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PRIVATE EQUITY AND EMPLOYMENT

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Abstract

The impact of private equity on employment arouses considerable controversy. Labor groups concerned about the fortunes of workers employed at buyout firms contend private equity firms destroy jobs. By contrast private equity associations and other groups have released a number of recent studies that claim positive effects of private equity on employment following the takeover. In this paper we conduct a comprehensive examination of the impact of private equity buyouts on the employment outcomes of firms that are taken over by these investment firms. We focus the analysis on two different dimensions. First, what are the employment outcomes of workers employed at establishments existing at the time of the buyout? Second, what is growth performance of the firm following the buyout? We conduct the analysis using a unique linked dataset covering the universe of US firms and information regarding US buyout operations between 1980 and 2005. We find targeted establishments exhibit net employment contraction, higher job destruction and establishment exit relative to controls.

However, targeted firms exhibit higher greenfield entry and more acquisition and divestiture.

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1. Introduction

The impact of private equity on employment arouses considerable controversy. Speaking about hedge funds and private equity groups in April 2005, Franz Müntefering, then chairman of the German Social Democratic Party (and soon to be German vice chancellor), contended that “Some financial investors don’t waste any thoughts on the people whose jobs they destroy.”²

Contentions like these have not gone unchallenged. Private equity associations and other groups have released several recent studies that claim positive effects of private equity on employment. Examples include European Venture Capital Association (2005), British Venture Capital Association (2006), A.T. Kearney (2007), and Taylor and Bryant (2007). While efforts to bring data to the issue are highly welcome, these studies have significant limitations:³

- Reliance on surveys with incomplete response, giving rise to concerns that the data do not accurately reflect the overall experience of employers acquired by private equity groups.
- Inability to control for employment changes in comparable firms. When a firm backed by private equity sheds 5 percent of employment, the interpretation depends on whether comparable firms grow by 3 percent or shrink by 10 percent.

²<http://www.telegraph.co.uk/money/main.jhtml?xml=/money/2005/10/14/cnmunt14.xml> (accessed 3 November 2007). John Adler of the Service Employees International Union uses less inflammatory language but offers a similar assessment: “Typically it’s easier to decrease costs quickly by cutting heads, which is why buyouts have typically been accompanied by layoffs” (“Private equity and the jobs cut myth,” by Grace Wong, *CNNMoney.com*, 2 May 2007 at http://money.cnn.com/2007/05/02/markets/pe_jobs/index.htm (accessed 10 December 2007).) For remarks with a similar flavor by Phillip Jennings, general secretary of the UNI global union, see “Davos 2007: Private equity under fire” by Larry Elliot, *Guardian Unlimited*, 25 January 2007 at <http://www.guardian.co.uk/business/2007/jan/25/privateequity.globalisation> (accessed 10 December 2007).

³ See Service Employees International Union (2007) and Hall (2007) for detailed critiques. We discuss academic studies of private equity and employment in Section 2 below.

- Failure to distinguish cleanly between employment changes at firms backed by venture capital and firms backed by other forms of private equity. Both are interesting, but the recent debate focuses on buyouts and other later-stage private equity transactions, not venture capital.
- Difficulties in disentangling organic job growth from acquisitions, divestitures, and reorganizations at firms acquired by private equity groups. The prevalence of complex ownership changes and reorganizations at these firms makes it hard to track employment using only firm-level data. Limiting the analysis to firms that do not experience these complex changes is one option, but the results may then reflect a highly selected, unrepresentative sample.
- Inability to determine where jobs are being created and destroyed. Policy makers are not indifferent to whether jobs are created domestically or abroad. Some view foreign job creation in China, India and other emerging economies with alarm, especially if accompanied by job cuts in the domestic economy.

In this study, we construct and analyze a data set that overcomes these limitations and, at the same time, encompasses a much larger set of employers and private equity transactions. We rely on the Longitudinal Business Database (LBD) at the U.S. Bureau of the Census to follow employment at virtually all private equity-backed companies in the United States, before and after private equity transactions. Using the LBD, we follow employment at the level of firms *and* establishments – i.e., specific factories, offices, retail outlets and other distinct physical locations where business takes place. The LBD covers the entire non-farm private sector and includes annual data on employment and payroll for about 5 million firms and 6 million establishments.

We combine the LBD with data from Capital IQ and other sources to identify and characterize private equity transactions. The resulting analysis sample contains about 5,000 U.S. firms acquired in private equity transactions from 1980 to 2005 (“target firms”) and about 300,000 U.S. establishments operated by these firms at the time of the private equity transaction (“target establishments”). To construct control groups, we match each target establishment to other establishments in the transaction year that are comparable in terms of industry, age, size, and an indicator for whether the parent firm operates multiple establishments. We take a similar approach in constructing controls for 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 initial transaction usually 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 a private equity firm. We also exclude startup firms backed by venture capitalists.

Our analysis of employment outcomes associated with private equity transactions has two main components. First, we track employment at target establishments for five years before and after the private equity transaction, irrespective of whether these establishments are owned and operated by the target firm throughout the entire time period around the private equity transaction. We compare the employment path for target establishments to the path for the control establishments. This component of our analysis

circumvents the difficulties of firm-level analyses described above. Second, we consider outcomes for target firms – including the jobs they create at new “greenfield” establishments in the wake of private equity transactions. We quantify greenfield job creation by target firms backed by private equity and compare to greenfield job creation by control firms. Taken together, these two components yield a fuller picture of the relationship between private equity transactions and employment outcomes.

To summarize the main findings of our establishment-level analysis:

1. Employment shrinks more rapidly in target establishments than in control establishments in the wake of private equity transactions. The average cumulative two-year employment difference is about 7 percent in favor of controls.
2. However, employment also grows more slowly at target establishments in the year of the private equity transaction and in the two preceding years. The average cumulative employment difference in the two years before the transaction is about 4 percent in favor of controls. In short, employment growth at controls outstrips employment growth at targets after *and* before the private equity transaction.
3. Gross job creation (i.e., new employment positions) in the wake of private equity transactions is similar in target establishments and controls, but gross job destruction is substantially greater at targets. In other words, the post-transaction differences in employment growth mainly reflect greater job destruction at targets.
4. In the manufacturing sector, which accounts for about a quarter of all private equity transactions since 1980, there are virtually no employment growth differences between target and control establishments after private equity

transactions. In contrast, employment falls rapidly in target establishments compared to controls in Retail Trade, Services and Finance, Insurance and Real Estate (FIRE).

The foregoing results describe outcomes relative to controls for establishments operated by target firms as of the private equity transaction year. They do not capture greenfield job creation at new establishments opened by target firms. To address this issue, we examine employment changes at the target firms that we can track for at least two years following the private equity transaction. This restriction reduces the set of targets we can analyze relative to the establishment-level analysis. Using this limited set of targets, we find the following:

5. Greenfield job creation in the first two years post transaction is 15 percent of employment for target firms and 9 percent for control firms. That is, firms backed by private equity engage in 6 percent more greenfield job creation than the controls.

This result says that bigger job losses at target establishments in the wake of private equity transactions (result 1 above) are at least partly offset by bigger job gains in the form of greenfield job creation by target firms. However, we have not yet performed an apples-to-apples comparison of these job losses and gains. As mentioned above, our firm-level analysis – including the part focused on greenfield job creation – relies on a restricted sample.

Our firm-level analysis also uncovers another interesting result:

6. Private equity targets engage in more acquisitions and more divestitures than controls. In the two-year period after the private equity transaction, the

employment-weighted acquisition rate is 7.3 percent for target firms and 4.7 percent for controls. The employment-weighted divestiture rate is 5.7 percent for target firms and 2.9 percent for controls.

This final result, like the result for greenfield job creation, reflect outcomes in the restricted sample of target firms that we can match to the LBD and follow for at least two years post transaction. The selection characteristics of the restricted sample may lead us to understate the employment performance of target firms, an issue that we are currently exploring.

Especially when taken together, our results suggest that private equity groups act as catalysts for creative destruction. Result 1 says that employment falls more rapidly at targets post transaction, in line with the view that private equity groups shrink inefficient, lower value segments of underperforming target firms. We also find higher employment-weighted establishment exit rates at targets than at controls in both the full and restricted samples. At the same time, however, result 5 says that private equity targets engage in more greenfield job creation than controls. This result suggests that private equity groups accelerate the expansion of target firm activity in new, higher value directions. Result 6 says that private equity also accelerates the pace of acquisitions and divestitures. These results fit the view that private equity groups act as catalysts for creative destruction activity in the economy, but more research is needed to fully address this issue.

Our study offers a rich set of new results on employment outcomes in the wake of private equity transactions. However, our analysis also has significant limitations, two of which we mention now. First, employment outcomes capture only one aspect of private equity transactions and their effects on firm-level and economy-wide performance. A

full evaluation would consider a broader range of outcomes and issues, including the effects of private equity on compensation, profits, productivity, the health of target firms, and the efficiency of resource allocation. This paper seeks to provide useful evidence on just one element of a fuller evaluation. We intend to address many of the other elements of a fuller evaluation in follow-on work using the LBD database and other sources.

Second, the experience of the private equity industry in the United States, while particularly interesting given its size and relative maturity, may not reflect the experience in other countries. Thus, there is a real need to study the role of private equity in other countries with environments that differ in terms of corporate governance, financial depth, legal institutions and economic development. We think it would be extremely fruitful to study the role of private equity in other countries using the same type of rich firm-level and establishment-level data that we exploit in this study.⁴

The paper proceeds as follows. In Section 2, we review previous literature that considers the impact of private equity transactions on employment patterns in target firms. We then describe the construction of the data in Section 3. Section 4 describes our empirical methodology. We present the analyses in Section 5. The final section offers concluding remarks and discusses directions for future work.

2. Previous Literature

Economists have a longstanding interest in how ownership changes affect productivity and employment (e.g., Lichtenberg and Siegel (1987), Long and Ravenscraft (1993), McGuckin and Nguyen (2001)). However, only a modest number of empirical

⁴But see the works in the United Kingdom discussed in the next section, such as Amess and Wright (2007) and Harris, Siegel, and Wright (2005).

works explicitly focus on the impact of private equity on employment.⁵ Most previous studies of the issue consider small samples of transactions dictated by data availability.

Kaplan (1989) focuses on 76 public-to-private leveraged buyouts (LBOs) during the 1980s. He finds that the median firm lost 12% of its 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. Once he eliminates target firms with asset sales or purchases that exceed 10% of total value, the adjusted employment decline (for the 24 remaining firms) is -6.2%. Muscarella and Vetsuypens (1990) focus on 72 firms that complete an initial public offering (IPO) after an LBO between 1983 and 1987. In the 26 firms they can track, employment declines by an average of 0.6% between the LBO and the IPO. This outcome represents less job creation than 92% of the publicly traded firms in COMPUSTAT.

Lichtenberg and Siegel (1990), in the analysis closest in spirit to this one, use Census Bureau data to examine changes in employment at manufacturing plants of 131 firms undergoing buyouts between 1981 and 1986. They show that, on an industry-adjusted basis, employment declines after the buyouts. The rate of decline, however, is less dramatic than that beforehand (an annual rate of -1.2% versus -1.9% beforehand). The decline is more dramatic among non-production workers than blue-collar workers. Wright, Thomson and Robbie (1992) and Ames and Wright (2007) similarly find that buyouts in the United Kingdom lead to modest employment declines. These studies

⁵Economists have also written some more general discussions of these issues, largely based on case examples, such as Jensen (1989) and Shleifer and Summers (1998).

follow overall employment at a set of firms, and contrast it with aggregate employment at matching firms.⁶

These studies share certain features. First, they focus on the aggregate employment of private equity-backed firms. Thus, the sale of a division or other business unit is typically counted as an employment loss even if that business unit continues to have the same number of employees under the new owner. 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. While a number of the works discussed above attempt to address this issue by eliminating buyouts involving substantial asset sales, it is unclear how this type of sample restriction affects the results given the extent of “asset shuffling” by both private equity-backed and other firms.

Second, previous U.S. studies consider a relatively modest number of deals in the 1980s. The private equity industry is much larger today than in the 1980s. Using inflation-adjusted dollars, fundraising by U.S. private equity groups is 36 times greater in 1998 than in 1985. It is more than one hundred times greater in 2006 than in 1985.⁷ The tremendous growth in private equity activity allows us to examine a much larger sample, and it suggests that earlier relationships may not hold because of changes in the private equity industry (e.g., the increased competition for transactions and the greater operational orientation of many groups).

⁶These studies of British transactions also include management-led deals (which they term management buyouts). Some of these transactions may not have a financial sponsor playing a key role governing the firm, and thus may be quite different from traditional private equity transactions. The results described above apply primarily to the standard private equity transactions in the U.K. (which they term management buy-ins).

⁷<http://www.venturexpert.com> (accessed November 3, 2007).

Third, virtually all previous studies are subject to some form of selection or survival bias – especially those studies that focus on the firm rather than the establishment as the unit of observation. Even those previous studies that focus on establishments have typically been restricted to the manufacturing sector and even then with limitations on the ability to track establishment or firm closings.

Fourth, it is also desirable to look beyond the public-to-private transactions that dominated the earlier samples. Divisional buyouts, secondary buyouts, and investments in private firms may be fundamentally different in nature. Finally, it would be helpful to examine job creation and destruction separately. The recent literature on the dynamics of firms has highlighted the high pace of creative destruction in the U.S. economy. Gross job creation and destruction dwarf net changes. Moreover, the associated reallocations of workers across firms and sectors have been shown to enhance productivity (see, e.g., Davis and Haltiwanger (1999)). An open and important question is what role private equity plays in the process of creative destruction. The LBD data we use are well suited to investigate creative destruction in private equity targets relative to otherwise similar establishments and firms.

3. The Sample

The construction of the data set required the identification of as comprehensive as database of private equity transactions as possible, and the matching of these firms to the records of the LBD. This section describes the process.

A. Identifying Private Equity Transactions

To identify private equity transactions, we began with the Capital IQ database. Capital IQ has specialized in tracking private equity deals on a world-wide basis since 1999. Through extensive research, they have attempted to “back fill” earlier transactions prior to 1999.⁸

We download all recorded transactions that closed between January 1980 and December 2005. We then impose two sample restrictions. First, we restrict attention to transactions that entail some use of leverage. Many of the Capital IQ transactions that do not entail the use of leverage are venture capital transactions rather than private equity investments involving mature or later-stage firms. To keep the focus on private equity, we delete transactions that are not classified by Capital IQ as “going private,” “leveraged buyout,” “management buyout,” “platform” or a similar term. A drawback of this approach is that it excludes some private-equity backed “growth buyouts” and “expansion capital” transactions that involve the purchase of a minority stake in a firm with little or no leverage. While these transactions do not fit the classic profile of leveraged buyouts, they share other key characteristics of private equity transactions.

Second, the Capital IQ database includes a number of transactions that did not involve a financial sponsor (i.e., a private equity firm). We eliminate these deals as well. While transactions in which a management team takes a firm private using its own resources are interesting, they are not the focus of this study. After restricting the sample

⁸Most data services tracking private equity transactions were not established until the late 1990s. The most geographically comprehensive exception, SDC VentureXpert, was primarily focused on capturing venture capital transactions until the mid-1990s.

in these two ways, the resulting database contains about eleven thousand transactions worldwide.

We supplement the Capital IQ data with data from Dealogic. In many cases, Dealogic has much more comprehensive data on the features of the transactions, such as the multiple of earnings paid and the capital structure. It also frequently records information on alternative names associated with the firms, add-on acquisitions, and exits. We also use a wide variety of databases, including those from Capital IQ and SDC and compilation of news stories, to identify the characteristics of the transaction and the nature of the exit from the investment.

B. Matching to LBD Data

The LBD is constructed from the Census Bureau's Business Register of U.S. businesses with paid employees and enhanced with survey data collections. The LBD covers all sectors of the economy and all geographic areas and currently runs from 1976 to 2005. In recent years, it contains over 6 million establishment records and almost five million firm records per year. Basic data items include employment, payroll, four-digit Standard Industrial Classification (SIC) (and more recently 6-digit North American Industrial Classification (NAICS)), employer identification numbers, business name, and information about location.⁹ Identifiers in the LBD files enable us to compute growth rate measures for establishments and firms, to track entry and exit of establishments and firms, and to identify changes in firm ownership. Firms in the LBD are defined based on

⁹ Sales data are available in the LBD from 1994. Sales data from the Economic Censuses are available every five years for earlier years. More recent years in the LBD record industry using the newer NAICS scheme.

operational control, and all establishments that are majority owned by the parent firm are included as part of the parent's activity measures.

To merge data on private equity transactions with the LBD, we match the names and addresses of the private equity portfolio firms (i.e., the targets) to name and address records in the LBD.¹⁰ We use a three-year window of LBD data centered on the transaction year identified in the private equity transactions data to match to the Capital IQ/Dealogic private equity sample. A three-year window is used to cope with issues arising from difference in the timing of transactions in the two datasets.

Once we identify target firms in the LBD, we use the firm-establishment links in the LBD to identify all of the establishments owned by target firms at the time of the private equity transaction. We then follow these establishments before and after the transaction. Given the interest in examining dynamics pre- and post-private equity transaction, we need to define the private equity transaction year carefully relative to the measurement of employment in the LBD. In the LBD, employment is measured as the total employment at the establishment for the payroll period that includes the week of the 12th of March. Accordingly, for dating the private equity transaction year, we use the month and year information from the private equity transaction data and relate this to whether the private equity transaction occurred before or after March. For all private equity deals with a closing date after March 1st in any given calendar year, we date the year zero of the transaction so that it matches up to the LBD in the subsequent calendar year.

¹⁰ For some of the non-matched cases, we have been successful in matching the name of the seller in the Capital IQ to the corresponding LBD firm. We plan to use such seller matches to fill out our matches of target firms, but the use of these matches requires us to determine which components of the seller firm are involved in the private equity transaction.

Of the approximately eleven thousand firms in our private equity sample, a little more than half are companies not headquartered in the United States.¹¹ After dropping foreign firms, we are left with a little more than 5,000 U.S. target firms acquired in private equity transactions between 1980 and 2005. We currently match about 86% of these targets to the LBD, which yields an analysis sample of about 4,500 firms. The matched target firms operated about 300,000 U.S. establishments as of the private equity transaction year. On a value-weighted basis, we currently match about 93% of target firms to the LBD.

Figure 1 shows the number of U.S. private equity targets by year and the number that we currently match to the LBD. It is apparent from Figure 1 that the number of transactions grew rapidly in the late 1990s. Figure 2 shows the dollar value of private equity targets and matched targets by year. The total market value of target firms is very large in the later years: for example, in 2005 the total market value is about 140 billion dollars. Figure 3 shows that in 2005, for example, target firms account for about 1.9 percent of total non-farm business employment.

4. Methodology

This section describes three key methodological choices in the empirical analysis that follows. The first relates to the unit of analysis. In sections 5.A and 5.B, we focus on establishments owned by the target firm immediately after the private equity transaction. This approach restricts attention to the employment outcomes of workers at target establishments at the time of the private equity transaction. By following these units over

¹¹ Some foreign firms that are targets in private equity transactions are likely to have U.S. establishments. We will explore this issue and seek to capture U.S. establishments of foreign-owned private equity targets in a future draft.

time, we are not necessarily examining entities that remain under the control of private equity investors. For example, the target firm may be taken public at a later date or some of its establishments may be sold. We take a different approach in Section 5.C and look at firm-level changes. The firm-level approach allows us to capture greenfield job creation, as well as asset sales and acquisitions after the private equity transaction.

The second key choice relates to the use of controls. The use of suitable controls is important for at least two reasons:

- The distribution of private equity transactions across industries and by firm and establishment characteristics is not random. For example, practitioner accounts often suggest that transactions are concentrated in industries undergoing significant restructuring, whether due to regulatory action, foreign competition, or technological change. Figures 4a and 4b show the distribution of private equity transactions by broad industry sector for the 1980-2001 and 2002-2005 periods. Even at this high level of industry aggregation, it is apparent that target firms are disproportionately concentrated in manufacturing and financial services.
- By construction, target establishments have positive employment in the year of the private equity transaction. To the extent that newer establishments continually replace older ones, any randomly selected set of establishments is expected to decline in size going forward. Hence, the interesting issue is not whether target establishments lose employment after transaction, but what happens to their employment compared to other establishments that also have positive

employment in the year of the private equity transaction.¹² Our use of controls deals with this issue in a natural way.

The choice of the specific benchmark in constructing control groups also presents some issues. While the huge number of firms and establishments in the LBD might seem to allow infinite specificity of controls, as one chooses more dimensions along which to control simultaneously, the degrees of freedom diminish rapidly. Our basic approach is to define a set of control establishments for each target establishment based on observable establishment characteristics in the private equity transaction year. Once we identify the control establishments, we then follow them before and after the transaction year in the same way that we follow target establishments. This approach enables us to compare employment paths for targets to the employment paths for controls with the same observable characteristics in the transaction year. There are close to 300,000 target establishments in our analysis sample and more than 1.4 million control establishments.

In constructing control groups we use 72 industry categories, 3 establishment age classes, 3 establishment size classes based on relative size within the industry and age class, and an indicator for whether the establishment is part of a multi-establishment firm or a single-establishment firm.¹³ Fully interacting these factors yields about 1300 control cells per year. After pooling across years, there are about 30,000 potential control classes in our analysis. In practice, target establishments populate about 7500 of these classes.

We now provide some additional remarks about the controls and their motivation:

¹² The same issue arises in the firm-level analysis, but it is much more pronounced in the establishment-level analysis.

¹³ To construct our relative size measure, we first group establishments by the 72 industries and 3 age classes in each calendar year. Next, we rank establishments by number of employees within each industry-age-year cell. Finally, we define cutoffs for small, medium and large establishments so that each size class category accounts for about one-third of employment in the industry-age-year cell.

- Industry: By matching targets to controls in the same industry, we alleviate concerns that the non-random industry distribution of targets (Figures 4a and 4b) drives our results. We match targets to controls at the 2-digit SIC level for the 1980-2001 period and at the 4-digit NAICS level (roughly equivalent to 2-digit SIC) for the 2002-2005 period.
- Establishment Age: Figure 5 shows that target establishments are older than other establishments on an employment-weighted basis. Previous research on business dynamics emphasizes that the mean and variability of employment growth rates vary systematically with establishment and firm age (e.g., Davis, et. al. (2006, 2007)). Recent findings highlight especially large differences between very young establishments (firms) and more mature establishments (firms). To alleviate concerns that the non-random age distribution of targets drives our results, we use three age classes for establishments: 0-4, 5-9, and 10 or more years since first year of positive payroll for the establishment. Given the large differences in the mean and variability of employment growth by establishment age, net employment and volatility of growth rates across establishments, controlling for such age differences is likely to be very important.
- Relative Size: The recent literature also finds that average net growth as well as the volatility of net growth varies systematically by business size. However, the size distribution of establishments also varies dramatically by industry, with manufacturing establishments typically much larger than say retail establishments. As such, we control for the relative size of establishments in each industry. We classify each establishment into a small, medium, or large size class

based on its relative size in the establishment's industry-age-year cell. We choose the size thresholds so that each relative size class contains one third of employment in the industry-age-year cell. The right panel of Figure 5 shows that the targeted establishments are disproportionately in the middle and larger relative size classes, compared to the LBD universe of establishments on an employment-weighted basis

- **Single-Unit versus Multi-Unit:** Another factor that has been shown to be important for firm and establishment dynamics is whether the establishment is part of a single-unit firm or part of a firm with multiple establishments. Examples of multi-unit firms include Wal-Mart with many retail and wholesale establishments and the Chrysler with many automobile assembly plants and other facilities.

A third choice relates to the time frame of the analysis. The establishment-level analyses focuses on the change in employment in the five years before and after the transaction. This corresponds to typical holding periods by private equity groups (Stromberg (2007)), and should give a reasonably comprehensive sense of the impacts of the transactions.

For the firm-level analysis, we must confront the fact that firms are constantly being reorganized through mergers, acquisitions, and divestitures, as well as whole-firm changes in ownership. The exit of a firm often then does not imply that all the establishments in the firm have ceased operations and likewise the entry of a firm often does not imply that greenfield entry. We deal with this limitation of the firm-level analysis in a number of ways. While our firm-level analysis is based on firms that we can

accurately track over time, we focus on a relatively short horizon after buyout transactions (two years) so that the tracking of firms is more reliable. In addition, we use our establishment-level data integrated with the firm to quantify the impact of selection bias in our firm-level analysis.

Finally, in sections 5.A and 5.B, we compare employment dynamics at the establishments of target firms to the employment dynamics of the control establishments. It is useful to define the measure of employment and growth that we use in this analysis. Let E_{it} be employment in year t for establishment i . Recall this is a point-in-time measure reflecting the number of workers on the payroll for the payroll period that includes March 12th. We measure establishment-level employment growth as follows:

$$g_{it} = (E_{it} - E_{it-1}) / X_{it},$$

where

$$X_{it} = .5 * (E_{it} + E_{it-1}).$$

This growth rate measure has become standard in analysis of establishment and firm dynamics, because it shares some useful properties of log differences but also accommodates entry and exit. (See Davis et al. (2006), and Tornquist, Vartia, and Vartia (1985)). Aggregate employment growth at any level of aggregation is given by the appropriate employment weighted average of establishment-level growth:

$$g_t = \sum_i (X_{it} / X_t) g_{it} \text{ where } X_t = \sum_i X_{it}$$

It is instructive to decompose net growth into those establishments that are increasing employment (including the contribution of entry) and those establishments decreasing employment (including the contribution of exit). Denoting the former as

(gross) job creation and the latter as job destruction, these two gross flow measures are calculated as:

$$JC_t = \sum_i (X_{it} / X_t) \max\{g_{it}, 0\}$$

$$JD_t = \sum_i (X_{it} / X_t) \max\{-g_{it}, 0\}$$

In addition, computing the respective contribution of entry to job creation and exit to job destruction is useful. These measures are given by:

$$JC_Entry_t = \sum_i (X_{it} / X_t) I\{g_{it} = 2\},$$

where I is an indicator variable equal to one if expression in brackets hold, zero otherwise, and $g_{it}=2$ denotes an entrant.

$$JD_Exit_t = \sum_i (X_{it} / X_t) I\{g_{it} = -2\},$$

where $g_{it}=-2$ denotes an exit.

Given these definitions, the following simple relationships hold:

$$g_t = JC_t - JD_t, \quad JC_t = JC_Cont_t + JC_Entry_t \quad \text{and} \quad JD_t = JD_Cont_t + JD_Exit_t,$$

where JC_Cont and JD_Cont are job creation and job destruction for continuing establishments respectively.

The firm-level analysis uses the same basic measures but with the caveat that firm-level entry and exit must not be interpreted in the same manner as establishment-level entry and exit. As discussed above, establishment-level entry is the opening up of new (greenfield) establishment at a specific location and establishment-level exit indicates that the activity at the physical location has ceased operations. In contrast, firm-level entry may reflect a new organization or ownership of previously operating entities and firm-level exit may likewise reflect some change in organization or ownership.

5. Analysis

A. Basic Establishment-Level Analyses

We conduct an “event study,” exploring the impact of the private equity transaction during as well as before and after the transaction. As noted above, we focus on the window of time from five years before to five years after the transaction. We compare and contrast the employment dynamics for the target (private equity-backed) establishments to the control establishments. For any given target establishment, the control establishments are all the establishments that have positive activity in the transaction year of the target that are in the same industry, age, relative size, and multi-unit status cell. Since we look at the impact five years prior to and five years subsequent to the transaction for this initial analysis, we focus on transactions that occurred in the 1980-2000 period.¹⁴

The first exercise we explore is the differences in net growth rates of employment for the establishments of the targets vs. the controls. Figure 6a shows the net growth rate differences in the transaction year and for the five years prior and subsequent to the transaction. To construct Figure 6a, we pool all of the private equity transactions in our matched sample from 1980 to 2000 and calculate differences in growth rates relative to controls on an employment-weighted basis. Prior to and in the year of the private equity transaction, there is a systematic pattern in terms of less job growth (or more job losses) for the targets than the controls: the differences in net growth are between 1% and 3% per year. This is consistent with depictions of private equity groups investing in troubled

¹⁴ Our firm-level analysis in later sections focuses on a two year horizon after the transaction and thus considers all transactions up through those in 2003. For the firm-level analysis, we have found that the results are quite similar whether we consider transactions only up through 2000 or 2003 suggesting that the establishment-level analysis is likely not very sensitive to this restriction. We plan to explore this issue further in future work.

companies. After a similarly lower rate for net job growth for targets in the first year after the transaction, the difference in the job growth rates widens in the second and third year after the transaction: the rate is about 4% below that of the controls in each year. In the fourth and fifth years after the transaction, the pattern reverses, with the targets having slightly greater employment growth.¹⁵

To help understand these patterns, we explore different dimensions of the differences between establishments of targets and controls. In Figure 6b, we show the net growth rate patterns separately for targets and controls. The basic patterns of net growth for targets and controls are quite similar. Prior to the transaction, both targets and controls exhibit large positive growth rates. Subsequent to the transaction, both targets and controls exhibit large negative growth rates. These patterns highlight the critical need to include controls in evaluating the employment dynamics of establishments of targets. If one looked at employment dynamics of establishments of targets in isolation (focusing only on the targets in Figure 6b), one might draw the very misleading conclusion that targets shrink consistently and substantially after the private equity transaction.¹⁶

Figure 6c compares the actual employment level of private equity transactions pre- and post-transaction with the implied employment of these targets had they grown at

¹⁵ We do not report standard errors in this draft but will report standard errors for key exercises in subsequent drafts. For example, the reported net differences in Figure 6a can be interpreted as being consistent with pooling the target and control data over all years and estimating an employment-weighted regression of net employment growth on fully saturated controls and private equity transaction dummies for targeted establishments.

¹⁶ It is important to note that the pattern of positive net employment growth prior to the transaction year and negative net growth after seen in figure 6b and the inverted v-shape in figure 6c reflect a generic feature of the data. Namely, if one randomly observes establishments at some fixed point in their lifecycle, they will, on average, exhibit growth up to the point and will, on average, exhibit decline from that point on.

the same rate as the controls.¹⁷ This exercise permits evaluating the cumulative impact of the differences in net growth rates between targets and controls. To conduct this counterfactual exercise, the employment level of the controls is normalized to be exactly equal to that of the targets in the transaction year. The pattern for the controls shows the counterfactual level of employment that would have emerged for targets if the targets had exhibited the same pre- and post-transaction employment growth rates as the controls. Figure 6c shows that, five years after the transaction, the targets have a level of employment that is 10.3 percent lower than it would be if targets had exhibited the same growth rates as controls.¹⁸

In interpreting the results from Figures 6a through 6c, it is important to emphasize that the observed net changes may stem from several margins of adjustment. The recent literature on firm and establishment dynamics has emphasized the large gross flows relative to net changes that underlie employment dynamics (see, e.g., Davis, Haltiwanger and Schuh (1996)). Figures 7a and 7b show the underlying gross job creation and destruction rates for targets and controls. It is apparent from Figures 7a and 7b that the rates of gross changes for both targets and controls are large relative to the net changes observed in Figure 6. Both targets and controls have higher job creation rates prior to the transaction than after and have higher destruction rates subsequently than beforehand. As discussed above, this pattern reflects the nature of the sample construction process.

While the overall patterns are similar, there are some interesting differences in the patterns of the gross flows between targets and controls. Figures 8a and 8b show the

¹⁷ The sum of employment for targets across all years reported in Figure 6c is about 3.3 million workers. This represents the sum of employment in the transaction years for targeted establishments over the 1980-2000 period.

¹⁸ The 10.3 percentage point calculation derives from the difference in the level between private equity transactions and controls in year five (about 34,000 employees) divided by the initial base in year zero.

differences between creation and destruction rates, respectively, between targets and controls. Prior to the transaction, there is no systematic pattern of differences between the private equity-backed targets and the controls in terms of creation and destruction rates, except for the decline in job creation by the targets in the year before the private equity transaction. Subsequent to the transaction, the targets tend to have substantially higher destruction rates in the first three years after the transaction—though this rapidly drops off thereafter—and about the same creation rates as the controls.

One implication is that the net differences exhibited in Figure 6a after the transaction year are associated with the job destruction margin. An interesting suggested implication is that private equity transactions trigger a period of accelerated creative destruction relative to controls that is most evident in the first three years after the transaction.

Given its relevance to the net employment pattern the job destruction margin can be explored further in terms of the patterns of establishment exit. Figure 9a shows the employment-weighted exit rate (or put another way, the job destruction from exit) for the targets and the controls. Both sets of establishments exhibit substantial exit rates after the transaction, reflecting that establishment exit is a common feature of the dynamics of U.S. businesses. The targets exhibit higher exit rates in the first three years after the transaction relative to controls. The difference in the exit rates is reported in Figure 9b. For example, in the second year after the transaction, private equity transactions have a two percentage point higher exit rate than controls. In the fourth and fifth years, the exit rate of the targets is actually lower than that of the controls

B. Changes in Sub-Samples of Transactions

The results presented in Section 5.A reflect the results from pooling across all private equity transactions over the 1980-2000 period. The controls account for differences in the net growth patterns along many dimensions, but we have not examined whether the patterns differ by observable characteristics of the private equity transactions. In this section we consider a number of simple classifications.

To begin, we consider differences in the net growth patterns between private equity transactions and controls by time periods, industry, establishment age, and establishment size. Figure 10 shows the equivalent of Figure 6a for different sub-periods of transactions. The pattern in the overall data on employment is more pronounced for the transactions that occurred from 1995 onwards. Since the number of transactions accelerated rapidly over the post-1995 period, it is not surprising that much of the overall employment effects depicted in Figure 6c are during this time period.

Figure 11 shows how the patterns vary by broad sector. We focus on three of the broad sectors where most of the private equity transactions are concentrated. Within manufacturing, we find relatively little systematic difference in net growth patterns between private equity transactions and controls. We find that the level of employment for private equity transactions five years afterwards is about the same as if the targets had grown at the same rate as the controls. (More specifically, the targets are 2.4 percent lower at the end of five years). Manufacturing is a sector where a large fraction of private equity transactions are concentrated and in this sector at least, there are few differences between targets and controls.

We know, however, from Figure 6a that there are non-trivial differences between private equity transactions and controls in the pooled data. Figure 11 shows that for establishments in Retail Trade and Services, we see more pronounced but volatile differences between targets and controls. While the patterns are volatile and differ across these sectors, the cumulative reduction in employment for the private equity transactions compared to the controls is large in both sectors. In Retail Trade, the cumulative impact of the private equity transaction after five years yields a 9.6 percent lower employment level than would have occurred if the targets had the same growth rates as the controls. The cumulative five-year impact is 9.7 percent lower employment in Services for the targets compared to the controls.¹⁹

Figure 12 shows the variation in the differences between different types of private equity transactions. There are few concerted differences across the categories: each displays a similar pattern. One exception is the fact that the period of reduced employment growth is considerably larger and concentrated immediately after the transaction for the secondary buyouts,²⁰ which presumably have already undergone a restructuring under their previous owner.²¹ By contrast public to private buyouts

¹⁹ While not reported in Figure 11, we have also examined the patterns for the FIRE broad sector. We find even more volatile patterns for FIRE than for Retail Trade and Services and a very large net difference between targets and controls

²⁰ It is important to note the differences in scale for the figure depicting secondary buyouts – we chose to use a different scale given the very large net negative difference between targets and controls for the first year after the transaction.

²¹In unreported analyses, we examine relative establishment growth rates across age and size classes. There are some differences, but the post-transaction patterns are quite similar across age and size classes. One notable difference between private equity transactions and controls is the pre-transaction net growth differences for very young (between 0 and 4 years old) establishments: very young control establishments have substantially higher net growth compared to targets prior to the private equity transaction. This pattern likely reflects differences between targets and controls in the age distribution of the parent firms of very young establishments. That is, among very young establishments, targets are likely part of older firms that are ripe for restructuring.

experience reduced employment growth the first two years after the transaction relative to divisional and independent buyouts.

C. Firm-Level Changes including Greenfield Entry, Acquisitions and Divestitures

In our establishment-based analysis in the last two sections, we focused our attention on the ultimate outcomes for establishments and workers in target firms at the time of the deal. Alternatively, one could focus on similar outcomes for the entire target firm. While the analysis contained in the prior two sections is an appropriate way to trace the employment impacts of private equity transactions for establishments and workers at target firms at the time of the buyout, it ignores the opening of new establishments and other actions that private equity firms or other subsequent owners may take that impact employment at target firms post-buyout.

In this final section, we address this shortcoming of the establishment-based analysis. However, to do this we, by necessity, restrict our attention to the subset of target firms that we can observe for some period post buyout. With the LBD, we are able to readily follow establishments over time, even if they undergo ownership or other changes. Tracking firms over time is more problematic due to mergers and other events that lead to changes in the firm-level identifiers in the LBD. This was not an issue in our establishment analysis above, since we only needed to find the target firm in the year of the private equity transaction and then follow its establishments over time, regardless of any changes in their associated firm identifiers.

The disappearance of a firm ID in the LBD can be associated with a firm death, where all the firm's establishments are shut down, or some form of organizational change

such as a merger. It is possible to utilize the LBD's robust longitudinal establishment linkages to provide a rich description of these organizational changes. Such analyses are, at this point, very time consuming and resource intensive. Thus, we restrict our firm-level analyses to a subset of target firms and similar control firms that we can observe two years after the buyout.

In order to be able to identify target firms that we can observe two years after the buyout, we must restrict the set of transactions in this firm level analysis relative to the establishment-level analysis given the differences in the matching of the private equity transaction data at the establishment and firm level. For the establishment-level analysis, as discussed in section 3.B, the matching of the private equity transactions to the LBD is based on a 3-year window centered on the transaction year. This is feasible and reasonable because with the establishment-level analysis we use the information from the CapitalIQ data to date the transaction and given high quality establishment longitudinal identifiers only use the set of establishments that exist in that deal year. In contrast, for the firm level analysis we are forced to restrict attention to matched cases where the match to the LBD occurs in the transaction year. This matching restriction implies that even before restricting on two year survivors, our firm level analysis starts with about 65 percent of the matched transactions used in the establishment-level analysis. Using this subset, we impose the further restriction that we observe the target firm in the transaction year and two years later. This latter restriction implies that in the firm-level analysis we have about 55 percent of the matched transactions in the establishment-level analysis (approximately 1300 transactions). Note, however, that conditional on matching in the transaction year, this survivor restriction yields 97 percent of the employment from all the

firms that match in the transaction year. Thus, it is the matching restriction and not the survivor restriction that matters most for our firm level subset.

Given that we track the target firms for two years and find a comparable group of controls, we can compare growth in employment and the number of establishments at target firms to similar control firms. In addition, the LBD permits us to overcome many of the challenges that plague earlier work in that we can identify the components of the firm changes due to greenfield entry, the closing of the establishments owned by the firm, acquisitions of new establishments, and divestitures of establishments owned by the firm. However, its important to note that, since we restrict attention to firms that can be followed for at least two years post private equity transaction, the firm-level analysis focuses on a subset of targets and controls that are at least somewhat likely to be older, larger and more likely to be successful than the target and control establishments examined in the above two sections. Given the fact that the considerable majority of the target and control firms are older, larger ones, it is likely that the firms that are disappearing are doing so due to an acquisition or other restructuring, not because of the outright liquidation of the firm. Because we are only looking two years forward, we now include transactions through 2003.²²

In this section, we employ a slightly different approach from that above. Rather than simply looking at the differences across firms, we undertake regression analyses. We use as observations all firms in the LBD that are present in the transaction year and two years thereafter. The dependent variables—the key measures we are seeking to explain—are the employment growth rates from the transaction year to two years after as well as the associated growth in the number of establishments.

²² The patterns are similar, but the differences somewhat wider, when we only look through 2000.

We use as independent, or control variables measures that are similar to those we used to match the establishments and firms in the above analyses:

- SU/MU is an indicator as to whether the firm in the transaction year has more than one establishment.
- Firm age classes are again defined based on the age of the oldest establishment in the transaction year.
- Firm size classes are defined based upon total employment in the transaction year.
- Finally, we use in Table 1 a dummy variable denoting whether the observation was a private equity target (as opposed to a control firm), and, in Table 2, a set of controls for the various private equity transaction types.

All regressions are weighted by employment, so larger transactions are given more influence.

Again, it is important to note that these firm-level analyses include the effects of employment changes at existing establishments, the opening of new greenfield facilities, the closing of establishments (conditional on the survival of the firm) as well as acquisitions and divestitures of already-existing establishments. The analyses in sections 5.A and 5.B only capture the changes at existing establishments and the closing of establishments. While the firm analysis includes establishment exits, it does not include cases where the entire firm disappears. These differences apply to both targets and controls.

Our firm-level results are given in Tables 1 through 3. Table 1 presents the results of regressions measuring the difference in employment and establishment growth rates (computed in the same manner as in the establishment-level analyses above)

between targets and controls where we control for the affects listed above in a fully interacted model. The non-target controls consist of all LBD firms in the same year, firm age category, size category, and status as a single or multi-unit firm as the targets. This allows for a more parsimonious and manageable analytic dataset.

The estimated coefficients in Table 1 imply that the target firms grow at a lower rate relative to controls over a two-year horizon: the targets have a 3.6% lower net employment growth rate than controls over this period. When we examine the number of establishments, targets have a slightly higher growth rate, with the difference a little under 1 percent.

In Table 2 we report the results of regression where we exploit rich detail in the type of buyout transactions. The results indicate significant variation in outcomes across transaction types. Note that all coefficients are relative to the omitted control group. The difference in net employment growth relative to controls is especially large in magnitude for public-to-private and secondary transactions, as Table 2 reveals. Interestingly, divisional transactions have a higher net growth rate than controls. The results are quite similar when we restrict to deals through 2000, although now buyouts gain another half a percent relative to controls in terms of net employment growth at the firm level.

While the results in Tables 1 and 2 are similar to the establishment-level results in that targets contract relative to controls, the establishment-level results imply a larger net difference relative to the firm-level results. Using Figure 6c, we see that, over a two-year horizon, existing establishments of targets contract at about a 6.7 percent larger rate than controls, while for the firm-level results the net difference is 3.6 percent. These results are not directly comparable given that the firm-level analysis is on a restricted sample of

firms that continue for two years. Nevertheless, we know that the contribution of greenfield entry, acquisitions, and divestitures that are missed in the establishment-level analysis potentially account for some of this difference. To explore the contribution of the latter effects, Table 3 shows two-year employment weighted greenfield entry rates, establishment exit rate, establishment acquisition rates and establishment divestiture rates for the sample of targets and controls.

The results in Table 3 are striking on a number of dimensions. We find that target firms exhibit a very high greenfield entry rate relative to controls. Target firms have a greenfield entry rate of 14.9 percent relative to 8.9 percent for controls. However, target firms also have a very high establishment exit rate relative to controls. Target firms have an establishment exit rate of 16.7 percent relative to 8.1 percent for controls. Thus, the net entry effect from establishments contributes to targets shrinking relative to controls.

We also find that targets have a higher pace of both acquisitions and divestitures. Target firms have an employment-weighted acquisition rate of 7.4 percent compared to 4.7 percent for controls and an employment-weighted divestiture rate of 5.7 percent compared to 2.9 percent for controls. The greater change in establishment ownership via acquisitions and divestitures for targets does not yield much of a net effect, but when combined with the entry and exit rate results, implies much greater overall rate of change at the target firms.

Some caution needs to be used in comparing the weighted mean rates in Table 3 with the results in Table 1. For Table 3, we compute the weighted mean rates amongst the targets and the controls for the indicated categories (e.g., greenfield entry). If one adds up the components of Table 3 (i.e., Entry-Exit+Acquisition-Divestiture+Continuing),

one obtains the weighted mean growth rates for targets and controls reported in the last row of the table. This implies a net difference between targets and controls based upon weighted net growth rates of -4.5 percent. The regression in Table 1 yields a net difference of -3.6 percent. While Tables 1 and 3 use exactly the same sample of targets and controls (so that the controls are the LBD firms in the industry, firm age, firm size, Single Unit/Multi-unit and year cells as the target firms), Table 1 is based upon a employment-weighted firm level regression with a rich set of fully interacted effects. There is a regression equivalent of Table 3 that would require computing and using as dependent variables firm-level greenfield entry rates, exit rates, acquisition and divestiture rates and include the full set of interacted control effects in the regression. We will explore such regression specifications in future drafts of the paper.

Overall, the results in Table 3 strongly show that target firms are undergoing much more restructuring than we observe at similar non-target control firms. These results suggest that the employment impact of private equity buyouts is much more complex than may be widely understood. While, on net, we find slower employment growth associated with private equity transactions, we also find substantial greenfield entry and acquisition of establishments by target firms post-buyout. This is indicative of substantial investments in and commitments to the continued operation and success of the target firms by private equity firms. It is also consistent with the view that private equity transactions are catalysts for a wave of creative destruction in target firms, accelerating both job destruction and divestitures, on the one hand, and job creation and acquisitions, on the other.

Before concluding this section, it is instructive to discuss the possible implications of the differences in the sample of private equity transactions comparing the firm-level and the establishment-level results. The challenge that we face in our firm-level analysis and shared by virtually all the existing firm-level studies of the impact of private equity on employment is that the analysis is based upon firms that can be accurately tracked longitudinally over time. As we have discussed, tracking establishments longitudinally, while a challenge as well, is a much less complex exercise. In contrast, firm reorganizations and ownership changes make measures of firm entry and exit difficult to interpret. This implies the firm level analysis is based on a subset of the transactions relative to the firm level analysis for both matching and survival reasons.

In terms of comparing the establishment-level to the firm-level results, the establishment-level results have the virtue that they are based on a larger sample of transactions and also are not subject to survivor bias. However, as noted, the firm-level results have the advantage relative to the establishment-level results that we can quantify the contribution of greenfield entry as well as acquisitions and divestitures for surviving firms.

While it is difficult without further analysis to quantify the implications of the sample restrictions for the firm level analysis, comparisons of some aspects of the establishment-level and firm-level results are insightful for possible directions of the bias and areas for further inquiry. In particular, the tabulations in Table 3 can be used to generate results for “existing” establishments in a manner analogous to the establishment-level results for the survivor firms. By combining the continuing establishment and exiting establishment effects, Table 3 implies that for targets existing establishments of

surviving firms had a two year net growth rate of -18.4 percent. For controls, Table 3 implies that existing establishments of surviving firms had a two year net growth rate of -8.2 percent. In comparing these results to the establishment-level results using all targets (and associated controls), Figure 6c yields that “existing” establishments from targets exhibited a two year net growth rate of -17.7 percent while controls exhibited a two year net growth rate of -10.9 percent.

On the basis of these comparisons, it is tempting to conclude that the sample selection bias is more of an issue for controls than targets since the difference between -18.4 and -17.7 is smaller than the difference between -8.2 and -10.9 percent. If true, this would suggest the adjustment for sample selection bias in our firm level results would push towards a smaller gap between targets and controls. Moreover, it is possible to construct a rationale for such a difference in sample selection bias. For the targets, the firms that cannot be matched in the transaction year or cannot be tracked subsequently are plausibly being reorganized as a consequence of the private equity transaction. Indeed, our findings of higher acquisition and divestiture rates for targets suggest more reorganization of targets relative to controls. For controls, the firms that cannot be tracked are also likely to be driven primarily by reorganizations, but also by whole firm exits (i.e., cases where the firms’ operations cease entirely). Drawing inferences about the implications for the survivor bias adjustments needed for targets and controls is too speculative, however, since sample selection bias likely impacts not only continuing and exiting establishment rates but also greenfield entry. It is also clear that computing greenfield entry rates for firms undergoing ownership changes and reorganizations is a very difficult task -- but one that we plan to pursue in future research. In addition, the

speculation above about reorganizations vs. firm exits for targets and controls requires further research.

These difficult conceptual and measurement issues associated with sample selection bias make it difficult to derive a “bottom line” number about the impact on employment from private equity transactions. We can say with confidence that the net impact on existing establishments is negative and substantial. We can also say with confidence that for a sample of surviving firms, we observe more greenfield entry, more acquisitions, divestitures, and establishment shut-downs, and a negative net impact on employment that is substantial but smaller than that from the establishment-level results that ignore greenfield entry. The computation of a “bottom line” overall net number, however, requires further research exploring the dynamics of the firms and establishments that are in the establishment-level sample but not in the firm-level sample.

6. Conclusions and Future Areas of Investigation

This paper examines the question of employment outcomes at the targets of private equity transactions at a far more granular level than earlier studies. We focus on three hundred thousand U.S. private equity-backed establishments in the period from 1980 to 2005. Among the most interesting results that emerge are:

1. The establishments of target firms that exist at the time of the transaction exhibit lower rates of net employment growth in the years before, of, and immediately after a private equity transaction, when compared to a group of similar control establishments.

2. In the second and third years after such transactions, these targets have considerably lower net job growth than control establishments.
3. By the fourth and fifth years, job growth of the target firms is slightly above that of the controls.
4. Target establishments seem to create roughly as many jobs as similar control establishments. The lower net job growth of about 10 percent over the five years after the transaction appears to be generated via higher gross job destruction as the new private equity-backed owners shed presumably unprofitable segments of the target firms.
5. These patterns are exclusively confined to Retail Trade, Services and Financial Services: there is little difference in the post-transaction growth of the target firms in Manufacturing.
6. When we examine greenfield entry, the target firms have a substantially higher job creation rate (as a share of employment) through the opening of new greenfield facilities in the two years after the transaction than the controls. However, target firms also exhibit a much higher cumulative job destruction rate from establishment exits relative to controls.
7. In like fashion, we find that target firms have both higher acquisition and divestiture rates (on an employment weighted basis) relative to controls. Combined with the results on entry and exit, target firms have a much higher overall rate of change in establishment owned than controls.

The LBD and related micro datasets contain a rich array of information beyond simply information on employment levels. These include information on the composition

and compensation of employees, labor and total factor productivity. We intend to explore these consequences of private equity transaction in subsequent papers using these data. These efforts are particularly relevant, given that the formulation of policy recommendations regarding private equity must consider not just changes in employment levels, but a wide variety of other considerations.

Also in future work, we plan to examine the employment and productivity outcomes for corporations that sell to private equity groups. Many divisional buyouts consist of divestitures of underperforming units that may be consuming management attention. Schoar (2002) documented that acquisitions may lead to managers to neglect core business, a pattern she called the “new toy” effect. It will be interesting to observe whether the same pattern exists in reverse for the sellers in divisional buyouts.

Finally, we highlight the need to focus on the experience outside the United States. While the U.S. has the oldest and largest private equity industry, the industry elsewhere is experiencing rapid growth (Stromberg (2007)) and in many cases, evolving in different ways. Understanding whether the dynamics of private equity and employment are similar or different in these markets is an important challenge.

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Figure 1: Matches of Private Equity Targets to LBD

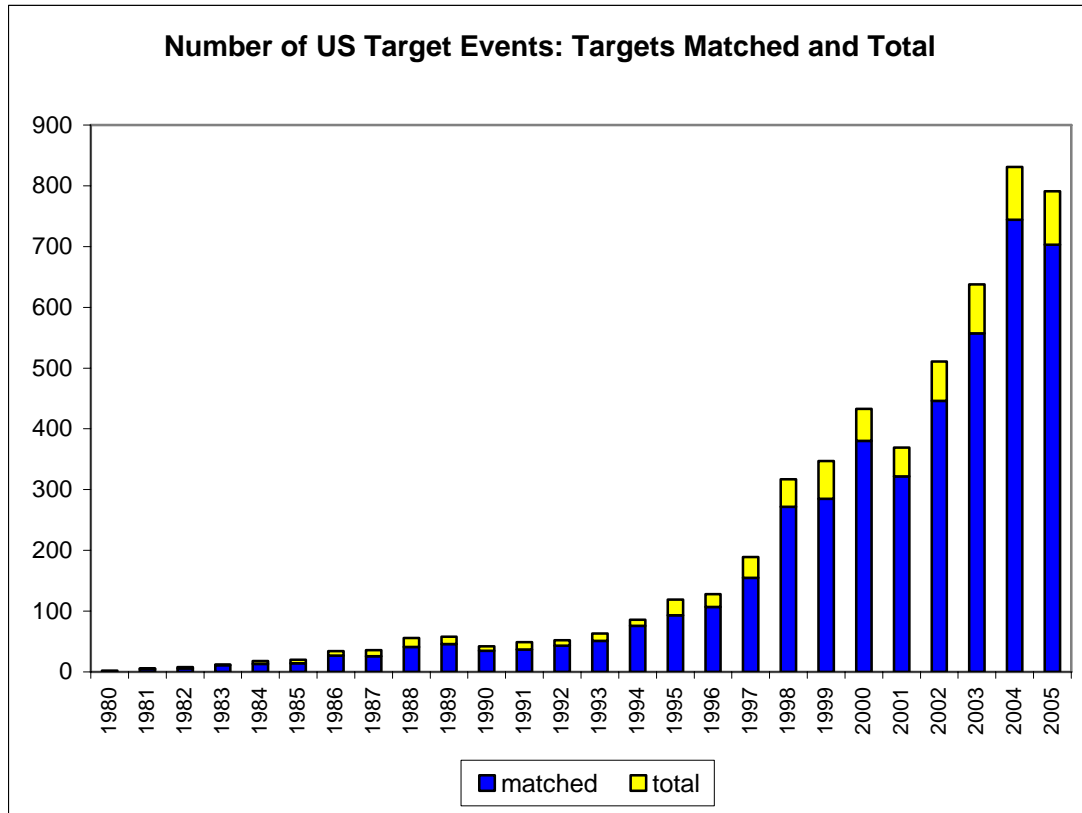


Figure 2: Value of Private Equity Targets (Total and Matches)

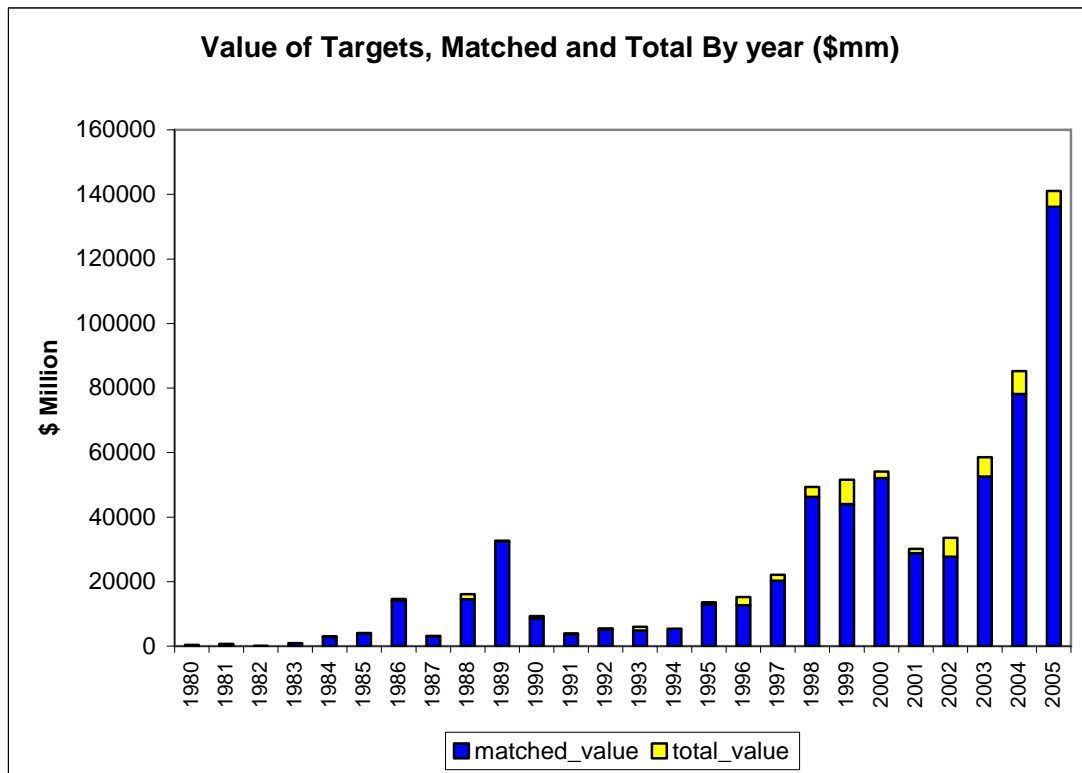


Figure 3: Employment of Matched Targets (Level and Percent of LBD Total)

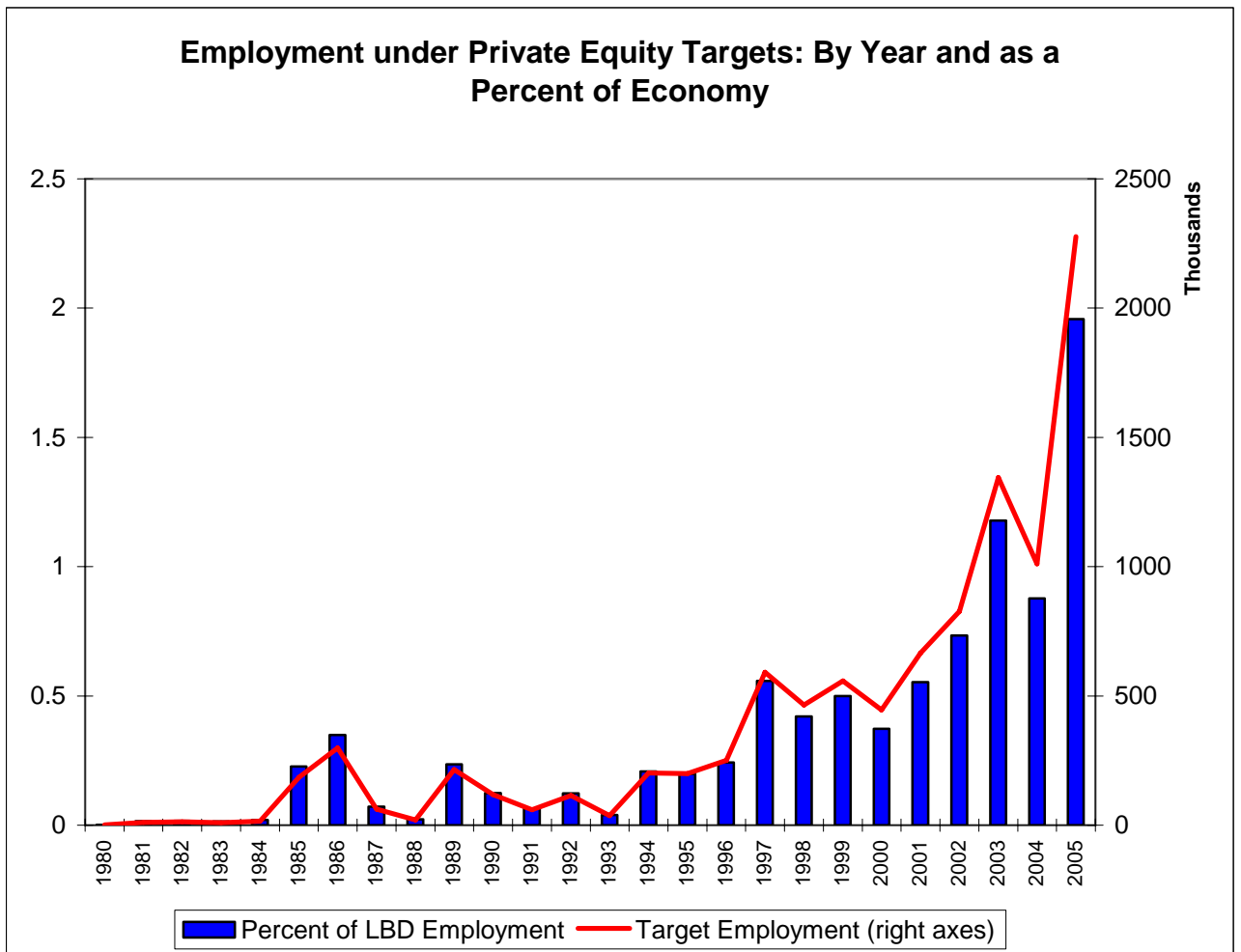


Figure 4

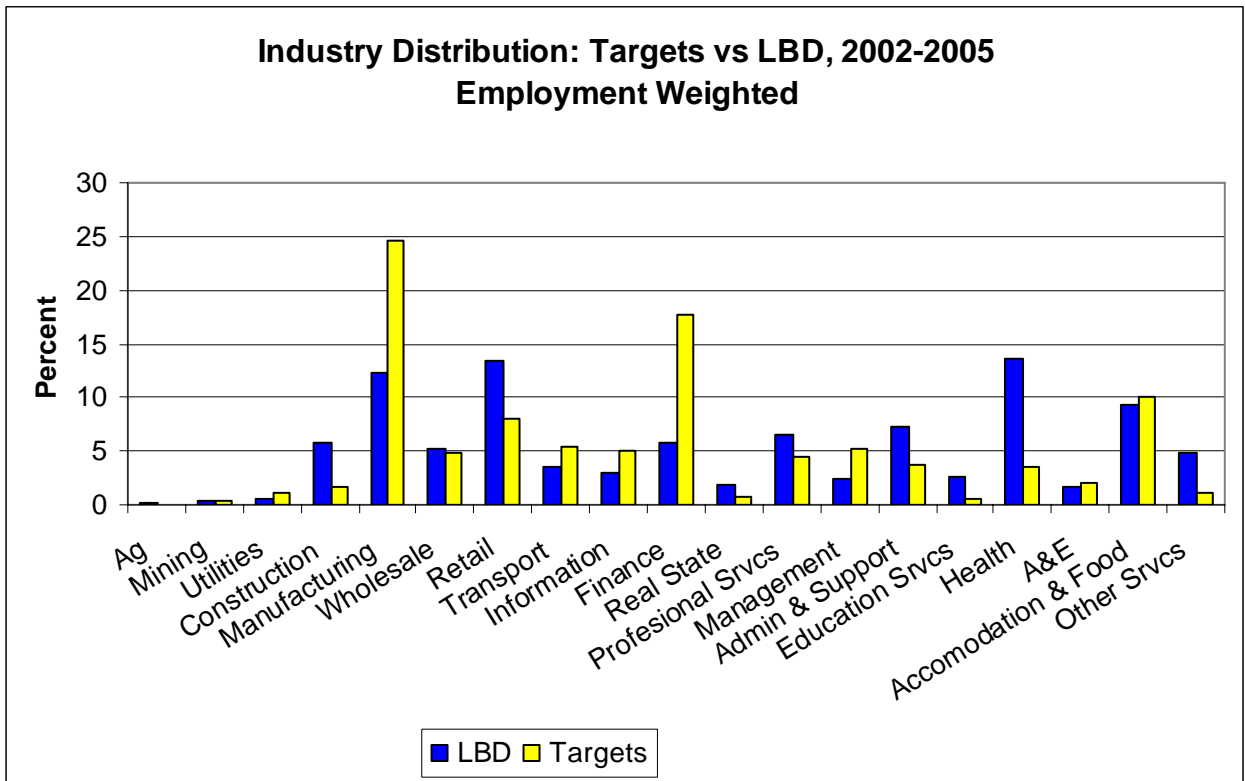
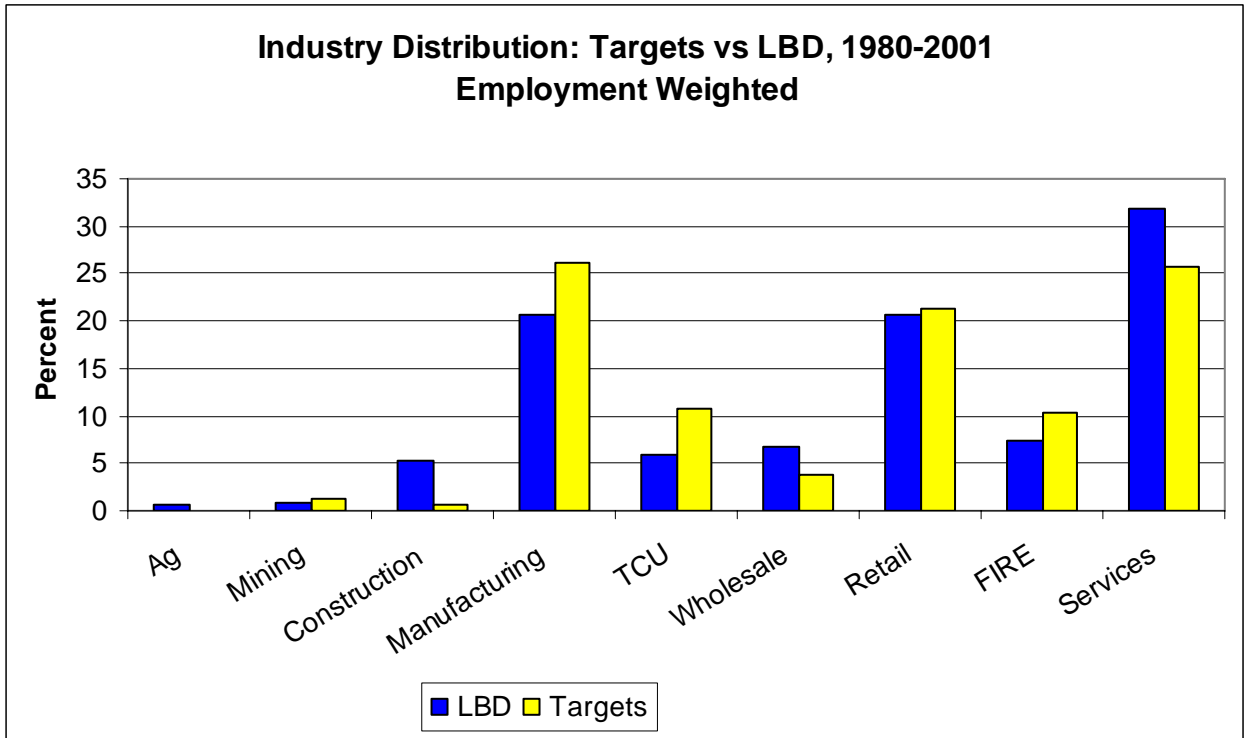


Figure 5

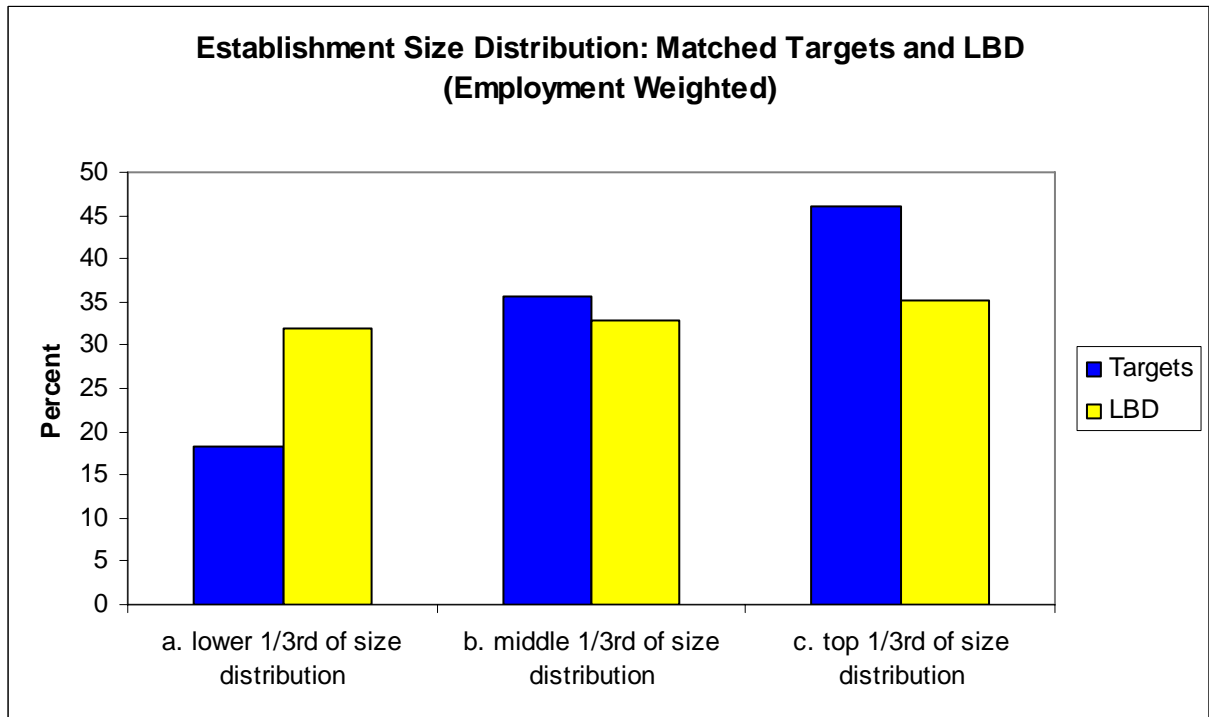
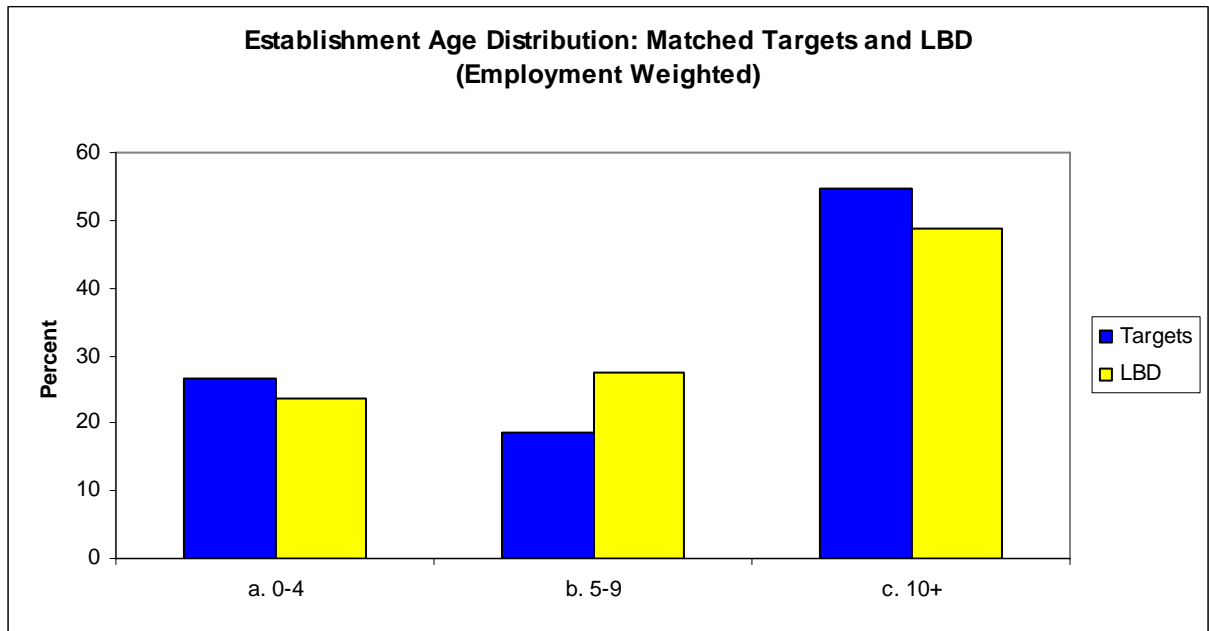


Figure 6a

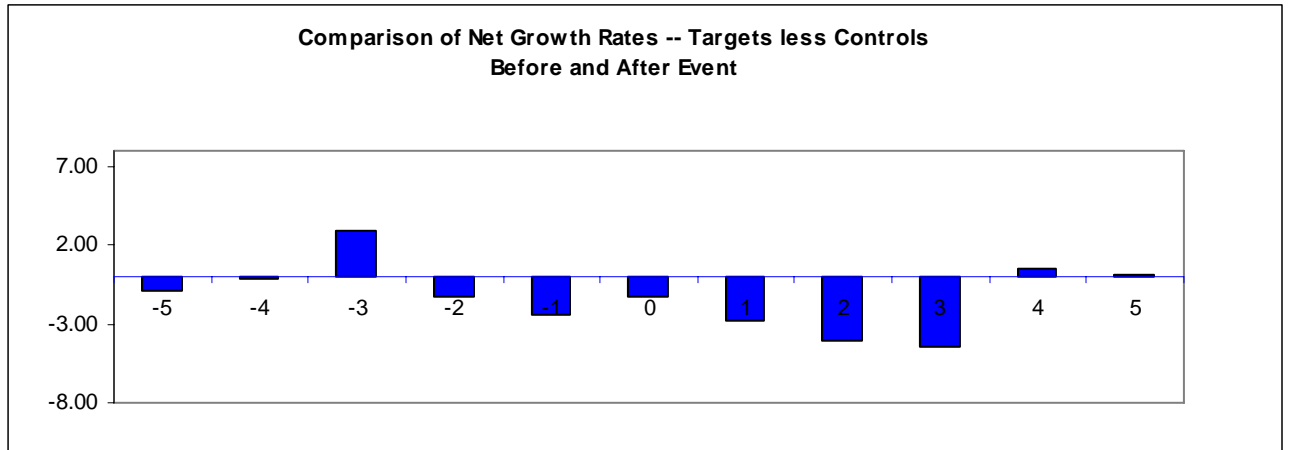


Figure 6b

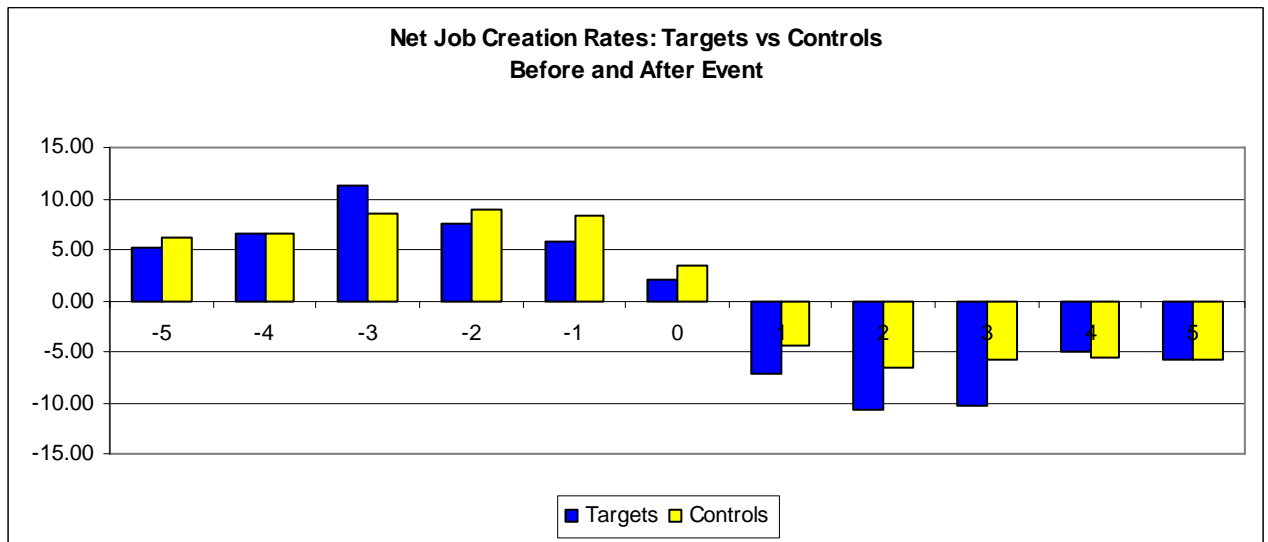


Figure 6c

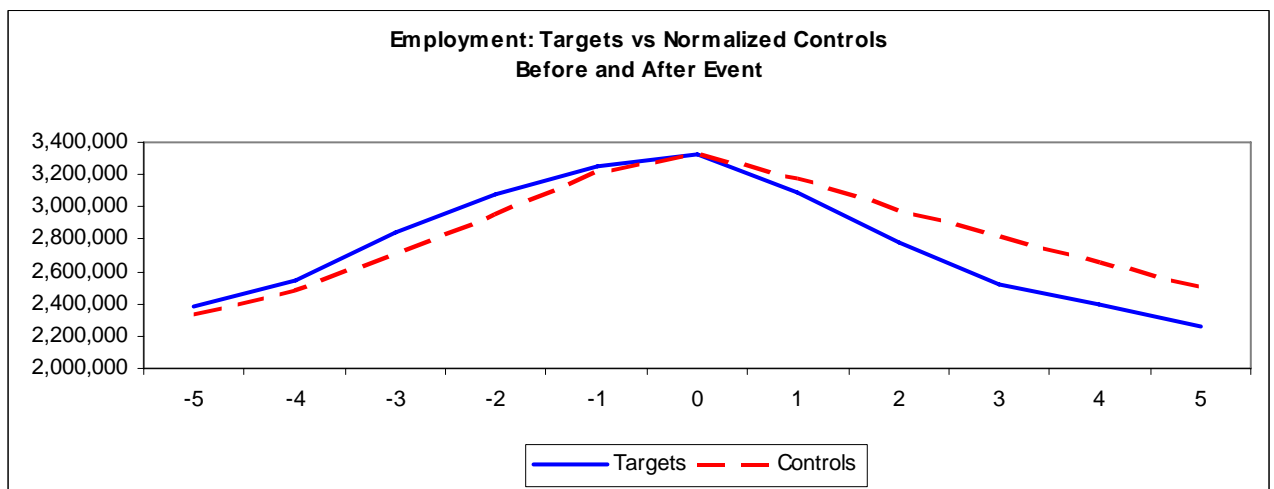


Figure 7a

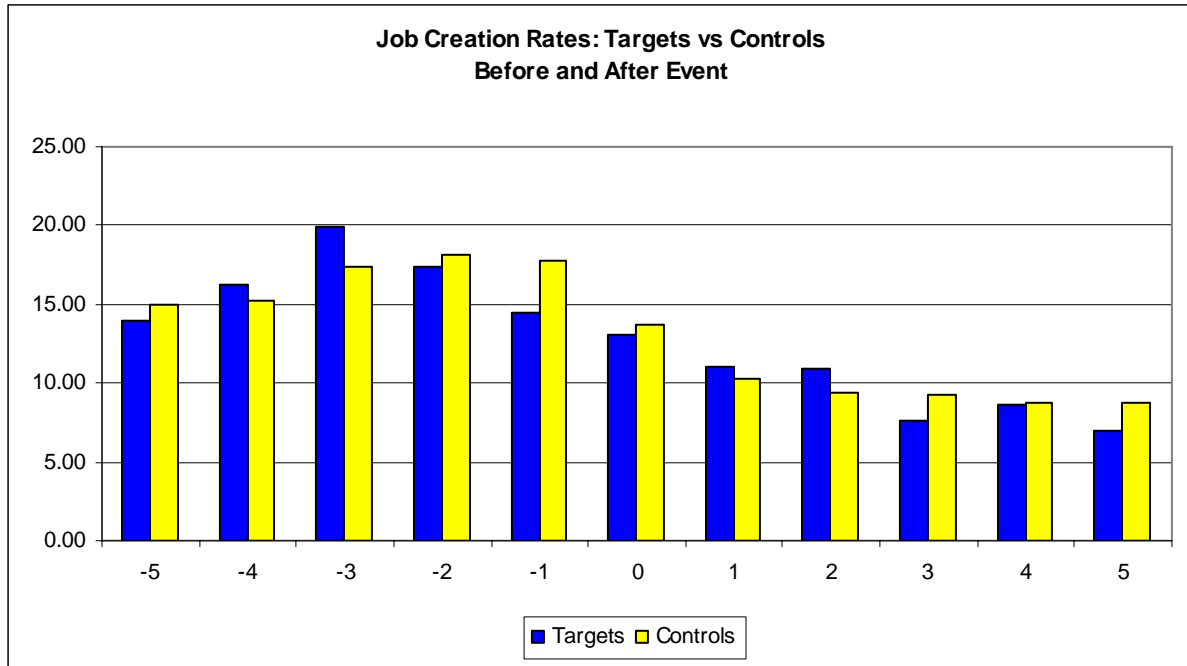


Figure 7b

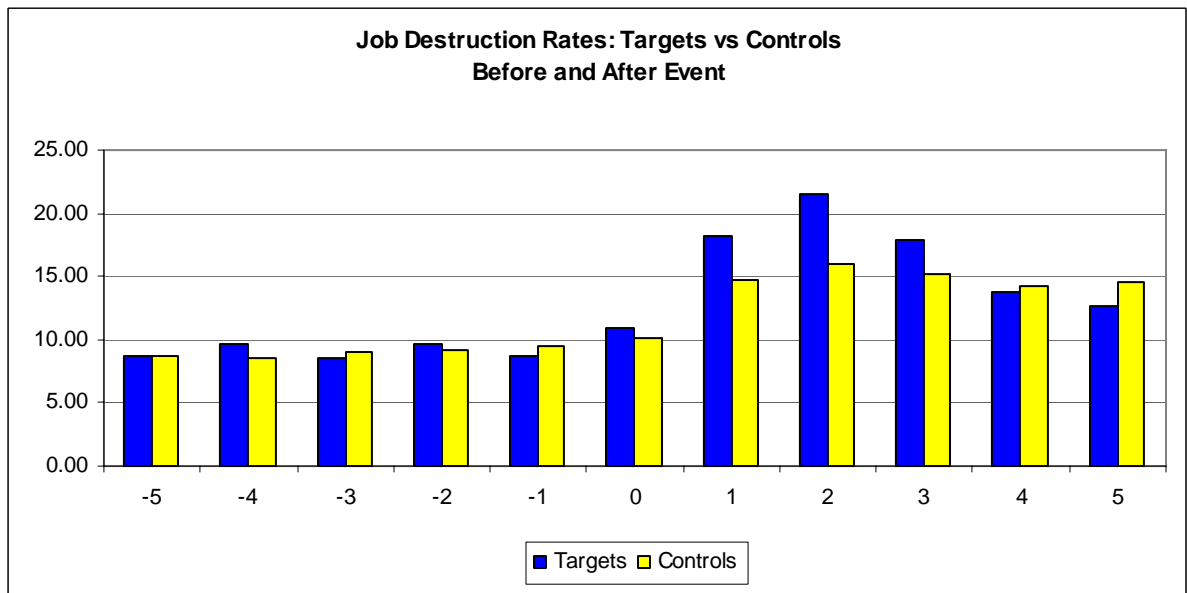


Figure 8a

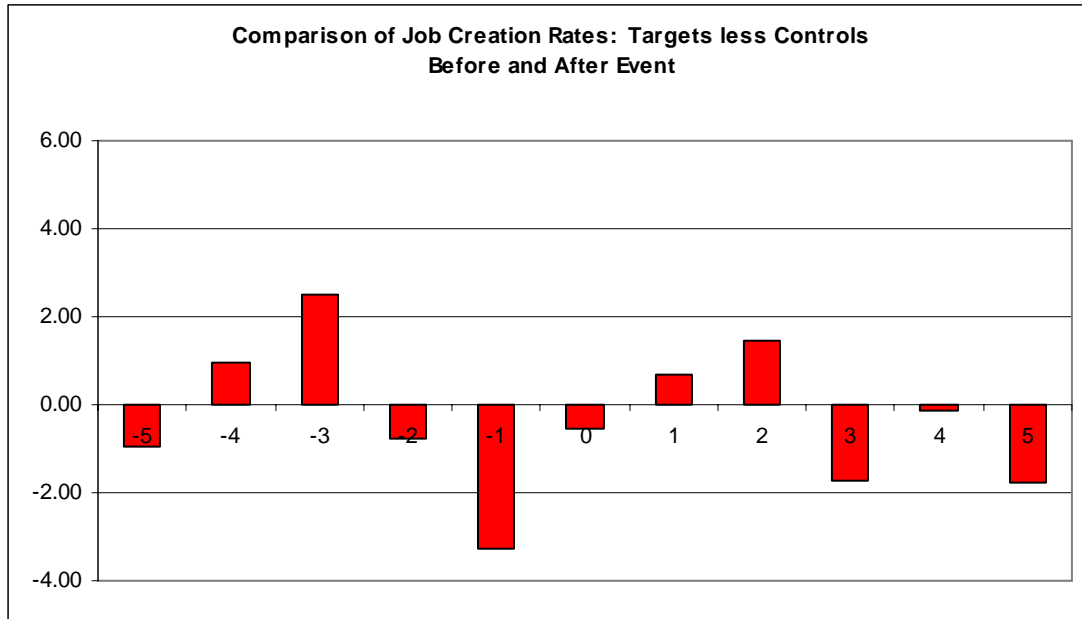


Figure 8b

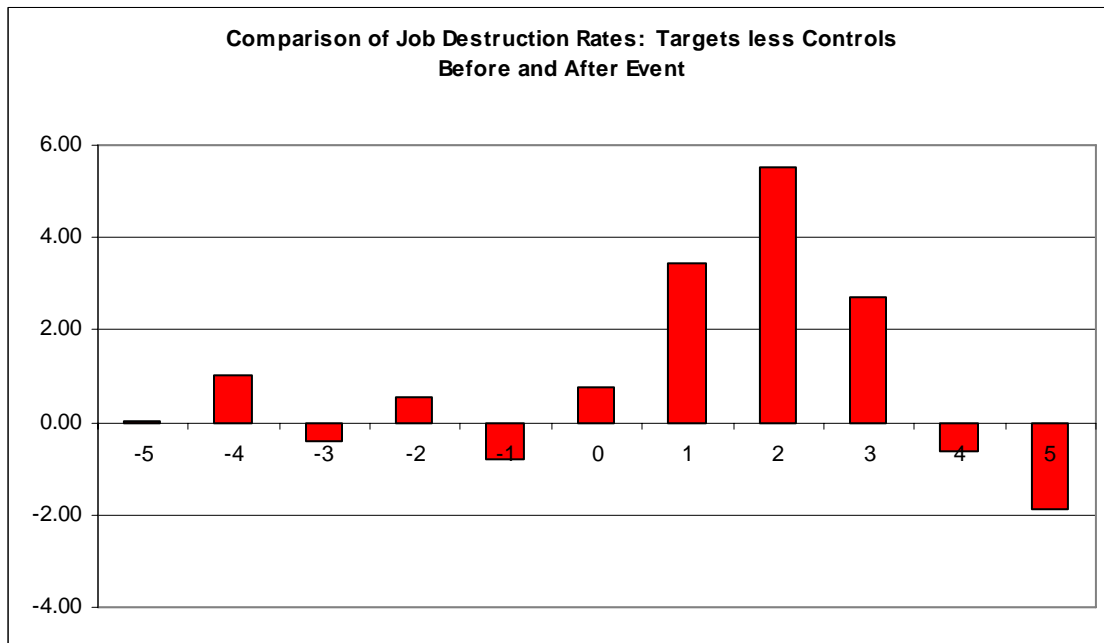


Figure 9a

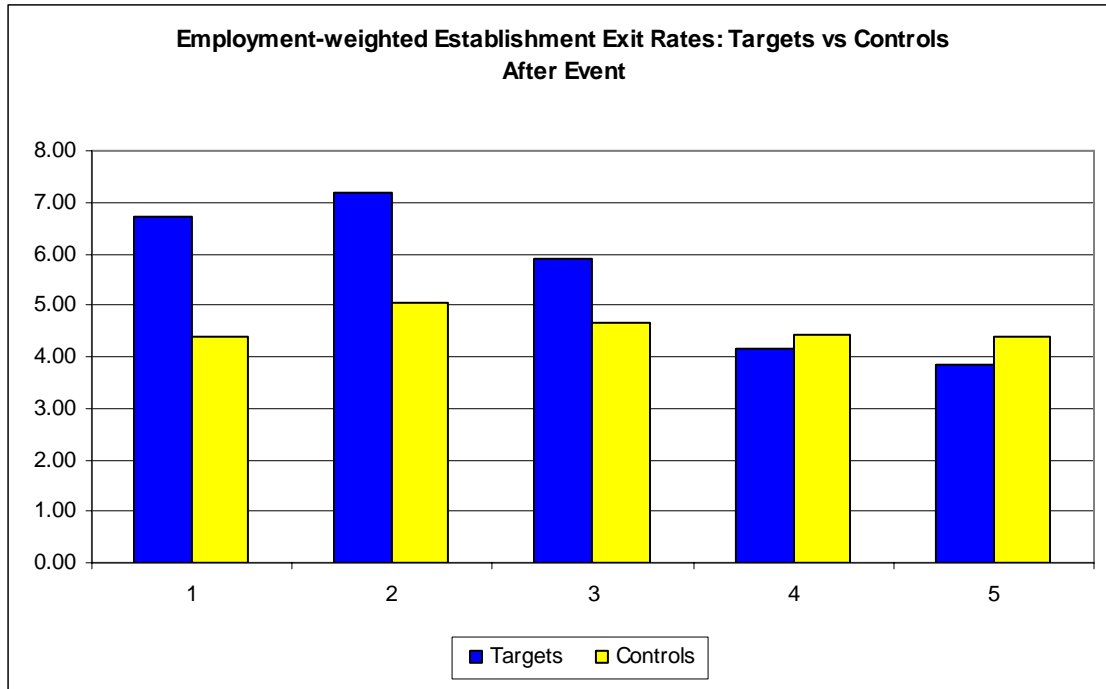


Figure 9b

[0]

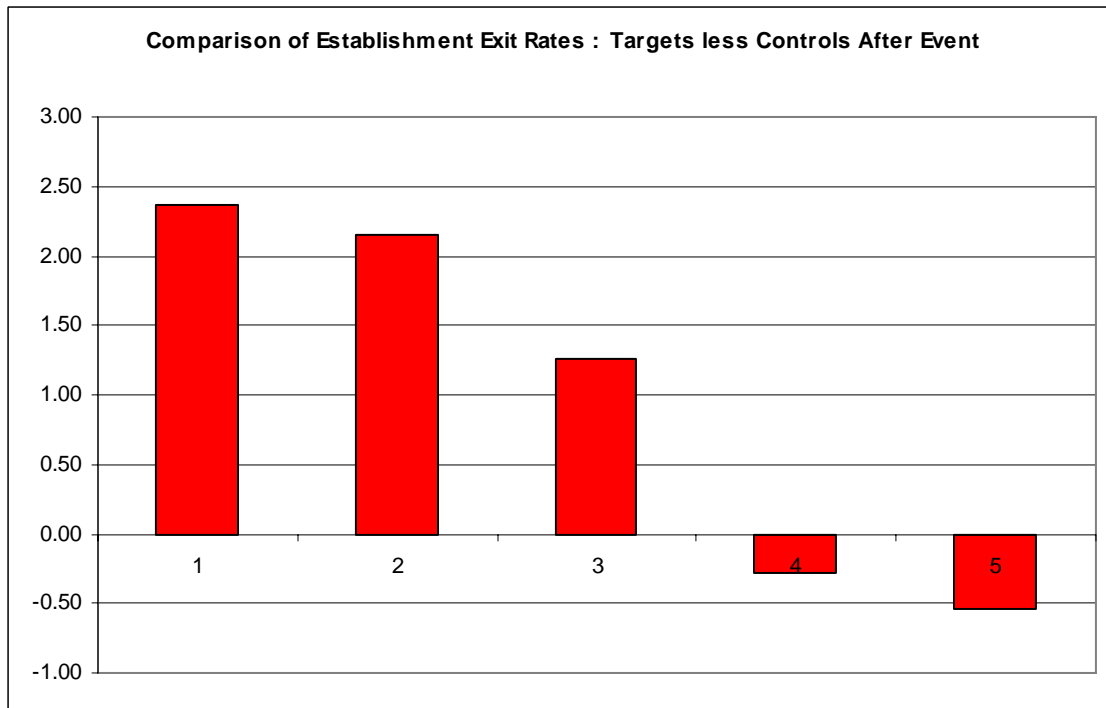


Figure 10: Differences in Impact by Targets and Controls Across Different Time Periods

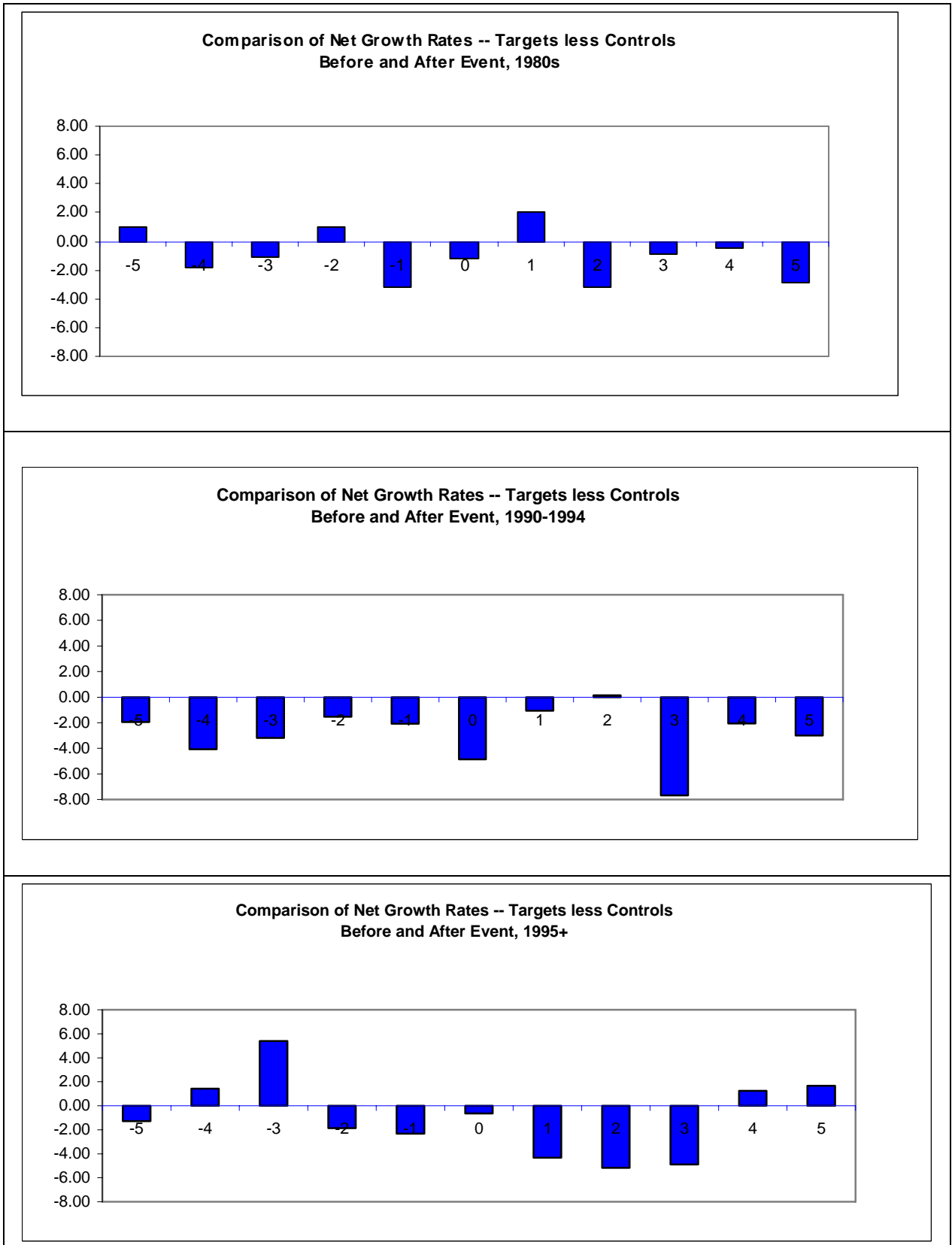


Figure 11: Variation in Impact on Employment Across Broad Sectors

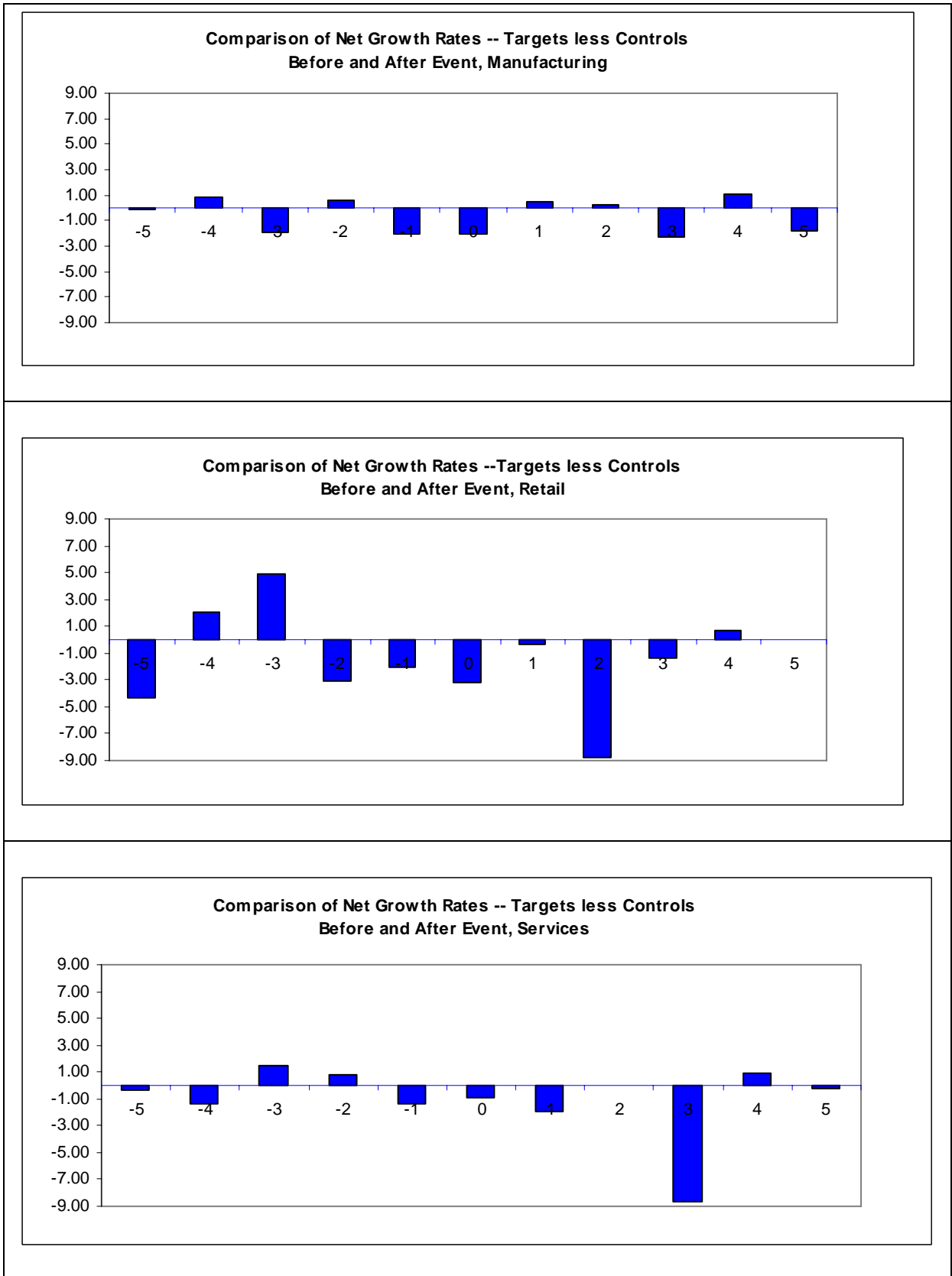


Figure 12a: Differences in Impact of Targets vs. Controls by LBO Type

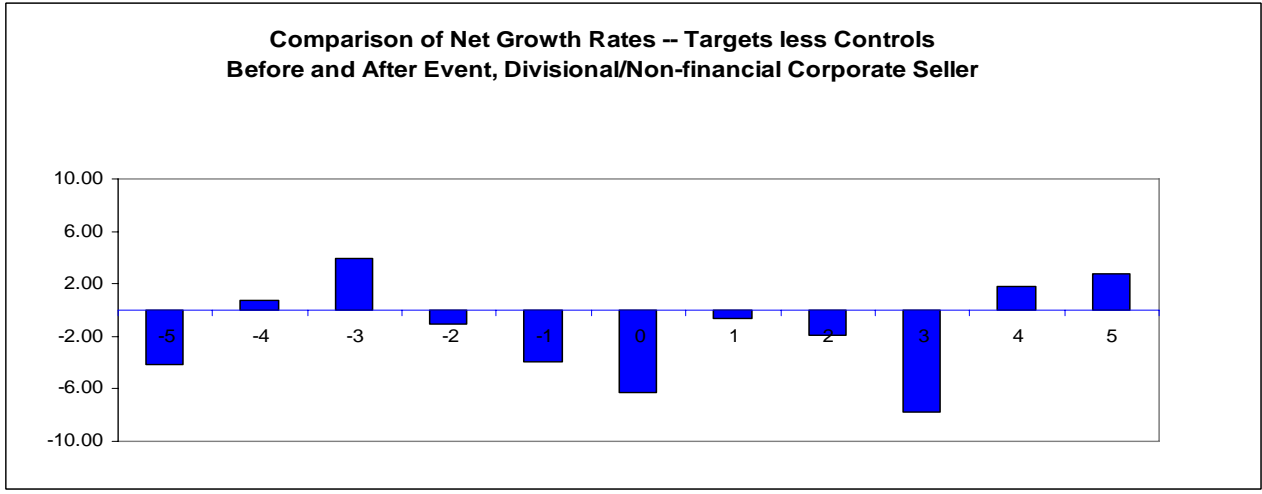
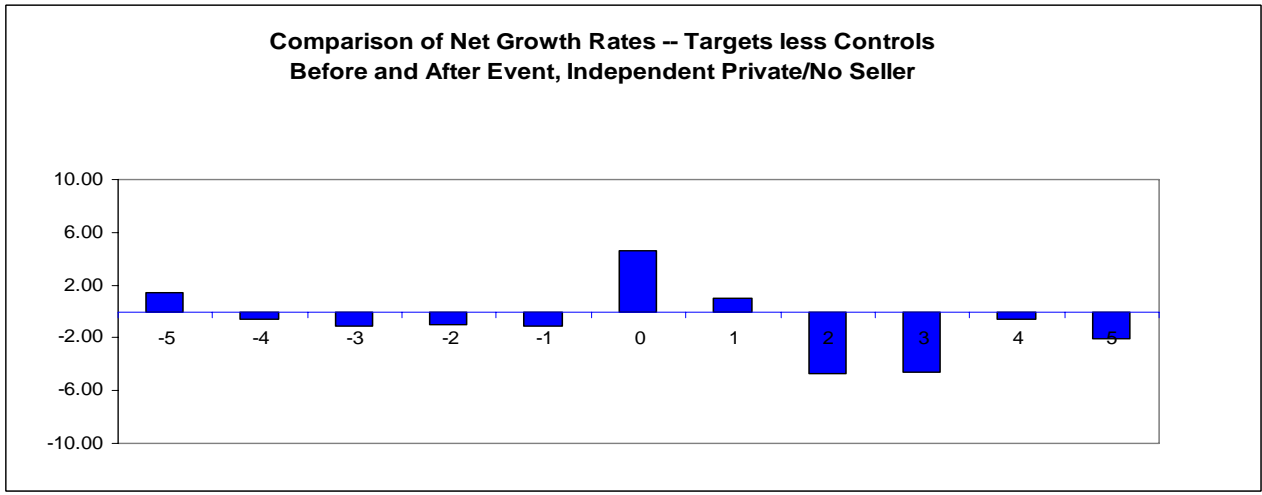
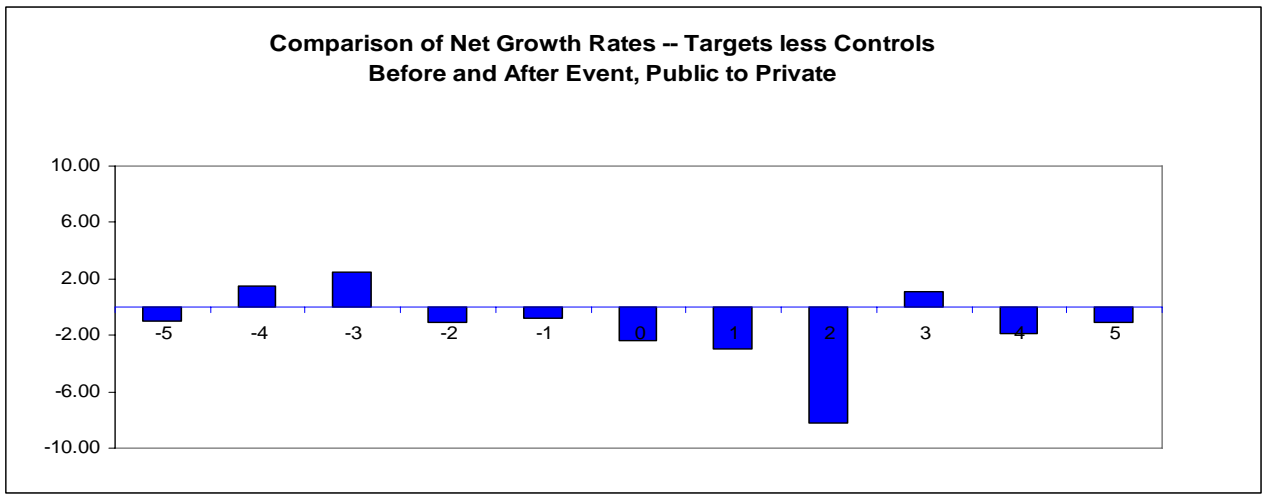


Figure 12 (cont): Differences in Impact of Buyouts vs. Controls by LBO Type

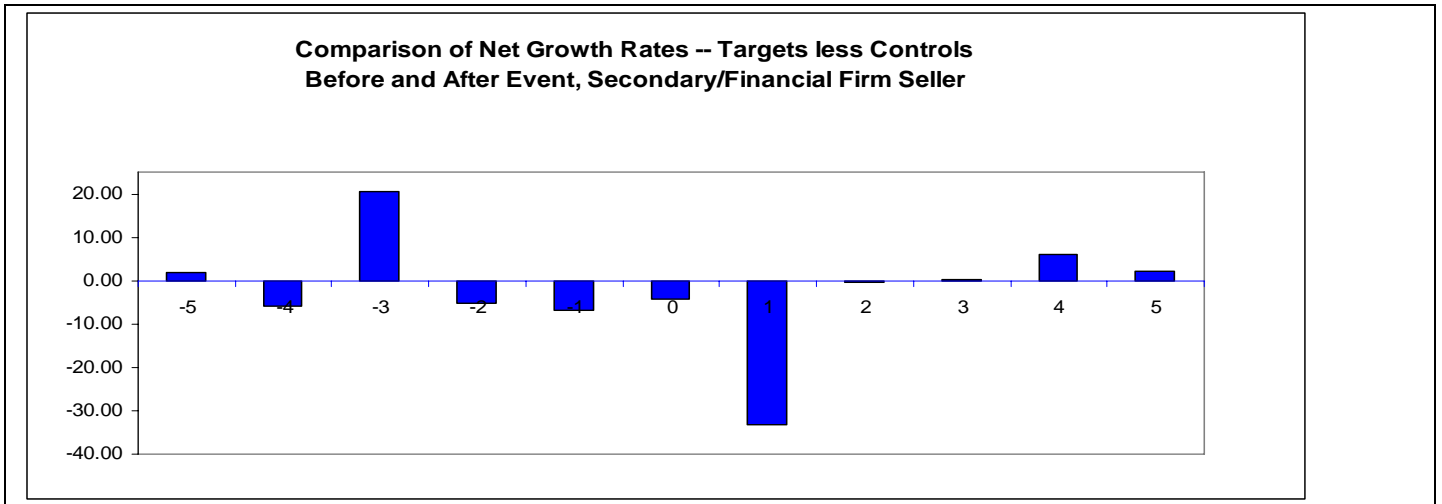


Table 1: Employment and Establishment Growth Rates: Target Firms relative to Controls (Deals through 2003)

	Dependent Variable:	
	Employment Growth Rate	Establishment Growth Rate
Target Dummy	-0.036 (0.002)	0.009 (0.002)
R-squared	0.134	0.09
Number of Observations	675,640	675,640

Notes: Regressions based on sample of Target and Control firms with growth rates calculated over two year horizon from event year t to t+2. All specifications include fully interacted industry, year, firm age, firm size, and Single Unit/Multi-Unit effects. All specifications are employment-weighted. Omitted group are non-Target firms.

**Table 2: Employment and Establishment Growth Rates: Target Firms relative to Controls:
By Deal Type (Deals through 2003)**

	Dependent Variable:	
	Employment Growth Rate	Establishment Growth Rate
LBO Type:		
Public to Private	-0.161 (0.005)	-0.065 (0.005)
Independent private/no seller	-0.028 (0.004)	0.082 (0.004)
Divisional/Non-financial Corporate Seller	0.048 (0.003)	-0.002 (0.003)
Secondary/Financial Firm Seller	-0.235 (0.007)	-0.053 (0.007)
All other LBO types	0.015 (0.025)	0.003 (0.023)
R-squared	0.137	0.091
Number of Observations	675,640	675,640

Notes: Regressions based on sample of Target and Control firms with growth rates calculated over two year horizon from event year t to t+2. All specifications include fully interacted industry, year, firm age, firm size, and Single Unit/Multi-Unit effects. All specifications are employment-weighted. Omitted group are non-Target firms.

Table 3: Greenfield Entry, Establishment Exit, Acquisitions and Divestitures (Two Year Employment Weighted Rates)

	Targets	Controls
Greenfield Entry Rate	14.9	9.0
Establishment Exit Rate	-16.7	-8.1
Establishment Acquisition Rate	7.4	4.7
Establishment Divestiture Rate	-5.8	-2.9
Continuing Establishment Net Growth Rate	-1.7	-0.1
Weighted Two Year Growth Rate	-1.9	2.6

Reported are Employment-Weighted Means of Rates as Percent of Average of Firm Employment in Event Year and Event Year + 2.
