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 in Sweden. A reform of the last-in-first-out principle increased employment growth with over 4,000 additional jobs per year in firms with less than eleven employees. Firms with ten employees became 3.4 percentage points less likely to increase their workforce, indicating that an introduced threshold kept them 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.

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1 Introduction

Recent studies have questioned whether politicians should support small firms since they are less productive, less entrepreneurial, and have a high risk of business failure (Shane, 2009; Nightingale and Coad, 2014). There is also evidence of a "missing middle" in the firm-size distribution, with large firms growing larger, but small firms remaining small (Tybout, 2000; Sleuwaegen and Goedhuys, 2002).

However, small firms are heterogenous and many might remain small despite having the financial resources to grow. Almost 10 percent of Swedish limited liability firms, for example, did not hire more employees even though they had high profits during 1997-2010 (Bornhäll et al., 2013). Nearly one-third of this 10 percent continued to have high profits, but no employment growth, during two subsequent three-year periods. If it is growth barriers that hinder these firms from hiring more employees, many new jobs could be created if these barriers were removed.

Growth barriers suggested in the literature include high regulatory burden (Klapper et al., 2006); poorly defined property rights (North, 1973); high 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).

Empirical evidence on whether these possible growth barriers affect growth comes primarily from cross-country studies (Davis and Henreksson, 1999), or surveys (Giudici and Paleari, 2000; Aidis, 2005; Robson and Obeng, 2008). Cross-country studies typically suggest that institutional factors, such as employment-protection legislation and credit-market regulations, may explain why certain countries have more rapidly-growing firms than do others. However, these studies suffer from an omitted variable problem since unmeasured factors correlated with the independent variables might be the true causal factors driving the results. It is also difficult to create comparable cross-country indices of institutional differences (Howell et al., 2007).

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

We take a different approach by using a natural experiment in Sweden to investigate the effect of one possible barrier, the strictness of employment-protection legislation, on firm-growth. Natural experiments have seldom been used in the firm growth literature, although it has been recognized that they are ideal for identifying causal effects (Angrist and Pischke, 2009). The idea is that a natural experiment mimics a randomized trial by changing the variable of interest, while keeping control variables constant (Angrist and Lavy, 1999).

Sweden has one of the strictest employment-protection legislation in the world (OECD, 1994), with an uncommon detail (also enforced in the Netherlands), being the so-called last-in-first-out principle (Skedinger, 2008). In case of redundancies, this principle states that firms must dismiss the last-hired employee first. This may keep firms from hiring more employees, because it is costly to revoke a bad recruitment decision. It may also protect insiders in the labor market, possibly explaining why high unemployment tends to persist (Lindbeck and Snower, 1989, 1991).

In 2001, a reform was enacted in Sweden that made it possible for firms with less than eleven employees to exclude two of them from the first-in-last-out principle. Using a difference-in-difference approach on a longitudinal firm-level data-set, covering all limited liability firms in Sweden during 1997-2010, and utilizing this change in the employment-protection legislation across firm size and time, we can identify the effect of the last-in-first-out principle on employment growth.

We assume 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 (9 employees or less)[1]. 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. It is, however, unlikely that this reform was anticipated by Swedish firms, since it was only decided upon in late 2000 (Lindbeck et al., 2006). It was also unclear how many workers would be excluded from the last-in-first-out principle, and what firm size would be eligible to exclude. 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 ideal for establishing causal effects[2].

Four recent studies have used this approach to investigate how the reform 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 employment protection legislation affect job flows (Kugler, 2004; Autor et al., 2007; Bauer et al., 2007; Martins, 2007); employment probabilities for the unemployed (Kugler and Saint-Paul, 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 the United States, where no last-in-first-out principle is enforced.

We find 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. We also find that firms with 10 employees, i.e., just beneath the size-threshold, refrained from new hiring, presumably because they would then be subject to the stricter rule. 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 the principle completely, could provide new job opportunities and increase overall employment.

The next section provides a more thorough description of Swedish employment protection legislation. Theory and hypotheses to be tested, as well as previous empirical studies, 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, while Section 6 summarizes and draws conclusions.

2 Employment protection in Sweden

Job protection for workers older than 45 has existed since 1971 in Sweden, and even further back there were legal restrictions on dismissal of state employees and those that were pregnant or performed military service. The Swedish employment protection legislation received its current form in 1974 when the Social Democratic government passed the Employment Protection Act. The aim was primarily to protect employees against unfair dismissal, as well as fluctuations in income, by limiting possibilities for firms to lay-off employees. It also included rules concerning the use of temporary employees (Skedinger, 2008).

The current formulation (SFS 1982:80) states that employment contracts are by default permanent, with up to six-months trial periods. Temporary contracts are only allowed if justified by the nature of the work, and then for a maximum of six months. Firms must also apply the last-in-first-out principle when dismissing permanent personnel, so that the employee with the least seniority has to be the first lay-off. This individual must then receive priority in case of hiring during the following nine months.

However, there are a number of ways for employers to circumvent these rules. In specific cases, firms can usually negotiate with their labor union to deviate from the last-in-first-out principle, which can be preferable to lay-off a key employee, even though it might mean a higher lay-off cost. Depending on the union involved, workers may also be divided into groups according to the nature of their work, with the last-in-first-out principle only applying within each group. Union contracts may also agree upon other deviations from the last-in-first-out principle.

Another way to circumvent the principle, is to use temporary employees, who by definition do not fall under the last-in-first-out principle. Finally, firms can hire employees through a temporary employment agency. The last-in-first-out principle is then not applicable to the firm, since the employees have permanent contracts with the agency.

It is thus debated how efficient the last-in-first-out rule is in reality in protecting individuals against dismissal. Skogman Thoursie (2009), for example, argued that the last-in-first-out principle in practice is inefficient since there are so many possibilities for firms to circumvent it. However, small firms do not have the same possibilities, since they are less likely to have collective agreements and to hire through temporary employment agencies.

The Swedish employment protection legislation is one of the strictest in the world (OECD, 1994), with the last-in-first-out principle quite uncommon elsewhere. But, as noted earlier, a reform in 2001 allowed firms with ten employees or less to exclude up to two of them from the last-in-first-out principle. Small firms could thus retain employees considered important even if, they under the first-in-first-out principle, would have been dismissed first.

Starting in late April 1999, a majority of the Green Party and the center-right opposition forced the reform upon the Social Democratic government, which opposed it (Lindbeck et al., 2006). A February 2000 report from the Ministry of Industry suggested that either all firms be allowed to exempt two employees from the principle, or that only firms with less than ten employees be allowed to do so. The Social Democrats preferred the first alternative since they thought this might split the fragile alliance between the center-right opposition and the Green Party, but the Green Party would only accept the second. In September 2000, the Labor Market Committee changed the second alternative to "ten employees or less" instead of "less than ten employees". A majority consisting of the Swedish Green Party and the center-right opposition passed the reform in Parliament on October 11, 2000, to take effect from January 1, 2001.

Because of the timing and the unusual - and fragile - cooperation in Parliament between the Green Party and the center-right opposition which passed it, as well as the late change in the threshold for exclusion, it is unlikely that the reform was anticipated by Swedish firms (Lindbeck et al., 2006). Thus, we consider this an exogenous change in Swedish employment protection, making it possible to evaluate the causal effect of the reform on firm growth.

The reform applies at firm level, and not establishment level, to make sure that the exclusion of two employees is independent of the number of establishments within the firm. However, managers, members of the employer's family, and workers participating in employment-subsidy programs are not counted when determining the size of the firm. But no difference is made between permanent and temporary employees.

Four recent studies have investigated effects of the reform. Lindbeck et al. (2006) analyzed effects on work absence, finding a reduction of about 0.25 days (3.3 percent) per year in the treated firms relative to the control group. Employees with a record of high absence tended to leave firms subject to the reform, but the firms became less reluctant to hire individuals with a record of high absence.

Olsson (2009) also analyzed effects on work absence, finding a reduction of about 13 percent in the treatment group relative to their control group, with the strongest effect on shorter sickness spells, and in firms with fewer female temporary employees.

Using firm-level micro data, Bjuggren (2013) analyzed effects on labor productivity, finding an increase of about 2.5 percent for the treatment group relative to the control group, or 6 percent when the samples were restricted to downsizing firms that stayed within either the treatment or the control group during the study period.

Finally, using matched employee-employer data from all Swedish firms, von Below and Skogman Thoursie (2010) analyzed effects on employment decisions within the firm. Their results indicated that both hirings and firings increased by about 5 percent, while no statistically significant effect on net employment levels was found.

3 Employment protection legislation and employment growth

3.1 Theory and hypotheses

Though highly researched, employment protection legislation remains a highly controversial topic. Some researchers have argued that potential costs are justified by the need to protect employees from unfair dismissal. Employment protection may also encourage employees to acquire firm-specific human capital, increasing their productivity (Mortensen and Pissarides, 1999, Pissarides, 2001). On the other hand, employment protection might reduce hirings since it makes a possible future dismissal more expensive (Skedinger, 2011). Lindbeck (1993) even argues that stricter protection might lead to more permanent unemployment following a depression (see also Blanchard and Wolfers, 2000).

If Sweden's last-in-first-out principle reduces hiring, then, after the 2001 reform, firms with less than ten employees might have been more likely than those in the control group to hire an additional employee. But since employment protection makes both hiring and dismissing more costly, it is not clear what the net effect would be when that protection was loosened, more employment or less (Bertola, 1999). Stricter employment protection leads to fewer dismissals during a recession, but fewer hirings during recovery, making the combined effect over the business cycle ambiguous.

However, Swedish policy makers implemented the 2001 reform in hopes of increasing job creation. Since theory is ambigious, our first hypothesis -

based simply on policy - is

H1 *Firms with less than ten employees increased their number of employees more than did larger ones after being granted the power to exclude two employees from the last-in-first-out principle.*

Small firms might be more sensitive to the cost-increasing effects of employment protection than are larger firms. But exempting them from stricter employee protection could also provide them with an incentive to remain below the size-threshold, where they would become subject to stricter rules (Skedinger, 2011). Our second hypothesis is therefore

H2 *After the 2001 reform, firms with ten employees became less likely than firms with nine to increase their number of employees.*

3.2 Empirical studies

In recent decades, several countries have reformed employment protection for small firms but not for larger ones (Portugal in 1989; Italy in 1990; Germany in 1996, 1999, and 2004; and Sweden in 2001), creating natural experiments that, as noted, have been used to investigate their effects.

Prior to 1990, Italian firms with less than 15 employees were exempted from employment protection, but a sudden reform removed this exemption. Kugler and Pica (2003, 2008), Cingano et al. (2010), and Garibaldi et al. (2004) found that both hirings and dismissals were reduced more in small firms than in larger ones following the reform, and Schivardi and Torrini (2004) found that average firm size increased by almost 1 percent.

Politicians in Germany have also implemented employment protection legislation reforms. Bauernschuster (2009) found that hirings, but not dismissals, increased when dismissal protection in small firms was relaxed under the German 2004 reform, resulting in a net positive effect on employment. Firms also became more likely than before to hire employees on permanent rather than temporary contracts.

The 1989 Portuguese reform, which loosened employment protection for firms with at most twenty employees, increased employment levels in small firms relative to larger ones, but the effect was small (Martins, 2009). Permanent employees in firms with looser employment protection were then more likely to be dismissed (Boeri and Jimeno, 2005), but no threshold effect on firm growth was found.

Comparing these results is not straight forward because the reforms differed in many ways. In any case, none of the studies concern exemptions from a last-in-first-out-rule, which does not exist in those countries.

Cross-state differences of employment protection legislation in the United States, or cross-country differences elsewhere (Autor et al., 2004, 2006, 2007), have also been used to investigate its effects on net employment levels. In general, stricter employment protection has been found to reduce both hirings and dismissals. However, most reforms have lacked treatment and control groups, complicating evaluation. Results might thus be driven by omitted variables correlated with the reforms, which themselves might have been driven by employment trends.

4 Data and empirical method

4.1 Data

Limited liability firms in Sweden are required to submit an annual report to the Swedish patent and registration office (PRV). The dataset we use includes all variables found in the annual reports, - i.e., measures of profits, number of employees, salaries, fixed costs, and liquidity - gathered from PRV by PAR, (a Swedish consulting firm) on limited liability firms active at some point during 1996-2010. We focus on limited liability firms since they tend to have higher growth, and growth ambitions than other legal forms (Storey, 1994; Harhoff et al., 1998).

We use firm-level data since, as noted the exemption from last-in-first-out was applied at firm-level, not on establishment-level. Firms with less than 5 or more than 16 employees were excluded to avoid having too large differences between the treatment and the control groups. The final sample then consists of 47,896 firms, and 169,353 firm-year observations.

Truncating the treatment group - excluding firms with 1-4 employees - will bias the results upwards if some firms fall out of the group during the study period. On the other hand, truncating the control group - excluding firms with 16 or more employees - will bias the results downward if some firms grow out of the group during the study period, and hopefully the two biases will cancel each other. There was little movement between the groups either before or after the reform[3], and no obvious differences in the distribution of firms by size-class before and after the reform (Figure 1).

[Figure 1 about here]

Another potential problem is that after the reform, firms could self-select into treatment, which would reduce our estimate of the treatment effect. However, the reform was quite sudden, and reducing employee numbers takes time. To minimize the possibility of this behavior affecting our results, we restrict the number of post-reform years to three[4].

When investigating firms growth, researchers need to choose the growth indicator, type of growth measurement, and process of growth they are interested in (Delmar and Davidsson, 1998). 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 they tend to be only modestly correlated (Shepherd and Wiklund, 2009; Coad, 2010), most studies suggest that the results are not very sensitive to which is chosen (Daunfeldt et al., 2014). We use employment as growth indicator since our purpose is to study the effects of the last-in-first-out principle on employment.

Researchers also need to choose the type of growth measurement, i.e., whether growth is measured absolute, or relative, both of which can be biased. Relative-growth measures tend to favor small firms due to regression to the mean, whereas absolute measures tend to favor large firms (Delmar et al., 2003). We focus on absolute changes since the aim of relaxing employment protection legislation is to increase the total number of employees, not relative growth rates[5]. Any bias in favor of large firms will result in a more conservative estimate of the reform effect.

Finally, researchers need to choose the process of growth they are interested in, organic (new hiring internal to the firm) or acquired (gaining employees through external acquisitions mergers). Due to lack of data on mergers and acquisitions, most studies use total growth (the sum of organic and acquired growth), as do we.

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

$$G_{i,t} = employees_{i,t} - employees_{i,t-1} \tag{1}$$

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

The probability that a company hires an additional employee was lower during the 3 years after the reform, than before, especially for firms at the ten-employee threshold (Figure 2). Growth for firms with 10 employees is around 3.4 percentage points lower than one would expect, indicating that the incentive to grow has been reduced for firms close to the threshold size.

[**Figure 2 about here**]

The probability of hiring an additional employee is increasing in firm size, i.e., smaller firms are much less likely to grow than are larger ones, confirming their lower growth ambitions (Nightingale and Coad, 2014). We therefore restrict our treatment group to firms with 5-9 employees, and separately analyze firms with just 9 or 10 employees. The likelihood of firms having 9 or 10 employees prior to the reform is presumable more or less random, but afterward firms with 10 employees can no longer grow without passing the exclusion threshold. Differences in employment growth after the reform are thus probably related to introduction of the threshold.

4.2 Empirical method

We first test Hypothesis 1 - that firms with 5-9 employees increased their number of employees more than did larger ones after being granted the power to exclude two from the last-in-first-out principle - by estimating

$$\begin{aligned} G_{i,t} = & \alpha_0 + \beta_1 D_t + \beta_2 D^g + \beta_3 (D_t * D^g) + \beta_4 S_{i,t-1} + \\ & \beta_5 Age_{i,t} + \gamma_1' I_j + \gamma_3' I_j * trend + \gamma_3' R_m + \gamma_4' T_v + \\ & \gamma_5' R_m * trend + \epsilon_t \end{aligned} \tag{2}$$

where D_t is a dummy variable for the treatment period (2001-2003); D^g is a dummy for belonging to the treatment group; $S_{i,t-1}$ is total revenue in period $t-1$; $Age_{i,t}$ is firm age; I_j and R_m, and T_v are industry-specific, regional-specific, and time-specific fixed effects. Industry-specific and region-specific fixed effects control for whether employment growth is determined by time-invariant heterogeneity across industries and regions, while time-specific fixed effects control for time-variant heterogeneity (e.g., business cycle effects) that might explain differences in employment growth. 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 increased their number of employees after the reform more than did firms with 11-15 employees, i.e., firms above the exclusion threshold.

We control for firm age and size since they are usually included as controls in the firm growth literature (van Praag & Versloot, 2008). Many empirical studies have tested Gibrat's (1931) proposition that firm-growth is independent of firm size (Sutton, 1997; Caves, 1998). Recent studies tend to reject this hypothesis, 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) found that there may be no systematic relationship between firm size and firm growth after controlling for firm age.

Using a linear probability model, we then test Hypothesis 2 - that, after the 2001 reform, firms with ten employees are less likely than firms with nine employees to grow - by estimating

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

where $DG_{i,t}$ is a binary dependent variable equaling one if firm i had positive growth in period t, otherwise zero. We expect $\widehat{\delta}_3 < 0$, i.e., that firms with 10 employees grow less than did firms with 9 employees after the reform, so that they can maintain their ability to exclude two key employees in event of lay-offs.

5 Results

When using difference-in-difference estimation, a key assumption is that the outcome variables would have had parallel trends for the treatment and control groups after the reform in the absence of treatment. This is not formally testable, but Figure 4 shows the trends in absolute employment growth for firms in a neighborhood below the exclusion threshold (5-9 employees) and above it (11-15 employees), before and after the reform. The results indicate that trends were similar before the reform.

[Figure 3 about here]

Our results when estimating Equation (2) are presented in Table 1. First, results are presented without any control variables (Model 1). Control variables for firm size (lagged), and firm age are then added in Model 2, and industry-specific and region-specific time trends in Model 3 and 4, respectively. All estimated models contain fixed effects for regions, industries and years. The results indicate that the estimated coefficients appear robust to the inclusion of control variables[6].

[Table 1 about here]

The estimated treatment effect ($D_t * D^g$) is positive and significant, with a coefficient of about 0.16 in the full model specification. This indicates that, on average, firms with 5-9 employees increased their number by 0.16

individuals per year more than did firms with 11-15 employees after the reform. Our treatment group with 5-9 employees consists of 26,539 firms, indicating that the reform added 4,246 more new jobs per year after the reform. We thus cannot reject hypothesis 1, suggesting that the last-in-first-out rule acts as a growth barrier and prevents small firms from increasing their number of employees.

To reduce any problem of self-selection into the treatment group, we alternatively excluded more observations near the exclusion threshold, including firms with 5-7 and 13-15 employees in the treatment and control groups, respectively. The estimated treatment effect was then slightly smaller (0.15), see Table A3 in the Appendix.

Four similar models are also estimated based on Equation (3), testing whether firms with ten employees are less likely than firms with nine to increase their workforce after the reform (Table 2). Firms with ten employees are thus our treatment group, whereas firms with nine constitutes our control group, reducing our dataset to 31,207 firm-year observations.

[Table 2 about here]

The estimated treatment effect ($D10 * D_t$) is -0.0341 in the full model specification (Model 4), indicates that, on average, after the reform, firms with ten employees (just below the exemption threshold) are 3.4 percentage points less likely to add an employee than are firms with nine employees. Among the 16,066 firms in the treatment group, 548 more per year would thus have added an employee if doing so wouldn't have taken them over the exclusion threshold.

To test the validity of these results, we alternatively analyze whether firms with nine employees are less likely to hire an additional employee after the reform than are firms with eight, and whether firms with ten employees are more or less likely than firms with eleven. No substantial effects are found (Table A4 in the Appendix), suggesting that the lower hiring by firms with ten employees compared to those with nine is driven by the 2001 reform.

6 Summary and conclusions

Many firms do not grow, despite of having high profits. We realized that a 2001 legislative reform in Sweden provided a natural experiment to investigate whether the strictness of the employment protection legislation prevents small firms from growing. After the reform, firms with up to ten employees could exclude two employees from the so-called last-in-first-out rule, if they were deemed to be of high value to the firm. This rule stipulates that firms need to dismiss the last hired employee in event of lay-offs, regardless of that employees value to the firm. The reform was unexpected, and did not affect all firms uniformly, making the use of difference-in-difference estimation to establish causal effects ideal.

Our results indicated that the last-in-first-out principle in Swedish employment protection legislation prevented firms from growing. Firms with 5-9 employees were found to increase their number of employees by 0.16 more per year than did firms with 11-15 employees. This implies that, just during 2001-2003, 4,246 new jobs were created each year because of the reform.

After the reform, firms with ten employees would exceed the non-exemption threshold if they added an employee. Such firms were also found to be 3.4 percentage points less likely to add an employee after the reform than were firms with nine employees. This implies that firms with ten employees wanted to maintain their ability to exclude two key employees in event of lay-offs, and that the size-threshold thus hindered firms from growing.

Previous studies have found that the 2001 reform of the Swedish employment protection legislation led to a number of other 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).

Our results differ from von Below and Skogman Thoursie (2010), who found no effect on net employment. One reason might be that we included firms with 5-9 employees in the treatment group instead of firms with 2-10 employees. Finding causal effect with difference-in-difference estimation depends on the treatment and control groups being as similar as possible so that the treatment can be regarded as randomly assigned. But firms with 2 employees differ significantly from firms with 11 employees, not least with regard to growth ambitions (Figure 3; Nightingale and Coad, 2014). They also used a relative growth measure, changes in hiring and dismissal relative to total number of employees as their dependent variable, while we used the absolute number of employees within the firm. We believe our measure is preferable since firms seeking to grow usually define the number of additional employees they want to hire, rather than targeting some percentage of growth.

However, we found a net positive effect on employment even when firms with 2-4 employees were included in our treatment group, and our results were qualitatively similar even when we used a relative growth measure (results available upon request). Possibly our results differ from those of von Below and Skogman Thoursie (2010) because our study was restricted to limited liability firms, whereas they also included public firms and sole proprietorships. Their not finding an effect on net employment might thus 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 of potential employees in the labor market. Employers may be less likely to hire applicants who are considered risky - for example, those with less work experience, with foreign education or long time unemployed - when employment protection is restrictive (Kugler and Saint-Paul, 2004). This could lead to higher unemployment among those groups (Skedinger, 2010). Seniority rules (such as the last-in-first out) benefit older native-born workers more than younger ones or immigrants. The effect of the reform could thus also differ across industries, since the last-in-first-out principle might hinder growth more in those which tend to hire the young and immigrants.

Employment protection reforms might also influence *how* firms grow, not just how *much* they grow. Future studies should therefore investigate whether mergers and acquisitions are influenced by reforms intended to reduce hindrances to growth.

We know that many firms do not grow, despite high profitability. It thus becomes important to investigate whether it is growth barriers that prevents them from growing. If this is the case, many more jobs could be created if these barriers were removed. There may be other possible hindrances to growth - besides the last-in-first-out principle - and we believe that future studies should consider if it exist other natural experiments that can be used to analyze their effects on firm growth.

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7 Footnotes

1. We exclude firms with 10 employees from the analysis since they are in the treatment group but would move beyond it with a new hiring.

2. If the reform was unexpected, we would not see an effect before 2001. To test this, we perform placebo estimations with hypothetical reform years. Our results indicate no effects of the hypothetic reform years on employment growth, and are presented in Tables A1 and A2 in the Appendix.

3. We investigate this by calculating transition probabilities. Our results are presented in Table A5 in the Appendix.

4. We have also tried restricting the study period to two years. The results are qualitatively similar, and available from the authors upon request.

5. We cannot distinguish between permanent and temporary employees. This is not a problem when defining firm size, for which purpose the legislation includes both, but the last-in-first-out rule only applies to permanent employees.

6. We also estimated all models without time-, industry-, and regional-specific fixed effects. All results remained qualitatively similar, and are available from the authors upon request.

8 Tables and figures

Figure 1: Number of firms by size-class and period)

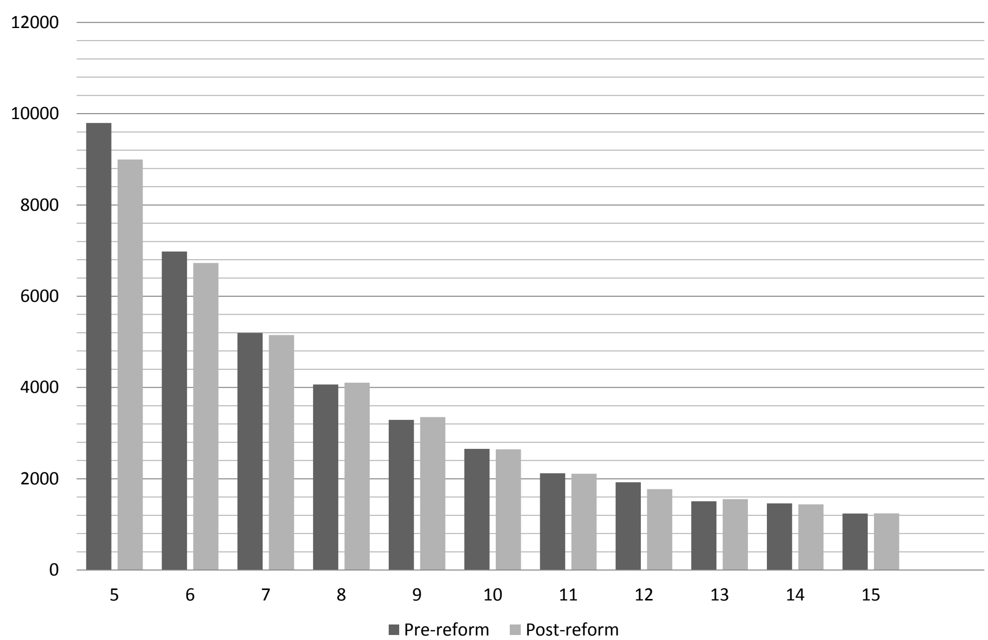

Figure 2: Probability of positive growth by number of employees and period (averages over 1997-2003)

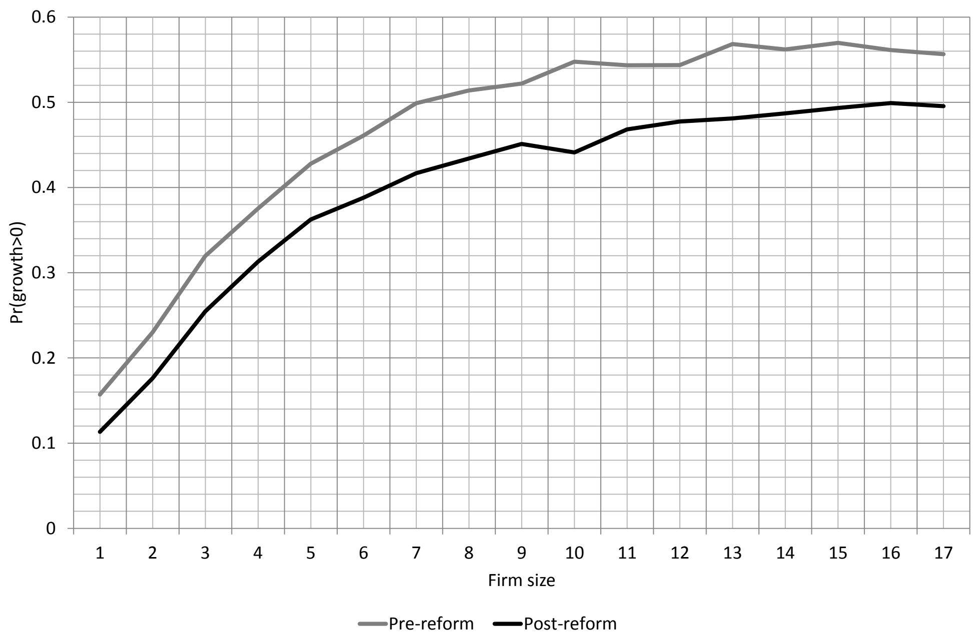

Figure 3: Absolute growth trends by number of employees, 1998-2004

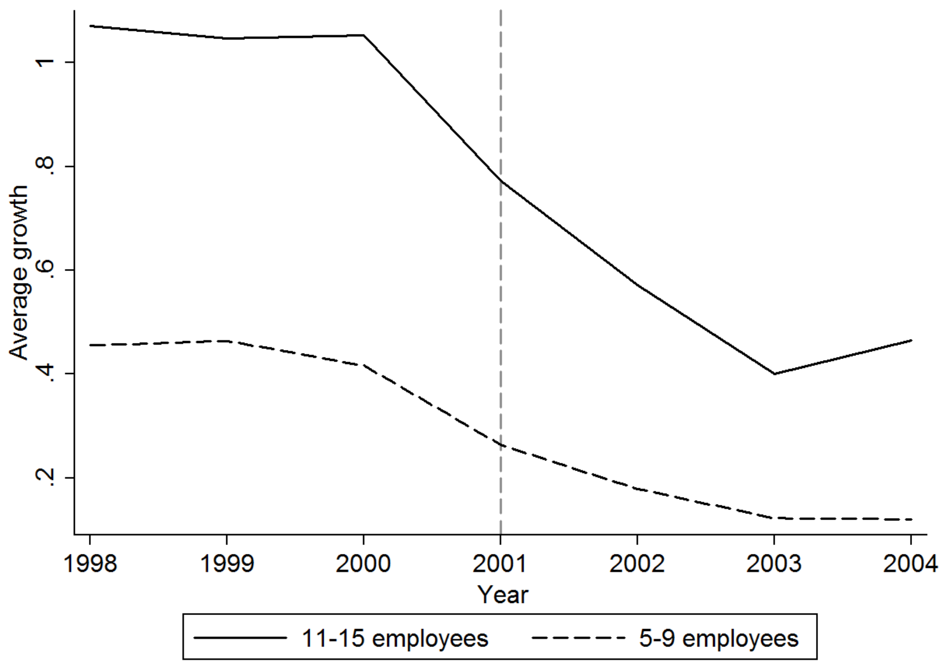

Table 1: Effects on employment growth in treated firms compared to control group, difference-in-difference estimation, 1998-2003.

VARIABLES	Model 1	Model 2	Model 3	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
Industry FE	YES	YES	YES	YES
Regional FE	YES	YES	YES	YES
Time 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: Threshold effect comparing firms with 9 to firms with 10 employees, difference-in-difference estimation, 1998-2003

VARIABLES	Model 1	Model 2	Model 3	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
Industry FE	YES	YES	YES	YES
Regional FE	YES	YES	YES	YES
Time 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.

9 Appendix: Robustness checks

If, as we believe, the reform was unexpected by Swedish firms, we would not see an effect before 2001. To validate this, we performed alternative estimations with hypothetical reform years 2000 and 2002, using the same empirical model as in Table 1 (Table A1). In order to reduce problems with hypothetical treatment years overlapping the true reform year, the estimations in Table A1 is based on only one year before and one year after the hypothetical treatment years. This also explains why no trends are included. No significant effects of the hypothetical reforms were found, indicating that the 2001 reform was unexpected by firms.

When doing placebo estimations for the threshold effect given hypothetical reform years (Table A2), it is not possible to limit to only one year before and after. Just as on Tables 1 and 2, we therefore used three years before and after the hypothetical reforms. Due to limitaions in the data, the estimation using 1999 as reform year only includes two years before and after the reform, which is why the dummy variable for the treatment period is omitted.

Though smaller than the effect of the true reform in 2001, a statistically significant effect for a hypothetical reform in year 2000 is found. But this is not surprising, since two out of the three hypothetical treatment years are actual treatment years when firms with less than ten employees in fact received treatment.

To reduce the probability of effects from self-selection into the treatment group after the reform, we also excluded firms close to the size-threshold

Table A1: Effects from hypothetical reforms in 2000 and 2002, difference-in-difference estimations

VARIABLES	2000	2001	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: Threshold effect at hypothetical reform years, 1999-2003, difference-in-difference estimations

VARIABLES	1999	2000	2001	2002	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.

so that the treatment group consists of firms with 5-7 employees, and the control group of firms with 13-15 employees. The estimated treatment effects is slightly smaller than in the standard model shown in Table 1.

Table A3: Treatment effect, excluding firms close to the size threshold, difference-in-difference estimation (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.

If our results (Model 2, Table 2) are driven by 2001 reform, no significant treatment effects should be observed if we compare firms with 8 vs. 9 employees(Table A4, $D9*D_t$) or firms with 10 vs. 11 (Table A4, $D11*D_t$), and this is indeed the case, which strengthens our conclusion that the treatment effects observed for 9 vs. 10 employees (Table A4, $D10 * D_t$) were driven by the reform.

Table A4: Effects of hypothetical threshold levels, difference-in-difference estimations

VARIABLES	8 vs 9	9 vs 10	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.

Since self-selection into the treatment group after the reform is possible, we also analyze movements between groups by calculating transition probabilities (Table A5). The probability of staying in the original group is high in both periods, 0.87-0.90 in the pre-reform period and 0.84-0.93 in the post-reform period. The probabilities of moving from the treatment group

Table A5: Transition probabilities during pre- and post-reform periods

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

to the control group is similar those of moving oppositely. Firms became somewhat less likely to move from the treatment group to the control group after the reform (0.07), whereas the opposite result is found for the control group (0.16). However, it is not possible to draw conclusions regarding causal effects just from this.