Do Leveraged Buyouts Lead to Unemployment for Workers? Evidence from Matched Employer-Employee Data

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Abstract

Using matched employer-employee data from Sweden we study the effects of leveraged buyouts on workers. Workers are not more likely to become unemployed after a leveraged buyout relative to workers in similar non-acquired firms. The lack of negative effects on workers is robust to various measures of unemployment, control groups, and alternative worker outcomes. Moreover, unemployment incidence is 20% lower relative to unemployment incidence for workers in firms acquired by strategic non-financial buyers. Some workers, however, have more to fear than others: white-collar workers and older workers have higher unemployment incidence relative to blue-collar workers and younger workers.

Keywords: Acquisitions, Employment, LBO, Private Equity, Restructuring, Unemployment.
JEL Codes: G32, G34, J60.

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1 Introduction

Leveraged buyouts (LBOs) are common across the world. LBOs are undertaken by private equity firms who buy, improve, and resell mature firms using capital invested in private equity funds. Between 1985 and 2006, private equity firms bought corporate assets in the US at a rate of around 1% of the total US stock market value. Between 2004 and 2007, over 8 500 LBOs were undertaken world wide (Kaplan and Strömberg, 2009; Strömberg, 2008).

The spread of the LBO business model has not escaped criticism. In the wake of the financial crisis in Europe, labor unions have claimed that LBOs, through systematic layoffs and wage cuts, generate returns to investors at the expense of workers.[1] The debate also stirred in the US in 2012, as the Republican Party presidential candidate Mitt Romney was a former private equity executive.[2] Claims of layoffs should be taken seriously. Workers entering unemployment often leads to, among other things, wage cuts after accepting a new job offer, consumption reductions, as well as declines in happiness.[3] Given the size of the private equity industry and the extensive media coverage it has generated, any evidence on how individual workers fare in the wake of LBOs is of general interest.

This paper provides such evidence using rich matched employer-employee data and a sample of 194 LBOs undertaken in Sweden during 1998-2004.[4] Using a difference-in-difference estimator (DiD), we compare unemployment incidence for workers for eight years around the announcement of the LBO relative to workers in similar non-acquired firms and to workers in firms acquired by non-financial strategic buyers. We make three contributions to the literature.

First, our paper studies the effects of LBOs on individual workers using matched employer-employee data. We find no evidence that unemployment incidence increases on average after a leveraged buyout relative to unemployment incidence in similar non-acquired firms. This result is robust to various measures of unemployment and control groups and we find no negative effects on income or job-to-job

[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] See, e.g.,Farber (2005), Katz and Mayer (1990), Jacobson, LaLonde, and Sullivan (1993), Gruber (1997) and Di Tella, MacCullock, and Oswald (2001).

[4] Using data from Sweden has numerous benefits. First, detailed matched employer-employee data and full accounting information on firms is available for the universe of workers and firms. Access to accounting data on all firms allows the creation of control groups of workers in firms that either underwent an acquisition by a strategic non-financial buyer or that are similar on observable characteristics to LBO targets. Detailed demographic information on workers such as age, gender, location, and income means we can control for observable current and historic worker heterogeneity in our analyses. Second, individual worker data is available for a long time period (1990 to 2008) so we can examine pre- and post-trends in unemployment incidence for several years around the LBO. Finally, Sweden has a very active private equity market with up to a third of all LBOs undertaken by foreign private equity firms which ensures that the sample of LBOs we gather is sufficiently large for statistical inference and that our results are not specific to activities by domestic private equity firms.

transitions either. The lack of effects on workers is mirrored in a lack of relative short run (two year) effects on firm level variables such as firm size, fixed assets, sales, and value added per worker.

Second, we document that a leveraged buyout is substantially better for workers than an acquisition by a strategic non-financial buyer. On average, unemployment incidence decreases by 2.2 percentage points or 20% when comparing workers in LBO targets to workers in firms acquired by non-financial buyers. At the firm level and in relative terms, LBO targets expand in size, number of establishments, fixed assets and in sales. Establishments increase in size and hirings and separations decrease. A plausible explanation for these effects is that strategic acquirers already have assets in the market which causes reorganization to the detriment of the acquired firm's workers.

Finally, we directly evaluate if some subgroups of workers have relatively more to fear from LBOs than others. White-collar workers and workers older than 30 years are more likely to become unemployed relative blue-collar workers and younger workers, but they are not more likely to become unemployed in absolute terms. A plausible explanation for these results is that they relate to the substitution of monitoring for incentive contracts post-LBO. Since white-collar workers and older workers are more likely to undertake monitoring activities, these are more likely to become redundant. Additionally, white-collar workers and older workers have jobs where firm-specific human capital, career concerns and internal labor markets are more important. Finding a new job can then take longer, which increases unemployment incidence given a separation. We do not, however, find any evidence of higher unemployment incidence post-LBO for (i) workers in firms more likely to be in financial distress (low cash, low ROA, and high leverage); (ii) older more tentured workers as a result of breach of implicit contracts; or (iii) workers easier to fire because of weaker employment protection.

The main econometric concern is selection: firms targeted for LBOs are not selected at random. The DiD approach combined with detailed data provides an opportunity to overcome selection bias. We can account for imbalances between treated and control workers in observable individual characteristics and firm characteristics, unobserved time invariant group effects, as well as common time effects, industry effects and region effects. Although it is difficult to evaluate selection on unobservables, three pieces of evidence suggest unobservables are a minor concern. First, private equity firms seem to select targets on firm characteristics, not worker characteristics. Compared to a random sample of firms, LBO targets are different in observable firms characteristics but workers in LBO targets are not. Second, the treated and control workers have parallel trends in unemployment incidence prior to the announcement of the LBO. Parallel pre-trends suggests that shocks in past has affected the workers in a similar way and thus potentially will do so in the future. Finally, our results are robust to alternative outcome variables:

analyzing total days of unemployment, worker income, and following workers over a full eight years post-LBO gives similar results.

These results are important, because what economists currently know about the effects of LBOs on workers has to be inferred from studies of employment at the firm or establishment level. Academic evidence from the US suggest modest declines in establishments' employment growth where lower growth among existing firms is offset by the creation of new establishments (Davis, Haltiwanger, Jarmin, Lerner, and Miranda, 2011).[5] Evidence from the UK suggest declines in firm level employment growth, but the effect appears to be weaker for more recent LBOs than for LBOs in the 1980s (Wright, Thompson, and Robbie, 1992; Amess and Wright, 2007, 2012). There is no sign of employment growth in Sweden (Bergström et al., 2007), but in France LBOs seem to provide capital to credit constrained firms and thereby spur firm level employment (Boucly, Sraer, and Thesmar, 2011). While these studies have been important for enhancing our understanding of how LBOs affect employment, data limitations have prevented them from pinning down the effects on individual workers—an issue central to the media debate. For example, declines in employment growth could come from involuntary layoffs (hurting workers) or from natural attrition and reductions in hirings (not hurting workers). And increases in employment growth could come entirely from new hirings, while old workers are involuntarily laid off. Focus on individual workers and access to matched employer-employee data allows us to avoid these concerns and provide the first direct evidence of the effects of LBOs on workers.

The rest of the paper is organized as follows. Section 2 describes our data, sample construction, and empirical strategy. Our main results are laid out in Section 3. We provide additional robustness checks in Section 4, and offer concluding remarks in Section 5.

2 Data and empirical strategy

2.1 Data

We study LBOs in Sweden between 1999 and 2005 using detailed matched employer-employee data obtained from Statistics Sweden. The Swedish private equity market developed in the early 1990s as a result of considerable deregulation of the financial markets. Most of the large domestic private equity firms were founded by investment bankers with experience from the US financial markets in the late 1980s. In addition to domestic private equity firms, the Swedish private equity market has a substantial presence of foreign private equity firms. Around one third of all LBOs are undertaken by foreign private

[5]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).

equity firms. According to the Swedish Venture Capital Association, investments undertaken by private equity firms in Sweden amounted to around 0.7% of GDP in 2008. In terms of investment to GDP, the private equity market in Sweden was the second largest in Europe next to the UK market.

We use two sources of information on LBOs: the Capital IQ database and LBOs identified in Bergström et al. (2007). Our starting point is transactions in the Capital IQ database where the targets' geographical location is Sweden and the announcement date is between 1999 and 2005.[6] 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".[7] We then keep transactions marked as "Closed" or "Effective" and remove transactions involving minority stakes or which are private investments in public equity.[8] To this list we add LBOs identified in Bergström et al. (2007) that Capital IQ did not record (39 transactions) providing us with a sample of 322 LBOs.[9]

To match the LBOs to workers, we must find the legal firm registration numbers of the target for each LBO. We use the IFN Corporate Database containing information on names and the registration number for all firms in Sweden to add firm IDs to the LBOs. 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 253 LBOs 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. After obtaining firm registration numbers for the targets, we correct for the corporate group structure of these firms. To correct for corporate group structure is important, because an LBO often take place in holding company with majority ownership in several other firms (who in turn can own other firms). If we do not correct for the corporate group structure, a LBO would show up as affecting zero workers when all workers are

[6]We start from 1999 because we have firm level information from 1998 onwards. We end in 2005 because our matched employer-employee data ends in 2008 and we want sufficient post-periods of worker information.

[7]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). "

[8]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.

[9]Bergström et al. (2007) describe their sample of LBOs 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 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).

registered as working in firms owned by the holding company. We use the IFN Corporate database to mark firms as being part of an LBO if they were directly or indirectly majority owned by the targeted firm. If there are two LBOs in the same firm registration number in a given year, we drop the second making our sample unique on firm and year.[10]

With the firm registration numbers we can identify workers affected by LBOs in the LISA database, available from Statistics Sweden.[11] The LISA database covers the population over 16 years of age in Sweden from 1990 to 2008 and links workers to employers. The yearly variables we collect from the LISA database are age, sex, highest attained education level, the firm registration number for a worker's main source of income, establishment identifier for the worker's main source of income, labor income, capital income, registered number of days in unemployment, and the establishment's industry code, municipality and geographic location.[12] We keep workers aged 20 to 63 to ensure we have at least four pre and four post periods of worker level information. For each LBO, we match the LBO's announced year with the previous year's firm and worker information. We match with one year lag to ensure that all firms and workers are untreated as of time zero ($t = 0$). In the merge to the worker level data we lose 61 LBOs (we go from 255 to 194). These are firm registration numbers which have no workers reporting that firm registration number as their main source of income. This could be because the firm has operations in Sweden but is not incorporated here or because the LBO involves taking a part of a firm private (a divestiture) and that the previous part of the firm had no firm registration number prior to the LBO.

The final sample of LBOs is summarized in Table I. We end up with 194 LBOs affecting 547 firm registration numbers, 2 016 establishments and 67 617 workers between 1998 and 2004 (one year prior to the LBO announcement year).[13] The average LBO affects 350 workers in 2.82 firm registration numbers. The number of LBOs per year increases generally over time. Table II shows that the sample of 194 LBOs for which we can identify workers (Column 1) corresponds well to the total sample of 253 LBOs in Sweden registered in the Capital IQ database for which we could find a firm identifier (Column 2); both

[10]Multiple annual accounts can be submitted by firms each year if, for example, the length of the accounting year is changed or if firms wish to make corrections to their previously submitted annual accounts. We always use the latest annual account submitted in a given year. Note also that the adjustment for the group structure implies that in some cases we potentially include "too many" firms as being part of a LBO because a LBO taking place the next year may exclude some non-treated subsidiaries if the firm is split up at the time of the LBO. This biases our results towards zero.

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

[12]We also calculate the tenure of workers in the firm. Tenure goes back to 1990, so years of tenure at the firm is truncated at 8 for LBOs in 1998 and 14 for LBOs in 2004. The age of the firm is based on the date of registration at Statistics Sweden and goes back to 1970.

[13]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).

Table I: The sample of LBOs

	LBOs	Firms	Establishments	Individuals
1998	20	76	190	7 874
1999	22	51	141	5 742
2000	31	120	284	11 393
2001	26	43	153	6 825
2002	24	46	126	3 987
2003	31	78	470	13 164
2004	40	133	652	18 632
Sum	194	547	2 016	67 617

Notes. This table displays how the sample of LBOs used in the analysis are spread out over time.

average transaction value and the distribution of transaction types are similar. Corporate divestitures, cross border LBOs, and management buyouts (MBOs) account for the bulk of all LBOs. For the period 1998 to 2004, Sweden ranked ninth in the world with 1.7% of all LBOs in the Capital IQ database. Comparing Column 1 with Columns 3-5 illustrates that the average transaction value for LBOs in our sample is smaller than in the US, but larger than in the U.K and France. Sweden has more corporate divestitures and cross-border LBOs, but fewer MBOs.

2.2 Empirical strategy

Our econometric strategy is based on a difference-in-difference estimator comparing unemployment incidence between a treated and control group of workers. Combined with rich firm and worker level data, the DiD estimator allows us to control for potential imbalances between the treatment group and the control group in observable individual characteristics, observable firm characteristics, unobserved time invariant group effects, as well as common time, industry and region effects.[14] We model unemployment incidence as

$$Y_{ifgt} = \alpha + \lambda_t + \gamma LBO_g + \beta(Post_t \times LBO_g) + \phi X_{ifgt} + \pi F_{fgt} + \varepsilon_{ifgt}, \tag{1}$$

where Y_{ifgt} is a dummy measuring whether worker i in firm f in group g was officially registered as unemployed for at least one day at time t. Time effects are represented by λ_t, individual covariates by X_{ifgt} and firm covariates by F_{fgt}. To estimate Equation 1 we normalize time such that year zero ($t = 0$)

[14]When concentrating on workers employed at the year prior to the LBO, the composition of the treatment group and control group is constant over time so we need not worry about selection effects from shutdowns or changes in hiring and separation practices induced by the LBO. However, we can expect any LBO effect to fade over time since worker turnover means that fewer treated workers actually remain in the LBO targeted firm over time.

Table II: Comparison to the Capital IQ sample

	Sweden		Foreign		
	Sample	Full	US	UK	France
	(1)	(2)	(3)	(4)	(5)
	A. Transaction types				
Going Private	2.6%	2.7%	5.7%	6.6%	2.5%
Corporate Divestiture	33.0%	33.6%	28.1%	31.8%	16.4%
Secondary Buyout	4.1%	5.5%	4.0%	5.2%	6.3%
Bankruptcy Sale	0%	0.4%	2.8%	1.8%	0.8%
Management Buyout	24.7%	25.3%	30.6%	71.8%	45.6%
Family Succession	3.1%	2.3%	1.7%	1.8%	3.0%
Cross Border	32.0%	32.4%	4.8%	10.3%	31.1%
Platform	2.6%	4.7%	9.1%	3.1%	3.9%
	B. Transaction value				
Mean ($mm 31.12.08)	201.6	206.1	267.5	106.2	206.8
Standard deviation	(401.5)	(379.1)	(3123.7)	(479.8)	(535.2)
Observations	194	253	4958	2210	881

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

is the year prior to when the LBO was announced.[15] The interaction term, $Post_t \times LBO_g$, takes the value one for all years after the LBO annoucement for the treated group and zero otherwise. The coefficient of interest, β, captures the local average treatment effect for workers employed in an LBO target the year before the announcement year (since LBO_g does not vary over time). The identifying assumption is that the treatment group and the control group have parallel trends in the absence of treatment conditional on X_{ifgt} and F_{fgt}.

When measuring differential effects between subgroups of treated and control workers, we extend the above model to a difference-in-difference-in-difference (DiDiD) model by adding the following terms:

$$\rho D_{gq} + \nu(LBO_g \times D_{gq}) + \omega(Post_t \times D_{gq}) + \theta(Post_t \times LBO_g \times D_{gq}) \tag{2}$$

The DiDiD model introduces a third difference between groups of workers (indicated by D_{gq}). The DiDiD estimate θ gives the marginal treatment effect between the two groups.

[15] Normalization of time has the advantage that aggregated trends that potentially affect treated and controls differently are less likely to influence the estimates.

2.3 Construction of control groups

We assemble two control groups of workers. The acquired controls are workers in firms acquired by non-financial strategic buyers and the matched controls are workers in comparable non-acquired firms. We compare to workers in firms acquired by non-financial strategic buyers for two closely related reasons. First, these are firms that we know were up for sale—an important unobservable firm level characteristic. Because Sweden is a rather small country, it is likely that private equity firms bid or considered bidding for these firms which makes workers in these firms an appealing control group. Second, the comparison group is interesting in itself because being acquired by a strategic industry buyer is a realistic alternative to a leveraged buyout.

To obtain the acquired controls, we collect information on mergers and acquisitions from the commercial data base Zephyr. We start with all mergers and acquisitions in which the target's location is Sweden, the transaction was announced between 1999 and 2005, the acquisition was a majority acquisition, and the acquisition was not an acquisition of a division or a department. There are 2 495 such transactions. We then match the name of the acquired firm with the name and registration numbers in the IFN Corporate Database to obtain firm registration numbers. We find 1 798 transactions in which we are positive that the firm registration number is the correct one. As with our sample of LBOs, we use the IFN Corporate Database to adjust for the corporate group structure of firms, we match an acquisition's announced date with last year's firm and worker information to ensure that firms and workers are untreated at $t = 0$, and we only treat a firm as acquired the first time it is acquired (in our sample). Finally, we remove 186 acquisitions we previously identified as LBOs.[16]

To obtain the matched controls, we start with the universe of firms in Sweden with more than ten employees and proceed in three stages. In stage one, for each year we take the minimum and maximum value of our LBO targets in terms of firm size, value added, sales, fixed assets, value added per employee, return on assets and debt to assets ratio. We narrow the pool of potential matches by excluding all potential control firms with values above 1.25 times the maximum value and below 0.75 times the minimum value for firm size, sales, fixed assets and debt to assets ratio of the LBO targets. For value added, value added per employee, and return on assets—variables that can take on negative values—we exclude control firms with 1.25 times the maximum and minimum of the LBO targets. In stage two, following Davis et al. (2011) and Boucly et al. (2011), we create cells containing treated and control firms based on

[16]That is, 34% of the LBO targets we identify from Capital IQ and Bergström et al. (2007) are also present in the Zephyr database. Our definition of a "strategic acquisition" is thus a majority acquisition in the Zephyr database that is not an LBO.

- number of employees in bins of 10 to 20, 21 to 50, 51 to 100, 101 to 250, 251 to 500, 501 to 750, 751 to 1000, 1001 to 1500, 1501 to 2000, 2001 to 2500, 2501 to 3500 and 3501 or greater.
- the location of the firm (located in the north of Sweden or not).
- firm age in bins of 0 to 5 years, 6 to 10 years, 11 to 50 years and older than 50 years.
- ROA (return on assets) in four bins consisting of above and below the median ROA for all firms with positive ROA and above and below median ROA for all firms with negative ROA. The median for firms with positive ROA is 10.2% and the median for firms with negative ROA is -8.4%
- industry classification of the firm (17 categories).

We match on characteristics in the year prior to the LBO announcement and ensure that control firms never appear as acquired or LBO targets. If there are multiple control firms for each treated firm in a cell, we randomly drop controls to obtain a one-to-one match. With these narrow cells we match 87% of the treated firms. In stage three, we perform a second match for unmatched firms from the first round, now with broader cells based on firm size bins as in the first round and a 17 digit industry classification of the firm. In total, we find matches for 399 firms affecting 57 799 workers.

2.4 Summary statistics

Table III presents average individual and firm level characteristics for the treated and control groups in the acquired and in the matched sample (Columns 1-2 and 4-5) at $t = 0$. Column 7 compares to a random sample of Swedish firms (Column 7).[17] Private equity firms do not appear to select LBO targets on observable worker characteristics. In Panel A, Columns 8 and 9 reveal that workers in LBO targets are similar to the average worker in Sweden in terms of age, skill level, gender, and unemployment history. They do, however, appear to have slightly higher labor income and shorter tenure. Unsurprisingly, private equity firms do select targets on observable firm characteristics. LBO targets are larger, older, and have higher value added per employee, fixed assets and sales than an average Swedish firm (Panel B). However, the geographical location, debt to asset ratio, and return on assets do not differ much.

These patterns underscore the need for control groups that account for firm level characteristics (more than for worker level characteristics).[18] Indeed, for the acquired controls, Column 3 illustrates

[17] The random sample is weighted across years so we obtain the same distribution over time as the LBO targets. For each LBO target, we randomly pick five firms with non missing information on size, value added per employee, fixed assets, sales, return on assets, and debt to assets.

[18] In our earlier working paper, Olsson and Tåg (2012), we matched at the worker level omitting firm level characteristics. We obtained slightly different results: decreases in unemployment incidence and increases in labor income post-LBO relative

Table III: Characteristics of the samples

	Acquired sample			**Matched sample**			**Random sample**		
	LBOs	Acquired	T-test (1)-(2)	LBOs	Matched	T-test (4)-(5)	Sample	T-test (1)-(7)	T-test (4)-(7)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Worker characteristics									
No. workers	67 617	233 008		57 799	57 161		39 261		
Age	39.96	40,58	-0.13	40.08	39.86	0.04	41.22	-0.25	-0.23
Share high skilled	0.20	0.17	0.03	0.21	0.21	-0.00	0.12	0.09	0.10
Female	0.44	0.30	0.15	0.41	0.44	-0.03	0.26	0.19	0.15
Tenure	3.11	3.74	-0.23	3.16	3.74	-0.21	3.89	**-0.29**	**-0.27**
Labor income	241.91	250.01	**-0.45**	249.21	233.22	**0.91**	236.62	**0.29**	**0.71**
Unemployment history	0.54	0.54	0.01	0.52	0.54	-0.02	0.78	-0.15	-0.16
B. Firm characteristics									
No. firms	547	2 651		399	399		2545		
Size	124.97	89.79	**1.30**	145.75	144.05	0.07	15.44	**4.61**	**5.55**
Age	22.96	19.56	**0.52**	25.16	25.06	0.02	14.31	**1.46**	**1.80**
Share located in north	0.10	0.11	-0.01	0.10	0.10	0.00	0.13	-0.04	-0.04
Value added/employee	608.41	914.61	**-2.86**	639.76	517.57	**3.35**	422.68	**4.39**	**5.29**
Fixed assets	43 196.32	68 202.99	**-28.06**	53 662.09	33 456.68	**33.17**	6 654.07	**67.91**	**82.97**
Sales	206 861.00	187 673.00	**13.09**	256 139.69	208 088.04	**43.89**	28 638.29	**173.06**	**212.94**
Debt to total assets	0.63	0.77	-0.07	0.62	0.67	0.07	0.63	-0.00	0.05
Return on total assets	0.03	-0.21	0.11	0.10	0.09	-0.01	0.04	-0.01	0.05

Notes. The table displays means for various firm and worker level characteristics of the samples. Columns (3), (6), (8) and (9) present normalized t-tests of differences in means. Normalized t-values above 0.25 are bolded. All monetary values are in thousands of 2005 SEK. High skilled workers are workers with more than 2 years of undergraduate studies. Labor income is total gross labor income for the full year. Unemployment history takes values from zero to five and counts unemployment incidence for the four preceding year (for example, a worker unemployed at $t = -2$ and $t = -1$ would have a score of 2 while a worker never unemployed during the last four years would have a score of zero).

that differences in tenure vanish and differences in firm size, age, value added per employee, fixed assets, and sales narrow. Workers in acquired firms do, however, tend to have higher labor income than workers in LBO targets. The matched controls are even more similar to the LBO targets. Column (6) shows no differences in firm size and age, but the matched controls are slightly smaller in terms of value added per employee, fixed assets, and sales.[19] The control groups are not perfect, so we make sure to include controls of observable firm and worker level characteristics in all our regressions. Moreover, the DiD approach will take care of observed and unobserved time invariant group effects and common time, industry and region effects. The differences between the treated and control groups we observe in Table III are problematic only if they indicate the presence of differences in time-variant unobservables that correlate with unemployment incidence. However, as we show below, workers in the treated and control groups have parallel trends in unemployment incidence prior to the LBO announcement so unobserved factors have at least in the past affected the treated and control groups in a similar way.

3 Analysis

3.1 Baseline estimates

We start with the baseline estimates. The right subfigure of Figure I plots the yearly share of workers having at least one day of unemployment in the treated and the acquired control groups. The left subfigure gives the same plot relative matched controls. The share of workers reported as unemployed decreases up until $t = 0$ and then starts to increase in both subfigures. The decrease up until $t = 0$ is a result of all workers having a job at $t = 0$ (but some might still have had days in unemployment during the year). 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 periods before treatment makes a common pattern after treatment more likely. Both treatment and control groups have parallel pre-tends. Post-LBO, unemployment incidence is consistently lower relative acquired controls but not relative matched controls.

Column 1 in Table IV show that the decrease relative acquired controls corresponds to a 1 percentage point reduction in unemployment incidence. Relative matched controls (Column 4), we find no statisti-

matched controls. A drawback of matching at the worker level is that the control workers come from different firms. Relating changes in firm or establishment level outcomes (such as hirings, separations, sales size and fixed asset growth) to changes in workers level outcomes is then impossible since new controls have to be found for the establishment and firm level analysis.

[19] The smaller average size of the firms is partially an artifact of our matching strategy. Because we randomly drop controls within each cell, we on average select smaller controls since the size distribution within each cell is left-skewed as a result of the whole size distribution of firms in Sweden being left-skewed. We could include value added per employee, fixed assets, and sales in the matching procedure to make these differences smaller but these three variables are highly correlated with size and age. Adding more bins comes at the cost of finding fewer matches for the LBO targets.

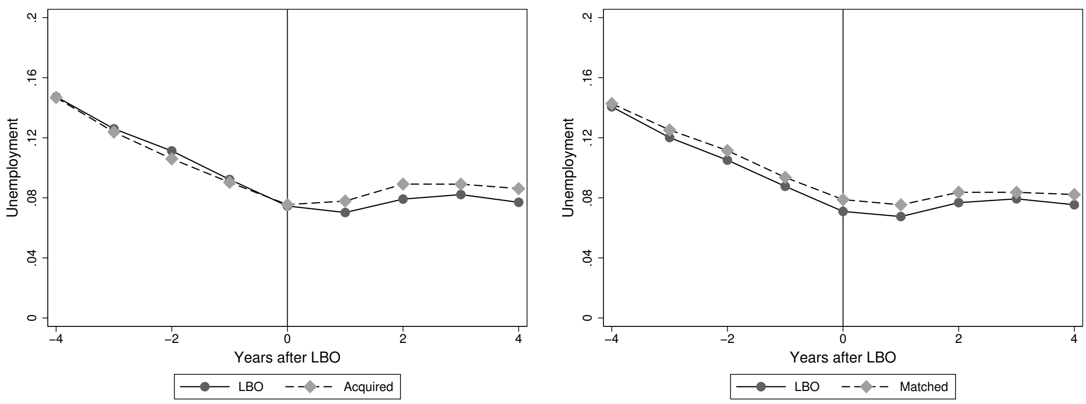

Figure I: Effect on unemployment incidence

Notes. These figures display the share of unemployed workers in LBO firms and control firms (at $t = 0$) for normalized time around the LBO. The left figure uses acquired firms as controls and the right figure the matched firms. We observe a declining unemployment before time zero and an increasing unemployment after time zero because all workers are employed at the time zero by construction. At time zero, unemployment is non-zero as some workers have been unemployed during year zero.

cally significant changes in unemployment incidence. If our control groups account for selection effects sufficiently well, the inclusion of exogenous variables should not alter the estimated effect. When we add worker level controls (Columns 2 and 5) the estimates. However, adding controls for firm age, firm age squared and firm size in addition to country dummies, industry dummies and year dummies changes the estimate relative acquired controls. We now find a statistically significant decrease in unemployment incidence of 2.2 percentage points (Column 3). This confirms the finding in Table III that LBO targets are chosen based on firm—and not worker—characteristics. Relative to the average pre-period unemployment incidence for treated workers, the effect converts to a 20% decrease in unemployment incidence. The addition of individual and firm level controls does not change the estimate relative matched controls (Column 6).

3.2 Alternative outcomes

The baseline estimates suggest that LBOs are not followed by systematic layoffs leading to increased unemployment incidence for workers. The conclusion that workers do not fare worse after the LBO is robust to several alternative unemployment outcomes and measures of worker welfare.

First, the effects on the extensive margin of unemployment is similar to effects on the intensive margin. Estimates for the intensive margin of unemployment can be obtained from estimating several versions of the following model:

Table IV: Effect on unemployment incidence

	Acquired sample			Matched sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LBO	-0.010*	-0.010*	-0.022***	-0.001	-0.001	-0.001
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
LBO	0.002	-0.000	0.005	-0.005	-0.001	-0.001
	(0.009)	(0.003)	(0.003)	(0.0011)	(0.003)	(0.003)
Post	-0.023***	-0.023***	-0.013***	-0.029***	-0.029***	-0.026***
	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)	(0.005)
Individual controls	No	Yes	Yes	No	Yes	Yes
Firm controls	No	No	Yes	No	No	Yes
Industry dummies	No	No	Yes	No	No	Yes
County dummies	No	No	Yes	No	No	Yes
Year dummies	No	No	Yes	No	No	Yes
Observations	2 678 827	2 678 827	2 678 827	1 023 419	1 023 419	1 023 419
R^2	0.002	0.283	0.287	0.003	0.263	0.267

Notes. This table displays worker level difference-in-difference estimates of unemployment incidence. All regressions include a constant. Worker level controls X_{ifgt} include age, age squared, gender, skill level, tenure at the firm, labor income and five dummies for unemployment incidence from $t = -4$ to $t = 0$. Firm level controls F_{fgt} include size, age and age squared. Industry dummies are at the 2-digit SNI level and there are 20 county (in Swedish: "län") dummies. Standard error are clustered at the firm level as of $t = 0$.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

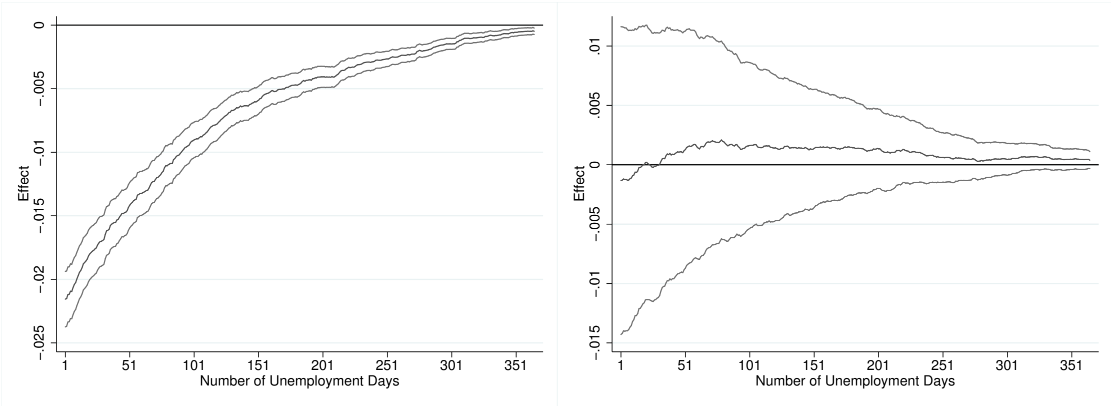

Figure II: Effect on number unemployment days

Notes. These figures display the point estimate and confidence intervals for running a difference-in-difference regression corresponding to Columns 3 (for the left figure) and 6 (for the right figure) in Table IV with the dependent variable (dummy for unemployed at year $t = 0$) defined as being registered for $s \in \{1,2,...,364\}$ unemployment days in year t.

$$P(Y_{ifgt} \geq s) = \alpha + \lambda_t + \gamma LBO_g + \beta(Post_t \times LBO_g) + \phi X_{ifgt} + \pi F_{fgt} + \varepsilon_{ifgt}, \tag{3}$$

The model is a linear probability model and the dependent variable indicates whether an unemployment spells exceed s days (for $s \in \{1,2,...,364\}$) or not. When s is one, β represents the DiD for the extensive margin (the baseline specification for the incidence). For $s > 1$, β represents the DiD on various levels of the intensive margin. Figure II displays DiD estimates for $s \in \{1,2,...,364\}$. Relative acquired controls, the decrease occurs both for the extensive margin and the intensive margin (the left subfigure) and is decreasing in absolute terms with s. Relative matched controls, however, we find no effect over the whole unemployment duration distribution. To get a more direct measure on how unemployment changes relative acquired controls, we can analyze total days of unemployment. Table V displays estimates based on Equation 1 but with total yearly days of unemployment as the outcome variable.[20] Relative acquired controls, total days of unemployment drops by 2.4 days per year (Column 3) while relative matched controls total days of unemployment are unaffected (Column 6).

Second, extending the post-period to a full eight years does not change the overall picture. Long-run negative effects on workers could be caused by resale of the LBO target or by reduced long-run investments as a result of the LBO.[21] Figure III replicates Figure I but it restricts attention to transactions undertaken in 1999-2001 ($t = 0$ in 1998-2000). Since our data ends in 2008, we can examine the post-period for eight years for these transations. The figure illustrates a transitory effect of the LBO relative

[20] In comparison to spell data, total days also includes observations with no unemployment days during a year.

[21] Although the concern that private equity firms sacrifice long-run investments for short term profit gains is often voiced in the media, there is little academic evidence that this occurs (see e.g. Ughetto (2010) and Lerner et al. (2011))

Table V: Effect on number unemployment days

	Acquired sample			Matched sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LBO	-0.553	-0.555	-2.398***	0.395	0.355	0.355
	(0.778)	(0.774)	(0.868)	(1.025)	(1.022)	(0.876)
LBO	-0.825	-0.804*	0.425***	-0.674	-0.353	-0.322
	(0.936)	(0.328)	(0.449)	(1.207)	(0.459)	(0.461)
Post	-0.839*	-0.801*	1.198*	-1.474*	-1.418*	-1.093
	(0.462)	(0.460)	(0.085)	(0.849)	(0.845)	(0.740)
Individual controls	No	Yes	Yes	No	Yes	Yes
Firm controls	No	No	Yes	No	No	Yes
Industry dummies	No	No	Yes	No	No	Yes
County dummies	No	No	Yes	No	No	Yes
Year dummies	No	No	Yes	No	No	Yes
Observations	2 678 827	2 678 827	2 678 827	1 023 419	1 023 419	1 023 419
R^2	0.000	0.174	0.180	0.000	0.162	0.168

Notes. This table displays worker level difference-in-difference estimates of unemployment incidence. All regressions include a constant. Worker level controls X_{ifgt} include age, age squared, gender, skill level, tenure at the firm, labor income and five dummies for unemployment incidence from $t = -4$ to $t = 0$. Firm level controls F_{fgt} include size, age and age squared. Industry dummies are at the 2-digit SNI level and there are 20 county (in Swedish: "län") dummies. Standard error are clustered at the firm level as of $t = 0$.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

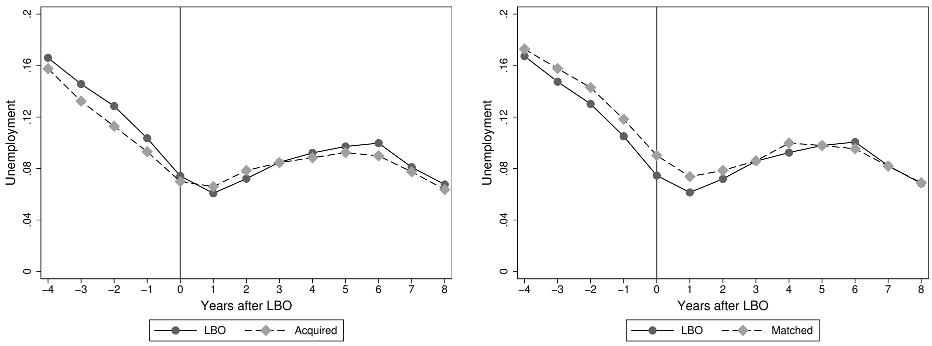

Figure III: Effect on unemployment incidence for 8 post years

Notes. These figures displays unemployment incidence for $t = -4$ to $t = 8$ for LBOs undertaken in 1999, 2000, and 2001.

Table VI: Effect on unemployment incidence for 8 post years

	Acquired sample	Matched sample		Acquired sample	Matched sample
	(1)	(2)		(3)	(4)
Effect $t+1$ to $t+4$	-0.013*	0.004	Effect $t+1$ to $t+8$	-0.007	0.008
	(0.008)	(0.012)		(0.009)	(0.013)
Observations	1 540 496	419 374	Observations	2 215 641	603 781

Notes. This table displays worker level difference-in-difference estimates of unemployment incidence per year. All regressions include a constant, worker level controls X_{ifgt} (age, age squared, gender, skill level, tenure at the firm, labor income and five dummies for unemployment incidence from $t = -4$ to $t = 0$), and firm level controls F_{fgt} (size, age and age squared). Industry dummies are at the 2-digit SNI level and there are 20 county (in Swedish: "län") dummies. Standard error are clustered at the firm level as of $t = 0$.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

acquired controls and shows a relative increase in unemployment incidence relative matched controls. The transitory nature of the effect is not surprising, as less workers remain with the LBO target as time progresses. The pattern of a slight relative increase over time is interesting: long-run effects could play a role. That long-run effects hurt workers does not, however, show up in the DiD estimates with full sets of firm and worker controls (Table VI). For four post periods (Columns 1 and 2), the results for transactions in 1999-2001 correspond to the results in Table IV (Columns 3 and 6) for the full sample with slightly lower reduction in unemployment incidence relative acquired controls (1.3 vs 2.2 percentage points). For the full eight year period, we find no statistically significant effects on unemployment incidence in any of the samples.

Third, relative to acquired controls, job-to-job relocations drop but not relative to matched controls. Job-to-job transitions is another measure of worker welfare, as relocations involve transaction costs borne by the worker. We measure job-to-job transitions by studying transitions of workers between

Table VII: Effect on job-to-job transitions

	Acquired sample			Matched sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LBO	-0.038***	-0.038***	-0.025***	-0.010	-0.010	-0.010
	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)
LBO	0.027**	0.015	0.007	0.023**	0.010	0.009
	(0.011)	(0.010)	(0.007)	(0.011)	(0.008)	(0.008)
Post	0.016***	0.015***	0.017*	-0.011	-0.011	-0.008
	(0.015)	(0.005)	(0.009)	(0.008)	(0.008)	(0.008)
Individual controls	No	Yes	Yes	No	Yes	Yes
Firm controls	No	No	Yes	No	No	Yes
Industry dummies	No	No	Yes	No	No	Yes
County dummies	No	No	Yes	No	No	Yes
Year dummies	No	No	Yes	No	No	Yes
Observations	2 385 915	2 385 915	2 385 915	912 153	912 153	912 153
R^2	0.001	0.039	0.055	0.001	0.044	0.054

Notes. This table displays worker level difference-in-difference estimates of transitions between jobs. All regressions include a constant. Worker level controls X_{ifgt} include age, age squared, gender, skill level, tenure at the firm, labor income and five dummies for unemployment incidence from $t = -3$ to $t = 0$. Firm level controls F_{fgt} include size, age and age squared. Industry dummies are at the 2-digit SNI level and there are 20 county (in Swedish: "län") dummies. Standard error are clustered at the firm level as of $t = 0$.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

establishments (the physical locations of the job where business, services or industrial operations are performed). A drop in job-to-job transitions relative acquired controls is apparent in Figure IV, which displays the share of workers in each sample relocating from one establishment to another between two years (previous and current). Table VII presents estimates of the DiD model in Equation 1 with an indicator variable for job-to-job transitions as the outcome variable. The decline relative acquired controls with full sets of individual and firm controls is 2.5 percentage points (Column 3), but job-to-job transitions relative matched controls remain unchanged (Column 6).

Finally, we find no evidence that wages are hit negatively by the LBO. Our data contains information on yearly capital and labor income of workers (from the tax records). We use these as proxies for wages. Consistent with capital gains arising as a result of selling shares in connection with an acquisition, the bottom two part of Figure V clearly shows spikes in capital income at $t = 1$ for workers who are part of an acquisition, but not for the matched controls. The uppermost part of the figure shows no clear differences in trends in labor income or capital income pre-LBO and no divergent trends post-LBO. DiD estimates with full sets of firm and worker controls confirm the visual evidence. Relative matched

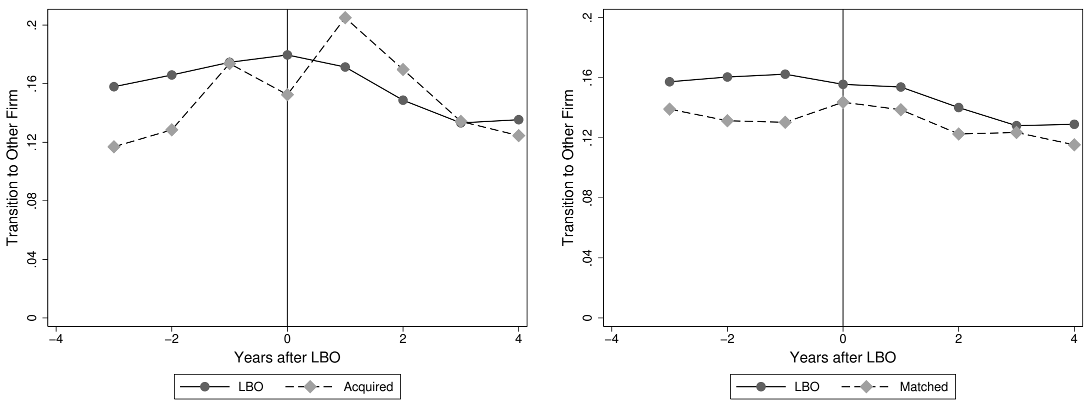

Figure IV: Effect on job-to-job transitions

Notes. These figures displays job-to-job transitions around the LBO announcements. The left figure is relative acquired controls and the right figure relative matched controls.

controls, Column 4 in Table VIII show that capital income relative matched controls increases by on average 12 875 SEK, but there are no effects on labor income (Column 3). Relative acquired controls, Columns 1 and 2 shows that both capital and labor income remains unchanged in relative terms.

3.3 Firm and establishment level outcomes

Next, we run several DiD regressions at the firm and establishment level to get a better picture of how changes in unemployment incidence relate to firm and establishment level changes post-LBO. The changes at the firm and establishment level are consistent with what we find at the worker level. For two years around the LBO announcement, we find no statistically significant changes on average relative the matched controls. The reduction in unemployment incidence relative acquired controls, however, can be explained by reorganization affecting workers in the control group. Strategic acquirers already have assets in the market and may thus have incentives to shift production away from the acquired firms to plants where the average production costs are lower. Combined with potentially overlapping overhead costs, workers in firms targeted for strategic acquisitions could face reduced demand for their labor. Consistent with this explanation, we find that relative acquired controls, LBO targets expand in size, fixed assets, and sales and we observe increases in number of establishments and declines in hirings and separation rates.

The firm level accounting data in our sample starts in 1998, so we restrict attention to LBOs announced between 2001 and 2005. We also restrict attention to two post-periods since, as pointed out by Davis et al. (2009), firms are difficult to track over time because of likely substantial reorganization after the LBO in terms of acquisition and divestitures and internal rearrangement of legal firm identifiers.

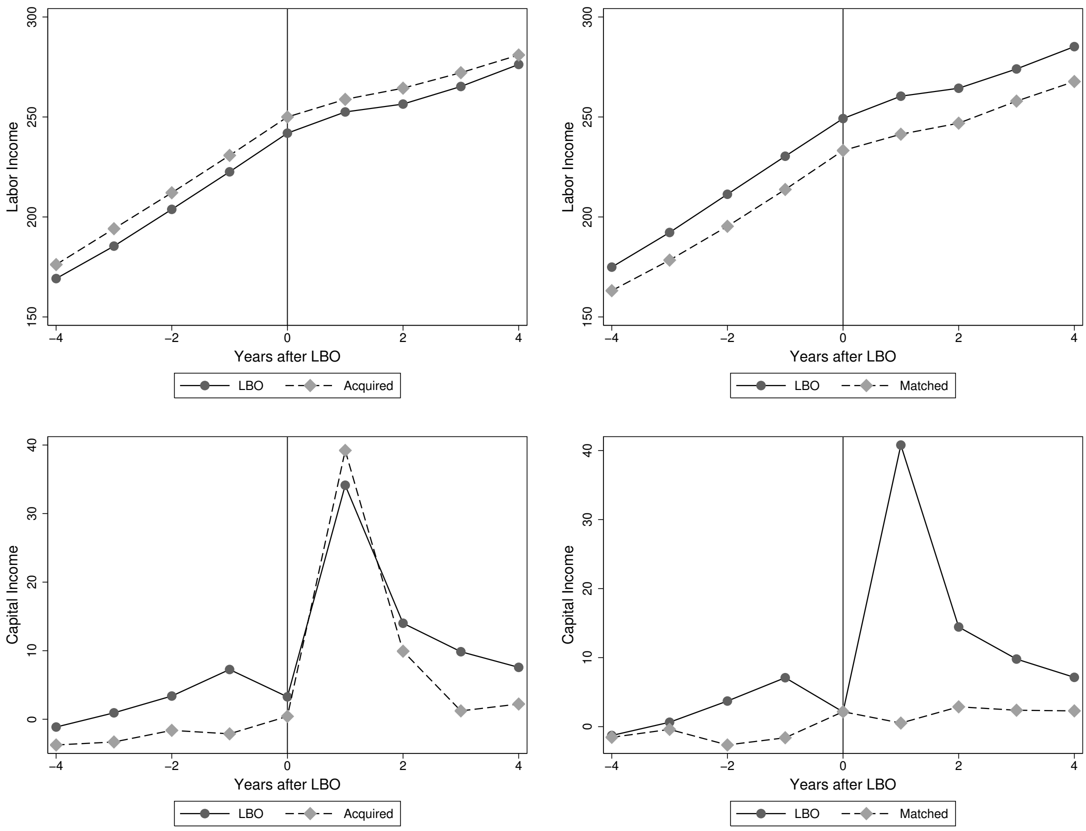

Figure V: Effect on labor and capital income

Notes. These figures display the labor and capital income of workers in LBO targeted firms and control firms for normalized time around the LBO. The left figures are relative to acquired firms and the right figure relative to matched firms.

Table VIII: Effect on labor and capital income

	Acquired sample		Matched sample	
	(1)	(2)	(3)	(4)
	Labor income	Capital income	Labor income	Capital income
Post*LBO	35.500	6.960	21.360	128.750***
	(28.080)	(42.280)	(34.000)	(41.650)
LBO	-13.690	-12.200	34.400	32.880
	(25.190)	(30.580)	(30.480)	(23.890)
Post	65.290***	177.650***	130.264***	41.650
	(2.3670)	(49.970)	(43.71)	(38.140)
Individual controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Industry dummies	Yes	Yes	Yes	Yes
County dummies	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Observations	2 678 827	2 678 827	1 023 419	1 023 419
R^2	0.252	0.001	0.282	0.002

Notes. This table displays worker level difference-in-difference estimates on yearly labor and capital earnings. All regressions include a constant. Worker level controls X_{ifgt} include age, age squared, gender, skill level, tenure at the firm, and five dummies for unemployment incidence from $t = -4$ to $t = 0$. Firm level controls F_{fgt} include size, age and age squared. Industry dummies are at the 2-digit SNI level and there are 20 county (in Swedish: "län") dummies. Tenure goes back to 1990, so years of tenure at the firm is truncated at 8 for LBOs in 1998 and 14 for LBOs in 2004. The age of the firm is based on the date of registration at Statistics Sweden and goes back to 1970. If a worker/establishment/firm is treated multiple times in our sample we include the worker/establishment/firm as treated only the first time of treatment. Standard error are clustered at the worker level.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

Moreover, the effect on unemployment incidence relative acquired controls that we estimate appears instantaneously after the LBO announcement. Any firm level explanations for the effect should thus also appear in the short run.

For firm and establishment level changes in growth, hiring, and separation *rates*, we follow Davis et al. (2009) and weigh observations to account for higher variance in employment growth in small firms. We measure weighted employment growth at time t for establishment/firm i as the net change in employment defined as

$$gw_{it} = \left(\frac{0.5(E_{it} + E_{it-1})}{\sum_i [0.5(E_{it} + E_{it-1})]} \right) g_{it} \tag{4}$$

where E_{it} is number of workers at establishment/firm i at time t and the growth rate at establishment/firm i at time t is

$$g_{it} = \frac{E_{it} - E_{it-1}}{0.5(E_{it} + E_{it-1})}. \tag{5}$$

The hiring and separation rates are weighted in a similar fashion with E_{it} corresponding to the number of new hires or the number of separated workers (since $t-1$) at i at time t.

Table IX presents worker level DiD estimates for unemployment incidence for the smaller sample of transactions from 2001 to 2005 (Column 1) and DiD estimates at the firm level for size, fixed assets, sales, value added per employee and return on assets (Columns 2-6). Column 1 in Panel A reveals that the estimated decrease in unemployment incidence relative acquired controls—a statistically significant 2 percentage points—is roughly in line with the estimate of 2.2 percentage points for the full sample. The estimated effects relative matched controls, Column 1 in Panel B, is still statistically not significantly different from zero. Column 2-6 in Panel A shows that relative to acquired controls, LBO targets expand in size, fixed assets, and sales, but productivity and profitability remains unchanged. Panel B for the same columns show that relative the matched controls, the LBO has little short run effects on firm level outcomes.

Next consider the changes in growth rates, hiring, and separation rates. Table X displays DiD estimates for firm size, growth, hiring rate, and separation the firm level (Columns 1-4) and at the establishment level (Columns 6-9). Column 5 displays DiD estimates for the number of establishments at the firm level. Panel A presents the results relative to acquired controls. Initially the size of the firm increases (Column 1), which goes hand in hand with a relative increase in the number of establishments post-LBO (Column 5). However, we do not find evidence of short run changes in employment growth, hirings, or separation rates at the firm level. At the establishment level we observe a decline in em-

Table IX: Effect on firm level outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment	**Log(Size)**	**Log(FA)**	**Log(Sales)**	**Log(VA/emp)**	**ROA**
	Panel A: Acquired sample					
Post*LBO	-0.020***	0.180***	0.523***	0.276***	0.104	0.037
	(0.006)	(0.054)	(0.117)	(0.100)	(0.089)	(0.051)
LBO	0.000	0.436***	0.221	0.255*	-0.026	0.025
	(0.002)	(0.083)	(0.150)	(0.143)	(0.096)	(0.034)
Post	0.006*	0.145***	-0.080	0.231***	-0.026	-0.130*
	(0.003)	(0.051)	(0.098)	(0.078)	(0.064)	(0.070)
Observations	846 417	8,582	8,285	8,284	8,056	8,275
R-squared	0.388	0.186	0.253	0.147	0.062	0.020
	Panel B: Matched sample					
Post*LBO	-0.009	-0.006	-0.128	0.087	0.063	0.009
	(0.006)	(0.048)	(0.098)	(0.080)	(0.072)	(0.026)
LBO	0.002	-0.024	-0.183	-0.122	-0.070	-0.014
	(0.002)	(0.096)	(0.168)	(0.142)	(0.085)	(0.020)
Post	-0.003	0.083	0.225*	0.218**	-0.017	-0.054***
	(0.004)	(0.073)	(0.123)	(0.101)	(0.059)	(0.018)
Observations	443 483	2 943	2 893	2 894	2 845	2 893
R-squared	0.355	0.158	0.328	0.147	0.066	0.068

Notes. This table displays, for $t = -2$ to $t = 2$, worker level difference-in-difference estimates for unemployment incidence and firm level difference-in-difference estimates for log size, log fixed assets, log sales, value added per employee and return on assets. All regressions include a constant. The specification for the estimates in Column (1) is the same as in Columns (3) and (6) in Table IV. The firm level specifications includes industry dummies are at the 2-digit SNI level, 20 county (in Swedish: "län") dummies, and year dummies. Standard error are clustered at the firm level.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

Table X: Effect on employment growth rates, hirings, and separations

	Firm level					Establishment level			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log(Size)	Growth	Hirings	Separations	No. est.	Log(Size)	Growth	Hirings	Separations
	Panel A: Acquired sample								
Post*LBO	0.180***	-0.023	-0.003	0.020	0.840**	0.046**	-0.003***	-0.008***	-0.005***
	(0.053)	(0.020)	(0.021)	(0.014)	(0.395)	(0.022)	(0.001)	(0.001)	(0.001)
LBO	0.436***	0.039***	0.079***	0.040***	1.401**	0.126***	0.004***	0.017***	0.013***
	(0.083)	(0.012)	(0.015)	(0.009)	(0.670)	(0.044)	(0.001)	(0.001)	(0.001)
Post	0.145***	-0.009	0.002	0.011	0.067	0.120***	-0.002***	0.003***	0.005***
	(0.051)	(0.006)	(0.008)	(0.006)	(0.368)	(0.027)	(0.001)	(0.000)	(0.000)
Observations	8 582	6 567	6 567	6 538	8 582	23 597	18 167	18 167	18 167
R^2	0.186	0.026	0.084	0.081	0.070	0.172	0.011	0.108	0.117
	Panel B: Matched sample								
Post*LBO	-0.006	-0.038	-0.014	0.023	0.403	0.010	-0.003	-0.003	-0.000
	(0.048)	(0.026)	(0.030)	(0.021)	(0.416)	(0.026)	(0.003)	(0.003)	(0.002)
LBO	-0.024	0.060**	0.059*	-0.000	0.343	-0.047	0.002	0.001	-0.001
	(0.096)	(0.028)	(0.032)	(0.015)	(0.814)	(0.059)	(0.002)	(0.004)	(0.003)
Post	0.083	-0.001	0.021	0.021	-0.512	-0.031	-0.001	0.002	0.003
	(0.073)	(0.014)	(0.018)	(0.013)	(0.819s)	(0.048)	(0.003)	(0.002)	(0.003)
Observations	2 943	2 328	2 328	2 327	2 943	11 609	9 066	9 066	9 066
R^2	0.129	0.029	0.068	0.091	0.132	0.201	0.012	0.061	0.080

Notes. This table displays—for $t = -2$ to $t = 2$—firm level and establishment difference-in-difference estimates. All regressions include a constant, industry dummies at the 2-digit SNI level, 20 county (in Swedish: "län") dummies, and year dummies. Standard error are clustered at the firm level for the firm analysis and at the establishment level for the establishment analysis.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

ployment growth (Column 7) driven by reduced hirings (Column 8), which are partly offset by reduced separations (Column 9). Reduced separations is consistent with our earlier finding of a 2 percentage point lower unemployment incidence following the LBO. Relative to matched controls (Panel B), we find no statistically significant effects at the firm or the establishment level.

Overall, these firm and establishment level results are consistent with the notion that acquired controls drive the reduction in unemployment incidence in the acquired sample and that on average little changes in the short run after the LBO in the matched sample. It is worthwhile briefly relating these findings to the previous literature on the employment effects of LBOs. The results are broadly consistent with the earlier firm level study of Swedish LBOs. Using a smaller sample, Bergström et al. (2007) find little evidence of radical changes in firm level outcomes relative comparable firms but they do find a 3 percentage point increase in EBITDA margin.[22] Their post-LBO period is, however, longer than ours since they consider the full period of ownership from acquisition to exit. It is fully plausible that profitability improvements in our sample have not had time to materialize.

Relative to evidence from other countries, these findings are most similar to the U.K. evidence for more recent LBOs (Wright, Thompson, and Robbie, 1992; Amess and Wright, 2007, 2012) where few statistically significant changes in firm level employment are observed. Our results are dissimiliar to the increases in firm level growth observed in France by Boucly et al. (2011) and also slightly different from the U.S. evidence of reorganization post-LBO in Davis et al. (2011) and Davis et al. (2009). Davis et al. (2011) and Davis et al. (2009) document substantial reorganization with continuing establishments declining in employment growth, but more productive establishments taking their place (for the manufacturing sector). Reorganization activities to improve productivity can create costs for workers.[23] In earlier version of this paper, Olsson and Tåg (2012), we closely replicated Davis et al. (2011) using the Swedish data. In terms of effects on continuing establishments (creating matches at the establishment level), we found decreases in employment growth rates for establishments corresponding to the decreases in the US data. 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 in the US. The point estimate for the average establishment employment growth rate in the four years following a buyout is -1.2 percentage points. At the firm level, we found no effects on employment growth consistent with new greenfield establishment creation potentially offsetting the declines in effects on continuing establishments. An important point made in Olsson and Tåg (2012) is that the decline in

[22]Their measure of profitability is EBITDA divided by sales. Our measure is EBITDA over total assets.

[23]We would expect productivity improvements as a result of reduced agency costs (Jensen, 1989) or as a result of stronger incentives to restructure the firm due to temporary ownership (Norbäck, Persson, and Tåg, 2010).

establishment employment growth is driven primarily by hiring reductions rather than by increased separations. This evidence—obtain using similar matching techniques as Davis et al. (2011)—suggests that perhaps the effects of LBOs in the U.S. are not too dissimilar to the effects in Sweden.[24]

3.4 Heterogeneity analysis

So far, we have established that on average there is little evidence of negative effects on individual workers following LBOs. However, the average effects on unemployment incidence may not extend to sub-groups of workers. Previous theoretical and empirical research indicates that some workers could be affected more negatively than others. Since this research relates primarily to outcomes relative to matched workers, we omit the acquired sample in this subsection.

First, we find no evidence of breach of implicit contracts. A motivation for hostile takeovers could be to capture value from workers through breach of implicit contracts between managers and workers (Shleifer and Summers, 1988). The reason is that moral hazard among workers can make it optimal for an employer to pay workers 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, workers and managers can implicitly agree on wages increasing with tenure. For cost cutting reasons, such agreements could be broken after an ownership change. If this is done in practice, we would expect that older workers and workers with longer tenure in the firm would to a greater extent transition to unemployment. Table XI shows DiDiD estimates for workers less than 50 years of age versus workers who are at least 50 years old and workers with fewer than eight years of firm specific tenure as of the year of the LBO ("Non-tenured") versus workers with at least eight years of firm specific tenure as of the year of the LBO ("Tenured"). For both groups, the point estimate is positive (consistent with breach of trust) but it is not statistically significant.

Second, we find some indications that workers higher up in the corporate hierarchy fare worse that workers further down. LBOs are often followed by cost cutting activities to remove slack in the organization. Jensen (1989) points out that LBOs substitute monitoring for incentives and compensations to elicit effort from workers. Since workers higher up in the corporate hierarchy are the ones doing most of the monitoring, they are most likely to become redundant.[25] Additionally, white-collar middle managers have jobs where firm-specific human capital, career concerns and internal labor markets are

[24]The diverging establishment level effects between this paper and Olsson and Tåg (2012) relates to the matching strategy. In Olsson and Tåg (2012), we followed Davis et al. (2011) and found comparable establishments by matching at the establishment level. Here, however, we match at the firm level to be able to better compare the firm, establishment and worker level effects.

[25]There is some evidence of this occurring: Lichtenberg and Siegel (1990) show that LBOs reduce the labor ratio between white and blue-collar workers.

Table XI: Unemployment incidence and breach of implicit contracts

	Matched sample	
	(1)	(2)
	Tenured vs Non-tenured	Age>50 vs Age<=50
DiDiD	0.010	0.012
	(0.011)	(0.007)
DiD	-0.001	-0.004
	(0.007)	(0.007)
R^2	0.270	0.269
Observations	1 023 419	1 023 419

Notes. This table displays worker level difference-in-difference-difference estimates for unemployment incidence by years of tenure and age. The specification for the estimates in each subsample is the same as in Columns (3) and (6) in Table IV. Standard error are clustered at the firm level as of $t = 0$.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

more important (Ichino et al., 2010). Finding a new job can then take longer, so they have a higher chance of a period of unemployment if they get dismissed. To find evidence for this, we rely on worker level information occupations (SSYK codes). These are available for around 66% of all workers in the Swedish economy for the years 2001 to 2004.[26] We use this data to compare middle managers to all other workers and blue-collar workers to white-collar workers.[27] We also compare high skilled workers to low skilled workers with skill measured based on education (high skill workers have more than two years of undergraduate studies). Table XII displays the results of DiDiD estimates on unemployment incidence for these subgroup categorizations. High skill workers and middle managers are not differently affected than other workers (Columns 1 and 2). Column 3, however, shows that the average unemployment incidence is 3.4 percentage points higher for white-collar workers than for blue-collar workers. The increase in unemployment incidence for white-collar workers is not, however, statistically significant in absolute terms. That is, when we run the DiD model with full sets of firm and worker controls for white-collar workers only. The point estimate is positive, but not statistically significant (at the 10% level).

Third, we do not find that workers in financially distressed firms are more likely to end up unemployed. They could be more at risk, because these firms are likely in dire need of cost cutting measures. For evidence, we split the sample such that we compare workers in firms in the bottom and the top

[26]See Tåg et al. (2013) for a detailed description of the data.

[27]The middle managers are SSYK 122 (Production and operations managers) and SSYK 123 (other specialist managers). Blue-collar workers are SSYK 400-999 which consists of clerks; service workers and shop sales workers; skilled agricultural and fishery workers; craft and related trades workers; plant and machine operators and assembers; and elementary occupations (such as janitors).

Table XII: Unemployment incidence by occupation and skill

	Matched sample		
	(1)	(2)	(3)
	High vs Low skill	Middle Managers	White vs Blue
DiDiD	0.012	-0.004	0.034**
	(0.007)	(0.010)	(0.012)
DiD	-0.004	-0.007	-0.019
	(0.008)	(0.009)	(0.010)
R^2	0.268	0.237	0.238
Observations	1 023 419	438 848	438 848

Notes. This table displays heterogeneity analyses on worker level difference-in-difference estimates for unemployment incidence. The specification for the estimates in each subsample is the same as in Column (6) in Table IV. Standard error are clustered at the firm level as of $t = 0$. "High vs Low skill" refers to a model where we compare skilled workers with non-skilled workers based on whether they have at least two years of university education. "Middle managers" refers to a model where we compare workers with the occupational definition middle managers to workers with the occupational definition not being middle managers. "White vs Blue" refers to a model where we compare white-collar workers to blue-collar workers.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

quartile of the distributions of return on assets, cash on hand, and debt to asset at $t = 0$ for the treated and control firms. Our prior is that firms with low return on assets, low cash on hand, and high debt to assets just prior to the LBO would have greater unemployment incidence post-LBO relative the matched controls (where cost cutting measures potentially are more likely to be delayed). This is, however, not the case. Table XIII displays the DiDiD and the DiD estimates of unemployment incidence for these subgroups of workers. None of the estimates in Columns 1-3 are statistically significant.

Finally, we find no evidence that workers with weaker employment protections (easier to fire) are more likely to end up unemployed. We can think of two tests for whether the unemployment incidence increases for workers with weaker employment protection, both based on that workers on temporary contracts are protected less by laws than workers on permanent contracts. Though we do not observe the types of employment contracts, we can identify groups of workers where temporary contracts are common. The first group is based on the age of workers. OECD reports that 41.3% of workers between 15 and 24 years old in Sweden had temporary contracts in 2000 (OECD, 2002). The same figure for the group of workers aged 25 to 54 was 10.5%. The second group is based on that workers on temporary contracts being more likely to jump in and out of unemployment. Workers with unemployment days in the years pre-LBO are thus more likely to be on a temporary contract. Table XIV presents the results of DiDiD estimations for these subcategories. We estimate a reduction of the unemployment incidence for young workers affected by LBOs relative old workers affected by LBOs (Column 1) and no effects between workers affected by LBOs with and without an unemployment history (Column 2). These

Table XIII: Unemployment incidence by financial status prior to LBO announcement

	Matched sample		
	(1)	(2)	(3)
	ROA	Cash	Debt to Asset
DiDiD	-0.016	-0.021	-0.004
	(0.020)	(0.015)	(0.021)
DiD	-0.001	0.015	0.005
	(0.014)	(0.010)	(0.011)
R^2	0.254	0.262	0.264
Observations	387 913	613 643	462 149

Notes. This table displays heterogeneity analyses on worker level difference-in-difference estimates for unemployment incidence. The specification for the estimates in each subsample is the same as in Column (6) in Table IV. Standard error are clustered at the firm level as of $t = 0$. All models compares the top quartile with bottom quartile of respective distribution as of time zero.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

results are inconsistent with the hypothesis that these workers are more at risk because they are easier to fire. 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 finds no support for the notion that their finding of increased firm level employment growth after the LBO is affected by the strong French labor market regulations.

4 Robustness

Before we conclude, we undertake two robustness checks. First, we show that our baseline results on unemployment incidence are robust to two alternative control groups: a random sample of firms and matched firms based on propensity score matching. Second, our baseline results are robust to clustering at the group or worker level.

4.1 Alternative controls

In addition to comparing to workers in cell matched firms and workers in firms acquired by non-financial strategic buyers, we can think of two alternative comparison groups. The first consists of workers in a random sample of firms not targeted for an LBO or an acquisition (the random sample we compared to in subsection 2.4). The second alternative control group of workers is based on propensity score matching at the firm level. A clear advantage with propensity score matching is that the creation of a

Table XIV: Unemployment incidence by strength of employment protection

	Matched sample	
	(1)	(2)
	Old vs Young	Unem vs Non Unem
DiDiD	0.022*	-0.010
	(0.012)	(0.011)
DiD	-0.019*	-0.000
	(0.011)	(0.005)
R^2	0.269	0.313
Observations	1 023 419	1 023 419

Notes. This table displays heterogeneity analyses on worker level difference-in-difference estimates for unemployment incidence. The specification for the estimates in each subsample is the same as in Column (6) in Table IV. Standard error are clustered at the firm level as of $t = 0$. "Old vs Young" refers to a model that compares workers older than 30 years with workers younger than 30 years. "Unem vs Non Unem" refers to a model that compares workers with any unemployment days in the pre-period with workers without any unemployment days in the pre-period.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

propensity score makes it possible to match on more firm characteristics compared to cell matching. The firm characteristics we use to create the propensity score are return on assets, debt to asset ratio, whether a firm is a market leader defined as being one of the three firms with highest sales figures in a five digit industry, firm size measured as number of employees, growth rate of firm size from $t-1$ to t, sales, value added, fixed assets, age of the firm, two-digit industry classification dummies, 24 county dummies, 12 bins for number of employees, and five firm age bins (at $t = 0$, the year before the LBO was announced). Based on the propensity score, we pick the nearest neighbor to all treated firms and drop all treated firms with a propensity score higher/lower than the maximum/minimum propensity score of the matched control firms to guarantee common support. We then track all workers for four pre and post years.

Table XV contains summary statistics for LBOs and the propensity score matched control group. As with the cell matched control group, worker characteristics are evenly distributed between the groups but firm characteristics are not. LBO firms have on average more employees, more fixed assets, higher value added per employee, and lower sales the year before the transaction. Table XVI displays the DiD estimates with the random sample of firms and the propensity score matched sample of firms as the control groups (estimates are based on the model in Equation 1). Columns (1-3) shows that unemployment incidence remains unchanged post-LBO relative to workers in the random sample of firms. The estimates are unaffected by adding worker level and firm level controls. We obtain similar results when using propensity score matching (Columns 4-5). The point estimates (independent of controls) are not

Table XV: Characteristics of the Propensity Score matched sample

	Propensity score match sample		
	LBOs	PSM firms	T-test (1)-(2)
	(1)	(2)	(3)
	A. Worker characteristics		
No. workers	58 741	50 723	
Age	40.04	40.02	0.00
Share high skilled	0.20	0.20	0.00
Female	0.44	0.38	0.06
Tenure	3.23	4.08	**-0.31**
Labor income	243.73	246.64	-0.16
Unemployment history	0.52	0.54	-0.02
	B. Firm characteristics		
No. firms	367	367	
Size	159.94	138.87	**0.84**
Age	25.20	25.71	-0.08
Share located in north	0.10	0.10	0.00
Value added/employee	643.42	607.50	**0.91**
Fixed assets	56 782.10	51 496.36	**7.64**
Sales	265 551.34	281 508.41	**-12.95**
Debt to total assets	0.68	0.67	0.01
Return on total assets	0.09	0.10	0.00

Notes. The table displays means for various firm and worker level characteristics of the samples. Column (3) present normalized t-tests of differences in means. Normalized t-values above 0.25 are bolded. All monetary values are in thousands of 2005 SEK. High skilled workers are workers with more than 2 years of undergraduate studies. Labor income is total gross labor income for the full year. Unemployment history takes values from zero to four and counts unemployment incidence for the four preceding year (for example, a worker unemployed at $t = -2$ and $t = -1$ would have a score of 2 while a worker never unemployed during the last four years would have a score of zero).

statistically significant.

4.2 Clustering of standard errors

At present, there is no clear consensus on how to compute the standard error for the DiD estimator. We have chosen to cluster the standard errors at the firm level because this is the "treatment level" of the analysis and because firm characteristics are more important than individual characteristics in predicting LBOs (Table III). However, workers can and do transition in and out of the firm they are employed in at $t = 0$ which can make firm level clustering imprecise.

There are other reasonable cluster levels. Donald and Lang (2007) point out that the relevant variation for a DiD estimator is at the group level where the variation is. 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 worker level analysis because

Table XVI: Effects on unemployment incidence for alternative control groups

	Random sample			Propensity Score match sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Post*LBO	-0.004	-0.004	-0.004	-0.009	-0.010	-0.008
	(0.006)	(0.006)	(0.006)	(0.008)	(0.007)	(0.007)
LBO	-0.046**	-0.011***	-0.001	0.005	0.001	0.001
	(0.020)	(0.004)	(0.003)	(0.009)	(0.003)	(0.003)
Post	-0.030***	-0.029***	-0.022***	-0.020***	-0.020***	-0.019
	(0.003)	(0.003)	(0.005)	(0.006)	(0.006)	(0.005)
Individual controls	No	Yes	Yes	No	Yes	Yes
Firm controls	No	No	Yes	No	No	Yes
Industry dummies	No	No	Yes	No	No	Yes
County dummies	No	No	Yes	No	No	Yes
Year dummies	No	No	Yes	No	No	Yes
Observations	955 525	955 525	954 620	972 805	972 805	972 805
R^2	0.008	0.317	0.321	0.002	0.265	0.270

Notes. This table displays worker level difference-in-difference estimates of unemployment incidence. All regressions include a constant. Worker level controls X_{ifgt} include age, age squared, gender, skill level, tenure at the firm, labor income and five dummies for unemployment incidence from $t = -4$ to $t = 0$. Firm level controls F_{fgt} include size, age and age squared. Industry dummies are at the 2-digit SNI level and there are 20 county (in Swedish: "län") dummies. Tenure goes back to 1990, so years of tenure at the firm is truncated at 8 for LBOs in 1998 and 14 for LBOs in 2004. The age of the firm is based on the date of registration at Statistics Sweden and goes back to 1970. If a worker/establishment/firm is treated multiple times in our sample we include the worker/establishment/firm as treated only the first time of treatment. Standard error are clustered at the firm level as of $t = 0$.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
*** Significant at the 1 percent level.

our groups consist of a large and constant number of observations over time. Therefore, to account for possible intra-class correlation within time periods we can aggregate our data to a group-time level and estimate the following model:

$$\triangle Y_t = \rho + \beta(Post_t) + \triangle\varepsilon_t, \tag{6}$$

where

$$\triangle Y_t = Y_{Tt} - Y_{Ct}, \triangle\varepsilon_t = \varepsilon_{Tt} - \varepsilon_{Ct}. \tag{7}$$

The variable $Post_t$ indicates the year of the LBO and all years after, so β is the DiD estimator. The drawback of using this model, however, is that in our setting it entails estimating the effects using only 9 observations. This can lead to imprecise estimates and the few degrees of freedom does not allow us to include covariates. Another approach is to cluster the standard errors at the worker level. Worker clusters deal with repeated observations for workers but misses out on group and firm level shocks, which can make the standard errors too small.

For both of these alternative levels of clustering the standard errors, the statistical significance of our baseline results remain unchanged. Table XVII presents estimates of the effect on unemployment incidence using the two levels of clusters. Columns 1 and 2 show that the statistical significance remains unchanged for both the acquired and the matched sample when clustering at the worker level (decrease in unemployment incidence relative acquired, no effect relative matched). The dependent variable in Columns (2) and (4) is the difference in unemployment incidence between the treatment and control group (clustering at the group level). The difference-in-difference estimator is here represented by the coefficient for Post. Relative acquired controls, we still estimate a statistically significant reduction in unemployment incidence (Column 2). Relative matched controls, we find no effect of the LBO on unemployment incidence (Column 4).

5 Concluding remarks

In this paper we have studied the effects of leveraged buyouts (LBOs) on unemployment incidence for workers in Swedish firms between 1998 and 2004. We made three contributions to the literature. First, our paper is the first one to study the effects of LBOs on individual workers using matched employer-employee data. We found no evidence that unemployment incidence increases on average after a lever-

Table XVII: Effects on unemployment incidence for different cluster levels

	Acquired sample		Matched sample	
	(1)	(2)	(3)	(4)
	Cluster: worker	Cluster: group	Cluster: worker	Cluster: group
Post*LBO	-0.022***		0.001	
	(0.001)		(0.001)	
LBO	0.005***		-0.001*	
	(0.000)		(0.001)	
Post	-0.013***	-0.010***	-0.026***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Individual controls	Yes	No	Yes	No
Firm controls	Yes	No	Yes	No
Industry dummies	Yes	No	Yes	No
County dummies	Yes	No	Yes	No
Year dummies	Yes	No	Yes	No
Worker-Year Obs.	2 678 827	9	1 051 033	9
R^2	0.287	0.097	0.267	0.085

Notes. This table displays worker level difference-in-difference estimates of unemployment incidence for clustering the standard errors at the worker level and at the group level. All regressions include a constant. Worker level controls X_{ifgt} include age, age squared, gender, skill level, tenure at the firm, labor income and five dummies for unemployment incidence from $t = -4$ to $t = 0$. Firm level controls F_{fgt} include size, age and age squared. Industry dummies are at the 2-digit SNI level and there are 20 county (in Swedish: "län") dummies. If a worker/establishment/firm is treated multiple times in our sample we include the worker/establishment/firm as treated only the first time it was treated. The dependent variable in Columns (2) and (4) is the difference in unemployment incidence between the treatment and control group, consequently the difference-in-difference estimator is represented by the coefficient for Post.

* Significant at the 10 percent level.

** Significant at the 5 percent level.

*** Significant at the 1 percent level.

aged buyout relative to unemployment incidence in similar non-acquired firms. Second, we are the first to document that a leveraged buyout is substantially better for workers than an acquisition by a strategic non-financial buyer. On average unemployment incidence decreases by 2.2 percentage points or 20% when comparing workers in LBO targets to workers in firms acquired by non-financial buyers. A plausible explanation is that strategic acquirers already have assets in the market which causes reorganization to the detriment of the acquired firm's workers. Third, we are the first to directly evaluate if some subgroups of workers have relatively more to fear from LBOs than others. White-collar workers and workers older than 30 years are more likely to become unemployed relative blue-collar workers and younger workers, but they are not more likely to become unemployed in absolute terms. We did not find evidence of higher unemployment incidence post-LBO for (i) workers in firms more likely to be in financial distress (low cash, low ROA, and high leverage); (ii) older more tentured workers as a result of breach of implicit contracts; or (iii) workers easier to fire because of weaker employment protection.

Overall, these findings suggests that LBOs in Sweden have not been connected with systematic layoffs leading to unemployment or loss of income for workers. As the private equity industry have grown, we have seen an increased eagerness to regulate the industry.[28] Despite claims to the contrary by labor unions, our study suggestion that caution should be excersised in motivating restrictions on the industry with concerns of negative effects on workers.

It is, of course, challanging to evaluate the external validity of our results. The firm and establishment level results do contrast somewhat with recent evidence of increased rates of creative destruction and productivity growth in the wake of LBOs in the US (Davis et al., 2011). If there is a tradeoff between productivity gains in the firm and worker's unemployment incidence, then certainly a detailed study on the effects on individual workers in a country with observed productivity gains—such as the US—seems warranted. Whether less reorganization in Sweden than in the US is a result of stronger labor market regulations is hard to tell. 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

[28]In Europe the AIFMD imposes new rules of alternative investors such as private equity firms, politicians are eager to be seen standing on the sides of workers fighting foreign locusts, and the taxation of partners in private equity firms is a constant source of controversy. See http://ec.europa.eu/internal_market/investment/alternative_investments/index_en.htm (Accessed January 2013) for more on the AIFMD; "Locust, Pocus" (May 7th, *The Economist*) on the remarks of SPD head Franz Müntefering that likened private equity firms to "swarms of locusts that fall on companies, stripping them bare before moving on."; and "Testing the Model: Private Equity Faces a More Hostile World" (Jul 9 2009, *The Economist*), "Editorial, New Rules for Private Equity" (August 30 2009, *New York Times*) or "Private Equity Fights Tax Plan" (February 27 2009, *Financial Times*) for more on the debate on taxation.

with Sweden's. If labor market regulations do play a role, our results would apply mostly to LBOs in countries with similar labor market regulations. A full 20% of all LBOs undertaken worldwide between 1998 and 2004 in the Capital IQ database occurred in countries with similar employment protection as Sweden.

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