typically it's easier to decrease costs quickly by cutting heads, which is why buyouts have typically been accompanied by layoffs."[1] Responding to similar contentions, several industry-sponsored studies claim positive employment and other effects of private equity. Examples include European Venture Capital Association (2005), British Venture Capital Association (2006), A.T. Kearney (2007), and Shapiro and Pham (2008).

Efforts to bring data to these issues are highly welcome, but these studies have serious limitations. First, they rely on surveys with incomplete and perhaps selective responses, raising doubts about representativeness and accuracy. Second, the underlying data offer little scope to control for concurrent changes at comparable firms. When a firm backed by private equity sheds 5% of employment, the interpretation depends greatly on whether comparable firms grow by 3% or shrink by 10%. Third, these studies do not distinguish cleanly between changes at firms backed by venture capital and firms backed by other forms of private equity. Both are interesting, but the controversy involves buyouts, not venture capital. Fourth, these studies face major challenges in measuring organic job growth because they lack establishment-level data. As a result, it is hard to disentangle organic changes from the acquisition and sale of particular

[1] See Rasmussen (2008) and remarks attributed to John Adler in Grace Wong, "Private Equity and the Jobs Cut Myth", *CNNMoney.com,* 2 May 2007 at http://money.cnn.com/2007/05/02/markets/pe_jobs/index.htm (accessed August 25, 2011).

facilities and business units. Fifth, the lack of establishment-level data also precludes a breakdown of firm-level employment changes into job creation and job destruction components, i.e., gains and losses at the establishment level. As we show, private equity buyouts have quite different effects on these two margins of employment change.[2]

In this study, we construct and analyse a dataset that overcomes these limitations and, at the same time, encompasses a much larger set of private equity buyouts and controls. We rely on the Longitudinal Business Database (LBD) at the U.S. Census Bureau to track employment and earnings before and after buyouts at firms *and* establishments – i.e., specific factories, offices, retail outlets and other distinct physical locations where business takes place. The LBD covers the entire nonfarm private sector and contains annual data for about 5 million firms and 6 million establishments. To obtain high-quality productivity measures, we turn to the Annual Survey of Manufactures (ASM) and the Census of Manufactures (CM). In addition to their other strengths, the establishment-level information in the LBD, ASM and CM enables us to explore important aspects of within-firm restructuring activity in the wake of private equity buyouts.

We combine the LBD, ASM and CM with data from CapitalIQ and other sources to identify and characterize private equity transactions. The resulting matched sample contains about 3,200 U.S. firms acquired in buyouts from 1980 to 2005 ("target firms") and 150,000 U.S. establishments operated by these firms as of the buyout year ("target establishments"). We match each target firm to other firms that are comparable in terms of industry, age, size, and single/multi-establishment status, and then follow targets and matched controls over time. We take a similar approach to controls for target establishments.

[2] See Service Employees International Union (2007) and Hall (2007) for other critiques. We discuss the broader academic literature on the economic effects of private equity in Section 2. Few academic studies of private equity focus on employment outcomes, and the main exceptions consider data for France and the United Kingdom. To our knowledge, no previous studies exploit linked firm-level and establishment-level data to examine the within-firm reallocation effects of private equity buyouts and their relationship to productivity gains at target firms.

To clarify the scope of our study, we consider later-stage changes in ownership and control executed and partly financed by private equity firms. In these transactions, the (lead) private equity firm acquires a controlling stake in the target firm and retains a significant oversight role until it "exits" by selling its stake. The buyout event typically involves a shift toward greater leverage in the capital structure of the target firm and, sometimes, a change in its management. We exclude management-led buyouts that do not involve acquisition by a private equity firm. We also exclude startup firms backed by venture capitalists.

Our establishment-level analysis yields three main findings: First, employment shrinks more rapidly, on average, at target establishments than at controls after private equity buyouts. The average cumulative difference in favor of controls is about 3% of initial employment over two years and 6% over five years. Second, the larger post-buyout employment losses at target establishments entirely reflect higher rates of job destruction at shrinking and exiting establishments. In fact, targets exhibit *greater* post-buyout creation of new jobs at expanding establishments. Adding controls for pre-buyout growth history shrinks the estimated employment responses to private equity buyouts but does not change the overall pattern. Third, earnings per worker at continuing target establishments fall by an average of 2.4% relative to controls over two years post buyout.

The establishment-level analysis misses job creation at newly opened establishments, whether by target or control firms. To capture this activity, we move to a firm-level analysis and identify new establishments opened post buyout. The combination of firm and establishment data in the LBD is what enables us to isolate and quantify "greenfield" job creation at facilities opened post buyout. For this part of our analysis, we shorten the time window to two years post buyout. Lengthening the window involves a greater incidence and complexity of ownership changes, threatening the integrity of our firm-level longitudinal linkages or forcing us to narrow

the sample. We find that target firms engage in more greenfield job creation than control firms, with a cumulative two-year difference of nearly 2% of initial employment. That is, greater greenfield job creation partly offsets the relative employment drop at target establishments. Our firm-level analysis also yields another interesting result: Private equity targets engage in more acquisitions and more divestitures than controls. Summing over job creation and destruction at continuing establishments, job losses at shuttered establishments, job gains at greenfield establishments, and contributions of acquisitions and divestitures, employment shrinks by less than 1% at target firms relative to controls in the first two years after private equity buyouts.

We uncover a much larger response in the pace of job reallocation. Specifically, over the first two years post buyout, establishment-level job gains and losses at target firms exceed gains and losses at controls by 14% of initial employment. This extra job creation and destruction activity amounts to 25% of baseline job reallocation at control firms. A more rapid pace of organic job creation and destruction accounts for 45% of the extra reallocation activity at target firms, and greater acquisitions and divestitures account for the rest. These results indicate that private equity buyouts catalyze the creative destruction process, as measured by job creation and destruction and by the transfer of production units between firms.

Our productivity findings reinforce this view. Specifically, compared to control firms, target firms more aggressively close manufacturing plants in the lower part of the total factor productivity (TFP) distribution. They also open new plants in the upper part of the TFP distribution at nearly twice the rate of control firms, and they are much less likely to open low productivity establishments. On average, over the first two years post buyout, we estimate that private equity buyouts raise TFP by 2.1 log points. (Baseline TFP growth for controls is slightly negative.) Three quarters of the post-buyout TFP gains reaped by target firms reflect a greater propensity to close low productivity plants and to open new, high productivity plants. In short,

buyouts improve productivity mainly through the directed reallocation of resources across units within target firms. These TFP results and our results on worker earnings imply that private equity buyouts materially improve operating margins at target firms.

The next section briefly reviews related research and offers additional motivation for our study. Section 3 describes the construction of our analysis datasets, and Section 4 explains our empirical methods. Sections 5 and 6 present our main establishment-level and firm-level analyses of employment and job reallocation effects. Section 7 considers effects on TFP and earnings per worker, and Section 8 offers concluding remarks.

2. Related Work and Additional Motivation

Economists hold a longstanding interest in how ownership changes affect productivity and employment. Examples include Lichtenberg and Siegel (1987), Long and Ravenscraft (1993), McGuckin and Nguyen (2001), and Harris, Siegel and Wright (2005). One ownership change that attracts particular attention is the acquisition of firms by professional private equity firms. Jensen (1989) and Shleifer and Summers (1988), among others, discuss the economic effects of private equity buyouts based largely on case study evidence. Kaplan and Strömberg (2009) provide a useful overview of research on the economic effects of private equity.

Few previous studies focus on the employment effects of private equity, and the exceptions typically rely on small samples dictated by data availability. Kaplan (1989) considers 76 public-to-private leveraged buyouts (LBOs) during the 1980s. He finds that the median firm lost 12% of employment on an industry-adjusted basis from the end of the fiscal year prior to the private equity transaction to the end of the fiscal year after the transaction. After dropping target firms with asset sales or purchases that exceed 10% of total value, the adjusted employment decline is 6.2% for the remaining 24 firms. Muscarella and Vetsuypens (1990) consider 72 firms

that completed an initial public offering (IPO) after an LBO between 1983 and 1987. For the 26 firms they can track, employment declines by an average of 0.6% between the LBO and the IPO.

Lichtenberg and Siegel (1990) use U.S. Census Bureau data to examine changes in employment at the manufacturing plants of 131 firms undergoing buyouts between 1981 and 1986. On an industry-adjusted basis, employment falls by 1.2% per year after buyout, as compared to a 1.9% rate of decline beforehand. Declines are larger for non-production workers than blue-collar workers. Wright, Thompson and Robbie (1992) and Amess and Wright (2007) similarly find that buyouts in the UK lead to modest employment declines. These studies follow overall employment at a set of firms, and contrast it with aggregate employment at matching firms.[3] Boucly, Sraer and Thesmar (2009) find that employment grows much more rapidly at target firms than at controls in the wake of French private equity buyouts, a result they attribute to an important role for private equity in relaxing financing constraints.

These studies share certain weaknesses. First, most focus on the company-wide employment of firms backed by private equity. Thus, the sale of a division or other business unit is typically counted as an employment loss, even if the sold business unit continues with the same number of employees under new ownership. Likewise, the acquisition of a division or other business unit is counted as an employment gain, even if there is no employment change at the business unit itself. Several studies attempt to address this issue by dropping buyouts that involve substantial asset sales, but this sample restriction may greatly influence the results, given the extent of "asset shuffling" by firms backed by private equity. The handful of previous U.S. studies that treat establishments as the unit of observation are typically restricted to the manufacturing sector, and even then have limited ability to track establishment or firm closings.

[3] The samples in these UK studies include management-led deals (management buyouts), which need not involve a financial sponsor that acquires a controlling stake in the target firm. Management-led deals potentially differ substantially from the traditional private equity buyouts that we consider.

Second, previous studies of U.S. private equity deals rely on highly selected samples – potentially an important source of bias in the findings. The public-to-private buyouts that dominate earlier samples account for less than one-quarter of the employees directly impacted by private equity buyouts and only 12 percent of the deals (Table 2 below). Most previous U.S. studies consider deals before the 1990s only, but fundraising by U.S. private equity groups rose 36-fold from 1985 to 1998 and more than 100-fold by 2006.[4] The tremendous growth in private equity activity allows us to examine a much larger set of deals. Moreover, the nature of private equity activity has also changed over time – competition for attractive deals has intensified, and many private equity firms now have a strong operational orientation, as opposed to the financial engineering approach that characterized many groups during the 1980s.

Our study overcomes these weaknesses, as we have explained. In addition, we exploit the establishment-level aspect of our data to examine job creation and job destruction outcomes, as well as net employment changes. In this regard, we are motivated in part by previous work that documents a rapid pace of establishment-level job creation and destruction. Davis and Haltiwanger (1999) review work in this area. Earlier empirical work also shows that the reallocation of jobs and workers across establishments plays a major role in medium-term productivity gains. Many important theoretical models also feature distinct roles for the creation and destruction margins of employment adjustment. Caballero (2007) provides an insightful, detailed analysis and extensive references to the relevant literature.

Numerous case studies provide detailed descriptions and analyses of particular private equity deals. By our reading, these studies deliver four sets of insights.

First, private equity groups sometimes generate few or no productivity gains because they fail to achieve their goals for target firms. For instance, when Berkshire Partners bought

[4] Using inflation-adjusted dollars and data from Thomson Reuters VentureXpert, http://www.venturexpert.com (accessed August 20, 2011).

Wisconsin Central, it had an ambitious plan to raise productivity. However, technological problems arose soon after the buyout and prevented the deployment of a computerized control system that was crucial for planned cost savings. As a result, the numbers in an ambitious business plan were never met (Jensen, Burkhardt and Barry, 1990). In the Revco transaction a crippling debt load, management disarray, an inexperienced LBO sponsor, and a disastrous midstream shift in strategy led to a failure to achieve performance goals (Wruck, 1991).

Second, the Revco case also points to tax savings as the primary source of private value creation in certain buyouts. Consistent with this view, Kaplan (1989b) provides evidence that greater leverage and other organizational shifts imposed by private equity investors can yield substantial tax savings that are "an important source of the wealth gains in leveraged buyouts." If tax savings are the principal motive for buyouts, there is no compelling reason to anticipate positive effects on productivity at target firms.

Third, many case studies find substantial productivity gains at target firms through improvements to existing operations. In the Hertz buyout, for instance, Clayton, Dubilier and Rice (CD&R) addressed operational inefficiencies to increase profitability. Specifically, CD&R lowered overhead costs by reducing inefficient labor expenses and cutting non-capital investments to industry standard levels, and more closely aligned managerial incentives with return on capital (Luehrman, 2007). Similarly, the buyout of O.M. Scott & Sons led to substantial improvements in the firm's existing operations, partly through powerful incentives offered to management and partly through specific suggestions by the private equity investors (Baker and Wruck, 1989). In examples like these, profitability increases and private value creation are likely to go hand in hand with productivity gains.

Finally, in a number of other cases, private equity targets achieved substantial efficiency improvements not by enhancing existing operations, but rather by divesting units. Beatrice had

acquired a large number of unrelated businesses as part of a conglomerate strategy, many of which operated in segments in which it had little expertise. Its private equity investor, Kohlberg, Kravis and Roberts, divested many of these laggard operations (Baker, 1992). Similarly, the buyout group that purchased Kaiser Steel shut down its outdated and inefficient steel operations. The group focused its operational attention on Kaiser's coalmines, which it regarded as the firm's "hidden jewel" (Luehrman, 1992). Greater profitability and private value creation are also likely to involve productivity gains in these examples, though mainly through productivity-enhancing reallocation rather than operational improvements within continuing units.

These case studies illustrate a wide range of motives for and effects of private equity transactions. Our study can be seen as an effort to determine which of these stories best characterizes the impact of private equity buyouts on average, especially with respect to employment and productivity outcomes.

3. Constructing the Analysis Samples

Our analysis requires a comprehensive database of private equity transactions and the matching of target firms to firm-level and establishment-level records in the LBD, ASM and CM. This section describes the data construction process and the resulting samples.

A. Identifying private equity buyout transactions

CapitalIQ has specialized in tracking private equity deals on a worldwide basis since 1999 and, through extensive research, backfilled transactions prior to 1999.[5] We consider all recorded transactions in CapitalIQ that closed between January 1980 and December 2005. We then impose two sample requirements. First, we omit transactions that do not involve a financial sponsor, i.e., a private equity firm. Second, we restrict attention to transactions that entail some

[5] Most data services tracking private equity transactions were not established until the late 1990s. The most comprehensive exception, SDC VentureXpert, mainly focused on capturing venture capital transactions until the mid-1990s. See Stromberg (2007) for a discussion of the completeness of the CapitalIQ database.

use of leverage. Many transactions that do not involve leverage are venture capital investments rather than investments in mature firms. Given our focus, we omit transactions not classified by CapitalIQ as "going private," "leveraged buyout," "management buyout," "platform," or a similar term. This approach excludes "growth buyouts" and "expansion capital" investments to purchase a minority stake using little or no leverage. Such transactions may share some characteristics of private equity deals but do not fit the classic profile of leveraged buyouts.

After restricting the sample in these two ways, the resulting database contains about 11,000 transactions worldwide. Dropping transactions that involve firms with foreign headquarters leaves about 5,000 U.S. target firms acquired in private equity buyouts between 1980 and 2005. (We do not consider U.S. establishments operated by foreign targets.) To fill out our information about private equity transactions and target firms, we supplement the data drawn from CapitalIQ with data from Dealogic, Thomson Reuters SDC, VentureXpert databases, and news stories. Dealogic, in particular, often contains greater detail about transaction characteristics. Other useful information in the supplementary sources includes alternative names associated with target firms and their later acquisitions and sales.

B. Matching to LBD Records

The LBD derives from the Census Bureau's Business Register, which contains annual data on U.S. businesses with paid employees. The LBD covers the entire nonfarm private sector from 1976 to 2005. In recent years, it contains over 6 million establishment records and almost 5 million firm records per year. The Business Register and the LBD draw on administrative records and survey sources for data on firms and their establishments. Core data items include employment, payroll, four-digit Standard Industrial Classification (SIC) or six-digit North American Industrial Classification (NAICS), employer identification numbers, business names,

and information about location.[6] Identifiers in the LBD files enable us to compute growth rate measures for establishments and firms and to track their entry and exit and ownership changes. Firms in the LBD are defined based on operational control, and all establishments majority owned by a parent firm are included in the parent's activity measures.

To merge data on buyouts into the LBD, we match names and addresses of private equity portfolio firms (i.e., target firms) to LBD name and address records. To cope with timing differences between datasets, we search over a three-year window in the LBD centered on the transaction year for each target firm. We adopt a conservative approach to matching that requires either an exact match on name and address or an approximate match on both name and address according to probability-based matching algorithms. Our procedures match about 65% of target firms to the LBD, 70% on a value-weighted basis, yielding about 3200 matched target firms. Once matched, firm-establishment links in the LBD serve to identify all establishments owned by target firms as of the private equity buyout year. Matched target firms operate about 150,000 U.S. establishments as of the buyout year. LBD longitudinal links allows us to follow firms and establishments over time. Tracking firms is more challenging, as we discuss below, which influences the design of our firm-level analysis sample.

Given our interest in employment dynamics, the relationship of the LBD employment measure to the timing of private equity transactions requires careful treatment. The LBD reports total employment in the payroll period containing the week of March 12th. Accordingly, for buyout transactions that close before October 1, LBD employment in March of the same calendar year serves as our contemporaneous employment measure. We assign transactions that close on or after October 1 in year t to year $t+1$ for purposes of our analysis, treating the LBD

[6] Sales data in the Business Register are available annually from 1994 and once every five years in earlier years.

employment value in March of $t+1$ as the contemporaneous measure. October is the natural cutoff because it lies midway between March-to-March employment changes in the LBD.

Figure 1 shows the number of U.S. target firms acquired by year and the number matched to the LBD. It is apparent that the number of private equity buyouts grew rapidly beginning in the mid-1990s. Table 1 shows the enterprise value (debt plus equity, as valued at the time of the transaction) of all private equity targets and the matched targets by decade. The enterprise value of private equity acquisitions is very large in the later years, reaching 420 billion in the 2000-2005 period. Figure 2 displays employment data for our matched target firms. For example, target firms acquired in 2005 and matched to the LBD account for 0.83% of total nonfarm business employment in 2005. Given the extent of unmatched targets, the full set of firms that came under private equity control in 2005 accounts for more than 1% of private sector employment. Based on our data, we infer that more than 7% of private employment came under private equity control at some point in the ten-year period from 1998 to 2007.[7]

C. Analysis Samples

Our study considers several related analysis samples. For descriptive statistics on the number and volume of private equity buyouts and their distribution by industry and firm characteristics, we consider all matched targets through 2005. For our main establishment-level analysis, we consider buyouts from 1980 to 2000. This sample allows us to track target (and control) establishments for five years before and after the buyout year. For our firm-level analysis sample, we consider buyouts from 1980 to 2003, so that we can track firm-level outcomes for two years post buyout. We also consider various subsamples, the most important of which focus on buyouts in the manufacturing sector from 1980 to 2003. For the manufacturing

[7] We arrive at this inference by summing the employment percentages of matched targets from 1998 to 2005, dividing the sum by 0.7 to account for unmatched targets, and making the assumption (supported by other data sources) that private equity activity continued at record levels in 2006 and the first half of 2007.

subsample, we draw on ASM and CM data to construct plant-level TFP measures. Table 2 reports summary statistics on matched targets for our analysis samples.

4. Empirical Methods

This section describes key methodological choices in our empirical study. The first relates to the unit of analysis. Section 5 considers establishments owned by target firms in the buyout year. We track these units over time, irrespective of their ownership in earlier or later years. For example, if the target firm goes public or sells an establishment, we continue to track that establishment and associate it with the buyout event. Section 6 takes a different approach, treating the firm as the unit of analysis, which lets us capture greenfield job creation and the acquisition and sale of establishments after the buyout event.

The second key choice relates to controls. We need suitable controls because the distribution of private equity buyouts across industries and business characteristics is not random. For example, practitioner accounts often suggest a concentration of transactions in industries undergoing significant restructurings due to regulatory action, foreign competition, or technological change. Target firms in our data are disproportionately concentrated in manufacturing, information services, and accommodation and food services, as shown in Figure B.1 of the web appendix. Target firms are also considerably larger and older than the average firm, as shown in Figure B.2.[8] The literature on firm dynamics concludes that growth and volatility vary systematically with firm size and age. See, for example, Caves (1998), Davis et al. (2007), and Haltiwanger, Jarmin and Miranda (2010). Thus, it is important to control for these characteristics when evaluating the reallocation and other effects of private equity buyouts.

[8] Firms with 500 or more employees account for 96% of employment at matched targets, as compared to 51% of all LBD employment in the 1980-2005 period. Firms 10 years or older account for 91% of employment at matched targets as compared to 64% in the LBD.

The huge number of firms and establishments in the LBD allows us to control for a full set of interactions among industry, size, age, multi-unit status, and year of the buyout transaction. We sort target firms into cells defined by the cross product of these characteristics.[9] We then identify all firms in the LBD not backed by private equity that fall into the same cell as a given target firm, and we treat those firms as controls. Specifically, we control for the interaction of 72 two-digit industries, 10 firm size categories, 6 firm age categories, a dummy for firms with multiple establishments, and 24 distinct transaction years. The cross product of these categorical variables yields over 8,000 control cells per year. Of course, many cells are unpopulated, but the richness of our controls is evident. In our regression analysis, we also control for pre-buyout employment growth histories. We follow the same approach in the establishment-level analysis. To obtain controls for a given target establishment, we select all establishments in the same control cell from among the set of active establishments in the transaction year, excluding establishments owned by a firm under private equity control.

A related choice involves our statistical approach to estimating the effects of buyouts on employment outcomes. We consider nonparametric comparisons that control for the cross-product of our categorical variables, semi-parametric regressions that include additional controls, and propensity score methods. Ideally, we seek to estimate the average treatment effect on the treated, i.e., the average effect of buyouts on target firms. As discussed in Woolridge (2002, chapter 18), consistent estimation of average treatment effects requires conditional mean independence: conditional on the controls and the treatment indicator, outcomes for the treated and non-treated are independently distributed. Compared to previous research, our rich set of controls lends greater plausibility to this identifying assumption. Even if one questions the

[9] We define industry for multi-unit firms based on the modal industry of their establishments, computed on an employment-weighted basis.

conditional mean independence assumption, our study yields a rich set of new findings about outcomes at private equity targets. These findings throw light on alternative views about the economic role of private equity, as we discuss below. Our findings also provide useful evidence for formulating and evaluating theoretical models of private equity behavior and effects.

A fourth choice relates to the time window around private equity transactions. Our establishment-level analysis considers employment outcomes for five years on either side of a private equity transaction. Five years is a typical holding period for target firms (Stromberg 2007). For our firm-level analysis, we must confront the reorganization of firms through mergers, ownership changes, partial divestitures, and acquisitions of establishments from other firms. Because it tracks both firms and establishments over time and contemporaneously links establishments to firms, the LBD offers greater scope for identifying these changes than most other business-level datasets. Nevertheless, some private equity targets undergo complex post-buyout restructurings that challenge the maintenance of high-integrity longitudinal links. We deal with this challenge in two ways. First, our firm-level analysis considers a relatively short window of two years after each buyout transaction, thereby limiting the linkage issues that arise from complex firm-level reorganizations. Second, we use our establishment-level data to assess the impact of potential sample selection bias in our firm-level analysis.

Before proceeding, we define our employment and growth rate measures. Let E_{it} be employment at establishment or firm *i* in year *t*; i.e., the number of workers on the payroll in the pay period covering March 12. The employment growth rate is $g_{it} = (E_{it} - E_{it-1})/X_{it}$, where $X_{it} = .5*(E_{it} + E_{it-1})$.[10] Employment growth at any higher level of aggregation is the weighted

[10] 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 accommodating entry and exit. See Davis, Haltiwanger and Schuh (1996) and Tornqvist, Vartia, and Vartia (1985) for discussion.

mean of establishment or firm growth rates given by $g_t = \sum_i (X_{it} / X_t) g_{it}$, where $X_t = \sum_i X_{it}$.

We consider the contributions of expanding and shrinking establishments, establishment entry and exit, and acquisitions and divestitures to firm-level employment changes, and compare outcomes between targets and controls on each of these adjustment margins.

5. Establishment-Level Analysis of Employment Outcomes

A. Nonparametric comparisons

We begin with an "event study" that compares employment outcomes at target establishments to outcomes at control establishments. To encompass a window of five years before and after buyouts, we consider transactions in the 1980-2000 period. As discussed above, we construct control cells as the cross product of industry, size of parent firm, age of parent firm, multi-unit status, and buyout year. Our firm size categories are 1-4, 5-9, 10-19, 20-49, 50-99, 100-249, 250-499, 500-999, 1000-2499, 2500-4999, 5000-9999, and 10000 or more employees. Our firm age categories are 0-5 years, 6-10, 11-15, 16-20, and 21 or more years.[11] We use firm size and age measures to facilitate comparisons to our firm-level analysis below. Replacing firm size and age measures with measures based on establishment size and age yields similar results.

The solid curve in Figure 3a shows the employment path of target establishments around the buyout year. Establishments that came under private equity ownership between 1980 and 2000 employed 2.3 million workers as of the buyout year. The dashed curve shows the counterfactual path of employment at targets had they grown at the same rate as controls. To construct this counterfactual, we first rescale the employment of controls to match that of targets cell by cell in the buyout year. We then apply the actual growth rates of the controls to generate

[11] Following Davis et al. (2009), 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.

the dashed curve.[12] Comparing the solid and dashed curves highlights the critical need to evaluate target outcomes relative to controls. In particular, a simple comparison of outcomes at target firms before and after buyout events would produce a highly misleading impression about the employment effects of private equity.

Figure 3b tracks mean employment growth rate differences between target and control establishments from 5 years before to 5 years after the buyout year. Perhaps surprisingly, Figure 3b shows no systematic pattern of slower job growth at targets in the years leading up to buyouts. In the buyout year itself, employment growth at targets is actually 2 percentage points higher than at controls. However, there is a clear pattern of slower growth at targets post buyout, with growth differentials ranging from 0.5% to 2% per year. The differentials cumulate to 3.2% of employment in the first two years post buyout and 6.4% over five years. These results accommodate heterogeneous treatment effects over the cross product of industry, firm size, firm age, multi-unit status, and year of buyout. They recover the average treatment effect on the treated under the assumption of conditional mean independence, as we discussed above.[13]

Previous research finds large gross job flows relative to net employment changes (Davis and Haltiwanger, 1999), raising the question of how employment responds to private equity buyouts on job creation and destruction margins. Figure 4 tells an important story in this regard: Slower employment growth at private equity targets post buyout entirely reflects a greater pace of job destruction. Indeed, gross job creation rates are greater at target establishments in the wake of buyouts. These results are interesting for at least two reasons. First, they indicate that

[12] To be precise, we calculate the weighted mean growth rate over cells using the weights defined at the end of Section 4. The cell-level weights evolve over time in line with the growth experiences of targets (solid curve) and controls (dashed curve). For cells with multiple controls, each control receives equal weight.

[13] To be sure, consistent estimation of treatment effects also rests on the stable unit treatment value assumption (SUTVA): applying the treatment to one unit has no effect on outcomes at other units. This assumption fails if, for example, treatment effects on targets systematically alter equilibrium output and employment at controls. Given that controls greatly outnumber targets in our setting, equilibrium effects of this sort are unlikely to matter much. Moreover, the productivity effects we estimate below work to offset any output changes implied by the estimated employment effects on target firms, further lessening the scope for equilibrium employment effects on controls.

buyouts accelerate the pace of employment change on destruction *and* creation margins, a theme we return to below. Second, Figure 4b confirms that jobs at target establishments are at greater risk post buyout than jobs at controls. As seen in Figure 5, about half of this greater risk reflects a higher post-buyout shutdown propensity at target establishments.

B. Regression Analysis

We turn now to a regression analysis that allows for additional controls and an easy calculation of standard errors in the estimated effects of private equity buyouts. Table 3 reports regression results for the buyout year and five subsequent years. Each regression involves the matched target establishments in buyouts from 1980 to 2000 and their corresponding control establishments. The dependent variable is the employment growth rate in the indicated year following the buyout. The first column in Table 3 reports the mean growth rate differentials from Figure 3b. The second and third columns report results for semi-parametric regressions that include controls for the pre-buyout growth history of parent and target firms.

We include two variables to control for pre-buyout history. One variable considers the set of establishments owned by the target firm as of year 0 (the buyout year). We set the value of this variable to the employment growth rate of these establishments from year -3 to year -1. A second variable considers the parent firm that owned these establishments in year -3. If ownership was split across multiple firms in year -3, we select the firm with the largest share of employment among these establishments. We then set the second variable to the employment growth rate of that firm from year -3 to year -1. Often, but not always, these two control variables take on the same value.

The Table 3 regressions contain a large battery of additional controls. The column headed "ATE=ATE1" includes a fully interacted set of controls for two-digit industry, firm size, firm age, multi-unit status, and year. This specification posits a common treatment effect, given by

the coefficient on an indicator variable for target establishments in private equity buyouts. The column headed "ATE1 Heterogeneous" includes the same set of controls, but relaxes the assumption of uniform treatment effects by interacting the private equity indicator with the 6 firm age categories, 10 firm size categories, and the two measures of pre-buyout growth history.[14] This specification is more restrictive than the nonparametric specification in some respects but less restrictive in the inclusion of controls for pre-buyout growth history and in allowing the treatment effect to vary with pre-buyout employment growth. To recover the average treatment effect on the treated in this case, we compute a weighted average of the heterogeneous estimated treatment effects, using cell-level employment weights of targets in the transaction year. We calculate standard errors by the Delta method.

As seen in Table 3, the nonparametric and semi-parametric specifications deliver similar results. The two semi-parametric regressions also yield small standard errors and tightly estimated effects of private equity buyouts. Five-year cumulative employment losses at targets range from -4.7% to -6.4%, depending on specification, with somewhat smaller losses in the semi-parametric specifications.[15] In short, the evidence says that target establishments experience deeper job losses after private equity buyouts than control establishments.

6. Firm-Level Analysis of Employment Outcomes

A. Tracking Firms

Section 5 considers outcomes for establishments owned by target firms at the time of the buyout deal. We now shift to a firm-level analysis to capture new establishments opened after the deal as well as post-buyout acquisitions and divestitures. By necessity, we restrict attention to

[14] See the web appendix for explicit statements of the regression specifications in mathematical form.

[15] Smaller losses in the semi-parametric specifications point to a modest tendency for private equity to target firms with weaker employment growth prospects, which differs somewhat from the inference suggested by the pre-buyout comparison in Figure 3b. Recall that Figure 3 involves a comparison of growth rates between target and control *establishments*. In contrast, the semi-parametric regressions reported in Table 3 contain controls for the pre-buyout growth history of *parent firms*.

target firms that we can track post buyout. While we can readily track establishments over time in the LBD, tracking firms is more challenging for two reasons: the disappearance of firm identifiers (IDs) in some circumstances, and irregularities in Census Bureau tracking of private equity targets involved in divisional sales.

The disappearance of a firm identifier (ID) in the LBD can occur for various reasons. One is the death of a firm and closure of its establishments. Firm death in this sense presents no problem for our analysis, and we capture such events whether they involve target or control firms. A more difficult situation involves a firm ID in year 0 that disappears in later years, even though some of the establishments owned by the firm in year 0 continue to operate. This situation can arise because of a merger or complex reorganization (e.g., different components of the original firm are bought by multiple existing firms). It is inherently difficult to define and measure firm growth when the original legal entity ceases to exist, and we exclude these observations in our firm-level analysis. To reduce the number of observations lost for this reason, we limit our firm-level analysis to years 1 and 2 after the buyout.

In the course of our data development and analysis, we discovered that the Census Bureau did not accurately track firm IDs in certain private equity transactions. Inaccuracies sometimes arose when a private equity group acquired one or more divisions of a corporate entity, but not the whole firm. In principle, the Annual Company Organization Survey (sent to all large multi-unit companies) lets Census track these divisional sales. However, we identified divisional sales in which the firm ID of the (new) target firm remained the same as the ID of the selling firm. This problem did not affect the establishment-level analysis in Section 5, because we could rely on an alternative identifier – the Employer Identification Number (EIN) – to accurately identify, as of the transaction year, establishments involved in divisional sales. Unfortunately, EINs are unsuitable for tracking firms because new and acquired establishments

may obtain new EINs. We therefore exclude divisional cases from our firm-level analysis for those cases when Census does not provide an accurate ID for the target firm.[16]

For the firm-level analysis, we expand the sample period of buyout events to run through 2003. (A firm-level analysis for the period running from 1980 to 2000 yields similar results.) As reported in Table 2, our full matched sample contains 2265 target firms from 1980 to 2003. They account for about 4.3 million workers and 104,000 establishments as of the buyout year. Excluding the divisional, EIN cases that lack accurate firm IDs yields 1874 target firms with about 3.4 million workers and 79,000 establishments.[17] Further restricting attention to firms that we can track for two years after the buyout year, including deaths, yields a sample of 1,374 firms and 3.2 million workers. This sample represents 73 percent of the matched sample with accurate firm IDs and 93 percent of their employment. The latter statistic is more relevant given our focus on employment-weighted outcomes.

B. Firm-Level Employment Results

Our firm-level analysis considers the same type of semi-parametric regression specifications as in Table 3. Now, however, we explore employment responses on several adjustment margins, including the entry of new establishments post buyout. As before, the regressions include the pre-buyout growth variables and the cross product of industry, firm size categories, firm age categories, multi-unit status, and buyout year as controls. We weight observations by employment, as before. To obtain the effect of interest, we rely on indicator variables for target firms.

[16] We more fully discuss tracking issues related to divisional sales and our use of EINs in the web appendix. Table A.1 repeats the Section 5 analysis excluding establishments owned by divisional targets with inaccurate IDs, yielding results similar to Table 3 in Section 5, but with somewhat smaller relative employment losses at targets. The similarity of establishment-based results for the full sample and the subsample suggests that our firm-level analysis is not seriously distorted by the inability to accurately track firm IDs for some divisional sales.

[17] Although our firm-level analysis sample excludes some transactions covered by the establishment-level analysis, extending the sample period through 2003 captures a large number of more recent buyouts, as seen in Figures 1 and 2. As a result, the firm-level analysis sample actually covers more employment.

Table 4 presents firm-level regression results for cumulative responses over the first two years post buyout. Again, we report results for an ATE=ATE1 specification that posits a uniform treatment effect, and for an ATE1 Heterogeneous specification that allows treatment effects to vary with pre-buyout history and across firm age and size categories. The top row in Table 4 says that target firms shrink more rapidly than controls in the two-year period after buyouts – by 0.88 percentage points in the ATE=ATE1 specification and 0.65 percentage points in the ATE1 Heterogeneous specification. These estimated effects are much smaller than the cumulative two-year difference of 2.9 points for both specifications in Table 3. This comparison suggests that the additional adjustment margins captured by the firm-level analysis alter the picture of how private equity buyouts affect employment outcomes.

The remaining rows in Table 4 address the issue directly in the firm-level sample. Focus on the ATE1 Heterogeneous specification, and consider first the results for "Continuers" and "Deaths", two adjustment margins captured by the establishment-level analysis.[18] Summing these two components yields a two-year employment growth rate differential of -5.49 percentage points (-1.36 – 4.13) for targets, a large difference. But target firms create more new jobs at new establishments in the first two years after buyouts, a difference of 1.87 points in favor of targets. Combining these three adjustment margins yields a differential of -3.62 percentage points for targets.[19] Finally, bringing in the role of acquisitions and divestitures reduces this differential to

[18] Unlike the establishment-level analysis, however, the firm-level analysis does not encompass post-divestment employment changes at divested continuing establishments.

[19] The two-year differential of -5.49% for continuers plus deaths in Table 4 (ATE1 Heterogeneous) is larger than the corresponding two-year differential of -2.91% in Table 3. Recall that these two tables address different questions and use somewhat different samples. Table 4, unlike Table 3, excludes (a) EIN cases and other firms that we could not track post buyout and (b) the employment changes of establishments divested in years one and two post buyout. Table 3, however, does not capture employment at post-buyout establishment births. To obtain an estimated target-control growth differential that captures all organic adjustment margins (and only organic margins), sum the greenfield job creation differential in Table 4 (1.87% of initial employment in favor of targets) and the two-year growth differential from Table 3 (2.91% in favor of controls). This calculation yields an estimated 1.04% employment contraction at targets relative to controls over the first two years post buyout.

-0.81 points, close to the estimated differential in the top row. Thus, the overall impact of private equity buyouts on firm-level employment growth is quite modest.

As a robustness check, we also estimate the average treatment effect of private equity buyouts on firm-level employment growth using propensity score methods. We construct propensity scores by fitting logit specifications, one for each buyout year, for the likelihood that a firm becomes a private equity target. The logit specification includes the pre-buyout growth variables and the cross product of industry, firm size categories, firm age categories, and multi-unit status. Our second-stage regression includes an indicator for private equity targets, as before, plus the propensity score measure interacted with year effects. Using this second-stage regression, we estimate that a private equity buyout *raises* firm-level employment growth by 0.26 percentage points in the first two years post buyout, with a standard error of 0.18 points.[20] Thus, under the propensity score approach, we cannot reject the hypothesis that private equity buyouts have zero net impact on employment growth at target firms.

It is worth stressing that our firm-level and establishment-level regression analyses answer different questions. The establishment-level analysis tells us what happens to employment at establishments owned by target firms as of the buyout year. The firm-level analysis tells us what happens to employment at target firms, overall and on various adjustment margins. In practice, the main difference is that the firm-level analysis picks up large differentials between targets and controls in job creation at newly opened establishments and in employment changes associated with acquisitions and divestitures.

[20] The standard error is not adjusted for the first-stage estimation. As Woolridge (2002) notes, an advantage of including controls directly in the main regression is that it simplifies the computation of standard errors. He also points out that propensity score methods often yield similar results to methods that use controls in the main regression. When estimated with a linear probability model, a propensity score approach is equivalent to a one-stage approach that introduces the controls directly into the main regression, as in Table 4.

C. Private Equity: Agents of Reallocation within Firms?

Table 4 and Figure 4 suggest that buyouts act as catalysts for creative destruction. Target firms exhibit more job destruction in establishment shutdowns and more job creation in establishment births, larger job losses through divestment and larger job gains through acquisition. This evidence is consistent with two distinct hypotheses. One hypothesis holds that private equity acts as an agent of change – inducing some target firms to expand relative to controls and others to retrench. According to this hypothesis, the evidence reflects a combination of (a) upsizing target firms that add establishments and jobs more rapidly than controls and (b) downsizing target firms that shed jobs and establishments more rapidly than controls. The positive effects of buyouts on job creation and destruction then result by aggregating over upsizing and downsizing cases. A second hypothesis holds that private equity acts as an agent of restructuring within target firms, accelerating the reallocation of jobs across establishments in these firms and their pace of acquisition and divestment. These hypotheses are not exclusive because private equity could accelerate both types of creative destruction.

To evaluate these hypotheses, we now estimate buyout effects on firm-level reallocation measures. A firm's job reallocation is the sum of its gross job gains due to new, expanding, and acquired establishments and gross job losses due to exiting, shrinking, and divested establishments. Its excess reallocation is the difference between job reallocation and the absolute value of its net job growth.[21] If a given firm changes employment in the same direction at all of its establishments, it has zero excess reallocation. To the extent that a firm expands employment at some units and contracts employment at others, it has positive excess reallocation. If the firm adds jobs at some of its establishments and cuts an equal number of jobs at other establishments,

[21] This concept of excess reallocation is used often in the literature on gross job flows to quantify job reallocation within industries, regions and business categories. See Dunne, Roberts, and Samuelson (1989), Davis and Haltiwanger (1992), and, for a review of the literature, Davis and Haltiwanger (1999). Our approach here applies the same concept to the reallocation of jobs across production units within firms.

excess reallocation equals job reallocation. So, if private equity acts exclusively as agents of change, the entire creative destruction response of target firms involves higher job reallocation but no impact on excess reallocation. At the other extreme, if private equity acts exclusively as agents of restructuring within target firms, firm-level job reallocation and excess reallocation rates respond by the same amount to buyouts.

Table 5 reports regression results for firm-level job reallocation and excess reallocation rates using the same specifications and two-year horizon as in Table 4. In the ATE1 Heterogeneous specification, the job reallocation rate is 13.9 percentage points higher at target firms, and the excess reallocation rate is 9.3 points higher. Thus, two-thirds of the extra job reallocation associated with private equity buyouts reflects an accelerated pace of restructuring within target firms. For organic changes, the impact of buyouts on excess reallocation – 6.4% of initial employment over two years – is actually greater than the impact on total job reallocation.[22] In short, and especially for organic employment changes, Table 5 implies that private equity acts predominantly as an agent of (accelerated) restructuring within target firms.

The regression results in Tables 3, 4, and 5 identify only the differential responses of targets relative to controls. To recover information about the levels of creation and destruction activity, we return to the nonparametric approach of section 5.A and consider a counterfactual exercise along the lines of Figure 3. Specifically, we sort target and control observations in our 1980-2003 firm-level analysis sample into cells defined by the same cross product of industry, size, age, multi-unit status, and buyout year as before. For each cell, we calculate cumulative two-year changes post buyout for each employment adjustment margin. We then generate the weighted average outcomes for targets and controls using the same approach to weighting as in

[22] By definition, overall job reallocation equals or exceeds excess job reallocation for a given firm or group of firms. Our comparison here, however, involves the difference between job reallocation and excess reallocation responses for two distinct sets of firms, targets and controls.

Figure 3. These calculations reveal the extent of creation and destruction activity on each adjustment margin at target firms, and they tell us how target firm activity would differ if targets exhibited the same behavior as controls.

Table 6 reports the results of these calculations. They show high rates of creation and destruction at target firms in the wake of private equity buyouts. The two-year cumulative job reallocation at target firms is 52 percent of initial employment for organic changes (Panel A) and 69 percent inclusive of acquisitions and divestitures (Panel B). According to the "Difference" column in Panel A of Table 6, buyouts raise job creation, destruction and reallocation rates by, respectively, 2.0, 4.3 and 6.3 percent of initial employment, which amount to 9, 19 and 14 percent of the base rates at control firms. Panel B shows that the increases in creation, destruction, and reallocation associated with buyouts are considerably larger, in both absolute and relative terms, when including acquisitions and divestitures. To check the consistency of these results with Tables 4 and 5, the two rightmost columns in Panels A and B report the semi-parametric regression estimates of target-control differences. The two approaches yield similar estimated differences, and the differences are precisely estimated.

Table 6 also reveals that excess reallocation accounts for more than 95 percent of overall job reallocation for both target and control firms, whether considering organic changes or including acquisitions and divestitures. About two-thirds of excess reallocation occurs within firms for organic employment changes, 56-58 percent when including acquisitions and divestitures. Excess job reallocation within the same firm and county (not shown in the table) accounts for half of all excess job reallocation within control firms, and slightly less within target firms. Putting these results together, the movement of job positions across locations within the same firm and county account for about one-third of all excess job reallocation for organic adjustment margins and about one-quarter when including acquisitions and divestitures.

D. Differential Employment Responses by Time Period, Industry, and Buyout Type

The foregoing analyses could mask important differences in private equity effects by time period, industry, or type of buyout. Descriptive accounts suggest that private equity groups shifted to a more operational orientation over time, which could lead to time-varying effects on target employment. The scale of private buyout activity also increased enormously over time, which could alter the character of the marginal target and its post-buyout performance. Motivated by these observations, Figure C.2 in the appendix displays the evolution of target-control growth rate differences for buyouts that took place in the 1980s, 1990-94, and 1995-2000. In each period, employment contracts more rapidly at targets than at controls in the years following private equity buyouts.

Some accounts of private equity paint a picture of aggressive cost cutting through layoffs. This characterization suggests a potential for greater post-buyout job destruction rates in labor-intensive industries, reflecting a view that cost cutters focus on the largest cost sources. More generally, there are major differences in factor cost shares, market structure, demand conditions, and labor relations that might lead to important industry differences in the responses to private equity buyouts. Motivated by these ideas, Appendix Figure C.3 displays the evolution of target-control growth differences for three industry sectors that cover most private equity buyouts. Employment falls modestly at target establishments relative to controls post buyout in Manufacturing. Retail Trade exhibits a markedly different response pattern. In the years leading up to buyout transactions, controls and targets in Retail Trade exhibit similar growth rates. Post buyout, however, employment at target establishments falls by nearly 12 percent relative to controls over five years. The Service sector exhibits yet a different pattern. Targets grow much rapidly than controls before the buyout year but more slowly afterwards. These large industry

differences serve as a caution against painting with an overly broad brush when characterizing employment outcomes in the wake of private equity buyouts.

There are also good reasons to think employment effects vary by type of buyout. Public-to-private deals may be more likely to involve target firms with a strong need for cost cutting, as in the Beatrice case (Baker, 1992). Better funding access and relaxed constraints on capital investment and job creation are more likely to motivate deals for privately held firms. In light of these arguments, Table 7 reports estimated average effects on two-year post-buyout rates of employment growth and excess job reallocation by type of buyout. In public-to-private deals, target employment contracts more than 10 percent relative to controls over two years. As reported in appendix Table C.3, target firms in these deals experience much greater job losses due to establishment deaths and divestitures and less job creation through births. Along with the high visibility of public-to-private deals, these results help understand concerns about job loss related to private equity buyouts. In striking contrast, employment at independent targets (private-to-private deals) expands 10 percent relative to controls in the first two years post buyout. More rapid employment growth at independent targets reflects a higher pace of acquisition, consistent with the view that private equity investments facilitate firm-level expansion.[23] Most U.S. buyout transactions involve independent targets, even though public-to-private transactions garner much more attention. In terms of buyout-year employment, independent targets account for about 63 percent more jobs than publicly held targets (Table 2).

One common pattern emerges for all deal types in Table 7: excess reallocation rates are higher at target firms than at controls. The magnitude of the target-control difference in excess reallocation varies greatly by type of transaction, but it is positive and highly statistically significant in all cases.

[23] Our evidence for private-to-private deals in the United States is broadly consistent with the evidence on French buyouts and its interpretation in terms of relaxed capital constraints offered by Boucly, Sraer and Thesmar (2009).

7. Effects on Productivity and Worker Earnings

A. Productivity Measurement and Sample Weights

Our productivity analysis considers plant-level observations covered by the Census of Manufactures (CM) and the Annual Survey of Manufactures (ASM). The CM surveys all but the smallest manufacturing plants once every five years. The ASM, a rotating panel of manufacturing plants, samples the largest units with certainty and other units with probabilities increasing in size. Following Baily, Hulten, and Campbell (1992) and others, we compute plant-level log TFP as $lnTFP_{it} = lnQ_{it} - \alpha_K lnK_{it} - \alpha_L lnL_{it} - \alpha_M lnM_{it}$, where *i* and *t* index plant and year, respectively, Q is real output, K is real capital, L is labor input, M is materials, and α denotes factor elasticities. Operationally, we measure plant output as shipments plus the change in inventories, deflated by industry-level price indices. We measure capital separately for structures and equipment using perpetual inventory methods. Labor is total hours of production and non-production workers. We measure and deflate energy and other materials separately, and we measure the factor elasticities using industry-level cost shares.[24]

Because the CM takes place every five years and the ASM over samples larger plants, our study of productivity outcomes adjusts for the probability that a given observation appears in a given analysis sample. To do so, we first create indicator variables for whether each observation appears in a given analysis sample.[25] We then fit year-by-year logit models to the indicator variables, obtaining estimated sample inclusion probabilities for each observation. Explanatory variables are dummies for industry (4-digit SIC or 6-digit NAICS), payroll size

[24] See Foster, Grim and Haltiwanger (2013) for details on our measurement of inputs and outputs. As discussed in Syverson (2011), alternative methods for measuring the factor elasticities tend to yield similar plant-level TFP values even when they produce somewhat different elasticity values.

[25] Our assignment of controls to target plants for the productivity analysis is similar to the approach adopted in Section 5. Starting from our establishment-level analysis sample, we restrict attention to manufacturing plants operated by two-year continuing firms (i.e., two years from the buyout year) and industry-year cells for which the ASM/CM data contain at least one target and one control observation. We drop a few observations for which the ASM/CM employment figure differs from the LBD figure by more than 1,000. We also drop control observations on plants with more than 10,000 employees, of which there are none among target manufacturing plants.

classes, employment size classes, multi-unit status, target status, and interactions between multi-unit status and industry. The LBD contains data on these explanatory variables for every plant-year observation. We set the propensity weight for each observation to the reciprocal of its model-implied sample inclusion probability.[26]

B. Do Target Firms Direct Reallocation To More Productive Plants?

Section 6 shows that private equity buyouts accelerate job reallocation. Much of the extra reallocation involves establishment births and deaths. We now consider whether target firms direct this reallocation activity in ways that affect productivity. To that end, we first sort target plants and controls into terciles of the same-industry/same-year TFP distribution. We then investigate how plant entry and exit probabilities vary by location in the TFP distribution for targets in comparison to controls.

Panel A in Table 8 reports estimated exit probabilities in the first two years post buyout. We obtain these probabilities from a logit model fit to an exit indicator variable, where the explanatory variables are dummies for TFP terciles interacted with target and control dummies. Two results stand out: First, exit probabilities decline much more steeply with TFP for target plants than for controls. Second, targets exhibit greater exit probabilities in the bottom and middle terciles of the own-industry TFP distribution. The target-control exit differential is 5 percentage points in both terciles and significant at the 2.5% level in the bottom tercile.

Panel B provides information about the location of new plants in the TFP distribution, where new plants are those that enter in the first or second year post buyout. We obtain entry probabilities from a logit model fit to an entry indicator variable, where, as before, the

[26] Plant and firm age measures did not improve the logit models for sample inclusion – perhaps not surprisingly, given that Census mainly relies on industry, size, and multi-unit status in the ASM sample design. We checked that the propensity-weighted analysis samples adequately match the firm size, firm age and industry distributions of employment in the populations of targets and controls, and that they adequately reproduce changes along each adjustment margin – births, deaths, continuers, etc.

explanatory variables are dummies for TFP terciles interacted with target and control dummies. The Panel B results uncover a striking contrast between targets and controls in the productivity of new plants: The prevalence of new plants in the bottom TFP tercile is only half as large for targets as controls, and the prevalence of new plants in the top tercile is nearly twice as large for targets. New plants are concentrated in the upper part of the TFP distribution at firms backed by private equity. The opposite pattern holds at control firms.[27]

C. Quantifying Buyout Effects on Total Factor Productivity

We can briefly summarize the evidence in Tables 5, 6, 7 and 8 as follows: Private equity buyouts accelerate job reallocation within target firms, and target firms direct reallocation activity in ways that raise TFP. We now develop additional evidence on the relationship of private equity buyouts to TFP. We then pull together several results to quantify and decompose the effects of buyouts on firm-level TFP growth.

Table 9 reports results for propensity-weighted OLS regressions that compare log TFP between target and control plants in the same industry-year. In addition to the full set of industry-year effects, the regressions include an extensive set of firm size and age effects.[28] Panels A and B consider productivity outcomes in buyout years t and t+2, respectively, and Panel C considers TFP log changes for continuers. The most striking results involve entrant productivity: target entrants are 18 log points more productive in year t+2 than continuing control plants and 22 log points more productive than control entrants. These large TFP advantages reflect a concentration of target entrants in the upper part of the TFP distribution and the opposite pattern for control

[27] Previous work by Foster, Haltiwanger and Krizan (2001), among others, highlights rapid productivity gains among young plants through learning and selection effects. Thus, the concentration of control entrants in the bottom tercile of the TFP distribution should not be seen as evidence that entry lowers industry-level TFP over time. Nevertheless, Table 8.B reveals that new plants opened by private equity targets significantly out perform new plants opened by control firms – at least with respect to early-life TFP.

[28] Adding covariates for firm size and age moves us away from an exact matching estimator but, in our view, is preferable to (a) omitting the size and age effects or (b) a pure matching estimator that uses very coarse size-age-industry-year cells. See Chapter 3 in Angrist and Pischke (2009) for discussion of this issue.

entrants (Table 8). Although the effects are smaller and the evidence less powerful, Table 9 also confirms that plant exit patterns raise TFP at target firms, absolutely and relative to control firms. In contrast, we find no evidence that target continuers experience more rapid TFP growth than control continuers. Panel C makes this point directly: the estimated target-control differential for TFP growth is one log point, with a standard error of 11 log points. If private equity buyouts improve relative TFP in continuing plants, the effects are either too small to reliably discern in our sample, or the gains mount too slowly to capture in our two-year tracking interval.[29]

To quantify the overall effect of private equity buyouts on firm-level TFP and assess the role of various adjustment margins, consider the difference-in-difference $\Delta P_t - \Delta \widetilde{P}_t$, where

$$\Delta P_t = \left[S_{t+2}^C P_{t+2}^C - S_t^C P_t^C\right] + \left[S_{t+2}^N P_{t+2}^N - S_t^X P_t^X\right] + \left[S_{t+2}^A P_{t+2}^A - S_t^D P_t^D\right]$$

is the average two-year change in TFP among target firms, S denotes an employment share, P denotes a TFP value, and *C, N, X, A* and *D* denote continuers, entrants, exits, acquisitions, and divestitures, respectively. For example, P_{t+2}^C is the average TFP among continuing target plants two years post buyout, where each plant's TFP is expressed as a deviation from mean log TFP in the same industry-year cell. The average two-year TFP differential for controls, $\Delta \widetilde{P}_t$, is defined analogously. Now express the TFP terms as deviations about same-year TFP values for control continuers, cancel terms in $\Delta P_t - \Delta \widetilde{P}_t$, and rearrange to obtain:

$$\begin{aligned}\Delta P_t - \Delta \widetilde{P}_t = {} & S_{t+2}^C \left(P_{t+2}^C - \widetilde{P}_{t+2}^C\right) - S_t^C \left(P_t^C - \widetilde{P}_t^C\right) \\ & + S_{t+2}^N \left(P_{t+2}^N - \widetilde{P}_{t+2}^C\right) - \widetilde{S}_{t+2}^N \left(\widetilde{P}_{t+2}^N - \widetilde{P}_{t+2}^C\right) - S_t^X \left(P_t^X - \widetilde{P}_t^C\right) + \widetilde{S}_t^X \left(\widetilde{P}_t^X - \widetilde{P}_t^C\right) \\ & + S_{t+2}^A \left(P_{t+2}^A - \widetilde{P}_{t+2}^C\right) - \widetilde{S}_{t+2}^A \left(\widetilde{P}_{t+2}^A - \widetilde{P}_{t+2}^C\right) - S_t^D \left(P_t^D - \widetilde{P}_t^C\right) + \widetilde{S}_t^D \left(\widetilde{P}_t^D - \widetilde{P}_t^C\right)\end{aligned} \qquad (1)$$

[29] A recent study by Bharath, Dittmar and Sivadasan (2013) considers 406 publicly held U.S. manufacturing firms that went public in recent decades, including 115 through private equity buyouts. Consistent with our results, they find no evidence that public-to-private transitions generate productivity gains in continuing plants.

The top line of (1) isolates the contribution of target-control differences among continuing plants, the second line isolates the contribution of plant entry and exit (births and deaths), and the third line isolates the contribution of acquisitions and divestitures.

This decomposition is new to the literature to our knowledge and has some attractive features. It shows how to combine diff-in-diff estimates with an accounting decomposition that appears often in the empirical literature on firm-level productivity dynamics. To see this point, note that the expressions in parentheses can be read directly from Table 9. The shares *S* can be retrieved from the job creation and destruction statistics reported in Appendix Table C.2, the manufacturing analog to Table 6. Related, (1) lets us exploit the full LBD to compute the share variables, while relying on the ASM-CM sample to obtain the diff-in-diff estimates. The decomposition also sidesteps any need to compare TFP across industries or years, because all productivity terms in (1) are based on plant-level TFP deviations about industry-year means.

Table 10 exploits (1) to obtain the average TFP growth differential between target and control firms and its decomposition by margin of adjustment. Target firms out perform controls with respect to post-buyout TFP growth by 2.14 log points over two years, a large gain compared to the change of -0.38 log points among control continuers. Summing over terms in the second line of (1) yields a value of 1.59, implying that plant entry and exit effects account for 74 percent of the superior TFP growth at target firms. This result confirms the importance of the target-control differences on the entry and exit margins documented in Tables 8 and 9.

For additional insight into the nature of the entry and exit effects, we replace S^N and $\tilde{S}^N$ with their average in (1), do the same for S^X and $\tilde{S}^X$, and then recalculate the second line of (1) to obtain a value of 1.56 log points. This calculation corresponds to a counterfactual that turns off target-control differences in the *pace* of job reallocation to isolate the role of differences in its

direction. The message is clear: The stronger directedness of job reallocation in target firms accounts for almost all of the entry and exit contribution to (1) and, indeed, more than 70 percent of $\Delta P_t - \Delta \widetilde{P}_t$.

Two other remarks help put this finding in perspective. First, while directional differences are central to our explanation for superior TFP growth at target firms, they matter because entry and exit involve sizable rates of job creation and destruction. In this respect, both the pace of job reallocation and the target-control directional differences are essential. Second, reallocation rates are considerably higher outside the manufacturing sector, as readily seen by comparing Tables 6 and C.2. This fact has potentially important implications for the TFP effects of buyouts in the private sector as a whole. If we plug private sector share values from Table 6 into (1) alongside diff-in-diff estimates from Table 9, the implied TFP growth advantage of targets is 3.05 log points, 81 percent of which is due to entry and exit effects.

D. Effects on Earnings Per Worker

Tables 8-10 provide strong evidence that, on average, private equity buyouts improve operating performance, at least in the manufacturing sector. To investigate whether buyouts also affect operating margins via unit input costs, we now consider LBD data on annual earnings per worker (EPW) at the establishment level. ("Earnings" encompass all taxable forms of compensation.) We follow the same approach to selecting control establishments as in Section 5, and we again exploit the size of the LBD to include an extensive set of controls in the regression specifications. Table 11 reports the results, following the same layout as Table 9. As before, the units of the estimated effects are log deviations about industry-year means.

There are several noteworthy results. First, among plants destined to exit within two years post buyout, average EPW are 9-12 log points lower than at control continuers. This evidence rejects the view that firms backed by private equity tend to close establishments with high EPW.

Second, target firms divest establishments with high EPW, whereas controls do not. The EPW difference for divestitures is 22 log points in favor of reduced labor costs for targets, a huge difference. Third, Panel C reports that average EPW shrink by 2.4 log points at target continuers relative to control continuers over the first two years post buyout. Fourth, we also applied equation (1) to the diff-in-diff estimates in Table 11 and reallocation rates in Table 6 to construct the EPW counterpart to Table 10 above. That exercise reveals an overall two-year post-buyout EPW decline of 4.0 log points for target firms (relative to controls).[30] Continuers account for 79 percent of the relative EPW reduction at target firms, acquisitions and divestitures account for 29 percent, and net entry effects actually raise relative EPW at target firms.

Appendix Table C.4 reports additional EPW regressions. Continuing establishments operated by target firms experience large post-buyout EPW cuts of 6-8 log points (relative to controls) in Wholesale, Retail, and Services – industries that rely heavily on less skilled labor – while FIRE shows a large EPW gain of 9 log points, and Manufacturing shows virtually no change. EPW at target continuers decline by 7 log points in public-to-private deals and by 2 points in private-to-private deals. Divestitures contribute to relative EPW reductions at targets firms for all private equity deal types, with an especially pronounced divestiture effect in private-to-private deals. In summary, while the details of the EPW results differ somewhat by industry and buyout type, the prevailing pattern is one of reduced EPW at target firms in the wake of private equity buyouts. Related work by Neumak and Sharpe (1996) and Bertrand and Mullainathan (1999), for example, finds mixed results for the effects of hostile takeovers on worker earnings. We believe there is much room for additional research on how ownership changes affect labor costs and worker earnings.

[30] Our data do not let us decompose EPW changes into the effects of hours worked and wages per unit time. The wage per unit time could fall because of either wage reductions for workers of a given quality or a shift to less skilled workers who command lower wages. However, if wages decline at target firms because of a shift to lower skill workforces (relative to concurrent changes at controls), then our results understate buyout-driven TFP gains.

8. Concluding Remarks

Our study develops new evidence on the responses of employment, job reallocation, productivity, and worker earnings to private equity buyouts. Compared to previous research, we exploit a much larger sample of buyouts, a much more extensive set of controls, and a novel ability to track outcomes at firms *and* establishments. These advantages enable us to overcome important limitations in previous research and address controversies about the effects of private equity buyouts on jobs and operating performance. We also exploit the strengths of our data to explore new questions about private equity's role in the creative destruction process and its impact on restructuring activity inside target firms.

Our findings support the view that private equity buyouts lead to greater job loss at establishments operated by target firms *as of the buyout year*. Employment at these establishments shrinks by 3 percent relative to controls in the two-year period post buyout and by 6 percent over five years. Gross job destruction at target establishments outpaces destruction at controls by a cumulative 10 percentage points over five years post buyout. Thus, pre-existing employment positions are at greater risk of loss in the wake of private equity buyouts.

While noteworthy, these results make up only part of a richer and more interesting story. Using our ability to track each firm's constituent establishments, we examine how jobs respond to buyouts on several adjustment margins, including job creation at greenfield establishments opened post buyout. This aspect of our analysis reveals that target firms create new jobs in newly opened establishments at a faster pace than control firms. Accounting for the purchase and sale of establishments as well, the target-control growth differential is less than 1 percent of initial employment over two years.

Private equity buyouts involve much larger effects on the gross creation and destruction of jobs. The job reallocation rate at target firms exceeds that of controls by 14 percentage points

over two years post buyout. About 45 percent of the extra job reallocation reflects a more rapid pace of organic employment adjustments, and the rest reflects acquisitions and divestitures. We find greater job reallocation in the wake of private equity buyouts for public-to-private deals, private-to-private deals, divisional sales, and secondary sales. These novel findings provide evidence that private equity buyouts catalyze the creative destruction process as measured by gross job flows and the purchase and sale of business establishments. Digging deeper, we also address two distinct hypotheses about the nature of the increased reallocation activity associated with private equity buyouts. One hypothesis sees private equity as agents of change in the sense that buyouts accelerate retrenchments at some target firms, while accelerating expansion at others. Another hypothesis sees private equity as agents of restructuring in the sense that buyouts accelerate the reallocation of jobs across establishments within target firms. We show the restructuring effect predominates, and it is the entire story for organic employment changes.

Our investigation into the effects of private equity buyouts on TFP growth in the manufacturing sector yields a highly complementary set of results. Relative to controls, target firms more aggressively close plants with low TFP, and they more aggressively open new plants with high TFP. In other words, target firms direct job reallocation activity on the plant entry and exit margins in ways that raise TFP. On average, target firms out perform control firms with respect to TFP growth by 2.1 log points over two years post buyout. More than 70 percent of the estimated TFP gains arise from private equity influence on the direction of job reallocation on plant entry and exit margins. These results refute the view that the returns to private equity rest entirely on private gains to financial engineering and wealth transfers from other stakeholders.

We also find sizable reductions in earnings per worker in the first two years post buyout. Specifically, we estimate an average two-year post-buyout reduction in earnings per worker of 4 log points at target firms relative to controls, mostly due to reductions at continuing plants. In

sum, our evidence indicates that private equity buyouts improve operating margins at target firms by raising productivity and by lowering unit labor costs. The resulting gains in profitability are magnified in their impact on corporate earnings per share by the leveraged capital structures that characterize firms acquired in private equity buyouts.

By identifying a large sample of private equity transactions and linking them to the LBD, ASM and CM, this paper also sets the stage for new research into buyout effects on capital expenditures, other input costs, profitability, and other outcomes. A rich array of input and outcome measures are available at the firm and establishment level in Census Bureau data sets that can be linked to the LBD and our data on private equity transactions. Our plans for future work also include further investigation into how and why buyout effects differ by industry and type of buyout, and an examination of outcomes in corporations that sell to private equity groups. Many divisional buyouts involve divestitures of underperforming units that may place heavy demands on senior management. Schoar (2002) documents that acquisitions can lead managers to neglect core businesses, what she calls the "new toy" effect. The LBD allows us to investigate whether the same phenomenon operates in reverse when firms sell underperforming or poorly fitting divisions, thereby freeing senior management to focus on core activities.

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Figure 1: Number of Target Firms in U.S. Private Equity Buyouts, 1980 to 2005

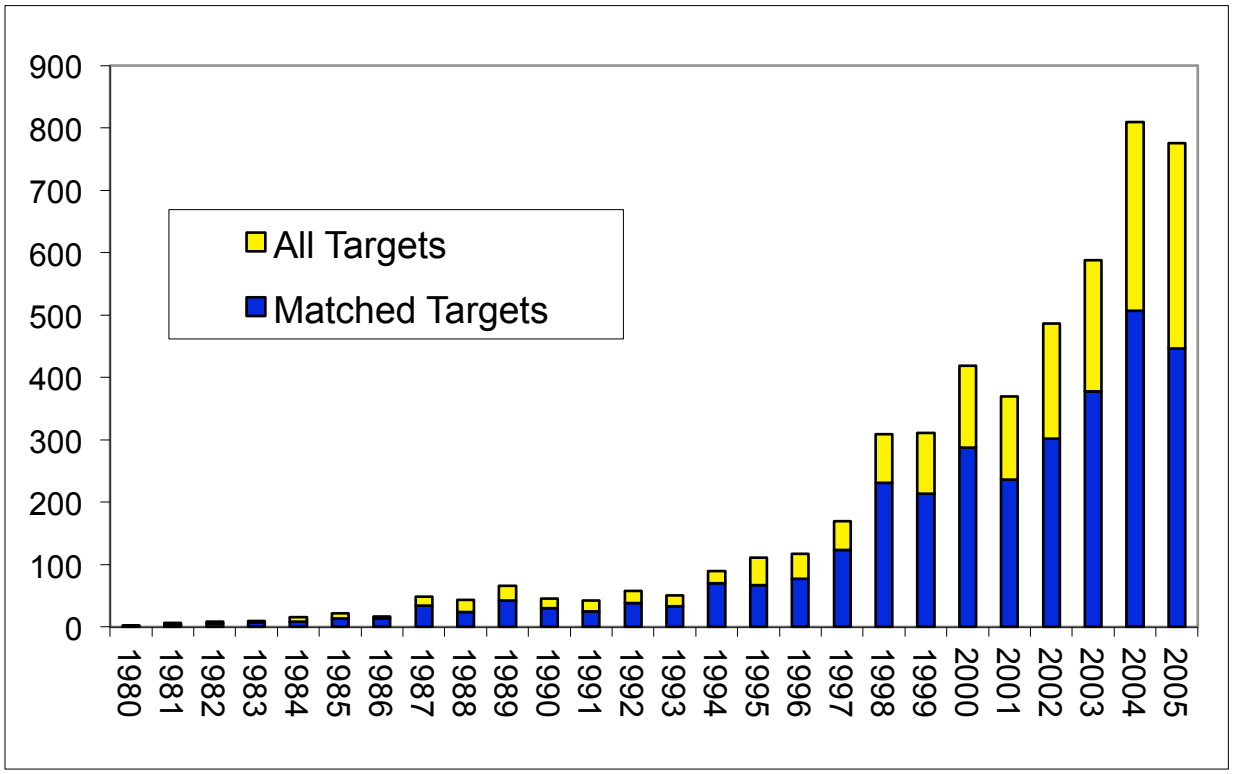


Figure 2: Employment at Matched Targets as of the Buyout Year, 1980 to 2005

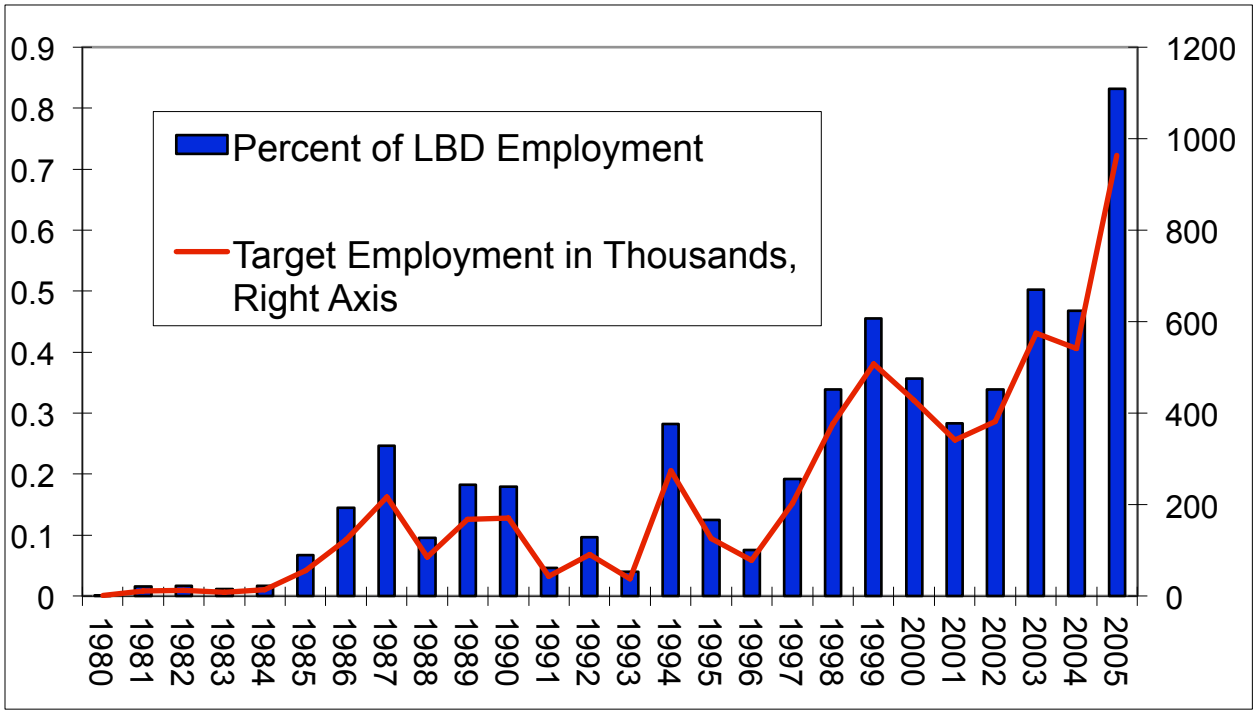


Figure 3A: Comparison of Employment Trajectory for Target Establishments to Controls, Buyouts from 1980 to 2000

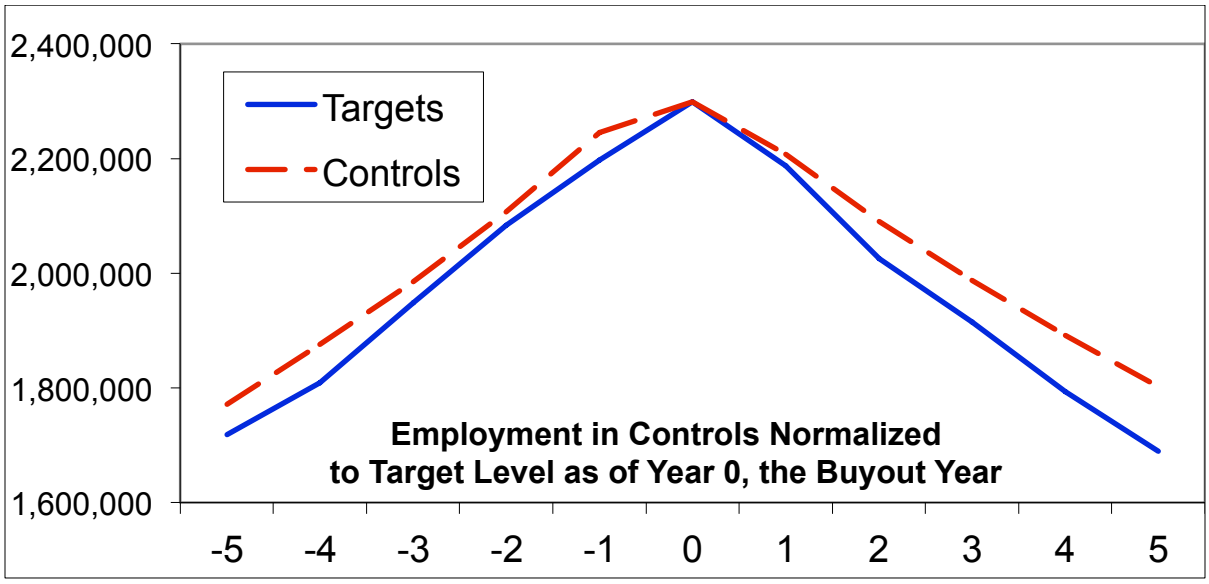


Figure 3B: Employment Growth Rate Differences before and after the Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000

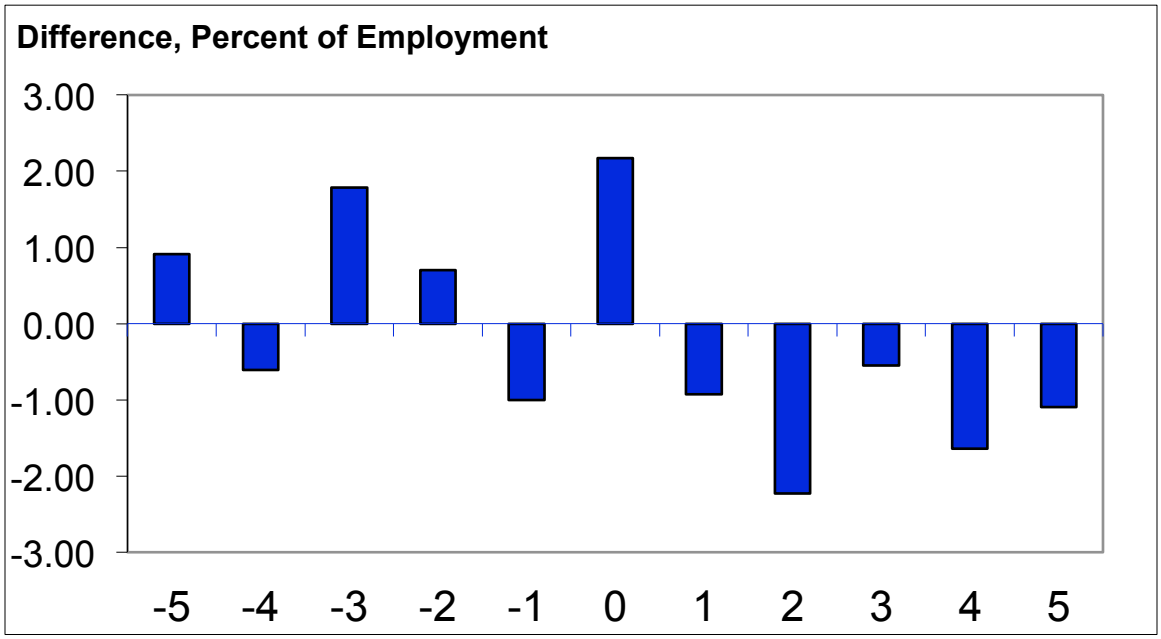


Figure 4A: Job Creation Rates before and after Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000

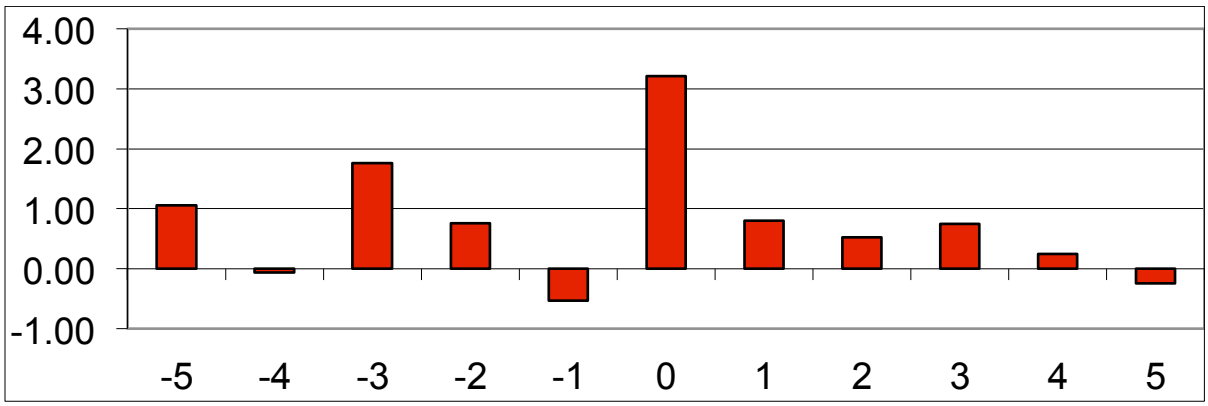


Figure 4B: Job Destruction Rates before and after Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000

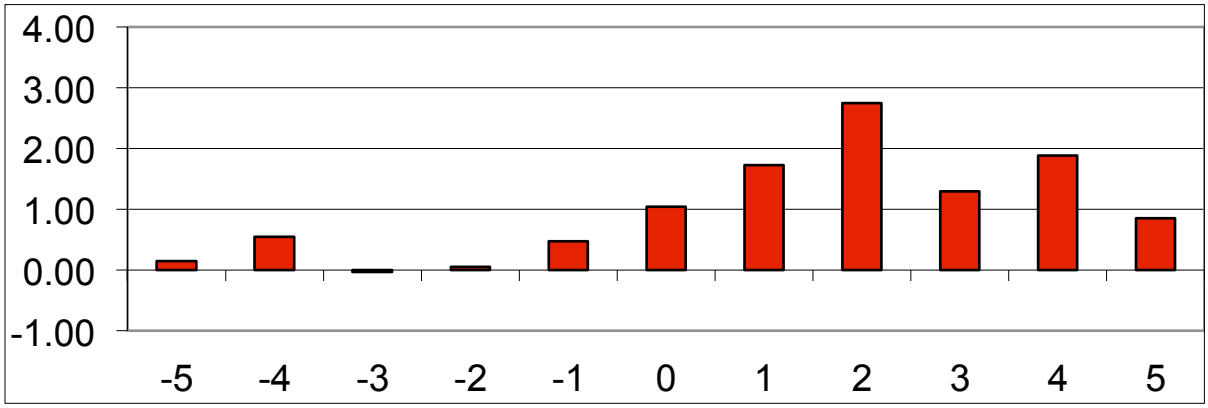


Figure 5: Emloyment-Weighted Establishment Exit Rates Post Buyout, Targets Minus Controls, Buyouts from 1980 to 2000

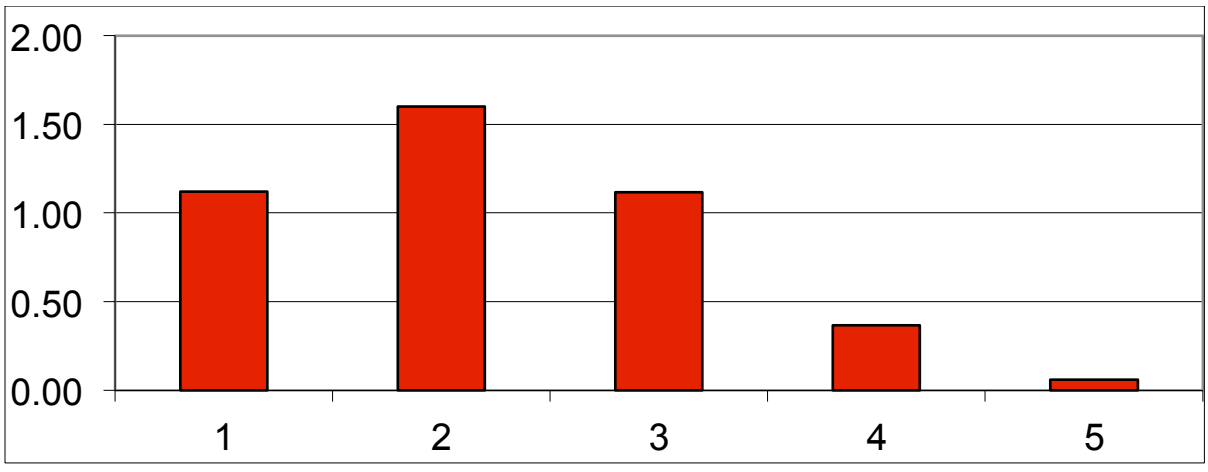


Table 1. Value of Private Equity Targets, Total and Matched by Decade ($millions)

	1980-1989	1990-1999	2000-2005
Matched	73,209	169,271	291,824
Total	122,115	211,615	420,245

Table 2. Private Equity Buyouts in the Analysis Samples

	Number of Transactions (Target Firms)	Value of Transactions, Millions of 2005 Dollars	Target Establishments, Transaction Year	Target Employment, Transaction Year
All, 1980-2005	3,218	573,224	151,529	5,828,532
Private to Private	1,350	88,919	59,865	2,224,530
Public to Private	390	261,164	36,717	1,371,129
Divisional Sales	918	132,330	35,259	1,359,139
Secondary Sales	396	72,969	13,455	637,591
Other	164	17,841	6,233	236,143
All, 1980-2003	2,265	431,871	103,671	4,323,558
Excluding EIN cases	1,874	377,303	79,131	3,410,598
Two-year continuers, excluding EIN cases	1,374	272,325	76,271	3,187,171
Private to Private	686	58,287	37,283	1,470,447
Public to Private	248	129,382	20,380	872,206
Divisional Sales	206	38,874	7,922	391,705
Secondary Sales	160	35,474	7,957	353,325
Other	74	10,309	2,729	99,488
All, 1980-2000	1,306	315,007	54,729	2,385,163
Private to Private	647	60,865	24,593	901,284
Public to Private	171	162,567	18,454	854,779
Divisional Sales	342	60,615	6,557	416,055
Secondary Sales	107	23,010	3,885	161,557
Other	39	7,951	1,240	51,488
Mfg., 1980-2003	539	109,644	9,174	805,328
Multi-Unit Firms Only	427	105,843	9,062	792,864
Multi-Unit with TFP	286	68,442	2,053	496,699

Notes:

1. We exclude single-unit matched targets from our analysis of the manufacturing sector. They account for only 112 of 9,744 target manufacturing establishments and less than 1.5 percent of target manufacturing employment.
2. The last row in the table reports data for matched multi-unit targets for which we can obtain data on total factor productivity in the transaction year from the Census of Manufactures or the Annual Survey of Manufactures.

Table 3. Post-Buyout Employment Growth Rates at Target Establishments Relative to Controls, Buyouts from 1980 to 2000

	Nonparametric Comparison From Figure 3b	Semi-Parametric Regressions	
		ATE=ATE1	ATE1 Heterogeneous
Buyout Year	2.17	2.08	2.28
		(0.17)	(0.17)
Buyout Year +1	-0.93	-0.72	-1.15
		(0.20)	(0.20)
+2	-2.23	-1.74	-1.76
		(0.20)	(0.21)
+3	-0.55	0.00	0.08
		(0.21)	(0.21)
+4	-1.64	-1.31	-1.16
		(0.22)	(0.22)
+5	-1.09	-0.95	-1.23
		(0.22)	(0.23)
Cumulative, Years 1 to 5	-6.44	-4.72	-5.22

Notes:

1. Table entries report estimated employment growth rate differences between targets and controls in the buyout year and following years. For example, the entries for "Buyout Year +2" report the estimated growth rate difference from Year 1 to Year 2 following the buyout. Each reported coefficient is for a different nonparametric comparison or regression. Standard errors are in parentheses. They are computed by the delta method in the "ATE1 Heterogeneous" regression.
2. The nonparametric comparison reflects the patterns displayed in Figure 3b. As explained in the text, this comparison allows for heterogeneous treatment effects and controls for the fully interacted cross product of 72 industries, 10 firm size categories, 6 firm age groups, multi-unit status, and buyout year.
3. The semi-parametric regression specifications include fully interacted industry, year, firm age, firm size and multi-unit effects plus additional controls for pre-buyout growth history. The "ATE=ATE1" specification imposes a uniform treatment effect, while the "ATE1 Heterogeneous" specification allows the treatment effect to vary by firm size, firm age, and the two measures of pre-buyout growth history.
4. The average number of establishment-level observations in each regression or nonparametric comparison is about 4.9 million. The observation count falls with each successive year following the transaction year because of target deaths and deleted observations for the corresponding control establishments.

Table 4. Buyout Effects on Employment Growth Rate at Target Firms Relative To Controls, Buyouts from 1980 to 2003

Dependent Variable	Regression Specification			
	ATE=ATE1		ATE1 Heterogeneous	
Firm-level Employment Growth Rate from Buyout Year *t* to *t*+2	Buyout Effect	R^2	Buyout Effect	R^2
	-0.88 (0.18)	0.07	-0.65 (0.16)	0.07
By Adjustment Margin				
Continuers	-1.57 (0.12)	0.09	-1.36 (0.11)	0.09
Creation	-1.07 (0.08)	0.16	-0.93 (0.08)	0.16
Destruction	0.71 (0.07)	0.09	0.64 (0.07)	0.09
Deaths	4.12 (0.09)	0.06	4.13 (0.08)	0.06
Births	1.80 (0.05)	0.22	1.87 (0.05)	0.22
Acquisitions	5.62 (0.05)	0.12	5.56 (0.05)	0.13
Divestitures	2.77 (0.05)	0.06	2.75 (0.04)	0.06

Notes:

1. Employment-weighted regressions on Target and Control firms, with rates calculated over the two-year horizon from the event year t to t+2. Standard errors in parentheses.
2. The semi-parametric regression specifications include fully interacted industry, year, firm age, firm size and multi-unit effects plus additional controls for pre-buyout growth history. The "ATE=ATE1" specification imposes a uniform treatment effect, while the "ATE1 Heterogeneous" specification allows the treatment effect to vary by firm size, firm age, and the two measures of pre-buyout growth history.
3. Each regression has 1,985,489 observations.

Table 5. Buyout Effects on Firm-Level Job Reallocation and Excess Reallocation, Buyouts from 1980 to 2003

Estimated Responses Relative to Controls from Buyout Year *t* to *t*+2	Regression Specification			
	ATE=ATE1		ATE1 Heterogeneous	
Dependent Variable	Buyout Effect	R^2	Buyout Effect	R^2
Firm-level Excess Reallocation – All Adjustment Margins	9.25	0.37	9.29	0.37
	(0.08)		(0.09)	
Firm-level Excess Reallocation – Births, Deaths & Continuers	6.38	0.38	6.40	0.39
	(0.08)		(0.08)	
Firm-level Job Reallocation – All Adjustment Margins	13.81	0.21	13.89	0.21
	(0.15)		(0.15)	
Firm-level Job Reallocation – Births, Deaths & Continuers	5.47	0.22	5.62	0.22
	(0.14)		(0.14)	

Notes:

1. Employment-weighted regressions on a sample of Target and Control firms, with rates calculated over the two-year horizon from the event year t to t+2. Standard errors in parentheses.
2. The semi-parametric regression specifications include fully interacted industry, year, firm age, firm size and multi-unit effects plus additional controls for pre-buyout growth history. The "ATE=ATE1" specification imposes a uniform treatment effect, while the "ATE1 Heterogeneous" specification allows the treatment effect to vary by firm size, firm age, and the two measures of pre-buyout growth history.
3. Each regression has 1,985,489 observations.

Table 6. Cumulative Two-Year Job Reallocation at Target Firms and Controls, Buyouts from 1980 to 2003

A. Organic Changes, Excluding Acquisitions and Divestitures

Rates Expressed as a Percent of Employment	Target Firms	Control Firms	Difference	*From Tables 4 and 5*	
				Difference	Standard Error
Job Creation	24.96	22.96	2.00	0.94	(0.10)
Continuers	11.51	11.74	-0.22	-0.93	(0.07)
Births (Entrants)	13.44	11.22	2.22	1.87	(0.05)
Job Destruction	26.89	22.62	4.27	4.69	(0.11)
Continuers	13.28	12.65	0.63	0.64	(0.06)
Deaths (Exits)	13.60	9.96	3.64	4.13	(0.08)
Employment Growth	-1.93	0.34	-2.27	-3.75	(0.16)
Job Reallocation	51.85	45.58	6.27	5.62	(0.14)
Excess Reallocation	49.91	45.23	4.68		
Within-Firm	33.09	27.02	6.06	6.40	(0.08)

B. All Adjustment Margins, Including Acquisitions and Divestitures

Rates Expressed as a Percent of Employment	Target Firms	Control Firms	Difference	*From Tables 4 and 5*	
				Difference	Standard Error
Job Creation	35.86	28.42	7.44	6.25	(0.11)
Job Destruction	33.21	26.67	6.53	7.03	(0.13)
Employment Growth	2.65	1.75	0.90	-0.65	(0.16)
Job Reallocation	69.07	55.10	13.97	13.89	(0.15)
Excess Reallocation	66.41	53.35	13.07		
Within-Firm	38.82	29.82	9.01	9.29	(0.09)

Notes:

1. For Target Firms and Control Firms, we aggregate over cells using the employment shares of targets. For cells with multiple controls, each control receives equal weight.

Table 7. Buyout Effects on Target Firms Relative to Controls by Type of Buyout, 1980 to 2003

	Type of Private Equity Buyout				
Dependent Variable	Public to Private	Independent to Private	Divisional Buyout	Secondary Buyout	Other
Employment Growth Rate from Buyout Year *t* to *t*+2	-10.36 (0.42)	10.51 (0.24)	-1.47 (0.45)	7.15 (0.58)	-6.45 (0.80)
Excess Reallocation Rate from Buyout Year *t* to t+2	5.09 (0.24)	4.69 (0.10)	20.32 (0.19)	29.79 (0.27)	6.16 (0.40)
Number of Observations	289,228	1,269,396	456,135	168,508	122,613

Notes:

1. Results are based on the semi-parametric ATE1 Heterogeneous specifications of Tables 4 and 5, fit separately to target and control observations for each type of private equity buyout.

Table 8. Entry and Exit of Manufacturing Plants by Location in Own-Industry Distribution of Total Factor Productivity, Buyouts from 1980 to 2003

A. Plant Exit Probabilities in the First Two Years Post Buyout (Logistic Specification)

Location in Own-Industry TFP Distribution as of the Buyout Year t	*Probability of Plant Exit by Year t+2*		P-value for Difference Between Targets and Controls
	Targets	Controls	
Bottom Tercile	0.143 (0.023)	0.091 (0.002)	0.025
Middle Tercile	0.112 (0.034)	0.062 (0.002)	0.139
Top Tercile	0.078 (0.015)	0.067 (0.002)	0.487

B. Plant Entry Probabilities in the First Two Years Post Buyout (Logistic Specification)

Location in Own-Industry TFP Distribution in t+2, Two Years After Buyout	*Probability that a Plant Operating in t+2 Entered in t+1 or t+2*		P-value for Difference Between Targets and Controls
	Targets	Controls	
Bottom Tercile	0.056 (0.015)	0.121 (0.006)	0.000
Middle Tercile	0.071 (0.016)	0.078 (0.003)	0.590
Top Tercile	0.127 (0.029)	0.072 (0.003)	0.058

Notes:

1. Predicted probabilities implied by logistic regressions, using the propensity weights described in Section 7.A. Standard errors are in parentheses.
2. The dependent variable in panel A equals 1 if the plant exits in the first or second year after the buyout year, zero otherwise. The dependent variable in panel B equals 1 if the establishment enters in the first or second year post buyout, zero otherwise. Entry and exit outcomes are determined using the full LBD.
3. Explanatory variables are terciles of the own-industry*year TFP distribution interacted with target and control dummies. Terciles are defined based on a plant's position in the same-industry/same-year distribution of total factor productivity, using 4-digit SIC and 6-digit NAICS industry definitions.
4. The Panel A regression has about 107,000 observations, and the Panel B regression has about 91,000 observations.

Table 9. Productivity of Manufacturing Plants, Targets and Controls, Buyouts from 1980 to 2003

A. TFP In Buyout Year *t* by Plant Status in Year *t*+2

Dependent Variable: Plant-level Log TFP in Year *t*			P-Value for Difference Between Targets and Controls
Plant Status	Targets	Controls	
Continuers	0.016 (0.012)	Omitted Group	0.180
Exits	-0.075 (0.035)	-0.032 (0.008)	0.232
Divestitures	-0.023 (0.027)	-0.044 (0.007)	0.462
R-Squared	0.538		

B. TFP In Year *t*+2, Two Years After Buyout, by Plant Status in Year *t+2*

Dependent Variable: Plant-level Log TFP in Year t+2			P-Value for Difference Between Targets and Controls
Plant Status	Targets	Controls	
Continuers	0.020 (0.011)	Omitted Group	0.076
Entrants	0.182 (0.055)	-0.039 (0.011)	0.000
Acquisitions	-0.010 (0.047)	-0.030 (0.007)	0.668
R-Squared	0.523		

C. TFP Growth at Continuing Plants, from Buyout Year *t* to *t*+2

Dependent Variable: Change in Plant-level Log TFP from Buyout Year t to t+2			P-Value for Difference Between Targets and Controls
	Targets	Controls	
Continuers	0.001 (0.011)	Omitted Group	0.954
R-Squared	0.071		

Notes:

1. OLS regressions using the propensity weights described in Section 7.A. The omitted group is continuing control plants. All specifications include industry-year effects as well as firm size and age effects. Standard errors in parentheses.
2. There are about 107,000 observations in the Panel A regression, 91,000 in the Panel B regression, and 62,000 in the Panel C regression. On a propensity-weighted basis, 83% of the target observations in Panel A are continuers, 8 percent are entrants and 7 percent are divestitures. Continuers account for 86 percent of target observations in Panel B, entrants for 8 percent, and acquisitions for 6 percent. Plant status is determined using the full LBD. Since target plants are overwhelmingly (99 percent) part of multi-unit firms, this analysis focuses on targets and controls part of multi-unit firms.
3. By design, this table considers firms that continue from the buyout year *t* to *t+2*. If we add firm exits to the sample, the "Exits" row of Panel A changes slightly: the coefficient becomes -0.085 (0.034) for Targets and -0.042 (0.007) for Controls.

Table 10. Impact of Private Equity Buyouts on Total Factor Productivity in the Manufacturing Sector, Buyouts from 1980 to 2003

Estimated Average Two-Year Post-Buyout Change in TFP at Target Firms Relative to Controls, Log Points	
TFP Log Change Differential	2.14
Excluding Acquisitions & Divestitures	2.01

Share of Total TFP Two-Year Change Differential By Margin of Adjustment	
Continuing Establishments	0.20
Entry and Exit	0.74
Acquisitions and Divestitures	0.06

Notes:

1. Table entries are calculated according to Equation (1) using diff-in-diff estimates from Table 9 and share measures retrieved from Table B.1 in the online appendix, as discussed in the main text. The lower panel in the table reports the shares of the TFP log change differential on the left side of equation (1) accounted for by each term on the right side of the equation.
2. The baseline average two-year TFP change for control firms is an estimated -0.38 log points with an estimated standard error of 0.24. This estimate is obtained from a propensity-weighted regression of the two-year log change in TFP on a constant and target status dummy in the sample of continuing target and control plants, the same sample used for Panel C in Table 9.

Table 11. Earnings per Worker (EPW) at Target and Control Establishments, Buyouts from 1980 to 2003

A. EPW In Buyout Year *t* by Establishment Status in Year *t+2*

Dependent Variable: Establishment Log Real EPW in Year *t*			P-Values for Difference Between Targets and Controls
Establishment Status	Targets	Controls	
Continuers	0.011 (0.003)	Omitted Group	0.000
Exits	-0.085 (0.006)	-0.115 (0.004)	0.000
Divestitures	0.163 (0.009)	-0.055 (0.006)	0.000
R-Squared	0.448		

B. EPW in Year *t+2*, Two Years After Buyout, by Establishment Status in Year *t+2*

Dependent Variable: Establishment Log Real EPW in Year *t+2*			P-Values for Difference Between Targets and Controls
Establishment Status	Targets	Controls	
Continuers	-0.031 (0.003)	Omitted Group	0.000
Entrants	0.015 (0.006)	-0.011 (0.004)	0.000
Acquisitions	0.010 (0.007)	-0.015 (0.006)	0.000
R-Squared	0.421		

C. EPW Growth at Continuing Establishments, from Buyout Year *t* to *t+2*

Dependent Variable: Change in Establishment Log Real EPW from Buyout Year *t* to *t+2*			P-Values for Difference Between Targets and Controls
	Targets	Controls	
Continuers	-0.024 (0.002)	Omitted Group	0.000
R-Squared	0.200		

Notes:

1. All specifications include the full cross product of industry, year, firm size, and firm age effects.
2. The reported results are for weighted regressions equivalent to a nonparametric matching estimator in Panel C and approximately equivalent in Panels A and B. The observation for each control establishment is weighted by the ratio of targets to controls in the same industry-year-size-age cell. Similar results obtain with equal weighting of all observations.
3. There are about 1.7 million observations in the Panel A regression, 1.8 million in the Panel B regression, and 1.3 million in the Panel C regression. Like Table 9, this analysis focuses on target and control plants part of multi-unit firms. Results (available upon request) including SU plants are very similar.
4. By design, this table considers firms that continue from the buyout year t to t+2. If we add firm exits to the sample, the "Exits" row of Panel A changes slightly: the coefficient remains -0.085 (0.006) for Targets and becomes -0.120 (0.004) for Controls.

A. Matching Issues and Robustness to EIN Cases

We consider private equity transactions that involve U.S. firms as buyout targets. Our analysis relies on a unique new dataset that combines information on private equity transactions from CapitalIQ and other sources with establishment-level and firm-level data from the Census Bureau's Longitudinal Business Database (LBD) and Annual Surveys and Censuses of Manufactures (ASM and CM). The linkage is accomplished by matching name and address information for target firms in private equity buyout transactions to name and address data of their establishments in the Census Bureau's Business Register (BR).[1] The BR, LBD, ASM and CM all contain common identifiers that facilitate easy linking of establishment and firm data across each source. We use the LBD to follow target firms and their establishments over time, obtaining annual observations. We also use the BR and LBD to identify controls (comparable firms and establishments) and follow them over time as well. Our productivity analysis involves additional matching to observations in the CM and ASM.

The LBD tracks establishments and their 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 ASM). Information about the company structure is incorporated in the LBD by attaching firm identifiers to records for establishments (physical locations where economic activity occurs). Ownership changes are identified when establishments switch parent firm through mergers, acquisitions and divestitures.

The Census Bureau assigns a unique firm ID to all the establishments under common ownership and control in a given year, including establishments that belong to subsidiaries under control of the parent corporation. This firm ID is distinct from a taxpayer ID such as the employer identification number (EIN). The relationships among the various IDs are as follows. In any given year, an establishment is uniquely associated with a single taxpayer ID and a single firm ID. Moreover, each taxpayer ID is uniquely associated with a firm ID. In the case of 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.

To match deals and target firms in the Capital IQ data to the Census Business Register, our main method works as follows. First, we use name and address information in the two data sources 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 matched establishment owned by the target firm – often but not always a headquarters facility. In a second step, we use the BR link between that establishment's ID and its firm ID to identify the target firm in the BR. In most cases, this method accurately identifies the target firm in the BR and all of its activity.

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

[1] The LBD is actually a value-added product constructed by longitudinally linking annual BR snapshots. The LBD is engineered to link trivially to the BR.

ownership change of the division or subsidiary involved in the deal. We identified these problematic cases by observing that the matched target establishment remained affiliated with the parent seller firm even after the transaction. It is our understanding that the Census Bureau on occasion had difficulty tracking the new firm in divisional buyouts because of nonresponse on the COS or other survey instruments. In considering these cases, it is important to note that the BR still accurately tracks the activity of the target establishments over time.

We thus had two types of divisional cases. The first are those where we could accurately identify the target firm using our main method. The second are those where we could not accurately identify the target firm using our main method. Even in those cases, we were able to link the matched establishment to at least a part of the target firm through the EIN (taxpayer ID). The complete target firm may or may not be identified in such cases, because the divisional business involved in the buyout may have operated with multiple EINs. In the main text and this appendix, we refer to such cases as EIN cases. In these EIN cases, we can accurately identify a part of the target firm in the transaction year and at least some of the corresponding target establishments.

Given the presence of EIN cases in our matched data samples, we proceed as follows. In the establishment-level analysis that tracks the pre and post outcomes of target and control establishments, we use both firm ID cases based on our main method and EIN cases. Longitudinal establishment links in the BR allow us to track the pre and post outcomes regardless of ownership, so the inclusion of EIN cases involves no issues for our establishment-level analysis.

We exclude the EIN cases in the firm-level analysis, because the EIN is not suitable for tracking firms over time. For example, a target firm that adds a new establishment may obtain a new EIN for that establishment for accounting or tax reasons. Table 2 in the main text reports statistics for the EIN cases in the sample for the 1980-2003 period. There are 391 EIN cases over this period out of a total of 2265 target firms.

To check the sensitivity of our results to the EIN cases, we repeated the establishment-level analysis in Section 5 of the main text for the subsample that excludes the EIN cases. Figures A.1 to A.3 and Table A.1 below report the results. In all cases, they are quite similar to those reported in the main text. We conclude that omission of the 391 EIN cases is unlikely to cause a serious bias in the firm-level analyses reported in the main text.

We also encountered other matching problems when integrating the Census Business Register and Capital IQ data. In a few case cases where we retimed the transaction forward, we could not accurately identify the target firm in the forward year. These 16 cases are excluded from our analysis.

Figure A.1A: Comparison of Employment Trajectory for Target Establishments to Controls, Buyouts from 1980 to 2000, Excluding EIN Cases

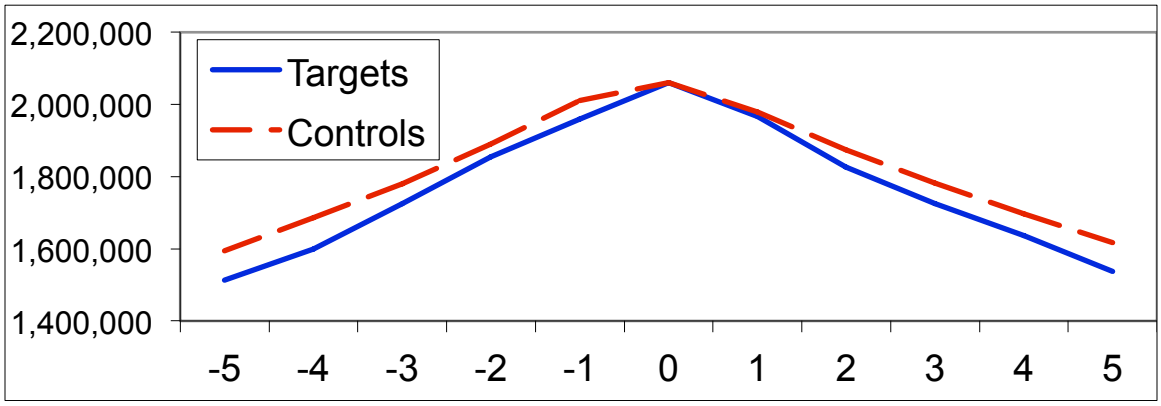


Figure A.1B: Employment Growth Rate Differences before and after the Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000, Excluding EIN Cases

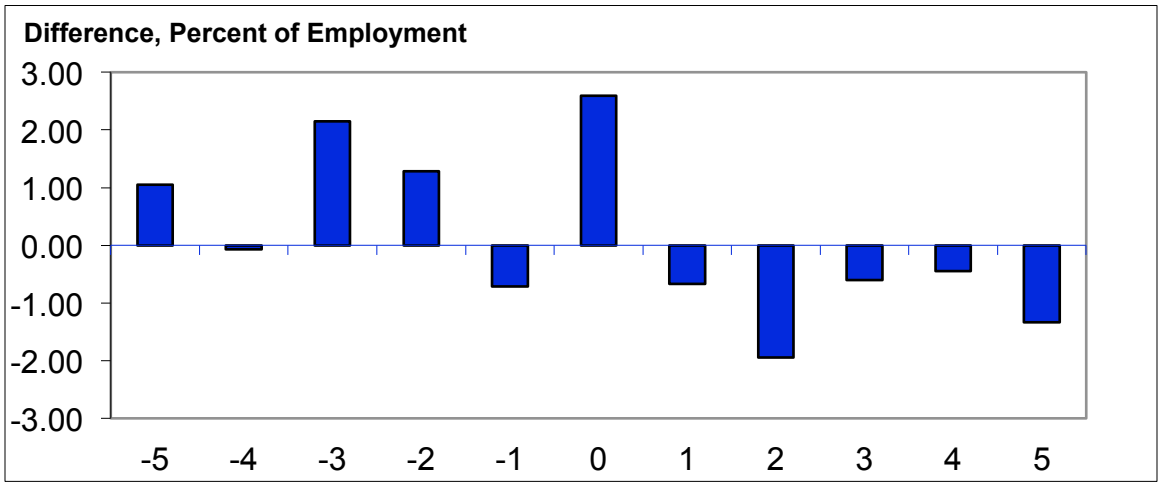


Figure A.2A: Job Creation Rates before and after Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000, Excluding EIN Cases

Figure A.2B: Job Destruction Rates before and after Buyout Year, Target Establishments Minus Controls, Buyouts from 1980 to 2000, Excluding EIN Cases

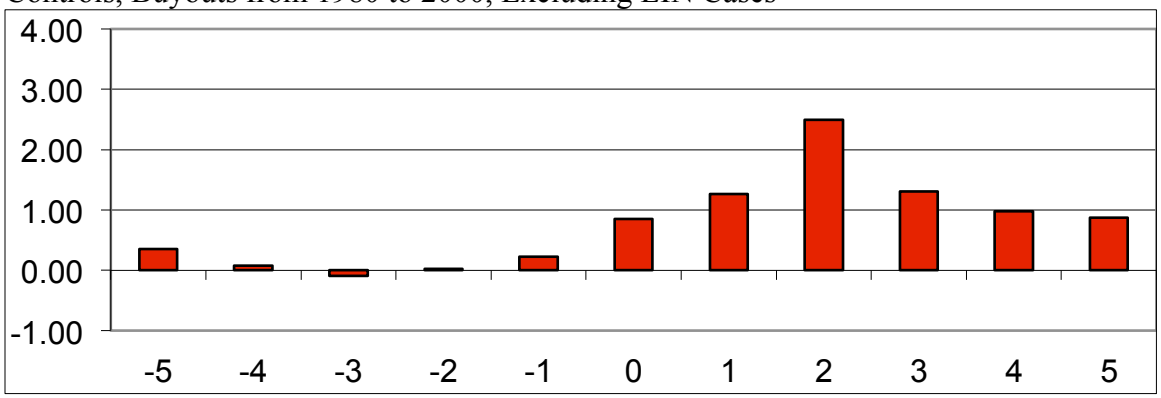


Figure A.3: Employment-Weighted Establishment Exit Rates Post Buyout, Targets Minus Controls, Buyouts from 1980 to 2000, Excluding EIN Cases

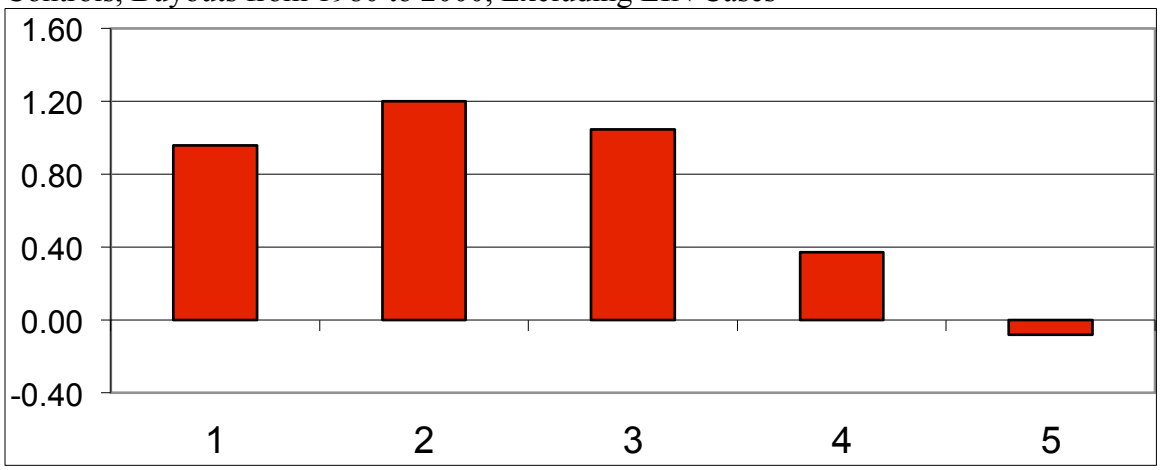


Table A.1 Post-Buyout Employment Growth Rates at Target Establishments Relative to Controls, Buyouts from 1980 to 2000, Excluding EIN cases.

	Non-Parametric Comparison From Figure A.1B	Regression Approaches	
		ATE=ATE1	ATE1 Heterogeneous
Buyout Year	2.59	2.75	2.97
		(0.18)	(0.19)
Buyout Year +1	-0.67	-0.45	-0.94
		(0.21)	(0.21)
+2	-1.94	-1.39	-1.44
		(0.22)	(0.22)
+3	-0.60	-0.09	-0.01
		(0.22)	(0.23)
+4	-0.45	0.13	0.25
		(0.23)	(0.23)
+5	-1.33	-1.32	-1.55
		(0.24)	(0.24)
Cumulative, Years 1 to 5	-4.99	-3.13	-3.69

Notes:

1. See notes 1, 2 and 3 to Table 3 in the main text.
2. The average number of establishment-level observations in each regression or nonparametric comparison is about 4.3 million. The observation count falls with each successive year following the transaction year because of target deaths and deleted observations for the corresponding control establishments.

B. Supplemental Descriptive Statistics Referenced in Section 4 of the Main Text

Figures B.1 and B.2 referenced in Section 4 of the main text are displayed below.

Figure B.1A: Industry Distribution of Employment, Buyout Targets Compared to the Full LBD, 2002 to 2005

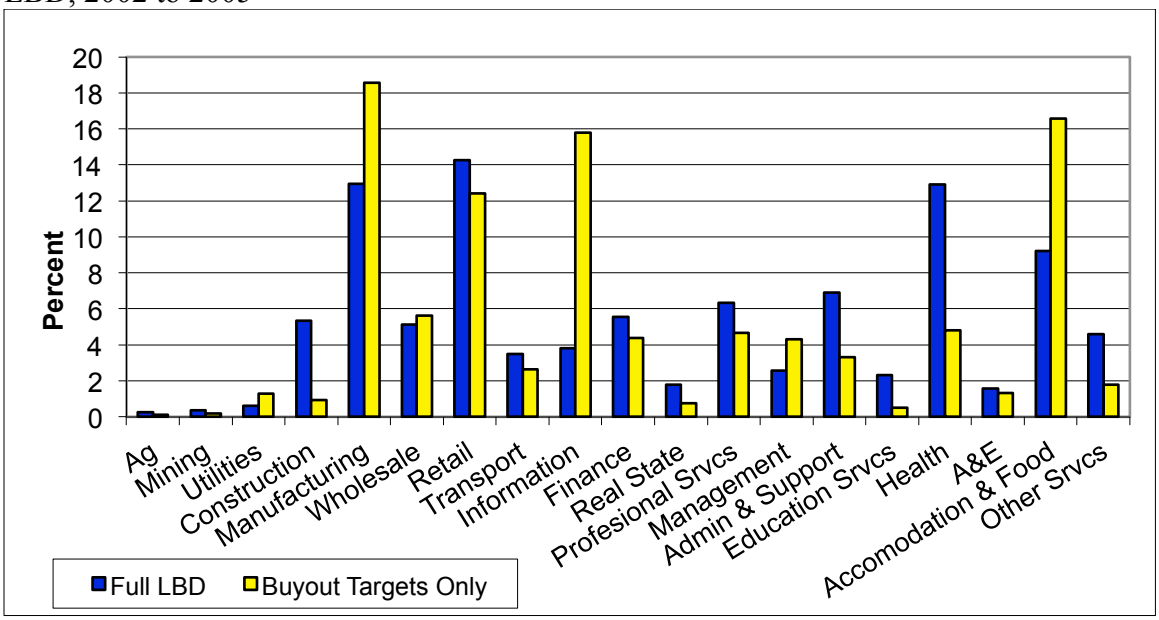


Figure B.1B: Industry Distribution of Employment, Buyout Targets Compared to the Full LBD, 1981 to 2001

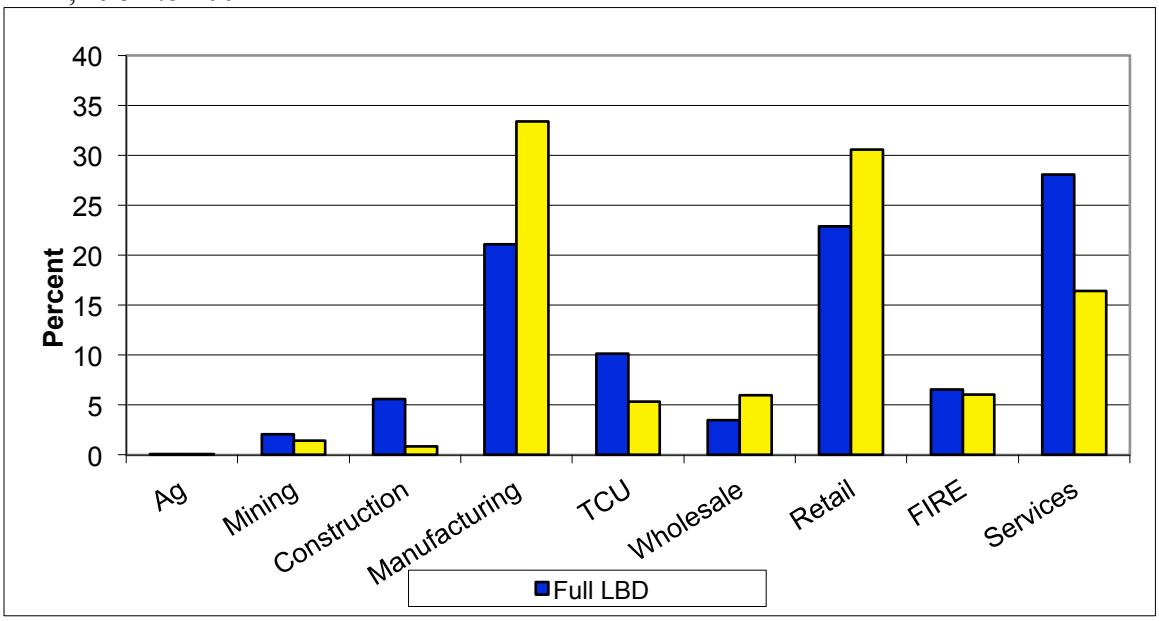


Figure B.2A: Distribution of Employment by Firm Size (Number of Employees), Buyout Targets Compared to Full LBD, 1980-2005

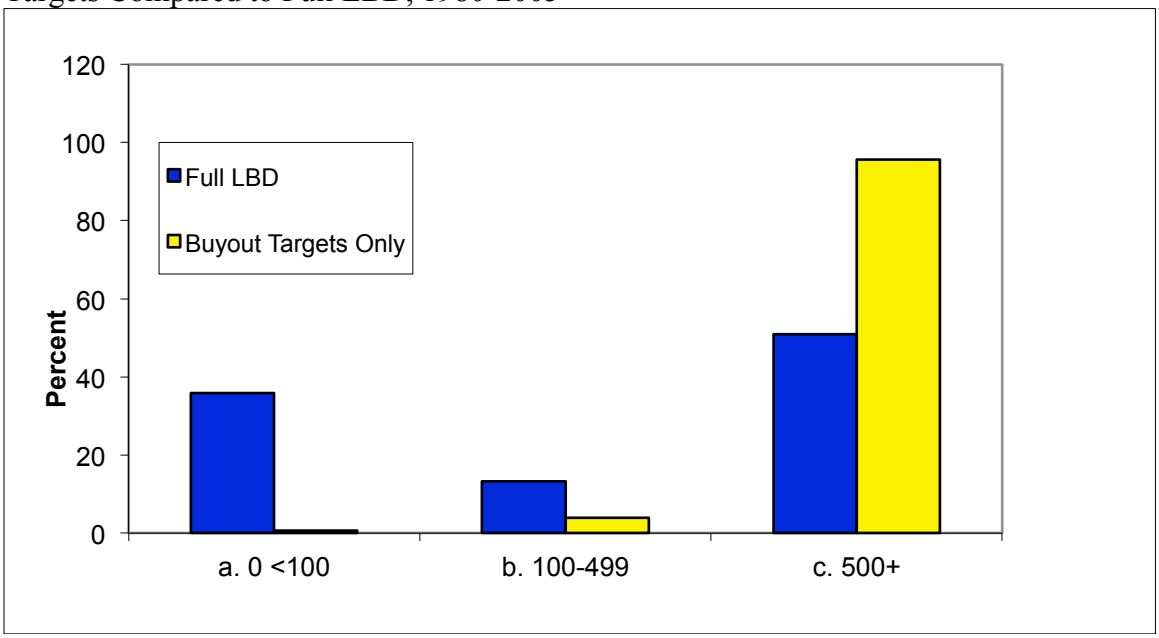
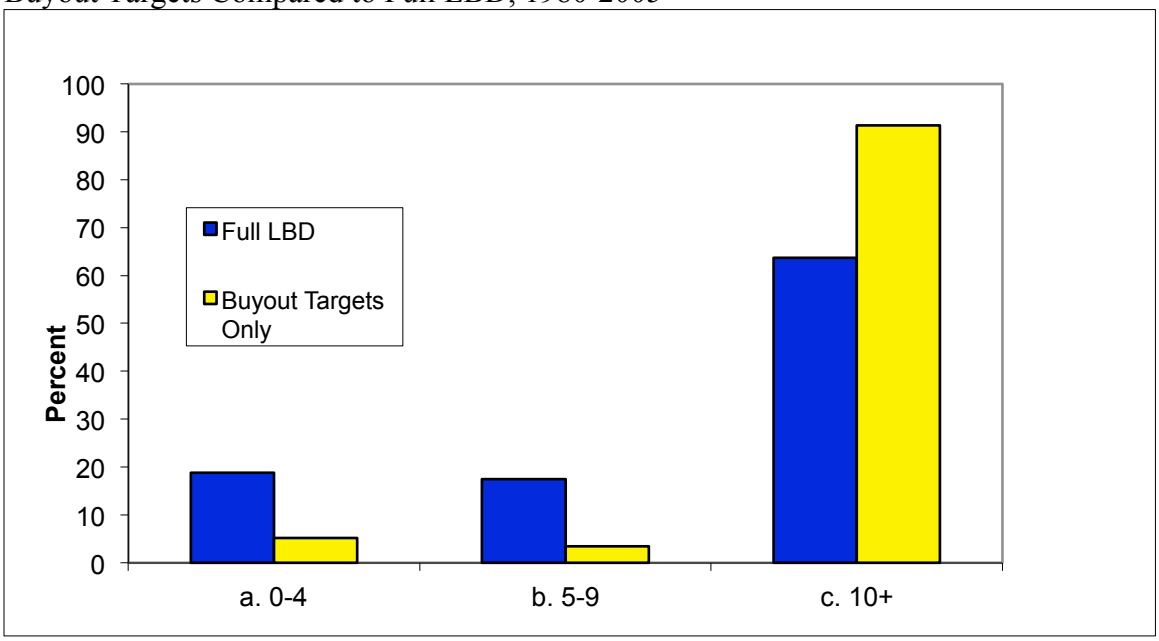


Figure B.2B: Distribution of Employment by Age of Firm's Oldest Establishment, Buyout Targets Compared to Full LBD, 1980-2005



C. Semi-Parametric Regression Specifications and Supplemental Results Referenced in Sections 5, 6 and 7 of the Main Text

Table 3, 4 and 5 report results for two types of semi-parametric regression specifications: an ATE=ATE1 specification that imposes uniform treatment effects and an ATE1 Heterogeneous specification that allows treatment effects to vary by firm size, firm age and our two measures of pre-buyout growth history.

The ATE=ATE1 specification can be written

$$Y_{i,t+j} = \alpha_j + \sum_c D_{cit}\theta_{c,j} + \lambda_{o,j}LFIRM_{it} + \lambda_{1,j}LEST_{it} + \gamma_j PE_{it} + \varepsilon_{i,t+j},$$

where Y_{it} is the outcome variable (e.g,. rate of growth, job creation, or job destruction) for firm *i* at time *t+j* for some $j \in \{-5,-4,...,0,...4,5\}$, , D_{cit} is a set of dummy variables for cell c for firm *i* at time *t* where the cells are defined in by the full cross product of buyout year *t*, industry, firm size category, firm age category and multi-unit status, $LFIRM_{it}$ is the growth rate from *t-3* to *t-1* of the parent firm as of *t-3*, $LEST_{it}$ is the growth rate from *t-3* to *t-1* for the establishments that constitute the target firm in year *t*, and PE_{it} is a dummy variable equal to 1 for a target firm.

The ATE1 Heterogeneous specification is

$$Y_{i,t+j} = \alpha_j + \sum_c D_{cit}\theta_{c,j} + \lambda_{o,j}LFIRM_{it} + \lambda_{1,j}LEST_{it} + \sum_k D_{kit}PE_{it}\gamma_{k,j} + \gamma_{o,j}LFIRM_{it}PE_{it} + \gamma_{1,j}LEST_{it}PE_{it} + \varepsilon_{i,t+j}$$

All variables are defined as above, but the ATE1 heterogeneous specification allows the treatment effect to vary by characteristics including a set of cell-based variables *k* (in practice by firm size and by firm age – not interacted) and the lagged growth terms. To recover the average treatment effect on the treated in this case, we compute a weighted average of the heterogeneous estimated treatment effects, using cell-level employment weights of targets in the buyout year. We calculate standard errors by the Delta method.

As remarked in Section 5 of the main text, employment expands at target and control establishments before the buyout year, and it shrinks at both sets of establishments after the buyout. This pattern is evident in Figure C.1 below.

Tables C.1 and C.2 provide additional information related to Table 6 of Section 6. Figures C.2 and C.3 and Table C.3 report results referenced in Section 6.D in the main text. Figure C.2 shows that target-control employment growth rate differences are negative for buyouts that took place in the 1980s, from 1990 to 94, and from 1995 to 2000. Figure C.3 shows the evolution of target-control employment growth rate differences before and after buyouts from 1980 to 2000 in three major industry sectors: Manufacturing, Retail Trade, and Services. Table C.3 provides an expanded version of

Table 7 in the main text. Table C.4 reports results referenced in Section 7.D for EPW regressions by type of private equity buyout and by major industry sector.

Figure C.1: Employment Growth Rates before and after the Buyout Year, Targets and Controls Compared, Buyouts from 1980 to 2000

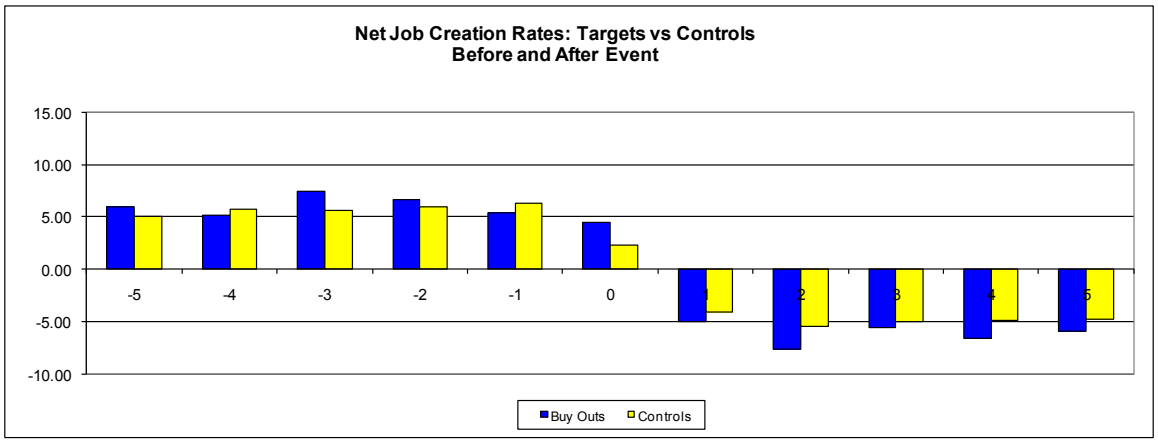


Table C.1: Cumulative Two-Year Reallocation at Target Firms and Controls, Buyouts from 1980 to 2003, Role of Within-Firm/Within-County Reallocation

Rates Expressed as a Percent of Employment	Target Firms	Control Firms	Difference
Organic Changes Only, Excluding Acquisitions and Divestitures			
Job Reallocation	51.85	45.58	6.27
Excess Within-Firm Job Reallocation	33.09	27.02	6.06
Excess Within-Firm/Within-County Job Reallocation	16.10	13.52	2.58
All Adjustment Margins			
Job Reallocation	69.07	55.10	13.97
Excess Within-Firm Job Reallocation	38.82	29.82	9.01
Excess Within Firm/County Job Reallocation	18.08	14.57	3.51

Notes: See notes to Table 6 and Section 6.C in the main text.

Table C.2: Cumulative Two-Year Job Reallocation at Target Firms And Controls, Buyouts from 1980 to 2003, Manufacturing Only

A. Organic Changes, Excluding Acquisitions and Divestitures

Rates Expressed as a Percent of Employment	Target Firms	Control Firms	Difference
Job Creation	13.34	14.62	-1.29
Continuers	7.81	9.25	-1.45
Births	5.53	5.37	0.16
Job Destruction	19.73	19.44	0.29
Continuers	11.35	11.50	-0.15
Deaths	8.38	7.93	0.45
Employment Growth	-6.39	-4.81	-1.58
Job Reallocation	33.07	34.06	-0.99
Excess Within-Firm Reallocation	17.13	17.70	-0.57

B. All Adjustment Margins, Including Acquisitions and Divestitures

Rates Expressed as a Percent of Employment	Target Firms	Control Firms	Difference
Job Creation	22.01	20.17	1.84
Job Destruction	30.22	23.80	6.42
Employment Growth	-8.22	-3.63	-4.58
Job Reallocation	52.23	43.97	8.26
Excess Within-Firm Reallocation	23.63	21.78	1.86

Notes: See notes to Table 6 in the main text.

Figure C.2: Employment Growth Rate Difference before and after the Buyout Year, Target Establishments Minus Controls, by Time Period of Buyout Event

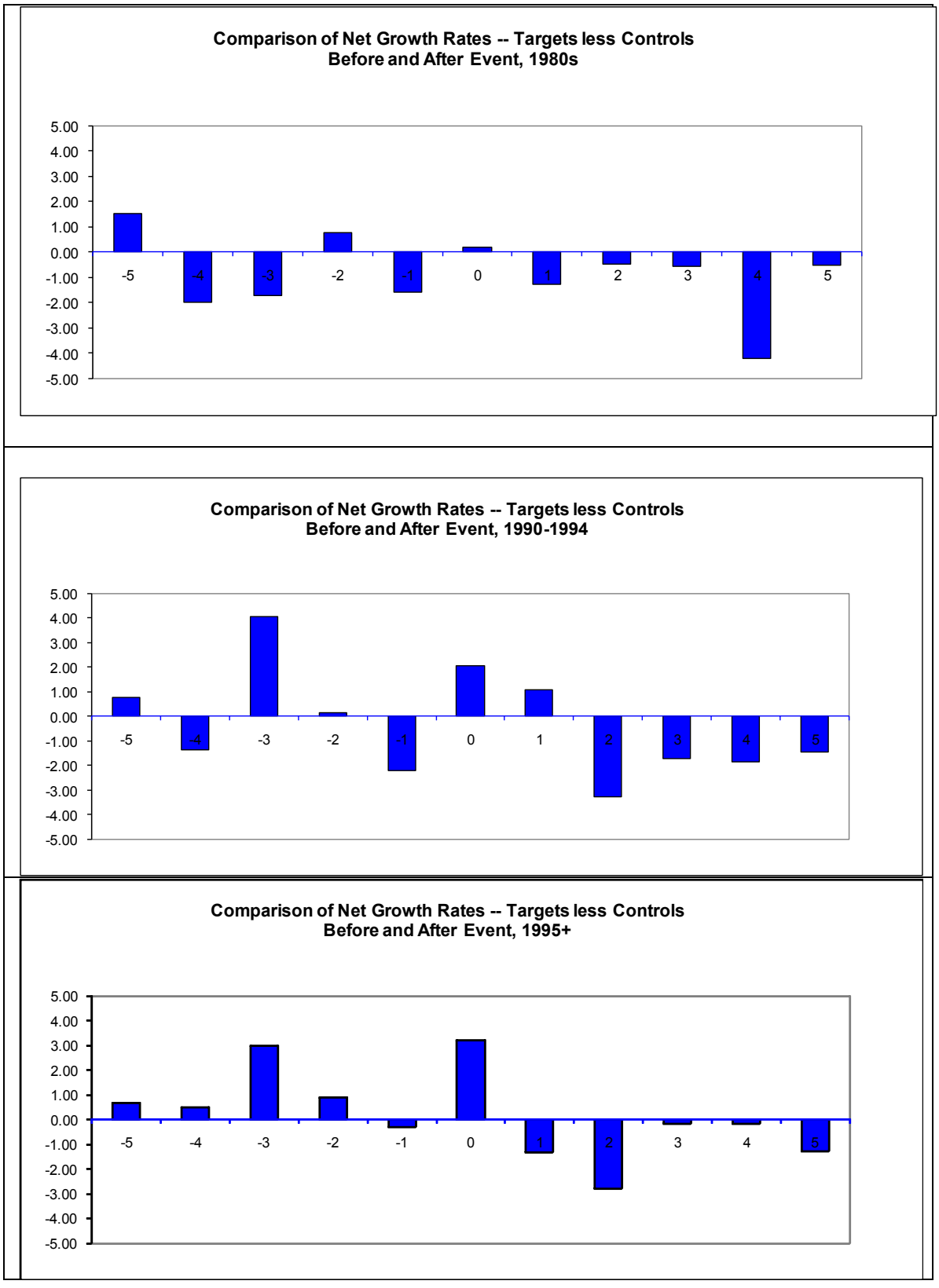
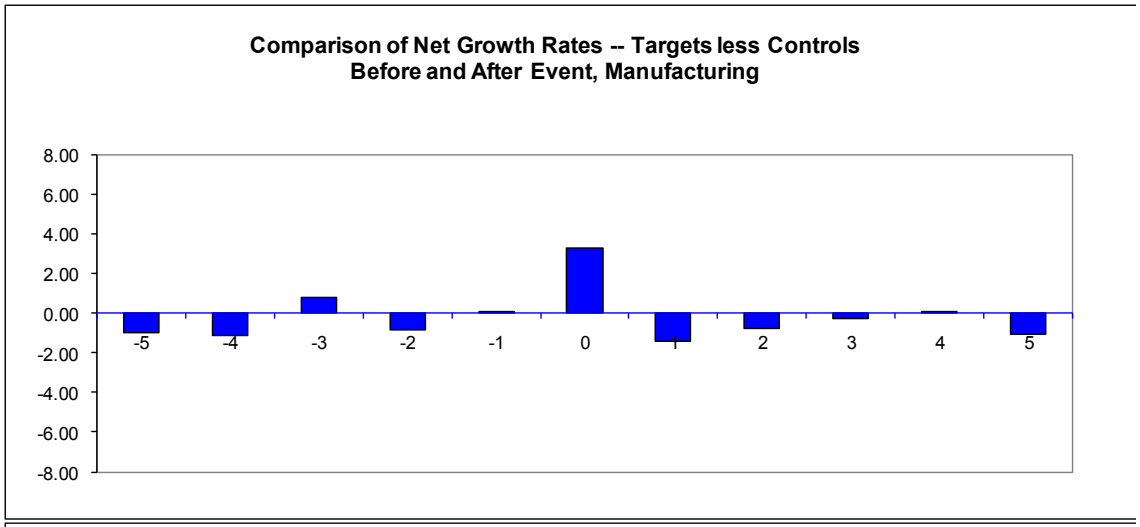
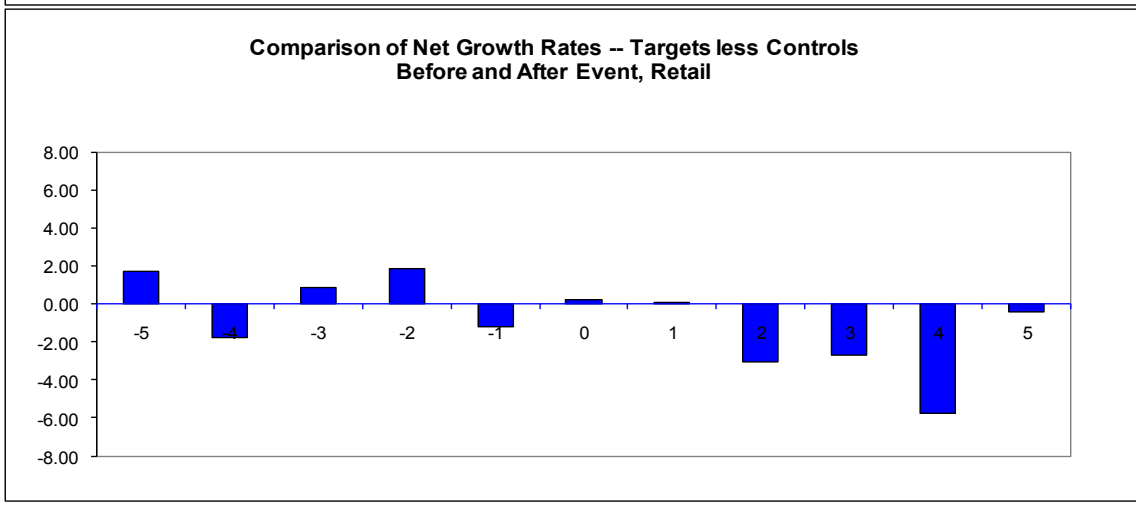


Figure C.3: Employment Growth Rate Difference for Selected Industry Sectors, Target Establishments Minus Controls, Buyouts from 1980 to 2000





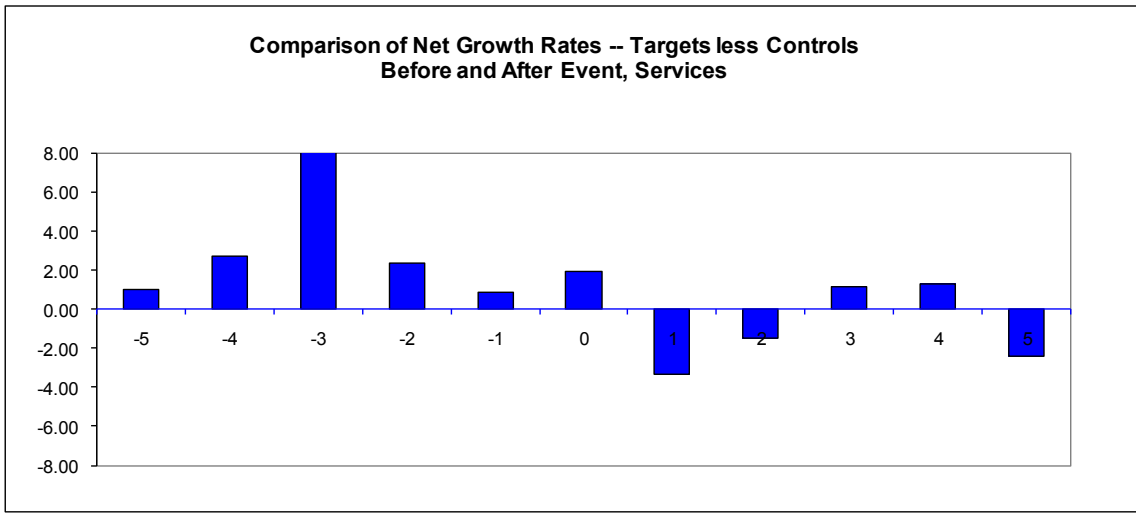


Table C.3: Buyout Effects on Target Firms Relative to Controls by Type of Buyout and Adjustment Margin, Buyouts from 1980 to 2003 (An Expanded Version of Table 7)

	Type of Private Equity Buyout				
Dependent Variable	Public to Private	Independent to Private	Divisional Buyout	Secondary Buyout	Other
Employment Growth Rate from Buyout Year *t* to *t*+2	-10.36	10.51	-1.47	7.15	-6.45
	(0.42)	(0.24)	(0.45)	(0.58)	(0.80)
Adjustment Margin					
Continuers	1.40	1.44	-1.42	3.11	-1.84
	(0.25)	(0.16)	(0.31)	(0.38)	(0.56)
Divestitures	6.80	0.15	5.34	-0.06	-1.52
	(0.13)	(0.05)	(0.11)	(0.16)	(0.19)
Deaths	5.95	1.50	5.97	12.14	9.28
	(0.19)	(0.12)	(0.23)	(0.30)	(0.44)
Births	-3.12	-0.38	3.32	17.23	1.5
	(0.14)	(0.07)	(0.13)	(0.19)	(0.25)
Acquisitions	3.98	10.98	7.61	-1.21	1.7
	(0.25)	(0.06)	(0.12)	(0.16)	(0.21)
Job Creation All	1.15	11.02	12.08	17.22	1.70
	(0.24)	(0.14)	(0.27)	(0.37)	(0.50)
Job Destruction All	11.62	0.55	13.81	11.23	8.12
	(0.26)	(0.17)	(0.30)	(0.40)	(0.56)
Excess Reallocation All	5.09	4.69	20.32	29.79	6.16
	(0.24)	(0.10)	(0.19)	(0.27)	(0.40)
Number of Observations	289,228	1,269,396	456,135	168,508	122,613

Notes:

1. The results are based on semi-parametric regressions that include fully interacted industry, year, firm age, firm size and multi-unit effects plus additional controls for pre-buyout growth history. The specification allows the treatment effect to vary by firm size, firm age, and the two measures of pre-buyout growth history.

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Table C.4: Earnings per Worker (EPW) at Target and Control Establishments, Buyouts from 1980 to 2003, By Type of Buyout and Selected Industry Sector

	A. Type of Private Equity Buyout									
	Public to Private		Private to Private		Divisional Buyout		Secondary Buyout		Other	
Dependent Variable: Establishment-Level Log Real EPW in Year *t*	Targets	Controls	Targets	Controls	Targets	Controls	Targets	Controls	Targets	Controls
Continuers	0.030 (0.006)	Omitted Group	-0.024 (0.003)	Omitted Group	0.184 (0.009)	Omitted Group	0.045 (0.008)	Omitted Group	-0.091 (0.011)	Omitted Group
Exits	-0.039 (0.012)	-0.184 (0.010)	-0.163 (0.010)	-0.106 (0.005)	-0.081 (0.018)	-0.107 (0.011)	0.026 (0.014)	-0.014 (0.010)	-0.075 (0.030)	-0.166 (0.012)
Divestitures	0.079 (0.013)	-0.112 (0.010)	0.251 (0.017)	-0.029 (0.009)	0.178 (0.018)	0.032 (0.014)	0.200 (0.059)	-0.173 (0.017)	0.155 (0.066)	0.136 (0.023)
Dependent Variable: Establishment-Level Log Real EPW in Year *t+2*										
Continuers	-0.057 (0.007)	Omitted Group	-0.060 (0.003)	Omitted Group	0.148 (0.009)	Omitted Group	0.089 (0.008)	Omitted Group	-0.105 (0.011)	Omitted Group
Entrants	0.017 (0.012)	0.008 (0.014)	-0.067 (0.009)	0.039 (0.005)	0.005 (0.017)	-0.217 (0.011)	0.131 (0.012)	-0.001 (0.010)	0.113 (0.030)	-0.024 (0.015)
Acquisitions	-0.238 (0.026)	0.039 (0.016)	0.062 (0.009)	0.038 (0.007)	-0.191 (0.022)	-0.119 (0.013)	0.180 (0.026)	-0.003 (0.013)	-0.040 (0.060)	-0.066 (0.026)
Dependent Variable: Change in Log Real EPW from *t* to *t+2*										
Continuers from t to t+2	-0.072 (0.005)	Omitted Group	-0.023 (0.003)	Omitted Group	-0.004 (0.008)	Omitted Group	0.048 (0.007)	Omitted Group	0.016 (0.009)	Omitted Group

	B. Major Industry Sector									
	Manufacturing		Wholesale Trade		Retail Trade		FIRE		Services	
	Targets	Controls	Targets	Controls	Targets	Controls	Targets	Controls	Targets	Controls
Dependent Variable: Establishment-Level Log Real EPW in Year *t*										
Continuers in Year *t*	-0.004 (0.010)	Omitted Group	0.140 (0.008)	Omitted Group	0.032 (0.004)	Omitted Group	-0.090 (0.006)	Omitted Group	0.072 (0.006)	Omitted Group
Deaths	-0.035 (0.017)	-0.079 (0.012)	0.058 (0.025)	-0.070 (0.012)	-0.069 (0.012)	-0.112 (0.007)	-0.176 (0.014)	-0.126 (0.006)	0.096 (0.014)	-0.152 (0.012)
Divestitures	-0.030 (0.015)	-0.017 (0.011)	0.027 (0.074)	0.002 (0.016)	0.324 (0.014)	-0.129 (0.007)	0.107 (0.055)	0.046 (0.012)	0.253 (0.028)	-0.318 (0.017)
Dependent Variable: Establishment-Level Log Real EPW in Year *t+2*										
Continuers in Year t+2	-0.012 (0.011)	Omitted Group	0.041 (0.009)	Omitted Group	-0.059 (0.004)	Omitted Group	-0.024 (0.007)	Omitted Group	0.020 (0.007)	Omitted Group
Entrants	0.028 (0.022)	0.016 (0.011)	0.134 (0.030)	-0.124 (0.009)	-0.028 (0.010)	0.030 (0.006)	0.081 (0.012)	0.024 (0.006)	0.184 (0.013)	-0.042 (0.019)
Acquisitions	-0.066 (0.021)	-0.064 (0.017)	0.102 (0.036)	-0.098 (0.016)	0.048 (0.009)	0.152 (0.011)	0.215 (0.058)	-0.039 (0.007)	-0.030 (0.022)	0.071 (0.018)
Dependent Variable: Change in Log Real EPW from *t* to *t+2*										
Continuers from *t* to *t+2*	0.009 (0.006)	Omitted Group	-0.081 (0.009)	Omitted Group	-0.074 (0.003)	Omitted Group	0.092 (0.006)	Omitted Group	-0.064 (0.006)	Omitted Group

Notes: See notes to Table 11. Panel A reports results by type of buyout, and Panel B reports results by industry for industries with large numbers of buyouts. In Panel B we assign establishments to the industry of its parent firm, defined by the model industry of the establishments operated by that firm. We exclude single-unit firms and firms that exit in the first two years post buyout. These sample selection criteria have little impact on the results, because target firms are predominantly multi-unit continuers.