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Private Equity and Employees

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Abstract

Using linked employer-employee data from Sweden, a difference-in-difference approach, and 201 private equity buyouts undertaken between 1998 and 2004, we show that unemployment risk declines and labor income increases for employees in the wake of a private equity buyout. Unemployment risk declines despite lower employment growth for continuing establishments—attributable to hiring freezes rather than to layoffs—and a lack of change in firm level employment growth. A plausible explanation is relaxed financial constraints: the effects are strongest in industries dependent on external finance for growth, for non-divisional buyouts, and for buyouts just prior to 2001.

Keywords: Buyouts, Employment, Financial Constraints, LBO, Private Equity, Restructuring.
JEL Codes: G24, G32, G34, J20, L25

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"The question is whether or not these companies were being manipulated by the guys who invest to drain them of their money, leaving behind people who were unemployed," Gingrich said. "Show me somebody who has consistently made money while losing money for workers and I'll show you someone who has undermined capitalism. That's an indefensible model."

- Guardian (2012) quoting Newt Gingrich.

1 Introduction

Every morning an increasing number of employees across the world find themselves working in a firm targeted for a buyout. Buyouts are undertaken by private equity firms who buy, improve, and resell mature firms using capital invested in private equity funds. During the period 1985 to 2006, private equity firms bought corporate assets in the US at an average yearly value of approximately 1% of the total US stock market value, with a top value of 3% in 2006 (Kaplan and Strömberg, 2009).

But the spread of the buyout business model has not escaped criticism. In the wake of the financial crisis in Europe, labor unions have claimed that buyouts, through layoffs and wage cuts, generate returns to investors at the expense of employees.[1] The question whether private equity firms are job creators or job destroyers has also stirred an intense media debate in the US, as one of the candidates for the 2012 Republican Party presidential nomination, Mitt Romney, is a former private equity executive. Our opening quote from Newt Gingrich, another candidate commenting on deals undertaken by Mr Romney's team, gives a good example of commonly heard views on buyouts.[2]

In light of the costs of unemployment, to employees and society, claims of systematic layoffs should be taken seriously.[3] Recent academic evidence on the employment effects of buyouts in the US suggest modest declines in firms employment growth, but indicate internal reorganization with employment growth declines in old establishments offset by the creation of new establishments (Davis, Haltiwanger, Jarmin, Lerner, and Miranda, 2011).[4] Evidence from the UK also suggest declines in firm level employment growth, but the effect appears to be weaker for more recent buyouts than for buyouts in the 80s (Wright, Thompson, and Robbie, 1992; Amess and Wright, 2007; Amess, Girma, and Wright, 2008). Things look different in France, where buyouts provide capital to credit constrained firms and thereby spur firm level employment growth (Boucly, Sraer, and Thesmar, 2011). These studies are important for enhancing our understanding on how buyouts affect employment. Yet data limitations have prevented

[1] See, for example, FSA (2008); ITUC (2007); PSE (2007).

[2] For more on the debate, see for example The Economist, Jan 28th 2012, "Monsters, Inc?"; The Economist, Jan 28th 2012, "Bain or blessing?"; Financial Times, Jan 10th 2012,"Video attacks Romney's record"; Financial Times, Jan 13th 2012, "The bane of Bain"; or The Wall Street Journal, Jan 9th 2012, "Romney at Bain: Big Gains, Some Busts".

[3] Evidence suggests that unemployment can lead to, among other things, wage cuts after accepting a new job offer, a consumption reduction and loss of income as well as a general decline in happiness. See, e.g.,Farber (2005), Katz and Mayer (1990), Jacobson, LaLonde, and Sullivan (1993), Gruber (1997) and Di Tella, MacCullock, and Oswald (2001).

[4] Declines in firm level employment growth rates are consistent with other papers utilizing data for shorter time spans (Kaplan, 1989; Muscarella and Vetsuypens, 1990; Lichtenberg and Siegel, 1990).

them from pinning down a central question in the heated debate: what happens to individual employees? The question cannot be fully answered using data at the establishment or firm level. Take the observed declines in employment growth as an example. These could be due to layoffs (hurting employees) or to natural attrition and reductions in hirings (not affecting employees). Given the growth of the buyout industry, the extensive media coverage, and the global political consequences of the debate, evidence on how individual employees are affected is of general interest.

In this paper, besides performing analyzes of employment growth at the firm and establishment level, we go beyond previous studies by evaluating the effects on individual employees' unemployment risk and labor income. We base our analysis on 201 buyouts undertaken between 1998 and 2004 in Sweden. Sweden has an active private equity market (with close to 50% of all buyouts undertaken by foreign private equity firms), and rich linked employee-employee data is available. Additionally, access to population data allows the construction of a control group of employees not affected by a buyout using detailed demographic data.

We present three novel findings.

1. The yearly unemployment risk is reduced by 1.1 percentage points or 12.7% on average for four years after the buyout. Yearly labor income increases by 3734 SEK or 1.4% on average over the same period. The labor income increase is not driven entirely by the lower unemployment risk in itself and occurs across most quartiles of the labor income distribution.

2. The effect on firm and establishment level employment growth are similar to effects observed in the US and the UK, but not those observed in France. The cumulative four year difference in employment growth rate between treated and control establishments is -6.0 percentage points in Sweden compared to -5.4 percentage points reported for the US in Davis et al. (2011). The point estimate for the average establishment employment growth rate in the four years following a buyout is -1.2 percentage points. The decline in establishment employment growth rate is driven by reduced hirings rather than by increased layoffs. In line with previous evidence from Sweden in Bergström, Grubb, and Jonsson (2007), we find no statistically significant effects on firm level employment growth.

3. A plausible explanation for the reduction in unemployment risk is that the buyout improves access to capital for financially constrained firms. The reduction in unemployment risk is mostly concentrated to industries dependent on external capital to grow, non-divisional buyouts in which the target firm is more likely to be financially constrained, and buyouts undertaken just prior to the economic slowdown following the IT stock market crash in 2001. Labor market regulations do not seem to restrict private equity firms from firing employees. On the contrary, employees with softer protection are more likely to benefit from a buyout.

These findings are based on a difference-in-difference estimator identifying the effects from variation between a treated group and a control group of employees over time. The central assumption behind the difference-in-difference estimator is parallel trending in the absence of treatment. Then, the trend in the outcome of the control group serves as the counterfactual outcome and

the estimated effect can be interpreted as causal. To ensure parallel trends, a good control group is essential. We match treated employees with similar employees not affected by a buyout using detailed information on current and historic observable characteristics such as sex, age, skill level, income, unemployment incidence, geographical location and firm/establishment characteristics. To address any remaining concerns of potential selection on unobservables, we perform two robustness checks. We estimate a staggered treatment model using employees in firms who have been or will be affected by a buyout as controls and, construct an alternative control group of employees who are affected by strategic acquisitions. Overall, the sign and the statistical significance of the average estimated effect remains constant indicating that the estimates in our main specification can be interpreted as causal.

Our paper contributes to the literature on the employment effects of buyouts by going beyond existing firm and establishment level studies to focus on the effects on individual employees. Moreover since most benefits to employees occur when financial constraints are important, our paper also offers a contribution to the more general literature on finance and labor which has not yet studied how financial constraints affect individual employees' unemployment risk.[5]

The next section presents a theoretical background and Section 3 discusses our data and identification strategy. Our main results are presented in Section 4 and we discuss interpretations of our results in Section 5. Additional analysis and robustness checks are available in an online appendix.[6]

2 Theoretical Background

In a foundational paper on the role of buyouts, Jensen (1989) argues that a private equity firm – or leveraged buyout association – is an organizational form superior to the public corporation because it is designed to reduce agency problems between dispersed owners and the manager of the firm. Dispersed ownership allows managers to avoid hard and unpopular tasks such as firing employees and reducing wages. Without careful monitoring and the right incentives, managers can engage in empire building by hiring too many employees, acquiring too many companies, or diversifying activities too much. A buyout could reduce these problems since private equity firms concentrate ownership, implement pay-for-performance schemes, and increase leverage (Leslie and Oyer, 2009). But an increase in leverage is also accompanied with an increase in bankruptcy risk (Andrade and Kaplan, 1998; Strömberg, Hotchkiss, and Smith, 2011), and slimming the organization to get rid of slack could involve layoffs and a reduction in wages. Indeed, a motivation for hostile takeovers could be to capture value from employees through breach of implicit contracts between managers and employees (Shleifer and Summers, 1988). As Lazear (1979) points out, moral hazard can make it optimal to pay employees a lower wage than the value of their marginal productivity early in their careers and a wage higher than the value of their marginal productivity later in their careers. If writing an explicit contract is not possible, employees and managers can implicitly agree on wages increasing with tenure. Such

[5]For more aggregated empirical studies on the connection between financial constraints and labor markets see, for example, Caggese and Cuñat (2008), Matsa (2010), Benmelech, Bergman, and Seru (2011) or Pagano and Pica (2012) and the references therein.

[6]See http://www.ifn.se/joacimt

agreements can be broken after an ownership change. If wages are hard to reduce, due to labor market regulations or collective agreements, dismissing employees with longer tenure being paid a wage higher than the value of their marginal productivity is optimal. Thus, cost cutting, a high debt load, and breach of implicit contracts can lead to *increased unemployment risk.*

But there are also reasons to why buyouts can benefit employees. As several authors have pointed out, private equity firms are, today, more oriented towards operational improvements and helping firms grow than cost cutting.[7] Boucly et al. (2011) argue that buyouts can be a substitute for other sources of capital and thereby accelerate firm level employment growth if firms are financially constrained prior to the buyout. There are several reasons to expect a buyout to relax financial constraints at the firm level. For example, private equity firms have connections and experience in dealing with banks; they are good at monitoring the firms so banks are more willing to lend; and they are more likely to reinvest earnings rather than pay out dividends because of tax reasons and a focus on the exit. If better access to capital makes the firm more resilient to negative profitability chocks and allows new investments to be undertaken, buyouts can lead to *decreased unemployment risk.*

It remains an empirical question whether the negative effects on employees from cost cutting and a higher debt load outweighs the positive effects of relaxed financial constraints.

3 Data and Identification

3.1 Sample construction

To study if the negative effects on employees from cost cutting and a higher debt load outweighs the positive effects of relaxed financial constraints we create a comprehensive data set on buyouts and employees. We use two sources of information on buyouts: the Capital IQ database and buyouts identified in Bergström et al. (2007). Our starting point is transactions in the Capital IQ database with the target's geographic location being Sweden and the announcement date being between 1998 and 2004 (10 397 transactions). From there on, we use similar selection criteria as Strömberg (2008). We select all transactions having secondary transaction features tagged as "Leveraged Buy Out" or "Management Buyout" and those having buyers/investor stage of interest tagged as "Buyout".[8] We then keep transactions marked as "Closed" or "Effective" and remove transactions involving minority stakes or which are private investments in public equity.[9] To this list we add buyouts identified in Bergström et al. (2007) that Capital IQ did not record (39 transactions) providing us with a sample of 322 buyouts.[10]

[7] See, for example, Boucly et al. (2011) or Kaplan and Strömberg (2009).

[8] Capital IQ defines a leveraged buyout as follows: "This feature is assigned when a financial sponsor acquires a mature business by combining equity with debt, raised by leveraging the business. This is only applicable: [i)]To strategic buyer transactions when it is explicitly mentioned in the press release. [ii)]To transactions where a majority stake is being acquired (i.e. 50% or more). "

[9] Capital IQ defines a transaction as closed when the transaction has been closed, but no hard information is available on whether it is effective. An effective transaction is a transaction that has been closed and where Capital IQ has found information that it is also effective. In practice, all closed transactions should be effective unless the transaction is recent.

[10] Bergström et al. (2007) describe their sample of buyouts as follows: "Our sample contains all private equity sponsored exits with a deal value of over $5 million exited in the period 1998 to the first half of 2006. The sample is further limited to deals where at least one of the private equity sponsors in the investor syndicate

We use the IFN Corporate Database containing information on names and the registration number for all firms in Sweden to add the firm registration number to each buyout.[11] Since Capital IQ only gives us the name of the target firm, we manually match names from Capital IQ to names and firm registration numbers in the IFN Corporate Database. After this procedure, we are left with 255 buyouts with firm registration numbers. The most likely reason for why we fail to find registration numbers is that the firm has changed its name or that it is not registered as a Swedish limited liability corporation.

Correcting for the group structure of limited liability corporations in Sweden allows us to keep track of majority ownership of firms by other firms. For example, a buyout can take place in a firm that is a holding company with majority ownership in several other firms (who in turn can own other firms). If we do not correct for the group structure, the buyout would show up as affecting zero employees since all employees are registered as working in firms owned by the holding company.

We use the IFN Corporate Database to obtain information on the ownership structure of firms in Sweden (information is available for 1997-2007). We take the date the buyout was announced and apply last year's ownership structure to the buyout if the ownership structure was reported before the first of November and this year's ownership structure if the buyout was reported after the first of November. We use the first of November as the basis for our merge because the employer-employee link is made for the first of November each year and because we want to ensure that employees are not treated before the buyout is announced. We then mark firms as being part of a buyout if they were directly or indirectly majority owned by the targeted firm. If there are two buyouts in the same firm registration number in a given year, we drop the second buyout making our sample unique on firm registration number and year.

Using the firm registration numbers we can identify employees affected by buyouts in the LISA database, available from Statistics Sweden.[12] The LISA database covers the population over 16 years of age in Sweden from 1990 to 2008 and links employees to employers. The yearly variables we gather from this database are age, sex, highest attained education level, the firm registration number for the individual's main source of income, establishment identifier for the individual's main source of income, labor income, registered number of days in unemployment, and the establishment's industry code and municipality. For each buyout, if it was announced before the first of November, we match that buyout with last year's employee information. If it was announced after the first of November, we use this year's employee data to ensure tagging employees as treated before the buyout was announced.

Our analysis will be based on data for six years prior to the buyout to four years after the buyout. Since the LISA database contains information on individuals above the age of 16, we

belongs to the 300 largest sponsors in the world by capital under management and the buyout firm is Swedish. This gives a total of 73 unique exits. [...] Private equity sponsored exits were identified through the mergers and acquisition database Mergermarket." We do think there is a slight cause for concern about coverage in the Capital IQ database since it only picked up 46% of the transactions analyzed by Bergström et al. (2007).

[11] The original source of the data is Swedish Companies Registration Office ("Bolagsverket"), the government agency that keeps track of official names, firm registration numbers, and accounting information of all limited liability corporations in Sweden. It does not cover one person firms with unlimited liability ("enskild firma") or partnerships. The information was gathered and validated by the consulting firm PAR.

[12] For more on the Longitudinal Integration Database for Health Insurance and Labour Market Studies (LISA) database, see the description at http:// www.scb.se/Pages/List____257743.aspx

Table I: The Sample

Year	Buyouts	Firms	Establishments	Employees
1998	19	71	179	6 733
1999	18	52	131	5 031
2000	34	131	318	12 357
2001	23	69	185	6 720
2002	36	60	181	6 980
2003	26	49	344	8 076
2004	45	164	566	20 161
# Observations	201	596	1 904	66 058

Notes. This table displays how the sample of treated employees, establishments and buyouts are spread out over time. "Firms" refer to firm registration numbers. Correcting for the group structure, on average a buyout affects around three firm registration numbers.

drop all employees younger than 22 years to ensure that each individual has at least six years of data before being affected by a buyout. In the merge we lose 54 buyouts (we go from 255 to 201). These are firm registration numbers which have no employees reporting that firm registration number as their main source of income. Out of our 201 buyouts, 25 are "divestitures" in the sense that we located the firm registration number not in the year the buyout is announced but one year later.[13]

The final sample is summarized in Table I. We end up with 201 buyouts affecting 596 firm registration numbers, 1 904 establishments and 66 058 employees between 1998 and 2004.[14] The average buyout affects 329 employees in 2.97 firm registration numbers. The number of buyouts per year increases in over time. The sample of 201 buyouts for which we can identify employees corresponds well to the total sample of 255 buyouts in Sweden registered in Capital IQ database.

Table II shows that average transaction values and the distribution of transaction types in our sample are similar to all transactions registered in Sweden. In both samples, corporate divestitures, cross border buyouts and management buyouts account for the bulk of all buyouts. During 1998 to 2004, in terms of the number of buyout in the Capital IQ database, Sweden ranked ninth in the world, with 1.7% of all buyouts worldwide being undertaken in Sweden. The final columns in Table II illustrate that buyouts in our sample do have smaller mean transaction value than in the U.S, but larger than those in the U.K and France. Sweden also has a lot of cross-border buyouts and corporate divestitures, but fewer management buyouts.

Table III presents average values for our sample and a 10% random sample of all employees in Sweden.[15] Employees in buyout targeted firms have on average higher yearly income and are more likely to be men; otherwise the samples correspond well to each other in terms of average age, average highest attained educational level (a seven degree scale from no education to having a PhD) and the geographical location of the establishment the employee works at.

[13] These are likely newly formed firms as a result of a divestiture in connection with the buyout.

[14] An establishment is defined as a geographical place of work. For example, a company with two stores at different locations has two establishments (one for each store).

[15] We weight the random sample such that its yearly size corresponds to the yearly distribution of employees in targeted firms.

Table II: Comparison to Capital IQ Sample

	Our Sample	Sweden	US	UK	France
Transaction Types					
Going Private	2.5%	2.7%	5.7%	6.6%	2.5%
Corporate Divestiture	31.8%	33.6%	28.1%	31.8%	16.4%
Secondary Buyout	4.9%	5.5%	4.0%	5.2%	6.3%
Bankruptcy Sale	0%	0.4%	2.8%	1.8%	0.8%
Management Buyout	24.8%	25.3%	30.6%	71.8%	45.6%
Family Succession	2.5%	2.3%	1.7%	1.8%	3.0%
Cross Border	33.8%	32.4%	4.8%	10.3%	31.1%
Platform	3.0%	4.7%	9.1%	3.1%	3.9%
Transaction value					
Mean ($mm 31.12.08)	212.29	206.13	267.50	106.17	206.84
Standard deviation	(400.54)	(379.13)	(3123.74)	(479.75)	(535.18)
# Observations	201	255	4958	2210	881

Notes. This table displays how our sample compares to the full Capital IQ sample for Sweden and to other countries. Data on transaction values are missing for around 70% of all observations. Transaction types are not mutually exclusive.

Table III: Characteristics of Treated employees

	Our sample	Random sample (10%)
Age	40.67	41.94
	(11.41)	(12.46)
Share female	0.42	0.48
	(0.49)	(0.50)
Education Level	3.25	3.36
	(1.12)	(1.87)
Labor Income	260 151 SEK	218 030 SEK
	(169 553 SEK)	(161 759 SEK)
Geographical Location	1.83	1.90
	(0.79)	(0.95)
# Observations	66 058	4 079 996

Notes. This table displays a comparison of our treated employees to a randome sample of all those employed in Sweden during 1998 and 2004. We have 21 189 missing values for education in the random sample of all those employed and 215 missing values in our sample. We weight the random sample such that its yearly size corresponds to the yearly distribution of employees in targeted firms. Standard deviations are given in parenthesis.

3.2 Identification

To identify the effect of buyouts on employees, we use a difference-in-difference estimator coupled with population data on employees in Sweden. The difference-in-difference estimator compares the relative unemployment risk between employees in a treatment group and a control group over time, and allows us to control for unobserved time invariant group effects as well as common time effects. The model we base our estimates on is specified as

$$Y_{igt} = \alpha + \lambda_t + \gamma BO_g + \beta(Post_t \times BO_g) + \epsilon_{igt}, \tag{1}$$

where Y_{igt} is a dummy measuring whether employee i in group g was officially registered as unemployed for at least one day at time t.[16] Time effects are represented by λ_t and BO_g is the treatment indicator taking on the value one if an employee is treated and zero otherwise. The interaction term, $Post_t \times BO_g$, takes the value one at the buyout year and all years after for the treated group and zero otherwise. Consequently, β captures the causal effect of the buyout on those employed at the targeted firm when the buyout was announced and represents a local average treatment effect.

To estimate Equation 1 we normalize time such that year zero is the year the buyout was announced. There is no consensus on how to compute the standard error for the difference-in-difference estimator but a conservative approach is suggested by Donald and Lang (2007). They point out that the relevant variation for a difference-in-difference estimator is at the group level and not at the individual level. Estimation with group-time aggregated data is efficient and inference can be made under the assumption that the underlying common group errors are normally distributed. The assumption likely holds in our individual level analysis because our groups have both the same number of observations and a large and constant number of observations. Therefore, to account for possible intra-class correlation within time periods we aggregate our data to a group-time level and estimate the following model:

$$\triangle Y_t = \rho + \beta(Post_t) + \triangle\varepsilon_t, \tag{2}$$

where

$$\triangle Y_t = Y_{Tt} - Y_{Ct}, \triangle\varepsilon_t = \varepsilon_{Tt} - \varepsilon_{Ct}. \tag{3}$$

The variable $Post_t$ indicates the year of the buyout and all years after, so β is the difference-in-difference estimator. In the establishment and firm level analysis we fall back to reporting estimates using the disaggregated data because the number of observations in each group is smaller.

The construction of the control group is crucial for identification. For β to have a causal interpretation the treatment and the control group must have parallel trends in the absence of treatment. Then the trend of the control group serves as the counterfactual trend in unemploy-

[16] Our formal definition of unemployment risk is thus "the share of employees in a group that has registered for receiving at least one day of unemployment benefits this year". Robustness checks indicate that our results remain all the way up to defining the dummy as the employee at time t being registered as unemployed for at least 100 days.

ment risk for the treatment group. Parallel trends in the absence of treatment is by definition unknown but is more likely to be fulfilled if the composition of the control group is similar to the composition of the treatment group. As pointed out by Card, Ibarraran, and Villa (2011), analyzing trends before treatment is the only way to examine the validity of the control group. A similar pattern for the treatment and control group in the period before the treatment makes a common pattern after treatment more likely, because common shocks have previously affected the groups in similar ways. We are well positioned to examine trends before the buyout because we have six years of data before each buyout.

We use the universe of registered employees in Sweden each year to construct the control group. We create yearly cells based on individual sex; age in ten year intervals; skill level measured by a dummy taking the value one if the employee has at least two years of undergraduate studies; four regional locations of the establishment; 17 categories of industry classification of the establishment; a dummy taking the value one if the employee has been registered as unemployed at least one time during the last three years; firm size quartiles one year prior to the buyout; a growth dummy taking the value one if the firm has positive employment growth during the last two years prior to the buyout; and five quantiles of average labor income over three years prior to the buyout.[17] If the number of control observations within a cell exceeds the number of treated observations, we randomly drop controls to have a perfectly balanced sample within each cell. If an employee is treated multiple times, we only include the employee as treated the first time he or she is treated.

The matching process leaves us with 65 395 treated employees paired with 65 395 controls. We lose 664 treated employees for which we fail to find a match. Table IV presents summary statistics for the quality of the match. The groups balance well: we have performed a normalized t-test for difference in means finding no statistically significantly difference.[18] Employees in our sample are on average 40 years of age at the time of the buyout, 21% have at least two years of undergraduate studies and thereby classified as high skilled, 42% are women, 44% were employed in the same firm three years prior to the buyout and 67% are employed in a firm that has had positive employment growth during the last two years. The employees were employed in firms with on average 116 employees.[19] The average yearly labor income is 260 423 SEK in the treatment group and 255 725 SEK in the control group. We fail to match employees with a history of unemployment and with more days in unemployment. The unmatched sample consist of on average older, more skilled employees with lower tenure and higher share with at least one day of registered unemployment during the last three years and total days in unemployment. A difference between the matched and unmatched sample is not a problem because the unmatched sample consists of less than 1% of all treated employees.

[17] An alternative matching procedure would have been to use propensity score matching. We chose not to go this route since we have such a large pool of employees to select controls from that we have no problems finding cell matches even when we use all the demographic information. A drawback of using propensity score matching is that the procedure relies on a correctly specified model for estimating the propensity score and a careful choice of matching algorithm. Using matching is simpler and more transparent.

[18] Imbens and Wooldridge (2009) suggests that if the normalized difference is less than a quarter, one need not worry about the model specification when using linear methods.

[19] Measured per firm registration number. The average buyout still affects 329 employees, because a buyout affects on average 2.97 firm registration numbers.

Table IV: Characteristics of Treated and Control Employees

	Treated	Control	Unmatched
Age	40.62	40.62	46.31
	(11.36)	(11.47)	(14.76)
Female	0.42	0.42	0.49
	(0.49)	(0.49)	(0.50)
High Skill	0.21	0.21	0.52
	(0.40)	(0.40)	(0.50)
Tenure	0.44	0.44	0.29
	(0.50)	(0.50)	(0.45)
Unemployment Incidence	0.06	0.07	0.20
	(0.24)	(0.25)	(0.40)
Unemployment Days	5.13	5.72	19.28
	(26.06)	(27.73)	(54.89)
Labor Income	260 424 SEK	255 725 SEK	233 351 SEK
	(169 962 SEK)	(177 056 SEK)	(119 835 SEK)
# of Employees in Firm at $t-1$	115.71	120.00	291.49
	(265.12)	(296.78)	(452.55)
Share in Growing Firms	0.67	0.67	0.57
	(0.47)	(0.47)	(0.50)
Geographical Location	1.83	1.83	2.08
	(0.79)	(0.79)	(1.23)
Mean Buyout Year	2001.62	2001.62	2002.05
	(2.09)	(2.09)	(2.18)
Most Common Industry	Manufacturing	Manufacturing	Health and Social Work
# Observations	65 394	65 394	664

Notes. This table displays summary statistics on how well our control group matches up with the treated employees at the matched year. The right most column present summary statistics for unmatched treated employees. Employees in the treatment and control group are on average equal on observable characteristics (we have performed a normalized t-test of difference in means between the treated and control group and all differences are statistically insignificant). "High Skill" refers to whether an employee has at least two years of undergraduate studies. "Tenure" displays the share of employees that had been employed for at least three years when the buyout occurred. "Share in Growing Firms" measures the share of firms that had a positive employment growth during the last two years prior to the buyout. Standard deviation in parenthesis.

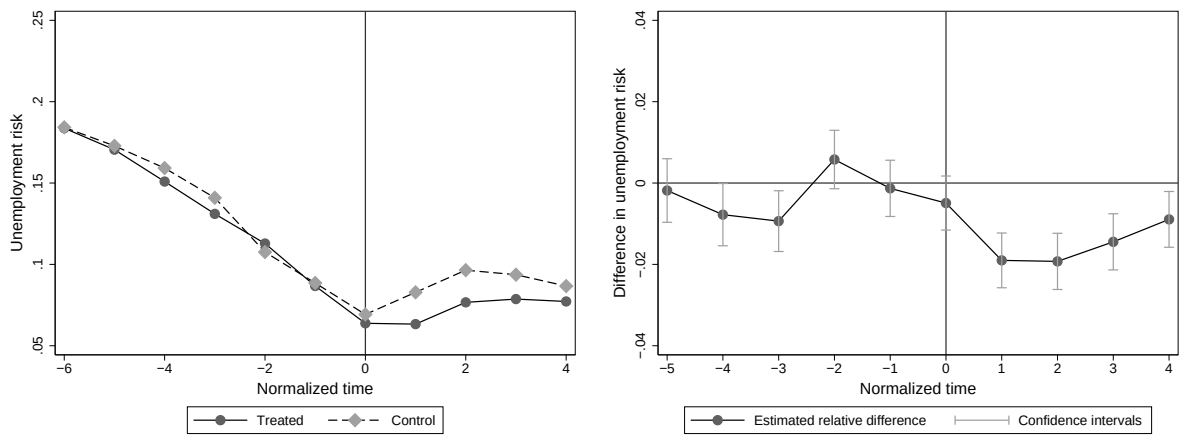

Figure I: Average Effect on Unemployment Risk

Notes. The left panel displays average unemployment risk for normalized time around the buyout for treated and control employees. The right panel presents the difference in unemployment risk for each year in normalized time around the buyout with the vertical lines representing 95% confidence intervals for tests of differences in mean between the normalized time and our base year $t-6$. We observe a declining unemployment risk before treatment and an increasing risk after treatment because all employees are employed at the buyout year. At $t=0$, the unemployment risk is non-zero as some employees are employed at the first of November but have been unemployed prior or after that.

4 Analysis

Armed with data and an estimation technique, we start with estimating the effects on employees' unemployment risk. For comparison with studies on the effects of buyouts on employment in other countries, we then turn to estimating the effects on establishment and firm level employment growth.

4.1 Effects on Employees

Unemployment risk. We start out by graphically analyzing unemployment risk around the time of the buyout. All individuals in our sample are employed at the buyout year so we expect a mean reversion pattern with a declining unemployment risk before treatment and an increasing risk after treatment. The left part of Figure I plots unemployment risk in a given year in the treatment and control group from six years prior to treatment to four years after. The right part of Figure I displays yearly differences between the treated and control group with confidence intervals showing whether the difference is statistically different at the 5% level in comparison with the difference at $t-6$. We use a critical value for the confidence intervals based on 11 degrees of freedom since we have 11 years of variation.

Because all employees are employed at the buyout year, a mean reversion pattern clearly shows up in the left figure. Unemployment risk decreases up until $t=0$ and then starts to increase.[20] Trends in unemployment risk before treatment are similar for treated and control

[20] At $t=0$, the unemployment risk is non-zero as some employees are employed in at the first of November but have been unemployed prior or after that. The mean reversion pattern can be compared to the "Ashenfelter dip" in the unemployment literature. A potential problem with a mean reverting pattern is that it occurs even

Table V: Difference-in-Difference Estimates.

	DiD estimate	% change.
Baseline Sample	-0.011*	-12.7%
	(0.004)	
Labor Income	3733.60*	1.4%
	(1522.10)	
Strategic as Controls	-0.006**	-7.9%
	(0.001)	
# Observations	11	11

Notes. This table displays difference-in-difference estimates at the individual level. We estimate the effect using group aggregated averages implying that we use 11 observations in our estimations to ensure that our standard errors are robust to all arbitrary correlations within groups at each point in time. The difference-in-difference estimate can be interpreted as percentage point effect. The percentage change is given with reference to the average value for the four year period after the buyout for the control group. Statistical significance at the 5% and 1% level is denoted by * and **. Standard deviations are given in parenthesis. Labor income is measured in SEK.

employees indicating that our estimates can be interpreted as causal. The right figure shows that the effect is strongest for the first two years and weakens slightly thereafter. Since we follow employees independently of whether they stay with the firm or not, a weakening of the treatment effect over time is not surprising as fewer employees remain with the treated firm.

The first row in Table V displays the estimate of the effect of the buyout using the aggregated difference-in-difference model outlined in Equation 2. The point estimate for unemployment risk is -1.1 percentage points on average for the four year post period and it is statistically significant at the 5% level. Using the post period average for controls as comparison, the effect of -1.1 percentage points constitutes a 12.7% decline in unemployment risk for treated employees. The effect is not concentrated to specific firm size quartiles or quartiles of the labor income distribution.[21]

Labor income. An alternative way to examine how buyouts affect employees is to study labor income. Because labor income is the result of a combination of wage and labor supply, a lower unemployment risk should translate into higher income in comparison to the control group. Figure II is equivalent to Figure I but plots labor income instead of unemployment risk. Again, the treatment and the control group have similar trends in average labor income before treatment. The increase in labor income before treatment reflects the requirement that both treated and control individuals must be employed at time zero. The difference-in-difference estimate reported in the second row of Table V reveals that labor income increases by 3734 SEK which converts to a 1.4% increase, on average, for four years after the buyout. The labor income increase is not driven by a specific quartile in the labor income distribution.[22]

The higher labor income is it not entirely driven by shifts to unemployment. We know that the probability of becoming unemployed after the buyout is reduced, so we expect that a larger

if a buyout has no effect on employees. Only by careful matching to find a control group experiencing a similar dip can the effect of the "dip" be minimized.

[21] See the online appendix for details.

[22] See the online appendix for details.

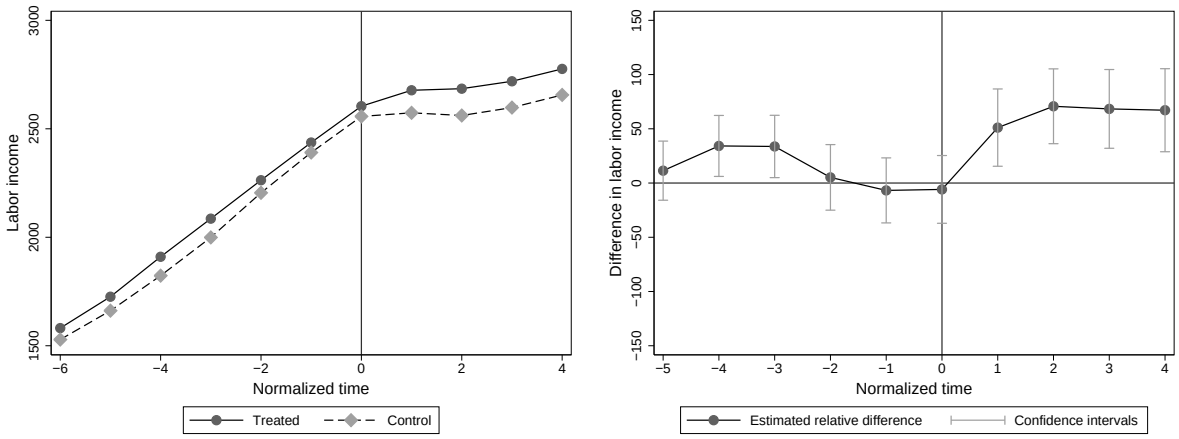

Figure II: Average Effect on Labor Income

Notes. The left panel displays average labor income for normalized time around the buyout for treated and control employees. The right panel presents the difference in labor income for each year in normalized time around the buyout with the vertical lines representing 95% confidence intervals for tests of differences in mean between the normalized time and our base year $t-6$.

share of individuals in the treatment group remain with their firm, relative individuals in the control group. Because the control group will have a higher share of unemployed individuals, the difference-in-difference estimate conditional on the employee remain with the treated firm can be seen as a lower bound on the effect of the buyout on labor income alone. Conditioning on being employed for the four years following a buyout, the difference-in-difference estimate is 2 991 SEK (or 1.1%) and it is statistically significant at the 1% level. Because we estimate a positive effect, the labor income effect in our main specification is not likely to be driven entirely by shifts to unemployment.

Strategic acquisitions. Even though we use rich demographic information in constructing the control group, there could still be unobservable time invariant characteristics making a firm a likely buyout target. If these unobserved firm characteristics correlate with idiosyncratic unemployment risk, we could falsely attribute the estimated effect on unemployment risk to buyouts rather than to selection. Given that we estimate a reduction in unemployment risk, a particular concern is that our matching procedure fails to capture that buyouts take place in firms that are positioned well to grow and thereby layoffs will be less likely.

The best way for us to deal with potential selection on unobservables is to use a different control group. If there are unobserved firm level characteristics that make a firm a particularly attractive target, our control group should be based on employees from potential or realized acquisition targets. We thus perform a robustness check by restricting the sample of possible control employees to employees in firms that were acquired by another firm in the same year as the buyout took place. We refer to these acquisitions as strategic acquisitions (as opposed to the financially motivated acquisitions undertaken by private equity firms).

To create the pool of potential control employees, we use the IFN Corporate Database to

Table VI: Estimates for Staggered Treatment

	(1)	(2)	(3)	(4)
Unemployment Risk	-0.025**	-0.019**	-0.020**	-0.024**
	(0.005)	(0.005)	(0.005)	(0.005)
Year Dummies	yes	yes	yes	yes
Industry*Year Dummies	no	yes	yes	yes
Year*Skill Dummies	no	no	yes	yes
Year*Tenure Dummies	no	no	no	yes
# Observations	641 454	641 454	641 454	641 454

Notes. This table displays difference-in-difference estimates for our staggered treatment approach using treated employees in other buyout years as controls for treated employees in a given buyout year. Columns (2), (3) and (4) present estimates accounting for industry-year specific trends, year-skill specific trends and tenure year specific trends. Standard errors clustered on buyout firm. Statistical significance at the 5% and 1% level is denoted by * and **. Standard deviations are given in parenthesis.

identify all firms that were acquired between 1998 and 2004 in Sweden.[23] We correct for the ownership structure by tagging firms as being a part of a merger or acquisition if, for a given year, they are directly or indirectly majority owned by a firm acquired in that year. We select controls from employees in these firms by applying the same matching procedure as for our baseline sample.

The second row of Table V displays the results from estimating Equation 2 on the aggregated data using the newly created treated and control group. The point estimate is a statistically significant and reveals a 0.6 percentage point decline in unemployment risk corresponding to a 7.9% reduction in unemployment risk. Though the result is slightly weaker, it gives us confidence that unobservable characteristics making a firm a particularly good acquisition target play a minor role. The result also suggests that strategic acquisitions do not affect employees in the same way as buyouts do: buyouts reduce unemployment risk to a greater degree.

Staggered treatment. Another way of dealing with potential selection on unobservables is to make use of the staggered treatment dates in our sample. The different treatment dates allow us to construct a control group within the group of affected employees along the lines of Bertrand and Mullainathan (1999) and Arai and Skogman-Thoursie (2009). That is, for a given year t, the control group consists of employees who are treated in earlier or later years. We estimate the following model using information for six years prior to each buyout and four years after each buyout:

$$Y_{it} = \alpha_i + \theta_t + \beta Deal_{it} + \varepsilon_{it}, \tag{4}$$

where Y_{it} measures whether employee i was registered for unemployment for at least one day during year t. Time constant employee heterogeneity is controlled for through employee fixed effects α_i, while θ_t captures all common time effects that influence unemployment risk. The variable $Deal_{it}$ is a dummy taking the value one if an employee is treated at time t or later.

[23] All mergers of firm registration numbers have to be reported to the Swedish Companies Registration Office, which is the basis for the merger information in the IFN Corporate Database.

The effect on unemployment risk or labor income is identified by β. The identifying assumption is, as before, that the groups have parallel trends in absence of a buyout. Since the staggered treatment dates do not allow us to use time-aggregated data to take group specific shocks into account, the standard errors are clustered at the firm registration number level for the buyout year. Unobservable characteristics within the group of treated employees are more likely to be similar, which mitigates any potential unsolved selection problems.[24]

Results are presented in Table VI. With our staggered treatment approach, the unemployment risk after a buyout decreases. The basic model (column one in Table VI), estimates the effect on unemployment risk to be -2.5 percentage points. Accounting for industry-year specific trends, year-skill specific trends and tenure year specific trends (columns two to four), the effect varies from -1.9 to -2.4 percentage points.[25] Because we still estimate statistically significant reductions in unemployment risk, it is not likely that our initial approach suffered from a severe selection problem.

4.2 Effects on Establishments and Firms

Continuing establishments. Having established statistically significant declines in unemployment risk for employees, we now turn our attention to what goes on at the establishment and firm level to compare to studies from other countries. We start by examining employment growth in continuing establishments, that is, we look at establishments existing at the time the buyout was announced. As with the individual level analysis, we use a difference-in-difference methodology with cell matching. Following Davis et al. (2011), we measure employment growth at time t for a group as the net change in employment at the group level defined as

$$g_t = \sum_i \frac{E_{it} - E_{it-1}}{\sum_i [0.5(E_{it} + E_{it-1})]}, \tag{5}$$

where E_{it} is number of employees at establishment i at time t. Weighting with the sum of the average firm sizes ensures that a group's employment growth rate is not driven by higher variance in employment growth in small firms.

Going from individual data to establishment data is trivial for the treatment group as we can simply use the establishments the treated employees were employed in. To create the control group, we match each treated establishment with an establishment that has not undergone a buyout using matching on a 17 category industry code; geographical location (four regions); deciles of firm size of the firm the establishment belongs to; deciles of the three year average size of the establishment; whether the average educational level at the establishment is at least two years of undergraduate studies; whether the firm that the establishment belongs to has had a positive employment growth over the last two years; and whether the firm is a multi-establishment firm or not.

To improve the accuracy of the match, we initially restrict the pool of potential control establishments to those employing fewer employees than 1.25 times the largest establishment in

[24] An alternative way of constructing a control group within the group of affected employees is to only use employees who are affected later as controls for employees subject to a buyout (thus omitting already treated employees from the control group). We do not use this approach because it reduces the usable sample size.

[25] The results are unchanged using firm fixed effects.

Table VII: Characteristics of Treated and Control Establishments

	Treated	Control	Unmatched
# of Employees in Establishment	43.67	43.48	39.46
	(86.89)	(88.37)	(145.48)
# of Employees in Firm	143.38	324.35	429.70
	(325.56)	(612.41)	(543.04)
Multi-establishment Firm	0.77	0.77	0.97
	(0.42)	(0.42)	(0.18)
Share in Growing Firms	0.70	0.70	0.88
	(0.46)	(0.46)	(0.33)
High Skill Establishment	0.02	0.02	0.09
	(0.13)	(0.13)	(0.28)
Geographical Location	1.83	1.83	2.07
	(0.87)	(0.87)	(1.14)
Most Common Industry	Wholesale/Retail	Wholesale/Retail	Transp./Storage/Commun.
# Observations	1376	1376	92

Notes. The first two columns display summary statistics for the treated and control establishments at the matched year. The right most column present summary statistics for unmatched treated establishments. We have performed a normalized t-test of difference in means between the treated and control group and all differences are statistically insignificant. Standard deviations are given in parenthesis. "High Skill" refers to whether the average employee at the establishment has at least two years of undergraduate studies. "Share in Growing Firms" measures the share of establishment that had a positive employment growth during the last two years prior to the buyout.

the treated group. We start out with a sample of 1 904 treated establishments that are directly or indirectly majority owned by a firm targeted for a buyout. We focus on establishments that are stable in the sense that they have been active for at least two years before the buyout was announced. After the match, we are left with 1 376 treated establishment and 1 376 controls. We are unable to find a match for 92 establishments and we lose 436 establishments because they are not present for two periods before the buyout was announced.

Table VII displays summary statistics for the matched data at the time the buyout was announced (at $t = 0$). The control group is on average identical or similar to the treatment group in terms of average education level, share of establishments in a multi-establishment firm and the average size of the establishment. We fail to create balance in firm size: the average firm size is 143 employees in the treatment group and 324 employees in the control group. We have performed a normalized t-test of difference in means between the treated and control group and all differences are statistically insignificant from each other except for firm size. If we compare the matched sample of establishments with the sample of treated establishment of which we fail to match, we see that the unmatched sample almost exclusively consists of establishments belonging to firms with more than one establishment (a multi-establishment firm) and that failed matches tend to employ high skill employees to a greater extent.

We start by examining how the sample of treated and control establishments evolve over time. The left panel of Figure III displays the cumulative share of establishments in each group that, for a given year, are present in the next year. The survival rates for treated and control

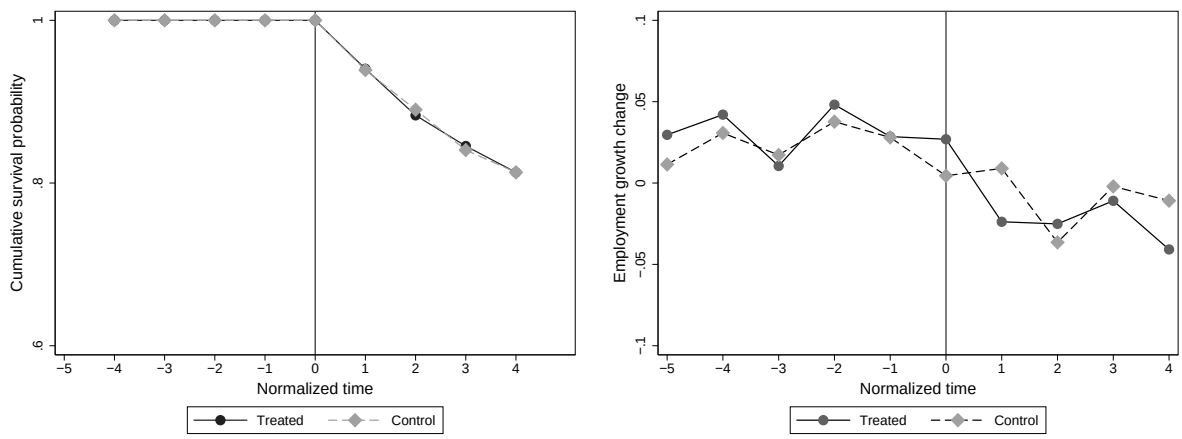

Figure III: Establishment Growth Change and Survival Probability

Notes. This figure displays the survival probability for an establishment (left) and the net employment growth change for the treated and control groups over time (right).

establishments are stable and around 80% of all establishments that were present at time zero are still active four years later. The similar survival rates suggests that treated establishments are no more likely to be shutdown than control establishments. The similar survival rates also make our results more robust to possible compositional changes in the treatment and the control group.

The right part of Figure III plots the average employment growth rates for the treatment and the control group. A similar pattern in employment growth prior to treatment reassures us of the quality of the matched control group. A slight relative decline in treated establishments occurs in the years following a buyout. Estimating Equation 1 using disaggregated data reveals that the effect corresponds to a statistically significant decline of 1.2 percentage points (with a standard error of 0.0004) for the post period when clustering the standard errors at the firm level.

Table VIII displays how our establishment level results compare to the establishment level results in Davis et al. (2011). Both studies apply the same measure for employment growth but Davis et al. (2011) analyzes buyouts during a period from 1980 to 2000 and has a data set covering more than 150 000 establishments in the US. Even though the US sample contains more buyouts over a longer period, the average change in employment growth is similar: the effect on the cumulative employment growth rate four years after the buyout is -6.0 percentage points in Sweden and -5.4 percentage points in the US. However, Davis et al. (2011) report that targeted establishments in the US are more likely to be shut down than control establishments, which we do not observe in Sweden.

Our individual level data allows us to go further than Davis et al. (2011) by decomposing changes in employment growth at the establishment level into hirings and separations. Our estimation is based on the following model:

$$Y_{igt} = \alpha + \lambda_t + \gamma BO_{gt} + \beta(Post_t * BO_{gt}) + X_{et}\eta + \epsilon_{egt} \tag{6}$$

Table VIII: Comparison Between Sweden and US

	Sweden	US
Time Period	1998-2004	1980-2000
# Obs Establishment (Treated)	1376	151 529
Establishment Match Variables	- Industry	- Industry
	- Size	- Size of Parent Firm
	- Size of Parent	- Age of Parent Firm
	- Location	- Multi-unit Status
	- Multi-unit Status	- Transaction Year
	- Average Skill Level	
	- Transaction Year	
Establishment Growth Difference		
$t+1$	-3.28%	-0.93%
$t+2$	1.13%	-2.23%
$t+3$	-0.88%	-0.55%
$t+4$	-3.00%	-1.64%
Cumulative $t+1$ to $t+2$	-2.15%	-3.16%
Cumulative $t+1$ to $t+4$	-6.03%	-5.35%

Notes. This table displays a comparison between establishment level employment growth in Sweden to that of the US (Table 3 in Davis et al. (2011)).

where Y_{igt} takes on different forms. For each year we track all employees at an establishment one year back and one year forward. To estimate the overall effect on hirings, Y_{igt} is a dummy taking on the value one if an employee is hired during a given year. To measure the effect on separations, Y_{igt} is defined as a dummy taking on the value one if an employee is doing his or her last year at an establishment given that the establishment is observed in data in the following year.

When we use aggregated data and the model represented by Equation 2, we find that the overall drop in hiring following a buyout is 4.1 percentage points and statistically significant (with a standard error of 0.009) while the estimate for separations is not significant (point estimate of -1.3 percentage points with a standard error of 0.009). Hence, reduced employment growth at the establishment level is due to hiring freezes rather than to increased separations.

Firm level employment growth. Moving up to the firm level, we start by examining net employment growth for group-adjusted firms consisting of the firm within an ownership group that was directly targeted by a private equity firm and its subsidiaries. Tracking firms over time is substantially more difficult than tracking individuals or establishments as firm registration numbers and group structures change frequently. A benefit of our data is that we can adjust for the group structure year by year and thereby can track acquisitions, divestitures, shutdowns and the creation of new divisions more accurately. However, we are still unable to correct for ownership structures that puts the targeted firm itself as a subsidiary; adjusting "upwards" risks that we aggregate up to the private equity fund level and thus include other portfolio firms as part of the treated firm. Because of the difficulty of accurately tracking firms over time, we restrict our analysis to two years before and after the buyout.

Table IX: Characteristics of Treated and Control Firms

	Treated	Control	Unmatched
Year	2001.63	2001.63	2001.24
	(1.98)	(1.98)	(2.06)
# of Employees in Firm	184.68	119.13	106.52
	(404.60)	(221.43)	(105.99)
# Establishments	4.95	4.36	3.45
	(14.40)	(11.31)	(4.26)
Geographical Location	1.97	1.97	2.19
	(0.94)	(0.94)	(1.30)
Share of Growing Firms	0.58	0.43	0.72
	(0.50)	(0.43)	(0.46)
# Est. in Group	46.09	46.26	80.31
	(92.72)	(99.08)	(161.11)
# Group Size	2407.19	2081.14	2596.76
	(7107.04)	(5903.54)	(4382.46)
Most Common Industry	Manufacturing	Manufacturing	Real estate, renting and business act.
# Observations	139	139	29

Notes. Th first two columns display summary statistics for the treated and control firms at the matched year. The right most column presents summary statistics for unmatched treated firms. "Share of Growing Firms" measures the share of firms that had a positive employment growth during the last two years prior to the buyout. "Group" refers to corporate group. Standard deviations are given in parenthesis.

We create a control group of firms matching on firm size divided into five quantiles and corporate size in five quantiles in terms of number of employees, number of establishments within the corporate group, geographical location (four districts), industry classification (17 categories), levels within a corporate group (five levels) and restrict the sample of potential control firms to those employing less than 1.25 times what the largest treated firm employs. The matching is done for each separate year between 1998 and 2004. Table IX shows summary statistics for treated and control firms at the year the buyout was announced. While the groups are similar, there is a clear difference in the number of establishments within the group and group size in terms of employment: treated firm registration numbers are more likely to come from larger corporate groups. Many buyouts we study are divisional buyouts from larger corporate groups. Matching these buyouts well is hard because of considerable variation in how corporate groups are structured and at what levels within the group subsidiaries are placed.

We follow the same procedure as in the establishment level analysis for calculating employment growth for the different groups over time.[26] Figure IV reveals that both groups show fairly similar trends in employment growth change before the buyout was announced. Figure

[26]Because we adjust for the group structure we end up with a few cases in which our growth measure at the firm level is close to 2 (the maximum value) as a result of the size of the firm growing by over 1000%. The high growth likely captures a misallocation of a subsidiary within a group (as a result of, for example, changing the firm registration number for administrative reasons). Since we weight each observation by the number of employees, our group aggregated measure gives a large weight to these incorrect observations. To deal with this, we drop 44 observations for which the growth from one year to the next is above 1.9 and in which the resulting firm size size is over 1000 employees.

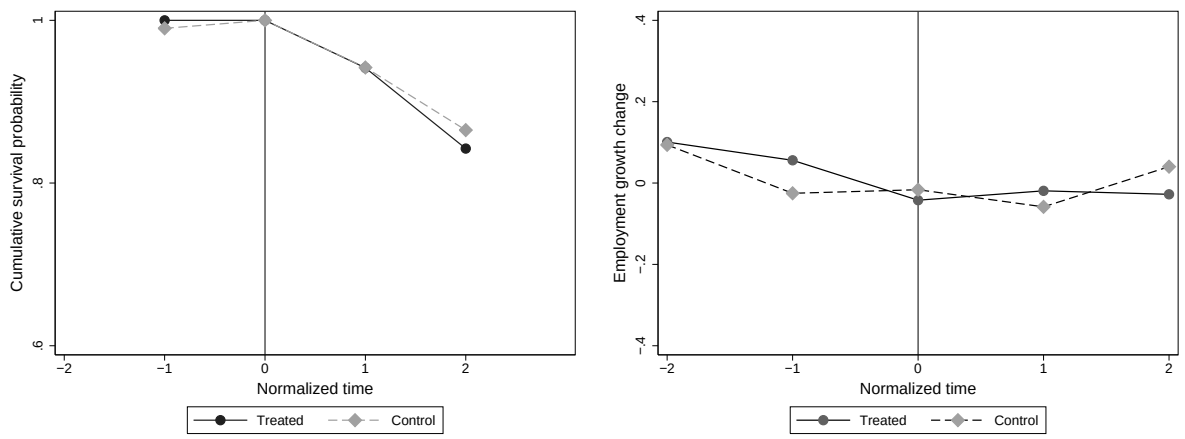

Figure IV: Firm Employment Growth and Survival Probability

Notes. This figure displays the survival probability for legal firm entities (left) and the net employment growth change for the treated and control group of firms (right).

IV also reveals no difference in employment growth change after the buyout. As shown by the right graph, there are no large differences between the treated and control group of firms in terms of survival rate after the buyout. Estimating Equation 1 using disaggregated data, the difference-in-difference estimate for employment growth is not statistically different from zero.

A central part of Davis et al. (2011) is showing that private equity firms seem to speed up the creative destruction process by closing down more establishments and opening up more new ones compared to their control group. As the establishment level analysis showed, an important difference between the US and Sweden is that we do not observe a greater degree of establishment shutdowns after the buyout. That there is no change in shutdowns is also evident from Table X, which presents descriptive statistics on the share of establishments shutdown, acquired created or divested between the buyout year and two years after. The only type of reorganization that seems to occur to a greater degree for treated firms is divestments of establishments. Neither greenfield creation nor new acquisitions of establishments are more common in treated firms relative control firms.[27] In sum, evidence of private equity firms speeding up the creative destruction process in Sweden is weaker than evidence from the US.

5 Interpreting the Results

We now turn to possible mechanisms that could explain a reduced unemployment risk for employees. Two interpretations strike us as probable. The first one draws on the theoretical background provided in Section 2: employees benefit because of relaxed financial constraints at the firm level. The second interpretation draws on the the argument that labor market regulations in Sweden restrict private equity firms from firing employees.

[27] A greenfield is identified in the data as a new establishment not present earlier. An acquisition is defined as an establishment existing before joining a firm.

Table X: Descriptives on Reorganization

		Treated	Control	Difference
		Panel A: Establishments		
At $t+2$	Remaining	87.2%	86.4%	0.8%
	Greenfield	7.4%	7.8%	-0.4%
	Acquired	5.4%	5.9%	-0.5%
From t to $t+2$	Remaining	72.5%	75.8%	-3.3%
	Divested	12.2%	8.7%	3.5%
	Shut down	15.3%	15.4%	0.1%

Notes. "At $t+2$" refers to a sample of establishments that were all present at $t+2$ where "Remaining" is the share of establishments that also were present at t, "Greenfield" is the share of newly started establishments and "Acquired" is the share of establishments that were not connected to a target firm at time t but were acquired later. "From t to $t+2$" refers to what had happened in $t+2$ to the sample of establishments that were all present in t, here "Remaining" refers to the share of establishments that were also present at t, "Divested" refers to the share of establishments that belonged to a target firm in t but not in $t+2$, and "Shut down" is the share of establishment that belonged to a targeted firm in t but did not exist at $t+2$.

5.1 Alleviating Financial Constraints

The financial constraints story receives support in the data. We perform three tests. The first relies on variation across industries in dependence of external capital needed for new investments. To measure industry dependence of external capital we follow Rajan and Zingales (1998) and Boucly et al. (2011) and calculate the difference between capital expenditure and gross cash flow divided by capital expenditures for all firms in a given industry and year. A high dependence of external capital to finance new investments is associated with a high ratio. We then take the average ratio for each industry over the whole period and define all industries with a ratio above the median as dependent on external capital.[28] We estimate separate difference-in-difference estimators for employees in industries with high dependence and for employees in industries with low dependence. The first two rows of Table XI display the result using the aggregated data. Consistent with the idea that a buyout relaxes financial constraints, employees in financially dependent industries are estimated to have a reduced unemployment risk of, on average, 1.6 percentage points in the post four-year period in contrast to employees in non-financially dependent industries for which we find no effect of the buyout on unemployment risk.

The second test is based on comparing divisional buyouts to other types of buyouts. Corporate divestitures involve taking parts of a larger firm out as a separate entity, so the parts bought out are less likely to be financially constrained prior to the buyout, due to internal capital markets. To investigate, we estimate a difference-in-difference model on aggregated data in accordance with Equation 2 where treatment is defined as being part of a corporate divestiture and control defined as being part of a non-corporate divestiture. The difference-in-difference estimate, row three in Table XI, shows a 1.8 percentage points or a 24% higher unemployment

[28] The main two industries classified as dependent on external capital are the manufacturing industry and the transport, storage and communication industry. These account for over 95% of the treated employees in all industries with a financial dependence ratio above the median.

Table XI: Heterogeneity in Financial Constraints

	Unemployment risk estimate	% change.
Financially dependent	-0.016**	-17.9%
	(0.004)	
Non-financially dependent	-0.006	-7.3%
	(0.003)	
Divestitures vs Non-Divestitures	0.018**	24.0%
	(0.004)	
Buyouts 1999-2000	-0.026**	-27.6%
	(0.006)	
Buyouts 2003-2004	-0.002	-2.5%
	(0.004)	
# Observations	11	

Notes. Statistical significance at the 5% and 1% level is denoted by * and **. The estimates for business cycle effects measures the average two years post effect based on 9 observations.

risk for employees in corporate divestitures relative to employees in non-corporate divestitures. A relative decrease in unemployment risk for non-corporate divestitures is consistent with unemployment risk decreasing because of relaxed financial constraints.

The third test relies on studying how the effects on employees vary across the business cycle. If a buyout relaxes financial constraints, it should make firms more likely to be able to withstand negative economic shocks because of easier access to capital injections from the private equity fund and the financial engineering expertize private equity firms possess. Our sample covers the IT stock market crash in 2001 that hit the Swedish economy hard. We divided our sample of buyouts into buyouts taking place before and after 2001. The final two rows of table XI display difference-in-difference estimates for unemployment risk for buyouts undertaken between 1999-2000 and 2003-2004 (for two periods after the buyout to ensure the first sample does not overlap the second) using aggregated data. We find no statistically significant effects for buyouts undertaken between 2003 and 2004, but statistically significant reductions in unemployment risk buyout between 1999 and 2000. These results are consistent with unemployment risk decreasing because of relaxed financial constraints.

5.2 Labor Market Regulations

While the financial constraints story is consistent with the data, we fail to find evidence that labor market regulations restrict private equity firms from laying off employees. We undertake three tests based on variation in employment protection across employees. The idea is that if labor market regulations are indeed an obstacle for private equity firms, they would seek to avoid them by concentrating layoffs to employees with weaker protections.

The first test is based on that employment protections in Sweden are increasing with tenure due to a "first-in-last-out" seniority rule. We can thus compare the effect of the buyout on employees with shorter tenure to those with longer tenure in the firm. We estimate Equation 2 using aggregated data separately for employees with three or more years of tenure at their place of work and for employees with less than two years of tenure. The first two rows of

Table XII: Heterogeneity in Labor Market Protections

	Unemployment risk estimate	% change.
Tenure > 2 years	-0.008*	-17.0%
	(0.003)	
Tenure < 3 years	-0.013**	-11.1%
	(0.004)	
Employed for three years	-0.005	
	(0.003)	
Not employed for three years	-0.041**	-14.8%
	(0.007)	
Age<30	-0.031**	-22.8%
	(0.006)	
Age>29	-0.005	
	(0.004)	
# Observations	11	11

Notes. Statistical significance at the 5% and 1% level is denoted by * and **.

Table XII present our estimates. For both samples we find a statistically significant decrease in unemployment risk, and the effect is slightly stronger for employees with shorter tenure.

The second test is based on variation in employment protection in Sweden that stems from the fact that employees on temporary contracts are protected less by laws than those on permanent contracts. Though we do not observe the types of employment contracts employees have, employees on temporary contracts are more likely to jump in and out of unemployment. Employees with unemployment days in the years prior to the buyout are then more likely to be on a temporary contract than employees that have not been unemployed prior to the buyout. The middle two rows of Table XII display the results of estimating Equation 2 using aggregated data for employees with at least one day of unemployment during the period $t-2$ to t and those that had no unemployment during the same period. We only estimate statistically significant declines in unemployment risk for employees with an unemployment history, suggesting that the buyout benefits employees with weaker employment protections to a greater extent.

As a third test for whether employment protection is important for how buyouts affect employees, we estimate separate effects for young and old employees. OECD reports that 41.3% of employees between 15 and 24 years old had temporary contracts in 2000 (OECD, 2002). The same figure for the group of employees aged 25 to 54 was 10.5%. The relative softer protection for younger individuals should make them more sensitive to cost cutting through downsizing of the workforce. We divide our sample into employees older and younger than 30 years when the buyout was announced. The last two rows of Table XII show estimates from using aggregated data. Most of the reduction in unemployment risk can be attributed to younger employees.

In sum, these tests suggest that labor market regulations do not restrict private equity firms from laying off employees. The minor role played by labor market regulations is in line with Boucly et al. (2011), who compares industries with different labor law rigidities and find no support that strong French labor market regulations affect their results of greater firm level employment growth after the buyout. But as they underscore, labor market regulations are likely

to restrict the types of buyouts that can be undertaken. In our setting, that would entail biasing buyouts towards only those types of buyouts that do not involve layoffs leading to unemployment. But even if labor market regulations are important for the effects on employees, our results say something about the effects on employees in countries with similar levels of employment protections as in Sweden. When OECD ranked the overall employment protections in member countries and other selected non-OECD countries in 2004, Sweden was ranked as having the seventh strongest protection among a total of 30 countries (OECD, 2004). Countries such as France, Germany, Netherlands, Italy, Spain and Finland are all indexed as having an overall employment protection in parity with Sweden's. In total, countries with similar strengths in employment protections as Sweden account for around 20% of all buyouts undertaken worldwide between 1998 and 2004 according to the Capital IQ database.

6 Conclusion

Using data from Sweden, we show that employees benefit from buyouts by experiencing a reduction in unemployment risk and an increase in labor income. These benefits pertain to employees across most quartiles of the labor income distribution. Moving up from the individual level to the establishment and the firm level, we show that the effect on firm and establishment level employment growth is surprisingly similar to those observed in the US and the UK. The reduction in unemployment risk is mostly concentrated to industries dependent on external capital to grow, non-divisional buyouts in which the target firm is more likely to be financially constrained, and buyouts undertaken just prior to the economic slowdown following the IT stock market crash in 2001. We find no evidence that labor market regulations restrict private equity firms from laying off employees. These findings are consistent with the hypothesis that the potenital negative effects of buyouts on employees, from cost cutting and higher debt load, are outweighed by the positive effects of relaxing financial constraints at the firm level.

Given these findings, concerns that private equity firms operating in Sweden have generated returns for investors at the expense of employees are unwarranted. Our analysis also suggests that caution should be exercised in interpreting declines in firm and establishment level employment growth observed in the UK and the US as detrimental to individual employees. Moreover, the connection between unemployment risk and access to capital we establish suggests more generally that policies designed to relax firms' financial constraints could trickle down to benefit individual employees by reducing transitions to unemployment.

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