The Effect of Employment Protection on Labor Productivity*

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Abstract

The theoretical predictions of how employment protection affects firm productivity are ambiguous. In this paper I study the effect of employment protection rules on labor productivity using micro data on Swedish firms. A reform of the employment protection rules in 2001 made it possible for small firms with less than eleven employees to exempt two workers from the seniority rules. I exploit the reform as a natural experiment. My results indicate that increased labor market flexibility increases labor productivity. The increase is not explained by capital intensity or the educational level of workers.

Keywords: Employment Protection, Labor Market Regulations, Labor Productivity, Last-in-First-out Rules

JEL classification: J23, J24, J32, J38, M51, K31, D22

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1 Introduction

There is a wealth of literature on employment protection and how it affects the labor market, but predictions on how employment protection affects productivity are ambiguous. Theory is more or less unanimous on the result that employment protection increases firms' firing costs. Restraining efficient job separation may reduce efficient job creation and firms' abilities to freely adjust their labor according to demand (Mortensen and Pissarides 1994; Lazear 1990; Saint-Paul 1997; Hopenhayn and Rogerson 1993). Higher adjustment costs will lead to reduced employee turnover, less hiring and firing, which could in turn result in slower adjustment to structural change. Restricting firms' abilities to freely adjust their labor according to demand would have a negative impact on productivity, but higher costs of firing could also create incentives for firms to increase their investments in R&D and human capital (Koeniger 2005; Nickell and Layard 1999). However, R&D investments that are made under a rigid labor market regime could be less productive in the long run since they are more likely to focus on improving existing products instead of introducing new ones (Saint-Paul 2002). Due to decreased risk of discharge and longer employment spells, job security regulations may also have the effect of workers acquiring more firm specific skills, which could increase firm productivity through increased human capital (Belot et al. 2007). Given the multiple mechanisms through which employment protection can influence productivity, the relationship between the two is anything but clear.

The empirical literature has focused mainly on the effect of employment protection on outcomes such as job flows (Autor et al. 2004; Kugler and Saint-Paul 2004; Kugler and Pica 2008). Studies on productivity are more scarce and has often been confined to cross country analyses (Bassanini et al. 2009; DeFreitas and Marshall 1998). A problem inherent in cross-country studies is the comparability of legislations across countries (OECD 2004). Only two previous studies use variation within a country to try to establish a causal effect of employment protection on labor productivity (Autor et al. 2007; Boeri and Garibaldi 2007).

In this paper I present empirical results that show that increased labor market flexibility increases labor productivity. I analyze how job security regulations affect labor productivity and the focus is on Sweden and its particular rules of seniority. I use a reform in the Swedish last-in-first-out (LIFO) rules as a quasi-experiment to estimate the effect of a less stringent employment protection on labor productivity. All firms in Sweden have to abide by the LIFO rules. The LIFO rules involves a list of priority and stipulates that the last one hired is the first one to go in case of redundancy. In 2001, a reform loosened the LIFO rules but only for small firms with less than 11 employees. I exploit the 2001 reform using a difference in differences (DiD) framework and I find that the reform increased labor productivity by about 2 percent in the treatment group of small firms compared to a control group of larger firms. The effect is not explained by an increase in capital intensity or an increase in workers' educational level.

With this study I am able to use within country variation to try to establish a causal effect of employment protection on productivity and I contribute to a field where there is so far a limited number of empirical studies (Autor et al. 2007; Boeri and Garibaldi 2007). The result that labor productivity increases with increasing labor market flexibility stand in contrast to previous studies. Autor et al. (2007) found that introducing restrictions on a firm's ability to fire workers, i.e., a decrease in labor market flexibility, led to an increase in labor productivity through capital deepening. This suggests that the effects of employment protection is complex and might not be symmetric. Boeri and Garibaldi (2007) found that an increase in the use of fixed term contracts decreased labor productivity.

In comparison to the two previous studies, I use full population data on firms in Sweden from 1998 to 2003. Using full population data avoids potential problems with sample selection. It also allows for the inclusion of variables such as firm age, which is shown to be important but has been left out in previous studies. In addition, the 2001 reform results in a discrete change in employment protection for a well defined group of firms, thereby providing a natural experiment to assess causality and estimate different treatment effects. However, this setting is accompanied by some potential difficulties. Firms are able to adjust the number of employees, which is the underlying variable in this quasi-experimental setting. Under ideal conditions the baseline DiD estimation will give me an estimate of the average treatment on the treated effect (ATT). In order to obtain estimates that are not biased by potential self-selection, whereby firms assign themselves in or out of treatment, I also estimate the intention-to-treat (ITT) and the local average treatment effect (LATE). The LATE in this setting will capture the effect of the reform on compliers, i.e., those firms that stay small. The main results are similar for both the ATT effect and the LATE. This indicates that self-selection is not driving the results.

The fact that the reform increased labor productivity by about 2 percent is non-negligible. According to official statistics, the annual percentage change in labor productivity in Sweden between 1998–2003 is estimated to have been 1.9 percent (Eurostat). A further elaboration reveals that the older firms appear to be driving the positive results. This could be an effect of older firms being more stable over time in terms of size, preventing them from growing out of the treatment group of small firms. It could also be a result of the time it takes for managers to learn about the idiosyncratic productivity of their workers. The results are also driven by the smallest firms, which is likely an effect of the specific outline of the reform. The reform made it possible for firms with less than 11 employees to exempt 2 workers from their lists of priority. Instead of having to fire the worker with the least tenure, they are free to choose among the 3 workers with the least tenure. Since the exemption is in absolute numbers, it is not proportional to size and the effect is greater the smaller the firm.

Based on the standard labor market models and theory on worker effort, I discuss different mechanisms that could explain the increase in labor productivity. The reform could have caused a behavioral change in workers, and/or change in employment turnover, and/or made it easier for small firms to retain or lay off personnel based on the worker's idiosyncratic productivity. In addition, an increase in labor could be a result of capital deepening or an increase in human capital. To investigate whether the results could be explained by capital deepening, I exploit data on book value to get a measure of both capital per labor and total factor productivity (TFP). The results indicate that the increase in labor productivity is explained by an increase in TFP rather than capital intensity. This suggests that the increase in labor productivity is due to an increase in efficiency. To investigate whether the reform affected human capital, I investigate the composition of the educational level of workers. The results indicate that the ratio of workers with the highest attained educational level, corresponding to at least 3 years of post-high school education, decreased after the reform. This could be an effect of a less stringent screening of new hires and is, if anything, likely to have a negative effect on productivity.

I begin by giving a summary of the Swedish LIFO-rules and the 2001 reform followed by a brief discussion on theoretical considerations and previous related studies. Section 4 describes the data and section 5 presents the empirical estimations including discussions on the empirical framework. Section 6 concludes the findings.

2 Institutional Setting

Since 1974, all Swedish firms have been adherent to the Swedish Employment Protection Act (EPA) (Skedinger 2008). The EPA imposes the last-in-first-out (LIFO) regulation, meaning that the last employed is the first one to go in case of shortage of work (SFS 1982:80). The LIFO-regulations stipulate that, in case of redundancy, the employer has to comply with the established lists of priority. The lists of priority rank individuals based on accumulated tenure within the firm. The lists apply to the establishment level, meaning that workers within the same firm but at different establishments are on different lists of priority. If two workers have accumulated the same tenure within the firm, priority is given to the oldest one (SFS 1982:80). The LIFO-rules also stipulates that if a worker has been laid off due to redundancy, he or she has priority if the firm is rehiring. Should a firm not comply with the LIFO-regulations the firm will have to pay damage. The dismissal will however not be invalidated. It should also be noted that the LIFO-rues only apply to workers of the same management unit and members of the same trade union. The LIFO-rules do not apply to members of the employer's family, workers in managing positions, persons hired to work in the employer's household, or workers participating in employment subsidy programs (1§ in SFS 1982:80).[1]

On January 1st in 2001, an exemption from the LIFO-rules was introduced for firms with 10 or less employees. These small firms are allowed to exempt two employees with the least accumulated tenure from the lists of priority, meaning that they are free to choose among the three employees with the least tenure in case of dismissal. The 2001 reform thus constitutes a discrete change in employment protection for a specific group of firms. In addition, it has two features that makes it particularly suitable as a natural experiment. The process from discussion to implementation was fast and it is unlikely to have been anticipated. The reform was not discussed in public until the beginning of February 2000. It was voted in favor for in October 2000, and was implemented on January 1st, 2001. Furthermore, the reform was a result of an unusual cooperation between the green party and the center right-wing opposition parties in parliament. It is reasonable to assume that it was not until sometime in the middle of 2000 that it became clear that the unlikely collaboration of political parties would prevail.[2]

Although the LIFO-rules apply to the establishment level, the 2001 reform threshold of 10 employees applies to the firm level. This means that a small firm can exempt at most 2 employees, independent of its number of establishments. When determining firm size, the law stipulates that one should disregard members of the employer's family, workers in managing positions, persons hired to work in the employer's household, and workers participating in employment subsidy programs. One should not, however, make a difference between types of contracts, meaning that workers on temporary and full-time contracts have equal weight.

The reform stipulates an exemption in absolute numbers, which means that it is not proportional to size. For example, a firm with 10 employees can make an exemption for the last two persons hired, still leaving 7 workers (70%) protected. In contrast, the reform leaves none of workers protected in a firm with 3 employees. It is thus designed to have a larger effect the smaller the firm (see Table 1).

The Swedish LIFO-rules are generally considered easy to circumvent although there is to my knowledge no comprehensive study on this(Calleman (2000); Skogman Thoursie 2009). Collective agreements can be used to contract upon a deviation from the LIFO-regulations in

[1]See (Skedinger 2008) for an elaborate discussion on the Swedish Employment Protection Act.

[2]The different actions by the parliament leading up to the reform are accounted for by Lindbeck et al. (2006)

advance. However, small firms are less likely to have these agreements (Företagarna 2011). More and more firms also use fixed- or short-term contracts, which do not fall under the LIFO-rules. In addition, firms are able to hire individuals through the use of temporary work agencies. The worker is typically on a fixed-term contract with the temporary work agency and there is thus no employment contract between the individual worker and the firm.

3 Theoretical Considerations and Previous Studies

The number of empirical studies that use within country variation to asses the effect of employment protection on productivity are limited. Only the two studies by Autor et al. (2007) and Boeri and Garibaldi (2007) use some type of exogenous variation to try to establish causality. Autor et al. (2007) use the adoption of wrongful discharge in US courts to study the effects of firing costs on productivity. They find that total factor productivity decreases with firing costs whereas labor productivity is increasing. This could be attributed to capital deepening and an increase in the skill level of workers. Boeri and Garibaldi (2007) use a reform in Italy in 1997 that gradually increased the use of fixed term contracts. They find that the increase in temporary workers lowered labor productivity. These results support their theoretical model, which predicts that in good times firms will expand the number of workers in the region of their production function that has a decreasing marginal productivity.

According to Autor et al. (2007), the standard models of the labor market can be divided into a competitive model (Lazear 1990) that is commonly used by labor economists, and an equilibrium unemployment model that is more used by macro economists (Mortensen and Pissarides 1994). Both models render ambiguous effects of employment protection on productivity, and both models assume that productivity could be negatively affected if employment protection causes firms to retain less productive workers. On the other hand the screening of new hires could become more stringent (competitive model), alternatively the firms could increase the productivity threshold at which they are willing to hire (equilibrium unemployment model). Both models assume that firm productivity is only affected if there is a decrease in job flows.

There is however an important aspect of employment protection and productivity that is disregarded by both standard models and the studies by Autor et al. (2007) and Boeri and Garibaldi (2007). Ichino and Riphahn (2005) develop a framework for understanding how employment protection affects worker's behavior. Employment protection is shown to limit the firm's willingness to monitor and fire workers that are lazy or shirking. The results relate to the theory on wages and the threat of firing as a method of disciplining a worker by

Shapiro and Stiglitz (1984). If employment protection affects the work effort of employees, it can have an effect on productivity, regardless of job flows.

Based on the theoretical observations, the Swedish reform could affect productivity in different ways. First, the increased labor market flexibility could have caused an increase in employment turnover rates. This could affect productivity in accordance with the standard models. Second, the reform could have caused a behavioral change in workers, changing their level of effort in line with the observations by Ichino and Riphahn (2005) and Shapiro and Stiglitz (1984). Third, a change in the cost of adjusting labor might change the choice of capital intensity which directly affects labor productivity. Fourth, the screening of new hires could change with the cost of adjusting the labor force. Lower adjustments costs would allow for a less stringent screening of new hires. If the composition of human capital within the workforce changes with a less stringent screening it could have an affect on productivity. There is also a fifth implication of the reform, more specific to the Swedish EPA. The LIFO-rules imply that firms cannot separate or keep workers based on their idiosyncratic productivity. Even if turnover rates, worker efforts, capital intensity and human capital would not change with the reform, one could expect an effect on productivity from the increased possibility to retain more productive personnel. The effect would come simply from having a larger pool of candidates to choose from. There is thus several channels through which the reform could have an effect on productivity.

There are two previous studies on the Swedish reform that relates to this discussion. Von Below and Thoursie (2010) investigate the effect of the 2001 reform on employment turnover and find that both hires and separations increased with about 5 percent in the smallest firms, leaving net employment unaffected. The effects are argued to be small but it is nevertheless an indication that the reform could have an effect on labor productivity according to the standard models. The study by Olsson (2009) investigates the effect on sickness absence, and finds that it is reduced for the group of small firms that were comprised by the reform. This relates to the theories on work effort. The effect of a decrease in sickness absence on productivity is however ambiguous. On the one hand, if the reform triggered a decrease in moral hazard behavior, productivity is likely to have increased. If the reform on the other hand made people attend work sick, productivity is likely to have decreased.

4 Data

The data used are firm and establishment data from Statistics Sweden (SCB) on all firms with at least one employee. Establishment data on employment, firm age, enterprise group affiliation, and education are obtained from the regional labor market statistics (RAMS) and are then aggregated to the firm/company level, meaning that it includes all of its establishments. Financial data are from the Structural Business Statistics (Företagens Ekonomi) and contribute information on value added, capital, ownership status and industry affiliation at the firm level. The two statistical sources are matched and the complete data set covers about 200,000 observations per year from 1998 to 2003.

SCB uses a unique firm id that traces firms through changes of corporate identity numbers that could occur due to mergers, acquisitions, and hiving-off.[3] This facilitates the process of following firms over time. Number of employees is defined according to number of employees in November earning a salary that exceeds a certain threshold (Statistics Sweden 2006a).[4] I am not able to separate workers with permanent and temporary contracts. However, the 2001 reform does not differentiate between permanent and temporary contracts when determining the size threshold.

To get an estimate of labor productivity, I use the natural logarithm of value added per employee. Firm value added is calculated by SCB as value of production minus value of depletion. To get an estimate of capital, I use book values of machinery and structures. As with the variable on employees, value added and book values are available only for firms that are classified as active in November each year. The financial data are deflated using the fixed consumer price index from SCB. To construct the measure of capital per labor used in my estimations, the deflated sum is divided by the number of employees before taking the natural logarithm. For the construction of total factor productivity (TFP), see section 5.3.1.

The data from SCB covers all firms in all industries except for certain firms within the finance sector. Moreover, as of 2001, fishing and forestry sectors together with self-employed are included in the statistics (Statistics Sweden 2006b). Fishing and forestry amount to about 4,500 observations, which are deleted in order to facilitate the identification of the reform. Moreover, the majority of self-employed are presumed to be in firms with one or two employees and removing these size categories will remove most of the inconsistency over time.

The 2001 reform took place in the middle of an information technology boom and bust cycle. To further facilitate the identification of the reform, all firms within the ICT industries are dropped from the estimations (see Table A2). The inclusion of these industries do not change the main results (see Table A3). The sample is further restricted to corporations (limited companies), and I exclude firms within the agricultural sector as well as government-

[3]The identifier is called FAD (Företagens och Arbetsställenas Dynamik).

[4]To determine the threshold, individuals are divided into 25 categories depending on variables such as age, gender, and retirement pension. As an example, in 2005, for a male of age 25–54, the threshold is an annual salary of 50,036 SEK (Statistics Sweden 2009). This is equivalent of about USD 5,900, using the the exchange rate in February 15, 2015.

owned corporations. Some firms demonstrate negative value added causing problems with log transformation of the data. One could be skeptical of data showing very large negative values for value added because of the construction of the variable (value of production minus value of depletion). All negative values are dropped in the log transformation. However, this group of firms could contain start-up companies that do not initially have positive revenues, but do have costs for labor. This could constitute a potential bias to the identification of the effect of the reform. There is a risk of an overestimation if, for example, the reform increased the number of small firm start-ups with negative value added. However, an increase in the number of small firms with small but positive value added would lead to an underestimation. In table A1, firms with negative value added are listed for each year. The ratio of firms with negative values ranges from 1–2 percent. To account for some of the potential bias created by the log-transformation for values close to zero, I have included an estimation where I shift the data before the log-transformation (see table A4). Moreover, the differences in firm entry and exit rates does not seem to be systematically affected by the reform (see discussion in section 5.3.3). Information on firm age is limited to firms born after 1986. The data are truncated so that firms born before 1986 will have 1986 as their birth date.

Figure 2 depicts labor productivity for firms with 1 to 20 employees. The values for firms with one and two employees are high. As noted above, these firms are dropped from all estimations. The majority of self-employed are presumed to be in firms with one or two employees and removing these size categories will facilitate the comparison of firms below and above the reform threshold. Disregarding the smallest firms, the relationship appears to be somewhat linear and increasing.

5 Empirical Estimation

In order to estimate the effect of the reform I will use a DiD framework that defines the group of small firms with less than 11 employees as a treatment group, and compare the outcome to a control group of larger firms. The DiD will under ideal conditions give an estimate of the average treatment on the treated effect (ATT). From a policy perspective this effect is relevant since it evaluates the effect on small firms of being exempted from the LIFO-rules. It is not likely that all firms will have the exact same response to treatment, even conditional on covariate characteristics. A firm most likely possesses certain unobservables, such as specific encouraging atmospheres, cultural surroundings, bargaining power against the union etc., that makes firms with the same covariate characteristics respond differently to treatment. It is possible that these unobservables are systematically different between smaller and larger firms. This study is therefore less likely to recover the average treatment effect (ATE), i.e., the effect of the reform in the event that it were applied also to the group of larger firms.

The DiD setting also allows me to estimate the intention-to-treat effect (ITT) and the local average treatment effect (LATE). Firm size is the underlying variable in this quasi-experimental setting and firms are able to adjust their size, possibly based on their own idiosyncratic effect. This constitutes a potential selection problem. Both ITT and LATE will give me estimates that are un-biased by such a selection. There are some aspects of the reform that makes a DiD framework the preferred method. First, as noted above, the reform effect is decreasing with size, i.e, in the direction of a hypothetical kink in productivity. Second, firms that grow in size will eventually grow out of the treatment group. This process is likely correlated with productivity growth. Especially young firms with large growth ambitions are likely to be born into the treatment group only to increase rapidly in size. This will act as to prevent the shaping of a kink in productivity. In combination with a potential non-random selection of firms around the reform threshold, this makes a DiD framework preferable to e.g., a regression discontinuity approach.

5.1 Difference-in-Differences estimation

The DiD estimation hinges on the use of a control and a treatment group. In this setting the treatment is the 2001 reform and the treatment group consists of firms with less than 11 employees. The control group consists of those firms that are still confined to the LIFO-rules, which I limit to firms with 11–15 employees. The choice of control group is due to the fact that DiD is more plausible when treatment and control are more similar. Descriptive statistics for the two groups before and after the reform are shown in Table 1. Labor productivity has increased with the reform for both control and treatment groups. The average increase is however larger in the group of small firms, 0.05, compared to the larger firms, 0.028.[5] The difference in differences is the average change in productivity for firms in the treatment group minus the average change in productivity for firms in the control group which here amounts to 0.022. This is a first indication of the effect of the reform.

A panel of individual firm data is used to estimate the following baseline model

$$Y_{it} = \alpha + \lambda_t + \delta d_{it} + \beta(Post_t \times d_{it}) + X_{it}\gamma + \upsilon_{it} \tag{1}$$

where Y_{it} is the natural logarithm of productivity in firm i at time t, λ_t is a full set of year dummies. $Post_t$ is a dummy variable taking the value 1 if being in the treatment period, and d_{it} is a dummy variable taking the value 1 if being in the treatment group. The coefficient

[5] The numbers refer to the logarithmized values of labor productivity.

β estimates the treatment effect of the 2001 reform. Finally, X_{it} is a vector of firm specific characteristics that includes a full set of firm age dummies, industry-specific effects (3-digit NACE-code), and a dummy taking the value one if the firm belongs to an enterprise group. To simplify, I will suppress the notation from here on so that $D_{it} = Post_t \times d_{it}$.

The key identifying assumption is that of no interaction between the treatment group d_{it} and the treatment period $Post_t$ except for the 2001 reform, that is $E(\upsilon_{it}|D_{it}) = 0$. The year dummies controls for symmetric time effects. It is however possible that there is a compositional bias, that firms within the two groups have systematically different characteristics before and after the reform, which justifies the inclusion of additional covariates.

Except for treatment, the unobserved differences between the two groups needs to be the same over time in order for the DiD estimator to be valid. Figure 3 shows yearly average productivity for treatment and control group, respectively. As noted above, the larger firms have higher productivity than the smaller ones on average. A comparison of the yearly averages before the reform indicates that the assumption of parallel trends seems to hold. After 2001 the two series converge, indicating a positive effect of the reform.

An additional way to get an indication of the validity of the parallel trends assumption is to estimate annual treatment effects. This will also capture some of the dynamics of the reform. To capture yearly effects of the reform, I estimate the following model:

$$Y_{it} = \alpha + \lambda_t + \delta d_{it} + \sum_{t=1999}^{2003} \beta_t(\lambda_t \times d_{it}) + \upsilon_{it} \tag{2}$$

where year dummies, λ_t, are interacted with the treatment indicator, d_{it}, to generate a DiD estimate for each year, using the year in 1998 as a benchmark. The results are presented in Figure 4. No effects are found in the pre-reform years which strengthens the assumption of parallel trends. The post-reform yearly effects are at the highest in 2002 and decreases somewhat in 2003.

The DiD identification hinges on individual firms not assigning themselves into treatment based on their own idiosyncratic effect, i.e., adjusting their size to the reform. An employer can choose to separate and hire workers, and workers are free to seek employment in firms both within treatment and control. It is therefore possible for firms to adjust their size in order to fall in or out of treatment. One would suspect that the 2001 reform made it more attractive for both firms and job seekers to assign themselves into the treatment group of small firms, whereas movements in the other direction is perhaps less likely. By assigning themselves into treatment, the employer and the firm would face lower adjustment costs, and the job seeker would prevent herself ending up as the first person that has to go in case of redundancy. Moreover, in order to bias the results the possible selection should be driven by the firms' and/or workers' idiosyncratic productivity.

Because of the size threshold, rapidly growing firms can grow out of treatment and firms that have to downsize can move from control to treatment. If growth in employees is positively correlated with productivity, this could potentially create a downward bias on the DiD coefficients, i.e., underestimating the effect of the reform. It is more difficult to come up with a rationale for firms to move in the other direction. The less productive firms are then supposed to grow from the treatment group to the control group, and the more productive firms would stay small. Alternatively, the more productive firms are supposed to downsize so that they enter the treatment group and the less productive firms stay large. If there is such a selection it is probably more likely at the margin, precisely around threshold. Figure 5 plots the distribution of the number of employees, 1998–2000 and 2001–2003. There is no visible discrepancy around the threshold and the distribution is similar for the two time periods. A selection could however come from both workers and firms, and it is still possible that these two effects could offset each other. This could happen if for example more productive firms select themselves into treatment at the same time as more productive workers select themselves to the larger firms in the control group. It is however hard to find a convincing rational for such a scenario. A potential bias could also arise from differences in the negotiation strength against the union. Employers can elude the LIFO-rules by negotiating with the union, and small firms engaging in such a negotiation might have a weaker bargaining position.

As noted before, the DiD is more credible when treatment and control are more similar. One way to get an indication of the differences between the two groups is to plot the distribution of covariates. Figure 6 and 7 show the distribution of firms for different industries and ages. The distribution of industries, Figure 6, is similar for the two groups with a few exceptions.[6] The age distribution in Figure 7 shows that the control group has a larger share of firms that obtain the maximum age of 12–17 years.[7] The figure thus indicates a possible survival bias for the control group of larger firms, i.e., at the initial year, 1998, there appear to be a larger share of firms alive of size 11–15 than of size 3–10, and this pattern prevails. Surviving firms are likely more productive than average, since it is a condition for survival. However, in order for these observed differences to have an effect on the outcome, they have to affect how firms respond to the reform of increased labor market flexibility. The main

[6] The control group has a larger concentration of firms in sectors such as wholesale trade and manufacture of fabricated metal products (NACE 51 and 28). The group of smaller firms are somewhat more concentrated in industries such as retail trade(NACE 52), sale, and maintenance and repair of motor vehicles and motorcycles (NACE 50), and other business activities (NACE 74).

[7] The age distribution is skewed because the data is truncated so that all firms born before 1986 will accumulate in the maximum age category.

estimations include fixed effects for both industry and age, which should mitigate some of these problems.

5.2 Intention-to-treat and local average treatment effects

Since it is possible for firms to adjust their size in order to fall in or out of treatment, it is interesting to study the ITT, as well as the LATE.[8] ITT will give an estimate that is independent of the effect of potential cross-overs, and LATE will capture the effect of treatment on compliers, i.e., the firms that stayed in the treatment group. The ITT can in this setting be captured by substituting treatment with the treatment status some time before the reform took place.[9] Instead of letting treatment status vary from year to year, I will let treatment status be determined by size in year 1999.[10] I then follow these firms past the reform, regardless if they move between control to treatment. The ITT is estimated using equation (3), where I have substituted d_{it} and D_{it} with d_{i99} and Z_{it}, i.e., the treatment status in year 1999 and the corresponding DiD estimator, $Z_{it} = Post_t \times d_{i99}$. All covariates in X_{it} are defined in the year 1999 in order to be exogenous.

$$Y_{it} = \alpha + \lambda_t + \delta d_{i99} + \beta Z_{it} + X_{i99}\gamma + \upsilon_{it} \tag{3}$$

To address the potential endogeneity problem caused by firms and workers being able to select themselves in and out of treatment and to get an estimate of the LATE, I will also introduce an instrumental variables regression (Imbens and Angrist 1994). I will use Z_{it} as an instrument in a two-stage least-squares regression to estimate the following equation

$$Y_{it} = \alpha + \lambda_t + \delta d_{i99} + \beta \hat{D}_{it} + X_{i99}\gamma + \upsilon_{it} \tag{4}$$

where $\hat{D_{it}}$ is the predicted values from the first stage equation (5).

$$D_{it} = \omega_0 + \lambda_t + \omega_1 d_{i99} + \omega_2 Z_{it} + +X_{i99}\omega_3 + \mu_{it} \tag{5}$$

The coefficient β will estimate the LATE. This effect can be expressed as a Wald estimator by dividing the ITT with the difference in compliance rates between treatment and control (Angrist and Pischke 2009). The compliance rate in 2001 is 94% for the treatment group, and

[8] See Imbens and Angrist (1994) for a discussion on LATE.

[9] A similar strategy to capture the different treatment effects of the reform has been used by Olsson (2013) and Lindbeck et al. (2006).

[10] In Table A5 I show that the results hold also when letting treatment status be determined by firm size in year 1998 or 2000.

the corresponding rate for the control group is 75%.[11] The instrument Z_{it} is exogenous since the size of firms in 1999 is unaffected by post-reform outcomes. The reform was not discussed openly in public until 2000 and it is unlikely that the unusual cooperation of political parties that favored the reform could have been anticipated. Moreover, Z_{it} is relevant since it is correlated with post-reform treatment status. First stage equations are presented in Table A6 in Appendix. The F-values from these estimations are high which indicates that the instrument is strong. LATE also hinges on the assumption of monotonicity. Monotonicity in this setting requires that having less than 11 employees in 1999 does not make treatment status after the reform (i.e., having less than 11 employees after 2001) less likely.

If there is an edogeneity problem caused by firms and workers being able to select themselves in and out of treatment, the IV regression will still give me consistent estimates. The ITT is interesting from a policy perspective since it will estimate the effects of the policy change rather than the specific effect of being treated with a loosened protection of workers.

5.3 Results

Table 2 shows the three different estimated effects (ATT, ITT, and LATE) of the 2001 reform. The columns, (1)–(3), add the controls stepwise. The DiD coefficient estimates are positive for all three effects. Starting with the ATT, the size of the estimated coefficients ranges from 0.018 to 0.023, indicating that being exempted from the LIFO-rules increases labor productivity with about 2 percent.[12] When using treatment status in year 1999, the estimated ITT effect of the reform is positive but is slightly lower at 1 percent. Finally, the instrumental variable regressions to capture the LATE yield an estimated effect of about 2 percent.

Including all covariates will likely result in a more accurate estimation of the effect of the reform. Comparing the outcomes for the ATT and the LATE reveals that the effects of the reform are similar, but not identical, in size. The similarity between the LATE and the ATT suggests that there are no large selection effects. The effect off the reform is then due to changes in behavior rather than firm and workers selecting themselves into treatment or control based on their idiosyncratic productivity. There is still however a risk of a selection bias in the ATT estimations. As discussed above, a potential selection could come from both workers and firms and it is still possible that a selection effect of the two could offset each other.

[11] The compliance rate is conditional on the firm surviving into 2001. 59% of the firms that belonged to the treatment group in 1999 survived to 2001, and the corresponding share for the control group is 53%.

[12] With a log-linear model, a coefficient c on a dummy variable can be interpreted as a percentage with the following transformation: $100 \times [\exp(c) - 1]$.

The estimated ITT effect indicates an increase in labor productivity of about 1 percent. The lower coefficient for the ITT could be due crossovers where productive firms are more likely to grow out of the treatment group. Unlike the ATT that has a constant inflow of new firms, the ITT follows firms that are defined in 1999. Some of the positive effect of the reform that is present in the ATT estimations could be offset by the lack of influx of new firms, together with a movement of productive firms from treatment to control.

Controlling for systematic differences between industries decreases the size of the estimated coefficients slightly. Including age as a control variable increases the estimated ATT effect somewhat. Table 3 shows the ATT, ITT and LATE estimates divided into old and young firms. Each row corresponds to a different cut-off age for defining a sub-sample of young and old firms. Recall that age is defined as firm age in 1999 for both the ITT and LATE, meaning that there is no rejuvenation in these estimations, whereas the ATT has a constant influx of new firms. For the different sub-samples of firms younger than 9 there are no significant coefficients, regardless of treatment effect. For the ITT and LATE, the coefficients are only significant for the sub-samples of firms that are at least 10 years old. This indicates that it is the older firms that are driving the results.

Previous literature have found that age plays a key role for firm behavior. Small firms are younger, more volatile, grow faster but they also have a higher likelihood of exit (Haltiwanger et al. 2013; Henrekson and Johansson 2010). The results in Table 3 might be because of successful and ambitious small and young firms growing out of the treatment group independent of the reform.[13] Similarly, it is reasonable to believe that the group of old firms is slower growing, meaning that they are more likely to be stable and stay within treatment and control. The reform threshold constitutes a growth marker and the reform could have triggered more of a behavioral effect in older small firms that perhaps are less likely to have the growth ambitions of its younger counterparts. Another explanation for the effect of firm age is that it could take considerable time for managers to learn about the idiosyncratic productivity of their workers. It is hard to relate these findings to previous studies on employment protection. Firm age has not been widely discussed in this literature and one possible explanation for this is the lack of suitable data.

Next, to disentangle the effect of firms of different size within the treatment group, I estimate the following equation:

$$Y_{it} = \alpha + \lambda_t + \sum_{s=3}^{10} \chi_s Size_{ist} + \sum_{s=3}^{10} \beta_s (Size_{ist} \times Post_t) + X_{it}\gamma + v_{it}^{j} \tag{6}$$

where $Size_{ist}$ is a dummy variable taking the value 1 if firm i is of size s at time t. The

[13] Entry and exit rates did not appear to change with the reform, see section 5.3.3.

β_s is a coefficient of the DiD estimate for each of the 8 size categories s. The firms in the control group, size 11–15, are used as benchmark. Figure 7(a), shows the estimated β_s for the different size categories. The figure reveals that the effect of the reform is larger for the smallest firms. As anticipated, the effect appears to be decreasing with size. Recall that the reform allowed the exemption of 2 workers from the lists of priority, i.e., it is defined in absolute numbers and not proportional to size. Similarly, the estimated ITT coefficients in Figure 7(b) are not statistically significant for firms of size 9 and 10 close to the threshold.

5.3.1 Capital deepening and total factor productivity

In the previous section 3, I identified different mechanisms that could account for an effect on labor productivity. Certain characteristics have to be systematically different before and after the reform for the smaller and larger firms. One possibility is that the reform affected employee behavior such that moral hazard became less of a problem in small firms. The small firms could also have become more able to adjust their workforce to rapid structural changes, i.e., the reform increased employment turnover rates. The reform did make it easier for small firms to retain valuable workers, or conversely, to lay off less valuable ones. Finally, capital per labor and human capital could have increased in the group of small firms.

In this study I cannot directly assess changes in worker efforts. However, the previous study by Olsson (2009) on the Swedish 2001 reform found that sickness absence was reduced on average in small firms. Olsson (2009) found that there was a behavioral effect on workers, but the reform also caused firms to hire persons with higher tendencies to report sick. The effect of sickness absence on labor productivity is not clear-cut. Reduced absenteeism in the form of less moral hazard would increase productivity, whereas attending work sick would do the opposite. Von Below and Thoursie (2010) studied the reform's effect on turnover rates, and found that both hiring and separations increased for the smallest firms of size 2–5. This could account for some of the estimated effect on labor productivity. However, Von Below and Thoursie (2010) argue that the effect on worker flows is to be considered small.

It is not obvious beforehand how the Swedish reform would affect capital per labor ratio. When it becomes easier to separate and hire workers one could expect capital intensity to fall as a result of labor being more accessible and flexible. On the other hand, the increased possibility to retain valuable personnel could increase the willingness and desire to invest in capital. To investigate whether capital intensity changed with the reform I use the natural logarithm of capital divided by number of employees as an outcome variable. Data on book values for machinery and structures is used to get an estimate of capital. As a reference point, I will also estimate the effect of the reform on TFP using the following production function for each 2-digit industry and year:

$$\log(Y_{it}) = \alpha + \psi_{jt}\log(L_{it}) + \gamma_{jt}^{m}\log(K_{it}^{m}) + \gamma_{jt}^{b}\log(K_{it}^{b}) + \xi_{it} \tag{7}$$

where Y_{it} is defined as value added in firm i at time t. L_{it} is the number of workers, K_{it}^{m} is the book value of machinery and K_{it}^{b} is the book value of structures. The function is estimated using OLS for each industry j and time t. The residuals from the regressions provide the TFP measure. I choose to use this rather crude approach to measure TFP that does not address problems such as input choices.[14] The aim of this exercise is not to try to get an exact measure of TFP, but to get an estimate that is consistent over time and that can be used as a reference point when analyzing the effect on capital intensity and labor productivity.

Summary statistics are presented in Table 4 where I have limited the sample to only firms for which there is a TFP measure. TFP can only be measured for firms that have book values for machinery and structures, which limits the sample considerably. The total sample size is reduced from 374,352 to 105,667 observations. It corresponds to about 28 percent of the previous sample, 27 percent of the treatment group and 35 percent of the control group. Comparing the new mean values in Table 4 with the previous values from Table 1 reveals that the firms are on average 0.5–1 years older. The other variables are similar. Labor productivity is slightly higher in the new sample and the small firms are on average larger by 0.2 employees.

Judging from the initial DiD presented in the right most column of Table 4, there appears to be a relative increase in both capital intensity, labor productivity and TFP for the small firms. To estimate the effect of the reform I will use the same specification as before, focusing only on the most saturated model (3). To get an idea of whether the parallel trends assumption is satisfied for the new sample and new variables, I plot the yearly averages of capital labor ratio and TFP in Figure 9. The trends of the two treatment groups seems to be parallel for the TPF measure whereas there might be some divergence before the reform for the capital labor ratio.

The DiD coefficients for the different treatment effects are shown in table 5. The effect on both TFP and labor productivity is positive and similar to each other in size for all the three treatment effects. There is no significant effect on capital labor ratio. With a standard Cobb Douglas production function $Y = AK^{\alpha}L^{1-\alpha}$, where A is TFP, K is capital and L is labor, we have that a growth in labor productivity is equal to the growth of TPF and the rate of growth of capital, $\dot{y} = \dot{A} + \alpha\dot{k}$ (Sargent and Rodriguez 2000). Using this expression to interpret the three columns in Table 5, I conclude that the increase in labor productivity

[14] See Syverson (2011) for an overview on productivity measures.

is rather due to TFP than a result of increased capital intensity.

To ensure that the estimated effects on TFP are a result of the reform, and to get an indication of the validity of the parallel trends assumption, I plot yearly effects of both capital labor ratio and TPF in Figure 10. The two figures include the full set of covariates that are used in Table 5. There is a definite increase in TFP after the reform, whereas the yearly estimates before the reform are close to zero. This reinforces the premise that the increase in TFP is due to the 2001 reform. The yearly effects on capital labor ratio does not seem to change systematically with the reform.

Based on the results in Table 5 and Figure 10, I conclude that it is unlikely that the reform triggered an increase in capital intensity that could serve as to explain the increase in labor productivity. The increase in TFP indicates that it is rather an increase in efficiency.

5.3.2 Composition of workers

An increase in labor productivity could also be a result of a change in human capital. Higher education is believed to increase the productivity of a worker Becker (1975). It is difficult to find a compelling argument as to why the reform should have spurred an increase in the education level of workers. The screening of new hires could be affected by the reform since it became easier to hire and separate workers. If anything, one would expect the screening of new hires to be less stringent, possibly lowering the education level. To investigate whether the reform caused a change in the education level of workers I will use information on the ratio of workers in each firm with: i) pre-high school education, ii) high school education, iii) post-high school education, iv) at least 3 years of post-high school. The mean values before and after the reform is presented in Table 6. The small firms in the treatment group seem to have had a small decrease in both the least and the highest educated workers, but an increase in the two middle educational levels.

To test the effect of the reform on the educational level of workers I will use the same specification as before (equation 1), using model (3) with all covariates but changing the outcome variable to each one of the educational level ratios. The DiD estimates are shown in Table 7. The only statistically significant coefficients are found for the ratio of workers with at least 3 years of post-high school. The coefficient ranges from -0.003 to -0.005, indicating that the reform decreased the ratio of workers with the highest level of education. Given that the mean ratio of workers with at least 3 years of post-high school was 0.07 before the reform (see Table 6), the negative effect of the reform meant a decrease in the ratio of about 4–7 percent. Yearly effects, including all covariates, are shown in Figure 11 and indicates that the change in the ratio of workers with the highest educational level can be attributed to the reform. No effect is found for any of the other educational levels.

The decrease in the ratio of high educated workers could be an effect of less stringent screening of new hires. The lower share of highly educated workers is under most circumstances more likely to decrease productivity than the opposite. I therefore conclude that the estimated increase in labor productivity does not seem to be a result of a change in the educational composition of workers.

5.3.3 Various specifications and robustness checks

The first evidence of the parallel trend assumption was presented in Figure 3 and 4. Another way to investigate parallel trends is to estimate placebo periods for which there were no reform. The timing of the reform is then moved backward and forward in time to check if any of the other years will produce a significant effect on productivity. This is done in Table 8, where I estimate the effect of placebo reforms in the beginning of 1999, 2000, 2002 and 2003, using data on one year before and one year after each placebo reform respectively. The results reveal no statistical significance for any of the estimated placebo-DiD coefficients. To investigate weather the definition of the treatment group and the cut-off point is spurious, I let the sample start at firms with size 11 and create a pseudo cut-off at size 13, 15, 20, and 25, respectively. None of the estimated pseudo DiD coefficients are statistically significant (see lower part of Table 8).

As discussed above, the choice of control group is due to the fact that DiD is more plausible when treatment and control are very similar. A larger size difference between firms increases the likelihood of omitted variables. As a robustness test I estimate the effect when letting the control group expand to 11–20, 11–50, and 11–100 employees, respectively. The three first columns in Table 9 reveals that expanding the control group does not change the results considerably.

Although the number of employees in a firm is a fairly crude variable, it could be associated with measurement errors. The exemption of the LIFO-rules applies to firms with less than 11 employees. However, when determining firm size the law disregards members of the employer's family, workers in managing positions, persons hired to work in the employer's household, and workers participating in employment subsidy programs. This makes the threshold between firms with 10 and 11 employees somewhat uncertain. The register data used in this study does not identify kinship or worker positions. To investigate how sensitive the results are to these type of employment measurement errors, I exclude firms with 10–11, 9–12, and 8–13 employees, respectively. The results are presented in the last three columns of Table 9, and are in line with previous results. As anticipated from the structure of the 2001 reform, whose effect decreases with size, the DiD coefficient estimates are somewhat higher. The share of smaller firms within the treatment group increases when the threshold expands, and as indicated by Figure 8, these firms are decisive for the effect.

Because labor productivity is estimated by value added per employee one could suspect that it will increase in the very short run when a firm chooses to lay off personnel. To get an idea of the extent of this potential bias I include the natural logarithm of value added as dependent variable. The first column in Table A4 in Appendix shows that the effect is still present. Hence the effect on labor productivity does not seem to be a result of the dependent variable being affected in the short run by changes in size. In the right most column I shift the data before log-transformation to account for values that are close to zero, and the results are again similar to before.

Different patterns of entry and exists before and after the reform could affect the results on labor productivity. In Figure A1 I plot the entry and exit rates for the treatment and control group respectively. There is an increase for the entry rates in year 2000 but they appear to be cyclical rather than changing with the reform. A further inquiry of entry and exit rates can be found in von Below and Skogman Toursie (2010), whose results indicate that neither entry or exit probabilities seems to be affected by the reform.

6 Conclusions

In this paper I found that increased labor market flexibility led to a non-negligible increase in labor productivity. Thus far, there has only been 2 studies which use within country variation to try to establish a causal effect of employment protection on labor productivity (Autor et al. 2007; Boeri and Garibaldi 2007). In comparison to these studies, I have access to full population data and I make use of a discrete change in employment protection for a well defined group of firms.

A further elaboration revealed that the older firms are driving the results. A rationale for this could be that older firms are more stable in terms of size, i.e., less likely to grow out of the treatment group, and therefore also more affected by the loosened restrictions on firing. Another explanation is that it takes time for managers to get to know the idiosyncratic productivity of the workers. Previous literature has payed little attention to how responses to employment protection changes with a firm's age. It would be an interesting task for future work to elaborate on this relationship.

I discuss three mechanisms that could serve as to explain the increase in labor productivity. First, the reform seemed to have increased turnover rates for the smallest firms. Second, the reform did make it easier for the smaller firms to retain valuable workers and to lay off less valuable ones. Third, a higher probability of dismissal could have caused a behavioral change in workers, mitigating problems of moral hazard. The standard labor market models have overlooked the effect that employment protection has on the work effort of employees. Further studies are needed to address the relationship between employment protection and work effort.

The increase in labor productivity does not seem to be a consequence of increased capital intensity or an increase in human capital. The results indicate that the increase in labor productivity is due to an increase in TFP rather than capital intensity. This reinforces the conclusion that the effect on labor productivity is because of increased efficiency. The results on the educational level of workers indicate that the ratio of workers with the highest educational level, corresponding to at least 3 years of post high school education, have decreased. The reform could have made the screening of new hires less stringent. This should, if anything, have a negative effect on productivity.

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A Tables

Table 1: Mean values before and after the 2001 reform, 1998–2003

	Treatment group		Control group		DiD
	Pre-reform	Post-reform	Pre-reform	Post-reform	
Log of labor productivity	5.723	5.773	5.798	5.826	0.022
	(0.618)	(0.633)	(0.597)	(0.591)	
Labor productivity	379.3	398.3	394.5	401.4	12.1
	(939.2)	(709.1)	(437.6)	(427.7)	
Value added	1965.3	2073.7	5023.4	5111.8	20.0
	(3842.2)	(3574.8)	(5763.2)	(5567.7)	
Firm size	5.197	5.240	12.71	12.71	0.043
	(2.111)	(2.125)	(1.399)	(1.399)	
Age	7.926	9.142	8.955	10.64	-0.469
	(4.706)	(5.758)	(4.491)	(5.476)	
Enterprise group	0.198	0.208	0.370	0.379	0.379
	(0.399)	(0.406)	(0.483)	(0.485)	
Obs.	161,609	163,110	24,182	25,451	

Standard deviation in parenthesis. Labor productivity is defined as value added per employee. Value added is measured in thousands of krona (SEK). DiD (difference in differences) is the change in the treatment group minus the change in the control group. Obs. stands for observations.

Table 2: Estimated effect of the 2001 reform on labor productivity, 1998–2003

Treatment effect		Model (1)	(2)	(3)
ATT	D_{it}	0.0228***	0.0175***	0.0231***
		(0.00588)	(0.00543)	(0.00523)
	Obs.	374,352	374,352	374,352
ITT	Z_{it}	0.0133**	0.0132**	0.0120**
		(0.00593)	(0.00553)	(0.00550)
	Obs.	244,076	244,076	244,076
LATE	$\hat{D_{it}}$		0.0199**	0.0181**
			(0.00834)	(0.00828)
	Obs.		244,076	244,076
		Year FE,	Year FE, Industry FE	Year FE, Industry FE, Ent. group FE, Age FE

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ATT (average treatment effect), ITT (intention-to-treat), LATE (local average treatment effect), are separate estimations. D_{it}, Z_{it}, and $\hat{D_{it}}$, are the corresponding difference-in-differences dummy variables from equations (1), (3), and (4). Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table 3: DiD estimations for different samples based on age categories

Cut-off	Young (firm age $< c$)			Old (firm age $\geq c$)		
age (c)	ATT	ITT	LATE	ATT	ITT	LATE
$c = 5$	0.0155	-0.0117	-0.0184	0.0271***	0.00657	0.00984
	(0.0159)	(0.0200)	(0.0315)	(0.00502)	(0.00537)	(0.00805)
Obs.	106,699	53,342	53,342	267,653	190,734	190,734
$c = 6$	0.0241*	-0.00864	-0.0138	0.0252***	0.00668	0.00997
	(0.0138)	(0.0167)	(0.0267)	(0.00514)	(0.00547)	(0.00815)
Obs.	125,594	66,525	66,525	248,758	177,551	177,551
$c = 7$	0.0214*	-0.00861	-0.0135	0.0263***	0.00854	0.0127
	(0.0123)	(0.0143)	(0.0225)	(0.00528)	(0.00557)	(0.00830)
Obs.	143,464	78,920	78,920	230,888	165,156	165,156
$c = 8$	0.0216*	-0.00538	-0.00843	0.0267***	0.0105*	0.0157*
	(0.0111)	(0.0126)	(0.0198)	(0.00543)	(0.00571)	(0.00850)
Obs.	160,107	90,122	90,122	214,245	153,954	153,954
$c = 9$	0.0237**	-0.000878	-0.00136	0.0256***	0.00841	0.0126
	(0.0102)	(0.0116)	(0.0179)	(0.00559)	(0.00584)	(0.00872)
Obs.	176,901	100,835	100,835	197,451	143,241	143,241
$c = 10$	0.0259***	-0.00553	-0.00852	0.0240***	0.0119**	0.0177**
	(0.00947)	(0.0107)	(0.0165)	(0.00574)	(0.00596)	(0.00888)
Obs.	193,086	113,349	113,349	181,266	130,727	130,727
$c = 11$	0.0279***	-0.00754	-0.0116	0.0227***	0.0138**	0.0206**
	(0.00889)	(0.00994)	(0.0153)	(0.00596)	(0.00613)	(0.00912)
Obs.	208,303	124,873	124,873	166,049	119,203	119,203
$c = 12$	0.0250***	-0.00610	-0.00932	0.0259***	0.0148**	0.0221**
	(0.00845)	(0.00939)	(0.0143)	(0.00613)	(0.00632)	(0.00942)
Obs.	222,398	134,998	134,998	151,954	109,078	109,078
$c = 13$	0.0334***	-0.00507	-0.00774	0.0276***	0.0162**	0.0241**
	(0.00751)	(0.00899)	(0.0137)	(0.00673)	(0.00644)	(0.00959)
Obs.	256,668	143,671	143,671	117,684	100,405	100,405

Robust standard errors, clustered on firms, in parentheses. The sample is split into two parts consisting of young firms (left columns) and old firms (right columns). c corresponds to the different cut-off ages for defining a firm as young or old. Each treatment effect ATT (average treatment effect), ITT (intention-to-treat), LATE (local average treatment effect) and cut-off entry (rows) represents a separate estimation. The coefficients correspond to the full model with all covariates. Obs. stands for observations.
*** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$

Table 4: Mean values before and after the 2001 reform, restricted to firms with TFP measure

	Treatment group		Control group		DiD
	Pre-reform	Post-reform	Pre-reform	Post-reform	
Log of labor productivity	5.755	5.818	5.838	5.874	0.027
	(0.681)	(0.714)	(0.632)	(0.602)	
Log of capital labor ratio	5.188	5.307	5.122	5.169	0.072
	(1.244)	(1.309)	(1.158)	(1.170)	
Total factor productivity	-0.0627	-0.0498	0.00915	0.00674	0.015
	(0.552)	(0.580)	(0.498)	(0.474)	
Firm size	5.421	5.483	12.73	12.73	0.062
	(2.170)	(2.179)	(1.402)	(1.398)	
Age	8.393	9.980	9.837	11.81	-0.386
	(4.835)	(5.871)	(4.300)	(5.190)	
Enterprise group	0.188	0.190	0.339	0.335	0.006
	(0.390)	(0.392)	(0.473)	(0.472)	
Obs.	45,728	42,789	8,683	8,467	

Standard deviation in parenthesis. Labor productivity is defined as value added per employee. Value added and capital is measured in thousands of krona (SEK). DiD (difference in differences) is the change in the treatment group minus the change in the control group. Obs. stands for observations.

Table 5: Estimated effect of the 2001 reform on capital intensity and TFP, 1998–2003

Treatment effect		Log of labor productivity	Log of capital-labor ratio	Total factor productivity
ATT	D_{it}	0.0227**	0.0295*	0.0230***
		(0.00923)	(0.0171)	(0.00792)
	Obs.	105,667	105,667	105,667
ITT	Z_{it}	0.0348***	0.00656	0.0369***
		(0.00906)	(0.0185)	(0.00843)
	Obs.	72,704	72,704	72,704
LATE	$\hat{D}_{it}$	0.0526***	0.00993	0.0558***
		(0.0137)	(0.0280)	(0.0127)
	Obs.	72,704	72,704	72,704
		Year FE Industry FE, Ent. group, Age FE	Year FE, Industry FE, Ent. group, Age FE	Year FE, Industry FE, Ent. group, Age FE

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ATT (average treatment effect), ITT (intention-to-treat), LATE (local average treatment effect), are separate estimations. D_{it}, Z_{it}, and $\hat{D}_{it}$, are the corresponding difference-in-differences dummy variables from equations (1), (3), and (4). Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table 6: Mean values of educational level before and after the 2001 reform, 1998–2003

	Treatment group		Control group		DiD
	Pre-reform	Post-reform	Pre-reform	Post-reform	
Pre-high school ratio	0.259	0.226	0.249	0.218	-0.002
	(0.250)	(0.236)	(0.184)	(0.172)	
High school ratio	0.562	0.578	0.576	0.591	0.001
	(0.269)	(0.272)	(0.197)	(0.203)	
Post-high school ratio	0.171	0.190	0.168	0.185	0.002
	(0.252)	(0.266)	(0.218)	(0.230)	
3 years post-high school ratio	0.0679	0.0855	0.0617	0.0818	-0.003
	(0.164)	(0.188)	(0.133)	(0.160)	
Obs.	161,609	163,110	24,182	25,451	

Standard deviation in parenthesis. The rows correspond to the ratio of employees within a firm with different educational levels. DiD (difference in differences) is the change in the treatment group minus the change in the control group. Obs. stands for observations.

Table 7: Estimated effect of the 2001 reform on the educational level of workers, 1998–2003

	Treatment effect		
	ATT	ITT	LATE
Pre-high school ratio	0.000380	-0.000441	-0.000666
	(0.00157)	(0.00183)	(0.00277)
High-school ratio	0.000723	0.00215	0.00325
	(0.00188)	(0.00218)	(0.00328)
Post-high school ratio	-0.000911	-0.00174	-0.00262
	(0.00165)	(0.00178)	(0.00269)
3 years post-high school ratio	-0.00420***	-0.00336**	-0.00507**
	(0.00121)	(0.00132)	(0.00199)
Obs.	374,352	244,076	244,076
	Year FE Industry FE, Ent. group, Age FE	Year FE, Industry FE, Ent. group, Age FE	Year FE, Industry FE, Ent. group, Age FE

Robust standard errors, clustered on firms, in parentheses. Coefficients for each treatment effect, ATT (average treatment effect), ITT (intention-to-treat), LATE (local average treatment effect), in columns. Rows correspond to separate estimations for each educational level. Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table 8: Placebo estimations

	Year of placebo reform, r, (time span)			
	r= 1999 (1998–1999)	r= 2000 (1999–2000)	r= 2002 (2001–2002)	r= 2003 (2002–2003)
D_{ir}	0.000120	0.0142*	0.00968	0.00169
	(0.00785)	(0.00788)	(0.00773)	(0.00754)
Obs.	122,873	124,809	125,738	125,602
	Placebo size cut-off, c, (bandwidth)			
	c= 13 (11–16)	c= 15, (11–20)	c= 20, (11–30)	c= 25, (11–40)
D_{it}	-0.00248	-0.0106	-0.000784	-0.00794
	(0.00913)	(0.00822)	(0.00775)	(0.00808)
Obs.	56,229	75,993	102,282	114,942

Robust standard errors, clustered on firms, in parentheses. Coefficients correspond to the ATT (average treatment effect) and the difference-in-differences dummy variable D_{it} from equation (1). The full model with all covariates is used for all estimations. Bandwidth and size cut-off refer to number of employees in a firm. Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table 9: Estimated effect of the 2001 reform using different bandwidths

	Bandwidth					
	3–20	3–50	3–100	3–15		
				excluding firms of size		
				10–11	9–12	8–13
D_{it}	0.0192***	0.0194***	0.0201***	0.0218***	0.0307***	0.0248***
	(0.00438)	(0.00368)	(0.00354)	(0.00597)	(0.00713)	(0.00870)
Obs.	400,712	447,170	461,230	345,553	315,634	283,151

Robust standard errors, clustered on firms, in parentheses. Coefficients correspond to the ATT (average treatment effect) and the difference-in-differences dummy variable D_{it} from equation (1). The full model with all covariates is used for all estimations. Bandwidth and size refer to number of employees in a firm. Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

B Figures

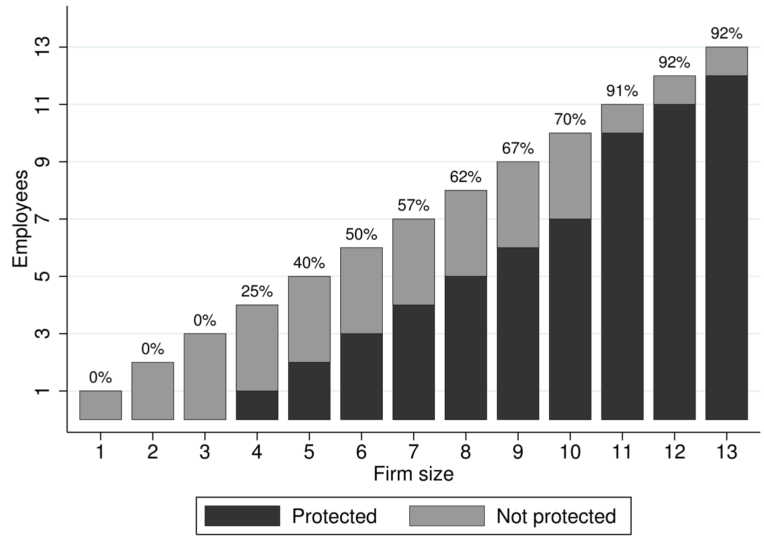


Figure 1: Protected workers after the 2001 reform
Note: The bars show the absolute number of protected and unprotected workers. The label over each bar refers to the percent of protected workers.

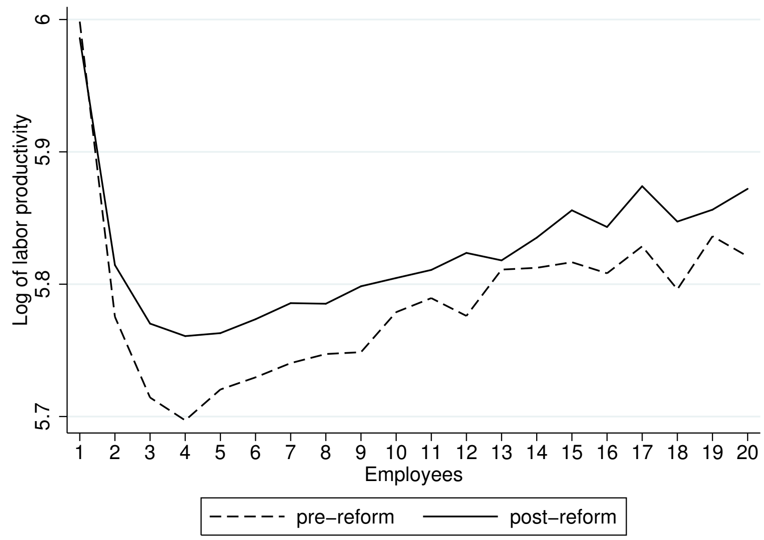


Figure 2: Firm productivity and number of employees

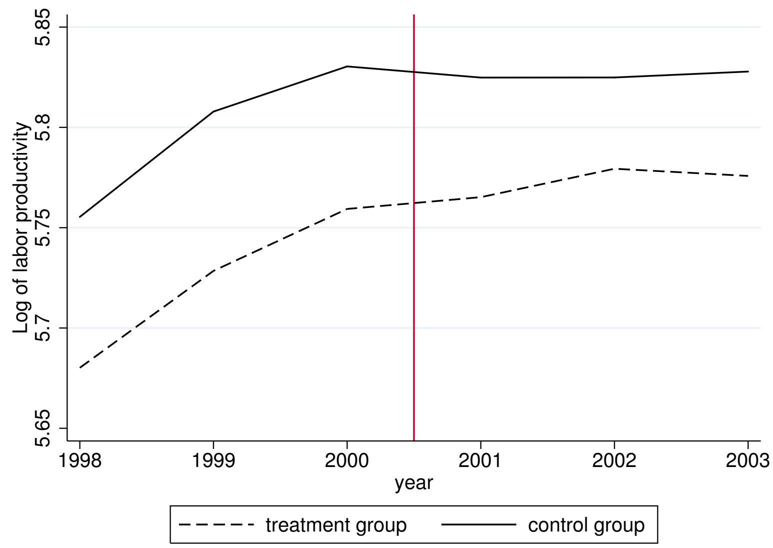


Figure 3: Labor productivity in treatment and control group, yearly averages

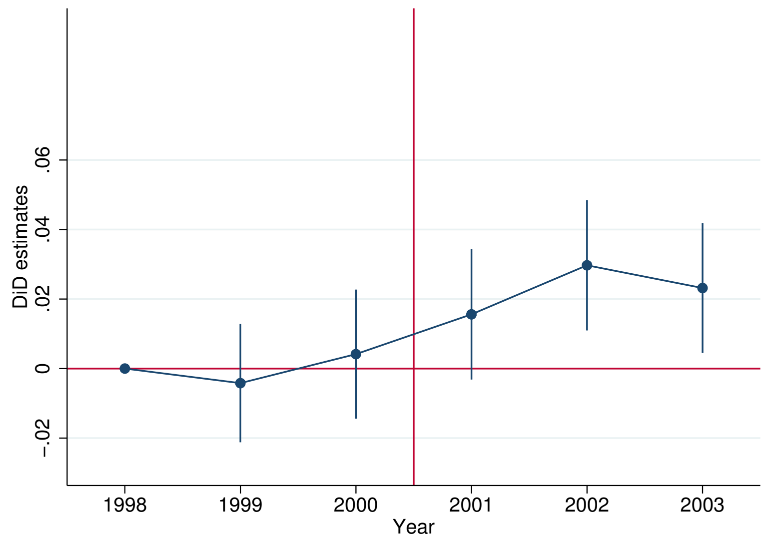


Figure 4: Year specific DiD estimates of the 2001 reform

Note: The DiD estimates on the y-axis are the estimated coefficients β_t from equation (2). The year 1998 is used as baseline. The vertical lines refer to a 95% confidence interval.

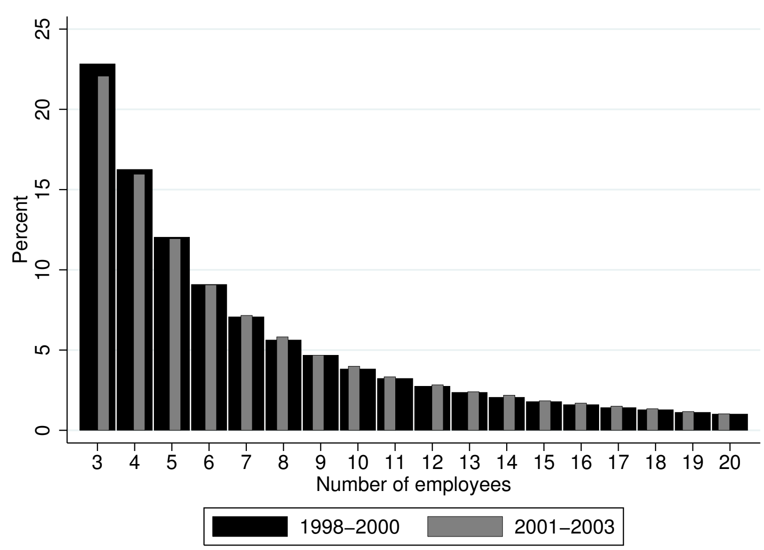


Figure 5: Histogram of number of employees 1998–2000 and 2001–2003

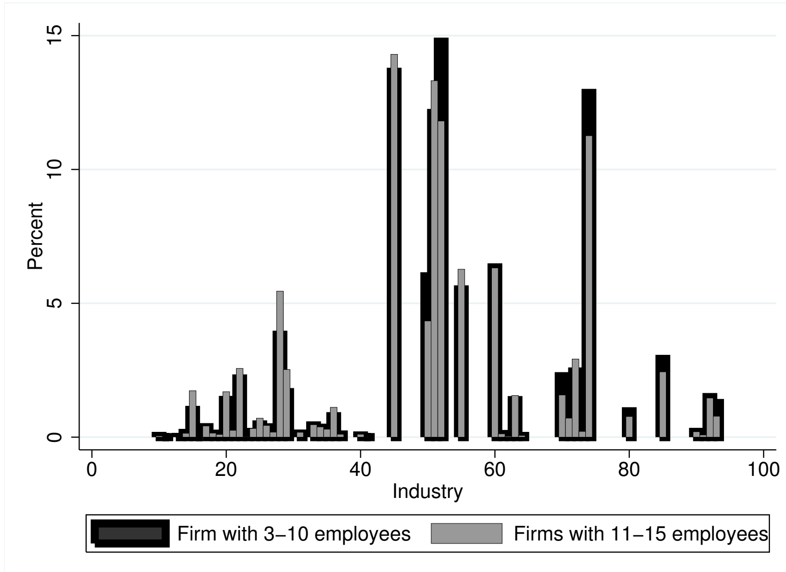


Figure 6: Distribution of industries for firms in the treatment and control group, 1998–2003

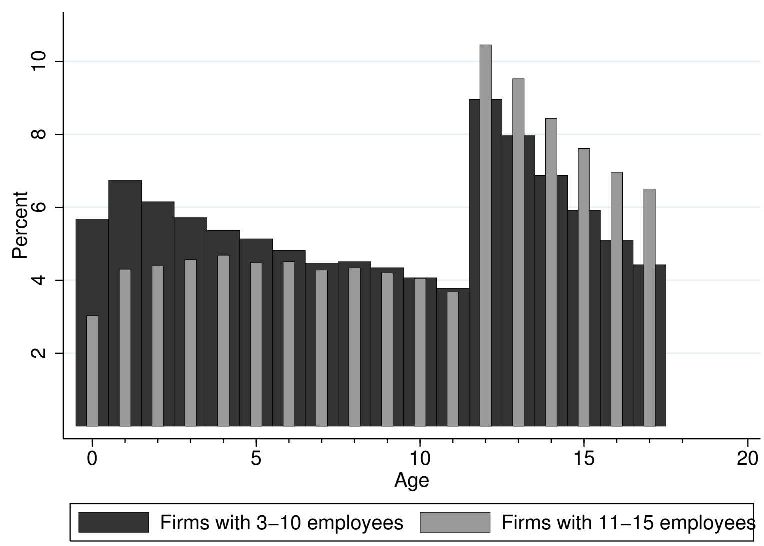
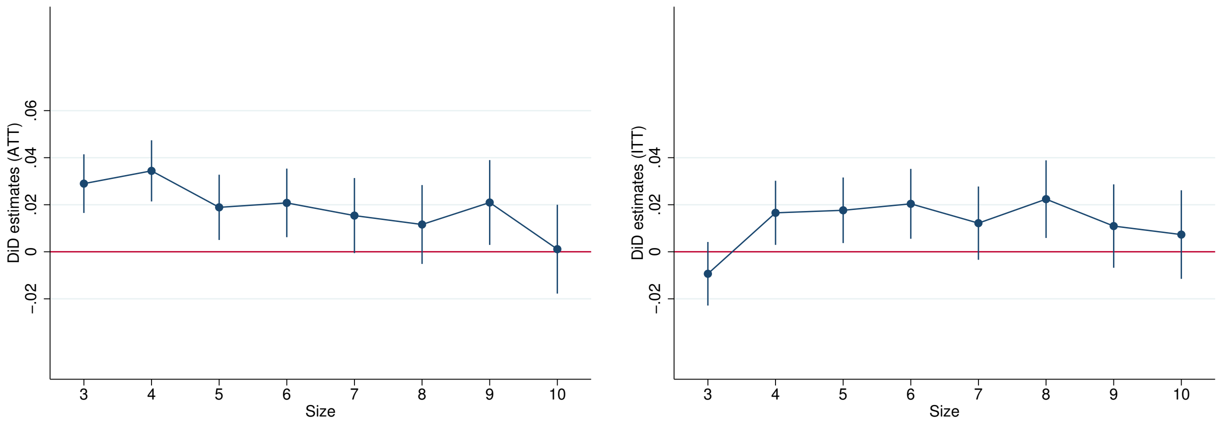


Figure 7: Distribution of age for firms in the treatment and control group, 1998–2003.
Note: The data is truncated so that all firms born before 1986 get 1986 as birth date. The maximum age is therefore 12 years in 1998 and 17 years in 2003, hence the skewed distribution.

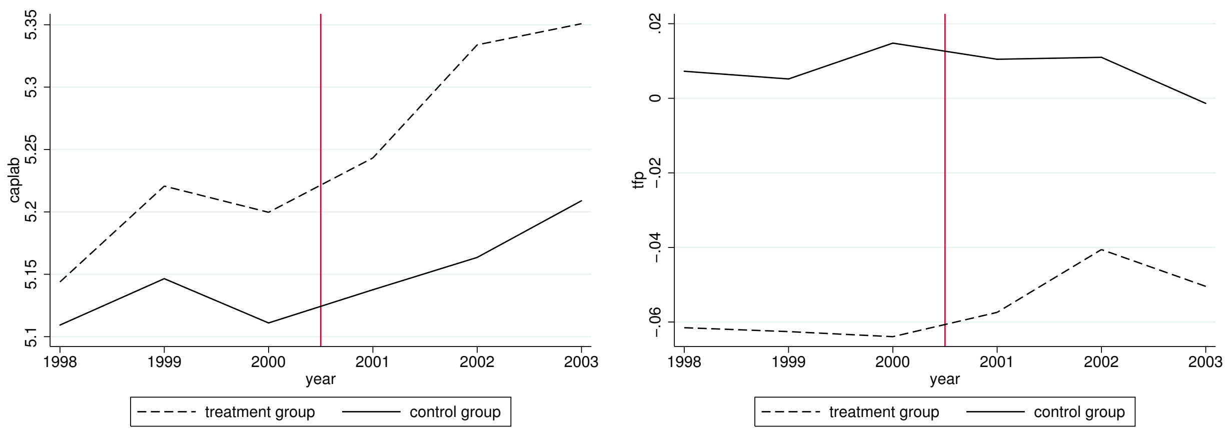


(a) Average treatment on the treated (ATT)

(b) Intention-to-treat (ITT)

Figure 8: Size specific DiD estimates of the 2001 reform

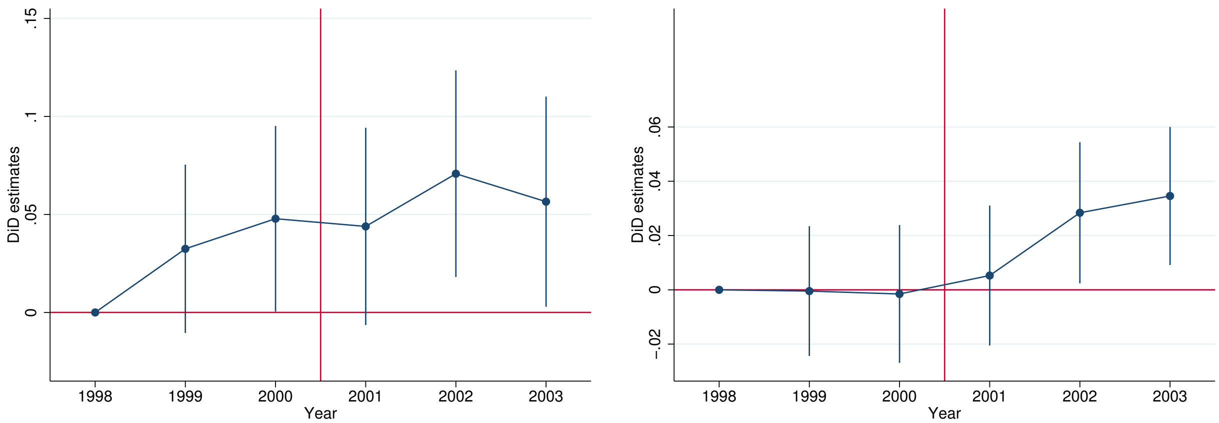
Note: The control group of firms with 11–15 employees is used as baseline. The vertical lines refer to a 95% confidence interval. The DiD estimates on the y-axis are the estimated coefficients β_s from equation (6). In (b), the size dummies from equation (6) are defined in the year 1999 to capture the ITT.



(a) Capital labor ratio

(b) Total factor productivity

Figure 9: Yearly averages of capital-labor ratio and TFP in treatment and control group



(a) Capital labor ratio

(b) Total factor productivity

Figure 10: Year specific DiD estimates of the 2001 reform on capital-labor ratio and TFP
Note: The DiD estimates on the y-axis are equivalent to the estimated coefficients β_t, when adding the full set of covariates from Table 5 to equation (2). The year 1998 is used as baseline. The vertical lines refer to a 95% confidence interval.

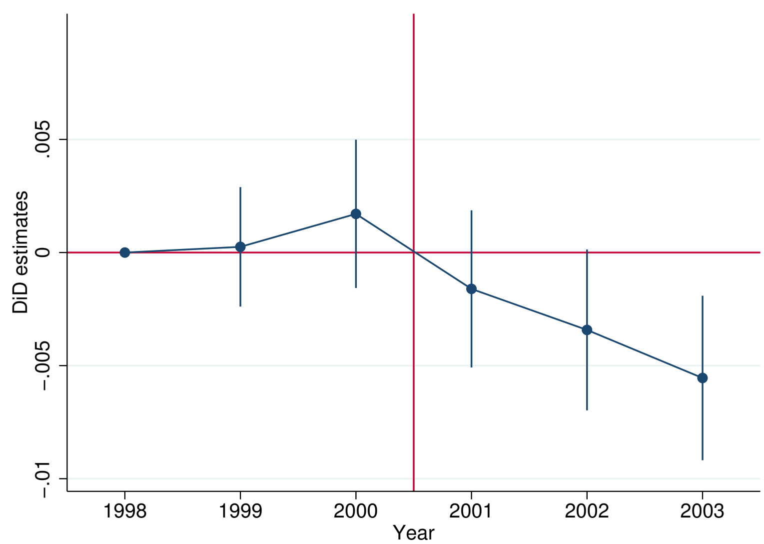


Figure 11: Year specific DiD estimates on the ratio of workers with at least 3 years of post-high school education
Note: The DiD estimates on the y-axis are equivalent to the estimated coefficients β_t, when adding the full set of covariates from Table 5 to equation (2). The year 1998 is used as baseline. The vertical lines refer to a 95% confidence interval.

Appendix

Table A1: Firms with negative value added, 1998–2003

	Treatment group		Control group	
Year	Obs.	Ratio	Obs.	Ratio
1998	599	0.0110	88	0.0109
1999	699	0.0126	126	0.0152
2000	946	0.0167	159	0.0185
2001	1026	0.0181	167	0.0191
2002	1008	0.0179	141	0.0161
2003	888	0.0154	101	0.0115

Note: Obs. stands for observations.

Table A2: ICT industries dropped from main estimations

Code	Industries	Observations
24650	Manufacture of prepared unrecorded media	28
24660	Manufacture of other chemical products n.e.c.	92
25240	Manufacture of other plastic products	766
30010	Manufacture of office machinery	48
30020	Manufacture of computers and other information processing equipment	398
31100	Manufacture of electric motors, generators and transformers	461
31200	Manufacture of electricity distribution and control apparatus	460
31300	Manufacture of insulated wire and cable	96
31620	Manufacture of other electrical equipment n.e.c.	483
32100	Manufacture of electronic valves and tubes and other electronic components	384
32200	Manufacture of television and radio transmitters and apparatus for line telephony and line telegraphy	147
32300	Manufacture of television and radio receivers, sound or video recording	103
33200	Manufacture of instruments and appliances for measuring, checking, testing, navigating and other purposes	506
36500	Manufacture of games and toys	107
52740	Repair n.e.c.	766
64201	Network operation	227
64202	Radio and television broadcast operation	5
64203	Cable television operation	22
72100	Hardware consultancy	426
72300	Data processing	400
72400	Data base activities	124
72500	Maintenance and repair of office, accounting and computing machinery	258
72600	Other computer related activities	215
74879	Various other business activities	185
	Total	6,707

ICT for manufacturing and service sector as defined by Statistics Sweden.

Table A3: The effect of the 2001 reform on labor productivity, including ICT industries, 1998–2003

Treatment effect		Model (1)	(2)	(3)
ATT	D_{it}	0.0219***	0.0167***	0.0222***
		(0.00585)	(0.00542)	(0.00523)
	Obs.	381059	381059	381059
ITT	Z_{it}	0.0146**	0.0147***	0.0133**
		(0.00586)	(0.00546)	(0.00543)
	Obs.	248649	248649	248649
LATE	$\hat{D_{it}}$		0.0222***	0.0202**
			(0.00825)	(0.00820)
	Obs.		248649	248649
		Year FE	Year FE, Industry FE	Year FE, Industry FE, Ent. group, Age FE

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ATT (average treatment effect), ITT (intention-to-treat), LATE (local average treatment effect), are separate estimations. D_{it}, Z_{it}, and $\hat{D_{it}}$, are the corresponding difference-in-differences dummy variables from equations (1), (3), and (4). Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table A4: Estimated effect of the 2001 reform on alternate outcome variables.

Treatment effect		ln(Value added)	ln(Y+1)
ATT	D_{it}	0.0345***	0.0220***
		(0.00556)	(0.00573)
	Obs.	374,352	374,929
ITT	Z_{it}	0.136***	0.0129**
		(0.00628)	(0.00616)
	Obs.	244,076	244,357
LATE	$\hat{D}_{it}$	0.205***	0.0195**
		(0.0102)	(0.00929)
	Obs.	244,076	244,357
		Year FE, Industry FE, Ent. group, Age FE	Year FE, Industry FE, Ent. group, Age FE

Robust standard errors, clustered on firms, in parentheses. ln(Y+1) stands for the logarithm of labor productivity plus 1. Each treatment effect, ATT (average treatment effect), ITT (intention-to-treat), LATE (local average treatment effect), are separate estimations. D_{it}, Z_{it}, and $\hat{D}_{it}$, are the corresponding difference-in-differences dummy variables from equations (1), (3), and (4). Obs. stands for observations.
*** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$

Table A5: Different years used to instrument the reform

	Year of instrument			
	1998		2000	
	ITT	LATE	ITT	LATE
	0.0142**	0.0234**	0.0110**	0.0158**
	(0.00570)	(0.00939)	(0.00520)	(0.00748)
Obs.	222,880	222,880	256,301	256,301

Robust standard errors, clustered on firms, in parentheses. Coefficients for each treatment effect, ITT (intention-to-treat), LATE (local average treatment effect), in columns. The full model with all covariates is used for all estimations. Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table A6: First stage equations on the DiD estimator D_{it}

	Model	
Instrument	(2)	(3)
Z_{it}	0.6626***	0.6627***
	(0.0059)	(0.0059)
F-statistics	12,731.4	12,741.7
Adj. R^2	0.8581	0.8582
Partial R^2	0.2627	0.2630
Shea's Adj. Partial R^2	0.2621	0.2623
Year FE	yes	yes
Industry FE	yes	yes
Ent. group		yes
Age FE		yes
Observations	244,076	244,076
Firms	62,349	62,349

Robust standard errors, clustered on firms, in parentheses. The estimations correspond to the first stage equation (5).
*** p< 0.01, ** p< 0.05, * p< 0.1

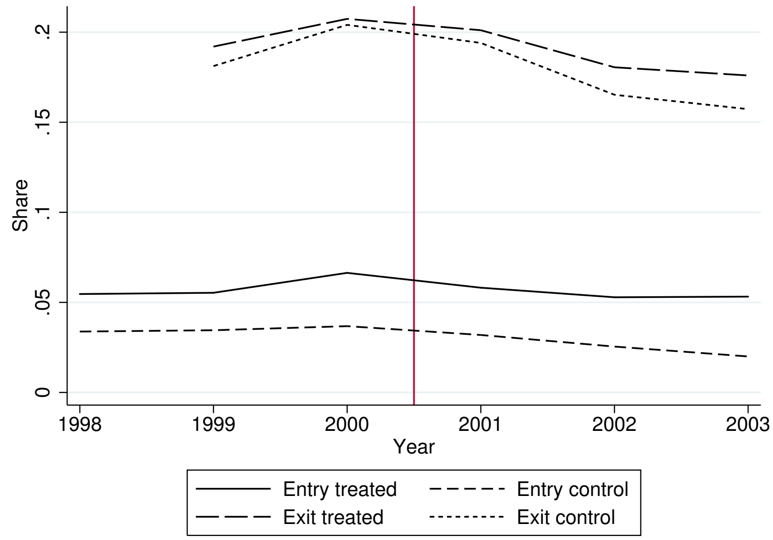


Figure A1: Entry and exit in treatment and control group, yearly averages