Employment Protection and Labor Productivity*

Carl Magnus Bjuggren†

December 21, 2015

Abstract

Current 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 Swedish register data. A reform of employment protection rules in 2001 enabled small firms with fewer than eleven employees to exempt two workers from the seniority rules. I treat this reform as a natural experiment. My results indicate that increased labor market flexibility increases labor productivity. This increase is not explained by capital intensity or by 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

*I am grateful to Sahaja Acharya, Lee Benham, Fredrik Heyman, Hans Hvide, Magnus Henrekson, Dan Johansson, Martin Korpi, Paul Nystedt, Martin Olsson, Per-Olof Robling, Thomas Stratmann, Hans Sjögren, Per Skedinger, and the participants at EEA 2015 and EALE 2014 for their valuable comments and suggestions, and to Fredrik Andersson for his excellent research assistance. I gratefully acknowledge the financial support from the Swedish Research Council for Health, Working Life and Welfare (Forte) grant number 2014–2740, the Jan Wallander and Tom Hedelius Research Foundation, and Sparbankernas Forskningsstiftelse.

†Research Institute of Industrial Economics (IFN), Box 55665, SE-102 15 Stockholm, Sweden.
Email: carl.magnus.bjuggren@ifn.se

1 Introduction

Although there is a wealth of literature on employment protection and how it affects the labor market, predictions on how employment protection affects productivity are ambiguous. Theory generally agrees that employment protection increases firms' firing costs. Restraining efficient job separation may reduce efficient job creation and firms' ability 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 less hiring and firing, which could, in turn, result in slower adjustment to structural change. Although restricting firms' abilities to freely adjust their labor according to demand would have a negative impact on productivity, 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). Due to decreased risk of discharge and longer employment spells, job security regulations may induce workers to acquire 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 unclear.

In this paper, I empirically show that increased labor market flexibility increases labor productivity. I analyze how job security regulations affect labor productivity, focusing on Sweden and its particular rules of seniority. I use a reform in the Swedish last-in-first-out (LIFO) rules as a natural experiment to estimate the effect of less-stringent employment protection on labor productivity. All firms in Sweden have to abide by the LIFO rules, which involve a list of priorities and stipulate that the last person hired is the first to be fired in the case of redundancy. Although a 2001 reform loosened the LIFO rules, it did so only for small firms with fewer than 11 employees. I analyze this reform using a difference in differences (DiD) framework, finding that this reform increased labor productivity by approximately 2 to 5 percent in the treatment group of small firms compared to a control group of larger firms.

Using register data from Sweden, I thoroughly assess the effect of employment protection on labor productivity. The register data allow me to analyze the effect of employment protection on capital deepening and total factor productivity, and I am able to relate these findings to effects on human capital and to decompose the effects on firm age, firm size, and industry affiliation. The Swedish context provides a natural experiment that allows me to analyze a causal effect of reduced employment protection on productivity by using an unexpected political reform. I address potential threats to identification by creating an instrument based on firm size prior to the reform. Because of the unexpected political collaboration that led to the reform and its rapid implementation, firms and individuals could not have anticipated the change in employment protection.

With this study, I contribute to a large body of literature on the various effects of employment protection on workers and firms. Previous 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 have 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). Few previous studies use variation within a country to establish a causal effect of employment protection on labor productivity (Autor et al. 2007; Boeri and Garibaldi 2007; Okudaira et al. 2013). Although there are several previous studies on the effect of dismissal restrictions based on firm size thresholds (Bauer et al. 2007; Garibaldi et al. 2004; Kugler and Pica 2008; Martins 2009; Olsson 2009; von Below and Skogman Thoursie 2010), this study is, to my knowledge, the first to focus on productivity.

The results indicate that the increase in labor productivity is explained by total factor productivity (TFP) rather than capital intensity, suggesting that the increase in labor productivity is due to an increase in efficiency. The result that labor productivity increases with increasing labor market flexibility contrasts the previous study by Autor et al. (2007), who find that introducing restrictions on a firm's ability to fire workers leads to an increase in labor productivity through capital deepening. In contrast, by studying court decisions in Japan, Okudaira et al. (2013) find that increased firing costs decreases both labor productivity and TFP. Boeri and Garibaldi (2007) find that an increase in the use of fixed-term contracts decreases labor productivity.

I contribute to the literature by relating the effects on productivity to human capital through an investigation of the composition of workers' educational level. The increased labor market flexibility did not change the educational level of workers. In addition, by decomposing the effect on firm age, I show that the positive effect on labor productivity is present only for older firms. This finding could indicate the time it takes for managers to learn about their workers' productivity. The results are also more apparent in smaller firms as an effect of the specific outline of the reform. The reform made it possible for firms with fewer than 11 employees to exempt 2 workers from their priority lists. Instead of having to fire the worker with the least tenure, firms are free to choose among the 3 workers with the least tenure. Because the exemption is in absolute numbers, it is not proportional to size, and the effect is greater as the size of the firm decreases. Moreover, the estimated increase in labor productivity seems to be concentrated in the service sector.

The fact that the reform increased labor productivity by approximately 2 to 5 percent is non-negligible. According to official statistics, the annual percentage change in labor productivity in Sweden between 1998 and 2003 is estimated at 1.9 percent (Eurostat). The increased threat of being fired could have caused a behavioral change in workers, which could account for some of the effect on labor productivity. In addition, the reform made it easier for small firms to retain or lay off personnel based on workers' idiosyncratic 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 paper.

2 Institutional Setting

Since 1974, all Swedish firms have adhered to the Swedish Employment Protection Act (EPA) (Skedinger 2008), which imposes the LIFO regulation, meaning that the last person employed is the first to be fired in the case of work shortage (SFS 1982:80). The LIFO regulations stipulate that, in the case of redundancy, the employer must comply with the established priority lists, which 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 priority lists. If two workers have accumulated the same tenure within the firm, priority is given to the oldest worker (SFS 1982:80). The LIFO rules also stipulate that if a worker has been laid off due to redundancy, he or she has priority if the firm rehires. Should a firm not comply with the LIFO regulations, the firm is liable to pay damages. The dismissal, however, will not be invalidated. It should also be noted that the LIFO rules apply only 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]

The Swedish EPA has undergone several changes over time. In 1994, a temporary change was made to the LIFO regulations that allowed firms to exempt two workers from the priority lists. This exemption was revoked in 1995. In 1997, a change in the EPA made it easier for firms to employ workers on temporary contracts. Between 1997 and 2007, only one major change was made to the EPA, namely, the 2001 reform, which is the only regulation that discriminates employment protection over firm size (Skedinger 2008).

On January 1, 2001, an exemption from the LIFO rules was introduced for firms with 10 or fewer employees. These small firms are allowed to exempt two employees with the least accumulated tenure from the priority lists, meaning that firms are free to choose among the three employees with the least tenure in the case of dismissal. The 2001 reform thus

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

constitutes a discrete change in employment protection for a specific group of firms. In addition, two features make it particularly suitable as a natural experiment. The process from discussion to implementation was fast, and it was unlikely to have been anticipated. The reform was not discussed in public until the beginning of February 2000. It was approved in October 2000 and implemented on January 1, 2001. Furthermore, the reform was a result of an unusual cooperation between the green party and the center and right-wing opposition parties in parliament. It is reasonable to assume that it did not become clear until the middle of 2000 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. Therefore, firms larger than 10 employees are not able to take advantage of the reform, irrespective of the size 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, differentiate between types of contracts, meaning that workers on temporary and full-time contracts are equally weighted.

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, leaving 7 workers (70%) protected. In contrast, the reform leaves none of workers protected in a firm of 3 employees. The reform is thus designed to have a larger effect as the firm's size decreases (Figure 1).

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

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

on fixed-term contracts with temporary work agencies; in these cases, there is no employment contract between the individual worker and the firm.

3 Theoretical Considerations and Previous Studies

Empirical studies that use within-country variation to assess the effect of employment protection on productivity are few. Autor et al. (2007) use the adoption of wrongful discharge in US courts to study the effects of firing costs on productivity, finding that total factor productivity decreases, whereas labor productivity increases, with firing costs. This finding 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, finding that an increase in temporary workers lowers labor productivity. Okudaira et al. (2013) exploit variations in court decisions in Japan to study the effect of employment protection on productivity, finding that labor productivity decreases with increased firing costs; an increase of 10 percentage points in the worker victory ratio decreases labor productivity by 0.4 percent.

Several countries in Europe have similar size thresholds to Sweden and discriminate employment protection across firms. These thresholds and some of the reforms that introduced them have been used to study the effect of employment protection on outcomes such as labor turnover rates, wages, firm growth, and performance (Bauer et al. 2007; Garibaldi et al. 2004; Kugler and Pica 2008; Martins 2009). However, to my knowledge, this paper is the first to study the effect of size discriminatory rules on productivity. In Portugal, firms with fewer than 20 employees are exempt from some dismissal restrictions. Martins (2009) investigates the reform that introduced the size threshold in Portugal in 1989, finding that firm performance increases with the increased flexibility for small firms.[3] France, Germany, and Italy also have rules that exempt small firms from some dismissal restrictions. In France,

[3]Martins (2009) measures performance as either the logarithm of sales per employee or the difference between sales and the wage bill.

these rules apply for firms with fewer than 10 employees, and in Germany, the size threshold has been changed several times, from 5 to 10 employees and back (Bauer et al. 2007).[4] In Italy, workers in firms with fewer than 16 employees are less protected against unfair dismissal (Garibaldi et al. 2004; Kugler and Pica 2008).[5]

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 often used by macro economists (Mortensen and Pissarides 1994). Both models render ambiguous effects of employment protection on productivity and assume that productivity may be negatively affected if employment protection causes firms to retain less-productive workers. However, the screening of new hires may become more stringent (competitive model); alternatively, firms may increase the productivity threshold at which they are willing to hire (equilibrium unemployment model). Both models assume that firm productivity is affected only by a decrease in job flows.

Worker effort, though important, is disregarded by these standard models. Ichino and Riphahn (2005) develop a framework for understanding how employment protection affects workers' behavior. Employment protection limits the firm's willingness to monitor and fire workers who exhibit laziness or shirking. The results relate to Shapiro and Stiglitz's (1984) theory on wages and the threat of firing as a method of disciplining a worker. If employment protection affects the work effort of employees, it can have an effect on productivity, regardless of job flows.

Productivity may be positively affected by employment protection when the higher costs of firing create incentives for firms to increase their investments in R&D and human capital and for workers to acquire more firm-specific skills (Belot et al. 2007; Koeniger 2005; Nickell and Layard 1999). However, R&D investments made under a rigid labor market regime could be less productive in the long run because they are more likely to focus on improving

[4] Bauer et al. (2007) study the German reforms and find no effect on worker turnover rates.

[5] Garibaldi et al. (2004) show that the Italian threshold acts as a growth barrier. Kugler and Pica (2008) study a reform in Italy in 1990 that increased the dismissal costs for small firms relative to larger firms, finding that the reduced flexibility decreases both accessions and separations.

existing products instead of introducing new ones (Saint-Paul 2002).

Based on these theoretical observations, the Swedish reform may affect productivity in different ways. First, increased labor market flexibility may have caused an increase in employment turnover rates, possibly affecting productivity in accordance with the standard models. Second, the reform may have caused a behavioral change in workers regarding 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 may have changed the choice of capital intensity, which directly affects labor productivity. Fourth, lower adjustment costs may allow for a less-stringent screening of new hires. Changes in the composition of human capital within the workforce induced by less-stringent screening may have an affect on productivity. A fifth implication of the reform relates more specifically to the Swedish EPA. The LIFO rules before the reform implied that firms could not separate or keep workers based on their idiosyncratic productivity. Even if turnover rates, worker effort, and capital intensity did not change with the reform, an effect on productivity from the increased possibility of retaining more productive personnel may be expected. Thus, there are several channels through which the reform may affect productivity.

Two previous studies on the Swedish reform relate to this discussion.[6] Von Below and Skogman Thoursie (2010) investigate the effect of the 2001 reform on employment turnover and find that both hires and separations increased approximately 5 percent in the smallest firms, leaving net employment unaffected. The reform's effects are argued to be small; nevertheless, the reform may have affected labor productivity through increased job flows. Olsson (2009) finds that sickness absence is reduced for the group of small firms. However, the effect of a decrease in sickness absence on productivity is ambiguous. On one hand, if the reform triggered a decrease in moral hazard behavior, productivity would increase. On the other hand, if the reform caused workers to attend work sick, productivity would decrease.

[6] In addition, there are two working papers on the Swedish reform. Lindbeck et al. (2006) study the effect on sickness absence, and Bornhäll et al. (2014) study the effect on firm growth.

4 Data

The data used are firm and establishment data from Statistics Sweden (SCB) for all firms with at least one employee between 1998 and 2003.[7] 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 level, that is, including all the firm's 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.[8]

The 2001 reform took place amid an information technology boom and bust cycle. To further facilitate identification of the reform's effects, all firms within the ICT industries are dropped from the estimations (Table A1).[9] To facilitate the comparison of different productivity measures, the sample is further restricted to firms with non-missing values for capital.[10] The sample is restricted to corporations (limited companies), excluding firms within the agricultural sector and government-owned corporations.

The data do not allow the identification of kinship, workers' positions, or permanent or temporary contracts. However, the reform does not differentiate between permanent and temporary contracts when defining the size threshold, and the data therefore provide an accurate size cut-off in this regard. The reform excludes the following when determining the firm size threshold: 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; these exclusions may affect the accuracy of the size cut-off at 10 or 11 employees. In Table A3, I expand the gap between the two treatment groups, i.e., excluding firms around the threshold, and the results do not change.

To estimate labor productivity, I use the natural logarithm of value added per employee.[11]

[7]The data from SCB cover all firms in all industries, except for certain firms within the finance sector.

[8]See the Appendix for additional details on the data.

[9]The inclusion of these industries does not change the main results (Table A2).

[10]Including firms with missing values for capital does not change the main results (Table A2).

[11]In Table A4, the results hold when I shift the data before log-transformation and when using the natural

Figure 2 depicts labor productivity for firms with 1 to 20 employees. The values for firms with one and two employees are high; these firms are dropped from all estimations. Disregarding the smallest firms, the relationship appears to be linear.

5 Empirical Estimation

To estimate the effect of the reform, I use a DiD framework defining small firms with fewer than 11 employees as a treatment group, which I compare with a control group of larger firms that have 11 to 15 employees and that remain confined to the LIFO rules. I choose this control group because DiD is more plausible when the treatment and control groups are more similar.[12]

Several aspects of the reform make a DiD framework preferable. 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. In particular, young firms with large growth ambitions are likely to be born into the treatment group only to increase rapidly in size, which will act to prevent the formation of a kink in productivity. Additionally, given, a potential non-random selection of firms around the reform threshold, a DiD framework is preferable to, e.g., a regression discontinuity approach.

5.1 Instrument and treatment effects

Firm size is the underlying variable in this natural experiment, and firms are able to adjust their size, posing a potential selection problem. Figure 3 plots the distribution of firm size for 1998–2000 and 2001–2003. Although there is no visible discrepancy around the size cut-off, there could still be a potential selection problem. To mitigate this problem, I let

logarithm of value added, not scaled on employees.

[12]The results are not sensitive to expanding the size of the control group (Table A3). Furthermore, no effect is found when altering the size cut-off to create placebo treatment groups (Table A5).

treatment status be determined by firm size in 1999, two years before the reform took place and one year before the reform was discussed in public.[13] Therefore, I can estimate the intention-to-treat (ITT) and the local average treatment effect (LATE).

The treatment and control groups need to follow parallel trends before the reform in order for the DiD analysis to be valid. Figure 4 shows yearly average labor productivity for the treatment and control groups, respectively. 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.

Descriptive statistics for the two groups before and after the reform are shown in Table 1. Labor productivity increases with the reform for both the control and treatment groups. However, the average increase is larger for the group of small firms, 0.134, than for the larger firms, 0.087.[14] 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 amounts here to 0.047. This finding is the first indication of the reform's effect.

To estimate the ITT, I use firm size in 1999 as a treatment indicator and follow the firms over time, regardless of whether they adjust their size (and thereby falling in or out of the treatment group). The ITT is estimated using equation (1),

$$Y_{it} = \alpha + \lambda_t + \delta d_{i99} + \beta(Post_t \times d_{i99}) + X_{i99}\gamma + \upsilon_{it} \tag{1}$$

where Y_{it} is the natural logarithm of labor productivity in firm i at time t, and λ_t is a full set of year dummies controlling for symmetric time effects. d_{i99} is a treatment dummy variable taking the value of 1 if a firm had fewer than 11 employees in 1999. $Post_t$ is a reform dummy variable taking the value of 1 for the year 2001 or later. The coefficient β estimates

[13] A similar strategy to capture the different treatment effects of the reform is used by Olsson (2013) and Lindbeck et al. (2006). Table A6 shows that the results hold when letting treatment status be determined by firm size in the year 1998.

[14] The numbers refer to the logarithmic values of labor productivity.

the treatment effect of the 2001 reform. There may be a compositional bias according to which firms within the two groups have systematically different characteristics before and after the reform; therefore, the inclusion of additional covariates is justified. X_{i99} 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 of one if the firm belongs to an enterprise group. All covariates in X_{i99} are defined in the year 1999 in order to be exogenous. To simplify, I suppress the notation from here on so that $Z_{it} = Post_t \times d_{i99}$.

In the next step, I use Z_{it} as an instrument in a two-stage least-squares (2SLS) regression to estimate the following equation:

$$Y_{it} = \alpha + \lambda_t + \delta d_{i99} + \beta \hat{D}_{it} + X_{i99}\gamma + \upsilon_{it} \tag{2}$$

where $\hat{D}_{it}$ is the predicted value from the first-stage equation (3).

$$D_{it} = \omega_0 + \lambda_t + \omega_1 d_{i99} + \omega_2 Z_{it} + X_{i99}\omega_3 + \mu_{it} \tag{3}$$

where $D_{it} = Post_t \times d_{it}$, and d_{it} is a dummy variable taking the value of 1 if in the treatment group at time t.

The coefficient β estimates the LATE, given the four assumptions of independence, exclusion, the existence of a first stage, and monotonicity (Angrist and Pischke 2009). First, independence requires Z_{it} to be independent of potential treatment assignment and of potential outcome. 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 was anticipated. In addition, there was no previous employment protection legislation discriminating over firm size. Firm size in 1999 can therefore be assumed to be independent of potential treatment assignment. Although there is an absolute difference in labor productivity between the treatment and control groups, there appears to be no difference in productivity trend between the groups prior to the reform (Figure 4). Firm size in 1999 can therefore be assumed to be independent of potential outcome.

Second, the exclusion restriction requires that Z_{it} affect labor productivity only though the correlation with post-reform treatment status, i.e., firm size after 2001. From the parallel trends assumption, Z_{it} does not affect labor productivity in the absence of the reform. Third, the first-stage equations exist and are presented in Table A7 in the Appendix. The F-values of these estimations are high, which indicates that the instrument is strong. Fourth, monotonicity in this setting requires that having fewer than 11 employees in 1999 does not make treatment status after the reform (i.e., having fewer than 11 employees after 2001) less likely. If a selection problem is caused by firms and workers adjusting their size (and thereby falling in and out of the treatment group), the IV regression nevertheless provides consistent estimates. LATE captures the effect of the treatment on compliers, i.e., the effect on firms that remained in the treatment group compared to firms that remained in the control group and that did not adjust their size because of the reform.[15] ITT gives an estimate independent of the effect of potential cross-overs and estimates the effects of the policy announcement, which is interesting from a policy perspective. Attrition is a potential threat to identification of the treatment effects. Figure A1 plots the exit rates for the treatment and control groups. There appears to be no obvious change as a result of the reform. A further inquiry of exit rates can be found in von Below and Skogman Thoursie (2010), whose results indicate that neither entry nor exit probabilities are affected by the reform.

As a reference, I estimate the as-treated effect (AsT). This estimate is the observed DiD including the average treatment effect of the treated group plus potential selection bias. I estimate the AsT with the following equation:

$$Y_{it} = \alpha + \lambda_t + \delta d_{it} + \beta D_{it} + X_{it}\gamma + \upsilon_{it} \tag{4}$$

[15] Because of monotonicity, there are no defiers. LATE excludes the effect of firms that insist on being treated independent of their size in 1999 either by reducing their size or by refraining from growing (Always-Takers). Likewise, LATE excludes the effect of firms that insist on not being treated independent of their size in 1999 either by growing or by refraining from downsizing (Never-Takers).

where the variables are defined as above. This setting allows treatment status to vary across years.

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

$$Y_{it} = \alpha + \lambda_t + \delta d_{i99} + \sum_{t=1999}^{2003} \beta_t(\lambda_t \times d_{i99}) + \upsilon_{it} \tag{5}$$

where year dummies, λ_t, are interacted with the treatment indicator, d_{i99}, to generate a DiD estimate for each year, using the year 1998 as a benchmark.[16] The results are presented in Figure 5. No effects are found in the pre-reform years, strengthening the assumption of parallel trends. The post-reform yearly effects are at their highest in 2002 and decrease somewhat in 2003.

5.2 Results

Table 2 shows the three different estimated effects (AsT, ITT, and LATE) of the 2001 reform. Columns (1)–(3) add the controls stepwise. The DiD coefficient estimates are positive for all three effects. The size of the estimated coefficients ranges from 0.02 to 0.07, indicating that exemption from the LIFO rules increases labor productivity by approximately 2 to 7 percent.[17] Including all covariates will likely result in a more accurate estimation of the reform's effect. The estimated LATE is 0.05 for the most saturated model, column (3), indicating an increase in labor productivity of approximately 5 percent for firms that remained in the treatment group. The estimated ITT effect of the reform is slightly lower, at 3 percent.[18]

[16]The same equation is estimated for d_{it} in Figure 5 (b).

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

[18]LATE is, by definition, always weakly larger than ITT. LATE can be expressed as a Wald estimator by dividing ITT with the difference in compliance rates between the treatment and control groups (Angrist and Pischke 2009).

In this setting, I have a single reform and single threshold with which to define the treatment and control groups, which could create potential inference problems. Failure to account for group error structures could lead to underestimation of standard errors, as described by Moulton (1986). To address this concern, I first collapse the data to yearly means in labor productivity for both the treatment and control groups. I then estimate the DiD for the 12 remaining data points. The results are shown in Table A8 and are statistically significant. Second, I cluster the standard errors on the firm size level. The baseline setting, in which I use firms with 3 to 15 employees, produces only 13 size categories in total. Therefore, the control group needs to be expanded in order to increase the number of groups on which to cluster to avoid again underestimating the standard errors. This process, however, makes the control group a less compelling counterfactual to the small firms in the treatment group. In Table A9, I expand the control group to encompass firms employing up to 50 and 100 employees while clustering the standard errors on size. The estimated coefficients are statistically significant for all specifications.

5.2.1 Capital deepening and total factor productivity

To disentangle the different components accounting for the increase in labor productivity, I begin 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 then have that 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). To investigate whether capital intensity changed with the reform, I use the natural logarithm of capital divided by the number of employees as an outcome variable. Book values of machinery and structures are used to estimate capital. I also estimate the effect of the reform on TFP using the following production function for each 2-digit industry and year:

$$\log(Y_{it}) = \alpha + \psi_{jt}\log(L_{it}) + \gamma_{jt}^{m}\log(K_{it}^{m}) + \gamma_{jt}^{b}\log(K_{it}^{b}) + \xi_{it} \tag{6}$$

where Y_{it} is defined as value added of 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.[19]

To estimate the reform's effect, I use the same specification as before, focusing only on the most saturated model (3). The DiD coefficients for the different treatment effects are shown in Table 3. The effects on both TFP and labor productivity are positive and similar to each other in size for all three treatment effects. There is no significant effect on the capital labor ratio. Therefore, the increase in labor productivity is likely due to TFP rather than increased capital intensity.

5.2.2 Human capital

An increase in labor productivity may also be a result of a change in human capital. Higher education is believed to increase worker productivity (Becker 1975). The screening of new hires may be affected by the reform, as it became easier to hire and separate workers. To investigate whether the reform caused a change in the education level of workers, I use the same specification as before, where I change the outcome variable to the ratio of workers with i) pre-high school education, ii) high school education, iii) post-high school education, and iv) at least 3 years of post-high school education. No effect is found for any of the educational levels (Table 4). 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.

Workers' educational level does not necessarily capture human capital acquired on the job. Workers may possess human capital that is difficult to assess with these data. Despite the above results, the firms' ability to retain more valuable personnel may positively affect labor productivity. To obtain an idea of whether this is the case, the data are divided into two samples: firms that separated at least one worker at any year during the post-reform period and those that did not (Table 5). The separation of workers is potentially affected by

[19] This measure of TFP does not address problems such as input choices. The aim of this exercise is not to obtain an exact measure of TFP, but rather an estimate that is consistent over time.

the reform itself; therefore, the estimates should be interpreted with caution. The exercise intends primarily to discern whether the increase in labor productivity is driven by firms that separated workers. The estimates are statistically significant only for the sample of firms that separated workers, supporting the premise that the positive effect on labor productivity might be due to firms' increased flexibility to retain more valuable personnel. However, the data do not allow for a distinction between voluntary separations and dismissals. In addition, the majority of firms, over 70 percent, separated at least one worker in the three years following the reform.

5.2.3 Firm age decomposition

Previous literature finds that age plays a key role in firm behavior (Haltiwanger et al. 2013). Figure 6 plots the age distribution for the treatment and control groups. The control group has a larger share of older firms, as confirmed by Table 1. Older firms are more likely to have reached their permanent size, and therefore less likely to cross over between treatment and control groups. In Table 6, the sample is 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. For the different sub-samples of firms younger than 12 years of age, there are no significant coefficients, regardless of the treatment effect. The coefficients in Table 6 are significant only for the sub-samples that include older firms, which indicates that these firms drive the results.

Recall that age is defined as firm age in 1999 for both ITT and LATE, meaning that there is no rejuvenation in these estimations, whereas all variables in the AsT estimations are allowed to vary over time. This may also explain why the AsT effect is lower than the LATE and ITT. There may be a potential selection bias in the AsT estimations driving successful and ambitious small and young firms to grow out of the treatment group, independent of the reform. Hence, this potential selection bias would lower the effect of the reform.

5.2.4 Firm size decomposition

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

$$Y_{it} = \alpha + \lambda_t + \sum_{s=3}^{10} \chi_s Size_{is99} + \sum_{s=3}^{10} \beta_s (Size_{is99} \times Post_t) + X_{i99}\gamma + \upsilon_{it} \tag{7}$$

where $Size_{is99}$ is a dummy variable taking the value of 1 if firm i is of size s in 1999. The β_s is a coefficient of the DiD estimate for each of the 8 size categories s. The firms in the control group, which have 11 to 15 employees, are used as a benchmark. Figure 7 shows the estimated β_s for the different size categories. The figure reveals that the effect of the reform is larger for smaller firms, and this relationship is most well visualized for firms of size 4 to 6. 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.

5.2.5 Industries

One would expect employment protection to be more binding in industries that experience higher employment turnover rates. Table 7 shows turnover rates for different industries, where turnover rates are defined as the yearly absolute change in employment divided by the average firm size in the current and previous year, $\frac{|size_t - size_{t-1}|}{(size_t + size_{t-1})/2}$, for each 1-digit industry level. Industries such as "Real estate, research and development", "Education and health", and "Sewage disposal, sanitation, and other services" have slightly higher turnover rates on average. In Table 8, I estimate the DiD for each separate industry. The industries are defined at the most aggregate level, 1-digit, in order to have enough observations for each estimation. The positive effect on labor productivity is present only in "Mining, manufacture of food and textiles", "Wholesale, retail trade, hotels and restaurants", and "Sewage disposal, sanitation, and other services". The largest effect is found in sector 5, "Wholesale, retail trade, hotels and restaurants", which is also by far the largest sector in terms of the number of firms.

There appears to be no obvious link between industry turnover rates and the estimated effects.

6 Conclusions

In this paper, I showed that increased labor market flexibility led to a non-negligible increase in labor productivity. The 2001 Swedish reform provided a natural experiment that allowed me to recover a causal effect of reduced employment protection on productivity. To address potential threats to identification, I used firm size prior to the reform as an instrument. It is unlikely that the reform was anticipated, as its implementation process was rapid and involved an unusual collaboration of political parties. The Swedish register data allowed me to relate the findings on productivity to human capital and to decompose the effect on firm age, firm size, and industry affiliation.

The increase in labor productivity does not seem to be a consequence of increased capital intensity or an increase in the educational level of workers. The results indicate that the increase in labor productivity is due to an increase in TFP rather than capital intensity, which reinforces the conclusion that the effect on labor productivity is due to increased efficiency. Further elaboration revealed that older firms drive the results, as it may take time for managers to get to know their workers' productivity. Previous literature has paid little attention to how responses to employment protection changes with firm age. It would be an interesting task for future work to elaborate on this relationship.

The reform made it easier for smaller firms to retain valuable workers and to lay off less valuable ones, which could explain some of the increase in productivity. Von Below and Skogman Thoursie (2010) study the reform's effect on turnover rates, finding that both hiring and separations increased for the smallest firms with 2 to 5 employees. A lower adjustment cost could account for some of the effect on labor productivity. However, Von Below and Skogman Thoursie (2010) argue that the effect on worker flows is considered small. Finally, an increased threat of firing may have caused a behavioral change in workers, mitigating moral hazard problems. In this study, I cannot directly assess changes in worker efforts. However, the previous study by Olsson (2009) on the 2001 Swedish reform finds that sickness absence was reduced on average in small firms. Although Olsson (2009) finds that there was a behavioral effect on workers, 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. Standard labor market models have largely 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.

References

Angrist, Joshua D., and Jörn-Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton, NJ: Princeton University Press, 2009).

Autor, David H., John J. Donohue, and Stewart J. Schwab, "The Employment Consequences of Wrongful-Discharge Laws: Large, Small, or None at All?" *American Economic Review* 94 (2004), 440-446.

Autor, David H., William R. Kerr, and Adriana D. Kugler, "Does employment protection reduce productivity? Evidence from US states," *Economic Journal* 117 (2007), F189-F217.

Bassanini, Andrea, Luca Nunziata, and Danielle Venn, "Job Protection Legislation and Productivity Growth in OECD Countries," *Economic Policy* 24 (2009), 349-402.

Bauer, Thomas K., Stefan Bender, and Holger Bonin, "Dismissal Protection and Worker Flows in Small Establishments," *Economica* 74 (2007), 804-821.

Becker, Gary. S., *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education.* 2nd ed. (Cambridge, MA: National Bureau of Economic Research, 1975).

Belot, Michele, Jan Boone, and Jan van Ours, "Welfare-Improving Employment Protection," *Economica* 74 (2007), 381-396.

Boeri, Tito, and Pietro Garibaldi, "Two Tier Reforms of Employment Protection: A Honeymoon Effect?" *Economic Journal* 117 (2007), 357-385.

Bornhäll, Anders, Sven-Olov Daunfeldt, and Niklas Rudholm, "Employment Protection Legislation and Firm Growth: Evidence from a Natural Experiment, " HUI Working Papers 102, HUI Research, (2014).

Calleman, Catharina, *Turordning vid uppsägning* 1st ed. (Stockholm: Norstedts Juridik, 2000).

DeFreitas, Gregory, and Adriana Marshall, "Labour Surplus, Worker Rights and Productivity Growth: A Comparative Analysis of Asia and Latin America," *LABOUR* 12 (1998), 515-539.

Eurostat, Data Explorer: Labor Productivity, Annual Data, `http://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=nama_aux_lp&lang=en` (accessed March 10, 2015).

Garibaldi, Pietro, Lia Pacelli, and Andrea Borgarello, "Employment Protection Legislation and the Size of Firms," *Giornale degli Economisti e Annali di Economia* 63 (2004), 33-68.

Haltiwanger, John, Ron S. Jarmin, and Javier Miranda, "Who Creates Jobs? Small Versus Large Versus Young," *Review of Economics and Statistics* 95 (2013), 347-361.

Hopenhayn, Hugo, and Richard Rogerson, "Job Turnover and Policy Evaluation: A General Equilibrium Analysis," *Journal of Political Economy* 101 (1993), 915-938.

Ichino, Andrea, and Regina T. Riphahn, "The Effect of Employment Protection on Worker Effort: Absenteeism During and After Probation," *Journal of the European Economic Association* 3 (2005), 120-143.

Imbens, Guido W., and Joshua D. Angrist, "Identification and Estimation of Local Average Treatment Effects," *Econometrica* 62 (1994), 467-475.

Koeniger, Winfried, "Dismissal Costs and Innovation," *Economics Letters* 88 (2005), 79-84.

Kugler, Adriana D., and Gilles Saint-Paul, "How do Firing Costs affect Worker Flows in a World with Adverse Selection?" *Journal of Labor Economics* 22 (2004), 553-584.

Kugler, Adriana, and Giovanni Pica, "Effects of Employment Protection on Worker and Job Flows: Evidence from the 1990 Italian Reform," *Labour Economics* 15 (2008), 78-95.

Lazear, Edward P., "Job Security Provisions and Employment," *Quarterly Journal of Economics* 105 (1990), 699-726.

Lindbeck, Assar, Mårten Palme, and Mats Persson. "Job Security and Work Absence: Evidence from a Natural Experiment," IFN Working Paper No. 660, Research Institute of Industrial Economics, (2006).

Martins, Pedro S., "Dismissals for Cause: The Difference That Just Eight Paragraphs Can Make," *Journal of Labor Economics* 27 (2009), 257-279.

Mortensen, Dale T., and Christopher A. Pissarides, " Job Creation and Job Destruction in the Theory of Unemployment," *Review of Economic Studies* 61 (1994), 397-415.

Moulton, Brent R., "Random Group Effects and the Precision of Regression Estimates," *Journal of Econometrics* 32 (1986), 385-397.

Nickell, Stephen, and Richard Layard, "Labor Market Institutions and Economic Performance" (3029-3084), in Orley C. Ashenfelter and David Card, eds., *Handbook of Labor Economics, Volume 3C*, 1st ed., (Amsterdam; North-Holland, 1999).

OECD, *OECD Employment Outlook 2004*, (Paris: OECD, 2004).

Okudaira, Hiroko, Miho Takizawa, and Kotaro Tsuru. "Employment Protection and Productivity: Evidence from Firm-Level Panel Data in Japan," *Applied Economics* 45 (2013), 2091-2105.

Olsson, Martin, "Employment Protection and Sickness Absence," *Labour Economics* 16 (2009), 208-214.

Olsson, Martin. 2013. Employment protection and parental child care, IFN Working Paper No. 952, Research Institute of Industrial Economics, Stockholm.

Saint-Paul, Gilles, "Is Labour Rigidity Harming Europe's Competitiveness? The Effect of Job Protection on the Pattern of Trade and Welfare," *European Economic Review* 41 (1997), 499-506.

Saint-Paul, Gilles, "Employment Protection, International Specialization, and Innovation," *European Economic Review* 46 (2002), 375-395.

Sargent, Timothy C., and Edgard R. Rodriguez, "Labour or Total Factor Productivity: Do We Need to Choose?" *International Productivity Monitor* 1 (2000), 41-44.

SFS 1982:80, *Lag 1982:80 om anställningsskydd* [Employment Protection Act 1982:80]. (Sweden: Statens författningssamling, Ministry of Employment).

Shapiro, Carl, and Joseph E. Stiglitz. "Equilibrium Unemployment as a Worker Discipline Device," *American Economic Review* 74 (1984), 433-444.

Skedinger, Per, *Effekter av anställningsskydd : vad säger forskningen?* 1st ed. (Stockholm:

SNS förlag, 2008).

Skogman Thoursie, Peter, ”Gjorde undantagsregeln skillnad?” *Ekonomisk Debatt* 97 (2009), 33-40.

Statistics Sweden, *Registerbaserad arbetsmarknadsstatistik 2006 AM0207* [Labor statistics based on administrative sources 2006 AM0207], (Örebro: Statistics Sweden, 2006a).

Statistics Sweden, *Företagens ekonomi 2006 Nv0109* [Structural Business Statistics 2006 Nv0109], (Örebro: Statistics Sweden, 2006b).

Statistics Sweden, *Background Facts, Labour and Education Statistics 2009:1, Integrated Database for Labour Market Research.* (Örebro: Statistics Sweden, 2009).

von Below, David, and Peter Skogman Thoursie, ”Last In, First Out? Estimating the Effect of Seniority Rules in Sweden.” *Labour Economics* 17 (2010), 987-997.

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	5.806	5.940	5.900	5.987	0.047
	(0.615)	(0.533)	(0.546)	(0.448)	
Labor productivity	415.6	453.4	422.5	450.4	9.90
	(676.3)	(526.6)	(348.1)	(503.4)	
Value added	2328.1	2794.7	5202.8	5345.1	324.3
	(3783.7)	(3586.0)	(4485.3)	(6107.7)	
Capital labor ratio	664.7	727.4	476.2	501.2	37.7
	(3814.0)	(4019.8)	(2550.4)	(2100.9)	
Log of capital labor ratio	5.207	5.310	5.178	5.260	0.021
	(1.236)	(1.255)	(1.126)	(1.125)	
Total factor productivity	-0.0228	0.0591	0.0463	0.0843	0.044
	(0.503)	(0.436)	(0.440)	(0.378)	
Firm size	5.568	5.779	12.61	12.47	0.351
	(2.167)	(2.166)	(1.381)	(1.365)	
Age	9.180	9.822	10.30	10.83	0.112
	(4.435)	(4.048)	(3.915)	(3.533)	
Enterprise group	0.196	0.182	0.351	0.328	0.009
	(0.397)	(0.386)	(0.477)	(0.470)	
Pre-high school ratio	0.301	0.284	0.292	0.275	0.000
	(0.252)	(0.238)	(0.185)	(0.180)	
High school ratio	0.567	0.594	0.588	0.613	0.002
	(0.256)	(0.243)	(0.184)	(0.181)	
Post-high school ratio	0.125	0.116	0.114	0.107	-0.002
	(0.207)	(0.191)	(0.163)	(0.146)	
3 years post-high school ratio	0.0455	0.0434	0.0350	0.0351	-0.002
	(0.128)	(0.120)	(0.0916)	(0.0857)	
Observations	36,568	24,651	7,040	4,445	

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.

Table 2: Estimated effect of the 2001 reform on labor productivity, stepwise inclusion of covariates

Treatment effect		Model (1)	(2)	(3)
AsT	D_{it}	0.0277***	0.0167*	0.0227**
		(0.0107)	(0.00982)	(0.00923)
	Obs.	105,667	105,667	105,667
ITT	Z_{it}	0.0494***	0.0437***	0.0348***
		(0.00997)	(0.00916)	(0.00906)
	Obs.	72,704	72,704	72,704
LATE	$\hat{D}_{it}$	0.0748***	0.0661***	0.0526***
		(0.0151)	(0.0138)	(0.0137)
	Obs.	72,704	72,704	72,704
		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, AsT (as treated), 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), (2), and (4). Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table 3: Estimated effect of the 2001 reform on labor productivity, capital intensity and TFP

Treatment effect		Log of labor productivity	Log of capital-labor ratio	Total factor productivity
AsT	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, AsT (as treated), 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), (2), and (4). Obs. stands for observations.
*** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$

Table 4: Estimated effect of the 2001 reform on the educational level of workers, 1998–2003

	Treatment effect		
	AsT	ITT	LATE
Pre-high school ratio	0.00337	0.0000361	0.0000547
	(0.00285)	(0.00328)	(0.00496)
High school ratio	-0.00350	0.00221	0.00335
	(0.00313)	(0.00358)	(0.00541)
Post-high school ratio	0.000716	-0.00170	-0.00257
	(0.00240)	(0.00264)	(0.00399)
3 years post-high school ratio	-0.00126	-0.00209	-0.00316
	(0.00158)	(0.00167)	(0.00253)
Observations	105,667	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. Coefficients for each treatment effect, AsT (as treated), ITT (intention-to-treat), LATE (local average treatment effect), in columns. Rows correspond to separate estimations for each educational level used as outcome variable.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: Estimated effect of the 2001 reform on labor productivity, for firms that did and did not separate workers in the post–reform period

	Treatment effect		
	AsT	ITT	LATE
Firms with separations	0.0560***	0.0474***	0.0733***
	(0.00950)	(0.00878)	(0.0135)
Observations	76,987	53,595	53,595
Firms without separations	-0.0224	-0.00524	-0.00769
	(0.0495)	(0.0644)	(0.0939)
Observations	28,680	19,109	19,109

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

Table 6: DiD estimations for different samples based on age categories

Cut-off	Young (firm age $< c$)			Old (firm age $\geq c$)		
age (c)	AsT	ITT	LATE	AsT	ITT	LATE
$c = 5$	-0.0125	-0.0407	-0.0602	0.0350***	0.0298***	0.0452***
	(0.0368)	(0.0379)	(0.0558)	(0.00844)	(0.00881)	(0.0133)
Obs.	25,711	12,592	12,592	79,956	60,112	60,112
$c = 6$	-0.00419	-0.0128	-0.0196	0.0347***	0.0281***	0.0425***
	(0.0313)	(0.0309)	(0.0473)	(0.00853)	(0.00896)	(0.0135)
Obs.	30,016	15,703	15,703	75,651	57,001	57,001
$c = 7$	-0.00520	0.00256	0.00387	0.0357***	0.0272***	0.0411***
	(0.0275)	(0.0265)	(0.0400)	(0.00867)	(0.00910)	(0.0137)
Obs.	34,227	18,884	18,884	71,440	53,820	53,820
$c = 8$	0.00353	0.0111	0.0169	0.0336***	0.0268***	0.0405***
	(0.0245)	(0.0243)	(0.0369)	(0.00884)	(0.00918)	(0.0138)
Obs.	38,289	21,698	21,698	67,378	51,006	51,006
$c = 9$	0.00870	0.0166	0.0251	0.0329***	0.0243***	0.0367***
	(0.0221)	(0.0222)	(0.0335)	(0.00899)	(0.00935)	(0.0141)
Obs.	42,451	24,419	24,419	63,216	48,285	48,285
$c = 10$	0.0166	0.0216	0.0325	0.0301***	0.0223**	0.0339**
	(0.0203)	(0.0202)	(0.0302)	(0.00916)	(0.00951)	(0.0144)
Obs.	46,604	27,925	27,925	59,063	44,779	44,779
$c = 11$	0.0208	0.0176	0.0264	0.0285***	0.0247**	0.0376**
	(0.0188)	(0.0185)	(0.0277)	(0.00942)	(0.00974)	(0.0147)
Obs.	50,665	31,177	31,177	55,002	41,527	41,527
$c = 12$	0.0202	0.0230	0.0344	0.0299***	0.0238**	0.0361**
	(0.0177)	(0.0173)	(0.0259)	(0.00958)	(0.00996)	(0.0151)
Obs.	54,447	33,992	33,992	51,220	38,712	38,712
$c = 13$	0.0382**	0.0278*	0.0419*	0.0285***	0.0207**	0.0314**
	(0.0150)	(0.0165)	(0.0247)	(0.0105)	(0.0101)	(0.0153)
Obs.	65,592	36,448	36,448	40,075	36,256	36,256

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 AsT (as treated),, ITT (intention-to-treat), LATE (local average treatment effect) and cut-off age (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 7: Turnover in different industries

Code	Industries	Turnover rate
1	Mining, manufacture of food and textiles	0.160
2	Manufacture of wood, chemicals, rubber, metals, and machinery	0.152
3	Manufacture of electrical and transport equipment, and other	0.155
4	Electricity, water, and construction	0.177
5	Wholesale, retail trade, hotels and restaurants	0.168
6	Transport, post and telecommunications, and financial intermediation	0.166
7	Real estate, research and development	0.209
8	Education and health	0.196
9	Sewage disposal, sanitation, and other service activities	0.218

Turnover rate is defined for the pre-reform period, 1998-2000, as the yearly absolute change in firms size divided by the average firm size in the current and previous year, $\frac{|size_t - size_{t-1}|}{(size_t + size_{t-1})/2}$, for each 1-digit industry level.

Table 8: Estimated effect of the 2001 reform on labor productivity in different industries

Code	Industries	AsT	ITT	LATE
1	Mining, manufacture of food	0.0156	0.0916**	0.140**
	and textiles	(0.0441)	(0.0446)	(0.0698)
	Obs.	3,103	2,178	2,178
2	Manufacture of wood, chemicals,	-0.000116	0.0243	0.0372
	rubber, metals, and machinery	(0.0167)	(0.0167)	(0.0256)
	Obs.	17,071	13,193	13,193
3	Manufacture of electrical and	-0.0201	-0.0303	-0.0461
	transport equipment, and other	(0.0396)	(0.0403)	(0.0611)
	Obs.	3,034	2,287	2,287
4	Electricity, water, and construction	0.0421**	-0.00368	-0.00565
		(0.0202)	(0.0219)	(0.0336)
	Obs.	15,882	11,550	11,550
5	Wholesale, retail trade, hotels	0.0321*	0.0482***	0.0689***
	and restaurants	(0.0174)	(0.0179)	(0.0255)
	Obs.	38,017	25,985	25,985
6	Transport, post and telecommunications,	0.0358	0.0466*	0.0718*
	and financial intermediation	(0.0274)	(0.0251)	(0.0387)
	Obs.	7,948	5,573	5,573
7	Real estate, research and development	0.0460	0.0478	0.0837
		(0.0485)	(0.0494)	(0.0863)
	Obs.	14,486	8,397	8,397
8	Education and health	-0.0197	-0.00219	-0.00321
		(0.0569)	(0.0636)	(0.0926)
	Obs.	3,432	2,076	2,076
9	Sewage disposal, sanitation, and	0.115	0.185**	0.325***
	other service activities	(0.0707)	(0.0737)	(0.124)
	Obs.	2,694	1,465	1,465

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, AsT (as treated), ITT (intention-to-treat), LATE (local average treatment effect), are separate estimations. Rows correspond to separate estimations for each industry. Obs. stands for observations.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

B Figures

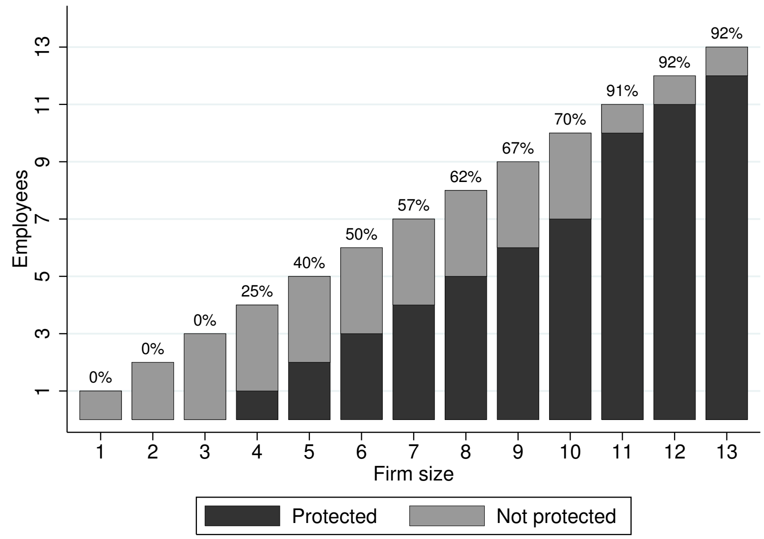


Figure 1: Protected workers after the 2001 reform
Note: The bars show the absolute number of protected and unprotected workers. The labels over each bar refer to the percent of protected workers.

Figure 2: Firm productivity and number of employees

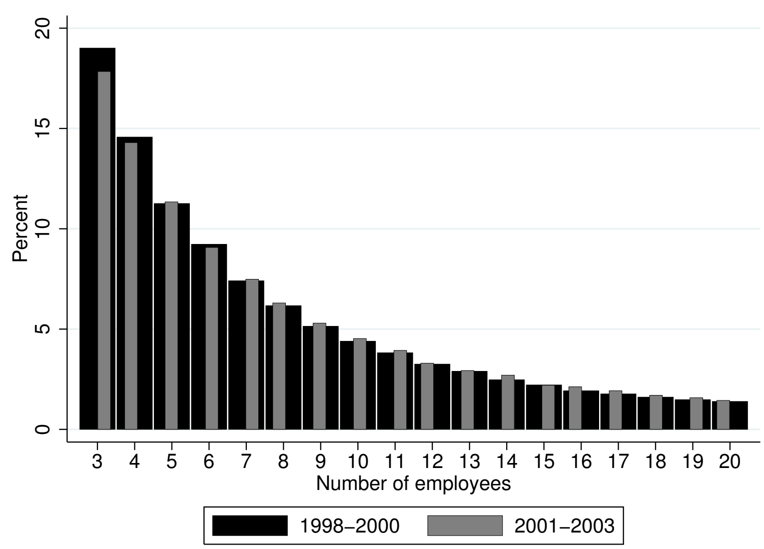


Figure 3: Histogram of number of employees 1998–2000 and 2001–2003

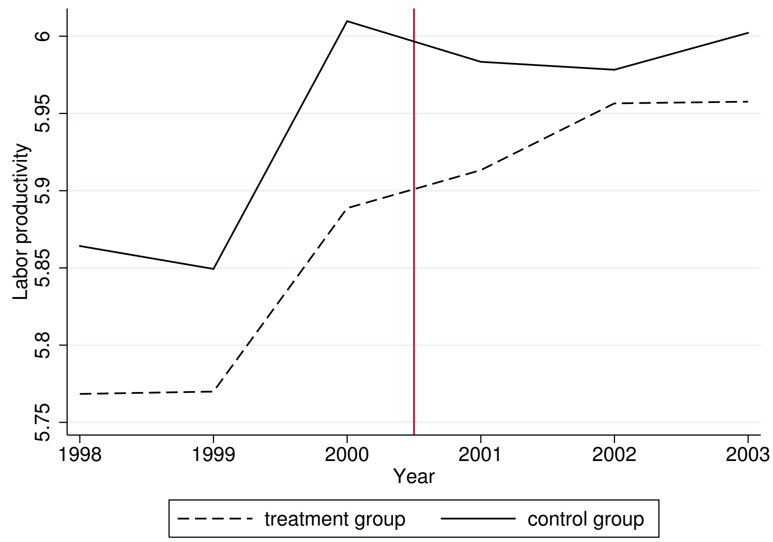
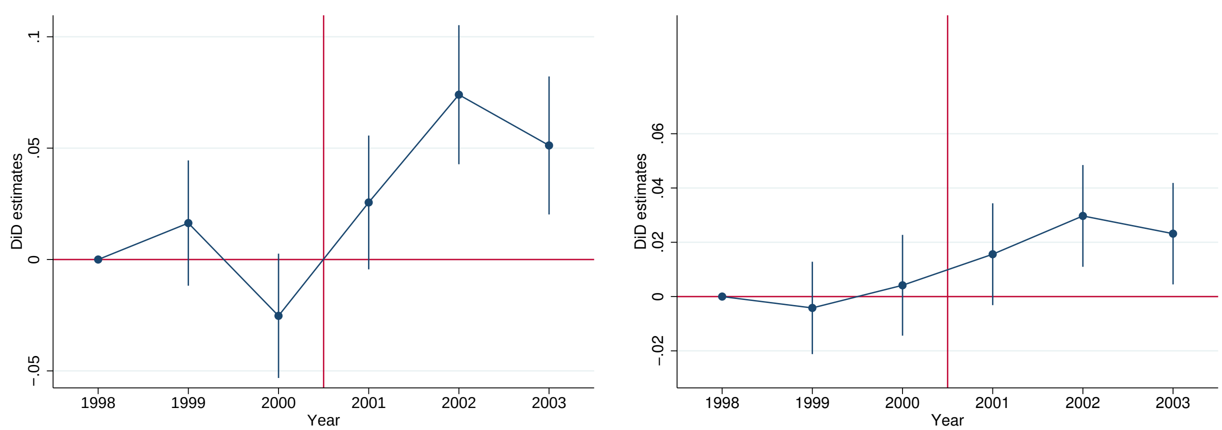


Figure 4: Labor productivity in treatment and control group, yearly averages



(b) ITT effects

(a) AsT effects

Figure 5: Year specific estimates of the 2001 reform on labor productivity

Note: The DiD estimates on the y-axis are the estimated coefficients β_t from equation (5). The year 1998 is used as baseline. The vertical lines refer to a 95% confidence interval.

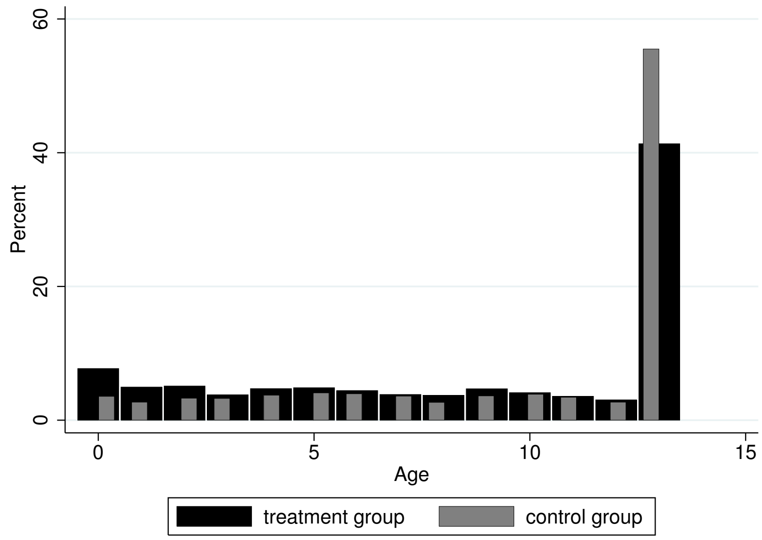


Figure 6: Distribution of age for firms in the treatment and control group in 1999
Note: The data is truncated so that all firms born before 1986 get 1986 as birth date. The maximum age is therefore 13 years in 1999, hence the skewed distribution.

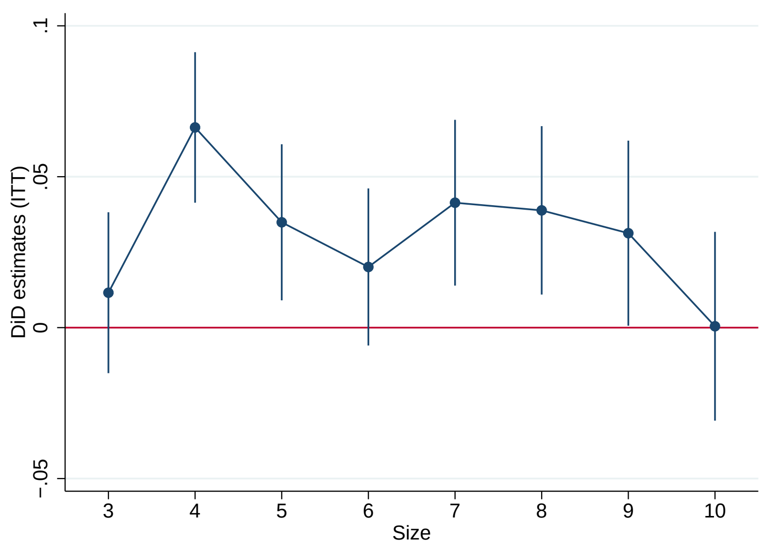


Figure 7: Size specific DiD estimates of the 2001 reform
Note: The control group of firms with 11–15 employees is used as baseline. The vertical lines refer to a 95% confidence interval. The DiD estimates on the y-axis are the estimated coefficients β_s from equation (7).

Appendix

Data description

Number of employees is defined according to number of employees in a firm in November each year. To be classified as an employee, she/he has to earn a salary that exceeds a certain threshold (Statistics Sweden 2006a). 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).[20] Individuals can only be classified as employed in one firm at a time, and this classification is based on the highest wage sum in November. Firm value added is calculated by SCB as value of production minus value of depletion. Like the employee variable, 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.

The time frame is limited to 1998–2003, three years before and three years after the form. In 2004, Statistics Sweden changed the way they defined closely-held firms, resulting in a sharp increase in the total number of firms. In 1997, there was a change in Swedish EPA that made it easier for firms to employ workers on temporary contracts.

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 excluded in order to facilitate the identification of the reform. Moreover, firms with one or two employees are excluded. These size categories presumably contain the majority of the self-employed, and the exclusion will hence remove most of the inconsistency over time.

[20] This is equivalent of about USD 5,900, using the the exchange rate in February 15, 2015.

Tables

Table A1: ICT industries dropped from main estimations

Code	Industries	Observations
24650	Manufacture of prepared unrecorded media	8
24660	Manufacture of other chemical products n.e.c.	60
25240	Manufacture of other plastic products	388
30010	Manufacture of office machinery	11
30020	Manufacture of computers and other information processing equipment	99
31100	Manufacture of electric motors, generators and transformers	192
31200	Manufacture of electricity distribution and control apparatus	114
31300	Manufacture of insulated wire and cable	29
31620	Manufacture of other electrical equipment n.e.c.	166
32100	Manufacture of electronic valves and tubes and other electronic components	90
32200	Manufacture of television and radio transmitters and apparatus for line telephony and line telegraphy	27
32300	Manufacture of television and radio receivers, sound or video recording	19
33200	Manufacture of instruments and appliances for measuring, checking, testing, navigating and other purposes	118
36500	Manufacture of games and toys	56
52740	Repair n.e.c.	102
64201	Network operation	28
64202	Radio and television broadcast operation	1
64203	Cable television operation	9
72100	Hardware consultancy	55
72300	Data processing	74
72400	Data base activities	24
72500	Maintenance and repair of office, accounting and computing machinery	47
72600	Other computer related activities	49
74879	Various other business activities	28
	Total	1,790

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

Table A2: Estimated effect of the 2001 reform on labor productivity without sample restrictions

Treatment effect		Sample: Including the ICT sector	Sample: Including firms with no data on capital and TFP
AsT	D_{it}	0.0244***	0.0231***
		(0.00791)	(0.00523)
	Obs.	107,457	374,352
ITT	Z_{it}	0.0402***	0.0120**
		(0.00836)	(0.00550)
	Obs.	74,013	244,076
LATE	$\hat{D_{it}}$	0.0609***	0.0181**
		(0.0126)	0.00828)
	Obs.	74,013	244,076
		Year FE, Industry FE, Ent. group FE, Age FE	Year FE, Industry FE, Ent. group FE, Age FE

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, AsT (as treated), 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), (2), and (4). Obs. stands for observations. *** p< 0.01, ** p< 0.05, * p< 0.1

Table A3: Estimated effect of the 2001 reform on labor productivity using different bandwidths

		Bandwidth					
		3–20	3–50	3–100	3–15		
					Excluding firms of size		
					10–11	9–12	8–13
AsT	D_{it}	0.0200***	0.0202***	0.0205***	0.0216**	0.0338***	0.0339**
		(0.00773)	(0.00651)	(0.00626)	(0.0105)	(0.0123)	(0.0152)
	Obs.	115,438	134,316	140,618	96,041	86,239	75,682
ITT	Z_{it}	0.0360***	0.0404***	0.0439***	0.0341***	0.0480***	0.0388**
		(0.00726)	(0.00599)	(0.00574)	(0.0104)	(0.0127)	(0.0165)
	Obs.	81,097	98,029	103,839	65,257	57,684	49,741
LATE	$\hat{D}_{it}$	0.0474***	0.0487***	0.0522***	0.0456***	0.0587***	0.0448**
		(0.00955)	(0.00720)	(0.00681)	(0.0139)	(0.0154)	(0.0190)
	Obs.	81,097	98,029	103,839	65,257	57,684	49,741

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, AsT (as treated), 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), (2), and (4). 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

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

Treatment effect		ln(Value added)	ln(Y+1)
AsT	D_{it}	0.0369***	0.0231**
		(0.00976)	(0.00913)
	Obs.	105,667	105,667
ITT	Z_{it}	0.166***	0.0348***
		(0.0104)	(0.00900)
	Obs.	72,704	72,704
LATE	$\hat{D}_{it}$	0.251***	0.0526***
		(0.0170)	(0.0136)
	Obs.	72,704	72,704
		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 value added per employee plus 1. Each treatment effect, AsT (as treated), 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), (2), and (4). Obs. stands for observations.
*** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$

Table A5: Placebo estimations

		Placebo size cut-off, c, (bandwidth) c= 13 (11–16)	c= 15, (11–20)	c= 20, (11–30)	c= 25, (11–40)
AsT	D_{it}	0.0162	-0.00463	0.000669	-0.00993
		(0.0153)	(0.0136)	(0.0126)	(0.0125)
	Obs.	19,481	26,921	37,341	42,586
ITT	Z_{it}	0.00364	-0.00446	0.0119	0.0146
		(0.0163)	(0.0129)	(0.0112)	(0.0112)
	Obs.	11,281	17,519	26,732	31,490
LATE	$\hat{D_{it}}$	0.0155	-0.0116	0.0206	0.0216
		(0.0686)	(0.0333)	(0.0193)	(0.0165)
	Obs.	11,281	17,519	26,732	31,490

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, AsT (as treated), 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), (2), and (4). 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 A6: Using firm size in 1998 as treatment indicator and instrument

Treatment effect		Log of labor productivity	Log of capital-labor ratio	Total factor productivity
ITT	Z_{it}	0.0377***	0.0163	0.0322***
		(0.00944)	(0.0200)	(0.00870)
LATE	$\hat{D_{it}}$	0.0630***	0.0273	0.0539***
		(0.0158)	(0.0334)	(0.0145)
	Obs.	67,441	67,441	67,441
		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, ITT (intention-to-treat) and LATE (local average treatment effect), are separate estimations. Z_{it}, and $\hat{D_{it}}$, are the corresponding difference-in-differences dummy variables from equations (1) and (2). Obs. stands for observations.
*** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$

Table A7: First stage equations on the DiD estimator D_{it}

Z_{it}	0.6607***
	(0.0092)
F-statistics	5158.87
Adj. R^2	0.8290
Partial R^2	0.2687
Shea's Adj. Partial R^2	0.2687
Year FE	yes
Industry FE	yes
Ent. group	yes
Age FE	yes
Observations	72,704

Robust standard errors, clustered on firms, in parentheses. The estimation corresponds to the first stage equation (3).
*** p< 0.01, ** p< 0.05, * p< 0.1

Table A8: Estimated effect of the 2001 reform, using yearly means

	Treatment effect	
	AsT	ITT
Labor productivity	0.0282**	0.0533**
	(0.00944)	(0.0185)
Obs.	12	12

Robust standard errors in parentheses. Each treatment effect, AsT (as treated) and ITT (intention-to-treat), are separate estimations. Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Table A9: Estimated effect of the 2001 reform on labor productivity, clustering standard errors on size

		Bandwidth	
		3–50	3–100
AsT	D_{it}	0.0202**	0.0205**
		(0.00917)	(0.00886)
	Obs.	134,316	140,618
ITT	Z_{it}	0.0404***	0.0439***
		(0.0100)	(0.00972)
	Obs.	98,029	103,839
LATE	$\hat{D}_{it}$	0.0487***	0.0522***
		(0.0120)	(0.0115)
	Obs.	98,029	103,839

Robust standard errors, clustered on firm size, in parentheses. Each treatment effect, AsT (as treated), 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), (2), and (4). The full model with all covariates is used for all estimations. Bandwidth refer to number of employees in a firm. Obs. stands for observations.
*** p< 0.01, ** p< 0.05, * p< 0.1

Figures

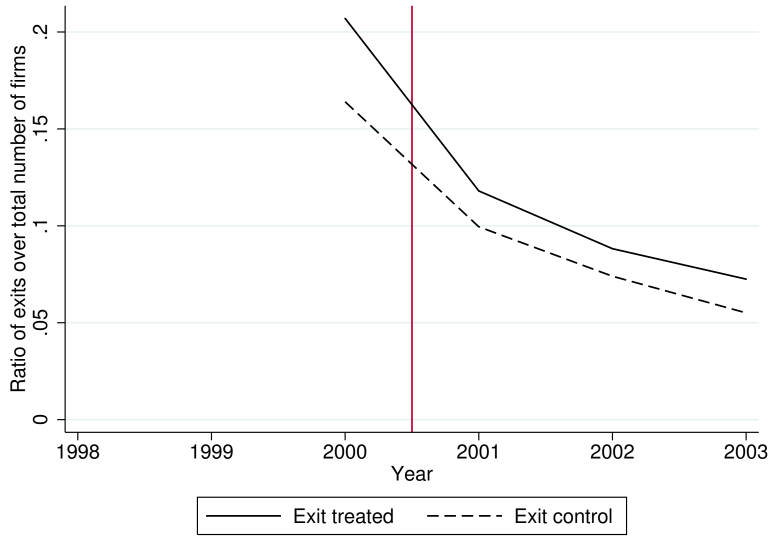


Figure A1: Exit in treatment and control group, yearly averages