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Employment protection and labor productivity

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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 explained by total factor productivity and capital intensity rather than the educational level of workers.

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 concurs that employment protection increases firms' firing costs. Increased firing costs may affect hiring decisions and restrict firms from freely adjusting their labor according to demand (Hopenhayn and Rogerson, 1993; Lazear, 1990; Mortensen and Pissarides, 1994; Saint-Paul, 1997). Although such a restriction 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 a 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 must 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. The LIFO rules thus limit firms' flexibility to choose who to retain or to lay off. 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, and find that this reform increased labor productivity by 2 to 3% in the treatment group of small firms in comparison 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 relate the findings on labor productivity to capital intensity, total factor productivity (TFP), and human capital, and to decompose the effects on firm age and firm size. In addition, I can extend the analysis to value added, revenues, and profit. 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.

I contribute to the literature by relating the effects on productivity to human capital through an analysis of workers' educational level. Increased labor market flexibility did not change the workers' educational level. In addition, by decomposing the effect on firm age, I show that the positive effect on labor productivity is present only for older

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firms. This finding could indicate the time it takes for managers to learn about their workers' productivity. The effect on labor productivity is also more apparent in smaller firms as a result 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 shortest tenure, firms are free to choose among the 3 workers with the shortest 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. Extending the analysis to additional outcome variables, I find that the reform increased both value added and revenues.

The fact that the reform increased labor productivity by approximately 2 to 3% is non-negligible. According to official statistics, the annual percentage change in labor productivity in Sweden between 1997 and 2003 is estimated at 2.2% (Eurostat). The increase in labor productivity can be attributed to an increase in both TFP and capital intensity. TFP accounts for most of the increase, 67%, and capital deepening accounts for 33%. The increased threat of being fired could have caused a behavioral change among workers, which could partially account for the increase in labor productivity. In addition, the reform made it easier for small firms to retain or lay off personnel based on workers' idiosyncratic productivity.

The lessons from the the Swedish reform are particularly relevant for understanding employment protection that involves priority rules in the case of redundancy. There is similar legislation in 81 other countries, including Austria, France, Germany, Greece, Italy, China, and India (World Bank, 2015). The Netherlands, in particular, has LIFO rules that are very similar to those in Sweden. In addition, priority rules are common practice within certain sectors. In the United States, most layoffs in school districts are determined by seniority rules (Boyd et al., 2011).

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 to cross-country studies is the comparability of legislations across countries (OECD, 2004). Few studies use variation within a country to establish a causal effect of employment protection on labor productivity (Autor et al., 2007; Okudaira et al., 2013). Several countries in Europe have similar size thresholds to that of Sweden and discriminate employment protection across firms. Although there are several studies on the effect of these firm size thresholds (Bauer et al., 2007; Cingano et al., 2016; Garibaldi et al., 2004; Kugler and Pica, 2008; Martins, 2009; Olsson, 2009; von Below and Thoursie, 2010), this study is, to my knowledge, the first to focus on labor productivity. The study by Cingano et al. (2016) focuses on capital intensity and TFP, but not labor productivity.

Unlike previous studies by Autor et al. (2007), and Okudaira et al. (2013), which analyze the costs of wrongful discharge, this study focuses on the costs of priority rules in the case of redundancy. Autor et al. (2007) use the adoption of wrongful discharge in US courts to study the effects of firing costs on productivity, finding that as firing costs increase, TFP decreases, whereas labor productivity increases. Okudaira et al. (2013) exploit variations in court decisions in Japan to study the effect of employment protection on productivity, finding that TFP and labor productivity decrease with increased firing costs, whereas no clear effect was found on capital. In addition, the studies by Autor et al. (2007), and Okudaira et al. (2013) analyze an increase in the protection of workers, whereas this study focuses on the effects of a decrease in the protection of workers. The results indicate that the effects of reduced employment protection are not necessarily the reverse of increased employment protection. All the previous empirical studies find that an increase in employment protection decreases TFP. However, Autor et al. (2007) find that an increase in capital intensity led to an increase in labor productivity. In contrast, by studying court decisions in Japan, Okudaira et al. (2013) find that increased firing costs decrease both labor productivity and TFP.

Based on theoretical predictions, the Swedish reform may affect productivity in different ways. Standard models of the labor market (see Lazear, 1990; Mortensen and Pissarides, 1994) assume that employment protection affects firm productivity through changes in job flows. The 2001 reform did cause an increase in employment turnover rates (von Below and Thoursie, 2010)[1], possibly affecting productivity in accordance with these models. Worker effort, though important, is disregarded by these standard models. Ichino and Riphahn (2005) develop a framework, related to Shapiro and Stiglitz's (1984) theory on wages and the threat of firing as a method of disciplining a worker, and show that employment protection limits the firm's willingness to monitor and fire workers who exhibit laziness or shirking. In line with this, the 2001 reform may have caused a behavioral change in workers regarding their level of effort.[2] Moreover, a change in the cost of adjusting labor may have changed the choice of capital intensity, which directly affects labor productivity. Changes in the composition of human capital within the workforce induced by a less-stringent screening of new hires may also affect productivity. Finally, an increase in the possibility to retain more productive personnel may increase productivity.

2. Institutional setting

Since 1974, all Swedish firms have adhered to the Swedish Employment Protection Act (EPA) (Skedinger, 2008), which imposes the LIFO regulations. The LIFO regulations stipulate that in the case of redundancy, the employer must lay off workers according to the established priority lists that rank individuals based on accumulated tenure within the firm. The person with the shortest accumulated tenure must be the first one to go. 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 these LIFO regulations, the firm is liable to pay damages.

In general, a Swedish firm cannot fire an employee without just cause (*saklig grund*), which exists only in the case of redundancy or for reasons concerning the worker personally, such as misconduct, which would deem the worker unfit to continue employment. It is typically difficult to fire an employee based on job performance unless extensive attempts to provide the employee with other jobs within the firm have been made and it has been shown that the employee constitutes a significant cost for the firm.

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 fixed-term contracts. Between 1997 and 2007, only one major change was made to the EPA – the 2001 reform, which I use as a natural experiment in this study.

The 2001 reform was introduced on January 1, and is the only regulation that discriminates employment protection over firm size

[1] von Below and Thoursie (2010) investigate the 2001 reform on employment turnover rates and find that both hires and separations increased approximately 5% in the group of small firms, leaving net employment unaffected.

[2] Olsson (2009) studies the 2001 reform and finds that it reduced sickness absence among the group of small firms. However, the effect of a decrease in sickness absence on productivity is ambiguous. On the 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.

Table 1
Number and share of protected and unprotected workers before and after the 2001 reform.

Firm size	Pre-reform		Post-reform		Percentage change in protected workers
	Protected	Unprotected	Protected	Unprotected	
2	1 (50%)	1 (50%)	0 (0%)	2 (100%)	−50
3	2 (67%)	1 (33%)	0 (0%)	3 (100%)	−67
4	3 (75%)	1 (25%)	1 (25%)	3 (75%)	−50
5	4 (80%)	1 (20%)	2 (40%)	3 (60%)	−40
6	5 (83%)	1 (17%)	3 (50%)	3 (50%)	−33
7	6 (86%)	1 (14%)	4 (57%)	3 (43%)	−29
8	7 (87.5%)	1 (12.5%)	5 (62.5%)	3 (37.5%)	−25
9	8 (89%)	1 (11%)	6 (67%)	3 (33%)	−22
10	9 (90%)	1 (10%)	7 (70%)	3 (30%)	−20
11	10 (91%)	1 (9%)	10 (91%)	1 (9%)	0
12	11 (92%)	1 (8%)	11 (92%)	1 (8%)	0
13	12 (92%)	1 (8%)	12 (92%)	1 (8%)	0
14	13 (93%)	1 (7%)	13 (93%)	1 (7%)	0
15	14 (93%)	1 (7%)	14 (93%)	1 (7%)	0

Cell entries refer to the number of workers, with the exception of the right-most column, and the number of protected and unprotected workers assumes that all workers are on open-ended contracts. The percentage of firm size is found in parentheses.

(Skedinger, 2008). The reform offered an exemption from the LIFO rules to firms with fewer than 11 employees. These small firms are allowed to exempt from the priority lists two workers who are of particular value to the firm. Instead of having to lay off the worker with the shortest accumulated tenure, these small firms can now choose among the three workers with the shortest accumulated tenure, meaning that firms are more flexible to choose who to retain or lay off. Table 1 summarizes the effect of the 2011 reform on the protection of workers. If, for example, a firm with 10 employees had to lay off a worker before the reform, it had to lay off the last person hired, leaving 9 workers (90%) protected by seniority. After the 2001 reform, a firm with 10 employees has the possibility to make an exemption for the last two persons hired, leaving 7 workers (70%) protected by seniority.[3] Similarly, in a firm with 5 employees, 4 workers (80%) were protected by seniority before the 2001 reform, and only 2 workers (40%) are protected by seniority after the reform. In general, the exemption to the LIFO-rules implies that the smaller the firm is, the larger the share of unprotected workers. The percentage change in protected workers after the reform is the largest for firms of size 3, and then decreases with firm size. The design of the reform is visualized in Fig. 1, where the share of unprotected workers is seen to decrease with size.

One may argue that the LIFO regulations are not an effective means of employment protection because there are mechanisms to circumvent them. However, as I will show, these mechanisms are cumbersome, may only be applied in select situations, and importantly, LIFO regulations are perceived by employers as a significant obstacle to retaining competent workers based on survey data. After the 2001 exemption to the LIFO regulations was implemented, 50% of survey respondents said they were likely to use the LIFO exemption and 32% used it in the last year. Almost all the firms (93%) that used the exemption stated that being able to do so was vital for the firm's future (Svenskt Näringsliv, 2009). Some of the select situations under which LIFO regulations may be circumvented include collective agreements, which are selectively approved and require sufficient bargaining power against the union, and fixed-term contracts, which automatically turn into open-ended contracts after two years.[4] All open-ended contracts are subject to the

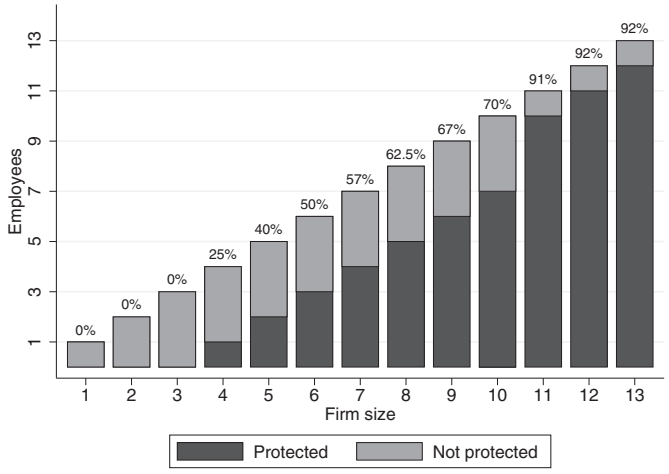

Fig. 1. Protected workers after the 2001 reform. Note: The bars show the absolute number of protected and unprotected workers. The labels over each bar refer to the percentage of protected workers.

LIFO regulations. 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 management positions, persons hired to work in the employer's household, or workers participating in employment subsidy programs (1§ in SFS, 1982:80).[5] As the LIFO rules are widely implemented, the 2001 exemption to these regulations were deemed significant by 95% of respondents in a 2009 survey (Svenskt Näringsliv, 2009). In addition, a survey of 3878 firms in 2014 shows that 60% of employers believe that a future repeal of the 2001 reform would impact their firm negatively (Svenskt Näringsliv, 2014).

Several features of the 2001 reform make it a particularly suitable setup for a natural experiment. First, the 2001 reform constitutes a discrete change in employment protection for a specific group of firms with fewer than 11 employees. Second, the process from discussion to implementation was fast, and unlikely to have been anticipated. The reform was not discussed in public until the beginning of February 2000 when the Ministry of Industry presented a memorandum with two alternatives for how to soften the seniority rules. One alternative, to

[3] The example assumes that all workers are on open-ended contracts. If a firm has a share of workers on fixed-term contracts, those who are on open-ended contracts still fall under the LIFO regulations. For example, for a firm with 10 employees, of which 5 are on open-ended contracts and 5 are on fixed-term contracts, the reform would change the number of protected workers from 4 (40%) to 2 (20%).

[4] See Heyman and Skedinger (2016), for an analysis of collective agreements and job flows.

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

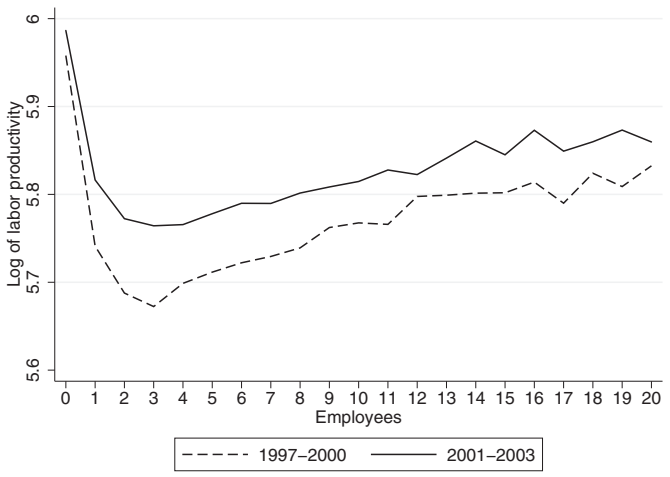

Fig. 2. Labor productivity and number of employees.

let all firms exempt two workers from the lists of seniority, was put forward as a government bill in May 2000, but it did not pass. A third alternative was presented by the Labor Market Committee in September 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. These are political fractions that are often known to be on opposite sides of the political spectrum. 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.[6]

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 and the reform threshold, the law stipulates that one should disregard members of the employer's family, workers in management 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 fixed-term and open-ended contracts are equally weighted.

3. Data

The data used are firm and establishment data from Statistics Sweden (SCB) for all firms that had at least one employee between 1997 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, sales, earnings before income taxes (EBT), ownership status and industry affiliation at the firm level.[8]

The 2001 reform took place amid an information technology boom and bust cycle. As a robustness check, all firms within the ICT industries were dropped from the estimations (see Tables B1–B2 in the Appendix for details). The inclusion of these industries does not seem to change the results. They are therefore included in the main estimations. The sample is restricted to corporations (limited companies), excluding firms within the agricultural sector and government-owned corporations. To facilitate the comparison of different output and input measures, estimations in Sections 4.3–4.5 will be restricted to firms with non-missing values for capital, revenue and profit.

The data do not allow the identification of kinship, workers' positions, fixed-term or open-ended contracts. I will assume that at least one worker in each firm holds a managerial position. The number of employees is therefore reduced by one for all firms. Moreover, the 2001 reform does not differentiate between fixed-term and open-ended contracts when defining the firm 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, 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 B3, 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.[9] Fig. 2 depicts labor productivity for firms with 0 to 20 employees. The values for the smallest firms are high. Firms with zero, one, and two employees are dropped from all estimations. Disregarding the smallest firms, the relationship between labor productivity and firm size appears to be linear.

4. 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.[10]

Since the reform's effect on employment protection decreases with firm size, a DiD framework is preferable. Fig. 1 and Table 1 reveal that the reform is designed to have a greater reduction in employment protection as the firm's size decreases. This is consistent with the DiD estimates in Fig. 5, in which no effect of the reform on labor productivity is found close to the threshold of 10 employees. Given that the reform's effect on employment protection is a function of firm size, a DiD framework is preferable to, for example, a regression discontinuity (RD) design. A RD design would not be ideal because it would measure only effects around the threshold of 10 employees, thereby ignoring smaller firms, for which the change in employment protection is most significant.

4.1. 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. Fig. 3 plots the distribution of firm size for 1997–2000 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 treatment status be determined by firm size in 1999, two years before the reform took place and one year before the reform was discussed

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

[7] The data from SCB cover all firms in all industries, except for certain firms within the finance sector. I can follow firms over time using the unique firm id, FAD (Företagens och Arbetsställenas Dynamik), which traces firms through changes in corporate identity numbers that could occur due to mergers, acquisitions, and hiving-off. This facilitates the process of following firms over time.

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

[9] See the data description in the Appendix for details on the labor productivity measure. Information on hours worked is not available in the Swedish data. Note that value added typically increases with the number of workers. Therefore, dividing value added by the number of employees will make it more comparable across firms of different sizes. In Table B4, the results hold when I shift the data before log-transformation and when using the natural logarithm of value added, not divided by number of employees.

[10] The results are not sensitive to expanding the size of the control group (Table B3). Furthermore, no effect is found when altering the size cut-off to create placebo treatment groups (Table B5).

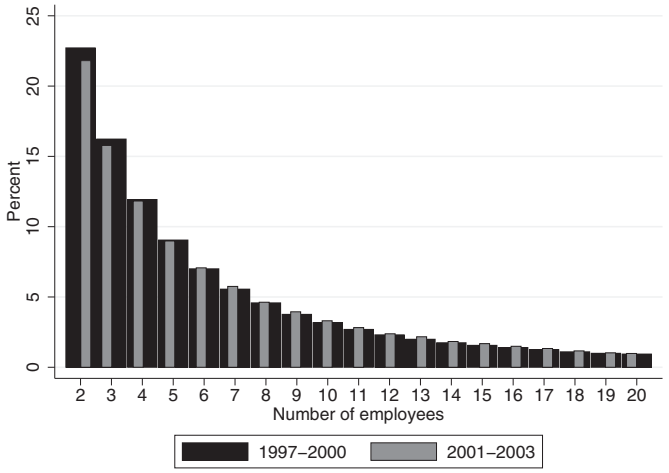

Fig. 3. Histogram of the number of employees, 1997–2000 and 2001–2003.

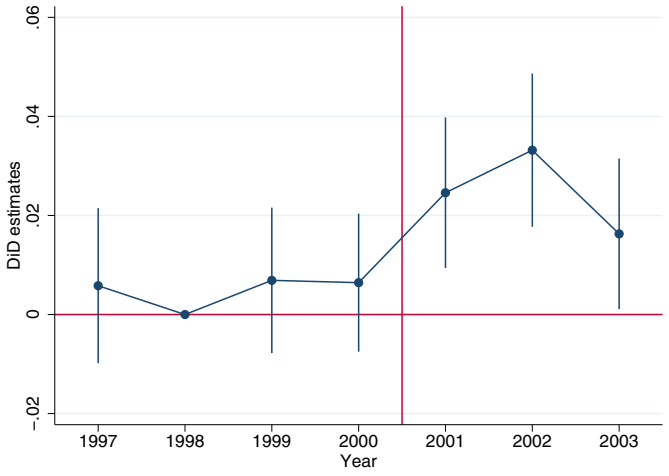

Fig. 4. Year-specific estimates of the 2001 reform on labor productivity. Note: The DiD estimates on the y-axis are the estimated coefficients β_t from Eq. (2). The year 1998 is used as a baseline. The vertical lines refer to a 95% confidence interval.

in public.[11] Therefore, I can estimate the intention-to-treat (ITT) and the local average treatment effect (LATE).

Descriptive statistics for the two groups before and after the reform are shown in Table 2. 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.[12] The DiD 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 by the following equation using OLS:

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

where Y_{it} is the natural logarithm of value added per employee 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

Table 2
Mean values before and after the 2001 reform, 1997–2003.

	Treatment group		Control group		DiD
	Pre-reform	Post-reform	Pre-reform	Post-reform	
Labor productivity	5.771	5.895	5.854	5.958	0.020
	(0.558)	(0.494)	(0.532)	(0.465)	
Value added/employee	381.2	418.1	402.4	436.0	3.30
	(561.1)	(561.9)	(374.4)	(393.6)	
Value added	2419.4	2910.7	5452.0	6475.8	−532.5
	(3676.5)	(3623.4)	(5273.2)	(5703.7)	
Firm size	5.373	5.461	12.73	12.74	0.078
	(2.153)	(2.171)	(1.390)	(1.391)	
Firm age	9.065	9.383	9.759	10.10	−0.023
	(4.265)	(4.188)	(4.031)	(3.887)	
Enterprise group	0.236	0.220	0.417	0.402	−0.001
	(0.424)	(0.414)	(0.493)	(0.490)	
Observations	130,896	71,662	23,647	14,002	

Standard deviation in parentheses. Labor productivity is defined as the natural logarithm of 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.

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 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 industry dummies (3-digit NACE code), and a full set of dummies representing industry-by year interactions (using a 1-digit NACE code), a dummy taking the value of one if the firm belongs to an enterprise group and a firm age dummy variable taking the value one if a firm has been alive for 13 years or more.[13] All covariates in X_{i99} are defined in the year 1999 in order to be exogenous.

The treatment and control groups need to follow parallel trends before the reform in order for the DiD analysis to be valid. To obtain an indication of the validity of the parallel trends assumption, I estimate annual treatment effects, which also capture some of the dynamics of the reform. To capture annual effects of the reform, I estimate the following model:

$$Y_{it} = \alpha + \lambda_t + \delta d_{i99} + \sum_{t=1997}^{2003} \beta_t(\lambda_t \times d_{i99}) + X_{i99}\gamma + \upsilon_{it} \quad (2)$$

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. The results for the full model with all covariates are presented in Fig. 4. 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. In addition, the yearly effects in Fig. 4 suggest that there are no large anticipation effects.

To simplify, I suppress the notation from here on so that $Z_{it} = Post_t \times d_{i99}$. To capture the LATE, I use Z_{it} as an instrument in a two-stage least-squares (2SLS) regression to estimate the following equation:

$$Y_{it} = \alpha + \lambda_t + \delta d_{i99} + \beta \widehat{D}_{it} + X_{i99}\gamma + \upsilon_{it} \quad (3)$$

where $\widehat{D}_{it}$ is the predicted value from the first-stage Eq. (4).

$$D_{it} = \omega_0 + \lambda_t + \omega_1 d_{i99} + \omega_2 Z_{it} + X_{i99}\omega_3 + \mu_{it} \quad (4)$$

where $D_{it} = Post_t \times d_{it}$, and d_{it} is a dummy variable taking the value of 1

[11] A similar strategy to capture the different treatment effects of the reform is used by Olsson (2017) and Lindbeck et al. (2006). Table B6 shows that the results hold when I let treatment status be determined by firm size in the year 1998.

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

[13] The data is truncated so that all firms born before 1986 receive 1986 as their birth year. The maximum age is therefore 13 years in 1999 (see Fig. C2 in the Appendix).

if in the treatment group at time t. The new coefficient β is scaling the previously estimated ITT parameter with the probability of treatment, similar to Havnes and Mogstad (2011)and Olsson (2017), and estimates the LATE given the assumptions of independence, exclusion, the existence of a first stage, and monotonicity (Imbens and Angrist, 1994). 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 that discriminates over firm size. Z_{it} 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 the productivity trend between the groups prior to the reform (Fig. 4). Z_{it} can therefore be assumed to be independent of potential outcome.

Second, the exclusion restriction requires that Z_{it} affects labor productivity only through the correlation with post-reform treatment status, i.e., firm size after 2001. From the parallel trends assumption, Z_{it} does not appear to affect labor productivity in the absence of the reform. Third, the first-stage equations exist and are presented in Table B7 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.[14] ITT gives an estimate independent of the effect of potential crossovers. Attrition is a potential threat to identification of the treatment effects. Fig. C3 in the Appendix plots the exit rates for the treatment and control groups. There appears to be no obvious change as a result of the reform. This is also confirmed by DiD estimations on exit rates in Table B8 in the Appendix. A further inquiry of exit rates can be found in von Below and Thoursie (2010), whose results indicate that exit probabilities are not affected by the reform.

4.2. Total effect on labor productivity

Table 3 shows the two different estimated effects (ITT and LATE) of the 2001 reform. Columns (1)–(3) add the controls stepwise. The DiD coefficient estimates are positive for all specifications. The size of the estimated coefficients ranges from 0.02 to 0.03, indicating that exemption from the LIFO rules increases labor productivity by approximately 2 to 3%.[15,16] Including all covariates will likely result in a more accurate estimation of the reform's effect. The estimated LATE is 0.03 for the most saturated model, column (3), indicating an increase in labor productivity of approximately 3% for firms that remained in the treatment group. The estimated ITT effect of the reform is slightly lower, at 2%.[17]

In the baseline setting above, I cluster the standard errors on the firm level. However, 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 14 remaining data points. The results are shown in Table B10 in the Appendix and are statistically significant.[18] Second, I cluster the standard errors on the firm size level. The baseline setting, in which I use firms with 2 to 15 employees, produces only 14 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 once again avoid 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 B10, columns 4–5, 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.

Table 3
Estimated effect of the 2001 reform on labor productivity, stepwise inclusion of covariates.

Treatment effect		Model		
		(1)	(2)	(3)
ITT	Z_{it}	0.0202***	0.0221***	0.0200***
		(0.00531)	(0.00501)	(0.00498)
LATE	$\widehat{D}_{it}$	0.0286***	0.0312***	0.0283***
		(0.00751)	(0.00708)	(0.00704)
Observations		240,207	240,207	240,207
				Year, Industry, Ent. group, Age, Industry × Year
		Year	Year, Industry	

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 $\widehat{D}_{it}$, are the corresponding DiD dummy variables from Eqs. (1), and (3). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The size of the estimated coefficients in Table 3 is non-negligible. By comparison, average annual percentage change in labor productivity in Sweden, 1997–2003, is estimated at 2.2% (Eurostat). The estimated effect is measured over a relatively short period of time, three years, and could come from changes in, for example, capital deepening, human capital, worker effort, and increased turnover rates. The firms in this setting are relatively small, and relatively young (Table 2). Young firms in particular have been found to be more volatile and exhibit higher rates of gross job creation and destruction (Haltiwanger et al., 2013). These features may explain part of the relatively large effect on productivity given the short time frame. Annual hiring and separation rates for the treatment and control groups are presented in Table B11 in the Appendix. Hiring rates range between 15 and 28% and separations rates between 13 and 27%.[19] By comparison, the average hiring rate in Sweden in 2006 was 9% (Statistics Sweden, 2017).[20]

Firms' increased ability to retain more valuable personnel may explain the positive effect on labor productivity. To obtain an idea of whether this is the case, I restrict the sample to firms that separated at least one worker at any year during the post-reform period

[14] 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).

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

[16] A further analysis reveals that the positive effect on labor productivity is present only in the 1-digit level industry code 5, encompassing wholesale, retail trade, hotels and restaurants (Table B9 in the Appendix).

[17] LATE is, by definition, always weakly larger than ITT.

[18] The data are collapsed without any weights to yearly means in labor productivity for each treatment group.

[19] The percentage of firms that had more than 5 hires in a year range from 1 to 3%, and the percentage of firms that had more than 5 separations in a year range from 0.2 to 5%.

[20] 2006 is the first year for which the data from SCB are available.

Table 4
DiD estimations for different samples based on age categories.

Cut-off	Young (firm age $< c$)		Old (firm age $\geq c$)	
age (c)	ITT	LATE	ITT	LATE
$c = 5$	0.0179	0.0260	0.0131***	0.0184***
	(0.0178)	(0.0258)	(0.00491)	(0.00690)
Obs.	44,589	44,589	195,618	195,618
$c = 6$	0.0166	0.0240	0.0129**	0.0182***
	(0.0145)	(0.0209)	(0.00501)	(0.00704)
Obs.	57,217	57,217	182,990	182,990
$c = 7$	0.0171	0.0243	0.0134***	0.0189***
	(0.0126)	(0.0179)	(0.00511)	(0.00720)
Obs.	69,608	69,608	170,599	170,599
$c = 8$	0.0138	0.0199	0.0157***	0.0221***
	(0.0113)	(0.0162)	(0.00521)	(0.00730)
Obs.	80,542	80,542	159,665	159,665
$c = 9$	0.0174*	0.0250*	0.0137**	0.0192**
	(0.0103)	(0.0147)	(0.00538)	(0.00754)
Obs.	90,874	90,874	149,333	149,333
$c = 10$	0.0101	0.0144	0.0178***	0.0251***
	(0.00947)	(0.0135)	(0.00551)	(0.00772)
Obs.	102,897	102,897	137,310	137,310
$c = 11$	0.00857	0.0122	0.0197***	0.0277***
	(0.00880)	(0.0125)	(0.00569)	(0.00797)
Obs.	114,151	114,151	126,056	126,056
$c = 12$	0.0103	0.0147	0.0194***	0.0271***
	(0.00831)	(0.0119)	(0.00588)	(0.00820)
Obs.	124,226	124,226	115,981	115,981
$c = 13$	0.0105	0.0150	0.0200***	0.0282***
	(0.00797)	(0.0113)	(0.00600)	(0.00841)
Obs.	132,900	132,900	107,307	107,307

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, ITT, LATE and cut-off age (rows) represents a separate estimation. The coefficients correspond to the full model with all covariates. Obs. stands for observations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

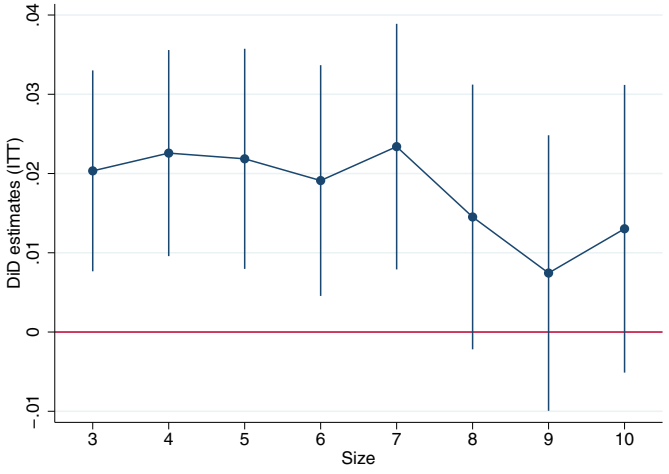

Fig. 5. Size-specific DiD estimates of the 2001 reform. Note: The control group of firms with 11–15 employees is used as a baseline. The vertical lines refer to a 95% confidence interval. The DiD estimates on the y-axis are the estimated coefficients β_s from Eq. (7).

(Table B12).[21] The separation of workers is potentially affected by the reform itself; therefore, the estimates should be interpreted with caution. The estimates are larger for the sample of firms that separated

Table 5
Estimated effect of the 2001 reform on firm performance.

Treatment effect		Labor productivity	Value added	Revenue	Profit
ITT	Z_{it}	0.0667***	0.0767***	0.0444***	0.0435
		(0.0110)	(0.0135)	(0.0136)	(0.0294)
LATE	$\widehat{D}_{it}$	0.0920***	0.106***	0.0612***	0.0601
		(0.0151)	(0.0190)	(0.0186)	(0.0405)
Observations		43,649	43,649	43,649	43,649

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it}, and $\widehat{D}_{it}$, are the corresponding DiD dummy variables from Eqs. (1) and (3). The full model with all covariates is used for all estimations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

workers, indicating an increase in labor productivity by approximately 3 to 4%, which supports 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.

4.2.1. Firm age decomposition

Previous literature finds that age plays a key role in firm behavior Haltiwanger et al. (2013). The control group has a larger share of older firms, as confirmed by Table 2. Older firms are more likely to have reached their permanent size and therefore are less likely to cross over between treatment and control groups. In Table 4, 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 13 years, there are no significant coefficients, regardless of the treatment effect. The coefficients in Table 4 are significant only for the sub-samples that include older firms, which indicates that these firms drive the results.

4.2.2. Firm size decomposition

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

$$Y_{it} = \alpha + \lambda_t + \sum_{s=3}^{10} \chi_s Size_{is99} + \sum_{s=3}^{10} \beta_s (Size_{is99} \times Post_t) + X_{i99}\gamma + \upsilon_{it} \quad (5)$$

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. Fig. 5 shows the estimated β_s for the different size categories. The figure reveals that the effect of the reform is present only for smaller firms of size 3 to 7, in line with the design of the reform outlined in Table 1, where the percentage change in protected workers after the reform is the largest for firms of size 3, and then decreases with firm size.

4.3. Total effect on firm performance

The reform may have had an effect on other firm outcome variables related to firm performance. In this section, I estimate the effect of the reform on value added, revenue, and profit. I use the natural logarithm of value added, revenue is measured as the natural logarithm of sales per number of employees, and profit is measured as the natural logarithm of earnings before income taxes (EBT) divided by sales. The sample I use is restricted to firms with non-missing values for capital, revenue and profit. The reduced sample consists of 18% of the full sample (43,649 out of 240,207 firms). A comparison of the reduced sample to the full sample reveals that treated firms in the reduced sample are approximately 5% larger and 5% older than in the full

[21] Due to lack of observations, I am not able restrict the sample to firms that did not separate any workers after the reform.

Table 6
Estimated effect of the 2001 reform on firm inputs.

Treatment effect		Total factor productivity	Capital-labor ratio	Compulsory school
ITT	Z_{it}	0.0503***	0.0738***	0.00252
		(0.00986)	(0.0215)	(0.00391)
LATE	$\widehat{D}_{it}$	0.0695***	0.102***	0.00348
		(0.0135)	(0.0296)	(0.00538)
		Upper secondary school	Tertiary education (< 3 years)	Tertiary education (= 3 years)
ITT	Z_{it}	0.000730	−0.00277	−0.00190
		(0.00429)	(0.00315)	(0.00194)
LATE	$\widehat{D}_{it}$	0.00101	−0.00382	−0.00262
		(0.00590)	(0.00434)	(0.00267)
Observations		43,649	43,649	43,649

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD dummy variables from Eqs. (1) and (3). The full model with all covariates is used for all estimations. Obs. stands for observations. *** p < 0.01, ** p < 0.05, * p < 0.1.

sample. In addition, value added is approximately 20% higher and value added per employee is approximately 15% higher than in the full sample (see pre-reform values for the treatment group in Table B13 in the Appendix). These firms may therefore be less representative of the full population. The effect on labor productivity will be re-estimated using this reduced sample. The results are presented in Table 5.[22] The effect on labor productivity is higher at 7–9%. A positive effect of 8–11% is found on value added, and a positive effect of 4–6% is found on revenue. No effect is found on profits, which may be explained by re-investments in capital (see next section) and increases in wages. Closely held firms in Sweden are obliged to declare a certain amount of its surplus as wages (SFS, 1999:1229).[23] In line with this argument, the reform seems to have increased wages by 6–8% (Table B14 in Appendix).[24]

4.4. Effect on firm inputs

To disentangle some of the components accounting for the increase in labor productivity, as well as value added and revenue, I estimate the effect of the reform on TFP, the capital-labor ratio, and workers' educational level. Book values of machinery, buildings, and land per number of employees are used to estimate capital. The measure is transformed by taking the natural logarithm. To estimate the effect of the reform on TFP, I use the following production function for each 2-digit industry and year:

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

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 equipment, K_{it}^{b} is the book value of buildings, and K_{it}^{l} is the book value of land. Similar to Autor et al. (2007), the function is estimated using OLS for each industry j and time t. The residuals from the regressions provide the TFP measure.[25]

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. I measure workers' educational level by 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.

Results are presented in Table 6.[26] Positive effects are found for TFP (5–7%) and for the capital-labor ratio (7–10%). No effect is found for any of the educational levels. Therefore, the increase in labor productivity is likely due to TFP and capital intensity rather than an increased educational level among workers.

4.5. Direct and indirect effect of the reform

In order to understand how much of the effect on labor productivity, value added and revenue can be attributed to the observed changes in

Table 7
Indirect and direct effects of the 2001 reform.

Output	Treatment effect	Eq. (7)			Indirect effect	
		TFP ($\hat{\xi}^{T}$)	Capital-labor ratio ($\hat{\xi}^{K}$)	Reform DiD ($\widehat{\beta_3}$)	TFP ($\hat{\beta}_2^{T}\hat{\xi}^{T}$)	Capital-labor ratio ($\hat{\beta}_2^{K}\hat{\xi}^{K}$)
Labor productivity	*ITT*	0.897***	0.306***	−0.00110	0.0451	0.0226
		(0.00544)	(0.00286)	(0.00482)	[67%]	[33%]
	LATE	0.897***	0.306***	−0.00152	0.0623	0.0312
		(0.00543)	(0.00285)	(0.00664)	[67%]	[33%]
Value added	*ITT*	0.907***	0.279***	0.0104	0.0456	0.0206
		(0.00799)	(0.00443)	(0.00984)	[69%]	[31%]
	LATE	0.907***	0.279***	0.0144	0.0630	0.0285
		(0.00799)	(0.00442)	(0.0137)	[69%]	[31%]
Revenue	*ITT*	0.751***	0.267***	−0.0131	0.0378	0.0197
		(0.0102)	(0.00555)	(0.0104)	[66%]	[34%]
	LATE	0.751***	0.267***	−0.00181	0.0522	0.0272
		(0.0101)	(0.00554)	(0.0144)	[66%]	[34%]
Observations		43,649	43,649	43,649		

Columns 3–5 refer to the estimated coefficients from Eq. (7). Columns 6–7 refer to the products $\hat{\beta}_2^{T}\hat{\xi}^{T}$ and $\hat{\beta}_2^{K}\hat{\xi}^{K}$. Each treatment effect, ITT, and LATE are separate estimations. Robust standard errors, clustered on firms, in parentheses. Share of total effect in brackets. Total effect is the sum of all statistically significant coefficients $\widehat{\beta_3} + \hat{\beta}_2^{T}\hat{\xi}^{T} + \hat{\beta}_2^{K}\hat{\xi}^{K}$. The full model with all covariates is used for all estimations. *** p < 0.01, ** p < 0.05, * p < 0.1.

[22] Annual effects that support the parallel trend assumption can be found in Fig. C4 in the Appendix.

[23] A substantial amount of smaller firms are likely closely held, meaning that four or fewer owners control at least 50% of the ultimate voting shares.

[24] Results for alternative measures of wages and revenue, not scaled by number of employees, are presented in Table B14 and Fig. C5 in the Appendix.

[25] 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.

[26] Annual effects that support the parallel trend assumption can be found in Fig. C6 in the Appendix. Results for an alternative measure of capital, not scaled by number of employees, are presented in Table B14 and Fig. C5 in the Appendix.

TFP and capital intensity, I will follow the product of coefficient method presented in Imai et al. (2010b). I first estimate the following equation using OLS:

$$Y_{it} = \alpha_3 + \lambda_t + \delta d_{i99} + \beta_3(Post_t \times d_{i99}) + \xi^T M_{it}^T + \xi^K M_{it}^K + X_{i99}\gamma + \upsilon_{it3} \quad (7)$$

where Y is labor productivity, value added or revenue, M^T is TFP, and M^K is the capital-labor ratio, the two inputs that had a statistically significant effect in previous Table 6. I let the estimations in Table 6 be represented by the following equation:

$$M_{it}^I = \alpha_2 + \lambda_t + \delta d_{i99} + \beta_2^I(Post_t \times d_{i99}) + X_{i99}\gamma + \upsilon_{it2} \quad (8)$$

where I is either TFP (T) or the capital-labor ratio (K). Under the assumptions of sequential ignorability and the no-interaction assumption, the indirect effect is given by the products $\widehat{\beta}_2^T\widehat{\xi}^T$ and $\widehat{\beta}_2^K\widehat{\xi}^K$, the direct effect is $\widehat{\beta_3}$, and the total effect is given by $\widehat{\beta_3} + \widehat{\beta}_2^T\widehat{\xi}^T + \widehat{\beta}_2^K\widehat{\xi}^K$ (Imai et al., 2010a,b). The effects are summarized in Fig. C7 in the Appendix.[27]

Table 7 shows the estimated coefficients from Eq. (7) and the indirect effects through TFP and capital intensity. Both TFP and the capital-labor ratio are positive and statistically significant in Eq. (7). The reform DiD indicator is, however, not significant, indicating that all of the effect of the 2001 reform on labor productivity, value added, and revenue goes through TFP and the capital-labor ratio. The indirect effects indicate that the increase in TFP accounts for 67% of the total effect on labor productivity, and 33% can be attributed to the capital-labor ratio. Similar shares are found for value added and revenue.

5. 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 an instrument based on firm size prior to the reform. 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 labor productivity to human capital, TFP, and capital intensity, and to decompose the effect on firm age and firm size. In addition, I expanded the analysis to include value added, revenue and profits.

The increase in labor productivity does not seem to be a consequence of an increase in the workers' educational level. The results indicate that the increase in labor productivity is due to an increase in both TFP and capital intensity. The increase in TFP makes up for 67% of the increase in labor productivity, which reinforces the conclusion that the effect on labor productivity is due largely to increased efficiency. In addition, the reform led to an increase in both value added and revenue.

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 change with firm age. It would be an interesting task for future work to elaborate on this relationship. Unlike previous contributions, which measure the effect of increased employment protection on labor productivity (Autor et al., 2007; Okudaira et al., 2013), this study focuses on the effects of decreased employment protection. The results indicate that the effect on capital intensity and labor productivity varies with the direction of the change in employment protection.

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 Thoursie (2010) study the reform's effect on turnover rates, finding that both hiring and separations increased for the group of small firms. A lower adjustment cost could account for some of the effect on labor productivity. However, von Below and 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. 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 employees' work effort. Further studies are needed to address the relationship between employment protection and work effort.

The lessons from the LIFO rules and the Swedish reform are particularly relevant to employment protection that involves priority rules for redundancies, and there is similar legislation in place in several other countries.[28] The findings in this paper are also based on the smallest firms, and it is unclear whether the results could be generalized to larger firms. Larger firms have increased bargaining power against labor unions, and they might be less sensitive to the occasional low-performing worker. The impact of the reform is also anticipated to be smaller for firms with a high share of workers on fixed-term contracts. In addition, there may be effects of employment protection on productivity that are not fully captured by the analysis in this paper. In particular, there could have been previous positive effects of employment protection on investments in workers to acquire firm specific skills and build human capital. The change to reduced employment protection after the 2001 reform could create opportunities for firms to lay off less productive workers who were not subject to such investments.

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[27] The assumption of sequential ignorability implies that there are no unobserved confounding variables that affects both the 2001 reform, labor productivity, TFP, and the capital-labor ratio. The no-interaction assumption implies that the controlled direct effect of the reform on labor productivity does not depend on the values of the mediators. These are strong assumptions, and the results should therefore be interpreted with caution.

[28] For example, more than 80 countries including The Netherlands, France, Germany, Italy, Austria, Greece, China, and India, have priority rules for redundancies (World Bank, 2015), and in the United States, most layoffs in school districts are determined by seniority rules (Boyd et al., 2011).

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Appendix A. Data description

Number of employees is defined according to the 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 their age, gender, and retirement pension. For example, in 2005 the threshold for a male age 25–54 is an annual salary of 50,036 SEK (Statistics Sweden, 2009).[29] Individuals can only be classified as employed in one firm at a time, and this classification is based on the individual's highest wage sum in November. Firm value added is calculated by SCB in their Structural Business Statistics 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.

To estimate labor productivity, I use the natural logarithm of value added per employee (Fig. A1). The minimum for this variable is −2.397, and the maximum is 11.598. The yearly means of this variable are presented in Table A1. Some firms exhibit negative level value added (not log transformed) and zeroes causing problems with log-transformation of the data. In my main estimations, these observations are dropped. Between 1997 and 2003, there are in total 2161 observations that report zero or negative value added (0.9% of the sample).

Given that the level value added per employee contains extreme outliers (Fig. A2), I remove the outliers below SEK −1,000,000 and above SEK 2,000,000 (in total 1321 observations) in order to estimate the effect of the 2001 reform on level value added per employee. Results are presented in Table B4. The distribution in levels for this reduced sample is shown in Fig. A2b.

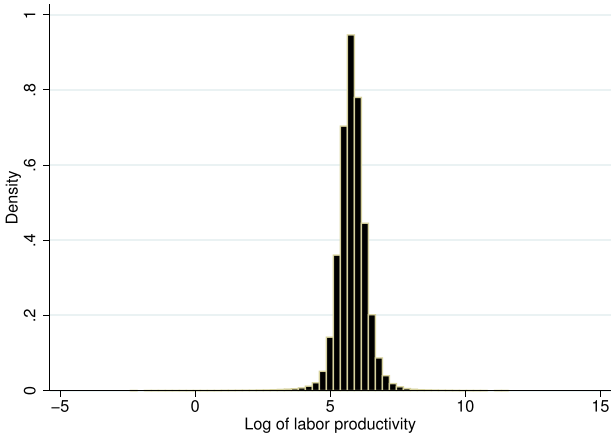
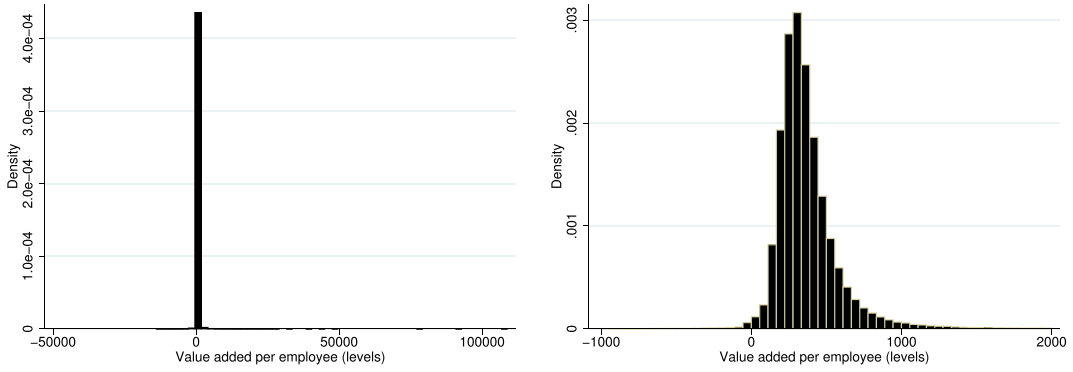


Fig. A1. Log of labor productivity.



(a) Including outliers (b) No outliers

Fig. A2. Histogram of value added per employee in levels.

[29] This is equivalent of about USD 5734, using the exchange rate in May 21, 2017.

Table A1
Mean values of labor productivity, 1997–2003.

Year	Treatment group	Control group
1997	5.722	5.806
1998	5.755	5.844
1999	5.737	5.817
2000	5.872	5.953
2001	5.885	5.951
2002	5.900	5.955
2003	5.901	5.970
Observations	37,649	202,558

Labor productivity is defined as the natural logarithm of value added per employee.

The time frame is limited to 1997–2003, four years before and three years after the 2001 reform. In 2004, Statistics Sweden changed the way they defined closely-held firms, resulting in a sharp increase in the total number of firms. 1997 is the first year for which there are financial data.

As of 2001, fishing and forestry sectors together with the self-employed are included in the statistics (Statistics Sweden, 2006b). Fishing and forestry amount to about 4500 observations, which are excluded in order to facilitate the identification of the reform. Moreover, firms with zero, 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.

Appendix B. Tables

Table B1
ICT industries.

Code	Industries	Observations
24650	Manufacture of prepared unrecorded media	22
24660	Manufacture of other chemical products n.e.c.	98
25240	Manufacture of other plastic products	711
30010	Manufacture of office machinery	50
30020	Manufacture of computers and other information-processing equipment	352
31100	Manufacture of electric motors, generators and transformers	473
31200	Manufacture of electricity distribution and control apparatus	469
31300	Manufacture of insulated wire and cable	97
31620	Manufacture of other electrical equipment n.e.c.	471
32100	Manufacture of electronic valves and tubes and other electronic components	326
32200	Manufacture of television and radio transmitters and apparatus for line telephony and line telegraphy	134
32300	Manufacture of television and radio receivers, sound or video recording	95
33200	Manufacture of instruments and appliances for measuring, checking, testing, navigating and other purposes	490
36500	Manufacture of games and toys	84
52740	Repair n.e.c.	751
64201	Network operation	93
64202	Radio and television broadcast operation	4
64203	Cable television operation	6
72100	Hardware consultancy	279
72300	Data processing	255
72400	Data base activities	62
72500	Maintenance and repair of office, accounting and computing machinery	213
72600	Other computer-related activities	117
74879	Various other business activities	41
	Total	5693

ICT for the manufacturing and service sectors as defined by Statistics Sweden.

Table B2
Estimated effect of the 2001 reform on labor productivity, excluding the ICT sector.

Treatment effect		Model		
		(1)	(2)	(3)
ITT	Z_{it}	0.0196***	0.0216***	0.0198***
		(0.00537)	(0.00506)	(0.00503)
LATE	$\widehat{D}_{it}$	0.0277***	0.0306***	0.0280***
		(0.00758)	(0.00713)	(0.00709)
	Obs.	235,473	235,473	235,473
		Year	Year, Industry	Year, Industry, Ent. group, Age, Industry × Year

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD dummy variables from Eqs. (1) and (3). Obs. stands for observations. The ICT sector is defined in Table B1.
*** p < 0.01, ** p < 0.05, * p < 0.1

Table B3
Estimated effect of the 2001 reform on labor productivity using different bandwidths.

		Bandwidth					
		2–20	2–50	2–100	2–15 Excluding firms of size 10–11	9–12	8–13
ITT	Z_{it}	0.0200***	0.0233***	0.0252***	0.0165***	0.0227***	0.0237***
		(0.00425)	(0.00364)	(0.00353)	(0.00559)	(0.00667)	(0.00822)
	Obs.	261,622	300,461	313,169	219,257	197,600	173,907
LATE	$\widehat{D}_{it}$	0.0260***	0.0283***	0.0303***	0.0210***	0.0268***	0.0267***
		(0.00552)	(0.00441)	(0.00423)	(0.00709)	(0.00788)	(0.00926)
	Obs.	261,622	300,461	313,169	219,257	197,600	173,907

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD variables from Eqs. (1) and (3). The full model with all covariates is used for all estimations. Bandwidth and size refer to the number of employees in a firm. Obs. stands for observations.
*** p < 0.01, ** p < 0.05, * p < 0.1

Table B4
Estimated effect of the 2001 reform on alternate outcome variables.

Treatment effect		log(Y + 1)	log(Y + 100)	level(Y)
ITT	Z_{it}	0.0222***	0.0123***	5.357**
		(0.00573)	(0.00386)	(2.093)
	Obs.	240,503	241,162	241,054
LATE	$\widehat{D}_{it}$	0.0314***	0.0174***	7.587**
		(0.00810)	(0.00546)	(2.962)
	Obs.	240,503	241,162	241,054

Robust standard errors, clustered on firms, in parentheses. log(Y + 1) and log(Y + 100) stands for the logarithm of value added per employee plus 1 and plus 100, respectively. level(Y) stand for value added per employee in levels, presented in Fig. A2b. Annual effects for level(Y) are presented in Fig. C1. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD dummy variables from Eqs. (1) and (3). 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 B5
Placebo estimations.

		Placebo size cut-off, c, (bandwidth)			
		c = 13 (11–16)	c = 15, (11–20)	c = 20, (11–30)	c = 25, (11–40)
ITT	Z_{it}	−0.00102	−0.000192	0.00506	0.00687
		(0.00862)	(0.00741)	(0.00708)	(0.00727)
	Obs.	42,923	59,064	80,550	91,260
LATE	$\widehat{D}_{it}$	−0.00299	−0.000408	0.00813	0.00980
		(0.0252)	(0.0157)	(0.0114)	(0.0104)
	Obs.	42,923	59,064	80,550	91,260

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD dummy variables from Eqs. (1) and (3). Bandwidth and size cut-offs refer to the number of employees in a firm. 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 B6
Using firm size in 1998 as a treatment indicator.

Treatment effect		Log of labor productivity
ITT	Z_{it}	0.0163***
		(0.00536)
LATE	$\widehat{D}_{it}$	0.0257***
		(0.00843)
	Obs.	204,677

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD dummy variables from Eqs. (1) and (3), where firm size in 1998 is used as a treatment indicator. 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 B7
First stage equation on the DiD estimator D_{it}.

Z_{it}	0.7069***	0.7067***
	(0.0051)	(0.0052)
F-statistics	18,894.9	18,821.6
Adj. R^2	0.8338	0.8341
Partial R^2	0.3130	0.3125
Shea's Adj. Partial R^2	0.3125	0.3118
Year FE	Yes	Yes
Industry FE	Yes	Yes
Ent. group		Yes
Age		Yes
Industry × Year		Yes
Observations	240,207	240,207

Robust standard errors, clustered on firms, in parentheses. The estimations correspond to the first stage Eq. (4).
*** p < 0.01, ** p < 0.05, * p < 0.1

Table B8
Estimated effect of the 2001 reform on exit rates, stepwise inclusion of covariates.

Treatment effect		Model		
		(1)	(2)	(3)
ITT	Z_{it}	0.00643	0.00679	0.0100*
		(0.00554)	(0.00547)	(0.00547)
Observations		147,013	147,013	147,013
				Year, Industry, Ent. group, Age, Industry × Year
		Year	Year, Industry	

Robust standard errors, clustered on firms, in parentheses. Exit rates are available only for the years after 1999, see Fig. C3.
*** p < 0.01, ** p < 0.05, * p < 0.1

Table B9
Estimated effect of the 2001 reform on labor productivity in different industries.

Code	Industries	ITT	LATE
1	Mining, manufacture of food and textiles	0.0439	0.0621
		(0.0424)	(0.0600)
	Obs.	5205	5205
2	Manufacture of wood, chemicals, rubber, metals, and machinery	0.0227*	0.0327*
		(0.0120)	(0.0172)
	Obs.	32,604	32,604
3	Manufacture of electrical and transport equipment, and other	0.0112	0.0165
		(0.0235)	(0.0347)
	Obs.	8328	8328
4	Electricity, water, and construction	−0.00528	−0.00754
		(0.0108)	(0.0154)
	Obs.	35,305	35,305
5	Wholesale, retail trade, hotels and restaurants	0.0312***	0.0427***
		(0.00890)	(0.0122)
	Obs.	90,557	90,557
6	Transport, post and telecommunications, and financial intermediation	0.0155	0.0228
		(0.0146)	(0.0215)
	Obs.	18,861	18,861
7	Real estate, research and development	0.00486	0.00708
		(0.0172)	(0.0251)
	Obs.	36,313	36,313
8	Education and health	0.0210	0.0289
		(0.0264)	(0.0361)
	Obs.	7399	7399
9	Sewage disposal, sanitation, and other service activities	−0.0145	−0.0197
		(0.0431)	(0.0587)
	Obs.	5635	5635

Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Rows correspond to separate estimations for each industry. Obs. stands for observations.
*** p < 0.01, ** p < 0.05, * p < 0.1

Table B10
Estimated effect of the 2001 reform on labor productivity using yearly means and clustering standard errors on size.

		Yearly means in labor productivity	Clustering standard errors on size Bandwidth	
			3–50	3–100
ITT	Z_{it}	0.0204***	0.0233***	0.0252***
		(0.00472)	(0.00590)	(0.00572)
	Obs.	14	300,461	313,169
LATE	$\widehat{D}_{it}$		0.0283***	0.0303***
			(0.00723)	(0.00693)
	Obs.		300,461	313,169

In the third column, the data are collapsed to yearly means in labor productivity for each treatment group. Bandwidth refers to the number of employees in a firm. Robust standard errors, clustered on firm size, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD dummy variables from Eqs. (1) and (3). 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 B11
Annual hiring and separation, 1997–2003.

	Hiring rate (mean)		Separation rate (mean)		Hires > 5 workers (percent)		Separations > 5 workers (percent)	
Year	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
1997					1.414	2.028	2.832	1.585
1998	0.230	0.238	0.147	0.170	1.485	2.085	2.697	1.431
1999	0.276	0.259	0.140	0.212	2.546	3.268	5.135	2.121
2000	0.185	0.228	0.170	0.230	0.881	2.468	0.281	1.082
2001	0.176	0.202	0.162	0.212	0.956	1.927	0.488	1.233
2002	0.161	0.182	0.133	0.266	0.945	1.781	0.524	1.165
2003	0.149	0.155	0.130	0.273	0.957	1.486	0.641	1.297

The hiring rate and separation rate are the number of new hires or separations in a firm in year t divided by the total number of employees in $t-1$. The four right-most columns refer to the per-year percent of firms that had more than 5 separations or hires.

Table B12
Estimated effect of the 2001 reform on labor productivity for firms that separated workers in the post-reform period.

	Treatment effect	
	ITT	LATE
Firms with separations	0.0277***	0.0397***
	(0.00467)	(0.00670)
Observations	181,896	181,896

Robust standard errors, clustered on firm size, in parentheses. Coefficients for each treatment effect, ITT, and LATE are in the columns. The full model with all covariates is used for all estimations.
*** p < 0.01, ** p < 0.05, * p < 0.1

Table B13
Reduced sample: mean values before and after the 2001 reform, 1997–2003.

	Treatment group		Control group	
	Pre-reform	Post-reform	Pre-reform	Post-reform
Labor productivity	5.847	6.043	5.939	6.055
	(0.658)	(0.501)	(0.555)	(0.428)
Value added	2899.0	3628.1	6007.5	7088.2
	(4532.1)	(4467.8)	(4792.9)	(4472.7)
Logarithm of value added	7.672	7.960	8.546	8.741
	(0.774)	(0.650)	(0.586)	(0.494)
Revenue	6.932	7.085	7.011	7.120
	(0.814)	(0.744)	(0.780)	(0.702)
Profit	5.951	1.264	5.923	1.183
	(1.218)	(1.274)	(1.201)	(1.240)
Capital-labor ratio	5.333	5.571	5.307	5.457
	(1.160)	(1.155)	(1.040)	(1.022)
Total factor productivity	0.0367	0.133	0.0929	0.124
	(0.502)	(0.388)	(0.428)	(0.363)
Compulsory school	0.295	0.276	0.284	0.259
	(0.237)	(0.224)	(0.182)	(0.166)
Upper secondary school	0.568	0.601	0.586	0.621
	(0.239)	(0.228)	(0.178)	(0.169)
Tertiary education (< 3 years)	0.131	0.117	0.123	0.115
	(0.204)	(0.181)	(0.167)	(0.146)
Tertiary education (≥3 years)	0.0460	0.0428	0.0371	0.0363
	(0.122)	(0.112)	(0.0888)	(0.0840)
Firm size	5.636	5.800	12.77	12.80
	(2.212)	(2.226)	(1.388)	(1.389)
Firm age	9.434	10.13	10.45	10.81
	(4.330)	(3.886)	(3.830)	(3.471)
Enterprise group	0.230	0.211	0.384	0.353
	(0.421)	(0.408)	(0.487)	(0.478)
Observations	22,391	12,754	5,239	3,265

Standard deviation in parentheses. Labor productivity is defined as the natural logarithm of value added per employee. Value added refers to the natural logarithm of value added, revenue is measured as the natural logarithm of sales per number of employees, and profit is measured as the natural logarithm of earnings before income taxes (EBT) divided by sales. The capital-labor ratio is the natural logarithm of book values on machinery, buildings and land per employee. Total factor productivity is defined in Eq. (6), and the educational levels refer to the shares of workers within each firm.

Table B14
Estimated effect of the 2001 reform on wages and alternative measures of wages, revenue and capital.

Treatment effect		Wages	Wages (not scaled by employees)	Revenue (not scaled by employees)	Capital (not scaled by employees)
ITT	Z_{it}	0.0576***	0.0674***	0.0544***	0.0838***
		(0.0112)	(0.0137)	(0.0156)	(0.0226)
	Obs.	43,619	43,619	43,649	43,649
LATE	$\widehat{D}_{it}$	0.0795***	0.0931***	0.0751***	0.116***
		(0.0153)	(0.0193)	(0.0217)	(0.0315)
	Obs.	43,619	43,619	43,649	43,649

Wages are defined as the natural logarithm of the firm's wage sum per employee. Wages (not scaled by employees) is defined as the natural logarithm of the firm's wage sum. Revenue (not scaled by employees) is defined as the natural logarithm of the firm's sales. Capital (not scaled by employees) is defined as the natural logarithm of the firm's book values of machinery, buildings, and land. Annual effects that support the parallel trend assumption can be found in Fig. C5. Robust standard errors, clustered on firms, in parentheses. Each treatment effect, ITT, and LATE are separate estimations. Z_{it} and $\widehat{D}_{it}$ are the corresponding DiD dummy variables from Eqs. (1) and (3). Obs. stands for observations.
*** p < 0.01, ** p < 0.05, * p < 0.1

Appendix C. Figures

Fig. C1. Year-specific estimates of the 2001 reform on level value added per employee. Note: The DiD estimates on the y-axis are the estimated coefficients β_t from Eq. (2). The year 1998 is used as a baseline. The vertical lines refer to a 95% confidence interval.

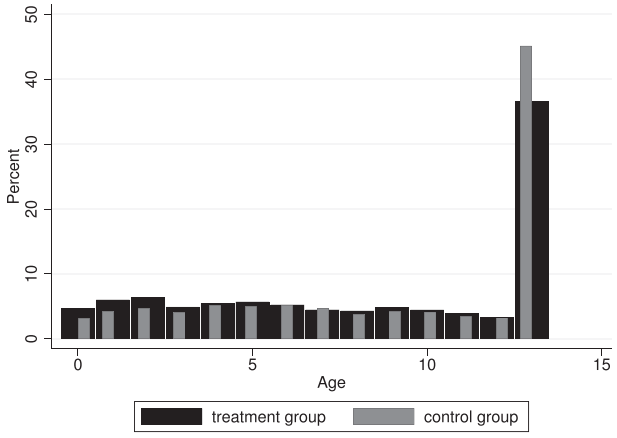


Fig. C2. Distribution of age of firms in the treatment and control groups in 1999. Note: The data are truncated so that all firms born before 1986 receive 1986 as their birth date. The maximum age is therefore 13 years in 1999, hence the skewed distribution.

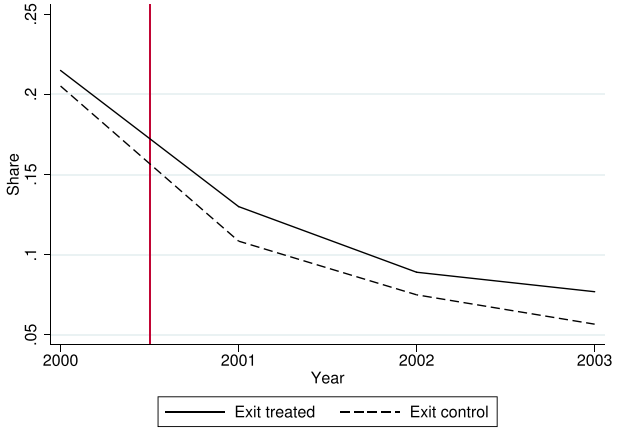
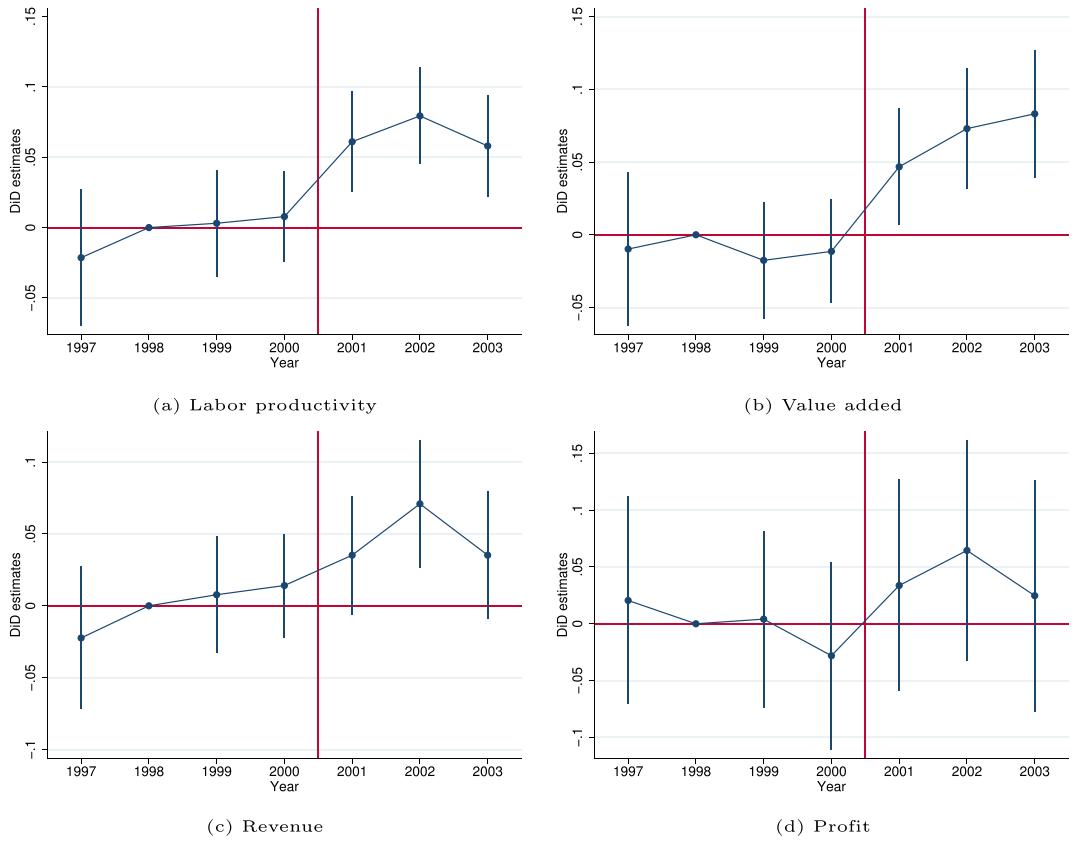


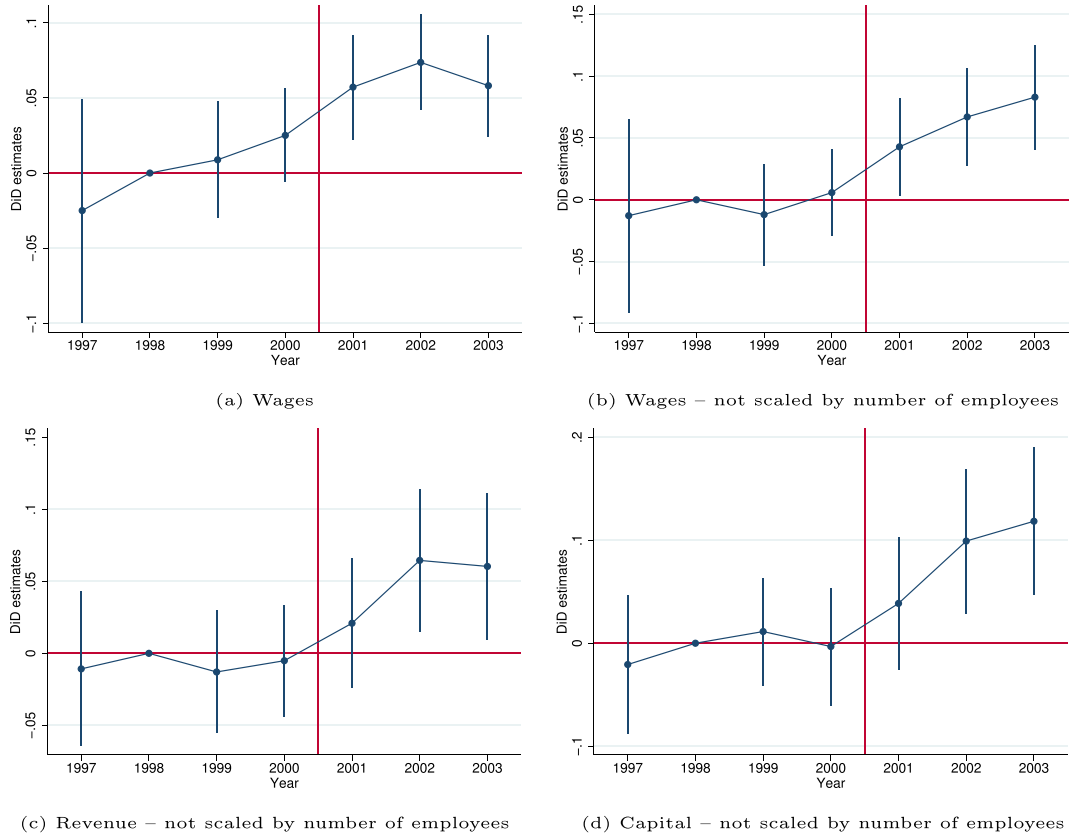
Fig. C3. Exit in treatment and control groups, yearly averages.



(a) Labor productivity (b) Value added

(c) Revenue (d) Profit

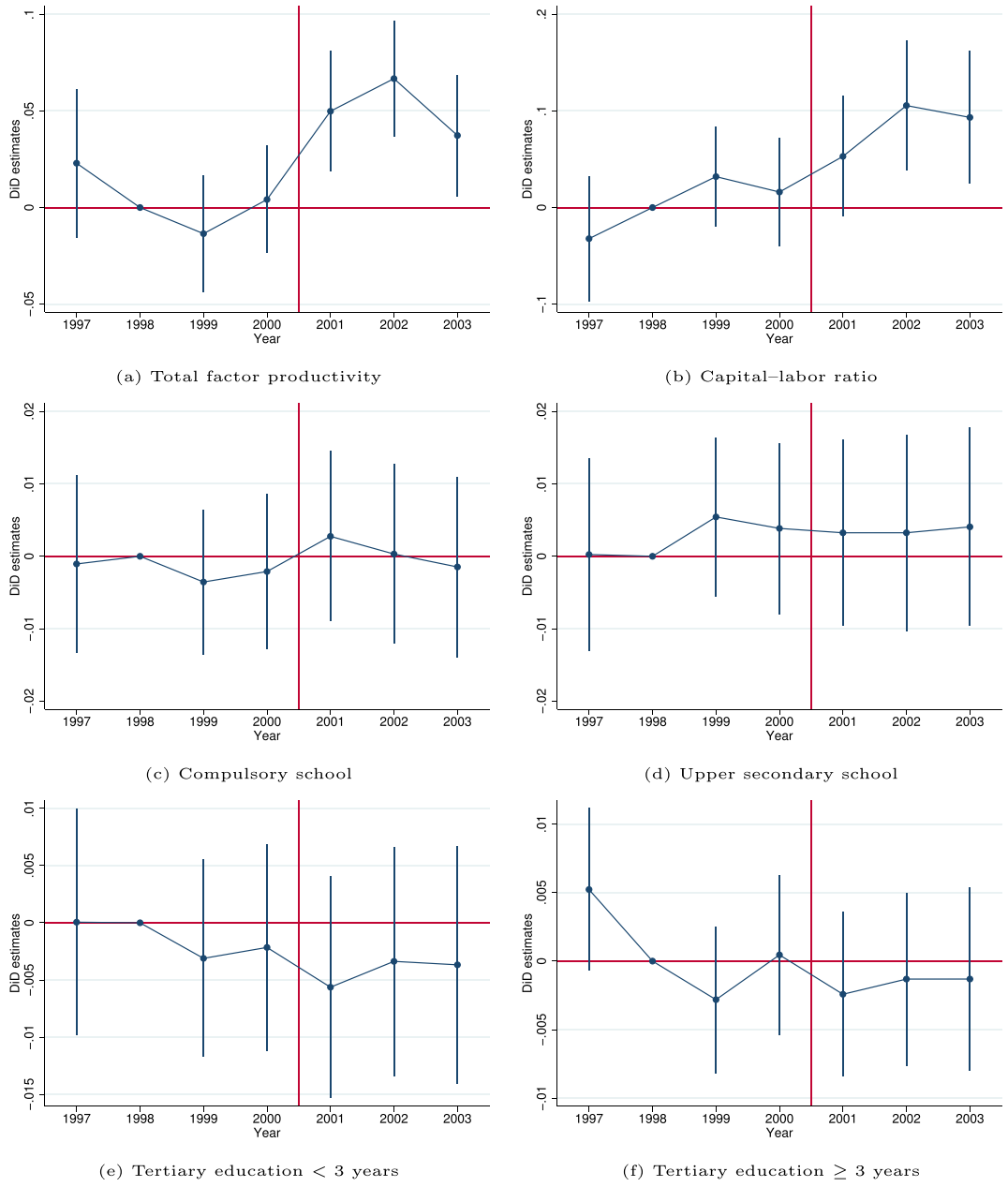
Fig. C4. Year-specific estimates of the 2001 reform on firm performance. Note: The DiD estimates on the y-axis are the estimated coefficients β_t from Eq. (2). The year 1998 is used as a baseline. The vertical lines refer to a 95% confidence interval.



(a) Wages (b) Wages – not scaled by number of employees

(c) Revenue – not scaled by number of employees (d) Capital – not scaled by number of employees

Fig. C5. Year-specific estimates of the 2001 reform on wages and alternative measures of wages, revenue and capital. Note: The DiD estimates on the y-axis are the estimated coefficients β_t from Eq. (2). The year 1998 is used as a baseline. The vertical lines refer to a 95% confidence interval.



(a) Total factor productivity

(b) Capital–labor ratio

(c) Compulsory school

(d) Upper secondary school

(e) Tertiary education < 3 years

(f) Tertiary education ≥ 3 years

Fig. C6. Year-specific estimates of the 2001 reform on firm inputs. Note: The DiD estimates on the y-axis are the estimated coefficients β_t from Eq. (2). The year 1998 is used as a baseline. The vertical lines refer to a 95% confidence interval.

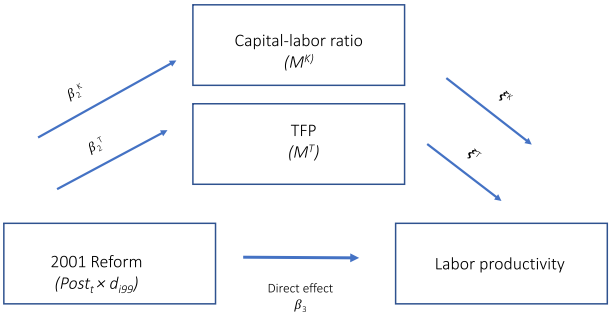


Fig. C7. Path diagram for indirect and direct effects of the 2001 reform.

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