Employment Protection Legislation and Firm Growth: Evidence from a Natural Experiment

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Abstract

A natural experiment is used to identify the causal relationship between employment protection legislation and firm growth. The natural experiment occurred in Sweden in 2001, when an exemption made it possible for firms with less than eleven employees to exclude two workers from the last-in-first-out principle when dismissing personnel. The estimated average treatment effect of the reform show that the growth in number of employees increased with 0.16 in firms with 5-9 employees relative to firms with 11-15 employees, which corresponds to over 4,000 additional jobs per year created by the reform. Firms with ten employees, just below the size threshold, became 3.4 percentage points less likely to increase their workforce to a level surpassing the threshold, indicating that the last-in-first-out rule prevented these firms from growing. Thus, employment protection legislation seems to act as a growth barrier for small firms.

Keywords: Firm growth; Growth barriers; Employment protection

JEL codes: D22; J23; K31; L25

1   Introduction

Recent studies have questioned whether politicians should support small firms (Shane, 2009; Nightingale and Coad, 2014) since they are less productive, less entrepreneurial, and have a high risk of business failure. It also seems to exist a "missing middle" in the firm size distribution where large firms grow larger, whereas small firms rarely grow into the next size category (Tybout, 2000; Sleuwaegen and Goedhuys, 2002).

However, small firms are heterogenous and many do not grow despite of having the necessary financial resources. Bornhäll et al. (2013), for example, found that almost 10 percent of all Swedish limited liability firms did not hire more employees even though having high profits, and that nearly one-third of these firms continued to have high profits but no employment growth during the next 3-year period. One problem is that most studies on firm growth takes the institutional framework as given, although small firms might remain small because of growth barriers that prevent them from hiring more employees. This would imply that many new jobs could be created if these barriers were removed.

A number of different growth barriers have been suggested in the literature, e.g., high regulatory burden (Klapper et al., 2006), not well-defined property rights (North, 1973), high level of taxes (Bohata and Mladek, 1999), poor incentives for wealth accumulation (Lindh and Ohlsson, 1996; Davidsson and Henrekson, 2002), high taxation of entrepreneurial income (Davidsson and Henrekson, 2002), strict employment protection legislation (Davidsson and Henrekson, 2002), credit constraints (Acs and Audretsch,

1990; Westhead and Storey, 1997; Berger and Udell, 2002), lack of qualified job candidates (Bohata and Mladek, 1999), and monopolization or unfair competition from the public sector (Davisson and Henrekson, 2002; Sappington and Sidak, 2003).

The empirical evidence on whether these potential growth barriers affect firm growth is primarily based on cross-country studies (Davis and Henreksson, 1999), or surveys (Giudici and Paleari, 2000; Aidis, 2005; Robson and Obeng, 2008). Evidence from cross-country studies typically indicate that institutional factors, such as employment protection legislation and credit market regulations, can explain why certain countries have more rapidly growing firms than others. However, these studies suffer from an omitted variable problem since unmeasured factors that are correlated with the independent variables might be the true casual factors driving the results. They can also be questioned since it is difficult to create comparable indices of cross-country differences in the institutional framework (Howell et al., 2007).

Surveys have typically found that different growth barriers are relatively common (Aidis, 2005), but they can only provide evidence on stated preference (i.e., perceived growth barriers) and not revealed preferences (i.e., actual growth barriers). It is well known that studies on stated preferences have problems with hypothetical biases, i.e., that respondents overstate their perceived values (List and Gallet, 2001). Firms might thus state that certain institutional conditions prevent them from hiring employees when they in fact are not important. Results from surveys are most often also based on small samples that are not representative (Coad and Tamvada, 2012).

We take a different approach by applying a natural experiment in Sweden

to investigate the effect of one possible growth barrier, namely the strictness of the employment protection legislation, on firm growth. Natural experiments have seldom been used in the firm growth literature, although it has been realized that they are ideal to identify causal effects (Angrist and Pischke, 2009). The idea is that we can find situations where natural experiments mimic a randomized trial by changing the variable of interest, while keeping all control variables of interest constant (Angrist and Lavy, 1999).

Sweden has one of the strictest employment protection legislations in the world (OECD, 1994), and one detail that is very uncommon in other countries is the so-called last-in-first-out principle (Skedinger, 2008).[1] It stipulates that firms need to dismiss the latest hired employee first in case of redundancies, and it has frequently been argued that this prevents firms from hiring more employees because it becomes more costly to revoke a bad recruitment decision. Lindbeck and Snower (1989, 1991) have also argued that this principle protects insiders in the labor market, possibly explaining why high unemployment rates tend to persist.

In 2001, a reform was implemented in Sweden that made it possible for firms with less than eleven employees to exclude two employees from the first-in-last-out principle at times of redundancies. It has been realized that such partial reforms in a natural way create appropriate control groups, which are assumed to be unaffected by the reforms (Skedinger, 2008). We use a difference-in-difference approach, and utilize this variation in the employment protection legislation across firm size and time, to identify the

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[1]One exemption is the Netherlands, which also has a last-in-first-out principle in their employment protection legislation.

effect of the last-in-first-out principle on employment growth. Our study is based on a longitudinal firm-level data-set, covering all limited liability firms in Sweden during 1997-2010.

Our identification assumption is that the average outcome for firms just above the size threshold (i.e., above 10 employees) represents a valid control group for our treatment group (i.e., 9 employees or less)[2]. One concern is the endogeneity of the treatment status, i.e., that firms would select themselves into the treatment group before the reform was implemented. However, as noted by Lindbeck et al. (2006), it is unlikely that this reform was anticipated by Swedish firms since it was decided on in late 2000. It was also unclear how many workers would be excluded from the last-in-first-out principle, and which firm sizes that would be eligible to make such exclusions. The fact that the reform was unexpected, and did not affect the full population of firms uniformly, make the use of a difference-in-difference approach to establish causal effects ideal.

Four recent studies have used this approach to investigate how the 2001 reform of the Swedish employment protection legislation affected job flows (von Below and Skogman Thoursie, 2010), labor productivity (Bjuggren, 2013), and work absence (Lindbeck et al., 2006; Olsson, 2009). Studies in other countries have also used natural experiments to investigate how changes in the employment protection legislation have affected job flows (Kugler, 2004; Autor et al., 2007; Bauer et al., 2007; Martins, 2007), employment probabilities for unemployed individuals (Kugler and Saint-Paul,

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[2]We exclude firms with 10 employees from the analysis since they are in the treatment group but would move out of treatment in the case of a new hiring.

2004; Nicholson and North, 2004), the overall employment level (Miles, 2000; Kugler et al., 2003; Autor et al., 2004, 2006; Verick, 2004; Schivardi and Torrini, 2008), wages (Friesen, 1996; Leonardi and Pica, 2007; Schivardi and Torrini, 2008), firm productivity (Autor et al., 2007; Martins, 2007), and work absence (Riphahn, 2004; Engellandt and Riphahn, 2005; Ichino and Riphahn, 2005). However, these later studies are most often based on data from countries, usually United States, where no last-in-first-out principle exist. Very few studies have thus investigated how exemption rules in the employment protection legislation, such as the Swedish last-in-first-out principle, affect firm growth.

Our results indicate that firms with 5-9 employees increased their number of employees with 0.16 relative to our control group after the reform, which corresponds to more than 4,000 additional jobs created per year in the post-reform period. The results also show that firms with 10 employees, i.e., just beneath the size threshold, refrain from new hirings since they would otherwise be subject to stricter seniority rules. The last-in-first-out principle thus seem to act as a firm growth barrier, suggesting that increases in the size threshold or removal of this principle would provide new job opportunities and increase overall employment.

The paper is organized as follows: the following section provides a more thorough description of the Swedish employment protection legislation. Theory and hypotheses to be tested are described in Section 3, while data and our empirical models are presented in Section 4. Results from the difference-in-difference estimations can be found in Section 5. Finally, Section 6 summarizes and concludes.

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2 Employment protection regulation in Sweden

Employment protection legislation in Sweden received its current design in 1974 when the Swedish social democratic government introduced the Employment Protection Act. The primary purpose of the new legislation was to give employees a protection against unfair dismissals and fluctuations in labor income by limiting the possibilities for firms to lay-off employees. It included, for example, rules concerning employers' ability to dismiss employees as well as the use of temporary contracts (Skedinger, 2008).[3]

The Swedish Employment Protection Act (SFS 1982:80) states that employment contracts are by default permanent contracts with up to six months trail periods. Time-limited contracts are only allowed if justified by the work task, and then for a maximum period of six months. The employment protection act also specifies that firms must apply the so-called last-in-first-out principle when dismissing personnel, implying that the employee with the shortest seniority has to be the first lay-off. This individual is then prioritized in case of a re-employment during the following nine months.

There are, however, a number of ways for employers to circumvent these rules. Firms can usually negotiate with the workers unions to deviate from the last-in-first-out principle. This can be seen as more preferable than to lay-off a key employee, even though it often implies a higher lay-off cost. Depending on the union involved, workers can also be divided into groups after their positions or work tasks meaning that the last-in-first-out princi-

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[3]Certain job protection for workers older than 45 years had existed already since 1971. And even further back in time there were legal restrictions on the ability of employers to dismiss state employees, and those that were pregnant or performed military duties.

ple would only be used within each group of workers. Collective agreements can also be used to contract upon a deviation from the last-in-first-out principle in advance. Another way of circumvent the seniority rules is to use short-term contracts since these employees do not fall under the last-in-first-out principle. Firms can finally hire employees through a work agency, which means that the last-in-first-out principle is not applicable since these employees have permanent contracts with the work agency.[4]

The Swedish employment protection legislation for employees with permanent contracts is one of the strictest in the world (OECD, 1994), and the last-in-first-out principle is something that is very uncommon in other countries. It has remained mostly intact since 1974, and mainly minor reforms concerning the conditions for temporary employment have been implemented. One important exception is a reform in 2001, which allowed firms with a maximum of ten employees to exclude up to two employees from the last-in-first-out principle. The reform thus made it possible for small firms to retain employees that were considered to be of extra importance, even if they were supposed to be dismissed first according to the last-in-first-out principle.

According to Lindbeck et al. (2006), the timing of the reform was the following. In late April 1999, the Swedish Green Party and the center-right opposition required that the Swedish social democratic government

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[4]It is thus debated how efficient the last-in-first-out rule in reality is in protecting individuals against dismissals. Skogman Thoursie (2009), for example, argued that the last-in-first-out principle in practice is inefficient since it exist so many possibilities for the firms to circumvent this principle. However, small firms do not have the same possibilites to circumvent the last-in-first-out principle since they are less likely to have collective agreements and employ personel from work agencies.

should propose a softening of the Swedish employment protection legislation for small firms, but the social democratic government opposed any such reform. However, since the green party and the center-right opposition was in majority in parliament, the government could be forced to present such a reform. In February 2000, a report was presented by the Ministry of Industry suggesting that either all firms should be allowed to exempt two employees from the last-in-first-out principle in case of redundancies, or that only firms with 10 employees or less should be allowed to do so. In late May 2000, the government suggested the first alternative. But the Swedish green party stated that they would only accept alternative two, implying that this was the only proposition that could win a majority in the parliament. In September 2000, the Labour market committee changed alternative two slightly to "ten employees or less" instead of "less than ten employees", and presented this alternative to the parliament. A majority consisting of the Swedish green party and the center-right opposition voted yes for the reform in the parliament on October 11, 2000, and the reform was implemented from January 1, 2001.

Since the reform was decided on in late 2000 and implemented on 1 January 2001, it is unlikely that this reform was anticipated by Swedish firms (Lindbeck et al., 2006). The reason is that the reform was possible through the unusual cooperation in the Swedish parliament between the green party and the center-right wing opposition, and it was not clear that this agreement actually would prevail. It was also unclear how many workers could be excluded from the last-in-first-out principle, which firm sizes that would be eligible to make exclusions and when the reform could be implemented.

Thus, we consider this to be an exogenous change in the Swedish employment protection legislation, making it possible to identify the causal effect of the reform on firm growth.

Note that the 2001 reform of the Swedish Employment Protection Act applies to the firm level, and not the work establishment level. This is to make sure that the exemption of two employees is independent of number of establishments within the firm. The Employment Protection Act furthermore stipulates that managers, members of the employer's family and workers that are participating in employment subsidy programs are not to be counted as employees when determining the size of the firm. Finally, no difference is made between workers that are on permanent or short-term contracts when determining the size of the firm.

Four recent studies have investigated how the reform of the Swedish employment protection legislation in 2001 influenced different outcome variables. Lindbeck et al. (2006) analyzed the effects on work absence, and found that the possibility to exclude two workers from the last-in-first-out principle decreased sickness absence with around 0.25 days per year. This corresponds to a 3.3 percent decrease in sickness absence in the treated firms relative to the control group. Their results also indicated that people with a record of high absence tended to leave the firms subjected to the reform, but that the same firms became less reluctant to hire individuals with a record of high absence.

Olsson (2009) also analyzed how the 2001 reform affected sickness absence, and found that the possibility to exclude two employees from the last in-first-out-principle decreased the average sickness absence rate by about

13 percent in the treatment group relative to their control group. The effect was found to be strongest for shorter sickness spells. In addition, the negative effect was largest among establishments with relatively few females or workers with temporary contracts.

Bjuggren (2013) investigated how the reform influenced labor productivity using firm-level micro data, finding that the reform increased labor productivity by 2.5 percent for the treatment group relative to the control group of larger firms. The positive effect of the reform on labor productivity increased to 6 percent when the sample was restricted to firms that were downsizing and stayed within the treatment and control group during the whole study period.

Using matched employee-employer data on all Swedish firms, von Below and Skogman Thoursie (2010) investigated how the reform of the last-in-first-out principle in 2001 influenced employment decisions within the firm. Their results indicated that the reform increased both hirings and firings with around 5 percent, while no significant effect was found on net employment levels.

3 Employment protection legislation and employment growth

3.1 Theory and hypotheses

Despite a highly researched area, employment protection legislation continues to be a highly controversial topic. Some researchers argue that such

legislation is needed in order to protect workers from unfair dismissals, and that potential costs with legislations therefore are justified. More security has also been argued to promote employees to acquire firm-specific human capital, and thereby increase their productivity (Mortensen and Pissarides, 1999, Pissarides, 2001).

On the other hand, employment protection legislation has been argued to reduce job creation since it decreases the flows in and out of employment and makes dismissals, and thereby hirings, more expensive. This effect arise since employers incorporates potential future costs in case of layoffs already in the hiring decision (Skedinger, 2011). If the last-in-first-out principle in the Swedish employment protection legislation prevents firms from hiring more personnel, we thus expect that firms with ten employees or less were more likely to hire one additional employee compared to our control group after the reform.

Note, however, that stricter employment protection legislation makes both hires and dismissals more costly. The net effect on employment is thus theoretically indeterminate and depends upon which of the two flows that dominates (Bertola, 1999). This also implies that more employment protection legislation leads to fewer dismissals during a recession, but also fewer hirings during an economic upturn, making the combined effect over a business cycle ambiguous.[5] However, Swedish policy makers implemented the reform with the purpose of increased job creation in small firms.

Our first hypothesis to be tested is therefore formulated on the basis of

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[5] However, Lindbeck (1993) argues that the stricter EPL might lead to more permanent unemployment following a depression. The negative effects from the more stringent EPL after macroeconomic shocks are also covered in Blanchard and Wolfers (2000).

policy, and not theory-driven

H1 *Firms with less than ten employees will increase their number of employees more than larger firms after they got the opportunity to exclude two workers from the last in-first out principle.*

Several countries have a less stringent employment protection rules for smaller firms. One argument for having more liberal rules for small firms is that these firms are more sensitive to cost-increasing effects of employment protection than larger firms. On the other hand, the more liberal rules for small firms could create incentives for firms to act strategically and not cross the size threshold where they would be subject to the more stricter rules (Skedinger, 2011). The Swedish reform in 2001 thus not only made it more favorable for small firms to increase their workforce, it also meant that firms just below the firm size threshold should become less likely to increase their number of employees. Our second hypothesis to be tested is therefore

H2 *Firms with ten employees became less likely than firms with nine employees to increase their number of employees after 2001.*

3.2 Empirical studies

A number countries (Portugal in 1989; Italy in 1990; Germany in 1996, 1999 and 2004; and Sweden in 2001) have implemented reforms of the employment protection legislation that affect small firms, but not larger firms. And a number of studies have realized that such reforms can be used as a natural experiment to investigate the effect of employment protection legislation on employment levels.

Italian firms with less than 15 employees were, for example, exempted

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from employment protection regulations prior to 1990. A reform that year suddenly removed this exemption, and thus increased their firing costs. Kugler and Pica (2003, 2008) have analyzed the effect of this reform, and found that both inflow and outflow of employment in small firms decreased after the reform relative to the corresponding flows in larger firms. Similar results were obtained by Cingano et al. (2010), Garibaldi et al. (2004), who showed that job reallocation decreased in small firms after the reform in Italy.

Schivardi and Torrini (2004) studied the effects of the same reform on the firm size distribution in Italy. Their results indicated that removing the threshold in Italian EPL would increase average firm size by less than 1 percent. These results are supported by Garibaldi et al. (2004), who found that firms close to the 15 employees shows more persistence than other firms, and that they are more likely to move backward than upward.

Germany has recently also implemented a number of reforms of the employment protection legislation. Bauernschuster (2009) examined the effect of the reform in 2004, finding that the relaxation of dismissal protection in small firms had a small positive effect on hiring but no effect on separations. Hence, the reform had a positive net effect on employment. It was also found that the German reform caused considerable substitution by type of employment contract. More specifically, firms became prone to hire workers on permanent rather than temporary contracts relative to the situation before the reform.

Martins (2009) analyzed the effects on workers flows of a reform in Portugal in 1989, where firms with at most twenty employees got a less restrictive employment protection legislation. Their results indicate that the reform

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increased employment levels in small firms relative to the control group, but the effect was quantitatively small. Boeri and Jimeno (2005) find that the workers under permanent contracts in firms with less restrictive employment protection legislation are more likely to be dismissed. However, they do not find an effect from the exemption threshold on firm growth.

It is not straightforward to compare the results presented above. The reason is that the reforms differed in many ways between the countries, which means that it becomes difficult to measure how the reform actually changed the adjustment costs of small firms. If the changes in adjustments costs are small, then it is reasonable to expect small reform effects and vice versa. None of the studies discussed above have been able to investigate how exemptions from a last-in-first-rule affects employment growth within the firm. The reason is simply that this rule does not exist in these countries.

Note finally that a number of studies have tested hypothesis 1 using cross-state variation of employment protection legislation within the United States (Autor et al., 2004, 2006, 2007), or cross-country differences in employment protection legislation. These studies tend to find that increased stringency in employment protection legislation reduces labor market dynamics. However, most of the reforms have been designed in such a way that it complicates evaluation since all employees are affected by the reform, which implies that there are few or no suitable control groups. The results might thus be driven by omitted variables that are correlated with the change in employment protection legislation, and it is also possible that the reforms of the employment protection legislation is driven by the employment trends.

4 Data and Empirical Method

4.1 Data

All limited liability firms in Sweden are legally bound to submit an annual report to PRV (the Swedish patent and registration office). We use data from PAR, a Swedish consulting firm that gathers this economic information from PRV, on limited liability companies active at some point during 1996-2010; 503,958 firms in total. The data include all variables found in the annual reports, i.e., measures of profits, number of employees, salaries, fixed costs, and liquidity. We focus on limited liability firms since they tend to be characterized by higher growth ambitions and actual growth than other legal forms (Storey, 1994; Harhoff et al., 1998).

We use firm-level data since the exemption rule from the last-in-first-out principle was applied on the firm-level, and not on the establishment level. Our unit of analysis thus corresponds with the objective of the reform. Data three years before and after the 2001 reform of the Swedish employment protection legislation were used, which means that our study covers the period 1998-2003. Firms with less than 5 employees or more than 16 employees were excluded to avoid having too large differences between the treatment group and the control group. The final sample then consists of 47,896 firms and 169,353 firm-year observations.

Delmar and Davidsson (1998) emphasize that researchers need to choose growth indicator, growth measurement and the process of growth when investigating firm growth. The growth indicator refers to the variable over which growth is observed. The most commonly used growth indicators

are employment and sales (Delmar and Davidsson, 1998, Daunfeldt et al., 2014). Although sales and employment growth tend to be modestly correlated (Shepherd and Wiklund, 2009; Coad, 2010), most studies suggest that the results do not seem to be sensitive to which one is chosen (Daunfeldt et al., 2014). We use employment as growth indicator since our purpose is to study whether a relaxation of the last-in-first-out principle in the employment protection legislation can stimulate employment growth.

The measurement of growth refers to whether growth is measured in absolute or relative terms. This choice is of importance since studies have shown that relative growth measures will lead to a bias of finding small firms with the largest changes in growth rates due to the regression to the mean effect, whereas large firms are overrepresented as fast growers when measuring growth in absolute terms (Delmar et al., 2003). Our study is focused towards absolute changes in the number of employees since we want to investigate if firms that got the opportunity to exclude two employees from the last-in-first-out principle became more likely to increase their number of employees compared to firms that were not allowed to make such exemptions.[6] The aim of relaxing employment protection legislation is albeitly to increase the total number of employees, and not the relative growth rates. Another reason why we focus towards absolute changes in the number of employees is that a relative definition of the growth variable would cause a bias for finding small firms having higher growth which could drive the

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[6]Note that we cannot distinguish between employees that have permanent and temporary contracts. This is not a problem when defining the firm size since the Employment Protection Act stipulates that both contracts should be taken into account when defining firm size, but the last-in-first-out rule is only relevant for employees with permanent contracts.

results. Now there will be the opposite bias instead which is preferred since it will lead to a somewhat conservative estimate of the reform effect on firm growth.

The process of growth concerns organic (internal) and acquired (external) growth. Organic growth refers to new employment internal to a firm, while acquired refers to gaining employment through external acquisitions or mergers. With few exceptions, most studies use total growth (i.e. the sum of organic and acquired growth) due to lack of data on mergers and acquisitions. We also investigate total growth since we cannot distinguish between these growth modes in our data.

We thus define firm growth $(G_{i,t})$ for firm i during period t as the absolute change in the number of employees, i.e:

$$G_{i,t} = no.employees_{i,t} - no.employees_{i,t-1} \tag{1}$$

A firm replacing one worker with another would have zero growth, which means that this definition captures the net effect on employment.

Figure 1 shows how firms and employment shares are distributed over firm size classes. We see the well-known pattern with most firms being small, while most of the employment is provided by large firms. Firms have been divided into size groups in order to give a better visualization of the data.

[**Figure 1 about here**]

Figure 2 presents a closer look at the number of firms by size class in a neighborhood around the 10-employee threshold. There are no obvious differences in the distribution before or after the reform.

Figure 3 shows the probability that a company hire at least one additional worker during the 3-year time periods before and after the reform. We can see a clear decrease in the growth probability at the ten employee threshold after the reform, showing that differentiation in the strictness in employment protection may cause unintended effects by altering the incentives for firms close to the threshold size. The actual growth for firms with 10 employees is around 3.4 percentage points lower than one would expect.

Note also that the probability of hiring at least one more employee is increasing in firm size, i.e., smaller firms are much less likely to hire more personnel than larger firms. This clearly indicates that smaller firms have lower growth ambitions than larger firms, confirming results from many previous studies (Nightingale and Coad, 2014). We therefore restrict our treatment group to firms with 5-9 employees, and also do a separate analysis for firms that have 9 and 10 employees. It is reasonable to assume that the likelihood of having 9 or 10 employees prior to the reform can be regarded as randomly assigned, but the latter firms can no longer grow without loosing their opportunity to exclude two workers from the last-in-first-out principle. This means that any observed changes in employment growth after the 2001 reform are likely to be related to the introduction of exemption rules in the employment protection legislation.

It is worth noting that restricting the intervention and control groups, in our case excluding firm-years that lie outside the 5-15 employee group of

firm-years, creates a dataset where firms are very similar in the two groups, but also where data is truncated. In some cases there will be firms with exceptional high growth that makes the firm move above the 15 employee limit, creating a downward bias in our difference-in-difference estimates of the effect of the reform on firm growth. However, there will of course also be cases where firms rapidly decline, and move below the 5 employee limit, thus creating an upward bias in the estimated reform effect.

Another potential problem is that firms after the introduction of the reform could self-select into treatment, something that could affect our results. In our opinion, it is reasonable to believe that firms above the threshold are more inclined to want to join the treatment group than vice versa. If this is so, our estimate of the treatment effect is conservative, and would be higher without such an effect. However, the reform was quite sudden, and making changes in the number of employees downward can only be done after some time. In order to minimize the impact of this behavior on our estimate of the reform effect, we restrict the number of after-reform years to three.[7]

4.2 Empirical method

We first test hypothesis 1, i.e., that firms with less than ten employees were more likely than larger firms to increase their number of employees after they got the possibility to exclude two workers from the last-in-first-out principle, by estimating

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[7]We have also tried to restrict the number of post-reform years to two. All results remain qualitatively similar and are available from the authors upon request.

$$G_{i,t} = \alpha_0 + \beta_1 D_t + \beta_2 D^g + \beta_3 (D_t * D^g) + \beta_4 S_{i,t-1} + \qquad (2)$$

$$\beta_5 Age_{i,t} + \gamma_1' I_j * trend + \gamma_2' R_m * trend +$$

$$\gamma_3' T_v + \gamma_4' I_j + \gamma_5' R_m + \epsilon_t$$

where D_t is a dummy variable for the treatment period (2001-2004); D^g is a dummy for belonging to the treatment group; $S_{i,t-1}$ is firm size measured as total revenues in period $t-1$; $Age_{i,t}$ is firm age; and T_v, I_j and R_m are time-specific, industry-specific and regional-specific fixed effects. Time-specific fixed effects control for time-variant heterogeneity (e.g., business cycle effects) that might explain differences in employment growth, while industry-specific and region-specific fixed effects control whether employment growth is determined by time-invariant heterogeneity across industries and regions. Interaction terms capturing industry-specific and region-specific time trends are also included.

Our key variable of interest is the interaction between D_t and D^g, which provides an estimate of the treatment effect. We expect $\widehat{\beta}_3 > 0$, i.e., that firms with 5-9 employees became more likely than our control group to increase their number of employees after they got the opportunity to exclude two workers from the last-in-first-out rule.

We also control for firm age and firm size since they usually are included as control variable in the firm growth literature (van Praag & Versloot, 2008). A large number of empirical studies have, for example, tested Gibrat's (1931) proposition that firm growth is independent of firm size (Sutton,

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1997; and Caves, 1998, provide overviews). Recent studies tend to reject this hypothesis that growth is independent of firm size, instead finding that small firms grow faster than larger ones (Coad, 2009). Some studies have also found that younger firms grow faster than older ones. In fact, Haltiwanger et al. (2013) argues that after controlling for firm age, there is no systematic relationship between firm size and firm growth.

Hypothesis 2 states that firms with 10 employees, just below the size threshold, will become less likely compared to firms with 9 employees to hire more personnel after the reform. The reason is that these firms do not want to fall out of the treatment group, i.e., they might want to forgo growth opportunities in order to keep the possibility to exclude key workers from the last-in-first-out principle in case of dismissals.

In order to test hypothesis 2, i.e., that firms with nine employees are more likely to hire one additional employee after the reform compared to firms with ten employees, we estimate the following equation using a linear probability model

$$DG_{i,t} = \alpha_0 + \delta_1 D_t + \delta_2 D10 + \delta_3(D_t * D10) + \delta_4 S_{i,t-1} + \delta_5 Age_{i,t} + \quad (3)$$
$$\gamma_1' I_j * trend + \gamma_2' R_m * trend + \gamma_3' T_v + \gamma_4' I_j + \gamma_5' R_m + \epsilon_t$$

where $DG_{i,t}$ is a binary dependent variable that equals one if firm i had positive growth in period t, otherwise zero. We expect firms with 10 employees to become less inclined to grow compared to firms with 9 employees after the reform, i.e., that $\widehat{\delta_3} < 0$.

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5   Results

A key assumption when using a difference-in-difference methodology is that the outcome variables would have had parallel trends for the treatment and control group after the reform in absence of treatment. This is not formally testable, but Figure 4 show the trend in absolute employment growth for firms in a neighborhood below (5-9 employees), and above (11-15 employees) the size threshold, before and after the reform, indicating that there was similar trends in the treatment and control groups before the treatment period.[8]

[**Figure 4 about here**]

The results from estimating equation (2) are presented in Table 1. Four different models are estimated. First, we estimate equation (2) without any control variables (Model I). Control variables for firm size and firm age are added in Model II, and industry-specific and region-specific time trends are then added one at the time in Model III and IV. This provides a simple sensitivity check whether the estimated coefficients are robust to the inclusion of control variables.[9]

[**Table 1 about here**]

The estimated treatment effect $(D_t * D^g)$ is positive and significant, with a coefficient around 0.16 in the full model specification (Model IV). This

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[8]If the reform was unexpected by Swedish firms, we would not see an effect of the reform before 2001. In order to validate this, we also perform placebo estimations with hypothetical reform years. Our results are presented in Table A1 and A2 in the Appendix.

[9]We have also estimated all our models without time, industry, and regional-specific fixed effects. All results remain qualitatively similar, and are available from the authors upon request.

indicates that firms with 5-9 employees increased their number of employees with, on average, 0.16 individuals after the reform. Our treatment group includes 26,539 firms, which implies that the possibility to exclude two employees from the last in-first out principle has contributed to 4,246 new jobs per year in firms with 5-9 employees after the reform. Note also that no estimates switch sign across the estimated models, and that the changes in magnitude are very small. We thus cannot reject hypothesis 1, suggesting that the last-in-first-out principle acts as a growth barrier and prevents small firms from increasing their number of employees.

In order to reduce any potential problems of self-selection into our treatment group after the reform, we also try excluding observations near the size threshold and instead include firms with 5-7 and 13-15 employees in our treatment and control group, respectively. We still cannot reject hypothesis 1, and the estimated treatment effect is now somewhat smaller (0.154 in our full model specification). The results are presented in Table A3 in the Appendix.[10]

Our results from testing hypothesis 2, i.e., whether firms that have ten employees are less likely than firms with nine employees to increase their workforce after the reform, are presented in Table 2. Firms with ten employees are thus our treatment group, whereas firms with nine employees constitutes our control group. This reduces our sample to 16,066 firms and 31,207 firm-year observations.

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[10]We also calculated transition probabilities to analyze movements between our treatment and control group. The results are presented in Table A5 in the Appendix, showing a low probability that firms move between our treatment and control group both before and after the reform.

[**Table 2 about here**]

The estimated treatment effect ($D10 * D_t$) is negative and significant across all models. In our main model specification (Model IV), the estimated coefficient is -0.0341. Thus, firms just below the size threshold have a lower growth probability than firms with nine employees following the reform. Without this threshold, we would expect that around 3.4 percentage points more of the firms with ten employees would experience positive employment growth each year during the post-reform period. This implies that an additional 548 firms per year would have chosen to expand their businesses if they were allowed to exclude two workers from the last-in-first-out principle. Hypothesis 2 thus cannot be rejected, suggesting that the reform has created a growth barrier for firms just below the size threshold and that firms want to stay beneath the threshold to retain their ability to exclude workers from the last-in-first-out principle.

In order to test the validity of our results, we also tested whether firms that have nine employees are less likely than firms with eight employees to increase their number of employees after the reform; as well as whether we can find any significant effects of the reform when comparing firms with ten and eleven employees. If our results are driven by the changes in the employment protection legislation in 2001, we should not be able to find any significant differences between these size groups. This is also what we find (see Table A4 in the Appendix), suggesting that the reduction in growth probability for firms with ten employees is driven by the employment protection legislation reform in 2001.

6   Summary and conclusions

The purpose of this paper has been to study whether the last-in-first-out principle in the Swedish employment protection legislation acts as a growth barrier, preventing firms from increasing their number of employees. The last-in-first-out principle stipulates that firms need to dismiss the latest hired employee in terms of redundancies, which means that dismissals (and thus hires) are associated with a higher risk since it becomes more costly to revoke a bad recruitment decision.

We investigated this question by realizing that a change in the Swedish Employment Protection Act in 2001 could be used as a natural experiment, thereby making it possible to estimate the causal effect of the last-in-first-out principle on firm growth. The reform implied that firms with a maximum of ten employees could exclude up to two employees from the last-in-first-out rule, thereby making it possible to retain individuals that were considered to be of extra importance for the firm. The fact that the reform was unexpected, and did not affect the full population of firms uniformly, make the use of difference-in-difference approach to establish causal effects ideal.

Our results indicated that firms with 5-9 employees increased their number of employees with 0.16 per year due to the reform. This corresponds to 4,246 new jobs created each year by the reform during 2001-2003, suggesting that the last-in-first-out principle is a growth barrier that prevents firms from expanding their businesses. We also noted that the reform introduced a size threshold since firms with ten employees no longer would be able to exclude two workers from the last-in-first-out principle if they increased

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their number of employees. Firms with ten employees were found to be 3.4 percent less likely to increase their workforce compared to firms with nine employees after the reform, clearly indicating that this size threshold has prevented these firms from hiring more personnel.

Previous studies have indicated that the reform of the Swedish employment protection legislation in 2001 have led to a number of positive effects such as reduced sickness absence (Lindbeck et al., 2006; Olsson, 2009), increased firm productivity (Bjuggren, 2013), and more job dynamics (von Below and Skogman Thoursie, 2010). But von Below and Skogman Thoursie (2010) also found that net employment was unaffected by the 2001 reform, and the question is why our results differ from theirs?

One possible explanation is that they included firms with 2-10 employees in the treatment group, while our treatment group consisted of firms with 5-9 employees. The identification of a causal effect in a difference-in-difference estimation depends on the treatment and control group to be as similar as possible so that the reform can be regarded as good as randomly assigned, and it is well known that firms with 2 employees differ significantly from firms that have 11 employees; not least with regard to growth ambitions (see Figure 3; Nightingale and Coad, 2014). However, this does not seem to explain the different findings since we found a positive effect on the number of employees also when firms with 2-4 employees were included in our treatment group.[11]

The difference seem neither to be explained by the use of different growth measures. von Below and Skogman Thoursie (2010) used the change in hires

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[11]Results available upon request.

and separations in relation to number of employees as their dependent variable, while we investigated the effect of the 2001 reform on the absolute number of employees within the firm. We believe that our choice is to prefer since firms that are trying to grow usually define the number of additonal employees they want to hire, rather than decide on what share of the total number of employees in the firm to hire. However, our results remain qualitatively similar also when we use a relative growth measure.[12]

The most likely explanation why our results differ is therefore that our samples are different, which is something that we cannot account for. Our study was restricted to study the effect on the number of employees in limited liability firms, while von Below and Skogman Thoursie's (2010) sample included firms with different legal forms (e.g., public firms, sole proprietorships). This means that the absence of an effect on net employment in their study might be driven by different behavioral responses of firms with different legal status.

We believe that future research should look more carefully at how the reform influenced different groups at the labor market. It has been suggested (Kugler and Saint-Paul, 2004) that employers are less likely to hire employees that are valued as risky when employment protection laws are restrictive. Employers might, for example, have more difficulties in estimating the value of applicants with less work experience or foreign education. This could potentially lead to a higher unemployment among certain groups in the labor market such as young, immigrants or long-time unemployed (Skedinger, 2010). Seniority rules also benefit senior workers more than young individ-

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[12]Results not presented, but available from the authors upon request.

uals and immigrants. This also implies that the effect of the reform could differ across industries since the last-in-first-out principle should act more as a growth barrier in industries where many potential employees are young or immigrants.

Reforms of the employment protection legislation could not only influence how much firms grow, but also how they grow. We therefore believe that future studies more carefully should investigate whether mergers and acquisitions are influenced by reforms that are supposed to reduce growth barriers.

Many firms do not grow despite of having high profits. If it is growth barriers that prevent them from expanding their businesses, then it becomes very important to further analyze potential growth barriers to increase our understanding of how we can promote firm growth. We believe that reforms that can be used as natural experiments are important in order to identify the reforms that actually has an effect on firm size, and that this constitute an important avenue for further research.

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7 Tables and figures

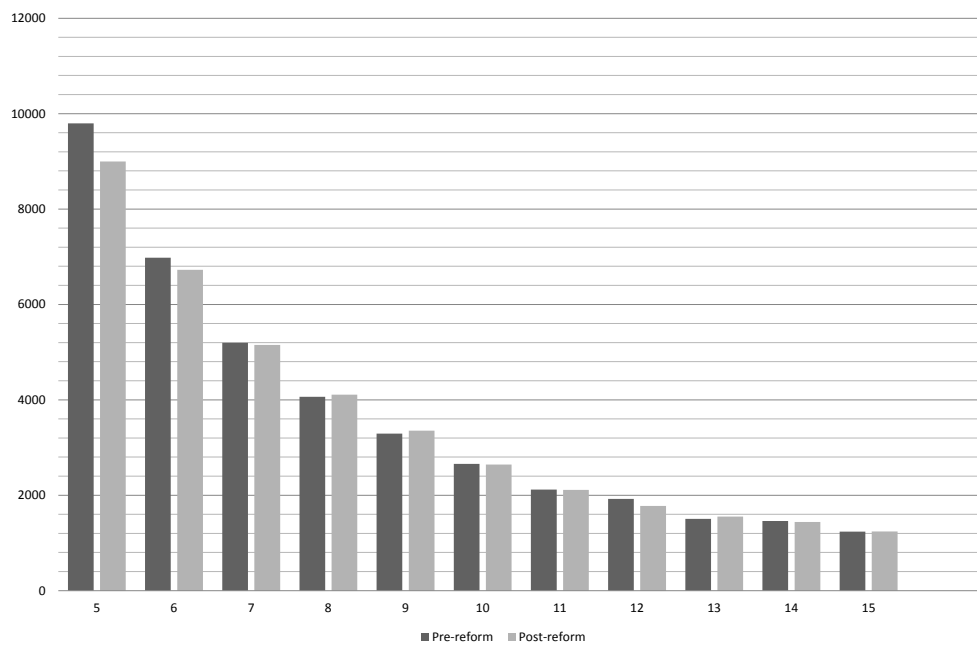Figure 1: Firm and employment shares by size class (Averages over the 1997-2003 period)

Figure 2: Number of firms by size class and period

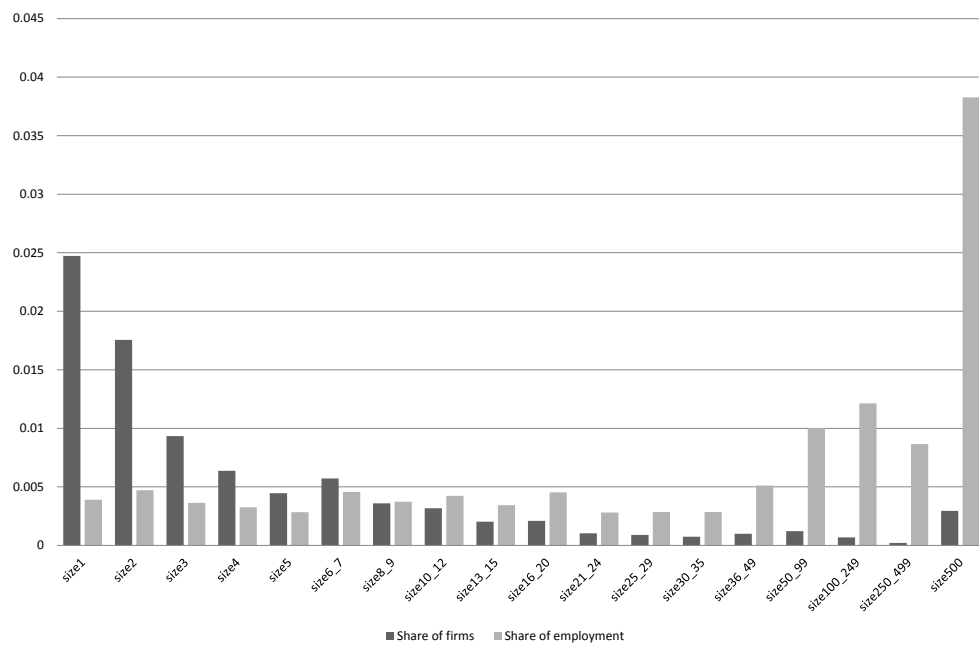Share of firms ■ Share of employment
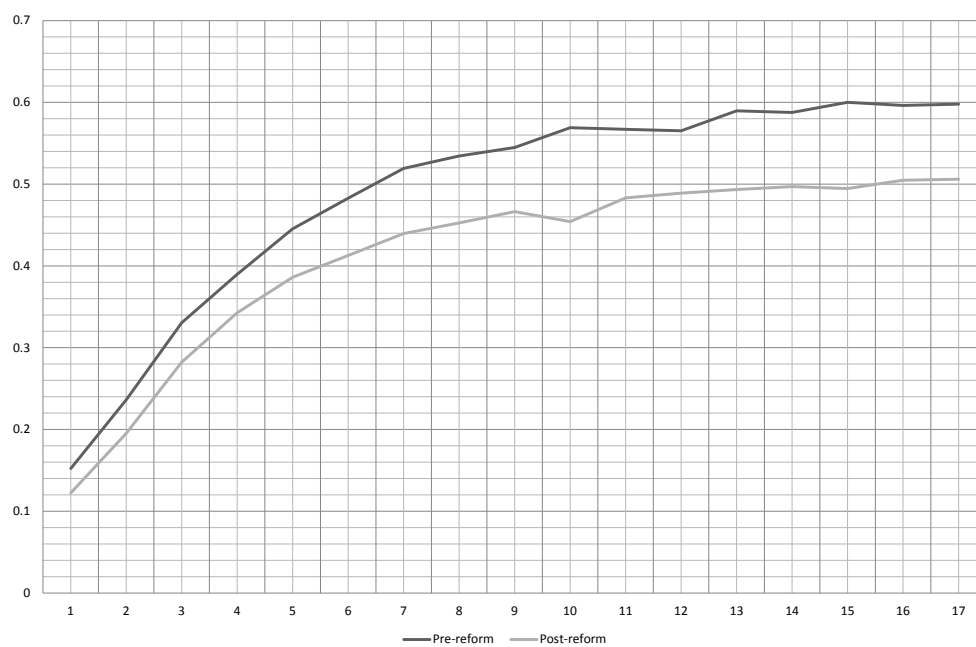
Figure 3: Probability of positive growth by size

Figure 4: Absolute growth trends by size group
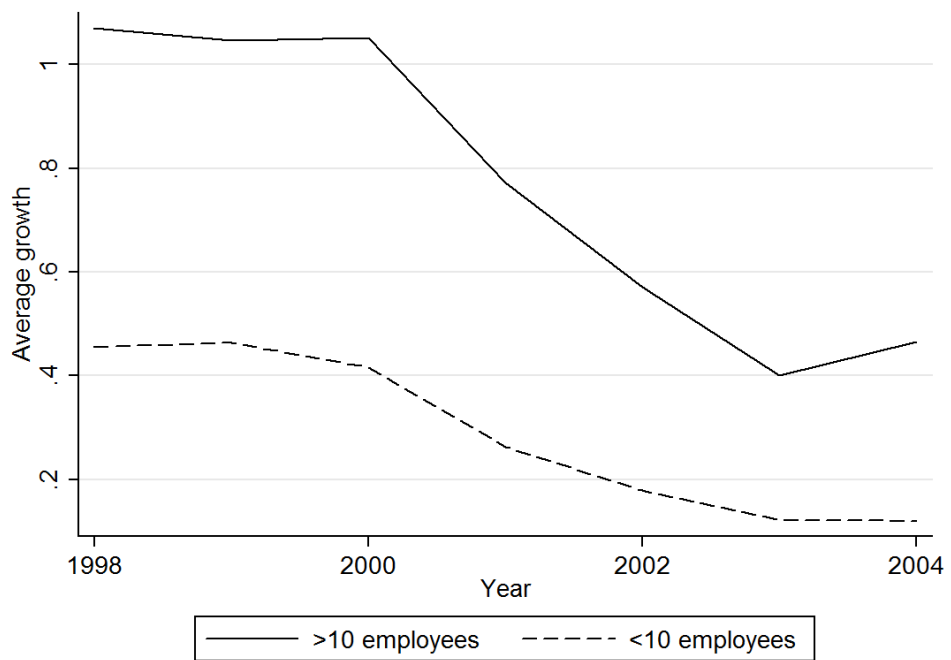
Table 1: Difference-in-difference estimation, 1998-2003. Estimating the effects on employment growth in treated firms compared to non-treated firms.

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
D_t	-0.424***	-0.458***	-0.303***	-0.448***
	(0.0329)	(0.0327)	(0.0468)	(0.0523)
D^g	-0.973***	-1.002***	-1.003***	-1.002***
	(0.0226)	(0.0224)	(0.0224)	(0.0224)
$D_t * D^g$	0.162***	0.162***	0.163***	0.160***
	(0.0307)	(0.0304)	(0.0304)	(0.0304)
Size (L)		-1.48e-07*	-1.48e-07*	-1.47e-07*
		(7.75e-08)	(7.75e-08)	(7.75e-08)
Age		-0.0174***	-0.0174***	-0.0174***
		(0.000414)	(0.000414)	(0.000414)
Industry-trend			-0.00169***	-0.00154***
			(0.000245)	(0.000246)
Region-trend				0.00391***
				(0.000629)
Constant	1.428***	1.997***	1.914***	1.956***
	(0.0747)	(0.0760)	(0.0761)	(0.0764)
Observations	145,379	145,353	145,353	145,353
R-squared	0.056	0.068	0.069	0.069
Time FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Regional FE	YES	YES	YES	YES

Firms with ten employees are excluded in the estimations,
Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$
Note: (L) indicates that firms' size during previous year is used.

Table 2: Difference-in-difference estimation, 1998-2003. Estimating the threshold effect by comparing firms with 9 employees to firms with 10 employees.

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
D_t	0.0642***	-0.0116		
	(0.0107)	(0.0114)		
$D10$	0.0247***	0.0297***	0.0297***	0.0294***
	(0.00759)	(0.00791)	(0.00791)	(0.00791)
$D_t * D10$	-0.0328***	-0.0344***	-0.0343***	-0.0341***
	(0.0109)	(0.0112)	(0.0112)	(0.0112)
Size (L)		-1.85e-07**	-1.85e-07**	-1.87e-07**
		(8.91e-08)	(8.91e-08)	(8.93e-08)
Age		-0.00453***	-0.00453***	-0.00453***
		(0.000218)	(0.000218)	(0.000218)
Industry-trend			-6.17e-05	-2.36e-05
			(0.000130)	(0.000130)
Region-trend				0.000944***
				(0.000325)
Constant	0.356***	0.524***	0.521***	0.532***
	(0.0452)	(0.0456)	(0.0461)	(0.0462)
Observations	33,139	31,207	31,207	31,207
R-squared	0.043	0.059	0.059	0.059
Time FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Regional FE	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: (L) indicates that firms' size during previous year is used.

8 Appendix: Robustness checks

If the reform was unexpected by Swedish firms, we would not see an effect of the reform before 2001. In order to validate this, we do estimations with hypothetical reform years. The effects of hypothetical reforms in year 1999[13] and 2000 are presented in columns 1 and 2 in Table A1, respectively, using the same empirical model as in Table 1. The results show no significant effect of the hypothetical treatments, indicating that the reform seems to have been unexpected by the firms. In order to avoid problems with hypothetical treatment years overlapping the true treatment years, the estimations in Table A1 is based on only one year before and one year after the hypothetical treatment years.

When doing placebo estimations for the threshold effect given hypothetical reform years, it is not possible to limit the time periods to only include one year before and one year after the reform. In line with Table 1 and 2, we therefore use three years before and after the hypothetical reforms.

A significant effect for a hypothetical reform in year 2000 is found in our size threshold analysis (Table A2). The effect is smaller and with a lower significance than the effect of the true reform in year 2001. However, this is not surprising since two out of the three hypothetical reform years are actual reform years where firms with less than ten employees in fact received treatment.

Table A3 shows the results from estimation of equation (2) when we ex-

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[13] The estimation using 1999 as reform year only includes two year before and two years after the reform. This is due to limitations in the data and also explains why the dummy variable for the treatment period is omitted.

Table A1: Results from difference-in-difference estimations with
hypothetical reform years.

VARIABLES	(1) 2000	(2) 2001	(3) 2002
D_t	-0.00248	-0.285***	-0.158***
	(0.0492)	(0.0479)	(0.0473)
D^g	-0.982***	-1.061***	-0.925***
	(0.0377)	(0.0377)	(0.0353)
$D_t * D^g$	-0.0655	0.119**	0.0821
	(0.0524)	(0.0509)	(0.0504)
Size (L)	-6.85e-07*	-1.49e-06***	-9.60e-08***
	(4.15e-07)	(3.73e-07)	(3.58e-08)
Age	-0.0219***	-0.0191***	-0.0144***
	(0.000749)	(0.000690)	(0.000656)
Constant	1.845***	2.024***	1.796***
	(0.104)	(0.137)	(0.141)
Observations	51,900	51,346	49,383
R-squared	0.088	0.085	0.059
Time FE	YES	YES	YES
Industry FE	YES	YES	YES
Regional FE	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: (L) indicates that firms' size during
previous year is used.

Table A2: Estimating the threshold effect at hypothetical reform years.

VARIABLES	(1) 1999	(2) 2000	(3) 2001	(4) 2002	(5) 2003
D_t		-0.185	-0.0937	0.0200*	0.0266**
		(0.176)	(0.236)	(0.0114)	(0.0109)
D10	0.0362**	0.0419***	0.0298***	0.0221***	0.0192***
	(0.0150)	(0.0100)	(0.00791)	(0.00602)	(0.00593)
$D10 * D_t$	-0.0124	-0.0256**	-0.0345***	-0.0281**	-0.0155
	(0.0177)	(0.0126)	(0.0112)	(0.0115)	(0.0115)
Size (L)	-1.54e-07*	-1.71e-07*	-1.85e-07**	-1.50e-07**	-1.17e-07*
	(8.24e-08)	(9.34e-08)	(8.91e-08)	(6.16e-08)	(6.12e-08)
Age	-0.00499***	-0.00499***	-0.00453***	-0.00447***	-0.00469***
	(0.000315)	(0.000240)	(0.000218)	(0.000203)	(0.000203)
Constant	0.518**	0.832***	0.606**	0.494***	0.461***
	(0.218)	(0.183)	(0.240)	(0.0417)	(0.0418)
Observations	15,341	25,915	31,212	36,333	37,013
R-squared	0.077	0.060	0.058	0.074	0.074
Time FE	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES
Regional FE	YES	YES	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: (L) indicates that firms' size during previous year is used.

clude firms close to the size threshold in our treatment and control group. Our treatment group now consists of firms with 5-7 employees, whereas our control group consists of firms with 13-15 employees. This reduces the probability of self-selection into the treatment- and control groups after the reform. The estimated treatment effect is slightly larger than in the standard model shown in Table 1. The larger estimated effect is expected since smaller firms are able to exclude a larger share of their workforce in case of downsizing.

If our results are driven by the employment protection legislation reform in 2001, no significant treatment effects should be observed if we compare firms with nine to eight employees and firms with eleven to ten employees. The results from this robustness check are presented in Table A4. In the first column, $D9 * D_t$ indicates whether the reform had an effect on the difference in growth probabilities between firms with nine employees and firms with eight employees, whereas $D11 * D_t$ in column 3 shows the effect of the reform on growth probabilities for firms with eleven and ten employees. We find no significant treatment effects, which strengthens our conclusion that the treatment effects observed in Table 2 are driven by the Employment Protection Legislation reform in 2001.

Since self-selection into our treatment group after the reform is possible, we also analyze movements between our treatment- and control group by calculating transition probabilities. Table A5 shows that the probability of staying in the original group is high in both periods, 0.89-0.90 in the pre-reform period and 0.84-0.93 in the post-reform period. The probabilities of moving from the treatment group to the control group is similar in size to

Table A3: Difference-in-difference estimation of the treatment effect on firm growth, excluding firms close to the size threshold. Treatment=5-7, control=13-15.

VARIABLES	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
D_t	-0.458***	-0.493***	-0.254***	-0.441***
	(0.0466)	(0.0462)	(0.0595)	(0.0655)
D^g	-1.360***	-1.395***	-1.396***	-1.394***
	(0.0325)	(0.0323)	(0.0323)	(0.0323)
$D_t * D^g$	0.156***	0.158***	0.159***	0.155***
	(0.0447)	(0.0444)	(0.0444)	(0.0444)
Size (L)		-1.21e-07**	-1.21e-07**	-1.20e-07**
		(5.82e-08)	(5.81e-08)	(5.80e-08)
Age		-0.0162***	-0.0162***	-0.0162***
		(0.000498)	(0.000497)	(0.000497)
Industry-trend			-0.00155***	-0.00136***
			(0.000281)	(0.000281)
Region-trend				0.00507***
				(0.000728)
Constant	1.724***	2.265***	2.190***	2.245***
	(0.0831)	(0.0850)	(0.0856)	(0.0860)
Observations	97,357	97,340	97,340	97,340
R-squared	0.081	0.092	0.092	0.093
Time FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Regional FE	YES	YES	YES	YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Note: (L) indicates that firms' size during previous year is used.

Table A4: Estimated effects of hypothetical threshold levels.

VARIABLES	(1) 8 vs 9	(2) 9 vs 10	(3) 10 vs 11
D_t	-0.0312***	-0.0116	0.0214*
	(0.0103)	(0.0114)	(0.0127)
D9	0.0172**		
	(0.00719)		
$D9 * D_t$	0.00867		
	(0.0102)		
D10		0.0297***	
		(0.00791)	
$D10 * D_t$		-0.0344***	
		(0.0112)	
D11			0.00284
			(0.00884)
$D11 * D_t$			0.00978
			(0.0126)
Size (L)	-1.60e-07	-1.85e-07**	8.05e-09**
	(1.19e-07)	(8.91e-08)	(3.68e-09)
Age	-0.00521***	-0.00453***	-0.00403***
	(0.000203)	(0.000218)	(0.000238)
Constant	0.619***	0.524***	0.439***
	(0.0382)	(0.0456)	(0.0532)
Observations	37,553	31,207	24,709
R-squared	0.059	0.059	0.063
Time FE	YES	YES	YES
Industry FE	YES	YES	YES
Regional FE	YES	YES	YES

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Note: (L) indicates that firms' size during previous year is used.

Table A5: Transition probabilities during pre- and post-reform period.

Pre-reform

	Control	Treated
Control	0.87	0.13
Treated	0.10	0.90

Post-reform

	Control	Treated
Control	0.84	0.16
Treated	0.07	0.93

the probability of moving in the opposite direction. Firms became somewhat less likely to move from the treatment group to the control group after the reform, whereas the opposite result is found for the control group. Note that Table A5 is descriptive, and that it is not possible to draw conclusions regarding causal effects from looking at Table A5.