

WORKPLACE COOPERATION AND FIRM PERFORMANCE – EVIDENCE FROM FINLAND

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Abstract

Workplace cooperation policies require firms to inform, consult and include employees in business decision-making. This paper studies the impacts of these cooperation policies on firm performance. Using a Finnish quasi-experimental setting and a differences-in-differences approach, this paper finds that the firm cohort introduced to workplace cooperation was associated with significantly lower worker entries, exits and employment growth compared to a non-covered cohort. No conclusive connection between cooperation and firm productivity or profitability is established, and no clear evidence of firm size manipulation is obtained. The results on worker flow developments support the notion of cooperation obligations increasing firing costs, and indirectly increasing hiring costs through future negotiations. The results, however, could still be capturing group-specific trends or reactions to the 2008 financial crisis. This paper is the first to provide insights on the effects of workplace cooperation in Finland, and through a rich data set and an advantageous setting, contributes to a body of international research where identifying causalities has been challenging.

Introduction

Workplace cooperation is a part of the employee participation framework. Employers, often required by law, inform, negotiate, and co-decide with their employees on firm matters. At the lowest level, the employer can simply disclose information about the firm to the employees. While this doesn't directly engage employees in any decision-making, obtaining information can still help the employees gain influence. The second level is consultation. Consultation includes the employer reserving the employees the opportunity to be heard before making decisions that affect the employees. The strongest levels are co-decision, where the employer and the employees must reach a consensus in their negotiations before decisions can take place, and co-determination, which refers to a situation where the composition of the decision-making body, such as the firm board, includes a considerable share of employee representatives. Employee participation can derive from statutory legislation, collective bargaining, or decisions made by the employer. The arising employee representation system can be direct or indirect, depending on whether employees participate first-hand or through a representative. (Knudsen, 1995)

These policies aim to reduce conflict in the workplace, secure the stance of workers, and ultimately, improve firm performance. The lines of theory behind workplace cooperation and employee participation, however, find potential mechanisms both benefiting and hindering the firm.

On the one hand, participation on the decision-making level can take decision-making power away from the employer, leading to inefficient business decisions (Jensen and Meckling, 1979). On the other hand, participation systems such as works councils can improve communication, worker innovation and increase surplus within the firm (Freeman and Lazear, 1995).

The gains from workplace cooperation and participation don't necessarily convert to larger profits for the firm. Cooperation procedures can lead to higher compensation through increased productivity, added direct costs of negotiation, and indirect costs of potentially losing business opportunities because of delays (Addison et al., 2001; Freeman and Lazear, 1995; *Laki yhteistoiminnasta yrityksissä*, 2007).

As the negotiations and other obligations add a cost to firing, worker outflow is reduced. It can also have an increased indirect cost to hiring, as the employer knows the potential firing process could be costly, thus reducing worker inflow as well, and increasing rigidities in worker flow overall.

Due to the ongoing debate and the large share of firms that typically are covered by cooperation policies, it is essential to evaluate the impacts of the policies and which of the proposed mechanisms apply. The current body of research, however, is mostly focused on evidence from German works councils and co-determination procedures on the board level, with examples of other European countries, such as France and the UK, as well as the US. Causal relationships, especially, have been difficult to identify, and economic outcomes in general have been overlooked in the industrial relations literature.

This paper contributes to the existing body of research on employee participation and cooperation by identifying an advantageous quasi-experimental setting and using a large panel data of over 10 000 Finnish firms from all applicable industries. Thus, the paper will add to the discussion on workplace cooperation and employee participation not only through a rich data set, but through a novel country and a system of implementing cooperation procedures. This will bring a new angle to the discussion from a Nordic labor market perspective. The paper investigates the connection between being subject to the cooperation obligations and the firm performance outcomes expected by the policymaker and the employer side; employment growth, entries and exits, labor productivity, and profitability. The paper takes advantage of the change in the Finnish cooperation legislation that introduced cooperation obligations to a new firm cohort in 2008. The current system requires firms with 20 or more employees to implement various information, negotiation, and co-decision obligations. The coverage was extended in 2008, dropping from previously being set at firms with 30 or more employees. This drop in the coverage threshold enables a differences-in-differences analysis, using firms left outside the legislation as a counterfactual.

The paper finds that firms that were required to follow the procedures after the reform, were associated with statistically significantly smaller firm employment growth and statistically significantly fewer worker entries and exits, thus adding rigidities to labor movements. Firms subject to the obligations were also associated with smaller labor productivity and profitability, though the estimates are not statistically significant. Causal relation cannot be conclusively drawn, however, between workplace cooperation and weaker firm performance, as the estimates vary between model specifications, suggesting other factors being incorrectly attributed to the workplace cooperation obligations. The control group of smaller-sized firms may not be a plausible counterfactual, as the groups of firms evolve somewhat differently prior to the reform. Despite efforts to control for the 2008 financial crisis, the results could be reflecting firms' reactions to the markets. Supporting

cautious interpretation of the regression results, no clear evidence is found of firms trying to actively avoid the legislation by manipulating their firm size.

Previous results on workplace participation and representation have reported both positive and negative dependencies between cooperation and firm performance outcomes. The empirical evidence has been suffering from sample size and endogeneity issues. Endogeneity issues rise when using survey data, and analyzing firms implementing voluntary participation practices, as the individuals are likely to have inherent features affecting the emergence of voluntary policies or survey responses (see e.g. Addison et al., 2001; Addison and Teixeira, 2006; Cable and Fitzroy, 1980; Freeman and Kleiner, 2000; Gurdon and Rai, 1990; Jones et al., 2007; Müller, 2012; Wagner, 2008). Especially the German data sets often include only certain industries or geographical areas, or otherwise limited group of individuals (e.g. Addison et al., 2001; Cable and Fitzroy, 1980; Fitzroy and Kraft, 1987a; Fitzroy and Kraft, 1987b; Fitzroy and Kraft 1993; Fitzroy and Kraft, 2005; Gorton and Schmid, 2004; Müller, 2012). The efforts to establish a causal relation have taken advantage of the change in board representation legislation in Germany in the 1970s, and the size-based regulation in France (Garicano et al. 2016), among others.

This paper, while acknowledging the differences between national frameworks, tackles both the sample size issues and endogeneity problems through a natural experiment and a rich panel data set, with which it is possible to look at firm performance and different factors contributing towards it as a whole.

The rest of this paper is organized as follows. Section 2 presents the theoretical framework for the potential mechanisms in more detail. Section 3 presents the institutional setting by first describing the different cooperation systems, and then focusing on the Finnish case. Section 4 presents empirical strategy and the data available. Section 5 presents the findings and discusses them. Finally, section 6 concludes.

Conceptual framework

The two main theoretical lines of argument formalizing the mechanisms of employee participation and workplace cooperation are property rights theory and participation theory.

According to the property rights theory, co-deciding will take some of the decision-making power away from the asset owner, i.e. the employer. This could lead to inefficient business decisions or delay decision-making (Jensen and Meckling, 1979). The property rights theory suggests that cooperation will only change the distribution of the surplus, and increase the employees' share of the surplus at the expense of the employer's share, or, even worse, decrease the entire surplus and diminish rent for both parties. According to participation theory, on the other hand, cooperation will increase productivity as cooperation reduces conflict and increases communication between the employer and employees, thus boosting employee motivation. This will increase the joint surplus and result in a larger share for both parties (Freeman and Lazear, 1995).

Property rights theory argues that if active participation was truly beneficial for the firm, it would introduce a participation system voluntarily, and thus no policy intervention is needed. While an important argument, there could be market failures standing in the way of firm implementation (Freeman and Lazear, 1995), such as information asymmetry between the employee and the employer. In addition, the transaction costs of implementing the procedures could be too high for the firm to bear (Freeman and Lazear, 1995; Renaud, 2007). Thus, to help mitigate the market failures, the policymaker intervenes by introducing regulation.

Through participation practices, the employees can, for instance, be better informed about the true financial state of the firm. This can encourage them to adjust their effort more efficiently during tough times to help the firm, instead of only relying on the managerial order to work harder (Freeman and Lazear, 1995), improving productivity. Information rights can also decrease opportunistic behavior from the employer's side. This will encourage employees to come up with new ideas to improve production, thus increasing productivity (Smith 1991). Employees can, however, abuse their position of increased influence, just as the employer might in a situation without employee voice. Thus, the productivity increases may not follow directly.

Increased productivity may lead to decreased profits through higher compensation. Another channel for reduced profitability are the time delays that negotiations cause (Freeman and Lazear, 1995). Market opportunities that require immediate response may be lost due to the need to consult the employees first. In addition to the duration of the negotiations, the preparation process can also be costly and require compensation to the employee representatives and any external assistance they may require (*Laki yhteistoiminnasta yrityksissä*, 2007).

In relation to worker entries and exits, Smith (1991) states that codetermination gives employees a way to secure the investment they make in the firm in terms of their human capital input. The exit-voice model suggests that if employees have a channel through which to voice their concerns, they will use it, rather than express their opinion through exiting the firm (Hirschman, 1978). Thus, cooperation practices could reduce exits in the firm. In addition, if the cooperation system includes possibilities for the employees to strengthen their position against dismissals, employees may feel more secure in the firm and ready to commit long-term. The required negotiations before dismissals essentially increase the cost of firing, and indirectly also the cost of hiring in terms of potential future negotiations. As hiring and firing grow costlier, they are decreased. Moreover, as committed employees, who feel they can voice their grievances, are also less eager to leave, quits are reduced. Thus, entries and exits as a whole, are reduced.

Institutional setting

Cooperation systems

In Europe, participation and cooperation systems vary between countries, even though there is also harmonized legislation on the European Community level [2]. In the Nordic countries, collective bargaining agreements are the dominant feature, compared to the independent works councils in Central Europe (Sippola, 2012).

The extent of employee participation is usually dependent on the establishment size, meaning that establishments above a certain worker count are required to implement cooperation obligations. This also varies between countries and systems [3].

Cooperation in Finland

Workplace cooperation in the Finnish context refers to different procedures aiming to enforce employer-employee interaction, exchange of information, and negotiation (Kärkkäinen and Äimälä, 2015).

The main source for the cooperation framework in Finland is the Act on Cooperation within Undertakings or the Cooperation Act (*Laki yhteistoiminnasta yrityksissä*, CA henceforth). [4] The original CA came into effect in 1978, aiming to include employees in shaping organizational changes (Sippola, 2012).

CA mostly consists of information and negotiation obligations. The employer must e.g. inform the employees about the financial state of the firm, wages in the firm, use of temporary and part-time workers, outside sourcing, business transfers, and individual agency work contracts. Negotiations generally have to take place if a planned business decision has effects on the staff. These effects can be the need to reduce staff, or for example the reorganization of tasks. The negotiations can take up to six weeks, depending on the number of employees affected. Other matters to be negotiated over include the staff strategy, training plan, and the use of agency work. The employee representatives do not have to form a permanent body for cooperation procedures, but an assembly can be established individually for each round of negotiations, depending on whom the planned changes concern.

CA is applied regardless of staff or employer union status or collective bargaining agreement coverage over the firm. Collective bargaining agreement coverage does indicate the representative of the staff in cooperation procedures. If the staff in question are covered by a collective bargaining agreement, an agreement-based shop steward acts as the staff representative. If the staff does not have a shop steward, another staff-elected delegate acts as the representative. The coverage of collective bargaining agreements in Finland in 2008 was 74 % in the private sector (Ahtiainen, 2016).

CA is procedural law, meaning that the employer still has the decision-making power, but CA dictates the procedures to be taken ahead of those decisions (Sippola, 2012). The employer and employees are not required to reach a consensus in the negotiations. Similarly, CA does not determine the basis for dismissals, but rather a protocol according to which the employer must carry out the dismissals. If a firm neglects its cooperation obligations in a dismissal process, for e.g. not carrying out long enough negotiations, it can be fined up to 30 000 euros per involved employee.

According to Finnish surveys, the employee respondents feel they don't have any say in the negotiations (Kairinen et al., 2005). The employer respondents' answers reveal that gaps in CA knowledge persist, and mandatory negotiations do not always take place (Eklund et al., 2010). Previous evidence from the Finnish context (Böckerman and Ilmakunnas, 2012) shows that job satisfaction leads to increased productivity. As cooperation is expected to increase worker influence as well as trust and openness between the workers and the employer, productivity increases could be present as a result of improved job satisfaction from the cooperation procedures.

Reform of the Cooperation Act

CA was reformed in 2007-2008. According to the Government Proposal, the most important driver for the reform was that the previous act had been modified so many times that there was a need for a more unified piece of legislation that would be easier to interpret and follow. The economic conditions under which the law was first made in the 1970's had also changed notably, and it was time to update the law to better match the needs of the current working life. (*Hallituksen esitys*, 2006) The most notable change brought by the new CA was the extension of the coverage of the law. Previously, CA covered firms with 30 or more regular employees. The reform dropped the primary coverage threshold from 30 to 20 employees. [5] It was estimated that the coverage extension would reach 2 800 new firms and 66 000 new employees. The new CA was then to include 7 800 firms and 880 000 employees overall. The new CA came into effect in two stages; in June 2007 for firms with 30 or more employees, and in January 2008 for firms with 20-29 employees. Thus, the cohort to which the coverage was extended, was specifically given a transitional period of additional six months to adjust to the new obligations. (*Hallituksen esitys*, 2006)

Certain specific obligations were set to be less strict for firms with 20-29 employees than for firms with 30 or more employees. For example, the firm may be only required to provide the employees with information upon request, compared to informing the employees unprompted. Certain topics are not required to be discussed at all in the new firm cohort, such as personnel funds, or equality strategies. Regarding negotiations, only firms with 30 or more employees are required to negotiate on the principles of recruiting, internal communications, equality plans, drugs testing, camera surveillance, access control monitoring and the principles of email and internet use. These adjustments were made to lighten the burden of the obligations for the smaller firms.

The first steps of the reform were establishing a committee in 2003 to outline the necessary changes. According to the legislator, the aims of the reform were increased firm productivity, efficiency and profitability on the firm side, and increased worker satisfaction on the worker side (*Hallituksen esitys*, 2006). The Federation of Finnish Enterprises opposed the law, stating it will only hinder firm growth, increase hiring threshold, and add bureaucracy (*Suomen Yrittäjät*, 2007). Despite the frequent calls for further renewing workplace cooperation and the strong statements made about the effects on the firm and staff, there is very little evidence of the actual impact in Finland.

Empirical strategy

Data

The data set for this paper is constructed from several databases, all compiled and managed by Statistics Finland. Access to the data is provided by the Research Institute of the Finnish Economy. The primary datasets are the Business Register and Finnish Longitudinal Employer-Employee Data (FLEED). The Business Register is a yearly data set that includes all businesses that are either in the employer or VAT register. The Business Register has information on e.g. the firms' financial statements from the Tax Administration and the Finnish Patent and Registration Office. The employment data in FLEED observes the location and duration of each employment relationship an individual has. This enables pinpointing the number of employees in a company at any given date. As employees can have several contracts in different firms and per year, the number of contracts may not reflect the factual situation of individuals leaving and entering the firms. Even though steps are taken to capture the entries and exits more accurately, the data may still not be an exact representation of reality.

Information on the consolidated corporations in Finland and the average employee characteristics in firms is obtained from separate data sets. The resulting final set is an unbalanced panel data set, which allows the use of fixed effects analysis. Fixed effects capture the firm- and year-specific effects, eliminating factors that are firm- and time invariant, thus helping mitigate omitted variable bias.

Differences-in-differences

The basis for the study is the 2007-2008 CA reform, which saw the primary coverage threshold drop from firms with 30 or more employees to firms with 20 or more employees. The analysis covers the years between 2003 and 2012. To emulate exogenous variation, the paper compares the differences in outcomes over time across the differently regulated groups. This is known as the differences-in-differences (DID henceforth) approach (e.g. Angrist and Pischke, 2015).

DID establishes an estimate of the treatment effect by analyzing the differences that develop between two or more groups over time, if one is subject to treatment and the other is not. This is done by comparing the change over time in the dependent variable among the treatment group units, to the change over time in the dependent variable among the control group units.

The CA reform creates one treatment group and two potential control groups, for which the status (treated/non-treated) stays the same over the period of interest. The treatment group includes firms with 20-29 employees, i.e. firms who, after the extension of the CA coverage, are now subject to the treatment of CA obligations. The first control group consists of firms with 10-19 employees. This group was not treated prior to the reform and remains untreated after the reform. The second control group includes firms with 30-39 employees. This group was subject to the treatment before and after the reform. Due to the second control group, firms with 30-39 employees, also going through certain changes in the CA reform, the first control group of firms with 10-19 employees, form a cleaner base for comparison. Therefore, the analysis is conducted using the first control group of small firms as the base level.

The identifying assumption for DID is the parallel trends assumption. For the DID setting to be valid, it is necessary to assume that in absence of the treatment, both treatment and control groups would have developed similarly over time. If this is not the case, it is not possible to identify the effect of the treatment, and the analysis captures the individuals inherently evolving differently instead.

The DID provides an intuitive approach for estimating the connection between cooperation procedures and firm performance. However, the parallel trends assumption is a strong one. In this case, the assumption is that firms with 10-19 staff and firms with 20-29 staff evolve similarly without the CA obligations. Recognizing the different dynamics between firms of different sizes, this proves to be a difficult assumption to satisfy.

Firms are assigned into treatment and control groups based on their employee headcount in January 2008, the time when the CA obligations came into effect for the new cohort. As mentioned above, there is evidence from surveys suggesting not all firms comply with CA in Finland. As it cannot be observed whether a firm is, in fact, abiding to the law, the analysis identifies the intention-to-treat effect (ITT henceforth), meaning the setting is based on initial assignment to treatment, rather than observing the actual treatment taking place in firms. There are two types of ITT elements in the setting at hand. Firstly, a firm may not comply with CA even if it is above the coverage threshold. Firms under the threshold are also free to engage in CA procedures if they wish. [6] Secondly, there can be crossover firms in the data, moving from a treatment group to a control group, or vice versa. The ITT nature must be kept in mind when interpreting the results; The actual treatment effect cannot be observed, only the estimates for the entire cohort of firms that were assigned to the treatment. [7] As there is no data on the compliance rate of firms, it is difficult to obtain the precise treatment effect

from the ITT estimates. This is especially important, as the observation period coincides with the 2008 financial crisis, and there is a notable share of firms laying off staff and crossing over to another treatment group.

The functional form for the DID analysis is

$$Y_i = \beta_0 + \beta_1(TREAT * POST) + \beta_2TREAT + \beta_3POST + \gamma_jX_j + \varepsilon,$$

where Y_i is the outcome, β_0 is the intercept or constant, β_1 is the differences-in-differences estimator, $TREAT$ is the dummy variable for treatment group status, $POST$ is the pre-post policy reform dummy that gets the value 1 when year is between 2008 and 2012, and value 0, when year is between 2003 and 2007, β_2 is the coefficient for belonging to a certain treatment group, X_j is the vector for different controls added to the equation, γ_j is the coefficient for the different controls, and ε is the error term.

The independent variable based on which firms are assigned into treatment and control groups is the headcount of employees in the firm in the beginning of January 2008. The CA defines the coverage to be limited to firms with 20 or more regular employees. [8] The number that determines whether CA will be implemented should be the headcount of all employees, regardless whether they are full- or part-time. Temporary workers must also be included, if they are e.g. substituting for a regular employee. Regular employees refer to those engaging in the regular operations of the firm. From FLEED, the headcount of employees in a firm can be observed at any given time. The basis of their employment, i.e. whether they are e.g. substituting for someone, is not observed. In addition, not observable is whether an employed person is taking part in the operations, or are they, for example, on parental leave, study leave or performing military duty. [9] The dependent variables of interest Y_i in this analysis are employment growth, worker entries and exits, labor productivity, and profitability. For the firm employment growth variable, the number of employees in the firm in observation year and the previous year are looked at. To further examine the components of employment change in a firm, entries and exits to and from the firm are investigated separately. The entry and exit rate variables are constructed separately from the FLEED employment relationship data, meaning they are not directly decomposed from the employment growth variable. The entry rates for year T_0 are constructed as the ratio between the number of new employment relationships starting in year T_0 and the number of employees on the last day of T_{-1} . The exit rate is constructed as the ratio between ending employment relationships in T_0 and the number of employees at the end of year T_{-1} .

The productivity variable is defined as the ratio of firm output to labor input. Firm output is obtained directly from the financial statement by using the value added. Labor input is measured by the number of workers, converted into full-time workers.

To measure profitability, firm net profit shared by the firm revenue reported in the financial statement is used. [10]

The CA reform coincides with the 2008 financial crisis. To control for this, time fixed effects are added. As the financial crisis had a different effect on different industries, also an interaction between individual years and individual industries is added to further control the influence of the financial crisis. In addition, an interaction of the quadratic firm size and individual years is included to account for different size firms developing differently. Firm-specific controls are also included. These controls are the average number of years of education among the employees, the average age of the employees, the firm-specific experience per employee, age of the firm, and independency of the firm. To help mitigate omitted time-invariant variables, fixed effects analysis is implemented also on the firm level.

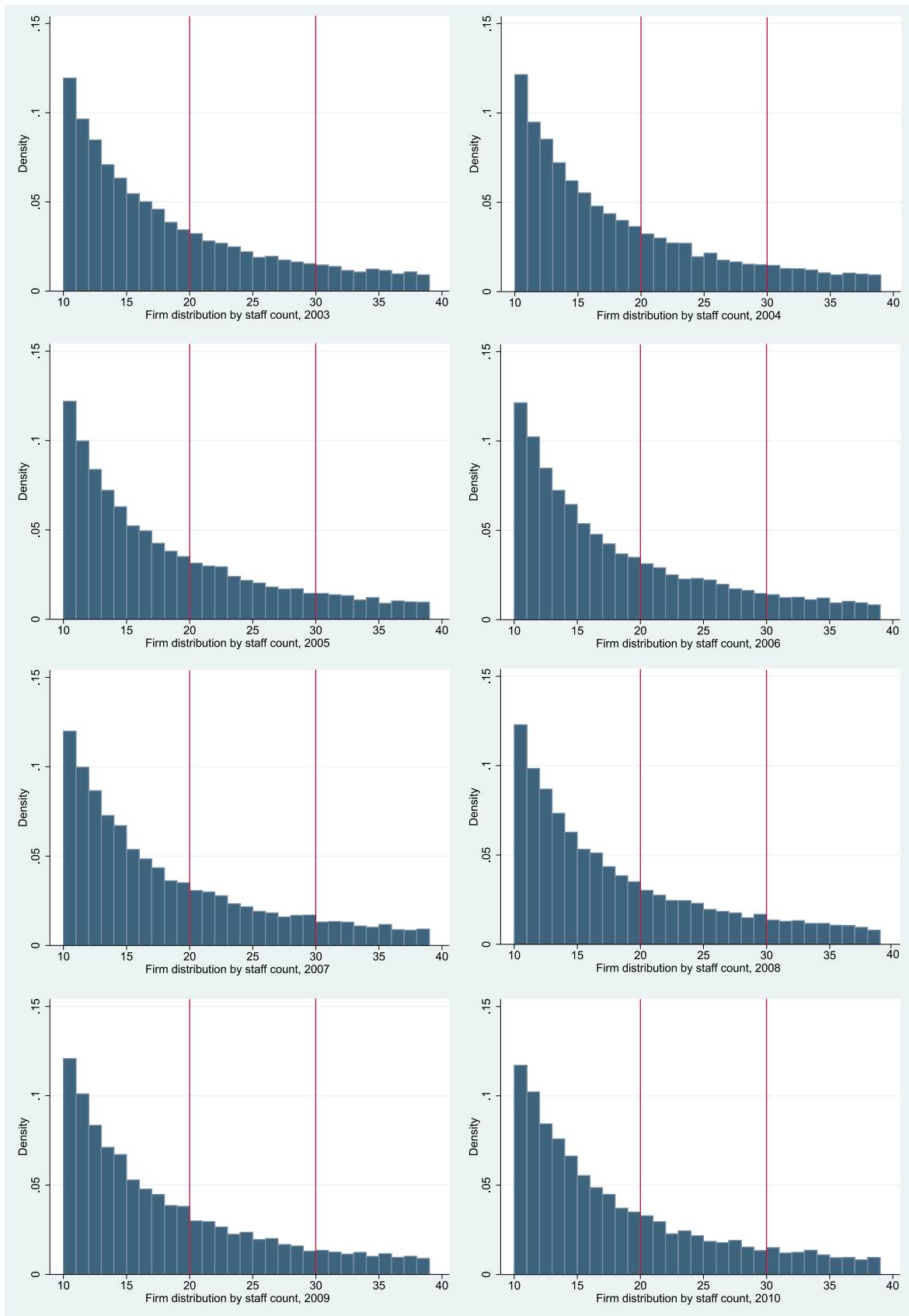
To prevent underestimating the standard errors by including observations from the same firm multiple times, standard errors are clustered to the firm level. As a robustness check, a placebo test on the timing of the policy reform is conducted. (Bertrand et al. 2004)

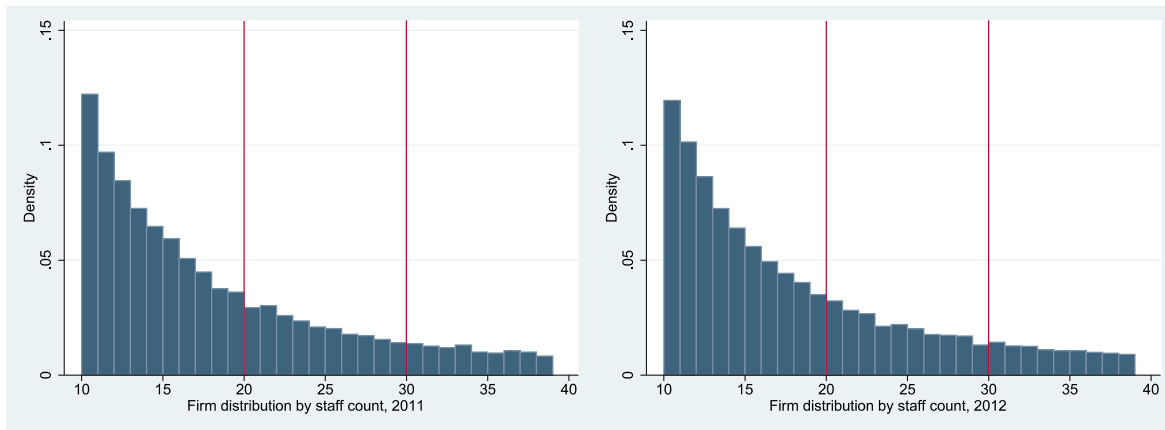
Findings

Firm size distribution

An obvious indicator of CA affecting the firm size is the size distribution of firms. If the legislation was truly harming the firm more than benefiting, the firm would try to avoid crossing the 20-staff threshold over to the coverage of the CA. This would be seen in the density distribution of firms as bunching right below the threshold.

First step is to look at the density of the firms per employee count to see if there is any clear discontinuity around the threshold. Graphs 1-10 present histograms of the density of the firms per





Graphs 1-10, Distribution of firms by worker headcount, years 2003-2012

staff count through years 2003-2012. The deep red line indicates the new CA coverage threshold, coming into effect in 2008, and the lighter red line represents the old, existing coverage threshold in place throughout the observation period. As the histograms show, no clear discontinuity or bunching seems to be visible. In 2008, 2009 and 2011, there seems to be a slight bump below 20, but the bumps do not persist over time. In addition, for the 30-staff threshold, some potential bunching is visible in 2007 and 2008, but not in other years. Thus, based on the histograms, no conclusion of bunching can be drawn.

The lack of visible manipulation in the density graphs would at first suggest that firm growth was not hindered at the threshold. The firms are, however, able to shift employees through restructurings. The CA is based on the firm staff count, so basing employees into different plants does not affect the liability. A firm can undergo a restructuring, however, to gain a new business ID under which to shift employees. This may explain why there is no clear bunching directly at the 20-staff threshold, as firms usually have to move an entire business unit during a restructuring, thus ending up with varying employee counts in both firms. Appendix A shows that the distribution of firms created through restructurings has shifted towards the smaller firm sizes, meaning that firms chop off smaller pieces of firms, creating more new small firms than before. This could be interpreted as the result of already relatively small firms under 20 staff shaving off employees to a new, small firm. The bunching is not evident because for a restructuring to qualify e.g. tax benefit -wise, an entire business unit is required to be separated. Thus, it is more difficult for firms just at the threshold only to lose a couple of employees. However, due to the limitations in the restructurings data, it is not possible to draw conclusions on whether the bunching is softened through restructurings.

Regression analysis

	Full sample	Subsample	
		Firms with 10-19 staff	Firms with 20-29 staff
Average worker age	39.708 (5.9396)	39.553 (6.1047)	39.408 (5.8791)
Firm-specific experience in months	69.019 (49.7114)	67.111 (50.5589)	68.696 (49.3575)
Years of education	12.479 (1.4583)	12.324 (1.4722)	12.454 (1.4359)
Firm employment	0.065 (1.6184)	0.203 (0.8099)	0.191 (0.9601)
Worker inflow rate	0.808 (1.6184)	0.830 (1.5039)	0.887 (1.9013)
Worker outflow rate	0.687 (1.3067)	0.657 (1.2543)	0.701 (1.5390)
Labor productivity	52 484.97 (128 212.3)	53 928.66 (64716.75)	54 090.8 (113 100.1)
Profitability	-1.330 (104.4327)	-1.379 (108.0098)	-0.071 (3.0327)
N	14 445	7850	3010

Standard deviation in parentheses.

Table 1: Comparison of subsample means

Table 1 presents a comparison of the full sample, and the treatment and control group. The values are initial values before the policy reform. The values between the full sample and subsamples should be similar to ensure there is no inherent differences between the groups that might be contributing towards differing outcomes. Based on the comparison, the groups of interest, the treatment group of 20-29 employees and the control group with 10-19 employees, do not notably differ in firm characteristics. The average age, average experience at the firm, and average years of education are very close to each other. The dependent variable outcomes differ somewhat, especially with profitability. This, however, is not a concern with DID, as the approach focuses on changes rather than absolute levels. The control group has the largest number of observations, at just under 8 000 firms. The treatment group consists of roughly 3 000 firms.

	(1)	(2)	(3)	(4)	(5)
Firm employment	-0.045*** (0.0103)	-0.047*** (0.0104)	-0.029*** (0.0114)	-0.039*** (0.0114)	-0.039*** (0.0114)
Worker inflow rate	-0.073*** (0.0210)	-0.076*** (0.0210)	-0.058*** (0.0214)	-0.069*** (0.0220)	-0.071*** (0.0220)
Worker outflow rate	-0.044*** (0.0194)	-0.041** (0.0194)	-0.043** (0.0188)	-0.051*** (0.0192)	-0.052*** (0.0192)
Labor productivity	-954.9 (1644.4)	-1225.5 (1640.5)	-120.5 (1122.5)	-576.3 (1150.9)	-566.2 (1167.5)
Profitability	-0.245 (0.608)	-0.223 (0.586)	-0.470 (0.616)	-0.353 (0.606)	-0.351 (0.614)
Year fixed effects		x	x	x	x
Company fixed effects			x	x	x
Quadratic firm size control				x	x
Firm characteristics					x
N	115 497	115 497	115 497	115 497	115 497

Standard errors are clustered to the firm level, reported in parentheses. *, **, *** indicate significance at the 90 %, 95 %, and 99 % level, respectively.

Table 2: DID estimator coefficients for firm performance outcomes

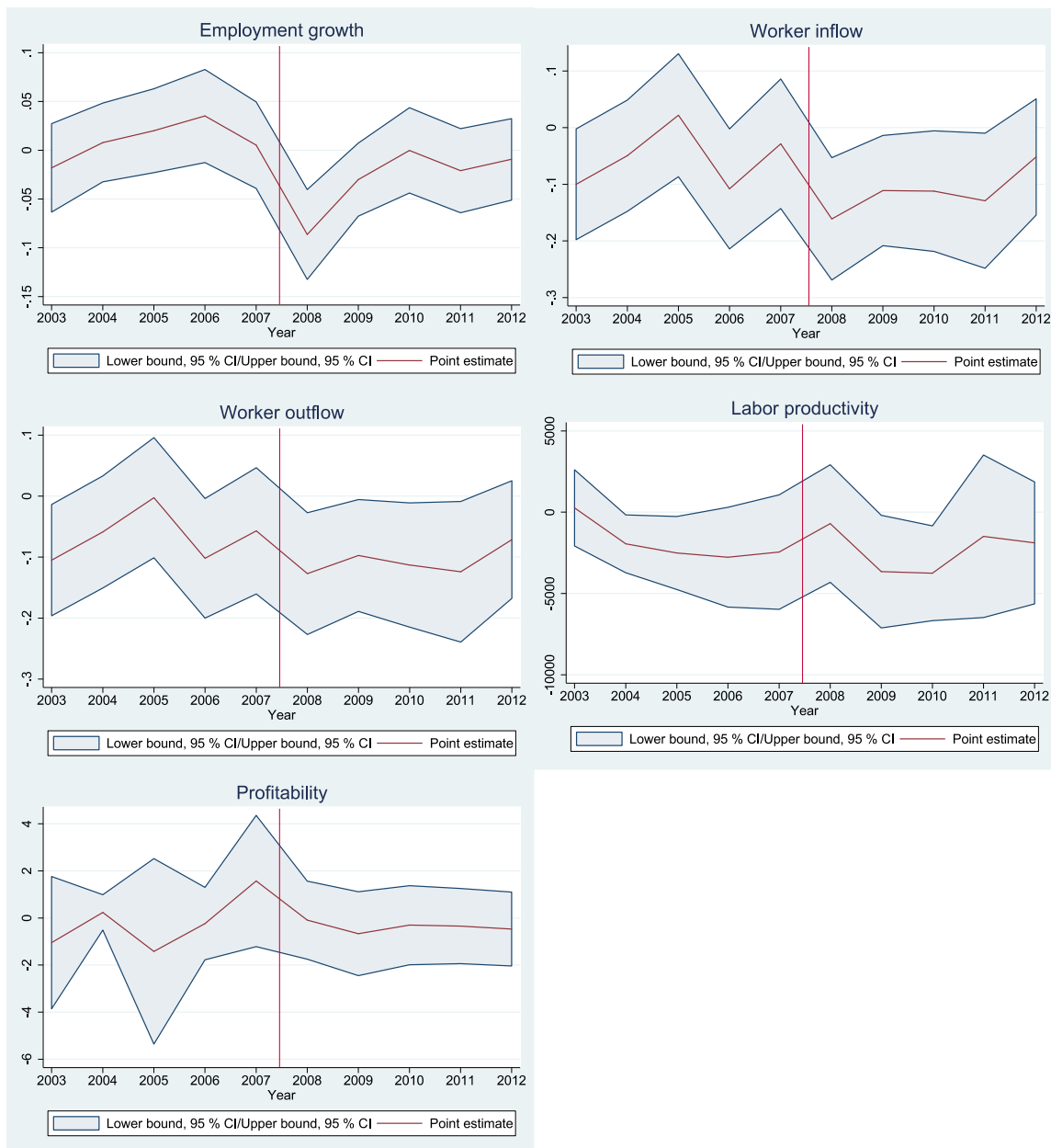
Table 2 presents the DID estimators for the regression models, comparing the treatment group of firms with 20-29 employees, to the control group of firms with 10-19 employees. The DID estimator is the coefficient for the interaction term between the treatment group and reform dummy, as presented above in the Empirical strategy section. There are five different specifications to the model, used as robustness checks, that add different controls to each model. Model 1 in column (1) is an OLS regression and includes no controls. Model 2 in column (2) adds year fixed effects, model 3 in column (3) also adds company fixed effects, model 4 in column (4) includes additional quadratic firm size control, and model 5 in column (5) finally adds additional firm characteristics. The coefficients presented describe the ITT for the firms with 20-29 workers, after the policy reform, compared to the firms with 10-19 workers. The firm employment growth variable, worker inflow rate, and worker outflow rate are measured in percentage points. Thus, for example, according to model 1, the inflow rate of workers in firms with 20-29 workers is roughly 7.3 percentage points lower than in firms with 10-19 workers, after the policy reform. The coefficient for profitability is also measured in percentage points. Labor productivity is not as easily interpreted but is on its own scale as the ratio of firm output and labor input. The detailed regression tables are presented in Appendix B.

For the treatment group, the DID estimators for firm employment development, worker inflow rate, and worker outflow rate are negative and statistically significant, yielding coefficients of -3 - -5

percentage points, -5 - -7 percentage points, and -4 - -5 percentage points, respectively. This suggests that firms subject to cooperation obligations experience slower employment growth than firms not subject to the legislation, post policy reform. These observations support the theory that CA obligations add to the cost of separations, thus decreasing them. The obligations also add an indirect cost to hiring, reducing entries. It must be noted that the estimates vary between models, which implies there could be omitted variable bias taking place. The deviation between the fixed effects model and OLS could also be a sign of a problematic unbalanced panel. There seems to be no relevant differences between industries.

From the regression table, it is seen that the DID estimators for productivity and profitability change notably from model to model, while remaining negative for all the specifications. This suggests the setting isn't capturing the true effect of the assignment on productivity and profitability but including unobserved factors in the estimate. The estimates are also statistically insignificant. Therefore, we cannot conclusively establish ITT on productivity or profitability. In addition, the models have low explanatory power across specifications. This is not entirely unexpected, as the examined outcomes are a result of several complex factors, of which cooperation procedures may not be the most crucial one.

As a robustness check, the reform year can be replaced with a different one. This placebo setting tests whether the results were truly related to the policy reform or if they would have arisen from the data in any case. By replacing the reform year 2008 with 2005, the regressions yield insignificant DID estimators for all the outcomes, except labor productivity. The coefficients are also close to zero, suggesting there is little evidence of bias in the original setting, apart from productivity and profitability coefficients. Detailed placebo test DID estimators are presented in Appendix C.



Figures 11-15. Comparison of treatment and control group evolution

While the regression results with pooled pre- and post-policy reform periods suggest a statistically significant reduction in firm entries and exits for firms above the 20-staff threshold when the new CA came into effect, the parallel trends assumption has to be satisfied in order to draw causal inference. Figures 11-15 graphically present a year-by-year regression of the treatment group of firms with 20-29 staff compared to the control group of firms with 10-19 staff. The shaded area of the graph illustrates the confidence interval on a 95 % level. These figures allow for examination of the

evolution of the two groups. The parallel trends assumption requires that the control and treatment groups must evolve similarly in absence of the treatment. It is not possible to obtain absolute information on the satisfaction of the parallel trends assumption, as it is not possible to know how the treatment group would have evolved without the assignment treatment. The closest approximation of evaluating the assumption is to examine the pre-policy years and the evolution of the groups prior to the intervention. From the graphical illustrations, it is visible that the 20-29 staff group evolves somewhat differently from the group of small firms already before the CA reform. Therefore, conclusive causal interpretation based on the DID estimations is not possible.

Discussion

The above results suggest that firms above the coverage threshold are associated with lower employment growth, consisting of lower entries and lower exits. Profitability and labor productivity seem to be lower, but the estimates are not precise. While employment growth is estimated to be lower in the regression analysis, the histograms of the firm size distribution show no evidence of the bunching the employer side was warning about. More likely, perhaps, is that firms don't necessarily hold back aggressively at the threshold, but when under the coverage, they become more cautious of hiring and firing because of the added cost of negotiations. Firms are reluctant to hire more employees as there is an added indirect cost of having to go through lengthy negotiations if the employee is eventually let go. Firms' cautious hiring could also be a sign of more selective recruiting, choosing 'quality over quantity', which could in turn enhance firm performance. It could also be feasible that firms that go over the threshold, already have some sort of cooperation practices in place, thus not bearing the initial setup costs.

The decreased exits can be interpreted in two ways. On the one hand, firing is costlier to firms that have to go through cooperation procedures before layoffs. Thus, firms may not be letting enough employees go in relation to the economic situation or for the firm to regain profitability. This is hindering the firm's performance. On the other hand, reduced exits may be a sign that the cooperation negotiations are working, and new solutions and innovations arise, allowing for cost reductions somewhere else besides staff expenditure. From this perspective, the CA is working in both employees' and employers' benefit. To confirm these views, more precise estimates are needed of the profitability and productivity of the firms. For example, if exits are reduced but the firm profitability is lower than in the control group, it would suggest that the workers that were spared from layoffs are

not contributing towards profitability. If exits are reduced and profitability is higher, it would suggest that the firm was able to come up with more productive solutions without losing staff.

The data only allows examination of exits as a whole, and separating firings and voluntary quits is not possible. The observation of the two components might bring more light to the issue; Are employees more committed to the firm, thus demonstrating that the employee voice is working, or is the firm just bearing the increased cost of firing.

In the light of the analysis, the firm side objectives stated in the Government proposal (*Hallituksenesitys*, 2006), increased profitability and productivity, yield inconclusive results. The estimates are negative, but no causal inference can be drawn. The reduced exits can be seen both as a merit as well as a failure. If the reduced exits are due to innovative and productive solutions found, allowing less staff to be let go, CA seems to have succeeded. However, if the reduced exits are due to firms being unable to react to market signals, CA seems to have created a market failure.

The unclear implications could be due to the CA being largely procedural law (Sippola 2012). The Finnish cooperation system, for the most part, does not include workers in the actual decision-making. Employers are free to make any business decisions they wish, but they have to consult the workers beforehand. As the prior qualitative surveys done on the subject indicate, CA is seen as a “necessary evil” before dismissals, rather than a tool for creating innovative solutions that the two parties could not have done on their own. Commentators have argued that CA focuses more on the procedures of dismissals and not in the true development of cooperation, and that there is no true involvement from workers (*STTK*, 2017a; *STTK*, 2017b).

Overall, the previous empirical literature on cooperation and employee participation is mixed in terms of the direction of the impact. There are studies suggesting both positive and negative profitability and productivity developments. The decrease in labor fluctuation and increase in labor rigidities suggested in this paper are in line with previous results and the intuitive mechanism behind cooperation obligations and labor movement outcomes.

The strength of the 2007-2008 reform can also be debated. The change in the coverage of the CA was not total. Even though the primary threshold for applying the law was lowered to 20, some obligations were still left only to be applied at the 30-staff threshold, to lighten the administrative burden of small

firms. In addition, even prior to 2008, for very vast dismissals, firms with 20 or more employees had to follow certain EU-based CA procedures.

To isolate the effect of the cooperation activities, there cannot be any other policy reforms affecting the treatment and control groups. The EU database LABREF [11] as well as Finnish database Finlex [12] were used to gain information on potential confounding effects. While no notable policy reforms were found, there could still be something in place either from the regulatory side, and certainly from the side of the 2008 financial crisis. Reduced hiring is consistent with an economic downturn and layoffs are more frequent, even though voluntary quits are less likely. Business cycles may affect not only the firms but the entire treatment altogether. During an economic downturn and mass layoffs, the policy is costlier as more negotiations take place, but during a boom the cooperational features may be given room. The costliest CA procedures take place when the firm might already be facing notable changes in operations. Based on surveys and case law, it seems that dealing with a dismissal situation is when a firm is likely to acknowledge its obligations. Information obligations are easily ignored and less litigated by the employee side. Thus, the impact of CA is more visible in economic downturns when firms are, in fact, forced to start implementing the CA procedures. This makes separating the impacts of the business cycle and the treatment itself more challenging.

The treatment is also suggested to be far from homogenous, not only in relation to time and economic cycle, but in relation to the nature of employment relations the participatory system handles, and the relationship between the employer and the employee (Backes-Gellner et al., 2015; Jirjahn, 2009). When the primary purpose of employer-employee interaction is to solve conflicts, the cooperation will have mostly distributive effects, meaning that the parties are more likely to only try to secure their stand in the matter and their share of the rent pie. If employment relations are more cooperative, looking for innovations, there is a bigger chance to increase the joint surplus. The success of the cooperation procedures depends on the employer's and employees' incentives and individual efforts. While the legal requirements may be formally fulfilled, the objective of CA may not be.

It is good to point out once more that the analysis conducted was limited to a specific group of firms. Rather than capturing the true treatment effect of the CA procedures, it describes the outcomes for the cohort that was freshly introduced to the obligations in 2008. In addition, there is a slight difference between the CA definition of an employee and how employees are presented in the data. Because specific tasks or the basis for employment cannot be observed, there could be measurement error in the firm staff count variable. This, however, could be considered unsystematic noise in the

data. As mentioned above, the estimates can only identify the intention-to-treat effect, not the actual average treatment effect. There is evidence that not all firms assigned to the treatment follow the legislation, and there could be firms below the threshold taking the treatment. A notable share of firms is moving between treatment and control groups over the observation period. Most importantly, when interpreting the results, it must be acknowledged that the control group may not offer a plausible counterfactual. The firms in the control group evolve differently than the treatment group, thus making causal relationships difficult to identify.

Overall, no radical developments in firm performance can be attributed to the implementation of CA in 2008. It's possible that the impacts captured by the analysis are result of different-sized firms reacting differently to the 2008 crisis, rather than the CA reform.

Conclusions

This paper is the first to offer quantitative evidence on the connection between Finnish workplace cooperation practices and firm performance. The research finds that employment growth, worker entries and worker exits were reduced among the firms that were freshly introduced to the obligations in 2008. This is in keeping with the notion of lengthy cooperation negotiations adding to the costs of hiring and firing. Causal inference cannot be drawn conclusively, as the DID setting could be capturing different-sized firms reacting differently to the 2008 financial crisis. Even prior to the crisis, the control group of firms with 10-19 staff evolves somewhat differently than the treatment group of firms with 20-29 staff. The size distribution of the firms does not reveal any visible bunching at the threshold, but the hindered growth could be spread more evenly in the treatment group than just at the cut-off.

The results help understand the mechanisms between cooperation obligations and firm reactions. The theoretical arguments behind the potential impacts are conflicting, but in the Finnish case, the preliminary data point towards cooperation policies slowing employment growth and adding potential rigidities to labor movements. Without any productivity increases, the slower employment growth is difficult to justify from the policymaker's viewpoint. The productivity and profitability effects are yet to be verified in the Finnish context, just as they have been difficult to identify in other systems as well. It is tempting to find explanations for the differing results between Finland and e.g. Germany in the different cooperation systems. The German system, for example, requires consensus between the employer and employee more widely than Finland. Could this be the reason the Finnish, mostly

procedural framework, does not have bite? It is also essential to distinguish between the reasons for reduced exits, for example. They could be a result of a successful negotiation round that yields alternative measures to laying workers off. They could also be evidence of the employer not being able to efficiently react to economic situations and being left with non-productive workers. In addition, observing the difference between firings and voluntary exits would offer more insight into the changes in labor turnover. The CA reform may have also affected the type of employment firms choose; there could be more use of agency work or temporary contracts in attempt to avoid the regularity condition in CA legislation. It is also good to note that due to the nature of the legislation and the fine associated with failing to follow the obligations, it will be challenging to estimate the true rate of compliance in the Finnish context.

The persistent public discussion around workplace cooperation and CA suggests that there is need for evidence of the effects of the policies. From a policy-point of view, the CA seems to not have reached its objectives, and should be revised for a more thorough analysis of its effects on labor market rigidities and whether potential increases in job satisfaction or worker influence justify the added costs for the firm and labor market. Refining the policy to something more effective requires evidence-based proposals and thus, more research.

Endnotes

[1] These outcomes include indicators such as labor productivity, total factor productivity, profitability, labor turnover, innovation, wages, investment, profit sharing, flexible working time, and R&D developments. For a review, see e.g. Doucouliagos, 1995; Frege, 2002; Renaud, 2007.

[2] European Community and Council directives have shaped the national legislation. The most central pieces concern European works councils, collective redundancies, business transfers and informing and consulting employees.

[3] For example, in France, the threshold for works councils is 50 workers (Askenazy & Fairris, 2010). In Denmark, the threshold is 35 workers. In Sweden, there is no separate size threshold, meaning all firms must abide to the cooperation regulations. Norway has a threshold of 100 workers for cooperation committees. In Germany, firms with 5 or more workers are required to have a works council, though the employees must initiate the set-up. (Koskinen and Lehisto, 2007)

[4] In addition to CA, which can be perceived as the main piece of legislation regarding workplace cooperation, there are also other acts specifically for e.g. employee board representation, workplace

cooperation regarding health and safety issues, cooperation for consolidated corporations and European firms, and cooperation practices in public sector organizations and municipalities.

[5] Already in 2005, an exception to the 30-staff coverage threshold was introduced: If a firm with more than 20 employees wanted to dismiss ten or more employees, i.e. a large share of its staff, it was obligated to follow some of the CA dismissals procedures. This was based on the collective redundancies directive from the European Council.

[6] Another, potentially cleaner setting would be to look at non-profit organizations and other communities with economic operations, such as cultural institutions, labor organizations, sports clubs and foundations. In the 2007/2008 reform, these organizations were also included under the coverage in the new CA, within the employee count limits. As they were universally uncovered regardless of size, they would have less problems with organizations potentially already preparing to crossover and implementing certain procedures. However, as these organizations often do not aim for profit and their output is difficult to measure, the results could prove to be less than meaningful.

[7] Another way to assign firms into treatment and control groups would be to only include firms that do not cross over from one group to another. This might seem to help get closer to the true treatment effect, as firms are either following or not following regulations for the entire period. This however, holds an endogeneity problem; Firms that stay in their group, i.e. do not grow may be of a certain type. Thus, the sample is not random.

[8] These employees mean individuals who are employed by the firm, but not for example the CEO of a limited company, as the CEO is a firm body rather than someone covered by an employment contract.

[9] This measurement error prevents the use of the regression discontinuity design (RDD). RDD takes advantage of the size-based regulation and examines individuals directly around the coverage cut-off. While CA has the size threshold needed for RDD, the measurement error in the running variable muddles the observations at the cut-off so that sharp separation of treatment and control group is not possible. The DID approach does not suffer from the measurement error as much as RDD, as the groups are larger, and the measurement error “fades in” with the group the further away from the threshold we move. While RDD would provide a more plausible counterfactual, with the data at hand, it is not a feasible tool.

[10] The financial statement information is reported under the calendar year during which the firm's fiscal year ended. As firms can decide the cycle of their fiscal year, it can differ from the calendar year. This means that for the first post-reform year, the financial statement may not be capturing many months of actual CA implementation. For example, if a firm ends its fiscal year in March, the financial

statement reported under 2008 only includes three months from 2008 (January, February, and March) and nine months from 2007. However, majority of the firms use the calendar year as their fiscal year. Years are also pooled into pre- and post-reform periods, so the first year's potential measurement error blends in with the rest of the post-reform years.

[11] <https://webgate.ec.europa.eu/labref/public/>

[12] <https://www.finlex.fi/en/>

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Appendix A.

Table A.1. Restructurings

Emergence of new firms via restructurings				
Year	All firms	Firms with <20 staff	Firms with 20- 29 staff	Firms with 30- 39 staff
2002	870	753	33	15
2003	886	774	31	14
2004	1047	911	29	18
2005	1123	961	30	20
2008	1588	1430	49	19
2012	1364	1232	34	20

Appendix B. Regression tables

Table B.1. Employment growth

Regression table: Employment growth					
	(1)	(2)	(3)	(4)	(5)
Firms with 20-29 staff	-0.003 (0.010)	-0.001 (0.010)	0 (.)	0 (.)	0 (.)
Firms with 30-39 staff	0.010 (0.018)	0.012 (0.018)	0 (.)	0 (.)	0 (.)
Post CA reform	-0.060*** (0.006)	-0.057*** (0.016)	-0.126*** (0.011)	-0.181*** (0.059)	-0.062 (0.060)
Firms with 20-29 staff * Post CA reform	-0.045*** (0.010)	-0.047*** (0.010)	-0.029*** (0.011)	-0.039*** (0.011)	-0.039*** (0.011)
Firms with 30-39 staff * Post CA reform	-0.071*** (0.018)	-0.072*** (0.018)	-0.055*** (0.021)	-0.081*** (0.022)	-0.085*** (0.022)
Year fixed effects, 2003		0.007 (0.016)	0.013 (0.011)	0.136* (0.075)	0.303*** (0.076)
Year fixed effects, 2004		0.009 (0.016)	0.005 (0.010)	0.100 (0.097)	0.277*** (0.099)
Year fixed effects, 2005		-0.003 (0.015)	-0.016* (0.009)	0.100 (0.089)	0.146* (0.088)
Year fixed effects, 2006		0.011 (0.015)	-0.023** (0.009)	0.073 (0.108)	0.136 (0.108)
Year fixed effects, 2007		0.029* (0.016)	-0.052*** (0.010)	0.104 (0.104)	0.168 (0.104)
Year fixed effects, 2008		0.002 (0.006)	0.004 (0.007)	-0.009 (0.042)	-0.060 (0.043)
Year fixed effects, 2009		-0.017** (0.007)	-0.015** (0.007)	0.075 (0.065)	0.025 (0.064)
Year fixed effects, 2010		0.027*** (0.008)	0.032*** (0.008)	0.136* (0.072)	0.105 (0.072)
Year fixed effects, 2011		0.028*** (0.011)	0.027*** (0.009)	0.133 (0.092)	0.113 (0.092)

Squared firm size and year interaction, 2002	0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2003	-0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2004	-0.000* (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2005	-0.000* (0.000)	-0.000** (0.000)
Squared firm size and year interaction, 2006	-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2007	-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2008	0.000*** (0.000)	0.000*** (0.000)
Squared firm size and year interaction, 2009	0.000* (0.000)	0.000*** (0.000)
Squared firm size and year interaction, 2010	0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2011	-0.000 (0.000)	0.000** (0.000)
Squared firm size and year interaction, 2012	-0.000 (0.000)	0.000** (0.000)
Independent firm		0.010 (0.012)
Firm age		-0.014*** (0.002)
Average firm specific experience in months		-0.003*** (0.000)
Average years of education		-0.077*** (0.009)
Number of plants		-0.024*** (0.008)
Age of the company		0.001 (0.000)

Constant	0.183*** (0.00525)	0.172*** (0.0149)	0.216*** (0.00675)	0.246*** (0.0543)	1.870*** (0.149)
Year fixed effects		x	x	x	x
Company fixed effects			x	x	x
Quadratic firm size control				x	x
Firm characteristics					x
N	115497	115497	115497	115497	115497
Adjusted R ²	0.003	0.004	0.009	0.013	0.024

Standard errors are clustered to the firm level, reported in parentheses. *, **, *** indicate significance at the 90 %, 95 %, and 99 % level, respectively.

Coefficients for industry, province and industry and year interaction are excluded for readability.

Table B.2. Worker inflow rate

Regression table: Worker inflow rate					
	(1)	(2)	(3)	(4)	(5)
Firms with 20-29 staff	0.046** (0.023)	0.050** (0.023)	0 (.)	0 (.)	0 (.)
Firms with 30-39 staff	0.039 (0.032)	0.044 (0.032)	0 (.)	0 (.)	0 (.)
Post CA reform	-0.208*** (0.013)	-0.259*** (0.026)	-0.399*** (0.020)	-1.368*** (0.394)	-1.275*** (0.395)
Firms with 20-29 staff * Post CA reform	-0.073*** (0.021)	-0.076*** (0.021)	-0.058*** (0.021)	-0.070*** (0.022)	-0.071*** (0.022)
Firms with 30-39 staff * Post CA reform	-0.073** (0.034)	-0.076** (0.034)	-0.105*** (0.040)	-0.139*** (0.042)	-0.148*** (0.042)
Year fixed effects, 2003		-0.062** (0.025)	-0.042** (0.019)	-0.397 (0.334)	-0.109 (0.335)
Year fixed effects, 2004		-0.082*** (0.025)	-0.079*** (0.019)	-0.495 (0.337)	-0.200 (0.338)
Year fixed effects, 2005		-0.029 (0.027)	-0.076*** (0.022)	-0.620 (0.377)	-0.580 (0.378)
Year fixed effects, 2006		-0.015 (0.029)	-0.122*** (0.021)	-0.607 (0.384)	-0.566 (0.385)
Year fixed effects, 2007		0.038 (0.028)	-0.150*** (0.023)	-0.588 (0.392)	-0.548 (0.393)
Year fixed effects, 2008		0.117*** (0.014)	0.093*** (0.013)	0.423*** (0.098)	0.370*** (0.098)
Year fixed effects, 2009		-0.039*** (0.011)	-0.044*** (0.013)	0.194*** (0.054)	0.140*** (0.053)
Year fixed effects, 2010		0.023* (0.014)	0.035** (0.014)	0.205*** (0.068)	0.174** (0.068)

Year fixed effects, 2011	0.048** (0.022)	0.048** (0.022)	0.224 (0.160)	0.203 (0.159)
Squared firm size and year interaction, 2002			0.000 (0.000)	0.000* (0.000)
Squared firm size and year interaction, 2003			-0.000 (0.000)	0.000** (0.000)
Squared firm size and year interaction, 2004			-0.000 (0.000)	0.000*** (0.000)
Squared firm size and year interaction, 2005			-0.000** (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2006			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2007			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2008			0.000*** (0.000)	0.000*** (0.000)
Squared firm size and year interaction, 2009			0.000*** (0.000)	0.000*** (0.000)
Squared firm size and year interaction, 2010			0.000 (0.000)	0.000* (0.000)
Squared firm size and year interaction, 2011			-0.000 (0.000)	0.000** (0.000)
Squared firm size and year interaction, 2012			0.000 (0.000)	0.000** (0.000)
Independent firm				0.001 (0.018)
Firm age				-0.009*** (0.003)
Average firm specific experience in months				-0.005*** (0.000)
Average years of education				-0.027* (0.014)
Number of plants				-0.033*** (0.009)

Age of the company					0.003** (0.001)
Constant	0.787*** (0.0117)	0.805*** (0.0244)	0.929*** (0.0160)	1.490*** (0.341)	2.521*** (0.397)
Year fixed effects		x	x	x	x
Company fixed effects			x	x	x
Quadratic firm size control				x	x
Firm characteristics					x
N	115497	115497	115497	115497	115497
Adjusted R ²	0.005	0.006	0.014	0.020	0.025

Standard errors are clustered to the firm level, reported in parentheses. *, **, *** indicate significance at the 90 %, 95 %, and 99 % level, respectively.

Coefficients for industry, province and industry and year interaction are excluded for readability.

Table B.3. Worker outflow rate

Regression table: Worker outflow rate					
	(1)	(2)	(3)	(4)	(5)
Firms with 20-29 staff	0.050** (0.021)	0.048** (0.021)	0 (.)	0 (.)	0 (.)
Firms with 30-39 staff	0.038 (0.030)	0.035 (0.030)	0 (.)	0 (.)	0 (.)
Post CA reform	0.185*** (0.012)	-0.274*** (0.024)	-0.332*** (0.019)	-1.389*** (0.394)	-1.375*** (0.395)
Firms with 20-29 staff * Post CA reform	-0.044** (0.019)	-0.041** (0.019)	-0.043** (0.019)	-0.052*** (0.019)	-0.052*** (0.019)
Firms with 30-39 staff * Post CA reform	-0.025 (0.031)	-0.021 (0.032)	-0.064* (0.035)	-0.092** (0.037)	-0.097*** (0.037)
Year fixed effects, 2003		-0.084*** (0.023)	-0.056*** (0.018)	-0.432 (0.333)	-0.318 (0.333)
Year fixed effects, 2004		-0.101*** (0.023)	-0.084*** (0.017)	-0.497 (0.336)	-0.384 (0.336)
Year fixed effects, 2005		-0.073*** (0.025)	-0.098*** (0.020)	-0.640* (0.376)	-0.631* (0.376)
Year fixed effects, 2006		-0.073*** (0.027)	-0.139*** (0.019)	-0.616 (0.383)	-0.610 (0.384)
Year fixed effects, 2007		-0.104*** (0.025)	-0.214*** (0.020)	-0.711* (0.390)	-0.706* (0.391)
Year fixed effects, 2008		0.063*** (0.013)	0.036*** (0.011)	0.315*** (0.096)	0.305*** (0.096)
Year fixed effects, 2009		-0.041*** (0.010)	-0.050*** (0.011)	0.0500 (0.053)	0.0380 (0.053)
Year fixed effects, 2010		-0.009 (0.013)	-0.003 (0.013)	0.078 (0.066)	0.072 (0.066)
Year fixed effects, 2011		0.037* (0.013)	0.033 (0.013)	0.146 (0.066)	0.140 (0.066)

	(0.021)	(0.021)	(0.158)	(0.158)
Squared firm size and year interaction, 2002			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2003			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2004			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2005			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2006			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2007			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2008			0.000*** (0.000)	0.000*** (0.000)
Squared firm size and year interaction, 2009			0.000*** (0.000)	0.000*** (0.000)
Squared firm size and year interaction, 2010			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2011			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2012			-0.000 (0.000)	-0.000 (0.000)
Independent firm				-0.008 (0.017)
Firm age				-0.004* (0.003)
Average firm specific experience in months				-0.002*** (0.000)
Average years of education				0.004 (0.013)
Number of plants				0.001 (0.006)
Age of the company				0.002**

					(0.001)
Constant	0.686*** (0.0108)	0.763*** (0.0234)	0.834*** (0.0150)	1.345*** (0.338)	1.551*** (0.385)
Year fixed effects		x	x	x	x
Company fixed effects			x	x	x
Quadratic firm size control				x	x
Firm characteristics					x
N	115497	115497	115497	115497	115497
Adjusted R ²	0.004	0.005	0.011	0.019	0.020

Standard errors are clustered to the firm level, reported in parentheses. *, **, *** indicate significance at the 90 %, 95 %, and 99 % level, respectively.

Coefficients for industry, province and industry and year interaction are excluded for readability.

Table B.4. Labor productivity

Regression table: Labor productivity					
	(1)	(2)	(3)	(4)	(5)
Firms with 20-29 staff	-649.9 (957.4)	-370.3 (970.2)	0 (.)	0 (.)	0 (.)
Firms with 30-39 staff		6064.7** (3042.8)	0 (.)	0 (.)	0 (.)
Post CA reform	6540.6*** (683.9)	13776.7*** (1132.8)	12645.6*** (1027.6)	4463.1** (1836.1)	3467.7 (2554.5)
Firms with 20-29 staff * Post CA reform	-954.9 (1644.4)	-1225.5 (1640.5)	-120.5 (1122.5)	-576.3 (1150.9)	-566.2 (1167.5)
Firms with 30-39 staff * Post CA reform	-4544.7* (2649.4)	-4862.4* (2620.2)	-4104.2 (2532.7)	-3549.7 (2421.4)	-3532.9 (2415.7)
Year fixed effects, 2003		994.9* (546.2)	1587.0** (632.7)	-3762.3** (1837.4)	-4821.1** (2113.6)
Year fixed effects, 2004		1782.0*** (572.1)	2328.8*** (661.0)	91.52 (1575.0)	-1089.2 (1870.3)
Year fixed effects, 2005		3799.3*** (666.6)	4248.2*** (716.9)	2063.4 (1351.7)	1831.6 (1442.1)
Year fixed effects, 2006		4459.5*** (982.0)	5223.9*** (1019.4)	6692.9*** (1638.4)	6221.4*** (1938.8)
Year fixed effects, 2007		6249.0*** (1076.9)	7560.5*** (1143.6)	100.3 (1501.0)	-399.7 (1915.5)
Year fixed effects, 2008		-3135.9*** (836.0)	-1514.8** (697.1)	-313.5 (2060.9)	144.9 (2160.7)
Year fixed effects, 2009		-6554.7*** (834.5)	-5789.5*** (649.4)	-2139.2 (1769.3)	-1710.2 (1832.7)
Year fixed effects, 2010		-5186.3***	-4323.9***	-2836.1*	-2593.2*

	(787.9)	(558.0)	(1465.9)	(1489.3)
Year fixed effects, 2011	-3466.8***	-2792.2***	-1911.8	-1775.6
	(614.0)	(481.6)	(1473.4)	(1486.3)
Year fixed effects, 2012	0	0	0	0
	(.)	(.)	(.)	(.)
Squared firm size and year interaction, 2002			-0.446***	-0.445***
			(0.158)	(0.159)
Squared firm size and year interaction, 2003			-0.200***	-0.199***
			(0.015)	(0.017)
Squared firm size and year interaction, 2004			-0.204***	-0.204***
			(0.016)	(0.017)
Squared firm size and year interaction, 2005			-0.485	-0.483
			(0.416)	(0.419)
Squared firm size and year interaction, 2006			-0.285**	-0.282*
			(0.144)	(0.146)
Squared firm size and year interaction, 2007			-0.310*	-0.307*
			(0.173)	(0.174)
Squared firm size and year interaction, 2008			-4.035*	-3.979*
			(2.282)	(2.303)
Squared firm size and year interaction, 2009			-0.074***	-0.067**
			(0.005)	(0.030)
Squared firm size and year interaction, 2010			-0.060***	-0.054**
			(0.004)	(0.027)
Squared firm size and year interaction, 2011			-0.051***	-0.046**
			(0.003)	(0.022)
Squared firm size and year interaction, 2012			-0.047***	-0.043**
			(0.003)	(0.021)
Independent firm				-326.0
				(1534.7)
Firm age				-12.17
				(91.10)
Average firm specific experience in months				18.60
				(19.86)
Average years of education				60.18

					(901.7)
Number of plants					-131.9
					(639.3)
Age of the company					51.12
					(163.4)
Constant	50893.2**	47428.2***	47772.0***	54085.1***	51460.4***
	(587.1)	(835.0)	(836.5)	(3101.9)	(12002.1)
Year fixed effects		x	x	x	x
Company fixed effects			x	x	x
Quadratic firm size control				x	x
Firm characteristics					x
N	106525	106525	106525	106525	106525
Adjusted R ²	0.002	0.002	0.005	0.041	0.041

Standard errors are clustered to the firm level, reported in parentheses. *, **, *** indicate significance at the 90 %, 95 %, and 99 % level, respectively.

Coefficients for industry, province, and industry and year interaction are excluded for readability.

Table B.5. Profitability

Regression table: Profitability

	(1)	(2)	(3)	(4)	(5)
Firms with 20-29 staff	-0.258 (0.511)	-0.285 (0.483)	0 (.)	0 (.)	0 (.)
Firms with 30-39 staff	-0.329 (0.541)	-0.358 (0.518)	0 (.)	0 (.)	0 (.)
Post CA reform	0.365 (0.310)	0.181 (0.238)	0.323 (0.509)	0.0889 (0.228)	0.146 (0.318)
Firms with 20-29 staff * Post CA reform	-0.245 (0.608)	-0.223 (0.586)	-0.470 (0.616)	-0.353 (0.606)	-0.351 (0.614)
Firms with 30-39 staff * Post CA reform	-0.549 (0.499)	-0.527 (0.479)	-0.825 (0.682)	-0.610 (0.699)	-0.591 (0.697)
Year fixed effects, 2003		-0.306 (0.516)	-0.241 (0.453)	-0.052 (0.038)	-0.393 (0.284)
Year fixed effects, 2004		0.131 (0.147)	0.105 (0.151)	-0.034 (0.039)	-0.376 (0.288)
Year fixed effects, 2005		-0.351 (0.540)	-0.349 (0.600)	0.007 (0.031)	0.055 (0.083)
Year fixed effects, 2006		0.139 (0.129)	0.146 (0.272)	0.031 (0.053)	0.106 (0.130)
Year fixed effects, 2007		-0.723 (0.762)	-0.578 (0.716)	0.024 (0.070)	0.103 (0.153)
Year fixed effects, 2008		-0.405 (0.313)	-0.315 (0.282)	0.799 (0.635)	0.858 (0.636)
Year fixed effects, 2009		-0.158 (0.249)	-0.048 (0.181)	0.021 (0.021)	0.075 (0.072)

Year fixed effects, 2010	0.323 (0.209)	0.281* (0.155)	0.018 (0.017)	0.042 (0.045)
Year fixed effects, 2011	0.107 (0.133)	0.125 (0.143)	0.017 (0.017)	0.028 (0.025)
Squared firm size and year interaction, 2002			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2003			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2004			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2005			-0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2006			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2007			0.000 (0.000)	0.000 (0.000)
Squared firm size and year interaction, 2008			-0.002 (0.001)	-0.002 (0.001)
Squared firm size and year interaction, 2009			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2010			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2011			-0.000 (0.000)	-0.000 (0.000)
Squared firm size and year interaction, 2012			-0.000 (0.000)	-0.000 (0.000)
Independent firm				0.429 (0.912)
Firm age				-0.040 (0.051)
Average firm specific experience in months				0.007 (0.006)
Average years of education				0.154 (0.219)

Number of plants					0.058 (0.128)
Age of the company					-0.003 (0.010)
Constant	-0.368 (0.309)	-0.138 (0.209)	-0.322 (0.223)	-0.170 (0.330)	-1.808 (1.879)
Year fixed effects		x	x	x	x
Company fixed effects			x	x	x
Quadratic firm size control				x	x
Firm characteristics					x
N	104431	104431	104431	104431	104431
Adjusted R ²	0.000	-0.000	-0.000	-0.001	-0.001

Standard errors are clustered to the firm level, reported in parentheses. *, **, *** indicate significance at the 90 %, 95 %, and 99 % level, respectively.

Coefficients for industry, province, and industry and year interaction are excluded for readability.

Appendix C. Placebo test

Table C.1. DID estimators for the placebo test

Placebo test DID estimators					
	(1)	(2)	(3)	(4)	(5)
Firm employment	-0.024 (0.017)	-0.024 (0.017)	-0.024 (0.019)	-0.054** (0.024)	-0.126 (0.027)
Worker inflow rate	-0.005 (0.032)	-0.004 (0.032)	-0.026 (0.031)	-0.004 (0.029)	-0.004 (0.025)
Worker outflow rate	-0.031 (0.030)	-0.006 (0.029)	-0.024 (0.027)	-0.034 (0.021)	
Labor productivity	-3 987.6** (2 012.0)	-3 997.3** (2 014.1)	-4 969.6* (2 929.9)	-11 508.3*** (3 806.5)	-7 526.4 (4 393.8)
Profitability	-1.352 (0.927)	-1.351 (0.928)	-1.542 (1.176)	-1.690 (1.155)	-1.965 (1.269)
Year fixed effects		x	x	x	x
Company fixed effects			x	x	x
Quadratic firm size control				x	x
Firm characteristics					x
N	115 497	115 497	115 497	115 497	115 497

Standard errors are clustered to the firm level, reported in parentheses. *, **, *** indicate significance at the 90 %, 95 %, and 99 % level, respectively.