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ABSTRACT

Private equity firms are often criticized for laying off workers, but the evidence on who loses their jobs and why is scarce. This paper argues that explanations for job polarization also explain layoffs after private equity buyouts. Buyouts reduce agency problems, which triggers automation and offshoring. Using rich employer-employee data, we show that buyouts generally do not affect unemployment incidence. However, unemployment incidence doubles for workers in less productive firms who perform routine or offshorable job tasks. Job polarization is also much more marked among workers affected by buyouts than for the economy at large.

Keywords: Automation, employment, globalization, job polarization, private equity buyouts, leveraged buyouts, offshoring, restructuring, task-biased technological change, unemployment.

JEL Codes: G32; G34; J50; J60; M51.

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1 Introduction

Academic evidence suggests that private equity buyouts are associated with modest net employment effects. Nevertheless, private equity firms are often portrayed by labor unions and the media as vultures that cut costs through layoffs, leaving workers unemployed.[1] One reason for the difference between the public view and the academic evidence is that most academic studies have focused on net employment. These studies provide important insights on the way buyouts affect overall employment. However, they provide little evidence about what happens to workers who are employed at a firm immediately before a buyout. For example, zero effects on net employment from one year to the next could mean that a share of existing workers was laid off and quickly replaced with new hires. Therefore, what are the effects of buyouts on existing workers? Do some workers have a greater reason to fear for their jobs than others? If so, why?

The goal of this paper is to provide new insights on these questions. We argue that layoffs after buyouts can be understood through the lens of the job polarization process, where employment shares among low- and high-wage occupations have increased over time relative to middle-wage occupations. At least three explanations have been given for this "hollowing out of the middle class": the automation of routine job tasks due to technological development, the globalization of product and labor markets, and the weakening of labor unions over time.[2]

These explanations could also explain layoffs after buyouts because private equity firms are effective in reigning in rampant agency problems in firms (Jensen, 1989).[3] Agency problems allow managers to avoid difficult and unpopular decisions (Bertrand and Mullainathan, 1999, 2003).[4] These decisions include pushing for investments in automation and in offshoring production, as well as exhibiting strength when negotiating with labor unions. Although entrenched managers may procrastinate regarding these decisions, they are crucial for improving productivity and profitability. Thus, firms in which they have been delayed could be prime targets for buyouts. Additionally, workers in routine jobs or offshorable jobs and workers who are entrenched because of ties to aggressive labor unions are likely prime targets for layoffs.

For empirical evidence regarding our claim, we look to Sweden. Sweden is a unique testing ground for this argument for several reasons. Sweden has an active private equity market, and job polarization has occurred during the last decades.[5] In fact, Lerner, Hardymon, and Leamon (2008) shows that, in 2006, Sweden had the highest level of private equity investments to gross domestic product in all of Europe. Moreover, detailed data on *all workers* in the country are available for 22 years. Thus, it is possible to find workers who were employed in target firms immediately before the buyout and to follow them over time to see whether they become unemployed. In addition, accounting data are available for *all* public and private firms operating in Sweden. Because private equity firms do not randomly select targets, selection bias is a central concern. However, the availability of comprehensive data means that we can rely on matching

[1] See, for example, FSA (2008), ITUC (2007), or PSE (2007) for criticism by labor unions. For more on the media 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".

[2] See, for example, Autor (2010), who also mentions declining minimum wages as one explanation. For recent evidence of job polarization, see, for example, Autor, Levy, and Murnane (2003), Autor, Katz, and Kearney (2006, 2008), Goos and Manning (2007), Blinder and Krueger (2013), Autor and Dorn (2013), and Goos, Manning, and Salomons (2014). Acemoglu and Autor (2011) provide a survey.

[3] Several studies have documented improvements in governance following leveraged buyouts related to board composition (Cornelli and Karakaş, 2012; Acharya, Gottschalg, Hahn, and Kehoe, 2013), pay-for-performance schemes (Kaplan, 1989; Acharya et al., 2013), management practices (Bloom, Sadun, and van Reenen, 2009), and leverage (Axelson, Jenkinson, Strömberg, and Weisbach, 2013).

[4] Bertrand and Mullainathan (1999) show that the implementation of antitakeover legislations in the 1980s in the United States led managers to increase annual wages by 1% to 2% and Bertrand and Mullainathan (2003) show that they were less likely to close down plants or open up new ones. Productivity and profitability also fell, suggesting that managers like to enjoy a quiet life.

[5] Both Goos et al. (2014) and Adermon and Gustavsson (2015) provide evidence of job polarization in Sweden.

methods and difference-in-differences regressions to mitigate selection biases.[6]

Our analysis focuses on unemployment incidence for the four years before and after buyout announcements and uses standard measures of the routine intensity and offshorability of job tasks. We analyze buyouts taking place between 2002 and 2008 and document two key findings for treated workers relative to control workers. First, consistent with the existing evidence of modest net employment effects, we find little evidence of average changes in unemployment incidence after buyouts. Second, consistent with automation and offshoring, the unemployment incidence nearly doubles for workers in firms that lag their peers in productivity and who perform routine or offshorable job tasks. The point estimates for workers performing routine job tasks show a 10.2-percentage-point (96.6%) increase in unemployment incidence, and the point estimates for workers performing offshorable job tasks show a 8.6-percentage-point (97.0%) increase in unemployment incidence. Despite the similar magnitudes of the estimates, offshorability is a separate measure from routine intensity. The correlation between these two measures in our sample is only 0.3, and additional analyses suggest that different workers drive the results. Consistent with these worker-level results, we find that the target firms that lag behind their peers in productivity tend to expand their intangible assets (a proxy for technological investments complementary to automation and offshoring) and increase their productivity relative to control firms.

Additional analyses provide another four insights. First, we fail to find compelling evidence that stronger bargaining power with labor unions explains layoffs after buyouts. Although this rationale may have been true for buyouts in the 1980s in the United States (Shleifer and Summers, 1988), it appears less relevant for buyouts in Sweden in the 2000s. Second, analyzing employment duration, job changes, and wages after buyouts reveals that unemployment after buyouts is not long term. Most workers that enter unemployment after buyouts find new jobs within a year. Third, a detailed comparison to Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014) suggests that there are differences between Sweden and the United States in terms of reorganization patterns. In Sweden, employment growth occurs in continuing establishments, and acquisitions play a minor role, while the opposite holds in the United States. Nonetheless, separating out ex ante low-productive from ex ante high-productive firms, we find indications that buyouts of low-productive firms show patterns of restructuring (as documented by Davis et al. (2014) for the United States), while buyouts of high-productive firms show patterns of growth (as documented in France by Boucly, Sraer, and Thesmar (2011)). It is important to keep in mind, however, that private equity transactions are heterogeneous and undertaken for a variety of reasons, so categorizing transactions into these two bins is a major simplification. Finally, we document that layoffs after buyouts are indeed concentrated among workers in the middle part of the wage distribution and that job polarization seems to be more marked among workers affected by buyouts than among workers in the economy at large. Using an index of job polarization in Sweden between 2001 and 2011, we calculate that excluding workers involved in buyouts drops the polarization index at the low end by 6.48% and at the high end by 0.95%. The modest impact on the aggregate polarization pattern is likely driven by the fact that private equity buyouts affect a relatively small segment of the labor force (2.7%).

Although estimating the causal effect of buyouts is inherently difficult, we believe that our results present a compelling case that layoffs after buyouts can be understood through the lens of the job polarization process. Evidence from surveys confirms this conclusion. During the spring of 2009, the Swedish labor union ”Unionen” asked the union chairmen at private equity-backed firms what changes had occurred since changing ownership. Approximately 62% of the chairmen answered that investments in new markets/products/services had occurred, and approximately 27% answered that the outsourcing of operations had occurred (Tjärnback, 2009).

Our paper contributes to the literature on private equity buyouts and employment. Most published

[6]Matching methods are particularly useful when many observable characteristics and potential controls are available (Heckman, Ichimura, and Todd, 1997).

studies regarding this topic focus on net employment. Recent evidence from the United States suggests that private equity firms catalyze the creative destruction process and improve productivity, while having small effects on net employment (Davis et al. (2014)). The firm-level results of modest net job losses reported by these authors correspond with those of other papers regarding buyouts in the United States, such as Kaplan (1989), Muscarella and Vetsuypens (1990), and Lichtenberg and Siegel (1990). Evidence from the United Kingdom also suggests decreases in employment growth, but the effects appear to be weaker for more recent buyouts (Wright, Thompson, and Robbie, 1992; Amess and Wright, 2010). In Sweden, it has been documented that employment remains unchanged (Bergström, Grubb, and Jonsson, 2007), but in France, private equity firms appear to provide capital to firms, which generates positive employment growth (Boucly et al., 2011).[7] Our results shed new light on how buyouts affect labor by including insights from the job polarization literature. We are aware of three other worker-level studies in the literature, but these studies do not analyze automation and offshoring as explanations for layoffs. Amess, Brown, and Thompson (2007) analyze management buyouts in the United Kingdom and show that buyouts reduce the number of hierarchical tiers and that craft and skilled service employees are most likely to receive reduced supervision and increased discretion. In addition, Agrawal and Tambe (2016) focus on how IT investments by private equity firms affect the careers of workers complementary to IT investments. Finally, Antoni, Maug, and Obernberger (2015) studies the impact of private equity investments on the wages and career paths of workers.

Furthermore, our results shed new light on the broader debate between value capture and value creation in buyouts. This debate began in early studies on buyouts. For example, Jensen (1989) argued that buyouts are a superior organizational form that creates value. This view is often compared with the claim presented by Shleifer and Summers (1988), which states that hostile takeovers capture value from workers and other stakeholders instead of creating it. Existing empirical evidence shows that buyouts are followed by productivity improvements, that there are few negative long-term investment effects and few tax gains and that firm-level employment generally does not decrease.[8] Thus, the standard conclusion is often that private equity firms create value, which corresponds to the findings of Jensen (1989). We agree that this conclusion holds on *average*, but we add that value creation does not rule out value capture. Our results show value capture from the subgroups of workers who are more likely to be laid off and enter unemployment instead of finding a new job. Simultaneously, we show productivity gains for firms that are less productive than their peers, which is consistent with value creation. Because we observe moves to unemployment for workers, our results provide new insights regarding welfare losses and value capture in buyouts. As noted by Shleifer and Summers (1988), "to see if the parties that lose in association with the acquired firm suffer wealth losses, one must trace their subsequent employment." If workers simply move to other forms of employment, wealth losses may not be large. However, if workers move to unemployment, they are far worse off.[9]

Finally, our paper contributes to the broader literature on finance and labor and to the job polarization literature by showing how financial intermediaries in the form of private equity firms can shape the labor policies of firms.[10] Moreover, the job polarization literature has typically focused on examining the long-run changes in wages and employment within and across countries.[11] Our paper provides novel within-firm

[7] Kaplan and Strömberg (2009) and Tåg (2012) provide summaries of the literature on private equity buyouts and employment.

[8] See, for example, Kaplan (1989), Lichtenberg and Siegel (1990), Amess (2002), Amess (2003), Harris, Siegel, and Wright (2005) and Davis et al. (2014) for evidence on productivity, Lerner, Sorensen, and Strömberg (2011) for evidence on long-run investments and Jenkinson and Stucke (2011) for evidence on taxes.

[9] Workers entering unemployment often face wage cuts after accepting a new job offer, consumption reductions, and decreased happiness. See, for example, Farber (2005), Katz and Mayer (1990), Jacobson, LaLonde, and Sullivan (1993), Gruber (1997), and Di Tella, MacCullock, and Oswald (2001).

[10] For studies linking finance and labor, see, for example, Rosett (1990), Bronars and Deere (1991), Matsa (2010), Pagano and Pica (2012), Benmelech, Bergman, and Enriquez (2012), or Graham, Kim, Li, and Qiu (2013).

[11] See, for example, Blinder and Krueger (2013), Autor and Dorn (2013), and Goos et al. (2014) for recent studies. In addi-

evidence that changes in the ownership of firms could affect job polarization.

Whether our results generalize beyond the borders of Sweden is an open question. However, the following observations suggest that they do.[12] First, the Swedish private equity market is characterized by a substantial presence of international private equity firms, and the same types of transactions that occur abroad occur in Sweden. Second, the Swedish private equity market evolved partly in response to the wave of buyouts in the United States and has resulted in the development of several large Swedish private equity firms that operate internationally. Third, the holding periods, performance, and outcomes of the target firms in Swedish buyouts are similar to those in other countries. Finally, when the 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 30 countries (OECD, 2004). Thus, Sweden has quite strong employment protection regulations.[13] However, if employment protection regulations in Sweden are important, any results that we find concerning more layoffs are likely to represent a lower bound for the effects of buyouts on workers in countries with weaker protections.

2 Data and empirical strategy

2.1 Data sources

Statistics Sweden. Our main source of data is Statistics Sweden's LISA database, which contains individual-level data from 1990 to 2011. LISA is a longitudinal matched employer-employee database that includes every person older than 15 that is registered as living in Sweden. The LISA database merges data from different government registers, including population registers, tax records, and statistical surveys. Because LISA is based on the government registration of inhabitants in Sweden, an individual exits the database only by dying or moving to another country. The database contains individual identifiers that enable us to track people over time, independently of labor market status. From LISA, we extract annual information on individuals' age, gender, education, occupation (available for 2001-2011), labor income, firm and establishment affiliation, industry, and the number of days registered as unemployed. We calculate firm tenure by observing work histories from 1990 to 2011. Table A1 provides details regarding these variables.

Swedish Companies Registrations Office. The firm data we use are from the annual accounts collected by the Swedish Companies Registrations Office (SCRO). Swedish law requires all limited-liability firms to deliver annual accounts to the SCRO.[14] To ensure the quality of the data, external auditors check the filed accounts of all firms. Non-compliance or submitting incorrect information results in liquidation and unlimited liability for board members. Regarding the annual accounts, we have information from 1998 to 2010 for the name of the firm, the firm identifier, sales, fixed assets, value added per employee, debt to assets, return on assets, and the group structure of the firms. We supplement the data with firm-level variables that are aggregated from the individual-level data in LISA. These variables include firm size, firm size growth, the number of establishments, a diversification dummy for whether the firm operates in more than one industry or not, firm age, average worker age, standard deviation of workers' labor income, average worker tenure, the share of workers doing routine job tasks, the share of workers doing offshorable job tasks, the share of blue collar workers, and the share of workers with unemployment in previous periods. We also

tion, Acemoglu and Autor (2011) provided a recent survey. Initially, the job polarization process was explained by skill-biased technological change, that is, how new technologies affected high- and low-skilled workers. Instead, recent work emphasizes the importance of the job task and provides evidence that the nature of the job task – whether it is routine and/or offshorable – matters more than the skill level of the worker.

[12] See Section A in the Appendix for a more detailed discussion of buyouts in Sweden in comparison to buyouts elsewhere.

[13] Despite differences in labor market institutions, Lazear and Shaw (2009) reported surprising similarities in how labor markets function across Scandinavian countries and Belgium, France, Germany, Italy, the Netherlands, and the United States.

[14] Årsredovisningslag [1995:1554] 8 sec. 3 and Bokföringslag [1999:1078] 6 sec. 2.

add industry affiliation and geographical location from LISA (again, see Table A1 for details).

Capital IQ and Bergström et al. (2007). We obtain information on the buyouts undertaken between 2002 and 2008 in Sweden from the Capital IQ database and Bergström et al. (2007). We apply the same selection criteria as Strömberg (2008) to the transactions in Capital IQ, add the buyouts identified by Bergström et al. (2007) that are not reported in Capital IQ, and remove the secondary buyouts.[15] We remove the secondary buyouts because the explanations for layoffs we study apply to the first time a company undergoes a buyout. We name match buyouts to the firm data from the SCRO and then use SCRO firm identifiers to link the buyouts to the individual data in LISA. Because buyouts often occur in holding companies, we treat subsidiaries to buyout targets as targeted. Appendix B provides additional details on how we construct the sample and how we deal with the corporate group structures available from the SCRO.

2.2 Empirical strategy

Our main econometric concern is selection because private equity firms do not randomly choose targets. As a result, buyout targets can differ from other firms regarding both their observable and unobservable characteristics. Selection bias occurs if these characteristics are correlated with workers' unemployment incidence. Our main strategy to mitigate selection bias is to use propensity score matching to identify a control group one year before the buyout announcement and then apply a difference-in-differences estimator. Propensity score matching mitigates biases that arise from selection based on observable characteristics, and the difference-in-differences estimator deals with unobserved time-invariant group effects and common time effects. Although we cannot make causal claims, Smith and Todd (2005) show that combining propensity score matching with difference-in-differences regressions performs well in mitigating selection biases. Moreover, propensity score matching is particularly useful when there are many potential controls (Heckman et al., 1997), which is the case here.

We model the unemployment incidence for worker i in firm f at time t as of calendar year r as

$$Y_{iftr} = \alpha + \delta POST_t + \gamma LBO_i + \beta POST_t \times LBO_i + X_i^{'}\theta + F_f^{'}\pi + \lambda_r + \varepsilon_{iftr}. \tag{1}$$

The dependent variable is the unemployment incidence, Y_{iftr}. This variable is one if an individual is registered as unemployed for one or more days during time t, and zero otherwise. The $POST_t$ variable is one for the year of the buyout announcement and all years after, and zero otherwise.[16] The LBO_i variable is one for the treated workers employed in the targeted firms one year before the announcement and zero for the control workers in matched control firms. The coefficient β captures the average treatment effect. We normalize time so that $t = 0$ is the year before the buyout announcement for both the treated and the matched controls, and we follow workers for four years around $t = 0$. Bergström et al. (2007) report a median holding period of 4.5 years for Swedish buyouts, so a four-year post-period allows us to capture a large part of the immediate effects of the buyouts on workers' unemployment incidence. Equation 1 also includes a vector of individual controls $X_i^{'}$ and a vector of firm controls $F_f^{'}$ to increase efficiency. The controls are all measured at $t = 0$. The individual controls are age, gender, labor income, education, tenure, and dummies for whether a worker has at least one day of unemployment at time $t = 0$, $t = -1$, $t = -2$, $t = -3$, and $t = -4$. The firm controls are size and its square, size growth, firm age and its square, and county dummies (in Section A.1 in the Web Appendix, we also run a firm fixed effects specification to check if potential additional time invariant firm characteristics affect our results). The vector of firm controls also includes match-specific dummies; therefore, we strictly compare workers in treated firms to workers in their respective matched control firm. Finally, the term λ_r represents year effects.[17]

[15] We thank Clas Bergström for providing us with the data matched with the legal firm identifiers.

[16] Nine out of ten buyouts in our sample are closed within the same year they are announced.

[17] The subindex r represents the actual year ($r = 1997, 1998, ..., 2011$), in contrast to the subindex t, which represents the normalized time period ($t = -4, -3, ..., 4$).

In specifications where we compare subgroups of workers, we also run a triple difference specification given by

$$\begin{aligned} Y_{iftr} = & \alpha + \delta POST_t + \gamma LBO_i + \beta POST_t \times LBO_i + \tau D_i + \mu LBO_i \times D_i + \\ & \mu POST_t \times D_i + \omega POST_t \times LBO_i \times D_i + X_i' \theta + F_f' \pi + \lambda_r + \varepsilon_{iftr}. \end{aligned} \tag{2}$$

We do this to obtain ω, the triple difference estimator that captures whether the treatment effects differ between the subgroups of interest (D).

Four remarks on our empirical strategy are in order. First, a potential drawback of propensity score matching is that it requires several decisions when creating the propensity score and pairing treated and control workers. As pointed out by Angrist and Pischke (2009), a straight OLS regression can be considered a particular sort of weighted matching and provides better transparency than propensity score matching. Thus, to increase transparency, we provide a robustness check in the Web Appendix (Section A.2), in which we select a 10% random sample of firms for each year in which buyouts take place. We then normalize time for these control firms and include an extended set of controls in the difference-in-differences regression model.

Second, the estimated treatment effect has a causal interpretation under the parallel trend assumption and the stable unit value treatment assumption (SUTVA). The parallel trend assumption requires that the treated and control groups would have had parallel trends in the absence of treatment. Because the counterfactual outcomes are unobservable, it is impossible to test this assumption. However, we can assess the plausibility of the assumption by comparing trends in unemployment incidence before treatment. Historical parallel trends indicate that shocks, at least in the past, have affected the two groups in a similar way. A problem arises, however, if something unobservable to us and unrelated to the buyouts affects the unemployment incidence for treated workers but not for the control workers *at the same time* as the buyout takes place. One example could be negative shocks to treated firms that push them close to bankruptcy, which we fail to account for by using a control group. Private equity firms are known to sometimes undertake "bankruptcy deals" in which they buy companies on the verge of, or already in, bankruptcy. Therefore, if negative shocks trigger buyouts, the unemployment incidence for treated workers can increase even in the absence of an effect from the buyout itself. If we fail to account for such negative shocks, our difference-in-differences estimate will be biased and not represent a causal buyout effect. Another example is granting a patent that causes a firm's growth potential to increase. This could attract private equity firms interested in growing firms, and such an event could lower workers' unemployment incidence by itself.

The SUTVA requires that buyouts have no general equilibrium effects on workers in the control group. A cause for concern would be, for example, if we select control workers from close competitors of the target firms. The estimated treatment effect on unemployment incidence may then be downward biased if layoffs in the target firms make them more formidable competitors and causes layoffs at other firms in the same industry employing control workers. These effects, however, are more likely in small markets with few firms. As we show in Section A.2 in the Web Appendix, our results continue to hold when we use a random sample of firms as controls, where the control firms greatly outnumber the treated firms.

A third remark we want to make is that the correlation between workers can result in standard errors that are too small, so called intra-class correlation (Moulton, 1986). We deal with this issue by clustering the standard errors at the corporate group and the local labor market (municipality) levels. By clustering at the corporate group level, we account for correlation between workers in different firms that belong to the same corporate group. By clustering at the local labor market level, we account for the reality that the workers can flow in and out of unemployment during our sample period. This does not, however, solve all inference issues. In particular, matching on an estimated propensity score can result in standard errors that are too small or too large (Abadie and Imbens, 2012). The alternative empirical strategy based on a random sample of controls that we present in the Web Appendix (Section A.2) therefore provides an important robustness check on our results.

Finally, the treatment group consists of workers employed at the target firms one year before the buyout announcement. We analyze these pre-existing workers before and after the buyout regardless of whether they remain employed at the same firm. Because workers leave the sample only by moving abroad or dying, with this strategy, we do not need to worry about compositional changes in the treatment and control groups. Indeed, four years after the announcement, 98.71% of the treated workers and 98.75% of the control workers remain in the sample. However, because workers can leave the firm before the buyout takes place, the coefficient β captures an *intention-to-treat effect*, which is smaller than the average treatment effect on the treated. It measures the average change in unemployment incidence for pre-existing workers over a four-year post-buyout period.

2.3 Creating the control group

We create the control group by matching at the firm level using one-to-one matching without replacement. Section A.2 in the Web Appendix shows that our main results are similar if we use a random sample of control firms, if we use one-to-four propensity score matching to create the control group, or if we use firms targeted in non-private equity M&As as controls. The matching process finds a control firm for each treated firm that is statistically similar as the treated firm. The matching is performed one year before the buyouts are announced. The details and motivations for the matching procedure are available in Appendix B. We use all limited liability firms in Sweden as potential controls with one exception: we exclude firms that were targeted for mergers or acquisitions based on data from the BvD Zephyr database. We exclude the merger or acquisition targets because the explanations for layoffs that we study rely on the counterfactual that the firm targeted for a buyout remains private and is not acquired by another firm.

The variables we include in the propensity score estimation are based on previous literature and on the theory behind the explanations for layoffs that we study. The opportunity for automation and offshoring should be more important for firms that employ workers who perform routine and offshorable job tasks and for firms that are plagued by agency problems. Agency problems should be more common in older firms that are less productive and have ceased growing, as well as in diversified firms and in firms with low wage dispersion, high free cash flows, and low leverage and where entrenched workers are paid high wages and have longer tenures in the firm.[18] Thus, we include the following variables in the propensity score estimation: the share of routine workers, the share of offshorable workers, the share of blue-collar workers, the share of unemployed workers at $t-4$ to $t-1$ and, separately, firm size, firm size growth from $t-1$ to t, firm age, fixed assets, sales, value added per employee, debt to assets, return on assets, average labor income for workers, the standard deviation of workers' labor income, average worker tenure, average worker age, and a dummy for whether a firm has establishments in several industries. In addition, Davis et al. (2014) and Boucly et al. (2011) use control firms of the same size and age and from the same year and industry.[19] We agree that these characteristics are important and match the predicted propensity scores within the strata of these variables.

Table A3 in the Appendix presents results for the propensity score match. This matching is performed one year before the buyouts are announced, and Column 1 provides some insights on how buyout targets differ from other firms. In contrast to the average firm in Sweden, target firms tend to employ workers who are younger, more likely to be offshorable but less likely to be blue collar and have higher labor incomes and lower tenure. Target firms also tend to have more employees, to be older and more diversified, and to

[18]These hypotheses are based on, among others, Jensen (1986), Shleifer and Summers (1988), Jensen (1989), Perotti and Spier (1993), Kaplan and Strömberg (2009), Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos (2009), and Norbäck, Persson, and Tåg (2013).

[19]Davis et al. (2014) performed a cell match for the industry, size, age, multi-unit status, and year of the buyout transaction, whereas Boucly et al. (2011) used industry, firm size and return on assets.

have a lower employment growth and leverage than the average firm in Sweden. Column 2 shows no such difference when the sample is restricted to the target firms and their matched control firms.

2.4 The final sample

Summary statistics on the buyouts in our final sample are displayed in Column 1 in Table 1. The final sample contains 239 buyouts involving 409 firms. This is 61% of the buyouts that we extracted from the Capital IQ database and Bergström et al. (2007). For comparison, Davis et al. (2014) matched 65% of the buyouts from Capital IQ to firm records, and Boucly et al. (2011) matched 70%.

The buyouts in our final sample resemble the buyouts from Capital IQ that we were able to find firm identifiers for (Column 3). Cross-border transactions, corporate divestitures, and management buyouts are the most common types of transactions, even if the transactions are weighted by employment (Column 2). The average transaction value in our sample is $124.5 million USD, and the number of buyouts is increasing over time. The transaction types and the time trend in Sweden are similar to buyouts in the United States (Column 4), the United Kingdom (Column 5), and France (Column 6). In all countries, the majority of the transactions are corporate divestitures or management buyouts. The major difference is that around 28% of buyouts in Sweden are cross-border transactions. Average transaction values are lower in Sweden than in the United States, but they are comparable to those in the United Kingdom and France. In all four countries, the number of buyouts has increased over time.

2.5 Assessing the control group

The matching procedure succeeds in ironing out observable average differences between the treated and matched control firms. Table 2 presents the average worker- and firm-level characteristics for the treated group, the matched control group, and a 10% random sample one year before the buyouts were announced.[20] The last columns present the difference between the treated and matched control group and the treated and random control group. At the worker level, the three groups are on average similar, and when we calculate the normalized difference, all values are below 0.25.[21] When comparing average values for firms, the treatment group and the matched control group are similar in statistical terms. Perhaps not surprisingly, there are large differences between the treated and random control firms. Firms targeted for a buyout are substantially larger than firms in the random sample in terms of average employment, fixed assets, and sales. In addition, targeted firms tend to be more diversified and have lower debt to assets before the buyout.

3 Analysis and results

3.1 Average effects

Figure 1 displays the average unemployment incidence for treated and control workers around the announcement of the buyouts.[22] Because all workers are employed at time $t = 0$, the unemployment incidence decreases in earlier time periods and is generally lower in the post period than in the pre-period.[23] Before the

[20] We create the random sample by taking a 10% random sample of firms for each year in our sample. The random sample is based on all limited liability firms in Sweden with more than one employee; we exclude firms that were targeted for mergers or acquisitions that we find in the BvD Zephyr database.

[21] Imbens and Wooldridge (2009) suggest using the normalized difference when the sample size is large. As a rule of thumb, a difference exceeding 0.25 is considered to be substantial.

[22] The figure displays normalized group-time mean values from a regression that omits the constant and includes the same controls as in Column 3 in Table 3.

[23] Average unemployment incidence does not reach zero at time $t = 0$ because Statistics Sweden measures the employment status on the first of November each year. Thus, workers could be unemployed before or after that date during a year.

announcement at time $t = 1$, both the trends and the levels in workers' unemployment incidence are similar in the treatment and control groups.

After the announcement of the buyouts, the trends in unemployment incidence remain close to parallel. On average, buyouts do not seem to affect the unemployment incidence for workers. Table 3 also shows that there is no evidence of an average effect on unemployment incidence. The estimated treatment effect in Column 1, i.e., the difference-in-differences estimate, is economically and statistically insignificant. Column 2 adds individual covariates, and Column 3 adds both individual and firm covariates. The inclusion of covariates improves the explanatory power of the model—the R^2 increases from 0.1% to 23.4%—but the estimated treatment effect remains statistically insignificant and economically small.

That buyouts seem to have no average effects on workers' unemployment incidence is consistent with previous firm-level evidence from the United States (Kaplan, 1989; Davis et al., 2014), the U.K. (Amess and Wright, 2010), and Sweden (Bergström et al., 2007) that documents no or modest employment effects after buyouts. However, an average treatment effect close to zero does not rule out that buyouts can affect group of workers differently. In the next subsections, we examine in detail if automation and offshoring can account for layoffs in buyouts.

3.2 Automation

3.2.1 Motivation and hypotheses

Investments in productivity-enhancing technologies after buyouts can lead to layoffs if they make workers redundant. Buyouts can trigger such investments if previous management avoided undertaking them because of widespread agency problems and because of a fear of worker backlash. In addition, buyouts can relax the financial constraints of the target firms and improve their management practices, which can also spur investments in productivity-enhancing technologies (Bloom et al., 2009; Boucly et al., 2011). Ample evidence exists that buyouts promote capital investments and improve the productivity of target firms. Boucly et al. (2011) show that capital expenditures increase after buyouts, and Agrawal and Tambe (2016) present evidence consistent with higher investments in information technologies after buyouts. Davis et al. (2014) show that private equity-backed firms increase productivity by shutting down less-productive establishments and opening up more productive ones, consistent with the productivity improvements documented in other studies (Kaplan, 1989; Lichtenberg and Siegel, 1990; Amess, 2003; Harris et al., 2005).

Investments in productivity-enhancing technologies are likely to affect some workers more than others. Recent evidence suggests that new technologies have reduced demand for workers performing routine tasks due to automation and increased demand for workers who perform non-routine tasks because these tasks are complementary to the new technologies (Acemoglu and Autor, 2011; Autor and Dorn, 2013; Goos et al., 2014). Goos et al. (2014) provide evidence from 16 European countries that this routine-biased technological change is important for explaining job polarization. In addition, routine-biased technological change has affected Sweden. Adermon and Gustavsson (2015) find that task-biased technological change has been increasing in Sweden since the 1990s, and Nilsson Hakkala, Heyman, and Sjöholm (2014) find that acquisitions by multinational firms increase the labor demand for non-routine job tasks.

Thus, based on the discussion above, we expect positive treatment effects for workers performing routine job tasks. The tasks these workers perform are substitutes for technological upgrades. Additionally, we expect negative treatment effects for workers who perform non-routine tasks because technological upgrades are complementary to the tasks that these workers perform. Finally, we expect a stronger pattern in target firms that lag behind their peers in productivity; such firms are more likely to be in need of technological upgrades.

3.2.2 Results

Table 4 presents results for how the unemployment incidence changes after buyouts in different subsamples of workers relating to automation. We divide workers into two groups based on the routine intensity of job tasks in their occupations (measured at $t = 0$). The routine intensity of job tasks in an occupation comes from the RTI index developed by Autor et al. (2003) and Autor et al. (2006, 2008). Goos et al. (2014) linked the RTI index with ISCO-88 codes, which can be mapped to the SSYK codes for occupations used by Statistics Sweden (Table A2 in the appendix outlines the mapping procedure we use). We define a worker as routine if his or her occupation has a positive RTI index value and as non-routine otherwise. We also divide firms into two groups on the basis of how productive they are at $t = 0$ relative to their peers. We define firms as low productive if their value added per employee is below the median in the industry in which they operate. Otherwise, we define them as high productive. Section A.3 in the Web Appendix shows that our results are robust to defining low-productive firms as those with negative productivity growth ex ante and to using value added per employee as a continuous term instead of transforming it into a dummy variable.

Columns 1 to 3 in Table 4 display results for routine and non-routine workers in all firms. Routine workers experience an increase in unemployment incidence by 1.3 percentage points (14.2%) on average in the four-year period after buyouts, but we observe no average decrease in unemployment incidence for non-routine workers. The increase for routine workers is only statistically significant at the 10% level, and the triple-difference estimate suggests that the estimated effects on routine and non-routine workers are not statistically different. Columns 4 to 6 repeat the exercise for workers employed in low-productive firms. In this subsample, routine workers experience an average increase in unemployment incidence by 10.2 percentage points (96.6%) after buyouts, and the increase is statistically significant at the 1% level. The estimated treatment effect on routine workers is statistically different from the estimated treatment effect on non-routine workers, as shown by the triple-difference estimate. The point estimate for non-routine workers is negative as expected, but it is neither precisely estimated nor economically large (-7.7%). Columns 7 to 9 reveal that the unemployment incidence for both routine and non-routine workers in high-productive firms remain unchanged after buyouts. Figure 2 accompanies Table 4 and shows the average unemployment incidence for routine workers and routine workers in low-productive firms. This figure allows us to examine the parallel trends assumption for the subsamples in which we find statistically significant effects. Both figures show parallel trends before $t = 0$ and a steady increase in unemployment incidence after $t = 0$ for treated workers.

These results confirm the hypothesis that routine workers are more likely to lose their jobs after buyouts, but we do not find evidence that non-routine workers benefit in terms of reduced unemployment incidence.

3.3 Offshoring

3.3.1 Motivation and hypotheses

One way for private equity firms to cut costs and improve profitability is to offshore operations. Offshoring refers to moving parts of the production chain abroad. Historically, offshoring was a phenomenon that was common only in the manufacturing industry. However, the rise of educated service workers in Asia has led to the spread of offshoring to other sectors of the economy (Blinder, 2006; Tambe and Hitt, 2012). Blinder and Krueger (2013) argue that approximately 25% of all jobs in the United States can be performed from abroad. As noted by Goos et al. (2014) and Blinder and Krueger (2013), not all offshorable job tasks are routine, and not all non-offshorable job tasks are non-routine, so treating the offshorability of job tasks as distinct from the routine intensity of job tasks makes sense.

Offshoring in the wake of buyouts can occur because buyouts align incentives between managers and owners and reduce agency problems in firms (Jensen, 1989; Kaplan, 1989; Leslie and Oyer, 2009; Kaplan and Strömberg, 2009). This alignment can result in a greater focus on cutting costs and on core activities.

Evidence suggests that private equity-backed firms have 40% fewer private jets than similar public firms (Edgerton, 2012), that they divest or close less productive pre-existing establishments (Davis et al., 2014), and that their patent portfolios tend to become more focused (Lerner et al., 2011). Harris et al. (2005) also note that one explanation for the occurrence of productivity improvements and reductions in labor intensity after buyouts could be the outsourcing of intermediate goods and services.

If offshoring after buyouts takes place, we can expect positive treatment effects for workers performing offshorable job tasks. In addition, we expect negative treatment effects for workers performing non-offshorable job tasks because offshoring allows firms to focus on the production of products and services that can be done in-house. Finally, as with routine intensity, we hypothesize that this pattern should be stronger for target firms that lag behind their peers in productivity. Such firms are more likely to require cost cutting and focus more on operations to improve profitability and productivity.

3.3.2 Results

Table 5 presents results for how the unemployment incidence changes after buyouts in different subsamples of workers relating to offshoring. This time, we divide workers into offshorable or non-offshorable workers. Whether a job is offshorable depends on the nature of the tasks. For instance, the work of a truck operator cannot be offshored, while the work of call center operators is easy to offshore. To classify occupations as offshorable or non-offshorable, we rely on Goos et al. (2014). They provide a classification of occupations mapped to ISCO-88 codes based on Blinder and Krueger (2013), who, in turn, code occupations based on the opinions of professionals regarding how easy it would be to offshore an occupation. In Section A.4 in the Web Appendix, we show that our results are robust to alternative measures of offshorability. Specifically, classifying occupations on the basis of the need for face-to-face communication (Firpo, Fortin, and Lemieux, 2011) and on the basis of actual offshoring of occupations (Goos et al., 2014).

Table A2 in the Appendix details the categorization of occupations into offshorable and non-offshorable that we use in the main analysis and highlights that not all offshorable jobs are routine and that not all non-offshorable jobs are non-routine: the correlation between the offshorability index and the RTI index is only 0.3 at the individual level in our sample. Indeed, our results are robust to excluding offshorable workers from the routine regressions and routine workers from the offshorable regressions (see Section A.5 in the Web Appendix).

Columns 1 to 3 in Table 5 display regressions on subsamples of offshorable and non-offshorable workers for all firms. There is no evidence of any change in workers' unemployment incidence after buyouts in any of the groups, and they do not change in relation to each other either. Only in Columns 4 to 6, which restrict attention to low-productive firms, do we estimate any effects. In this subsample, offshorable workers experience an average increase in unemployment incidence by 8.6 percentage points (97.0%) in the four-year period after buyouts, and the increase is statistically significant at the 1% level. The point estimate for non-offshorable workers is negative as expected, but it is neither precisely estimated nor economically large (-1.8%). The triple-difference estimate shows that the estimates for offshorable and non-offshorable workers are statistically different from each other. Columns 7 to 9 reveal that the unemployment incidence for both offshorable and non-offshorable workers in high-productive firms remain unchanged after buyouts. Figure 3 accompanies Table 5 and shows the average unemployment incidence for offshorable workers in all firms and in only low-productive firms. Both figures show that treated and control workers have parallel trends before $t = 0$ and an increase in unemployment incidence after $t = 0$ for treated workers. However, the increase is marked only for the subsample of offshorable workers in low-productive firms.

These results provide new insights regarding cost-cutting and focusing efforts after buyouts. The media often cover layoffs that result from offshoring in the wake of buyouts.[24] However, we are unaware of any

[24] A well-known example is Bain Capital, which was accused of offshoring American jobs to low-cost countries, such as China.

direct or indirect empirical evidence that indicates that offshoring after buyouts occurs systematically. The results in Table 5 provide indirect evidence that offshoring in low-productivity firms occurs and indicates that offshorable workers lose out.

4 Additional analyses

4.1 Firm-level evidence of automation and offshoring

In this section, we study firm-level outcomes for evidence of automation and offshoring. In sharp contrast with our worker-level analysis, our firm-level analysis could suffer from attrition bias. As discussed by Davis et al. (2014), buyouts are often associated with complex restructuring, such as internal organization, acquisitions, and divestitures, which can make it difficult to track target firms over time.[25] As Figure A1 in the Appendix shows, the treated firms leave the sample to a higher degree than the control firms over time. The higher attrition rate among the treated firms makes us less confident that the firm-level treatment effects can be interpreted as causal, so we urge the reader to interpret these results with care.

We estimate the following model on different dependent variables:

$$Y_{ftr} = \alpha + \pi_t + \gamma LBO_f + \sum_{\tau=1}^{m} \beta_{-\tau}(T_{t-\tau} \times LBO_f) + \sum_{\tau=1}^{q} \beta_{+\tau}(T_{t+\tau} \times LBO_f) + F_f\delta + \lambda_r + \varepsilon_{ftr}, \tag{3}$$

where Y_{ftr} is the dependent variable for firm f at normalized time t in calendar year r. The model includes m lags (β_{-1}, β_{-2}) to capture anticipatory effects (which should all be zero if the groups have parallel trends before the buyouts), and q leads ($\beta_{+1}, \beta_{+2}, \beta_{+3}, \beta_{+4}$) to capture post-treatment effects. The estimates for the leads and lags are relative to time $t = 0$. We include firm controls ($F_f^{'}$) to increase the efficiency and to control for common industry effects. The term λ_r represents calendar fixed effects.[26] To better link the worker-level effects to the firm-level effects, we weigh all of the firm-level models with the size of the firm the year before the buyout announcement ($t = 0$).

We first consider capital investments in intangible assets. Automation and offshoring are both complementary to IT investments by the firm, and the stock of intangible assets in the firm proxies for such investments. We use the accounting item "Other Intangible Assets," which includes corporate intellectual property, such as software, that can be used for automation and for handling operations abroad.

Columns 1 to 3 in Table 6 display estimates for the stock of intangible assets in the firm using Eq 3. Before $t = 0$, there are no statistically significant differences between the treated and control groups. Consistent with automation and offshoring in low-productive firms, however, we observe a statistically significant increase in intangible assets after buyouts in these firms, beginning with $t + 2$ and continuing to $t + 4$ (Column 2). Four years out, the point estimate suggests a 4,840,000 SEK increase in intangible assets on average for the treated firms relative to time $t = 0$, representing an almost 16-fold increase in other intangible assets in the treated firms.

Next, outsourcing decisions often involve selling off production assets locally and moving them abroad. Our data allow us to directly measure offshoring by creating a variable that takes the value of one if the firm is part of a group that has subsidiaries in a low-wage country and zero otherwise. A drawback of this measure, however, is that, while it picks up establishing new subsidiaries that perform production abroad, it does not pick up offshoring that occurs through buying services or production from external suppliers abroad

This criticism spilled over to the former Bain Capital CEO, Mitt Romney, in his presidential campaign in 2012. (See, for example, the Reuters article "Buyouts-Controversial Sensata a textbook case for Bain Capital" published on July 26, 2012).

[25] To mitigate the attrition bias, we also use FAD codes instead of firm legal identifiers when following firms over time. See the description in Table A1.

[26] The subindex r represents the actual calendar year ($r = 1997, 1998, ..., 2011$), in contrast with the subindex t, which represents the normalized time period ($t = -4, -3..., 4$).

(i.e., call center services from India or intermediate products from China instead of production in-house). Becker, Ekholm, and Nilsson Hakkala (2010) report that the most common countries to which Swedish firms tend to offshore are China, Poland, Brazil, Mexico, Estonia, the Czech Republic, India, Lithuania, Russia, Latvia, Hungary and Romania; therefore, we denote these countries as low-wage countries.

Columns 4 to 6 in Table 6 display the results. Before $t = 0$, there are no statistically significant differences between the treated and control groups. While there is a clear pattern of higher point estimates for the period $t+2$, $t+3$, and $t+4$ relative to the pre-period in the sample of low-productive firms (Column 5), the coefficients are imprecisely estimated and not statistically significant at the 10% level. There are no clear patterns for the full sample and for the sample of high-productive firms.

Finally, we consider productivity improvements after buyouts. We expect firms that undertake investments in automation and offshoring to become more productive (at least in the long run) as a result of these investments. Columns 7 to 9 in Table 6 display firm-level results for value added per employee. As for intangible assets and the presence of subsidiaries in low-wage countries, there are no statistically significant differences between the treated and control groups in productivity before $t = 0$. Consistent with long-run improvements in low-productive firms (Column 8), we observe statistically significant increases in value added per employee for $t+3$ and $t+4$. At $t+4$, value added per employee is 205,000 SEK higher, an 88% increase relative to the pre-period mean for treated firms.

4.2 Bargaining with labor unions

Existing literature suggests that a possible rationale for layoffs after buyouts unrelated to automation and offshoring is tougher bargaining with labor unions. Buyouts increase leverage and align the incentives of managers and owners (Jensen, 1989; Axelson et al., 2013).[27] Higher leverage and stronger incentives for management both contribute to improving the bargaining power of a firm with respect to labor unions. First, higher leverage works as a commitment device (Perotti and Spier, 1993). When leverage increases, the firm can credibly threaten to stop undertaking new investments unless the wage bill is reduced; otherwise, the firm goes bankrupt.[28] Second, stronger incentives for managers and an ownership change can break implicit collusive agreements between managers and workers (Shleifer and Summers, 1988; Pagano and Volpin, 2005). Pagano and Volpin (2005) argue that managers sometimes align themselves with workers to fend off hostile takeovers. This strategy can help managers keep their jobs. Managers reward workers with higher pay and other benefits. In turn, workers protest and increase their voice in the media if hostile takeovers become imminent. Such collusive agreements are likely useful for managers if unions are aggressive and ready to fight takeovers because workers have less to offer managers if they are not associated with aggressive labor unions.[29]

[27] Axelson et al. (2013) provide cross-country evidence for leverage associated with buyouts between 1986 and 2008. The average buyout in their sample raised 69% of the capital for the transaction through debt, resulting in an average debt-to-EBITDA ratio of 5.6. Buyouts in the United States in the 1980s resulted in a fourfold increase in management ownership (Kaplan, 1989). In addition, Kaplan and Strömberg (2009) analyze a sample of buyouts in the United States between 1996 and 2004. In their sample, the management team obtained an average equity upside of 16%.

[28] Empirical evidence suggests that corporate leverage is related to union bargaining power. For example, Bronars and Deere (1991) show that firms under the threat of union action choose to maintain higher debt-equity ratios, and Rosett (1990) show that wealth concessions made by unions can explain approximately 3% of the shareholder gains in hostile takeovers over a six-year period. Moreover, Matsa (2010) document that higher union coverage affects capital structure, and Benmelech et al. (2012) provide empirical evidence that financial distress in airline companies is related to pension underfunding and wage concessions.

[29] Cronqvist et al. (2009) provide evidence from Sweden that entrenched managers pay their workers more. This effect is stronger in industries where unions are more inclined to conflict. In addition, the collusion argument is supported by Rauh (2006), Kim and Ouimet (2014), and Atanassov and Kim (2009).[30] Finally, Ippolito and James (1992) present evidence that the buyouts in the United States in the 1980s were associated with pension terminations. Pensions can be seen as implicit promises of future payouts, which suggests that breaches of implicit contracts occurred, as hypothesized by Shleifer and Summers (1988).

Thus, tougher bargaining with labor unions could explain layoffs after buyouts. Consequently, we expect a positive treatment effect for workers affiliated with more aggressive unions. When unions are more aggressive, managers are more likely to avoid confrontation when strong incentives are absent, and they are more likely to collude with workers. In addition, we expect the treatment effect to be larger if the firm has low leverage in relation to its peers. Low leverage ex ante means that the increase in leverage and, thus, the beneficial effect on bargaining power will increase.

Swedish registry data do not contain information regarding union affiliation. Thus, we use the method of Cronqvist et al. (2009) and divide the workers based on their industry affiliation. Lindberg (2006) document that there are six highly conflict-inclined unions in Sweden, including the Swedish Electricians Union, the Swedish Painters' Union, the Swedish Builders Workers' Union, the Swedish Transportation Workers' Union, the Swedish Dock Workers' Union, and the Syndicalists. As demonstrated by Cronqvist et al. (2009), we code workers in the construction industry (SNI code 45) and the transportation industry (SNI code 60-63) as members of aggressive labor unions. Blue-collar workers in the construction industry tend to belong to the Swedish Electricians Union, the Swedish Painters' Union, or the Swedish Builders Workers' Union. Blue-collar workers in the transportation industry tend to belong to the Swedish Transportation Workers' Union, the Swedish Dock Workers' Union or the Syndicalists.

Columns 1 to 3 in Table 7 display regressions on subsamples of blue-collar workers in industries with aggressive and non-aggressive labor unions for all firms. There is no clear evidence of any change in unemployment incidence after buyouts for any of the groups, and they do not change in relation to each other, either. This is also true for Columns 7 to 9, which focus on firms with ex ante high debt. Only in Column 4, which restricts attention to firms with low debt ex ante, do we estimate any effect. Blue-collar workers associated with aggressive labor unions experience an increase in unemployment incidence by on average 1.3 percentage points (36.6%) in the period after buyouts, and the increase is statistically significant at the 10% level. However, the triple-difference specification shows that estimated treatment effects are not statistically different between workers affiliated with aggressive and non-aggressive unions (Column 6). Moreover, Figure 4, which accompanies Table 7, does not provide evidence consistent with a large treatment effect. The figure shows the average unemployment incidence for blue-collar workers affiliated with aggressive labor unions workers and for blue-collar workers affiliated with aggressive labor unions in low-debt firms. Both figures show fairly parallel trends before $t = 0$ but no clear increases in unemployment incidence after $t = 0$ for treated workers. As such, we conclude that the evidence in support of the hypothesis that tougher bargaining with labor unions could explain layoffs after buyouts is markedly weaker than the evidence that automation and offshoring are associated with layoffs.

4.3 Unemployment days, job changes, and wages

In this section, we analyze if buyouts affect workers on margins other than the unemployment incidence. We begin by estimating Equation 1 using total days of unemployment as the dependent variable. Any effect on total days of unemployment will combine the effect on the incidence and the duration.[31] Columns 1 to 3 in Table 8 present results for total days of unemployment. There are no statistically significant increases in the number of days of unemployment after buyouts for the full sample (Column 1), but in low-productive firms, we estimate a 13.9-day (147.8%) average increase per year for treated routine workers relative routine control workers and a 11.9 day (142.2%) average increase per year for treated offshorable workers relative to offshorable control workers.[32] The triple-difference specification suggests that these estimated effects are

[31]We do not analyze the unemployment duration because an effect on the incidence in a difference-in-differences setting can change the composition of the sample that has positive durations and thereby bias the estimate. By analyzing total days of unemployment, the sample of individuals is kept constant over time, and we avoid such a bias.

[32]These effects are not present in high-productive firms and are driven by increases in the subsample of routine and offshorable workers.

statistically different from the estimated effects for non-routine and non-offshorable workers.

Next, we turn to flows of workers. The employer-employee link in the LISA database allows us to analyze movements of workers between firms or from firms into non-employment (or vice-versa) between Novembers in two consecutive years. Table WA6 in the Web Appendix reveals that we estimate no statistical treatment effects on total movement between two consecutive years. Total movement is defined as a binary outcome variable that takes the value of one if a worker has moved between firms or from a firm into non-employment (or vice-versa) in two consecutive years and zero otherwise. However, our data allow us to distinguish between workers who move to a new firm in two consecutive years with or without having any unemployment days in between. We do not estimate any statistically significant treatment effects for workers moving between firms without having any unemployment in between. However, turning to movement between firms with an unemployment spell in between, we estimate that treated routine workers in low-productive firms, on average, are twice as likely to do this than similar workers in control firms. These two regressions are reported in Column 5 in Table 8 and in Table WA6 in the Web Appendix. Running a triple-difference model reveals that the estimated effect for treated routine workers is statistically different from non-routine workers who experience no change in such movements. Column 6 in Table 8 reveals the same pattern for treated offshorable workers in low-productive firms relative to similar control workers.[33] These results suggest that buyouts can trigger involuntary movements, rather than voluntary movements, among workers.

Finally, we look at how yearly labor income is affected after buyouts. Any treatment effects we find in these regressions will be a combination of income effects from moves to unemployment, from job changes and from earnings changes at the buyout-targeted firms. Columns 7 to 9 in Table 8 indicate that average labor income is unchanged for the full sample (Column 7), but we estimate a drop of 12.74% in labor income among routine workers in low-productive firms relative to similar control workers (Column 8). However, we estimate no statistically significant change in labor earnings for treated offshorable workers in low-productive firms relative to similar control workers (Column 9). Since our estimates show that the treatment effects on unemployment for routine and offshorable workers are similar, the drop in labor income for routine workers is likely to be the outcome of a relatively lower labor income at the new job.

In total, the results in Table 8 confirm that routine and offshorable workers in low-productive firms are, on average, worse off after buyouts. It seems, however, that unemployment after buyouts is not long term. Instead, most workers that enter unemployment find a new job within a year.

4.4 Comparison to Davis et al. (2014)

In this section, we compare the employment effects of buyouts in Sweden to those of the United States, as reported by Davis et al. (2014). We focus on replicating as close as possible Table 4 in Davis et al. (2014) by analyzing employment responses for different adjustment margins: growth by continuing establishments, shutdowns of establishments, births of establishments, divestitures of establishments, and acquisitions of establishments. The employment growth rate is calculated from the buyout year to two years after.[34] The employment growth rate is $g_{ft} = (E_{ft+2} - E_{ft})/(0.5 \times (E_{ft+2} - E_{ft}))$, where E_{ft} is the employment level at establishment or firm f at time t. In line with Davis et al. (2014), we estimate the difference in the growth rate from time t to time $t+2$ between treated and controls, where the regression model includes industry effects, firm age effects, year effects, firm size at t, pre-employment growth from time $t-2$ to time $t-1$, and a dummy variable indicating a multi-unit firm.[35]

[33] We find no statistical treatment effects on the probability of moving into non-employment in two consecutive years.

[34] In our main analysis, time is defined relative to the announcement year of the buyout (which is one year before the buyout). In practice, this means that we will analyze the employment growth rate from time $t+1$ to time $t+3$ in this section.

[35] Davis et al. (2014) use a more flexible model by fully interacting the effects. Our relatively smaller sample of buyouts does not allow us to follow this strategy.

The identification strategy here is thus somewhat different from the strategy in the main analysis. Here, we compare the post-difference in outcomes between treated and control and include pre-employment growth at the firm level in the regression models. In our main analysis, we compare the relative change in the difference between treated and controls over time (i.e. a difference-in-differences strategy) that explicitly account for pre-treatment trends.

Table 9 shows that, in Sweden, the average employment growth rate over a two-year period is 3.49 percentage points higher in target firms than in control firms. In the United States, the growth rate in target firms is -0.88 relative to controls after buyouts. Decomposing the growth rate into different adjustment margins reveals substantial differences between Sweden and the United States. In Sweden, most of the employment growth occurs in continuing establishments, but in the United States, continuing establishments have a negative growth rate relative to controls. In addition, in the United States acquisition of establishments has a relatively large positive effect on growth, while it has a small impact in Sweden. Another difference is that shutdowns of establishments ("Deaths") are common in targeted firms in the United States, but not in Sweden. In fact, the estimate for "Deaths" in Sweden suggests that shutdowns of establishments are less likely in treated firms than in controls.

We next split the sample into low-productive firms (Column 3) and high-productive firms (Column 4). In low-productive firms, the average two-year growth rate is 3.41 percentage points lower in targeted firms than in control firms. In more-productive firms, the average two-year post growth rate in targeted firms outpaces control firms by 5.33 percentage points. Decomposing the growth rate into different adjustment margins reveals a generally lower growth rate after buyouts for treated low-productive firms relative to their controls on most margins. The lower growth rate in treated firms stems from less growth in continuing establishments, more shutdowns, and less acquisitions of establishments. The only margin in favor of treated firms is divestitures. In high-productive firms, continuing establishments account for a substantial part of the overall growth relative to controls after buyouts. In addition, shutdowns of establishments are less common in treated firms than in control firms, but growth through births of establishments is more common. These results are consistent with the interpretation that less-productive firms are targets of buyouts that generally tend to focus more on restructuring (as documented by Davis et al. (2014) for the United States), whereas more-productive firms are targets of buyouts that generally focus more on expansion (as documented in France by Boucly et al. (2011)). It is important to keep in mind, however, that private equity transactions are heterogeneous and undertaken for a variety of reasons, so categorizing transactions into these two bins is a major simplification.

There are also apparent industry differences in growth rates in Sweden compared to the United States. Davis et al. (2014) find that the buyout effect on the firm employment growth rate varies between industries. In the United States, the employment growth in targeted firms falls modestly in the manufacturing industry, falls by 12 percentage points in the retail and trade industry, and grows more slowly in the service sector. In Sweden, treated and controls show no significant difference in the growth rates after a buyout in the manufacturing industry and in the retail and trade industry (see Table A4 in the Appendix). However, the growth rate for treated firms in the Swedish service industry outpaces the growth in control firms by 3.5 percentage points in the two-year period after buyouts. These industry differences between the United States and Sweden suggest that the effects of buyouts on workers may differ not only between industries within a country but also within industries between countries.

4.5 Do private equity firms contribute to job polarization?

The analysis so far has been about how we can understand layoffs after buyouts through the lens of the job polarization literature. However, another analysis of interest—one that is much harder to undertake—focuses on the extent to which private equity firms have contributed to the job polarization process.

Apparently, layoffs in private equity buyouts are concentrated in occupations in the middle part of the wage distribution. These are occupations that have lost out to occupations in the tails of the wage distribution in terms of occupation shares in many countries (Goos et al., 2014). Table 10 splits workers in our sample according to the categorization of occupations in Table 1 in Goos et al. (2014). Columns 1 to 3 show that workers in the middle part of the wage distribution experience an average increase in unemployment incidence by 1.4 percentage points (17.3%) after buyouts and that there is no average change in unemployment incidence for workers in occupations in the tails of the wage distribution. However, the triple-difference estimate in Column 3 suggests that the estimates of treatment effects for workers in the middle and in the tails of the wage distribution are not statistically different from each other. The effects in Column 1 and 3 are markedly amplified when we restrict attention to low-productive firms (Columns 4 to 6). For low-productive firms, workers in middle-wage occupations observe a statistically significant increase of 9.1 percentage points (105.0%), and here, the triple difference shows a statistically significant 9.5-percentage-point increase in unemployment incidence for workers in the middle-wage occupations relative to workers in high- or low-wage occupations.

However, one cannot directly take our results on unemployment incidence as evidence that private equity firms are contributing to the job polarization process. Moves to unemployment due to layoffs do not necessarily imply that workers switch occupations and, thus, contribute to shifting occupation shares of employment over time (the measure of polarization used by, among others, Goos et al. (2014)). To get a rough measure of how much private equity firms are contributing to the polarization process, we can study how the job polarization pattern in Sweden changes if we exclude workers that are affected by buyouts.

Columns 1 to 3 in Table 11 display the change in employment shares in low-wage, middle-wage, and high-wage occupations in Sweden for the period from 2001 to 2011. Column 1 displays the numbers for all workers, Column 2 for all workers employed at some point in firms targeted for a buyout during the period, and Column 3 for the remaining workers. Column 4 calculates the percentage change between Column 1 (including workers affected by buyouts) and Column 3 (excluding workers affected by buyouts).

Column 1 reveals a weak pattern of polarization, with the employment share of high-wage occupations increasing, middle-wage occupations decreasing, and low-wage occupations slightly decreasing. Column 2 shows that the polarization pattern is more pronounced if we focus attention on workers involved in a buyout at some point during 2001 to 2011. Here, both low-wage occupations and high-wage occupations are increasing their employment shares, while middle-wage occupations are decreasing their shares. Column 3 presents a pattern close to that of Column 1 for middle- and high-wage occupations, underscoring that excluding workers involved in buyouts has no major effects on these margins. The impact of buyouts seems to be somewhat larger in low-wage occupations, where excluding workers in private equity-backed firms results in a 15.38% lower employment change. Given that private equity buyouts affect only 2.7% of employment in Sweden, the impact on the employment change in low-wage occupations is relatively large. To quantitatively analyze the impact of workers involved in private equity buyouts on the job polarization pattern, the last two rows in Table 11 display a polarization index for each of the columns. This index captures the percentage change in the ratio of employment among low-wage occupations relative to middle-wage occupations (second to last row) and among high-wage occupations relative to middle-wage occupations (last row). Excluding workers involved in buyouts reduces the polarization index at the low end by 6.48% and at the high end by 0.95%.

Overall, these back-of-the-envelope calculations reveal a clear job polarization pattern for workers affected by buyouts. However, because buyouts affect a small fraction of employees in Sweden, they seem to have had a minor impact on job polarization in the economy at large.

5 Concluding remarks

What can explain layoffs after private equity buyouts? In this paper, we use rich registry data from Sweden to answer this question. We argue that layoffs in private equity buyouts can be understood through the lens of the job polarization process. Job polarization refers to the notion that employment shares of low- and high-wage occupations has increased over time relative to middle-wage occupations. The automation of routine jobs due to technological progress and the globalization of product and labor markets are two possible mechanisms driving this pattern. We argue that these same explanations could be important for understanding layoffs after private equity buyouts. Private equity firms reduce agency costs, which triggers investments that automate routine jobs and that lead to offshoring. Using detailed worker-level data, we present evidence in support of these two explanations. Workers in less-productive firms performing routine or offshorable job tasks double their unemployment incidence after buyouts.

A limitation of our analysis is that we are confined to analyzing effects within the borders of Sweden. A total of 153 out of the 239 buyout targets in our sample had a presence abroad at the time of the buyout. Labor force changes within Sweden may have implications abroad that we are not able to account for. Moreover, our data do not contain information on which workers emigrate or immigrate. We can, however, say with confidence that compositional changes in the treatment and control groups as a result of moves abroad are not a major issue. Four years after the announcement of the buyout, 98.71% of the treated workers and 98.75% of our control workers remain in the sample. The percentages are roughly the same for workers in low-productive firms.

For some groups of workers, the economic significance of being employed in buyout-targeted firms is large. One example of another kind of shock that affected workers during our sample period is the global financial crisis, which led to a 5.7% reduction in the Swedish gross domestic product between 2007 and 2009. The unemployment rate increased by approximately three percentage points (50.8%) between 2007 and 2009. A back-of-the-envelope calculation suggests that buyouts affected some groups of workers to a greater extent than the financial crisis. The unemployment incidence for routine workers employed in low-productive firms in 2007 increased by 6.7 percentage points (73%) between 2007 and 2009. In comparison, the estimated treatment effect from buyouts was 10.2 percentage points (97%). For workers in offshorable occupations in low-productive firms, the unemployment incidence increased by 7.9 percentage points (102%) between 2007 and 2009. In comparison, we estimate a treatment effect of buyouts of 8.6 percentage points (97%). However, the aggregate economic effects of the moves to unemployment are small because buyouts affect such a small fraction of all workers in the economy (2.7% in our sample period).[36] Moreover, on average, we do not find any changes in unemployment incidence from buyouts. Thus, while the economic significance of the unemployment effects for the affected groups of workers are economically large, they are not economically significant at the country level.

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[36]Our estimates for routine and offshorable workers in low-productive firms imply that buyouts lead to approximately 250 to 330 more worker unemployment spells per year during a four-year post period. Therefore, in the four-year post period, the total number of worker unemployment spells caused by buyouts ranged between 1,000 and 1,320. Another way to think about the overall impact is that the total number of unemployment days increases by approximately 11.9 days for offshorable workers and approximately 13.9 days for routine workers in this sub-sample. Therefore, the aggregated number of total days of unemployment amounts to approximately 136,000 to 181,000 days during the entire post period. These numbers appear huge, but they have a very small impact on the overall unemployment in the Swedish economy. During the 2002 to 2011 period, on average, 4.3 million workers per year had 6.6 total unemployment days each, which results in more than 28 millions days of unemployment. Therefore, the additional unemployment for routine and offshorable workers in low-productive firms affected by buyouts amounts to approximately 0.6% of total unemployment days per year.

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Table 1: Descriptive statistics

Sample	Final (1)	Weighted (2)	Sweden (3)	US (4)	UK (5)	France (6)
Panel A: Transaction types (non-exclusive)						
Going private	2.5%	9.5%	2.0%	4.8%	4.2%	1.1%
Corporate divestiture	30.5%	41.8%	35.4%	29.7%	29.8%	16.9%
Bankruptcy	0.8%	0.1%	1.5%	3.8%	4.2%	2.8%
Management buyout	20.1%	10.6%	21.9%	22.4%	57.1%	34.3%
Family succession	5.9%	3.8%	4.4%	4.2%	4.4%	8.3%
Cross border	28.5%	43.2%	27.8%	5.8%	11.2%	17.0%
Platform	2.5%	10.3%	3.2%	8.8%	2.3%	2.4%
Panel B: Transaction Values						
Average (million USD)	134.5	-	124.1	425.2	223.0	217.8
Standard deviation (million USD)	344.2	-	322.3	2,118.8	937.3	567.3
Panel C: Transactions						
2002	22	-	32	702	316	133
2003	22	-	28	757	404	170
2004	26	-	42	918	374	230
2005	29	-	46	975	408	299
2006	47	-	65	1,233	615	321
2007	53	-	69	1,432	672	375
2008	40	-	60	1,137	616	371
Total	239	-	342	7,154	3,405	1,899

NOTE. — This table reports the descriptive statistics for the buyouts in the final sample (Column 1), employment weighting in the final sample (Column 2) and based on the buyouts in Capital IQ that were matched to firm identifiers (Column 3). See Appendix B for a description of the sample selection process from Columns 3 to 1. The final three columns report descriptive statistics for the buyouts undertaken during the same time period in the United States (Column 4), in the United Kingdom (Column 5), and in France (Column 6) based on data from Capital IQ. Panel A displays the buyouts per transaction type (not mutually exclusive), Panel B displays the average and standard deviations of the transaction values (in millions of United States dollars at a nominal rate), and Panel C displays the distribution of buyouts over time.

Table 2: Descriptive statistics on workers and firms as of time $t = 0$

	Treated		Control		Random		Differences	
	Mean	SD	Mean	SD	Mean	SD	T-C	T-R
Panel A: Workers								
Age	41.13	11.88	41.67	11.87	40.11	12.76	-0.55	0.14
Wage	275.43	164.74	274.65	175.00	259.62	188.27	0.87	5.98
Tenure	4.75	4.77	4.73	4.83	4.27	4.69	0.01	-0.06
Routine job	0.42	0.49	0.44	0.50	0.49	0.50	-0.02	-0.07
Offshorable job	0.41	0.49	0.42	0.49	0.42	0.49	0.00	-0.01
Blue collar job	0.59	0.49	0.55	0.50	0.53	0.50	0.04	0.06
Unemployment$_{t-1}$	0.08	0.27	0.07	0.26	0.11	0.31	0.01	-0.01
Unemployment$_{t-2}$	0.10	0.30	0.09	0.28	0.12	0.33	0.02	-0.01
Unemployment$_{t-3}$	0.11	0.31	0.10	0.30	0.14	0.35	0.01	-0.03
Unemployment$_{t-4}$	0.13	0.33	0.12	0.33	0.17	0.38	0.01	-0.04
Observations	42,391		42,273		1,273,667			
Panel B: Firms								
Employment	125.26	274.73	126.11	302.58	20.45	208.01	-0.85	104.81***
Employment growth	0.03	0.21	0.04	0.17	0.04	0.27	-0.01	-0.01
Age	9.67	6.77	9.73	6.83	7.54	6.47	-0.05	2.13***
Fixed assets	51.61	264.66	65.90	516.09	12.57	216.48	-14.30	39.04***
Sales	216.24	395.74	213.28	496.44	42.81	512.40	2.95	173.43***
Value added per employee	0.73	0.97	0.69	0.69	0.56	4.74	0.04	0.17
Diversified	0.06	0.24	0.07	0.25	0.01	0.09	-0.01	0.05***
Debt to assets	0.11	0.18	0.12	0.18	0.17	0.26	-0.02	-0.06***
Return on assets	0.12	0.35	0.13	0.17	0.11	0.33	-0.01	0.01
Observations	409		409		51,050			

NOTE. — This table reports the descriptive statistics for the treated sample, the control sample, and for a sample of random firms. The descriptive statistics refer to the year before the buyout announcement ($t = 0$). The random sample is constructed by randomly drawing a 10% sample of firms for each year. The final columns report average differences between the treated and control groups (T-C) and the treated and random (T-R) groups. Because our sample size is large, we calculate the normalized difference as recommended by Imbens and Wooldridge (2009). Imbens and Wooldridge (2009) suggests that a difference exceeding 0.25 is substantial. All normalized differences in the worker sample are below 0.25. For firms, (***) indicates a statistically significant difference at the 1% level using a standard t-test of difference in means. Variable descriptions are available in Table A1. Fixed assets, sales, and value added per employee are given in millions of 2005 SEK (the USD/SEK exchange rate on June 30, 2005 was 7.6).

Table 3: Average effects

Dependent variable	Unemployment incidence		
	(1)	(2)	(3)
Post x LBO	0.006	0.005	0.005
	(0.007)	(0.006)	(0.006)
LBO	0.010	0.002	0.001
	(0.008)	(0.004)	(0.004)
Post	-0.018***	-0.020***	0.012**
	(0.004)	(0.003)	(0.005)
Constant	0.079***	0.038***	0.154***
	(0.006)	(0.014)	(0.046)
Individual level controls	No	Yes	Yes
Firm level controls	No	No	Yes
R^2	0.001	0.233	0.238
Percent change	6.3%	6.1%	6.1%
Observations	756,684	756,684	756,684

NOTE. — This table reports estimates and their standard errors (in parentheses) from three difference-in-differences specifications. Unemployment incidence is a dummy taking the value of one for workers with at least one day of unemployment during a given year and zero otherwise. "Post" is a dummy taking the value of one during the year of the buyout announcement and all years after, and zero otherwise. "LBO" is a dummy taking the value of one for treated workers that are employed in targeted firms at time $t = 0$ and zero for control workers. "Post x LBO" is the difference-in-differences estimate. The regressions cover nine years, including four pre-periods, four post-periods, and the year of the buyout announcement. The individual controls measured at $t = 0$ are age, age squared, gender, labor income, education fixed effects, and tenure. In addition, the model controls for match-specific dummies and five dummies representing whether a worker has at least one day of unemployment at time $t = 0$, $t = -1$, $t = -2$, $t = -3$, and $t = -4$. The firm controls measured at $t = 0$ are size, size squared, size growth from $t = -1$ to $t = 0$, firm age, firm age squared, and county fixed effects. Variable descriptions are available in Table A1. The percentage change is the coefficient on $POST \times LBO$ divided by the average unemployment incidence for treated workers in the pre period before the announcement of the buyout. The standard errors are clustered at the corporate group times municipality level, and we denote statistical significance at the 5% level by ** and at the 1% level by ***.

Table 4: Automation

Dependent variable	Unemployment incidence								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All firms			Low productive firms			High productive firms		
	Routine workers	Non-routine workers	All workers	Routine workers	Non-routine workers	All workers	Routine workers	Non-routine workers	All workers
Post x LBO	0.0133*	0.0003	0.0002	0.1020***	-0.0109	-0.0104	-0.0049	0.0052	0.0050
	(0.0079)	(0.0065)	(0.0068)	(0.0245)	(0.0124)	(0.0132)	(0.0073)	(0.0053)	(0.0054)
LBO	-0.0014	0.0022	0.0018	0.0036	0.0211*	0.0201*	-0.0000	-0.0062**	-0.0064**
	(0.0042)	(0.0045)	(0.0046)	(0.0169)	(0.0115)	(0.0112)	(0.0045)	(0.0029)	(0.0030)
Post	0.0061	0.0167***	0.0107**	-0.0437**	0.0268***	0.0121	0.0171**	0.0092*	0.0138**
	(0.0072)	(0.0052)	(0.0052)	(0.0184)	(0.0101)	(0.0089)	(0.0071)	(0.0054)	(0.0054)
Post x LBO x Routine			0.0126			0.1130***			-0.0100
			(0.0096)			(0.0259)			(0.0083)
Constant	0.1680**	0.1550***	0.1480***	0.2620**	0.0721	0.0942*	0.1470*	0.2020***	0.2020***
	(0.0712)	(0.0505)	(0.0442)	(0.1280)	(0.0515)	(0.0564)	(0.0759)	(0.0424)	(0.0413)
R^2	0.236	0.242	0.238	0.228	0.248	0.241	0.239	0.235	0.238
Percent change	14.2%	0.4%		96.8%	-7.7%		-5.4%	10.3%	
Observations	327,463	429,221	756,684	55,147	174,294	229,441	272,316	254,927	527,243

NOTE. — This table reports estimates and their standard errors (in parentheses) from six difference-in-differences models and three triple-differences models related to automation. ”Post x LBO” is the difference-in-differences estimate, and ”Post x LBO x Routine” is the triple-difference estimate. All specifications include the individual- and firm-level controls included in Column 3 in Table 3. Low-productive firms are defined as firms with a value added per employee at $t = 0$ below the median in the industry in which they operate. The other firms are defined as high-productive firms. Workers are categorized into routine and non-routine workers based on Table A2. The standard errors are clustered at the corporate group times municipality level, and we denote statistical significance at the 1% level by ***, statistical significance at the 5% level by **, and statistical significance at the 10% level by *.

Table 5: Offshoring

Dependent variable	Unemployment incidence								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All firms			Low productive firms			High productive firms		
	Offshorable workers	Non-offshorable workers	All workers	Offshorable workers	Non-offshorable workers	All workers	Offshorable workers	Non-offshorable workers	All workers
Post x LBO	0.0088	0.0033	0.0034	0.0862***	-0.0026	-0.0024	-0.0055	0.0060	0.0059
	(0.0061)	(0.0067)	(0.0069)	(0.0165)	(0.0128)	(0.0136)	(0.0065)	(0.0060)	(0.0059)
LBO	-0.0031	0.0038	0.0033	-0.0016	0.0197*	0.0197*	-0.0020	-0.0047	-0.0060*
	(0.0036)	(0.0051)	(0.0050)	(0.0113)	(0.0119)	(0.0116)	(0.0037)	(0.0033)	(0.0032)
Post	0.0152***	0.0108**	0.0006	-0.0174	0.0166*	0.0018	0.0212***	0.0064	0.0025
	(0.0056)	(0.0053)	(0.0050)	(0.0137)	(0.0092)	(0.0087)	(0.0059)	(0.0061)	(0.0055)
Post x LBO x Offshorable			0.0053			0.0911***			-0.0115
			(0.0082)			(0.0190)			(0.0079)
Constant	0.1580***	0.1710***	0.1550***	0.1280	0.0715	0.1110**	0.1680***	0.2440***	0.2060***
	(0.0477)	(0.0602)	(0.0452)	(0.0798)	(0.0518)	(0.0559)	(0.0498)	(0.0535)	(0.0412)
R^2	0.221	0.253	0.239	0.224	0.248	0.241	0.223	0.257	0.238
Percent change	11.8%	3.3%		97.0%	-1.8%		-7.6%	8.6%	
Observations	313,755	442,929	756,684	48,785	180,656	229,441	264,970	262,273	527,243

NOTE. — This table reports estimates and their standard errors (in parentheses) from six difference-in-differences models and three triple-differences models related to offshoring. ”Post x LBO” is the difference-in-differences estimate, and ”Post x LBO x Offshorable” is the triple-difference estimate. All specifications include the individual- and firm-level controls included in Column 3 in Table 3. Low-productive firms are defined as firms with a value added per employee at $t = 0$ below the median in the industry in which they operate. Other firms are defined as high-productive firms. Workers are categorized into offshorable and non-offshorable workers based on Table A2. The standard errors are clustered at the corporate group times municipality level, and we denote statistical significance at the 1% level by ***, statistical significance at the 5% level by **, and statistical significance at the 10% level by *.

Table 6: Firm level evidence for automation and offshoring

Dependent variable	Intangible assets			Subsidiary in low wage country			Value added per employee		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All firms	Low prod. firms	High prod. firms	All firms	Low prod. firms	High prod. firms	All firms	Low prod. firms	High prod. firms
T-2 x LBO	-0.107	1.586	-0.849	0.026	-0.037	0.007	0.019	0.124	-0.031
	(0.918)	(1.079)	(1.004)	(0.029)	(0.045)	(0.031)	(0.036)	(0.093)	(0.035)
T-1 x LBO	-0.011	0.013	-0.026	-0.007	-0.005	-0.007	0.045	0.098	0.022
	(0.574)	(0.197)	(0.790)	(0.023)	(0.035)	(0.024)	(0.076)	(0.080)	(0.102)
T+0 x LBO					Omitted				
T+1 x LBO	-0.453	0.366	-0.797	-0.020	-0.011	-0.026	0.084**	0.044	0.095**
	(0.512)	(0.446)	(0.660)	(0.032)	(0.030)	(0.043)	(0.039)	(0.066)	(0.045)
T+2 x LBO	0.137	1.408*	-0.444	-0.049	0.185	-0.155	0.057	0.081	0.036
	(0.607)	(0.820)	(0.672)	(0.100)	(0.150)	(0.094)	(0.037)	(0.079)	(0.041)
T+3 x LBO	0.960	2.927**	0.013	0.050	0.232	-0.038	0.092**	0.127*	0.067
	(0.777)	(1.323)	(0.716)	(0.098)	(0.144)	(0.094)	(0.044)	(0.070)	(0.058)
T+4 x LBO	0.649	4.840***	-0.848	0.029	0.243	-0.063	0.047	0.205**	-0.023
	(1.402)	(1.572)	(1.590)	(0.096)	(0.154)	(0.094)	(0.043)	(0.089)	(0.044)
R^2	0.101	0.372	0.132	0.284	0.517	0.259	0.240	0.637	0.194
Observations	4,927	1,017	3,910	4,927	1,017	3,910	4,923	1,016	3,907

NOTE. — This table reports yearly difference-in-differences estimates (using time $t = 0$ as the base year) and their associated standard errors (in parentheses) from regressions at the firm level for different dependent variables. All regressions use standard errors clustered at the corporate group level, and the regressions are weighted by firm size (as of time $t = 0$). Variable descriptions are available in Table A1. Low-productive firms are defined as firms with a value added per employee at $t = 0$ below the median in the industry in which they operate, and the other firms are defined as high-productive firms. The control variables are the same as those included in the propensity score in Table A3, excluding the dependent variable if it appears as a control in Table A3. We denote statistical significance at the 1% level by ***, statistical significance at the 5% level by **, and statistical significance at the 10% level by *.

Table 7: Bargaining with labor unions

Dependent variable	Unemployment incidence								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All firms			Low leverage firms			High leverage firms		
	Aggressive workers	Non-aggressive workers	All workers	Aggressive workers	Non-aggressive workers	All workers	Aggressive workers	Non-aggressive workers	All workers
Post x LBO	0.0228	0.0002	0.0002	0.0259*	0.0129	0.0128	-0.0416	-0.0082	-0.0082
	(0.0140)	(0.0086)	(0.0086)	(0.0144)	(0.0115)	(0.0116)	(0.0326)	(0.0119)	(0.0119)
LBO	-0.0099	0.0078	0.0090	-0.0095	0.0007	0.0011	-0.0327	0.0195**	0.0195**
	(0.0128)	(0.0058)	(0.0058)	(0.0135)	(0.0076)	(0.0068)	(0.0430)	(0.0093)	(0.0093)
Post	-0.0064	0.0106	0.0099	-0.0067	0.0031	0.0033	-0.0211	0.0158	0.0157
	(0.0110)	(0.0069)	(0.0068)	(0.0113)	(0.0097)	(0.0096)	(0.0276)	(0.0097)	(0.0097)
Post x LBO x Aggressive			0.0222			0.0129			-0.0284
			(0.0146)			(0.0171)			(0.0279)
Constant	-0.1360	0.1120	0.1690**	0.2500	0.2960**	0.3280***	-2.9020*	0.1580***	0.1580***
	(0.2870)	(0.0847)	(0.0776)	(0.3210)	(0.1170)	(0.0955)	(1.539)	(0.0469)	(0.0468)
R^2	0.313	0.248	0.254	0.318	0.242	0.257	0.280	0.254	0.255
Percent change	30.8%	0.2%		36.4%	12.1%		-36.1%	-6.2%	
Observations	51,777	380,697	432,474	48,240	167,419	215,659	3,537	213,278	216,815

NOTE. — This table reports estimates and their standard errors (in parentheses) from six difference-in-differences models and three triple-differences models related to bargaining with labor unions. ”Post x LBO” is the difference-in-differences estimate, and ”Post x LBO x Aggressive” is the triple-difference estimate. All specifications include the individual- and firm-level controls included in Column 3 in Table 3, and we cluster the standard errors at the corporate group times municipality level. The sample is restricted to blue-collar workers, and the workers are categorized into affiliated with aggressive and affiliated with non-aggressive labor unions on the basis of the classification in Cronqvist et al. (2009). Low-leverage firms are defined as firms with a debt-to-assets ratio at $t = 0$ below the median in the industry in which they operate. The other firms are defined as high-leverage firms. We denote statistical significance at the 1% level by ***, statistical significance at the 5% level by **, and statistical significance at the 10% level by *.

Table 8: Alternative worker outcomes

Dependent variable	Unemployment days			Job change			Labor income		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All firms	Low productive firms		All firms	Low productive firms		All firms	Low productive firms	
	All workers	Routine workers	Offshorable workers	All workers	Routine workers	Offshorable workers	All workers	Routine workers	Offshorable workers
Post x LBO	1.198	13.850***	11.860***	0.001	0.049***	0.043***	0.446	-25.840***	-6.024
	(0.787)	(3.378)	(2.590)	(0.004)	(0.016)	(0.010)	(2.883)	(6.659)	(6.485)
LBO	-0.132	-1.294	-0.632	0.001	-0.004	-0.007	-0.899	-0.829	-0.710
	(0.467)	(2.754)	(1.944)	(0.002)	(0.011)	(0.007)	(2.201)	(6.002)	(5.296)
Post	3.208***	-3.254	-1.121	-0.010***	-0.042***	-0.019***	-8.090***	5.514	-5.818
	(0.707)	(2.464)	(1.967)	(0.003)	(0.014)	(0.008)	(2.349)	(4.876)	(4.331)
Constant	3.100	7.684	-8.849	0.082***	0.062	0.018	-230.900***	-220.600***	-290.200***
	(6.313)	(20.580)	(11.610)	(0.0255)	(0.0588)	(0.0507)	(29.810)	(46.170)	(44.890)
R^2	0.141	0.145	0.144	0.110	0.100	0.101	0.665	0.482	0.524
Percent change	14.6%	148.2%	146.1%	1.3%	98.3%	101.6%	0.2%	-12.7%	-2.4%
Triple-difference		14.120***	11.670***		0.058***	0.049***		-34.630***	-8.294
		(3.560)	(2.913)		(0.017)	(0.012)		(9.401)	(7.822)
Observations	756,684	55,147	48,785	671,742	48,969	43,320	756,684	55,147	48,785

NOTE. — This table reports estimates and their standard errors (in parentheses) from difference-in-differences models with varying dependent variables (total days of unemployment, job changes, and labor income). All specifications include the individual- and firm-level controls included in Column 3 in Table 3, and we cluster the standard errors at the corporate group times municipality level. Low-productive firms are defined as firms with a value added per employee at $t = 0$ below the median in the industry in which they operate. Workers are categorized into offshorable/non-offshorable and routine/non-routine workers based on Table A2. ”Triple-difference” reports triple-difference estimates to test if the treatment effects for routine and non-routine (or offshorable and non-offshorable) workers in low-productive firms are statistically different. To save space wse do not report estimates for workers in high-productive firms, but none of them are statistically significantly different from zero. We denote statistical significance at the 1% level by ***, statistical significance at the 5% level by **, and statistical significance at the 10% level by *.

Table 9: Comparison to Table 4 in Davis et al. (2014)

Sample	Sweden (1)	US (2)	Sweden (3)	Sweden (4)
	All firms	All firms	Low productive firms	High productive firms
Employment growth t to $t+2$	3.49***	-0.88***	-3.41***	5.33***
	(0.13)	(0.18)	(0.29)	(0.17)
By adjustment margins				
Continuers	2.78***	-1.57***	-1.63***	4.41***
	(0.12)	(0.12)	(0.29)	(0.16)
Deaths	-1.69***	4.12***	2.44***	-2.76***
	(0.05)	(0.09)	(0.05)	(0.07)
Births	-1.47***	1.80***	-0.17	-1.71***
	(0.06)	(0.05)	(0.12)	(0.08)
Acquisitions	0.20***	5.62***	-3.50***	0.43***
	(0.04)	(0.05)	(0.11)	(0.03)
Divestitures	0.29***	2.77***	-4.32***	0.55***
	(0.03)	(0.05)	0.10)	(0.02)

NOTE. — This table displays estimates for firm-level employment growth from time t to time $t+2$ for our sample and for the sample of United States buyouts in Davis et al. (2014), Table 4. The firm-level growth rate is decomposed into continuing establishments, deaths of establishments, births of establishments, acquisitions of establishments, and divestitures of establishments. The firm growth rate is defined as $(E_{ft+2} - E_{ft})/(0.5 \times (E_{ft+2} - E_{ft}))$, where E_{ft} is the employment at time t. All results are employment-weighted. The numbers from Davis et al. (2014) are based on a model controlling for fully interacted industry, year, firm age, firm size, and multi-unit effects plus additional controls for pre-buyout history growth. Our numbers are based on a model controlling for industry effects, year effects, age effects, firm size at time t, firm size growth from time $t-1$ to time t, and a multi-unit dummy. Time $t = 0$ in this table refers to the year of the buyout (as opposed other tables in this paper, where it refers to the year before the buyout). We denote statistical significance at the 1% level by ***, at the 5% level by **, and at the 10% level by *.

Table 10: Position in wage distribution

Dependent variable	Unemployment incidence								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All firms			Low productive firms			High productive firms		
	Workers in the tails	Workers in the middle	All workers	Workers in the tails	Workers in the middle	All workers	Workers in the tails	Workers in the middle	All workers
Post x LBO	-0.0003	0.0144**	-0.0006	-0.0022	0.0913***	-0.0015	-0.0005	0.0018	-0.0012
	(0.0067)	(0.0071)	(0.0071)	(0.0126)	(0.0196)	(0.0133)	(0.0053)	(0.0073)	(0.0053)
LBO	0.0052	-0.0057	0.0054	0.0207*	0.0008	0.0199*	-0.0025	-0.0041	-0.0028
	(0.0049)	(0.0039)	(0.0049)	(0.0109)	(0.0131)	(0.0112)	(0.0034)	(0.0041)	(0.0033)
Post	0.0195***	0.0060	0.0108**	0.0229**	-0.0312*	0.0061	0.0161***	0.0126*	0.0178***
	(0.0053)	(0.0063)	(0.0053)	(0.0096)	(0.0160)	(0.0086)	(0.0054)	(0.0069)	(0.0054)
Post x LBO x Middle			0.0143			0.0953***			0.0026
			(0.0090)			(0.0215)			(0.0079)
Constant	0.0622	0.2590***	0.1560***	0.0587	0.1250	0.0986*	0.1100**	0.2730***	0.2010***
	(0.0526)	(0.0648)	(0.0451)	(0.0491)	(0.1140)	(0.0559)	(0.0498)	(0.0702)	(0.0420)
R^2	0.230	0.255	0.238	0.244	0.237	0.240	0.213	0.260	0.238
Percent change	-0.4%	17.3%		-1.6%	105.0%		-0.8%	2.1%	
Observations	447,694	308,990	756,684	185,213	44,228	229,441	262,481	264,762	527,243

NOTE. — This table reports estimates and their standard errors (in parentheses) from difference-in-differences models and triple-differences models related to the position of the worker in the wage distribution. "Post x LBO" is the difference-in-differences estimate, and "Post x LBO x Middle" is the triple-difference estimate. All specifications include the individual- and firm-level controls included in Column 3 in Table 3, and we cluster the standard errors at the corporate group times municipality level. Low-productive firms are defined as firms with a value added per employee at $t = 0$ below the median in the industry in which they operate, and the other firms are defined as high-productive firms. Workers are categorized into workers in occupations that are in the tails and in the middle of the wage distribution at $t = 0$, according to Table 1 in Goos et al. (2014). We denote statistical significance at the 1% level by ***, statistical significance at the 5% level by **, and statistical significance at the 10% level by *.

Table 11: Job polarization in Sweden 2001-2011

	(1)	(2)	(3)	(4)
	Percentage point change 2001-2011			Percentage change
	All workers	PE workers	Non-PE workers	(Col 3 to Col 1)
Low wage occupations	-0.65%	2.99%	-0.75%	-15.38%
Middle wage occupations	-3.64%	-6.89%	-3.56%	2.20%
High wage occupations	4.29%	3.90%	4.21%	1.86%
	Polarization index			
$\Delta(E^{low}/E^{middle})$	10.03	35.70	9.38	6.48%
$\Delta(E^{high}/E^{middle})$	25.29	38.11	25.05	0.95%

NOTE. — This table reports summary statistics on job polarization in Sweden between 2001 and 2011 using the categorization of occupations into low-wage occupations, middle-wage occupations, and high-wage occupations in Goos et al. (2014). Column 1 reports statistics for all workers in the economy, Column 2 reports statistics for workers having worked in firms involved in a buyout in our sample at any point in time, and Column 3 reports statistics for the remaining workers. The percentage change from 2001 to 2011 is calculated as the change in the occupation's share of all employment when including and excluding workers in firms involved in a buyout. The polarization index is based on Adermon and Gustavsson (2015) and captures the percentage change in the ratio of employment in low-wage occupations relative to middle-wage occupations and among high-wage occupations relative to middle-wage occupations: $\Delta(E^{low}/E^{middle}) = [(E^{low}/E^{middle})_{2011} - (E^{low}/E^{middle})_{2001}]/(E^{low}/E^{middle})_{2001}$ and $\Delta(E^{high}/E^{middle}) = [(E^{high}/E^{middle})_{2011} - (E^{high}/E^{middle})_{2001}]/(E^{high}/E^{middle})_{2001}$.

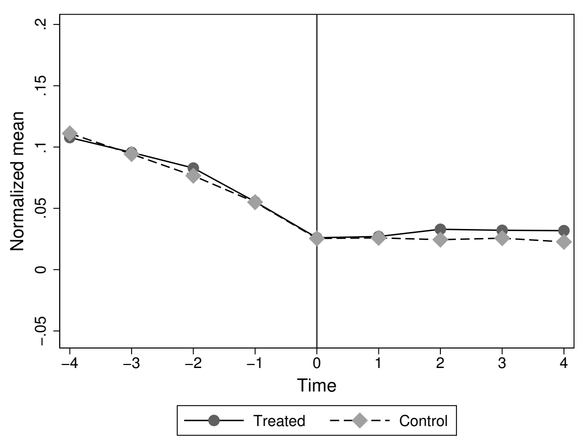

Figure 1: Trends in unemployment incidence

NOTE — This figure displays the average unemployment incidence in the years around the buyouts for treated and control workers. The normalized means are the estimated group-time effects from a regression that omits the constant and includes the same controls as in Column 3 in Table 3.

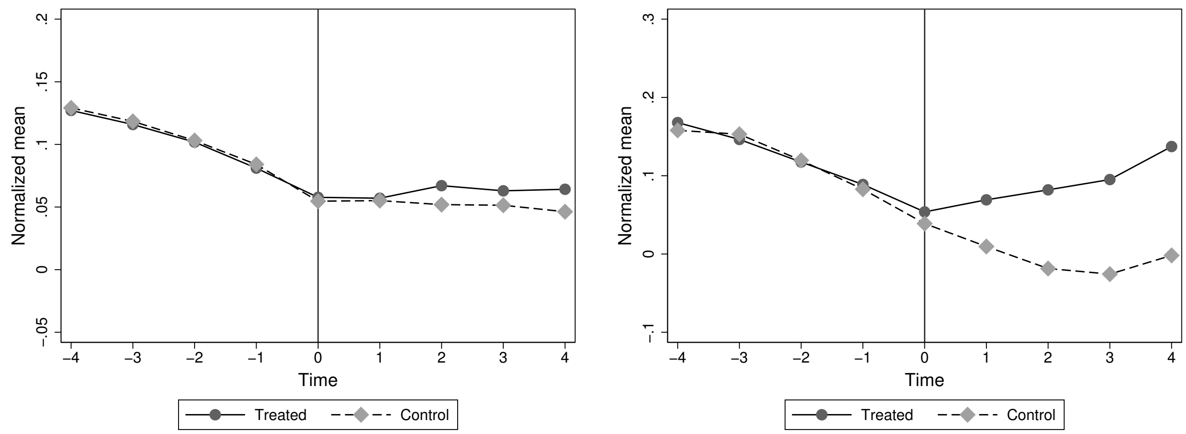

Figure 2: Trends in unemployment incidence by routine intensity subsamples

NOTE — This figure displays the average unemployment incidence in the years around the buyouts for subsamples of treated and control workers. The left figure show the trends for routine workers, and the right figure the trend for routine workers in low-productive firms. The normalized means are the estimated group-time effects from a regression that omits the constant and includes the same controls as in Column 3 in Table 3.

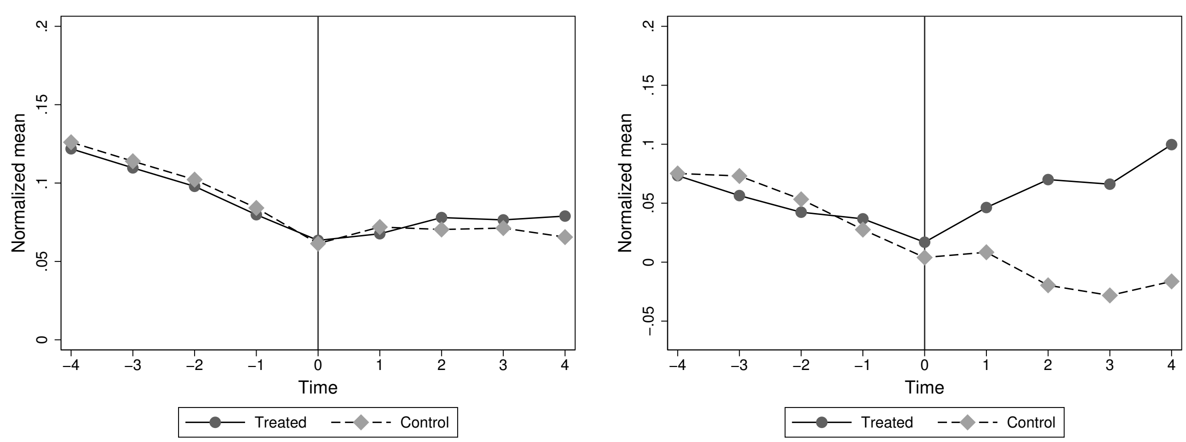

Figure 3: Trends in unemployment incidence by offshorability subsamples

NOTE — This figure displays the average unemployment incidence in the years around the buyouts for subsamples of treated and control workers. The left figure shows the trends for offshorable workers, and the right figure the trend for offshorable workers in low-productive firms. The normalized means are the estimated group-time effects from a regression that omits the constant and includes the same controls as in Column 3 in Table 3.

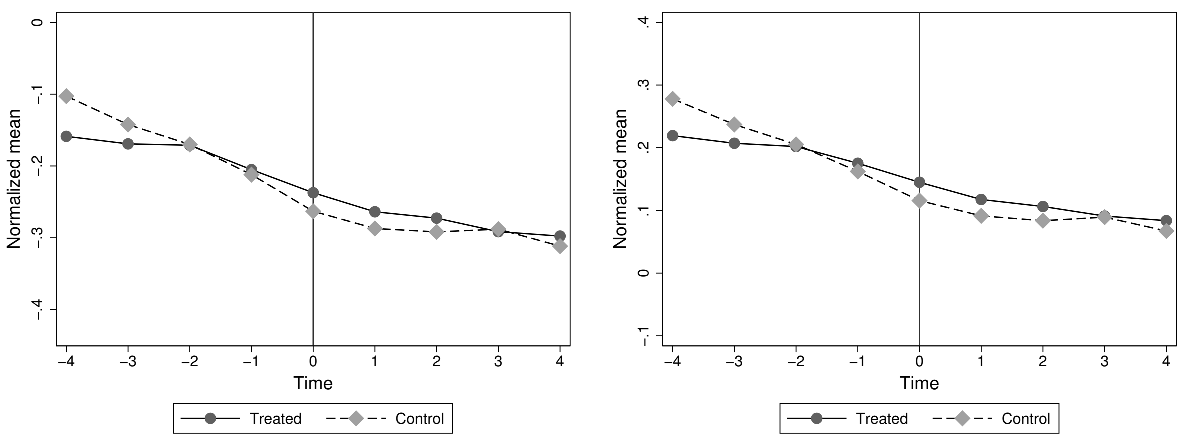

Figure 4: Trends in unemployment incidence by aggressiveness subsamples

NOTE — This figure displays the average unemployment incidence in the years around the buyouts for subsamples of treated and control workers. The left figure shows the trends for workers in industries with aggressive labor unions, and the right figure the trends for workers in industries with aggressive labor unions in low-leverage firms. The normalized means are the estimated group-time effects from a regression that omits the constant and includes the same controls as in Column 3 in Table 3.

Appendix

A The Swedish Private Equity market

In this section, we discuss the Swedish private equity market and how it compares to markets elsewhere. The evidence suggests that the goals of buyouts in Sweden are similar to those of buyouts elsewhere. First, the Swedish private equity market emerged in response to a wave of buyouts in the United States. In 1986, the first Nordic private equity company, Procuritas, was established. In 1989, IK Investment Partners (then Scandinavian Acquisition Capital Fund and later Industri Kapital) and Nordic Capital were founded, with EQT following in 1994. Since then, the Swedish private equity market has become one of the largest in Europe. In 2006, Sweden had the highest level of private equity investments as a percentage of GDP according to Lerner et al. (2008). During the period from 2007 to 2012, Nordic Capital raised 7.7 billion USD for their funds, and EQT Partners raised 6.2 billion USD, which ranked them as the 25th and 40th largest funds worldwide (Private Equity International), respectively. Anecdotal evidence supports the notion that the business model of Swedish private equity firms is similar to that of their United States counterparts. For example, EQT states, "Sales growth and margin expansion are achieved through multiple strategies, including geographic expansion, new products, acquisitions and strategic reorientation. [...] Almost all of the returns on investments are attributed to operational improvements such as increases in sales and efficiency gains." In comparison, Bain Capital Private Equity (ranked 9th by Private Equity International) states that they "help companies improve their competitive position by expanding into new products and markets, growing productivity and strengthening their organizations." Additionally, private equity industry associations in Sweden and elsewhere (such as the European Venture Capital Association and the Swedish Venture Capital Association) state similar targets for private equity buyouts, such as mature companies with development potential and in need of an active owner with financial strength.

Second, the Swedish buyout market has evolved much like markets in other countries and has a high presence of international private equity funds. Table 1 shows that the majority of all transactions are corporate divestitures and management buyouts in Sweden, the United States, the United Kingdom, and France. However, in Sweden, more than 30% of all buyouts are cross-border transactions. The average transaction values for the buyouts are similar for Sweden, the United Kingdom, and France. In all four countries, the number of buyouts increased between 2002 and 2008. Thus, the Swedish private equity market shares several features with the private equity markets in the United States, the United Kingdom, and France.

Finally, holding periods and the performance of target firms are not very different between Swedish buyouts and buyouts elsewhere. Bergström et al. (2007) report a median holding period of 4.5 years for Swedish buyouts that were undertaken between 1993 and 2006. In comparison, Kaplan and Strömberg (2009) report a median holding period of six years for transactions worldwide between 1970 and 2007. The shorter holding period in Sweden can be explained partly by an active initial public offering market during the period. Regarding performance, Bergström et al. (2007) report evidence of operating improvements in the EBITDA margin of approximately three percentage points. Furthermore, profitability improvements are observed for the United States (Kaplan, 1989), France (Boucly et al., 2011), and Western Europe (Acharya et al., 2013). The higher profitability in the wake of buyouts is also consistent with evidence in Lopez de Silanes, Phalippou, and Gottschalg (2015). The authors document that the median internal rate of return (IRR) for transactions in Scandinavia was 24% and only 22% in the United States, 21% in the United Kingdom, and 22% in France. Moreover, the number of investments with IRR above 50% was 31% in Scandinavia and only 27% in the United States, 25% in the United Kingdom, and 21% in France.

B Sample construction

B.1 Capital IQ

Our starting point in the Capital IQ database is transactions in which the target's geographical location is Sweden and the announcement date is between 2002 and 2008. We begin in 2002 because occupational information for workers is available from 2001 (and onwards), and we want to analyze workers who are employed in the targeted firms one year before the announcement. We end in 2008 to ensure that we obtain four post-periods of individual-level information for all workers in our sample (the matched employer-employee data end in 2011).

We then apply a similar selection criteria as Strömberg (2008). We select all transactions with secondary transaction features tagged as "Leveraged Buyout" or "Management Buyout" and those with a buyer/investor stage of interest of "Buyout".[37] Next, we keep the transactions marked as "Closed" or "Effective" and remove the transactions that involved minority stakes and private investments in public equity. In addition, we remove the transactions marked as secondary buyouts. This procedure leaves us with 393 buyouts.

B.2 Merging the datasets

To the list of buyouts from Capital IQ, we add 7 buyouts that were identified by Bergström et al. (2007) but not by Capital IQ. We then name match the buyout targets to the Swedish Companies Registrations Office data to obtain legal firm identifiers that are unique and can be matched to LISA. We are unable to match 51 firms because the reported name in Capital IQ does not match a reported name in the SCRO data. In some cases, the reason is obvious because the name listed in Capital IQ is not a real company name. Examples include "Two properties in Sweden" and "Retail Park in Uppsala, Sweden". In other cases, the name is a foreign entity and not a Swedish entity even though we screen the geographical location as Sweden ("Nordic Hotel Investment Partner Ltd"). After obtaining unique identifiers, we discover another 7 secondary buyouts Capital IQ did not pick up, leaving us with a sample 342 buyouts.

The next steps involve merging three datasets with different timing. The worker data are collected for November of each year, the accounting and group structure data are based on fiscal years, and we have the announcement date for the buyouts. The guiding principle in merging the datasets is that firms should not be treated at time zero. This is an important assumption behind the difference-in-difference approach. Because our analysis is focused on individuals, we take November in each year as the reference point in each of the merges we do, and we work only with firms that have employees and, thus, appear in LISA. As such, we standardize the accounting data to run on a November-to-November basis by dividing the fiscal year into months and taking sums for flow variables and means for stock variables to convert them to November-to-November calendar years. To merge the corporate group structure to this dataset, we take the last known group structure as of November. That is, filings in December apply to next year's November, and filings in January to November the same year.

To merge the information on buyouts to this dataset, we begin by merging the information on buyouts to the corporate group filings to mark subsidiaries to buyout targets as treated. We use the group structure filed the year prior to the year of announcement to ensure that it is not affected by the buyout (using November-

[37]Capital IQ defines a 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)." Capital IQ defines a transaction as closed when the transaction has been closed. However, no hard information is available regarding whether this transaction is effective. An effective transaction is one that has been closed and in which Capital IQ has found information that indicates it is effective. In practice, all closed transactions should be effective unless the transaction is recent.

to-November calendar years).[38] Merging the resulting set of targeted firms to the standardized firm dataset and removing observations with missing values for the variables we match leaves us with a sample of 241 buyouts. We lose 101 buyouts during this procedure for two main reasons. First, the targeted firm lacks accounting information for one year prior to the buyout. This occurs if there are administrative changes in the legal entities to which the firms belong or if the buyout was a divestiture of an entity that did not file accounts separately to the SCRO. Second, we lose buyouts because they take place in firms that have missing information on some of the variables we include in the propensity score match. The biggest culprits are the variables on the share of routine, offshorable, and blue-collar workers, as they build on occupation information in LISA, which is not collected for all firms (see Table A1 for a discussion of the coverage). We also lose some buyouts because we match on firm growth, which is lagged and, thus, requires filed accounts for two years prior to the buyout announcement.

B.3 Accounting for the group structure

To clarify how we account for the group structure, consider Firm M, which has subsidiaries D1 and D2. Subsidiary D1 also has subsidiaries D11 and D12. Suppose that all firms (M, D1, D2, D11, and D12) employ workers and file separate accounts to the SCRO. When Firm M is targeted for a buyout, we mark firms D1, D2, D11, and D12 as treated as well. Suppose now that D1 is targeted; in this instance, we mark firms D11 and D12 as treated as well, but not firms M and D2. The matching is then performed at the firm level, so that M is matched to a control, D1 is matched to a control, D2 is matched to a control, and so on. However, in our regressions, we cluster the standard errors at the group level (i.e., workers in M, D1, D2, D11 and D12 are in the same cluster). When a parent or subsidiary is foreign, we lack accounting information and worker information and do not include those firms and workers in the analysis.

A possible alternative is to use the consolidated accounts of firm M (aggregating from D1, D2, D11 and D12) and perform the matching to control firms with consolidated accounts. This could alleviate concerns that some of the accounting variables we match (mainly, return on assets and debt to assets) could be affected by profit reallocation within groups for tax reasons. There are two problems with this approach. First, a large share of our buyouts target subsidiaries (152 out of 239), and it is not clear how to consolidate the accounts in this case. Second, we do not observe accounting information or worker information for firms and workers abroad. Thus, we cannot reliably calculate consolidated accounts when part of the group is foreign (which occurs in 153 out of 239 cases). Only 48 out of the 239 buyouts target firms that are not in a group or correspond to targeting firm M, with all subsidiaries being Swedish.

Note also that possible tax planning is a problem mainly in the firm-level analysis because there may be tax planning before the buyout but not after, when the companies are bought out and operate independently. Our main worker-level analysis is affected only to the extent that tax planning affects the variables we match and only a subset of these variables are likely to be affected by tax planning (mainly, return on assets and debt to assets). Moreover, there is little evidence in the data that extensive profit reallocation actually takes place in our sample. Before the buyout, there are no statistically significant differences in return on assets and debt to assets between targets that are in a group and that are not in a group.

B.4 Propensity score matching

We use propensity score matching to create our control group of firms. In contrast to cell matching, propensity score matching allows us to match firms based on several characteristics without encountering the problem of dimensionality. The matching algorithm that we use is one-to-one nearest neighbor matching without replacement. To avoid matching firm and worker characteristics that have been affected by the

[38] We drop three deals that were announced in December 2008 because leaving them in would leave us unable to follow workers affected by these buyouts for the full four year post-period.

buyout, we conduct the match one year before the buyout announcement. The large number of potential control firms in our data allows us to match without replacement to increase the precision of our estimates. The extensive available dataset motivates nearest neighbor matching instead of kernel matching and local linear matching, which use observations in the pool of potential controls. In our setting, using the entire pool of potential control firms involves an analysis of more than one million firms and their employees with unclear pre- and post-treatment periods. Computational restrictions make estimation unfeasible when using such a large dataset. Furthermore, we use a one-to-one match because the pool of good matches for several firm characteristics is a function of the characteristics themselves. For instance, although firm size is an important predictor of the probability of undergoing a buyout, the distribution of firm size is skewed, with many small firms and few large firms. Consequently, finding multiple matches for smaller firms is easier than finding them for larger firms. A limitation of one-to-one matching is that the reduced bias comes at the expense of increased variance when one control firm is used for each treated firm (Smith and Todd, 2005). In Section A.2 in the Web Appendix, we perform several robustness checks on our main results using alternative matching strategies and also using a regression control approach, where we draw a random sample of 10% of Swedish firms for each year buyouts occur.

We begin with 415 firms that are announced as being targeted by a private equity firm in the period from 2002 to 2008 and for which we have firm and worker information. The pool of potential controls are limited-liability firms in Sweden, where we exclude 3,782 firms that are reported as being part of a non-private equity merger or acquisition in the Bureau van Dijk's Zephyr database. To estimate the propensity score, we run a probit model where the dependent variable takes the value of one if a firm is targeted by a buyout, and zero otherwise. Column 1 of Table A3 presents the results. We obtain estimated propensity scores for 531,739 firms. The variables are jointly statistically significant at the 1% level, and the pseudo-R^2 is 12%. The one-to-one nearest neighbor match is then performed within strata of firm size, firm age, year and industry bins.[39] Here, we are not able to find a one-to-one match within strata for six treated firms involved in three buyouts. This leaves us with our final sample of 239 buyouts. Column 2 presents results from the same probit model where we have restricted the matched sample. The variables are no longer jointly statistically significant, and the pseudo-R^2 is reduced to 2%. The low pseudo-R^2 and the joint insignificance of the variables suggest that the matched control group and the treatment groups are statistically similar with respect to their observable pre-treatment characteristics.

[39]The firm size bins are fewer than 20 employees, 20 to 40 employees, 41 to 70 employees, 71 to 100 employees, 101 to 150 employees, 151 to 300 employees, 301 to 500 employees, 501 to 1,000 employees, 1,001 to 1,500 employees and 1,500 to 3,500 employees. The firm age bins are less than 5 years, 5 to 7 years, 8 to 12 years, 13 to 16 years and 17 years and older.

Table A1: Variable definitions

Variable	Source	Notes
Panel A: Worker-level variables		
Individual identifier	LISA	Original source is social security numbers from the population registry.
Age	LISA	Original source is the population registry.
Annual labor income	LISA	Original source is Swedish Tax Office records. Annual labor income refers to total gross annual labor income from all sources.
Gender	LISA	Original source is the population registry.
Education	LISA	Information on highest completed education level comes from the Education Register at Statistics Sweden (Utbildningsregistret). The education level variable takes on the following values: 6. Postgraduate education; 5. Post-secondary education, two years or longer; 4. Post-secondary education, less than two years; 3. Upper secondary education; 2. Primary and lower secondary education; 9 or 10 years; and 1. Primary and lower secondary education, less than 9 years. We define a worker as skilled if he or she has at least two years of post-secondary education.
Tenure	LISA	We calculate the tenure of a worker based on observing worker-firm links between the years 1990 and 2011. A worker can thus have a maximum tenure of 20 years.
Days of unemployment	LISA	Statistics Sweden calculates the yearly number of days in unemployment based on registry data from Arbetsförmedlingen (Swedish Public Employment Service). Registering at Arbetsförmedlingen is mandatory for receiving unemployment benefits. An individual is designated as unemployed by Statistics Sweden if the person does not have a job, is looking for a job but cannot find one, and is not registered as being part of any government labor market policy program.
Occupation	LISA	Information on occupation classification is available from 2001 onwards. Swedish Standard Classification of Occupations (SSYK) is based on the International Standard Classification of Occupations (ISCO-88). The SSYK data available from the LISA database come mainly from the official wage statistics survey (Lönestrukturstatistiken) and from supplementary surveys of firms (primarily with 2-19 employees) that Statistics Sweden undertakes of firms not included in the official wage survey. The sampling design in the supplementary surveys is a rolling panel and all eligible firms are surveyed at least once every five years. Occupation information is available for each year, but the information may not be accurate for each year. To ensure that we have accurate occupation information for every year, we require that the information be collected in the relevant year and for the correct employer-employee link.
Panel B: Firm-level variables		
Firm identifier	FAD	The FAD identifier is developed by Statistics Sweden to correct for potential administrative changes in firm legal identifiers over time. It is obtained from the Statistics Sweden FAD database (The Dynamics of Enterprises and Establishments Database). The principle behind the FAD identifiers is that they remain the same from one year to another if the firm identifiers have changed but a majority of workers are the same between the two years. The details of how Statistics Sweden calculates the FAD identifiers are outlined in Andersson and Arvidson (2006).
Firm size	LISA	Number of employees. Aggregated from individual level data in LISA.

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Variable	Source	Notes
Panel B: Firm-level variables		
Size growth	LISA	Following Davis et al. (2014), the growth rate of a firm between two years is calculated as $g_{ft} = \frac{(E_{ft}-E_{ft-1})}{0.5(E_{ft}+E_{ft-1})}$ where where E_{ft} refers to the size of firm f at time t.
Number of establishments	LISA	Number of establishments is obtained by counting the number of establishments linked to a firm in each year.
Diversification	LISA	The diversification dummy takes the value one if a firm has establishments in more than one five digit industry.
Firm age	LISA	The firm age is calculated as the number of years since the FAD firm identifier emerged in the data for the first time. Because the FAD database at statistics Sweden starts in 1986, age of a company is censored.
Average worker wage	LISA	Aggregated from individual level labor income data in LISA.
SD of worker wage	LISA	The standard deviation of worker wage is aggregated from individual level data on labor income in LISA.
Average worker age	LISA	Aggregated from individual level data.
Average worker tenure	LISA	Aggregated from individual level data.
Share routine workers	LISA	Aggregated from individual level occupation data according to Table A2.
Share offshorable workers	LISA	Aggregated from individual level occupation data according to Table A2.
Share blue-collar workers	LISA	Aggregated from individual level occupation data. Blue-collar workers are workers with first digit ISCO-88 codes 5-9 (service and sales personnel, agriculture, forestry, hunting, and fishery workers, crafts and related workers, plant and machine operators, and elementary workers).
Share workers with unemployment history	LISA	Aggregated from individual level data on unemployment with lags one through four.
Industry and location	LISA	Information on the industry and location of an firm comes from Statistics Sweden who assigns identifiers, industry, and location codes to physical places of work (the underlying databases at Statistics Sweden are the RAMS and the Företagsdatabasen databases). The industry classification of firms and establishments changed in 2002 but changes where minor at the 17-category classification level we use.
Total assets	SCRO	Total assets (tangible and intangible).
Intangible assets	SCRO	The accounting item ”Other intangible assets”. This measures intangible assets that are not R&D expenditures, patents, or goodwill. It includes items such as the value of computer systems, software, and databases.
Sales	SCRO	Total sales.
Value added per employee	SCRO	Value added per employee is calculated by Bisnode and is obtained by adding up adjusted EBIT, deductions, and all labor costs in the firm and then dividing by the number of full time workers in the firm.
Debt to assets	SCRO	Total long term debt (maturity over a year) divided by total assets.
Return on assets	SCRO	Return to assets is defined as profit before taxes divided by total assets.
Group structure	SCRO	The group structure comes from consolidated financial accounts submitted by firms with subsidiaries. The group structure traces ownership links between firms with more than 50% ownership in each other and allows us to construct for each year a tree with a parent and subsidiaries, subsidiaries to the subsidiaries, and so on. The group structure also covers parents and subsidiaries abroad.
Subsidiary in low wage country	SCRO	A dummy taking the value one if a firm is in a group that has presence in a low wage country (China, Poland, Brazil, Mexico, Estonia, the Czech republic, India, Lithuania, Russia, Latvia, Hungary and Romania) and zero otherwise. These low wage countries are identified in Becker et al. (2010) as the most common countries Swedish firms tend to offshore to.

NOTE. — This table displays descriptions of the variables that we use from the Statistics Sweden's LISA database and from the Swedish Companies Registrations Office (SCRO). The Swedish Secrecy Act protects access to the data from Statistics Sweden, but researchers affiliated with a Swedish research institution can apply for access. A full detailed description of the variables in LISA is available from the Statistics Sweden homepage (scb.se). The SCRO data can be purchased from SCRO (bolagsverket.se) or in consolidated form from the consulting company Bisnode (bisnode.se).

Table A2: Occupation classifications

Occupation	ISCO-88 code (1)	RTI-Index (2)	Offshorability (3)
Managers of small enterprises	13	-1.52	-0.63
Drivers and mobile plant operators	83	-1.50	-1.00
Life science and health professionals	22	-1.00	-0.76
Physical, mathematical, and engineering professionals	21	-0.82	1.05
Corporate managers	12	-0.75	-0.32
Other professionals	24	-0.73	0.21
Personal and protective service workers	51	-0.60	-0.94
Other associate professionals	34	-0.44	0.10
Physical, mathematical, and engineering associate professionals	31	-0.40	-0.12
Life science and health associate professionals	32	-0.33	-0.75
Extraction and building trades workers	71	-0.19	-0.93
Sales and service elementary occupations	91	0.03	-0.81
Models, salespersons, and demonstrators	52	0.05	-0.89
Stationary plant and related operators	81	0.32	1.59
Laborers in mining, construction, manufacturing, and transport	93	0.45	-0.66
Metal, machinery, and related trade work	72	0.46	-0.45
Machine operators and assemblers	82	0.49	2.35
Other craft and related trade workers	74	1.24	1.15
Customer service clerks	42	1.41	-0.25
Precision, handicraft, craft printing, and related trade workers	73	1.59	1.66
Office clerks	41	2.24	0.40

NOTE. — This table details our classification of occupations into routine/non-routine and offshorable/non-offshorable occupations based on Goos et al. (2014). The RTI index measures the routine intensity in an occupation, where a higher value indicates more routine intensity. We define workers in occupations with an RTI index of zero or less as non-routine workers and workers in occupations with a positive RTI value as routine workers. In addition, we define workers in occupations with an offshorability value of zero or less as non-offshorable workers and workers in occupations with positive offshorability values as offshorable workers.

Table A3: Propensity score matching

	Pre-match (1)	Post-match (2)
Share offshorable workers	0.134**	-0.116
	(0.051)	(0.176)
Share routine workers	0.013	-0.201
	(0.068)	(0.232)
Share blue-collar workers	-0.213**	0.292
	(0.067)	(0.229)
Share workers unemployed at $t-1$	0.066	0.016
	(0.096)	(0.769)
Share workers unemployed at $t-2$	-0.197*	0.120
	(0.103)	(0.811)
Share workers unemployed at $t-3$	-0.005	0.586
	(0.104)	(0.707)
Share workers unemployed at $t-4$	-0.327***	-0.947
	(0.097)	(0.677)
Firm size	0.003***	0.001
	(0.001)	(0.001)
(Firm size)^2	-9.58E-07***	-2.43E-07
	(2.28E-07)	(2.15E-07)
Size growth	-0.154**	-0.102
	(0.059)	(0.254)
Firm age	0.023***	-0.014
	(0.004)	(0.014)
Fixed assets	3.87E-05	-1.94E-04
	(0.0000467)	(0.000188)
Sales	-2.50E-04***	-4.76E-05
	(0.0000635)	(1.55E-04)
Diversification	0.173*	-0.151
	(0.099)	(0.205)
Value added per employee	0.161**	0.030
	(0.055)	(0.131)
(Value added per employee)^2	-0.011**	0.017
	(0.005)	(0.030)
Debt to assets	-0.295**	-0.448
	(0.150)	(0.345)
Return on assets	-0.032*	-0.165
	(0.017)	(0.186)
Mean wage	4.14E-04**	7.55E-05
	(1.33E-04)	(8.29E-04)
Standard deviation of wages	1.17E-04	8.66E-04
	(1.02E-04)	(6.47E-04)
Mean worker age	-0.010***	0.004
	(0.002)	(0.010)
Mean worker tenure	-0.072***	0.045
	(0.011)	(0.039)
Constant	-13.270	-0.087
	(19.680)	(0.628)
F-test (Prob>chi2)	0.000	0.738
Pseudo R^2	0.12	0.02
Observations	531,739	818

NOTE. — This table presents results from the first-stage probit regression (standard errors in parentheses) where the dependent variable is a dummy for buyout targets. The regressions include industry and year effects. Column 1 shows the results before restricting the sample to treated and control firms, and Column 2 shows the results after. The standard errors are clustered at the corporate group level. Variable descriptions are available in Table A1. Statistical significance at the 1% level is denoted by ***, statistical significance at the 5% level by **, and statistical significance at the 10% level by *.

Table A4: Firm employment growth by industry

	(1) All firms	(2) Manufacturing firms	(3) Wholesale and repair firms	(4) Service firms
LBO	3.49***	0.30	0.20	3.56***
	(0.13)	(0.25)	(0.34)	(0.12)
Firms	621	267	139	153

NOTE. — This table displays the difference in firm level employment growth from time t to time $t+2$ between treated and control firms by industry. The firm growth rate is defined as $(E_{ft+2} - E_{it})/(0.5 \times (E_{ft+2} - E_{ft}))$ where E_{ft} is the employment at time t form firm f. All results are employment-weighted and all models control for year effects, age effects, firm size at time t, firm size growth from time $t-1$ to time t, and a multi-unit dummy (Section 4.4 provides more details on the model we estimate). We denote statistical significance at the 1% level by ***.

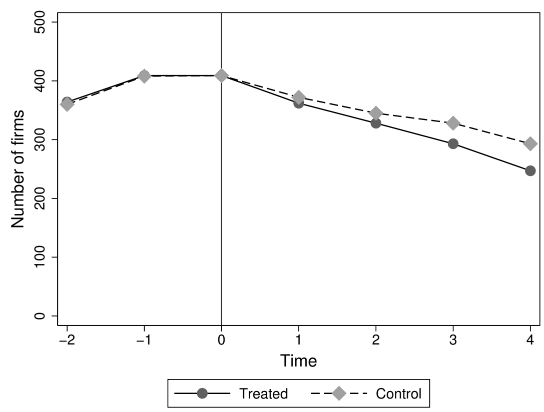

Figure A1: Attrition of treated and control firms over time

NOTE — This figure displays the number of treated and control firms over time in our sample.