

# Voice at Work\*

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## Abstract

We estimate the effects of worker voice on job quality and separations. We leverage the 1991 introduction of worker representation on boards of Finnish firms with at least 150 employees. In contrast to exit-voice theory, our difference-in-differences design reveals no effects on voluntary job separations, and at most small positive effects on other measures of job quality (job security, health, subjective job quality, and wages). Worker voice slightly raised firm survival, productivity, and capital intensity. A 2008 introduction of shop-floor representation had similarly limited effects. Interviews and surveys indicate that worker representation facilitates information sharing rather than boosting labor's power.

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# 1 Introduction

Workers can influence the decision-making of their employer either through *exit*—by quitting, or via the external labor market more generally—or through *voice*—giving feedback and even participating directly in the firm’s decision-making. Worker voice has been hypothesized to reduce turnover, boost job quality, and increase productivity by improving information flows (Hirschman, 1970; Freeman, 1980; Freeman and Medoff, 1985) and by fostering cooperation and coordination between the workforce and management (Malcomson, 1983; Freeman and Lazear, 1995). In many countries, the law grants workers rights, or establishes mandates, for formal voice institutions in firms. For instance, in many European countries, workers are given the right to elect representatives to corporate boards, or establish works councils. In contrast, countries without such mandates often feature an adversarial industrial relations system. In the United States, for example, unions are legally the only formal worker voice institution, and are narrowly focused on collective bargaining and largely barred from corporate decision-making (see, e.g., DiNardo and Lee, 2004; Lee and Mas, 2012; Farber, Herbst, Kuziemko, and Naidu, 2018, for effects of US unions). Nonetheless, US workers express substantial demand for more voice (Bryson and Freeman, 2013; Kochan, Yang, Kimball, and Kelly, 2019).

We estimate the causal effect of the 1991 introduction of formal worker voice through board representation in Finland. Finland was a late arrival to this form of worker voice among peer countries, providing a unique quasi-experiment that we can study using modern administrative worker and firm data.<sup>1</sup> This right was introduced in firms with 150 or more workers; no such right exists in smaller firms. By law, workers have a right to elect representatives from the workforce that take 20% of the seats on the board of directors, the board of supervisors, or the management body—with the specific body selected by the firm, and worker-elected members having the same rights as shareholder-appointed ones (except for wage negotiations, labor disputes, and the appointment or dismissal of senior management). Alternative forms of worker representation can be negotiated between the firm and workforce if both parties agree. Overall, the Finnish form of board-level representation is closer to an information-sharing institution than in other settings: oftentimes, agreements between firms and workers lead to the election of board representatives without formal voting rights, or the establishment of advisory councils instead of direct board membership for workers. This feature likely minimizes potential negative side effects from agency problems and hold-up resulting from a boost to workers’ formal authority and bargaining power (an influential

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<sup>1</sup>For example, Norway and Sweden introduced board-level codetermination in 1975 and 1980, respectively, well before the advent of administrative data collection for research purposes. Svejnar (1981) studies the introduction of parity codetermination in 1951 in the iron, steel and mining sector using industry-level data in Germany. Analyzing a natural experiment, Jäger, Schoefer, and Heining (forthcoming) study the abolition of board-level representation in certain newly established firms in Germany in 1994, rather than a change in mandates among incumbent firms, with a focus on wage effects and without observing full panel data for firms around the reform.

hypothesis going back to Jensen and Meckling, 1979).

We analyze the introduction in a difference-in-differences research design. We compare the outcomes of firms with pre-reform employment above or below the policy cutoff of 150 workers, before and after the reform. Pre-reform outcomes evolve in parallel, supporting our identification assumption, which we additionally support with a variety of robustness checks such as varying the bandwidth of employment around the policy cutoff.

We start by studying voluntary separations, putting Hirschman's (1970) exit-voice theory to the test in the labor market: through voice, workers can affect job quality by means other than quitting, meaning that access to voice should theoretically reduce separations. Voluntary job separations are also a revealed-preference measure of the overall attractiveness of an employer (see, e.g., Krueger and Summers, 1988, for a canonical study of separations as a measure of industry rents). We find small, if any, reductions in annual job-to-job transitions of about 0.7 percentage points in our preferred specification. Hence, from the vantage point of voluntary turnover, there is not strong evidence that the worker voice institution substantially increases job quality, although our estimates leave room for small gains. Similarly, we find no clear effects on a revealed-preference ranking of firm quality (Sorkin, 2018). As proxies for non-wage amenities hypothesized to be prioritized by worker representatives (see, e.g., Freeman and Medoff, 1985), we also study worker health and workplace safety as outcomes, drawing on administrative data on sickness spells. We find precisely estimated effects around zero, and we can reject that sickness incidence falls by more than 10%.

As a complementary measure of overall job quality from the perspective of workers, we study survey-based subjective job quality in a unique Finnish Quality of Work Life Survey, which we merge onto our data set. We find positive point estimates corresponding to an increase of about 0.15 (SE 0.09) to 0.18 (SE 0.08) control-group standard deviations, consistent with a moderate increase in perceived job quality.

Turning to a proxy for involuntary turnover, we find a small reduction in the annual separation rate of workers moving into nonemployment of about two percentage points, or 14%, in our preferred specification. Hence, worker representatives may have a small positive impact on entrenchment of workers (Pagano and Volpin, 2005) or on job security as predicted by implicit contract models (Malcomson, 1983; Kim, Maug, and Schneider, 2018). The pattern we document is also consistent with testimony from worker representatives that preventing redundancies and layoffs is among their key objectives.

We then turn to wages, a key determinant of job quality from the perspective of workers besides non-wage amenities. On the one hand, compensating differentials could lower wages and thereby offset any potential increase in non-wage amenities due to the reform (worker voice itself or the amenities it brings), as would be expected in a competitive labor market. On the other hand, workers may use their voice to push for higher wages, especially if the

institution raised their bargaining power. In the data, we find an increase of around two to three percent in raw firm-level mean log wages. The effect size drops by a third and is only marginally significant once we control for worker composition by studying firm wage fixed effects as outcomes (following Abowd, Kramarz, and Margolis, 1999). The point estimate in our preferred specification is 1.6 percent and confidence intervals permit us to rule out shifts in firms' wage policies above 3.2 percent (consistent with Jäger, Schoefer, and Heining, forthcoming, in Germany). We find some evidence consistent with pay compression, with small wage gains concentrated among lower earners within the firm. We detect only small increases in rent sharing and can reject increases in the labor share above 0.5 percentage points (with negative point estimates).

While we therefore find limited effects of worker voice on job quality from the *workers'* perspective, and limited increases in worker rents, an important strand of the literature predicts positive effects on the employer side of job quality, *firm* performance. Worker voice may improve information flows (Freeman and Medoff, 1985), or foster cooperation and coordination between the workforce and management (Freeman and Lazear, 1995). A parallel literature cautions that worker voice, to the extent that it has bite, may instead have negative effects, e.g., by exacerbating agency conflicts or discouraging capital formation (Jensen and Meckling, 1979; Grout, 1984). Using our administrative data on firms' financials, we find moderately positive but statistically insignificant effects on firm survival, total factor productivity, and the capital-labor ratio. Log value added per worker increases by 0.067 (SE 0.031) in our preferred specification (although the effects are smaller and not statistically significant in other specifications). We find precisely estimated zero effects on the profit margin. The overall absence of negative effects on firm performance may reflect the fact that Finnish firms can negotiate the specific form of codetermination with the workers. The absence of *large* positive effects is also broadly consistent with the argument that if worker voice did substantially raise productivity, firms would emulate the mandated forms even absent mandates (see, e.g., Jensen and Meckling, 1979; Romano, 1993; Enriques, Hansmann, and Kraakman, 2017).

A potential explanation of the limited effects we document is that the board-level representation induced by the reform may duplicate functions already fulfilled by another codetermination institution: as in many other countries with formal worker voice institutions, the Finnish system additionally features a lower level of codetermination at the shop floor. Shop-floor representatives hold information, procedural, and consultation rights, and resemble works councils in, e.g., the Austrian or German contexts (though Finnish shop-floor representatives lack veto powers). To assess whether shop-floor representation may by itself affect our outcomes (and leave little room for board-level codetermination), we leverage a second reform in 2008 that lowered the size threshold for mandatory shop-floor representation. Our analysis sample is among firms too small to have board-level representation rights (10-39

employees) and we compare firms with employment in the size category affected by the law to ones slightly smaller or larger. We find very similar—i.e., limited—effects of the introduction of shop-floor representation on worker and firm outcomes. Hence, our main finding extends to shop-floor representation, and the limited effects of board-level representation are unlikely to reflect the presence of shop-floor representatives.

We close our paper with qualitative evidence from participants in the institution, drawing on existing surveys as well as our own survey and interviews of Finnish shop-floor and board-level representatives. According to representatives, their main objectives are securing good working conditions and employment stability, and preventing redundancies, layoffs, and outsourcing. However, most representatives do not believe their role comes with direct power in corporate decision making; instead, they view their role as being about improving communication and information sharing, and fostering cooperation. In terms of actual influence, a large minority of worker representatives believe they can improve cooperation, welfare, and working conditions, but virtually no representatives believe they can affect wages, investment, outsourcing, or other strategic decisions. Turning to firms' perspectives on worker involvement, we find that Finnish firms have a high baseline level of informal or direct involvement of workers even without being subject to the board-level (or shop-floor) representation rights, which may explain the limited effects we estimate.

**Related Literature** Our paper relates to several strands of literature in labor economics, corporate governance, and industrial relations, which have studied the impacts of worker voice and shared governance on firm and worker outcomes (see Conchon, 2011, for an overview of studies of board representation in Europe, Addison, 2009 for studies of codetermination in Germany, Keskinen, 2017, for an analysis of shop-floor representation in Finland, and Jäger, Noy, and Schoefer 2021 for a recent review of the literature). In nearly all countries, codetermination rules are dependent on firm size (employment), and most codetermination laws were introduced decades ago, before the advent of high-quality firm and worker data. As a result, the literature has largely relied on propensity score matching, regression discontinuity designs using contemporaneous employment as a running variable, or studying firms moving across employment thresholds. By contrast, our identification strategy is a reform-based difference-in-differences design, relying on pre-reform employment as an instrument for codetermination status. Our strategy resolves the identification challenges faced by prior studies: for example, that employment may be endogenous (see, e.g., Garicano, Lelarge, and Van Reenen, 2016, on distortions from size-based regulations), and that treatment status is anticipated, transitory, or has lagged or leading effects.

The paper closest to ours is a second reform-based difference-in-differences analysis of the effects of codetermination across firms, studying a 1994 repeal of shared governance in shareholder corporations in Germany (Jäger, Schoefer, and Heining, forthcoming). Like our

paper, that paper exploits a reform to overcome the aforementioned identification challenges. Compared to Jäger, Schoefer, and Heining (forthcoming), our paper breaks new ground with respect to its substantive focus, nature of the variation, institutional context, and methodology. Substantively, our focus is on testing the exit-voice hypothesis (Hirschman, 1970; Freeman, 1980), implementing a comprehensive analysis of direct and indirect measures of job quality and margins of separations, as well as specific amenities such as job security and worker health. By contrast, Jäger, Schoefer, and Heining (forthcoming) ask whether shared governance affects wage setting and interpret results through a bargaining and hold-up framework. In terms of the variation, we study a reform more germane to the policy discussions in many countries today: an *introduction* of codetermination rights (in 1991 and 2008) imposed broadly on *incumbent firms* of any legal form larger than 150, making up *half* of the Finnish labor market; Jäger, Schoefer, and Heining (forthcoming) study a *repeal* of shared (board-level) governance in *new cohorts* of certain firm types, specifically in new stock corporations in Germany. In terms of institutional differences, Finnish boards are predominantly unitary (Lekvall et al., 2014; Ringe, 2016), closer to, e.g., the US system, whereas German boards are dual, with codetermination on the supervisory board mandated by law where applicable (rather than formulated as a right as in Finland).<sup>2</sup> Methodologically, the administrative firm data yield substantially higher precision for the effects on firm performance outcomes, and permit us to study the early years of the reform for all outcomes (whereas Jäger, Schoefer, and Heining, forthcoming, can do so mainly for wage outcomes, with other outcomes measured years after the repeal). Besides our comprehensive measure of job quality, we also analyze some overlapping outcomes, namely wages and firm performance, for which we find similarly limited or small positive effects.

Our results are also broadly consistent with a large and important body of research that uses cross-sectional or panel variation of firms located on different sides of or moving across the codetermination size thresholds—most prominently, regression-discontinuity (RD) regression estimates. (We implement such an RD design in the Finnish context as a complement to our main difference-in-differences design). In the German context, several papers have studied the wage effects of quasi-parity codetermination, comparing firms with fewer or more than 2000 employees, and have found small negative (Kim, Maug, and Schneider, 2018), zero (Gorton and Schmid, 2004), or small positive effects (Redeker, 2019). Similarly, studying the Norwegian context (with board structures being more similar to the Finnish context), Blandhol, Mogstad, Nilsson, and Vestad (2020) find no wage effects of board-level representation in Norway and

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<sup>2</sup>While the German law specifies which of the two boards (supervisory rather than executive) must be codetermined and imposes a mandate rather than a right to have worker representatives making up 33% (up to quasi-parity, i.e. 50% of seats with shareholders retaining the tie-breaking vote), Finnish law leaves the implementation of codetermination to negotiation, imposing only a default statutory right. In practice, Finnish board-level representation is largely implemented in the form of information sharing platforms rather than conveying formal authority, compared to the full voting rights at the highest (supervisory board) level in Germany.

no effects on rent sharing (pass-through of shocks into wages) in a regression discontinuity design comparing firms just above and just below the size threshold of 30 employees as well as in an event study tracking firms adopting worker representation.<sup>3</sup>

By studying separations, a core focus of our paper is on revealed-preference measures of job quality. While a long-standing theoretical body of work has posited that worker representation ought to decrease separations (Hirschman, 1970; Malcolmson, 1983), the empirical evidence is more ambiguous. In the context of US unions, Freeman (1980) documents a negative relationship between collective bargaining and turnover (see also Hammer and Avgar, 2005, for an overview). For parity representation, Kim, Maug, and Schneider (2018) find that skilled workers in German firms with quasi-parity codetermination (as opposed to one-third representation) are more insulated from layoffs, consistent with our results suggesting a small reduction in nonemployment separations. The evidence on the relationship between separations and the presence of a works council is more mixed (see, e.g., Kraft, 1986; Addison, Schnabel, and Wagner, 2001; Pfeifer, 2011; Hirsch, Schank, and Schnabel, 2010).<sup>4</sup> We add to this long-standing debate by presenting a reform-based difference-in-differences design and by bringing a larger set of separation effects into focus, importantly job-to-job transitions as a measure of job quality and voluntary worker exit, along with additional direct measures of job quality from the perspective of workers.

**Outline** In Section 2, we describe the reform and the Finnish codetermination and wage setting institutions. Section 3 presents the research design and the data. We present the results on separations and job quality in Section 4. Section 5 contains our results on wages and the wage structure. Next, Section 6 analyses the effects on firm performance. Section 7 reports the results of our analysis of the 2008 reform of shop-floor representation. Section 8 wraps up by reflecting on potential explanations of the results we estimate, and discussing qualitative evidence from our survey of worker representatives.

## 2 Institutional Context and Reform

We provide an overview of corporate governance in Finland and describe the 1991 reform introducing a worker right to shared governance. We then describe additional institutions of worker voice and wage setting in Finland.

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<sup>3</sup>While workers employed in Norwegian firms with codetermination do have higher wages, the authors show that this relationship is accounted for by firm size as an omitted variable and not driven by worker representation. See also Strøm (2007); Bøhren and Strøm (2010) for a comparison of Norwegian firms with and without employee directors, finding a moderately positive relationship with wages and a negative relationship with firm performance.

<sup>4</sup>See also Adhvaryu, Molina, and Nyshadham (2019), who study a field experiment giving workers in a garment factory the opportunity to give feedback, and find a reduction in turnover.

## 2.1 Corporate Governance in Finland

Finnish companies follow the Nordic board model and overwhelmingly feature a single-tier board structure, with a board of directors elected by the general meeting of the shareholders (see, Lekvall et al., 2014, Appendix B, for a detailed overview of corporate governance in Finland).<sup>5</sup> Figure 1 Panel (a) illustrates this board structure, without shared governance. The board of directors determines corporate strategy and appoints, dismisses, oversees, and sets the compensation for the managing director, who runs the firm on a day-to-day basis. The Finnish Corporate Governance Code advises that the majority of directors be independent. In practice, most boards are exclusively comprised of non-executive directors; in 2013, only 15% of listed firms had their managing director on the board of directors (Lekvall et al., 2014). Executives must follow and implement instructions from the board of directors (Ringe, 2016). The general meeting of the shareholders sets the compensation for directors.

## 2.2 The 1991 Introduction of Worker Representation in Large Firms

**1991 Reform** Until the 1991 reform, the law did not grant workers formal voice channels in firm-level decision-making at the board level, although workers in most firms had shop-floor representatives with some information and consultation rights. A 1991 reform introduced board-level representation in firms with at least 150 employees. The law (725/1990) was passed in 1990 by a coalition government between the center-right party (KOK) and the Social Democratic Party and two smaller parties. The law was the result of a political compromise, with employer associations opposing it, while Social Democrats called for a lower threshold of 30 employees (Marttila, 2016, p. 224). The law allowed for shared governance by mutual agreement starting on January 1, 1991, and then installed the statutory right to participation in board-level governance starting with the first general meeting held after July 1, 1992. The law has been in place without major changes since it was first passed.

As a consequence of the 1991 reform, workers in firms with at least 150 employees have a right to participate in the governance of their firms and to be involved in business and financial decisions through board-level representation. The typical form of representation is through an agreement between the firm and worker representatives of at least two employee groups (manual, non-manual, and managerial workers) representing a majority of employees. If no agreement is reached but at least two of the employee groups still demand representation, workers have a statutory right to appoint representatives to the board of directors (or the supervisory board, in the less common dual board structure) or the management group, with the firm choosing between these two options. We illustrate representation on the board of directors as well as in the management group in Figure 1 Panel (b). Statutorily, workers

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<sup>5</sup>Firms can, but rarely do, choose a two-tier structure with a supervisory board, as in, e.g., Germany.

make up 20% of the respective body (although, by agreement rather than default, firms could expand this share voluntarily). By law, worker representatives must be employees of the firm (rather than being outside union representatives), and have the same rights and duties as other non-worker representatives. Exceptions are the selection and dismissal of, and compensation setting for management, workforce wage setting, and other employment-related matters such as strikes.

***De Facto Implementation Following 1991 Reform*** The law explicitly permits a large degree of flexibility in organizing the form and scope of representation, besides the statutory default of 20% representation on boards, unlike in other countries such as Germany.

Overall, our review of four surveys paints a picture of worker representation primarily operating through agreements, with worker representatives not having the same rights as the other members of the respective board, with the institutional implementation pointing towards a boost in advisory, consultation, and information rights rather than formal authority, e.g., voting power. Our evidence is consistent with findings that formal worker board representation with the same rights as shareholders is rare, e.g., among listed firms (Lekvall et al., 2014; Thomsen, Rose, and Kronborg, 2016), although observers note that this decline might have occurred during the late 2000s.

To characterize the actually prevailing types of worker voice, we draw on three existing surveys and one we designed, all four of which cover worker representatives, such as shop-floor representatives or board-level representatives (see Table 1 for a summary). The earliest one is a 2001 survey among 203 shop-floor representatives conducted by the Finnish Metalworkers' Union (Sairo, 2001). In firms meeting the 150 employee threshold, 60% had formalized worker representation following the 1991 law. In 60% of these cases, workers had some representation on the management board, while 26% included representation on the board of directors. 51% noted that the worker representative had the same rights as other members of the relevant body. We find qualitatively similar results analyzing 2017 and 2019 surveys among representatives conducted by the trade union federation for industrial employees TP (Teollisuuden Palkansaajat, 2017, 2019). In these surveys, 51% (2017) and 47% (2019) of representatives in firms with at least 150 employees reported having formal forms of shared governance following the 1991 law. Among those, 26% and 25%, respectively, followed the statutory provisions rather than organizing representation by agreement, with 32% and 37% having worker representatives on the management team, 23% and 24% on the board of directors, and 17% and 8% on the supervisory board. Other forms of representation included advisory boards and regular meetings between top-level management and worker representatives. We also conducted our own survey in collaboration with the same federation in the fall of 2020, surveying 111 worker representatives. We find that 63% of respondents in firms with at least 150 employees had worker representation following the 1991 law, roughly

equally split between the board of directors (32%), management board (28%), and elsewhere (31%, including the supervisory board). In a majority of cases (54%), the basis is by agreement, with 31% following the statutory provisions.

Our results are based on surveying worker representatives, and may therefore skew towards firms with worker representation. Additionally, we have run a December 2020 phone survey of *firm* representatives, human resources managers, by calling a random sample of firms drawn from the Finnish Bureau van Dijk register, and overall confirmed the above patterns. We are not aware of any additional empirical quantification of the scope and forms of *de facto* Finnish codetermination following the 1991 law.

## 2.3 Other Worker Voice and Shared Governance Institutions

Several additional channels for worker voice exist in Finnish workplaces (Eurofound, 2020). First, oftentimes sectoral collective bargaining agreements provide for the election of shop-floor representatives at the establishment or company level. Besides company-level collective bargaining, shop-floor representatives have a variety of information and procedural rights, for example regarding the financial state of the firm or reorganization of tasks or personnel. These rights entail the power to *delay* implementation, but employers retain ultimate decision-making power. In companies with at least 20 employees where no collective bargaining agreement provides for the election of a shop-floor representative, the Act on Co-operation Within Undertakings mandates the election of a “cooperation representative” with the same rights as a shop-floor representative (except for the ability to engage in collective bargaining). A 2008 amendment to the Act lowered the size threshold from 30 employees to 20; we leverage this reform in Section 7. Shop-floor representatives often sit on works councils that are established to facilitate cooperation and negotiation between management representatives and the different employee groups. Additionally, establishments with at least 10 employees must elect a health and safety representative or committee.

Coverage of these formal institutions is high, especially in companies large enough to be covered by the 1991 reform. According to the 2009 European Company Survey (authors' own tabulations), 99% of Finnish establishments above 150 employees have a shop-floor representative and 100% have a health and safety representative or committee.

In addition to these formal channels for worker voice, in Section 8 we discuss informal channels through which workers can express their voice, and suggest that the existence of these channels helps rationalize our findings.

## 2.4 Wage Setting in Finland

While collective bargaining coverage is high in Finland, it leaves substantial room for firm-specific wage setting. Unions and employer associations negotiate collective agreements that mandate wage floors at the occupation and job level. The wage floors are rarely directly binding as most employees receive pay premia above the floors (Uusitalo and Vartiainen, 2009). Firms can deviate from wage increases negotiated in a collective agreement, and can even negotiate pay cuts with consent of the local bargaining parties. At the individual level, Dickens, Goette, Groshen, Holden, Messina, Schweitzer, Turunen, and Ward (2007) report relatively low downward nominal but high real wage rigidity in Finland in an international comparison. Firm-specific pay policies, with profit-sharing arrangements and links between wages and productivity, have become increasingly common since the 1990s (Uusitalo and Vartiainen, 2009). More than half of white-collar and about a third of blue-collar workers received some form of performance pay in 2000 (Snellman, Uusitalo, and Vartiainen, 2003). Consistent with such firm-specific wage flexibility, in Section 5, we directly estimate the elasticity of wages to value added per worker (rent sharing) at the firm level, estimating 0.06 to 0.08 in our sample (somewhat lower than the average elasticity of 0.10, as surveyed in Card, Cardoso, Heining, and Kline, 2018; Jäger, Schoefer, Young, and Zweimüller, 2020). In addition, depicted in Appendix Figure A.2, we find that firms differentiate wage policies substantially, with firm fixed effects (following Abowd, Kramarz, and Margolis, 1999, with our construction presented in Section 5) accounting for about half of the overall worker-level variation in wages in most years (a slightly lower, by about a quarter, share compared to Germany during the same sampling period, see Card, Heining, and Kline, 2013).

## 2.5 The 1990s Recession

Between 1990 and 1993, Finland experienced a deep recession, and the Finnish currency underwent a devaluation. In the prior decades, Finland had become increasingly dependent on bilateral trade with the Soviet Union. The dissolution of the Soviet Union led to a reduction in Finnish exports among the industries most exposed to Soviet trade (Gorodnichenko, Mendoza, and Tesar, 2012), and raised energy prices. Finland also experienced a credit crunch in 1992 (Gulan, Haavio, and Kilponen, 2014). Finland's recovery in the mid-1990s was accompanied by a sectoral reallocation of employment, with manufacturing employment mostly recovering, retail and construction remaining depressed, and an expansion of the service sector (Koskela and Uusitalo, 2003).

Our empirical design, described in Section 3, compares firms with slightly different sizes, so that aggregate shocks are netted out with year fixed effects; we will also vary bandwidths (firm size cutoffs) to probe for potentially heterogeneous effect by firm size, and we will

include industry-year fixed effects to account for the sectoral dynamics of the recession. We discuss these robustness checks below.

### 3 Research Design and Data

**Difference-in-Differences Design** We study the consequences of the 1991 introduction of board-level worker voice rights in firms with at least 150 employees using a difference-in-differences (DiD) design. We group firms into a treatment and control group based on whether their employment in 1988 (the earliest pre-reform year for which data is available) is above or below the 150 employee threshold. Our most basic specification plots outcomes of these two groups over time from 1988 through the 1991 introduction of the right to shared governance and beyond.

Formally, we estimate the following model for outcome  $y_{it}$  of firm  $i$  (equally weighted) in year  $t$ :

$$y_{it} = \alpha + \sum_{k=1988}^{1997} \psi_k^{\text{Treated}} \cdot \mathbb{1}[\text{Emp}_{i,1988} \geq 150] \times \mathbb{1}[t = k] + \sum_{k=1988}^{1997} \psi_k \cdot \mathbb{1}[t = k] + X_{it}\beta + \epsilon_{it}. \quad (1)$$

The coefficients of interests are  $\psi_k^{\text{Treated}}$  for  $k \geq 1991$ , which capture the effect of the right to shared governance in the post-reform period compared to the pre-reform year, 1990, for which we normalize the coefficient to zero ( $\psi_{1990}^{\text{Treated}} = 0$ ). The model includes year effects,  $\psi_k$ , so that common trends or year-specific shocks will not bias results. As a summary measure, we also report treatment effect averages (rather than year-specific coefficients) in the post-reform period where the regressor of interest is  $\mathbb{1}[\text{Emp}_{1988} \geq 150] \times \mathbb{1}[t \geq 1991]$  estimated with respect to base-year 1990, and an analogous pre-reform effect covering both 1988 and 1989. By contrast, pre-period effects capture potential pre-trends. We report standard errors clustered at the firm level.

**Potential Confounders and Control Variables** A potential source of bias is that firms in different size categories were on different trajectories in the early 1990s even in the absence of the reform. For example, Finland experienced a deep recession from 1990 to 1993, which could have had a differential effect on firms in different size categories. We implement several strategies to control for such confounders. First, as described below, our baseline specification draws on a sample that is quite local around 150 employees, rather than comparing very large to very small firms. In addition, we report more local specifications using even narrower bandwidths around the 150 employee threshold.<sup>6</sup> Second, we control for the primary amplifier

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<sup>6</sup>Relatedly, we have also experimented with placebo effects around counterfactual size cutoffs, which would pick up differential trends by firm size *during* the post-reform period unrelated to the reform (Ganong and Jäger,

of the Finnish recession identified in Gorodnichenko, Mendoza, and Tesar (2012) by including flexible and industry-year effects (NACE Level 1, i.e., letters), as the recession was inherited from Russia and mediated by industry-specific trade exposure. Third, in our most granular—and overall preferred—specification, we add firm fixed effects to gauge concerns of potentially differential attrition over time.

**Data** We use several administrative data sources on firm and workers from Statistics Finland. We CPI-adjust all nominal variables (measured in Finnish markka until 2001 and EUR thereafter) to 2010 EUR (which is inconsequential for our estimated treatment effects due to the inclusion of year effects). We winsorize all continuous outcome variables at the 1% level.

First, our matched employer-employee data draws on the FOLK Employment Relationship Data containing individual-level information, which we match to the firm-level data. It reports the length, in days, of the employee-employer relationship by calendar year. We merge the dataset to the FOLK Basic panel, which, for all Finnish individuals, reports demographics (gender, age, education) and annual total earned and capital income by year from 1988 to 2017.

Second, for firm financials, we draw on the Financial Statement Data Panel, which contains firm-level accounting data from Statistics Finland from 1988 to 2017. Its sources are Statistics Finland's survey from 1988 to 1993, including all large enterprises (larger than 100 employees in manufacturing and trade, and larger than 50 employees in construction and road transport) and a sample of smaller firms based on stratified sampling by industry and employment. Starting in 1994, the data captures the universe of firms.

Third, we merge on the Quality of Work Life Survey, an employee-level survey from 1990 through 2013 including information on labor relations and work quality, conducted as part of the October and November Labour Force Survey, and covering employed persons or wage earners aged 15 to 64 in face-to-face interviews. In our main analysis, we use the 1990 and 1997 waves, each of which covered around 4,000 workers (for earlier uses, see, e.g., Böckerman and Ilmakunnas, 2008; Böckerman, Bryson, and Ilmakunnas, 2012).

Fourth, we will draw on existing surveys of worker representatives from various years as well as our own survey from 2020, described in Section 2, as well as separate in-depth interviews, described in Section 8.

**Employment Measure** The employment concept relevant for the shared governance threshold is the number of employees excluding temporary and seasonal workers. We construct employment at the end of the year (on 31st of December), and drop workers with fewer than 91 days of contracted work in the year, and with zero earned income.

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2018). Consistent with systematic size-and-time-dependent shocks not driving our results, and consistent with the bandwidth robustness checks, we find no systematic pattern of placebo estimates across outcome variables.

**Summary Statistics** In lieu of a separate summary statistics table, we report control means, generally in the baseline preperiod of 1990 and separately by firm group, for each of our outcome variables in the regression tables. Appendix Figure A.1 reports on the industry composition of our sample. In terms of the firm size distribution, the 150 employee cutoff is at the median with 50.2% of employment in firms above the cutoff in the matched employer-employee data.

**Sample** In our main DiD specification, we draw on all firms with 1988 employment in a 100-employee bandwidth around the threshold, i.e., between 50 and 149 employees (control group) and 150 to 250 (treatment group). In robustness checks, we also use smaller bandwidths as well as “donut hole” specifications, leaving out firms with 1988-employment very close to 150 to assess the role of measurement error or limited persistence right around the cutoff.

We do not restrict the sample to firms surviving throughout our observation period. Instead, we compare results with and without firm effects, which mitigate the impact of attrition, and study survival effects directly as an outcome.

**Intent-To-Treat, Employment Persistence, and First Stage** Our DiD design is intent-to-treat as we assign firms into treatment and control firms based on pre-treatment employment in 1988, rather than on concurrent employment, which is an outcome potentially affected by the treatment.

In Figure 2, we gauge the persistence of this assignment.<sup>7</sup> In Panel (a), we show the evolution of the share of firms above the threshold, separately for the control firms (red line, hollow circles) and the treatment firms (blue line, solid circles). By construction, the shares are 0 and 1 in 1988, the base year in which we sort these firms by their employment. The graph then shows some convergence, such that by 1991, the onset of the reform, 5% and 64% of firms in the control and treatment groups are above the threshold and hence will be subject to a worker right to board-level representation. The gap between the two time series in the post-reform period captures the difference between the rates of treatment.

We also offer an instrumental variables (IV) interpretation with a formal first stage for scaling the reduced-form effect sizes we later report. Formally, the treatment is a worker right to shared governance, i.e.,  $D_{it} = \mathbb{1}[\text{Emp}_{it} \geq 150] \cdot \mathbb{1}[t \geq 1991]$ , which is a function of concurrent employment  $\text{Emp}_{it}$  (precisely, we will use end-of-year employment of the previous year, consistent with the practice we describe above). Note that this IV interpretation captures the effect of giving workers the right to shared governance (rather than capturing the effect of take-up), which can affect outcomes through threat effects even when workers do not exercise

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<sup>7</sup>This analysis draws upon the matched employer-employee data matched with the firm-level accounting data set. We display a parallel analysis in Appendix Figure A.3 in which we use the matched employer-employee data only and find similar results.

their statutory right to shared governance (see, e.g., Lewis, 1963; Farber, 2005). Precisely because of such potential threat effects, we would not want to interpret an IV specification with take-up of the institution as the endogenous variable. Figure 2 Panel (b) reports the year-specific coefficients from the difference-in-differences specification with this treatment indicator  $D_{it}$  as outcome—normalizing the difference to zero in 1990. Here, we find coefficients of about 0.6 in 1991 and coefficients stabilizing at or just below 0.4 starting in 1993. For the first stage that averages over the post-reform period through 1997, we find a coefficient of 0.454 (SE 0.028). Hence, an IV interpretation of our average reduced-form effects would roughly double them ( $1/0.454 \approx 2.20$ ). However, as we will find quantitatively small effects on most outcomes, even a doubling of effects would not substantively change our qualitative conclusions. Since the first-stage effect captures not just the persistence of the sorting but may also capture potential causal effects on employment, we focus on reduced-form effects below.

## 4 Worker Exit and Job Quality

Motivated by the influential framework distinguishing voice versus exit (Hirschman, 1970; Freeman, 1980), we estimate the effects of the reform-induced boost to worker voice on worker exit. We then turn to measures of job quality, namely a revealed-preference ranking of firms, worker health and workplace safety, and, finally, subjective evaluations of job quality.

### 4.1 Separations

Through various channels, worker voice is predicted to lower separations. A fundamental prediction of Hirschman (1970) is that improving stakeholders' voice will reduce their exit, by giving them a chance to change their institution from within. In our context, rather than taking amenities (or wages) as given as in, e.g., models of monopsony (e.g., Card, Cardoso, Heining, and Kline, 2018), workers can communicate their preferences and may directly affect workplace design. Moreover, separations serve as a standard catch-all revealed-preference measure of the relative attractiveness of an employer (see, e.g., Krueger and Summers, 1988). Finally, reducing *involuntary* layoffs is among the outcomes incumbent workers may value and, our surveys show, representatives prioritize.

**Overall Separations** We start by studying separations of any kind. To exposit our methodology, we report our specifications and robustness checks in detail for this outcome variable.

Our separations indicator takes our baseline employment definition, of the last day of the calendar year, and asks which fraction of workers are no longer with the same employer exactly one year later. This measure captures essentially permanent separations from the original employer. It comprises direct job-to-job transitions, which are more likely to reflect

voluntary worker quits, as well as separations into nonemployment, which are more likely to reflect involuntary layoffs. The baseline separation rate at the calendar year horizon in our firm and worker sample is 0.25 for our full worker sample in the treatment group in 1990, the year preceding the reform. In our plots, a year- $t$  separation rate denotes separations that occur in calendar year  $t$  among workers employed with their original employer on December 31st of the preceding, year  $t - 1$ .

We report results in Figure 3 Panel (a), the levels, and Panel (b), the year-specific DiD effects, and in Column (1) of Table 2, the DiD effect pooled over all post-reform years. We find that pre-trends are flat and that the separation rates in the treatment and control group track each other closely before—but also after the reform in 1991. In our most basic specification, the point estimate for the treatment effect in the post-reform period is -0.018 (SE 0.014). Since some of the post-reform period includes a recession, we additionally include year-specific industry effects, since industry exposure was a large mediating factor (see, e.g., Gorodnichenko, Mendoza, and Tesar, 2012, and Section 2.5). The point estimates remain stable at -0.013 (SE 0.014) effects, indicating that the recession is unlikely to affect the estimates. In our most fine-grained specification with industry-year and firm fixed effects (thereby controlling for selective attrition), we find a treatment, post-period effect of -0.029 (SE 0.013). The pooled pre-period estimate (1988 and 1989) relative to 1990 is -0.007 (SE 0.013), so that we cannot reject the parallel trends assumption underlying the identification assumption of our design in any of the specifications. Based on our preferred specification, we can rule out reductions in the separation rate of more than 0.055 and also reject zero at the 5% level. Compared to baseline separation rates (1990 control means reported in the table and can be read off the raw time series in Panel (a)), we thus find a reduction in separations of about 12% and can rule out effects larger than a 22% reduction. Overall, our point estimates are thus consistent with a small decline in separation rates.

*As with all other outcome variables,* we further assess the robustness by reporting on specifications with more local bandwidths or that exclude a set of observations around the 150-employee threshold. We report these results in Panels (a) and (b) of Appendix Figure A.9. At smaller bandwidths, potential biases from size-specific shocks may be less relevant; however, sample size falls, so standard errors increase (and the first stage would fall). We find very similar estimates when focusing on smaller bandwidths. We similarly find that our point estimates are robust to the exclusion of observations near the 150-employee threshold in “donut hole” specifications in Appendix Figure A.9 Panel (b).

**Job-to-Job Transitions** We now separately study job-to-job transitions, as proxies for voluntary separations and hence for the relative attractiveness of the employer.<sup>8</sup> Out of the

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<sup>8</sup>All separations not classified as job-to-job separations are into nonemployment. We track the original spell (which lasted through December 31st of the preceding year), and look for the end of the last spell with the

baseline any-separation rate of 0.225, job-to-job transitions occur at a rate of 0.097 (38.5%); the remaining transitions, 0.155 (61.5%), are employment-to-nonemployment transitions.

In Figure 3 Panels (c) and (d) and in Column (2) of Table 2, we report an effect of -0.012 (SE 0.012) in our basic specification and a similar point estimate when controlling for industry-year effects. From a baseline rate of 0.097, these effects imply around a 10% reduction in job-to-job transitions.

However, once we control for firm effects (i.e., our preferred specification), estimates are closer to zero, at -0.007 (SE 0.010), implying about a 7% reduction in job-to-job transitions. We can rule out small positive or negative point effects on job-to-job transitions outside of the 95% confidence interval spanning -0.027 and 0.013. We also find that effect sizes are robust to other bandwidths or varying donut holes (see Appendix Figure A.9).

As a quantitative benchmark of the implied increase in job quality that would correspond to a 7% decline in job-to-job separations, we draw on estimates of how firms' wage policies affect job-to-job transitions, as through the lens of a monopsony framework, in which separations are related to firms' relative wages. Bassier, Dube, and Naidu (forthcoming) estimate the elasticity of job-to-job separations to firm-level wage premia to be around 4 (based on the preferred estimates in Table 4 Columns (2) through (9) for EE separations). Inverting this elasticity implies that a 1.75% increase in a firm's wage premium would correspond to the 7% point estimate for the separations effect. In Section 4.2, we provide another quantitative interpretation of direct job flows as estimates of how workers rank different employers by revealed preference.

**Separations into Nonemployment** We also study separations into nonemployment, in Figure 3 Panels (e) and (f) and in Column (3) of Table 2. This outcome captures layoff risk or job stability. Of course, the effects mechanically correspond to the residual between overall separations and job-to-job transitions. Here, we find negative effects of -0.006 (SE 0.008) in the specification without controls and an estimate of -0.022 (SE 0.007) when controlling for industry-year and firm fixed effects. Since we include firm effects, this result is not driven by selective attrition of firms. The point estimates are robust to varying the bandwidth and the size of the donut hole (see Appendix Figure A.9 Panels (e) and (f)). Our estimates thus indicate small reductions in the probability of separations into nonemployment.

**Robustness to Alternative Separation Definitions** Our results are robust to two alternative separation rate definitions. First, in Appendix Figure A.5, we adjust for potentially spurious

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original employer in the calendar year under consideration. To account for potentially spurious and short gaps accompanying non-seamless direct job transitions, we permit up to a 30 day buffer of nonemployment; to avoid coding parallel spells ending at the same time as a job transition, we require that the next job last at least after the 31st day following the separation from the original job.

separations due to, e.g., relabeling of firm IDs for administrative reasons, or due to mergers and acquisitions. We identify clusters of employees separating from an origin firm and jointly moving into a destination firm in which they constitute the majority of employees (following the ideas in Hethey-Maier and Schmieder, 2013, see figure note for more details). In this check, we recode such exits as stays rather than separations. Second, to remove potential high-turnover workers that worker representatives may not represent as insiders, we also zoom into samples of employees with at least one year of tenure (Appendix Figure A.6), and of workers aged 20 to 55 (Appendix Figure A.7).

**Overall Assessment** Overall, the evidence points to small, if any, effects on job-to-job transitions, a revealed preference measure of job quality. This vantage point provides no evidence that the worker voice institution significantly increases job quality. We find evidence for small reductions of about two percentage points on annual separation rates into nonemployment, suggesting reduced layoffs and increased job security or entrenchment of workers.

## 4.2 Revealed-Preference Job Quality Based on Worker Flows

As a complement to our analysis on job-to-job separations, we study a revealed-preference measure of job quality that uses the quantity and direction of job-to-job transitions. Specifically, we draw on the extension of the PageRank algorithm to labor market flows by Sorkin (2018), who defines firm quality recursively such that “good firms hire from other good firms and have few workers leave.” We create an index of firm quality based on Sorkin’s methodology separately in the pre-reform period (1988-1990) and the post-reform period (1992-1997) and detail our implementation in Appendix D.1. We then report DiD effects on whether the treatment group firms increased their ranking relative to control group firms, comparing the post- to the pre-reform period.

We report results in Column (4) of Table 2 using the firm value (normalized to have zero mean and unit standard deviation in the pre-period) as the outcome variable. We find effects ranging between -0.043 (0.105) in our basic specification to -0.065 (SE 0.104) with controls for industry-year and firm fixed effects. The point estimates are thus close to zero and we can reject decreases (increases) in firm values of more than 0.27 (0.14) pre-period standard deviations. Studying alternative transformations of the outcome variable leads to similar conclusions (see Appendix Table A.5). In sum, we find no evidence for large increases in a revealed-preference measure of job quality, although confidence intervals do not rule out small effects in either direction.

### 4.3 Worker Health and Workplace Safety

We next focus on a measure that captures worker health and workplace safety—crucial job quality attributes, and a testable prediction of the long-standing hypothesis that worker representatives aim to improve health and workplace safety (see Freeman and Medoff, 1985, for US unions).

Our administrative outcome variable is sickness leaves (of more than ten working days, e.g., receiving benefits after a long illness or an accident). In the data, sickness leave benefits are coded in the same category as maternity leave benefits. To isolate primarily sickness rather than maternity leaves, we zoom into two subsamples of workers, those older than 40, and male workers. The firm-level outcome is the count of such spells normalized by employment in the respective worker subsample. The base rates are around 0.08 for both samples in our treatment group firms in 1990.

We report results in Columns (5) and (6) of Table 2 and in Appendix Figure A.8. For employees older than 40, we estimate a null effect of -0.002 (SE 0.003) across all specifications. We also find an economically small violation of the parallel trends assumption in the pre-period, marginally statistically significant, when not controlling for industry-year fixed effects. Zooming into the male sample, we find a similar small negative effect of -0.001 (SE 0.003). Overall, we thus do not find a significant reduction in measures of employee sickness or accidents, and can rule out even small improvements of this measure of worker health and safety.

### 4.4 Survey-Based Subjective Job Quality and Labor Relations

We close our analysis of job quality with subjective measures reported directly by workers. We draw on the Finnish Quality of Work Life Survey, and merge it to our administrative data. We conduct difference-in-differences analyses drawing on the 1990 wave and the first post-reform wave in 1997. As the survey is not a panel and is a sample of firms, we focus on contemporaneous employment as the assignment variable and do not impose size-based sample restrictions. The survey samples a randomly drawn worker in the firm (aged 15 to 64). Hence, we cannot assess pre-trends as we cannot link the pre-1990 surveys to administrative data on firm size. Finally, we caveat that worker representation may change reference points for job quality, in addition to standard concerns about subjective assessments.

We draw on multiple underlying survey items and use factor analysis to extract one underlying factor based on a weighted average of individual survey items, turned into a z-score with zero mean and unit standard deviation using the post-reform control group mean and standard deviation (in 1997). Our goal is to reduce the number of outcomes we study by focusing on a summary index. We detail the procedure and the individual variables

in Appendix D.2.

**Subjective Job Quality** For our analysis of subjective job quality, we draw on 21 survey items, such as whether problems at work make it hard to focus at work, whether one's supervisor is supportive and encouraging, and the relative importance of the content vs. the salary associated with the job (all variables listed in Appendix D.2). We report results for this job quality index in Column (4) of Table 2. We find a moderate increase of the job quality index in 1997, of 0.182 (SE 0.084) and of 0.146 (SE 0.088), for baseline and industry-year controls respectively (we cannot include firm effects due to the repeated cross-section nature of the survey). Overall, the survey points towards small to moderate increases in perceived job quality as a consequence of granting workers a right to formal worker voice and shared governance.

**Labor Relations** We create a labor relations index based on 10 survey items, e.g., asking workers about conflicts between management and employees or the timing of receiving information about changes in work tasks (all variables listed in Appendix D.2). Column (5) of Table 2 reports a small, statistically insignificant increase the labor relations index, of 0.063 (SE 0.083) or 0.063 (SE 0.089) with industry-year effects. We thus do not find that, from the perspective of worker respondents, labor relations have improved. However, since the survey samples a randomly drawn worker, it need not speak to the perceptions of worker representatives.

## 5 Wages, the Wage Structure, and the Labor Share

We now study whether the worker voice institution affects wages, the wage structure within the firm, rent sharing, and the labor share. First, wages are an important attribute of job quality, and hence of direct interest for the question of whether worker voice benefits workers. Second, analyzing wages completes our interpretation of the absence of large separation effects. Specifically, perfectly compensating differentials, between firms with and without worker voice institutions, would lead wages to fall if worker voice raised non-wage amenities, such that overall job quality differentials and separation rates remain unchanged. Third, even though the institution provides no veto rights, relatively few formal decision rights, and gives firms various paths to curbing worker power, it may also entail a formal boost of workers' bargaining power even in wage setting.

**Mean Wages** We start by studying the firm-level mean of raw worker-level log wages, and report results in Figure 4 Panels (a) and (b) and Column (1) of Table 3. Pre-trends are parallel.

We find a positive effect of 0.033 (SE 0.016) in our basic specification, and a slightly smaller estimate of 0.024 (SE 0.012) in our most fine-grained specification.

**Isolating Firm and Worker Pay (AKM) Premia: Composition Adjustment** Wage effects could reflect shifts either in firms' wage policies, or in worker composition. To estimate firms' pay premia, we use Abowd, Kramarz, and Margolis (1999) (AKM) regressions of worker-level log wages including firm effects (wage policies) and worker fixed effects (capturing the permanent earnings potential of a worker) and cubic controls for potential experience interacted with education groups (secondary, vocational, university degrees). We estimate AKM specifications in rolling three-year windows and use observations from  $t$ ,  $t + 1$ , and  $t + 2$  to calculate outcomes to associate with period  $t$ .

We report results in Figure 4 Panels (c) and (d) and Column (2) of Table 3. In our basic specification, we find that the effect on the AKM pay premium is about a third lower than the effect on mean log wages and is statistically significant at 0.019 (SE 0.009). However, in all of the specifications with control variables, we find smaller point estimates that are only marginally significant. In our preferred specification, we find a point estimate of 0.016 (SE 0.010). The confidence interval allows us to reject effects above 0.036 or -0.004. Overall, the institution thus did not strongly boost workers' wage premia, but our confidence intervals would accommodate small increases of less than 3.6 percent.

**Within-Firm Wage Structure** A long-standing hypothesis in the literature posits that worker representation may compress the wage distributions (as in the case of US unions, see, e.g., Freeman and Medoff, 1985; Western and Rosenfeld, 2011; Farber, Herbst, Kuziemko, and Naidu, 2018). We study effects on deciles of the within-firm wage distribution and report pooled post-period DiD estimates in Figure 4 Panel (e), as well as in Appendix Table A.2. Point estimates reveal pay increases of around 0.05 to 0.07 at the lower end of the within-firm wage distribution, which diminish, roughly linearly, to around 0.01 at the 90th percentile. Overall, while noisily estimated, the evidence appears consistent with some additional pay compression within the firm resulting from worker voice and any wage increases largely concentrated at the lower end of the within-firm wage distribution.

**Executive Compensation** Since Finnish boards set executive compensation, we now ask whether having workers on the board influences the firms' wage structure at the very top. Executive pay can reflect agency issues (Bertrand and Mullainathan, 2003), which worker representation may curb or exacerbate. Alternatively, executive pay setting can be viewed as a bargaining problem between labor and capital, or within labor (Edmans and Gabaix, 2016; Piketty, Saez, and Stantcheva, 2014).

We identify executives of a company using the board-level data set, as often the highest-level executives are not formally employees. We then obtain the total compensation of that executive (including bonus payments) in the matched employer-employee data. Importantly, unlike in other administrative data sets in which studying executive pay is not possible (e.g., in Germany, as in Jäger, Schoefer, and Heining, forthcoming), compensation in the Finnish data is not capped at a social security maximum. Appendix D.3 describes this variable construction in detail.

As the rightmost estimates in Figure 4 Panel (e) and Columns (10) and (11) of Appendix Table A.2 show, we report essentially a zero effect on executives' log compensation (-0.004, SE 0.035). As a second compensation concept we add to labor income *all* capital income from any source—which, however, need not come from the executive's employer. For this broader but more tentative compensation measure, the point estimates are small, positive and statistically insignificant (0.053, SE 0.041). We thus do not find strong effects on executive compensation in either direction.

**Rent Sharing** We next directly study rent sharing elasticities, capturing potential shifts in the wage setting process indicative of higher worker bargaining power. We focus on the cross-sectional relationship between firm-level composition-adjusted wage (AKM) policies and labor added per worker, using the typical log-log specification. In split-the-surplus rules like Nash bargaining, the pass-through of productivity into wages identifies the bargaining parameter (as in, e.g., Jäger, Schoefer, Young, and Zweimüller, 2020), which is hypothesized to increase following boosts to worker authority in corporate decisions (Grout, 1984; Jäger, Schoefer, and Heining, forthcoming).

In Figure 4 Panel (f), we plot pay premia (firm AKM fixed effects) against average log value added per worker (controlling for industry-year fixed effects to isolate *firm-specific* surplus shifts). For the control firms, the baseline rent sharing elasticity is 0.063 (SE 0.006) (in line with although in the lower end of estimates in other settings, as reviewed in, e.g., Jäger, Schoefer, Young, and Zweimüller, 2020). We find a statistically insignificant treatment effect of 0.017 (SE 0.012). Specifically, the point estimate implies that a hypothetical 10% increase of firm-specific labor productivity compared to its industry peers would only raise wages by an additional 0.17% with worker representation. The estimates permit us to rule out moderate boosts to wages from this channel.

**Labor Share** A potential reason for the absence of wage effects is that while workers grab a larger share of the pie, the pie may shrink due to less efficient production or discouragement of investment. We study effects on firm performance direction below in Section 6. We directly study the effect on the firm-specific labor share, calculated as the wage bill divided by value added, in Table 3 Column (3). We find small negative and statistically insignificant effects on

the labor share with estimates of -0.010 (SE 0.014) in our basic specification and -0.022 (SE 0.014) in our preferred specification, allowing us to rule out even small increases in the labor share by more than 0.5 percentage points. (The average labor share in our treatment group is 0.576 in 1990).

**Summary** In sum, we have found only small positive, if any, effects on wages as a consequence of granting workers a right to shared governance. Our estimates leave room for small increases in pay premia as well as evidence consistent with a small amount of pay compression. The point estimate in our preferred specification is 1.6 percent and confidence intervals permit us to rule out shifts in firms' wage policies above 3.2 percent. The positive point estimates, indicating small wage increases, are within the range of estimates in the reform-based DiD design building on the repeal of board representation in some new firms in Germany (Jäger, Schoefer, and Heining, forthcoming), and smaller than the estimates in an important early industry-based design studying the introduction of codetermination in Germany (Svejnar, 1981). For RD studies with firm-level employment as the running variable, there is a broader range of estimates, between -0.031 (Kim, Maug, and Schneider, 2018) and 0.066 (Redeker, 2019) for parity codetermination in Germany and -0.009 for codetermination in Norway (Blandhol, Mogstad, Nilsson, and Vestad, 2020). We include a cross-sectional RD design for our context in Appendix C, but build on our reform-based DiD design as our main research design.

## 6 Firm Performance

While effects on job quality from the *workers'* perspective appear limited, a large body of literature posits that boosting worker voice may increase productivity, by lowering turnover (Hirschman, 1970; Freeman, 1980) or by facilitating information flows and cooperation. Information sharing may reduce information asymmetries underlying coordination and contracting problems (Grossman and Hart, 1981; Malcomson, 1983). Shared governance may attenuate hold-up of workers by firms, as with firm-specific training or back-loaded compensation. Worker representatives could also apply more stringent monitoring, if executives wield more influence over shareholder directors than over worker directors (Hermalin and Weisbach, 1998), and worker representatives could curb managers' short-termism if their horizon is longer than that of outside shareholder directors. On the other hand, several theories also posit adverse consequences for firm performance, for example through hold-up or entrenchment of workers (Jensen and Meckling, 1979).

**Survival** As the basic measure of firm performance, we analyze effects on firm survival. This analysis also investigates potential attrition, as our remaining firm performance outcomes

are conditional on survival.

We report results in column (4) of Table 3 and in Panels (a) and (b) of Figure 5, where we plot the time series of the share of the firms surviving from 1988. Panel (a) shows the raw cumulative survival fractions, equal to one in 1988 by construction. The lines for the treatment and control groups lie on top of each other in the pre-reform periods, validating the pre-trends assumption. This remains true in 1991, when the reform takes action, implying no immediate survival effects. Starting in 1992, a small gap opens up between the treated and the control firms, indicating a positive effect on firm survival. Panel (b) plots the corresponding DiD regression estimates by year. The pre-period effects are insignificant and close to zero, supporting our parallel trends assumption. The post-period effects starting 1992 are positive, with an effect of 0.035 (SE 0.022) for the pooled post-reform observations without controls and 0.037 (SE 0.021) when controlling for industry-year effects (results are identical when controlling for firm effects since the panel is by construction balanced for this outcome variable). We report results of robustness checks in Appendix Figure A.12.

Overall, the estimates allow us to rule out effects on survival below -0.4 percentage points for our preferred specification. That is, the worker voice institution does not appear to affect firm survival negatively; if anything, the point estimates indicate a marginally significant positive effect. (We note that any composition effects through potentially selective firm survival would not mechanically affect our estimates of effects on other outcomes in the specifications with firm effects.)

**Labor Productivity** Our second measure of firm performance is labor productivity, i.e., log value added per worker. In our DiD framework and due to our inclusion of industry-year fixed effects, the effects also correspond to shifts in the marginal product of labor for instance with Cobb-Douglas production. We report results in Table 3 Column (5) and Figure 5 Panels (c) and (d). In our baseline specification, we document a positive but statistically insignificant effect of 0.043 (SE 0.035). Confidence intervals permit us to reject effects below -0.028 and above 0.106. With firm effects, we find slightly higher and statistically significant effects of 0.067 (SE 0.031). In sum, our evidence suggests small increases in labor productivity.

**Capital Intensity** With a Cobb-Douglas production function, effects on value added per worker could reflect shifts either in TFP or in the capital-labor ratio. These outcomes could even move in opposite direction, as, e.g., TFP may increase due to increased information sharing (Freeman and Medoff, 1985) while disinvestment lowers the capital-labor ratio (Grout, 1984; Jensen and Meckling, 1979; Jäger, Schoefer, and Heining, forthcoming).

Considering fixed assets as our capital proxy, we report effects on the capital-labor ratio in Column (6) of Table 3. Our basic specification gives positive effect of 0.099 (SE 0.078), which decreases to 0.035 (SE 0.048) with industry-year and firm fixed effects. These, if anything,

positive effects on capital formation are inconsistent with the disinvestment predicted by the Jensen and Meckling (1979) hold-up view (and consistent with findings in Jäger, Schoefer, and Heinrich, forthcoming). The point estimates can, in a back-of-the-envelope calculation, on their own account for a significant share of the increase in the labor productivity.<sup>9</sup> With standard errors of 0.048 in the specification with industry-year and firm fixed effects, the effects are however more noisily estimated than our effects on, for instance, worker-level wages. The 95% confidence interval allows us to rule out negative effects below -0.059. The upper bound permits us to rule out positive effects above 0.129, implying that the capital intensity effects could account for a substantial part of the labor productivity effect.

**An RD Design: Investment** We have also experimented with capital expenditure (investment) as an outcome variable, which is available in Finnish data only starting in 1994, and hence cannot be studied in our DiD design. However, it could be studied in a regression discontinuity (RD) design. In Appendix C, we report an RD design for log capital expenditure as an outcome and also report RD results for our other outcome variables. We find positive albeit imprecisely estimated effects. However, we do not interpret these coefficients, because the RD design is not a compelling setup as (i) there need not be a permanent policy discontinuity at 150 employees (due to firms above/below the cutoff moving in and out of transitory treatment, due to lagged or anticipation effects), (ii) the running variable is not sharply defined due to some discretion in the employment measure, and (iii) due to concerns of firm selection around the cutoff. These concerns have motivated our reform-based DiD design in the first place.

**Total Factor Productivity** We next study log total factor productivity (TFP).<sup>10</sup> We report these effects in Table 3 Column (7). In our basic specification, we find negative point estimates of -0.038 (SE 0.060). With industry-year and firm fixed effects, we find a marginally significant point estimate of 0.063 (0.034). The 95% confidence interval ranges from -0.004 to 0.130, such that our labor productivity effects could be driven total factor productivity increases.

**Profitability** We close by studying the capital side of income by measuring effects on profitability. We have already documented in Section 5 that the labor share, if anything, marginally decreased. We now study a measure for the profit margin, which we define as net income (i.e., earnings after depreciation, interest, and taxation) divided by revenue. Across specifications, we find estimates close to zero. We find a precisely estimated effect of

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<sup>9</sup>With Cobb-Douglas production, a percent shift in the capital-labor ratio entails a percent shift in value added per worker adjusted for the capital share, which is 0.424 in our sample if calibrated to one minus the labor income share.

<sup>10</sup>We assume a Cobb-Douglas specification and measure the 2-digit industry-level factor shares as the ratio of total payroll divided by total value added among firms with both variables being nonmissing, for each year.

0.006 (SE 0.008) which decreases to -0.001 (SE 0.008) when we include firm fixed effects and industry-year fixed effects. Our results thus indicate no (negative) effects on profitability.

**An RD Design: Dividends** The Finnish data has dividend data available only starting 1994, such that we cannot study dividends in our DiD design, as with the capital expenditure data. In Appendix C, we report an additional RD design for log dividends and log dividends over revenue. We find positive albeit imprecisely estimated effects. Again, we do not interpret these coefficients further due to the lack of credibility but include the estimates for the sake of completeness.

**Revealed-Preference Evidence from Bunching At 150 Threshold** As a revealed preference complement to our analysis of profitability, we implement a test of whether firms avoid being subject to the 1991 law by changing their size such that they are just below the size threshold of 150 employees. We report the density of firm size around the policy threshold, both before and after the 1991 reform, in Appendix Figure A.14. We find no visual evidence that firms bunch below the 150-employee threshold. Formal McCrary (2008) tests do not reject continuity of the density at the policy threshold either. We thus find no evidence that firms avoid being subject to the codetermination law (as they would have if shared governance imposed net costs, as in response to other labor regulations or tax incentives studied in Garicano, Lelarge, and Van Reenen, 2016; Benzarti and Harju, forthcoming).

**Overall Assessment** We find limited effects on margins of firm performance, suggesting that worker voice did not measurably lower firm performance, with point estimates pointing towards a positive direction for survival and labor productivity. In addition, our confidence intervals put tight bounds on the rent-extraction and agency cost views of worker involvement in corporate decision making.

## 7 Similar Effects From Alternative Codetermination Institution: 2008 Expansion of Shop-Floor Representation in Small Firms

One potential explanation for the limited effects of the 1991 introduction of worker voice through board-level representation and other negotiated firm-level variants is that the ensuing representation may simply duplicate (or be weaker than) a pre-existing codetermination institution, shop-floor representation. We have described this institution in Section 2.3. We estimate the effects of shop-floor representation on the same set of outcomes, leveraging a 2007

reform that made this labor representation mandatory in certain firms starting in 2008. More broadly, our evaluation of this reform provides a quasi-experimental design that may speak to the effects of this kind of shop-floor codetermination institution, akin to works councils in other countries, an area where causal estimates from quasi-experimental designs or sharp RD designs are lacking (see Addison, 2009, for an overview of existing studies of works councils in Germany).

**The Reform** Figure 6 Panel (a) visualizes the reform. Before 2008, the Cooperation Act mandated shop-floor representation in firms with at least 30 employees. A 2007 law change lowered the threshold to 20 employees.<sup>11</sup> The impetus was an effort to unify the previous versions of the law and promote interactive cooperation between the employer and the employees (HE 254/2006), as well as compliance with the European Commission Directive on informing and consulting employees in the European Community (2002/14/EC). Employer associations opposed the reform on the grounds that it would hobble small firms with bureaucracy (see, e.g., Suomen Yrittäjät, 2007, for a statement by the Federation of Finnish Enterprises). To our knowledge, there exists only one empirical study of the reform, a difference-in-differences analysis in an important master's thesis by Keskinen (2017), which we build on and expand with a wider range of outcomes and visual analysis of raw data.

**DiD Design** We follow an analogous DiD strategy as for the 1991 reform. We track a cohort of firms based on their pre-reform employment in 2005, again three years before the law change, and estimate effects relative to 2007, the baseline year before the reform becomes active. The treatment group comprises firms with 2005 employment between 20 and 29. We track two control groups separately: firms with 2005 employment of 10 to 19, and of 30 to 39.

**Results** Figure 6 Panel (b) reports a first stage for the firm being subject to the shop-floor representation mandate. For each of the three groups, it plots the average of the firm-level indicator of employment of at least 30 (if before 2008) or at least 20 (starting 2008, post-reform). We see a considerably sharper relative increase in the treatment group compared to either control group, especially the smaller firms, indicating substantial employment persistence.

In the other panels, we report time series for key outcome variables, for job-to-job transitions (Panel c)), mean log wages (Panel (d)), firm survival (Panel (e)) and labor productivity (Panel

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<sup>11</sup>To lighten the administrative burden, the law does not extend all rights to shop-floor representatives in firms with 20 to 29 employees; for example, they only have information rights upon request, and only firms with 30 or more employees are, for example, required to negotiate on recruiting details and gender equality plans. The reform also led to some minor changes in firms with 30 or more employees. In some cases, collective bargaining agreements prescribed the presence of shop-floor representatives in smaller firms not subject to the law. Surveys suggest that nearly 50% of firms in the 20 to 29 employee size category did not have shop-floor representation in 2007, so the law change applied to large share of those firms (see Suomen Yrittäjät, 2010, Figure 2).

(f)). In each panel we print the DiD estimates pooled for 2008 to 2013 (to mirror the time horizon as in the 150 reform, although the graphs show effects through 2017 for illustration), separately estimated against the smaller and larger firms as controls. Throughout, we find that the groups move in parallel pre-reform, and continue on those parallel paths from 2008 onward. The time series of raw data provides transparent visual evidence that the expansion of this additional dimension of worker voice did not result in larger effects than the 1991 reform. Appendix Tables A.3 and A.4 report the regression results for these as well as all other outcomes. Overall, effects are small and precisely estimated, with the strongest and statistically most significant effect being a positive one on the subjective labor relations quality index, again a positive point estimate on subjective work quality, and interestingly a small negative effect on AKM firm effects.

**Implications** The 2008 expansion of shop-floor codetermination also had at best small effects on worker and firm outcomes. We draw two conclusions. First, the baseline presence of shop-floor representation in our main analysis sample of firms is unlikely to explain the absence of large effects of board-level and alternative representation following the 1991 reform, as could have been the case if these two worker voice institutions had been substitutes. Second, as a substantive result in its own right in an area where causal estimates have been elusive, we find that shop-floor representation as an independent worker voice institution has similarly limited effects as the higher-level representation induced by the 1991 reform.

## 8 Discussion, Interpretation, and Interview Evidence

Overall, we have found that the boost to worker voice has not translated into substantially higher job quality from the perspective of workers. While we have found some evidence for moderate reductions in nonemployment separations, increases in subjective measures of job quality, and a small reduction in within-firm inequality, our estimates allow us to rule out even relatively small effects on most outcomes pertaining to job quality or surplus. Most importantly, we have found no reductions in job-to-job separations, which is where the rubber meets the road for the exit-voice theory. On the firm side, our estimates suggest small positive effects on survival and on labor productivity, but these need to be contrasted with largely zero effects on most outcomes and confidence intervals excluding even moderate positive or small negative effects.

We close our paper by reflecting on the nature of the institution we study and discussing two potential explanations for its limited effects. To guide our interpretation, we draw on surveys of board-level and shop-floor representatives, and organize our discussion by distinguishing between two broad channels of worker voice (see, e.g., Freeman and Medoff,

1985; Kochan, Yang, Kimball, and Kelly, 2019). By shifting *power*, worker voice allows workers to express and defend their interests against managers or owners, and (with formal decision rights) directly affect corporate decision making. Through the *information channel*, worker voice gives workers an avenue to communicate feedback to managers and owners and vice versa. The information channel has also been hypothesized to facilitate job security through better monitoring and enforcement of implicit contracts (Malcomson, 1983).

**Worker Power** One potential explanation of the limited magnitude of the effects of the 1991 reform is that it provided too small a boost of workers' power in corporate decision-making to substantially move the needle on core economic outcomes. There are three patterns consistent with this interpretation.

First, indicative of limited worker power conveyed by the law, only about 60% of above-threshold firms appear to adopt any form of worker representation as a result of the reform. Table 1 clarifies that the most common reason for failing to adopt worker representation is that the employer opposed it—presumably reflecting employers bargaining away or simply blocking the institution, perhaps particularly in instances where it would interfere with their interests and/or have negative effects. Thus, employers appear to hold significant power over workers' ability to take up their statutory rights. In this respect, Finland differs from other contexts such as Germany, where board-level representation is mandatory rather than a right (Jäger, Schoefer, and Heining, forthcoming).

Second and relatedly, even in firms that do adopt worker representation, worker representatives' real authority may be limited; after all, workers get only 20% of the seats on the board according to the statutory default, a low figure compared to, e.g., 33 and 50% in Germany (Jäger, Schoefer, and Heining, forthcoming). Additionally, employers can choose the modality of shared governance—which board workers will be involved in, if following the statutory provisions. When asked what impact would result from the institution, one respondent in our 2020 survey says bluntly:

The employer always has a majority. No direct effect.

Employers can also avoid this statutory default of board representation by bargaining employees down to even weaker forms of representation, such as advisory councils without any explicit decision-making power. We discussed these institutional details in Section 2. Consistent with this view, our paper enters an active policy debate in 2021 about a potential strengthening of the institution (see, e.g., Työ-ja Elinkeinoministeriö, 2020, a companion paper that builds on our survey evidence).

Third, representatives' self-assessed influence is relatively low. In our own 2020 survey of worker representatives, there is no area of decision-making where a majority of respondents claim they have an impact (Figure 7 Panel (b)). Between 35 and 50% report that "managers

are not willing to give workers a voice” or that “managers listen [to advice] but cannot take it into consideration” (Figure 7 Panel (c)). In the 2001 survey of shop-floor representatives, 76% say that worker representation “feels like a formality,” and 65% believe the rights of worker representatives are too narrow (Figure 7 Panel (d)). In our own 2020 survey of worker representatives, respondents state that “Our forums are not ‘real’ places of government” and “Management is likely to ignore the administrative representative and try to water down his position as much as possible.” Another respondent illustrates this situation in response to our open-ended questions:

[...] management has, in fact, already decided the course of action at the stage when I become aware of it. At that point, it is virtually impossible to influence the big lines anymore; maybe you can say your words and negotiate on some details.

Similarly, another respondent remarks that

I have a feeling that I cannot influence the company’s decisions [...] things cannot be influenced before decisions are made, they are only information about decisions already made.

Overall, in our 2020 survey, a majority of respondents believe that the institution does not affect profits, investment or wages (Figure 7 Panel (e)). While we were able to find some anecdotes in which representatives report having had concrete impact, and while worker representatives list ambitious goals such as preventing layoffs, raising wages, and improving working conditions among their key priorities (Figure 7 Panel (a)), our DiD estimates reveal that on average, their influence is limited.<sup>12</sup>

**Information Sharing** Even though the institution may not have boosted workers’ real power or distributed significant rents towards labor, it could have had an effect on firm and worker outcomes through an information-sharing channel, which is at the core of the influential exit-versus-voice framework of Hirschman (1970). Specifically, worker voice may improve firm performance by reducing worker turnover, improving task organization, raising social capital, cooperation, and trust, and helping managers take preventive action (see, e.g., Freeman and Medoff, 1985; Ichniowsky, Shaw, and Prennushi, 1997; Gant, Ichniowski, and Shaw, 2002; Bryson, Charlwood, and Forth, 2006; Böckerman, Bryson, and Ilmakunnas, 2012; Peutere, Saloniemi, Böckerman, Aho, Nätti, and Nummi, forthcoming).<sup>13</sup> The existing evidence rests either on documenting demand for worker voice (see Bryson and Freeman, 2013; Kochan,

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<sup>12</sup>For instance, a worker board representative of a shipbuilding company in Turku describes concrete examples of how he acts as an intermediary between the board and the workforce and exerts influence over decisions regarding production, workplace safety and environment, or outsourcing (Gold, Kluge, and Conchon, 2010).

<sup>13</sup>At a broader level, by boosting perceptions of the procedural justice of managers’ choices, worker voice may increase loyalty and commitment to tasks (Folger, 1977; Earley and Lind, 1987; Lind, Kanfer, and Earley, 1990).

Yang, Kimball, and Kelly, 2019, and references therein) or on correlational studies (Freeman and Kleiner, 2000; Batt, Colvin, and Keefe, 2002; Ng and Feldman, 2011), which (as authors in the literature frequently acknowledge) may be confounded by worker sorting and subject to other empirical challenges (as documented by, e.g., Morrison, 2011; Sluiter, Manevska, and Akkerman, forthcoming).<sup>14</sup>

Three patterns suggest that the worker voice institution we study can be understood as an information sharing platform.

First, in many instances, firms implement the institution in the form of a pure information exchange and advisory platform, as we discussed in Section 2, a unique feature of the Finnish context.

Second, many representatives view facilitating information flows as their main function—rather than wielding real authority, which we considered above. In our in-depth interviews covering representatives from five major companies, all five representatives were ambiguous about whether they actually possess influence, but they *all* mention information sharing as one of the ways they can contribute.<sup>15</sup> Three out of the five explicitly say that the primary function of a worker representative is to facilitate information sharing, with the first interviewee stating:

The body where I work is [...] really a way for the company to share information.  
[...] Providing information is our main task, and we can't make any decisions,  
everything comes already decided.

The second interviewee stated:

I personally think that the role of an administrative representative is to convey  
information [...]

A third one states:

It also often feels that the members of the management group want to talk to me  
because they feel that they are separated from the employees and want to hear my  
opinions. [...] I feel that I am a link between the employees and the management  
group.

A fourth interviewee says that:

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<sup>14</sup>As an exception, Jäger, Schoefer, and Heining (forthcoming) study a reform-based quasi-experiment in Germany. Blandhol, Mogstad, Nilsson, and Vestad (2020) study moves of workers into firms with and without worker representation, firms adopting it, as well as firms above and below size thresholds (see also Kim, Maug, and Schneider, 2018, and others for Germany). Keskinen (2017) analyzes the 2008 reform of shop-floor representation in Finland. Ong, Riyanto, and Sheffrin (2012) and Adhvaryu, Molina, and Nyshadham (2019) present lab and field experiments giving subjects the opportunity to give feedback or not.

<sup>15</sup>We found little support for the idea of procedural justice. Two respondents in our 2020 survey stated that “There was also a notion that ‘The staff feels better taken into account in the company’”, and that “Transparency increases.”

[...] I can bring the personnel's thoughts and ideas to the management team very freely. And bring different types of thinking from employees.

Moreover, a fifth representative believes that information sharing can indeed affect outcomes:

[The role] improves information flow in the company, and giving people access to information makes it possible for them to influence matters.

Similarly, in our 2020 survey, open-ended responses about the impact of representation included "Improving trust through cooperation and a better atmosphere for discussion," "Improved collaboration and communication between staff and managers," and "There will be more open discussion." Notably, none of the respondents point to concrete changes to company policy as a likely impact of worker representation, except for changes in the organization of work or benefit programs.

Third, even the small number of worker representatives who report influencing company policy refer to the information sharing enabled by the institution as a primary mechanism that allows them to wield influence. For example, Gold, Kluge, and Conchon (2010) conduct an extensive interview with a board-level worker representative for a Finnish shipbuilding company, who explains that he was only able to influence the allocation of surplus money from the company pension fund because he heard about the surplus through his position on the board. He also describes that while he does not directly participate in worker pay negotiations, his knowledge of the firm's economic situation acts as an input into the negotiation strategy of the shop-floor representatives.

Concerning the information channel, our findings have two interpretations. One possibility is that the reform led to an increase in information sharing—which, however, did not have large effects on the core economic outcomes we study, i.e., firm performance and job quality and wages. Our positive point estimates for labor productivity and capital intensity leave some room for those theories. Similarly, our evidence on small reductions in separations into nonemployment is consistent with models of implicit contracts, in which better information flows increase employment security (Malcomson, 1983; Freeman and Lazear, 1995). Alternatively, information exchange in the firm may happen extensively even absent formal representation from board representation or shop-floor representation. Indeed, Finnish managers in small firms (at most 49 employees, below the reform threshold) and large firms (250 employees and more, above the reform threshold) consult/involve workers in some (formal or informal) capacity with roughly equal frequency (77% vs. 82% for consultation, 66% vs. 68% for involvement in decision making), where we draw on the 2013 European Companies Survey.<sup>16</sup> We also surveyed human resource managers in firms with and without

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<sup>16</sup>A moderate gap in worker consultation/involvement emerges comparing firms with and without formal employee representatives in the cross section (80% vs. 64% for consultation, 68% vs 55% for involvement),

worker representation, and most managers in the latter group mentioned informal channels through which they directly consult workers.<sup>17</sup>

**Overall Conclusion** Our quasi-experimental design studying the size-based introduction of shared governance in Finnish firms reveals that board-level minority representation of workers, and alternative negotiated implementations of worker voice, have at best small positive effects on core economic outcomes in the dimensions of job quality and security, wages and firm performance, putting some bounds on the positive effects on job quality predicted by theories of worker voice (Hirschman, 1970; Freeman, 1980). At the same time, we can largely rule out small detrimental effects across many outcomes, including firm performance, wages, and the labor share, in contrast to a view of codetermination as a rent-extracting institution (Jensen and Meckling, 1979).<sup>18</sup> Our preferred interpretation is that the institution largely worked through increased information sharing and cooperation, but did little to shift workers' real power in corporate decision-making. Our quasi-experimental design studies firm-level variation, and thereby naturally leaves open the important question of potential spillovers or equilibrium effects through norm setting or market interactions, among the control firms.<sup>19</sup>

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although the latter group make up only a small fraction in Finland due to the broad definition of representation in the question.

<sup>17</sup>For example, managers mentioned “messages in Yammer [an online platform] or other direct communication,” “committees,” and “questionnaires” as ways that workers could directly express their voice.

<sup>18</sup>The absence of negative effects on firm performance is also consistent with findings in Germany (Jäger, Schoefer, and Heining, forthcoming). Moreover, absence of wage effects in the Finnish (and German) contexts are also consistent with the study of wage effects in Blandhol, Mogstad, Nilsson, and Vestad (2020), which features a unitary board structure, as is predominant in Finland.

<sup>19</sup>Jäger, Noy, and Schoefer (2021) review the existing literature on codetermination and do not find evidence for equilibrium effects of codetermination reforms in country-level difference-in-differences designs.

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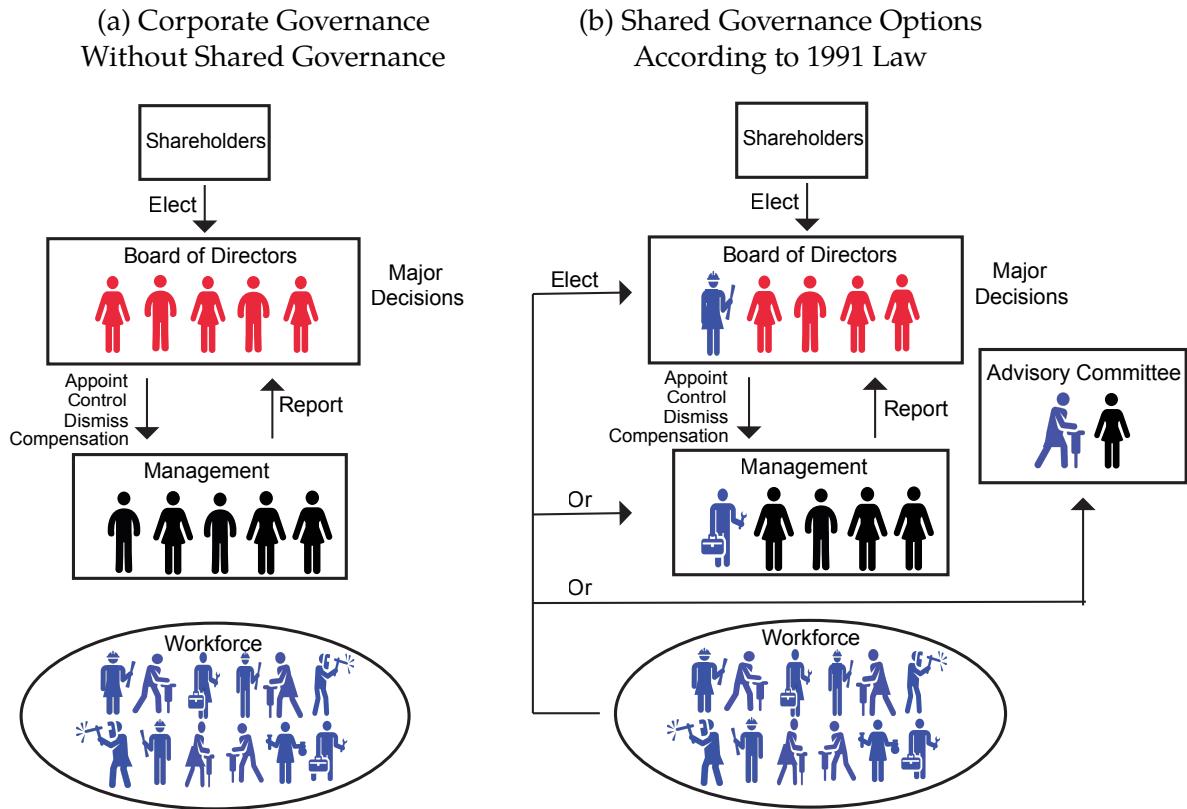
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# Figures

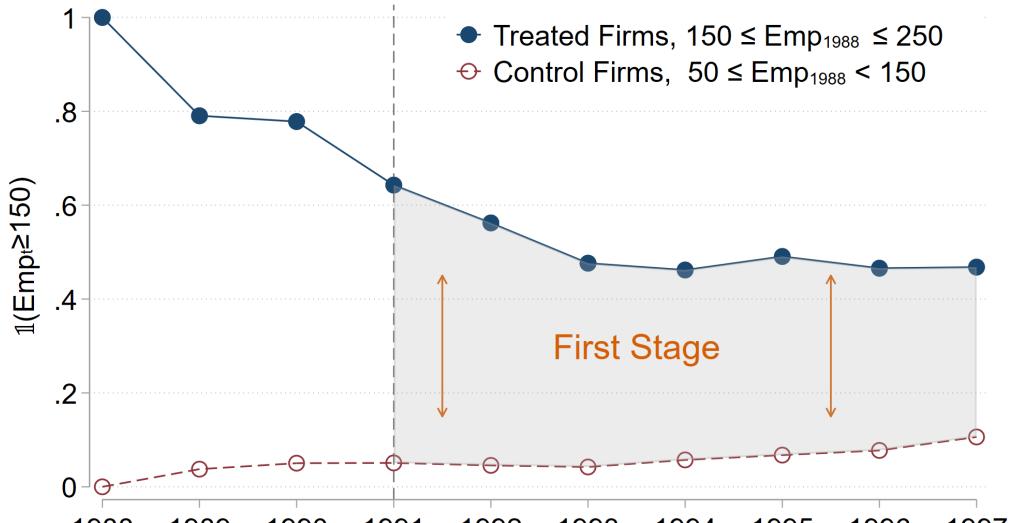
Figure 1: Corporate Governance and Worker Representation Options According To 1991 Reform



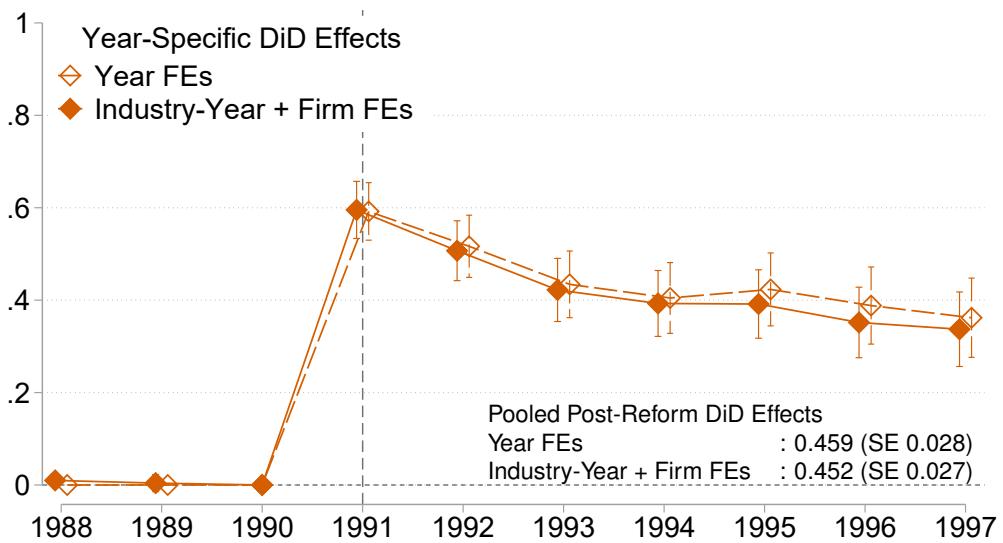
*Note:* Panel (a) illustrates the governance structure of a Finnish firm with a unitary board structure and without worker representation, which applied to firms before 1991 as well as to firms with fewer than 150 employees post-1991. Panel (b) illustrates the governance structure under the statutory provision of the codetermination law. We illustrate both the cases where workers exercise their statutory right to elect representatives to either the board of directors or the firm's management group as well implementation of worker representation in an advisory committee. Under the statutory provisions of the law, the firm can choose whether 20% worker representation occurs on the board of directors or as part of the management group. Worker representation is also often organized through an agreement stipulating an advisory committee, illustrated on the right. We do not illustrate the less common case of a dual board structure and the corresponding option for worker representation on the supervisory board.

Figure 2: Persistence of Treatment Assignment

(a) Fraction of Firms with Employment  $\geq 150$

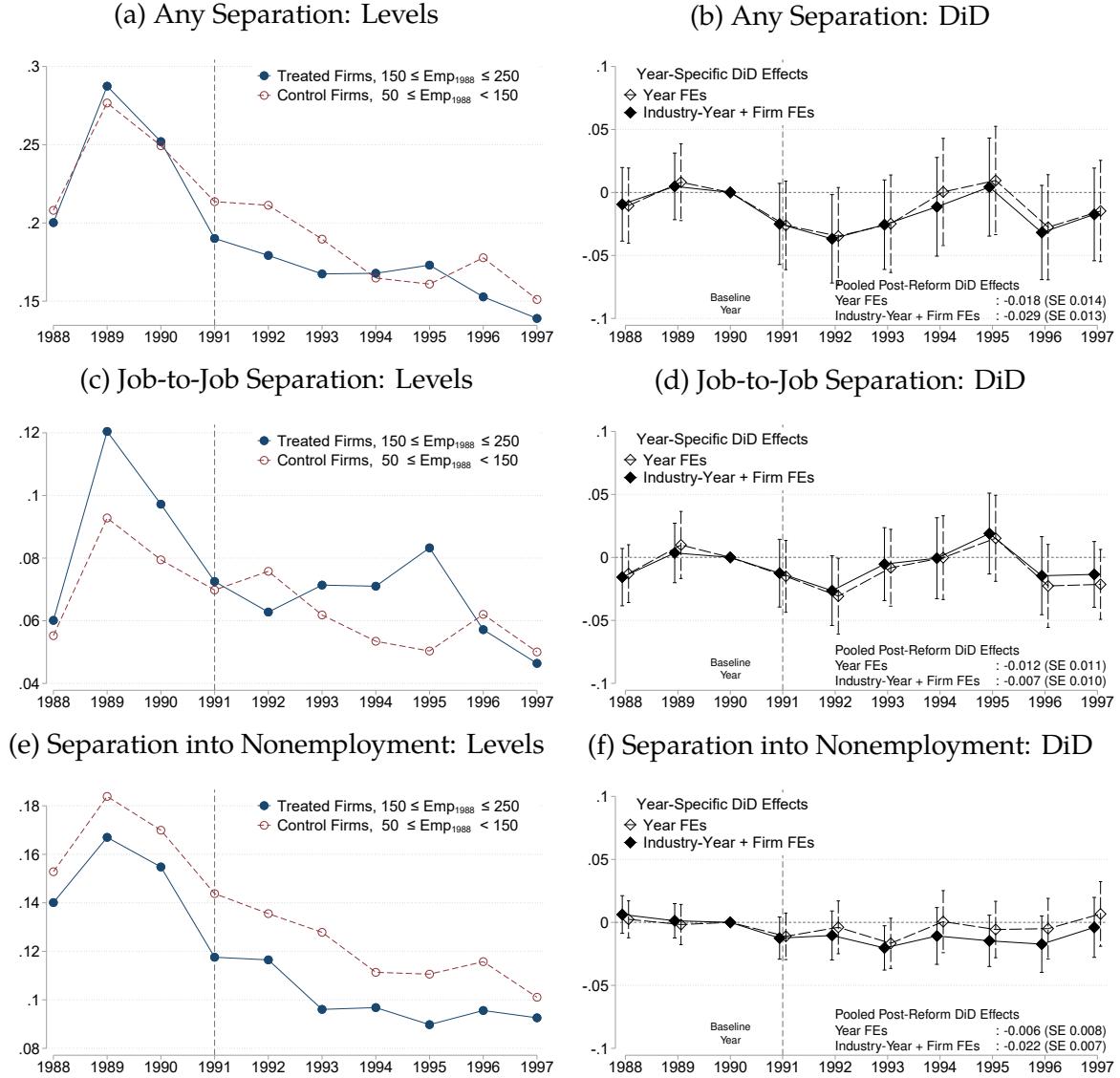


(b) DiD: Fraction of Firms with Worker Right to Shared Governance



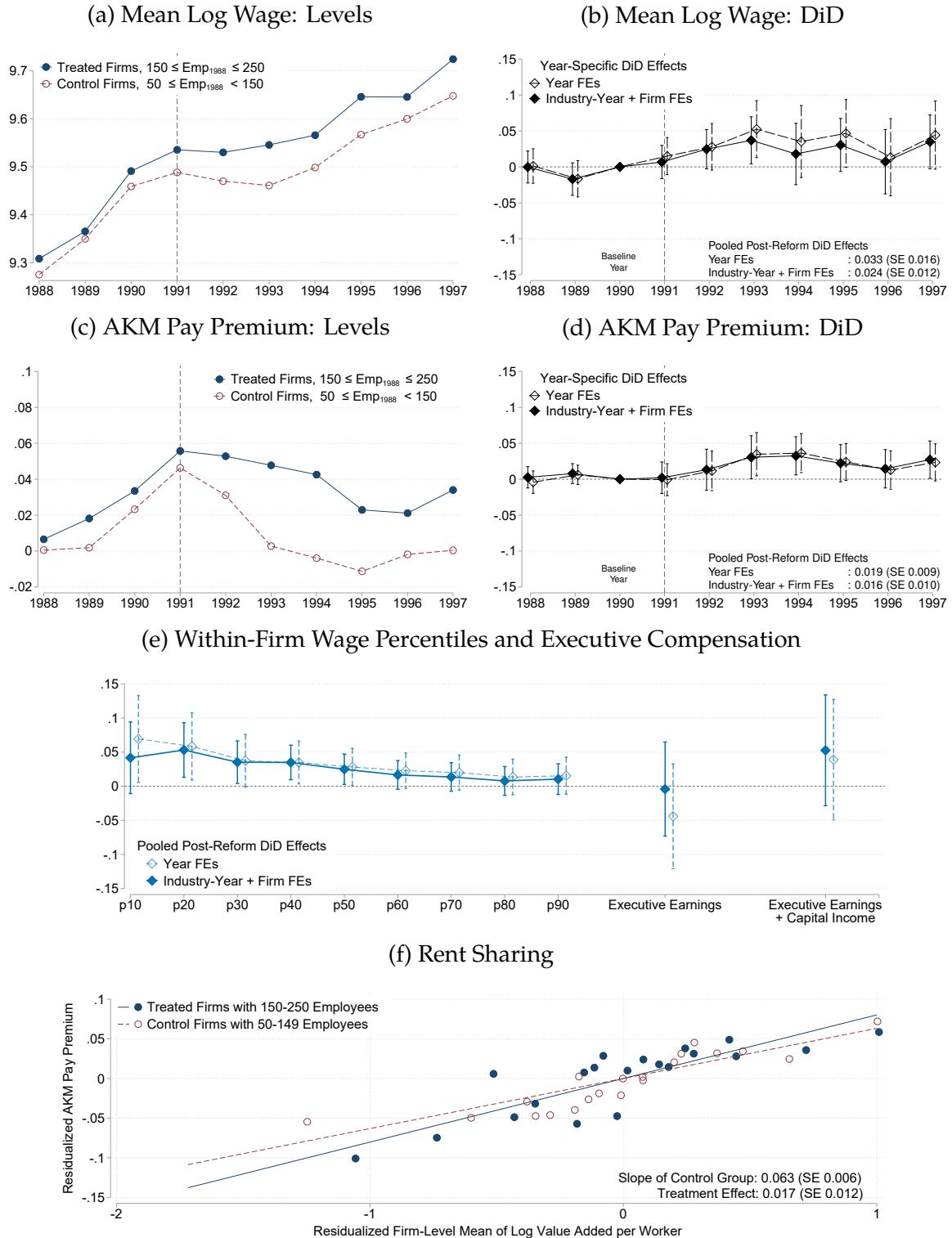
Note: The figure plots the persistence of treatment assignment in matched employer-employee data matched with the firm-level accounting data. Our DiD strategy sorts firms based on whether their pre-reform employment in 1988 was above or below 150 employees. Panel (a) plots the share of firms in the two groups with employment above 150 over time. The difference between the two time series in the post-reform period after 1991 captures the differential bite or first stage of our treatment assignment. Panel (b) plots results for a DiD specification as in Equation (1) with the outcome being an indicator for a worker right to shared governance (i.e. employment of at least 150 employees in the post-reform years). The vertical lines denote 95% confidence intervals based on standard errors clustered at the firm level. We also report results only based on the matched employer-employee data in Appendix Figure A.3. We report further robustness analyses in Appendix Figure A.4.

Figure 3: Job Separation Effects of Worker Voice



Note: The figure displays the effects of shared governance on separation rates. The left column displays the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 in 1988) over the period 1988-1997. The right column displays the difference-in-differences estimates decomposed by years normalized relative to baseline year 1990. The dashed vertical line in 1991 denotes the year the reform became active. In the right panels, the capped vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level; we report the pooled post-reform effect in the bottom right corner of each plot. We also report results in Columns (1) through (3) of Table 2. We report robustness analyses in Appendix Figures A.5-A.7, Appendix Figure A.9, and Appendix Table A.1.

Figure 4: Wage Effects of Worker Voice



Note: The figure displays the effects of shared governance on firm-level wages. Panels (a) and (c) display the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 in 1988) over the period 1988-1997. Panels (b) and (d) display the difference-in-differences estimates decomposed by years normalized relative to baseline year 1990. The dashed vertical line in 1991 denotes the year the reform became active. In the right panels (b) and (d), the capped vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level; we report the pooled post-reform effect in the bottom right corner of each plot (and in Table 3). We report robustness analyses in Appendix Figure A.10. Panel (e) reports DiD effects on different within-firm percentiles of the wage distribution as well as (log) executive earnings (see also Table A.2). Panel (f) reports the relationship between firms' AKM pay premium and average log value added in the post-reform period (controlling for industry effects).

Figure 5: Firm Performance Effects of Worker Voice

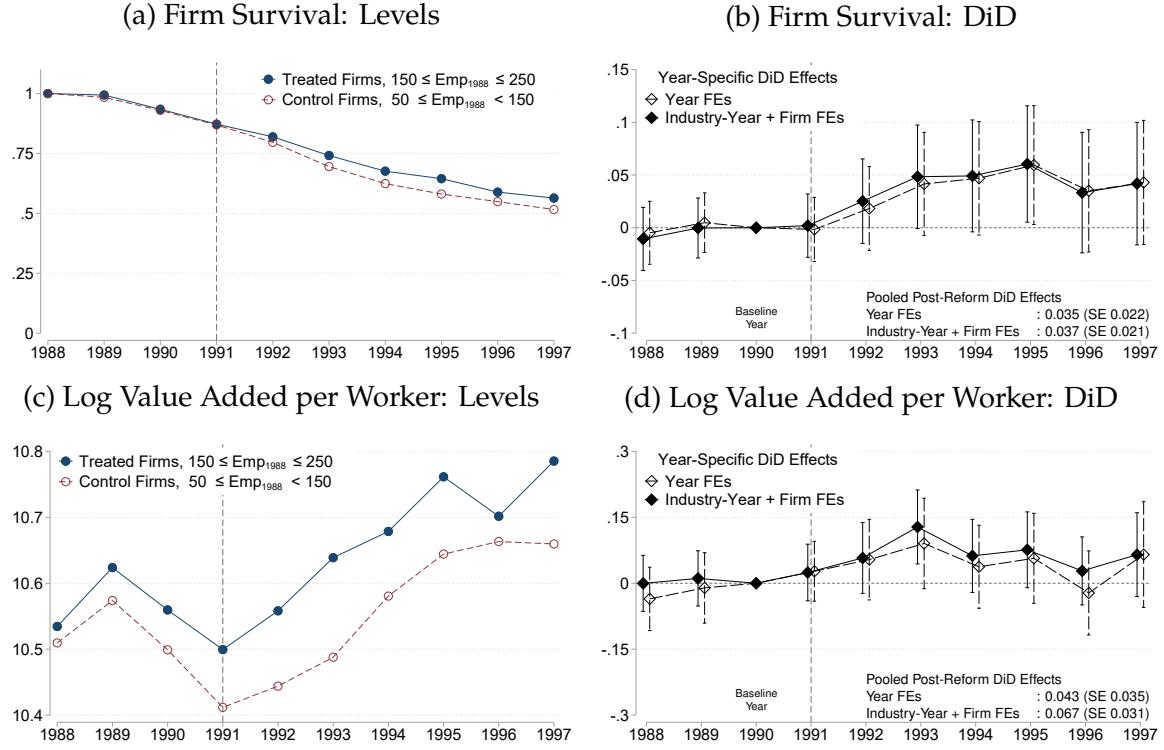
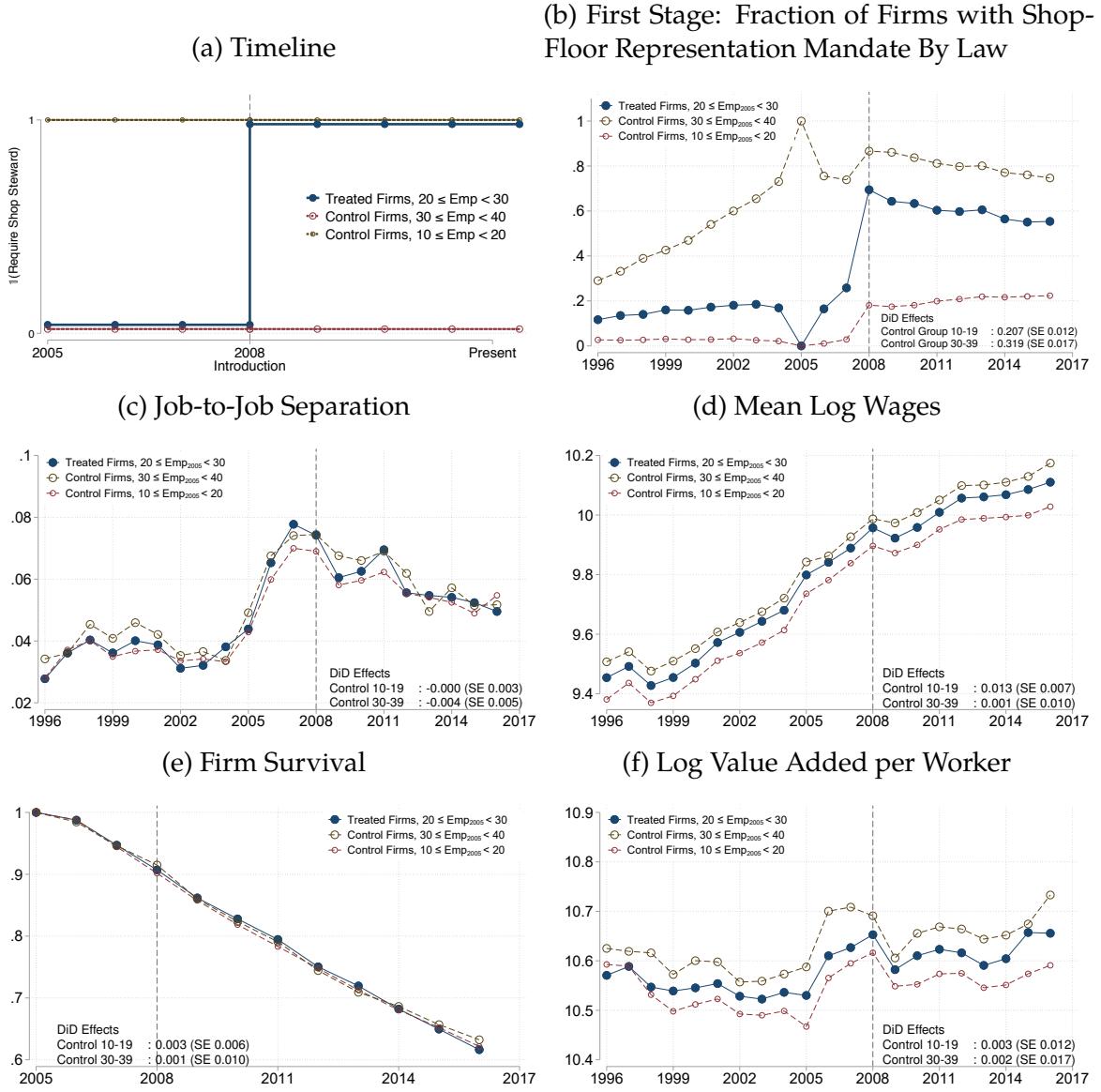
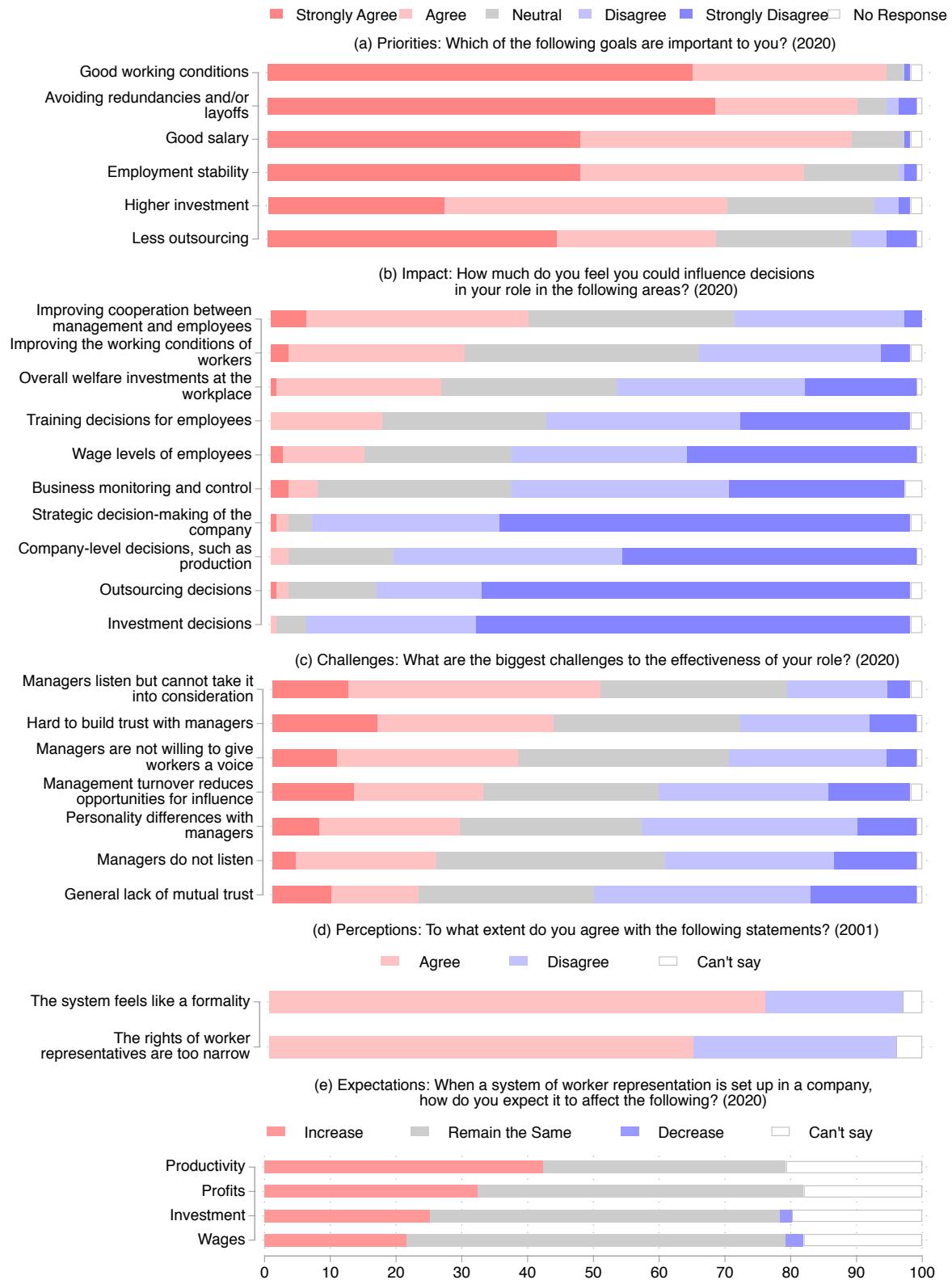


Figure 6: Effects of Alternative Worker Voice Institution: Shop-Floor Representation Reform



Note: The figure displays the effects of a 2008 reform that lowered the threshold for mandatory shop-floor representation from 30 to 20 employees. Panel (a) displays the timeline of the reform for firms in different size categories. Panels (b) through (f) report time series of outcomes for three groups of firms: a treatment group of firms with employment between 20 and 29 employees in 2005 and two control groups with 10-19 and 30-39 employees, respectively, in 2005. Panel (b) reports a first stage relationship indicating the share of firms with law-induced shop-floor representation mandate. Formally, the outcome variable is equal to one for firms with at least 30 employees in the pre-period and at least 20 employees in the post-reform period. We report the pooled post-reform (2008-2013) effect in each plot and in Tables A.3 and A.4. We display other outcomes in Appendix Figure A.13.

Figure 7: Survey Evidence on Worker Representatives' Perspectives



*Note:* The panels plot responses from our 2020 survey of 111 worker representatives in cooperation with a major trade union federation, and from the 2001 survey of 203 shop-floor representatives who are members of the Finnish metalworks union (Sairo, 2001). For further details and statistics, see notes of Table 1.

# Tables

Table 1: Survey Evidence on Prevalence and Forms of Worker Representation in Finnish Firms

	Panel (a): Do you have an administrative representative?			
	2001	2017	2019	2020
Yes	60%	51%	47%	63%
No	36%	40%	48%	37%
Missing response	4%	9%	5%	0%
	Panel (b): In which governance body do the worker representatives participate?			
	2001	2017	2019	2020
Management	60%	32%	37%	28%
Board of directors	26%	23%	24%	32%
Supervisory board	6%	17%	8%	7%
Elsewhere	9%	23%	23%	24%
Missing response	0%	5%	8%	9%
	Panel (c): What is the legal basis for this representation?			
	2001	2017	2019	2020
According to the law	-	26%	25%	31%
According to agreement	-	40%	59%	54%
Other	-	-	11%	4%
Missing response	-	35%	5%	10%
	Panel (d): If you meet the threshold, why is there no worker representation?			
	2001	2017	2019	2020
The employer did not want it	34%	40%	45%	49%
The employees did not want it	-	1%	5%	3%
Not aware of the right	14%	6%	8%	11%
Can't say	27%	19%	22%	-
Other reason	25%	33%	22%	38%
N	203	288	164	111
Restricted to $\geq 150$ employees	No	Yes	Yes	Yes

Note: The table presents results from four separate surveys of Finnish worker representatives, asking whether and in what form they have implemented the worker representation introduced by the 1991 reform. The 2001 survey was conducted among representatives who are members of the Finnish metalworks union, and covered 203 shop-floor representatives in metal and electronics companies (Sairo, 2001). This survey, unlike the others, is *not* restricted to firms above the 150 employee threshold and in these tabulations the sample is not restricted to those firms (these numbers are from a report and we cannot access the raw data and hence cannot re-calculate these numbers with the restriction imposed). However, 73% of respondents are in firms above the threshold. The 2017 and 2019 surveys were conducted by a major Finnish trade union federation for industrial employees, and covered 288 and 164 firms with more than 150 employees (Teollisuuden Palkansaajat, 2017, 2019). Respondents were worker representatives of various kinds (e.g., shop-floor representatives, European Works Council representatives) in those firms. The 2020 survey was conducted by us in cooperation with the same trade union federation, and covered 111 worker representatives of various kinds. The first panel reports responses to a question about whether the company has organised formal worker representation, and the second panel reports responses to a question about which governing body the representatives sit on, if they exist. Examples of responses from the free-form "Other" category for the body of representation include representation in multiple bodies, regular meetings between top management and worker representatives, and advisory boards. The "Missing response" category indicates respondents who did not know the answer or whose response was missing for a different reason. The third panel reports responses regarding the legal basis for the worker representation.

Table 2: Effects on Separations and Measures of Job Quality

	Any Separation (1)	Job-to-Job Separation (2)	Separation into Nonemployment (3)	Firm Value Log Index (z-score) (4)	Sickness Spell (Older than 40) (5)	Sickness Spell (Male) (6)	Job Quality (z-score) (7)	Labor Relations Quality (z-score) (8)
<i>DiD: Year FEs</i>								
Treatment (1991-1997)	-0.018 (0.014)	-0.012 (0.011)	-0.006 (0.008)	-0.043 (0.105)	-0.002 (0.003)	-0.001 (0.003)	0.182** (0.084)	0.063 (0.083)
Pre-Period (1988-1989)	-0.014 (0.015)	-0.008 (0.012)	-0.005 (0.007)		0.007* (0.004)	0.005 (0.004)		
<i>DiD: Industry-Year FEs</i>								
Treatment (1991-1997)	-0.013 (0.014)	-0.010 (0.011)	-0.002 (0.008)	-0.049 (0.104)	-0.002 (0.003)	-0.002 (0.003)	0.146* (0.088)	0.063 (0.089)
Pre-Period (1988-1989)	-0.014 (0.014)	-0.010 (0.011)	-0.004 (0.007)		0.006 (0.004)	0.004 (0.003)		
<i>DiD: Year and Firm FEs</i>								
Treatment (1991-1997)	-0.027** (0.013)	-0.006 (0.010)	-0.021*** (0.007)	-0.053 (0.107)	-0.002 (0.003)	-0.002 (0.003)		
Pre-Period (1988-1989)	-0.008 (0.013)	-0.009 (0.011)	0.000 (0.007)		0.008* (0.004)	0.004 (0.004)		
<i>DiD: Industry-Year and Firm FEs</i>								
Treatment (1991-1997)	-0.029** (0.013)	-0.007 (0.010)	-0.022*** (0.007)	-0.065 (0.104)	-0.002 (0.003)	-0.001 (0.003)		
Pre-Period (1988-1989)	-0.007 (0.013)	-0.009 (0.011)	0.002 (0.007)		0.007 (0.004)	0.003 (0.004)		
1990 Average (Control):	0.249	0.079	0.170	-0.008	0.070	0.070	0.057	0.041
1990 Average (Treated):	0.252	0.097	0.155	0.045	0.075	0.075	-0.045	-0.244
N, Firm-Years (Control):	8,635	8,635	8,635	4,402	8,577	8,545	1,394	1,399
N, Firm-Years (Treated):	1,833	1,833	1,833	1,409	1,827	1,829	701	703

*Note:* The table reports results of DiD specifications as in Equation (1). All point estimates are reported relative to 1990, the year for which we normalize the difference between treatment and control group to zero. Treatment indicates the DiD treatment effect in the post-reform period from 1991 to 1997. We also report the pre-period difference between the two groups relative to 1990 to test the parallel trends assumption in the pre-reform period. The first panel reports DiD results without additional control variables, the second panel includes industry-year effects (NACE Level 1), the third and fourth panel repeat the same specifications including firm effects. Standard errors clustered at the firm level are reported in parentheses. We plot the results for separations in Figure 3, and for sickness in Appendix Figure A.8. We report robustness analyses in Appendix Figure A.5-A.9. The post-period for the Job Quality and Labor Relations Quality outcomes draws on the 1997 wave; the previous wave is 1990.

Table 3: Effects on Wages and Firm Performance

	Mean Log Wage (1)	AKM Pay Premium (2)	Labor Share (3)	Firm Survival (4)	Log Value Added per Worker (5)	Capital Intensity (6)	Total Factor Productivity (7)	Profit Margin (8)
<i>DiD: Year FEs</i>								
Treatment (1991-1997)	0.033** (0.016)	0.019** (0.009)	-0.010 (0.014)	0.035 (0.022)	0.043 (0.035)	0.099 (0.078)	-0.038 (0.060)	0.006 (0.008)
Pre-Period (1988-1989)	-0.006 (0.013)	-0.000 (0.008)	0.016 (0.011)	0.000 (0.015)	-0.024 (0.033)	0.063 (0.073)	0.028 (0.060)	0.005 (0.005)
<i>DiD: Industry-Year FEs</i>								
Treatment (1991-1997)	0.017 (0.015)	0.016* (0.009)	-0.006 (0.014)	0.037* (0.021)	0.039 (0.034)	0.055 (0.075)	0.018 (0.049)	0.005 (0.008)
Pre-Period (1988-1989)	-0.015 (0.013)	0.001 (0.008)	0.012 (0.011)	-0.005 (0.015)	-0.014 (0.032)	0.032 (0.072)	0.018 (0.048)	0.005 (0.006)
<i>DiD: Year and Firm FEs</i>								
Treatment (1991-1997)	0.035*** (0.013)	0.019* (0.010)	-0.018 (0.014)	0.035 (0.022)	0.068** (0.031)	0.037 (0.049)	0.057 (0.036)	-0.000 (0.008)
Pre-Period (1988-1989)	-0.003 (0.010)	-0.000 (0.007)	0.015 (0.011)	0.000 (0.015)	-0.023 (0.028)	-0.039 (0.046)	0.018 (0.035)	0.004 (0.005)
<i>DiD: Industry-Year and Firm FEs</i>								
Treatment (1991-1997)	0.024** (0.012)	0.016* (0.010)	-0.022 (0.014)	0.037* (0.021)	0.067** (0.031)	0.035 (0.048)	0.063* (0.034)	-0.001 (0.008)
Pre-Period (1988-1989)	-0.007 (0.009)	0.001 (0.007)	0.012 (0.011)	-0.005 (0.015)	-0.016 (0.028)	-0.044 (0.047)	0.027 (0.034)	0.002 (0.005)
1990 Average (Control):	9.459	0.023	0.600	0.930	10.499	10.059	6.115	-0.005
1990 Average (Treated):	9.491	0.033	0.576	0.935	10.560	10.220	5.959	-0.010
N, Firm-Years (Control):	8,684	7,089	5,056	12,648	4,979	5,037	4,979	5,049
N, Firm-Years (Treated):	1,839	1,489	1,256	2,568	1,231	1,247	1,235	1,255

Note: The table reports results of DiD specifications as in Equation (1). All point estimates are reported relative to 1990, the year for which we normalize the difference between treatment and control group to zero. Treatment indicates the DiD treatment effect in the post-reform period from 1991 to 1997. We also report the pre-period difference between the two groups relative to 1990 to assess the parallel trends assumption in the pre-reform period. The first panel reports DiD results without additional control variables, the second panel includes industry-year effects (NACE Level 1), the third and fourth panel repeat the same specifications including firm effects. Standard errors clustered at the firm level are reported in parentheses. We plot wage effects in Figure 4 and firm performance results in Figure 5 and Appendix Figure A.11. We report robustness analyses in Appendix Figure A.10.

# Online Appendix of: Voice at Work

## Jarkko Harju, Simon Jäger, and Benjamin Schoefer

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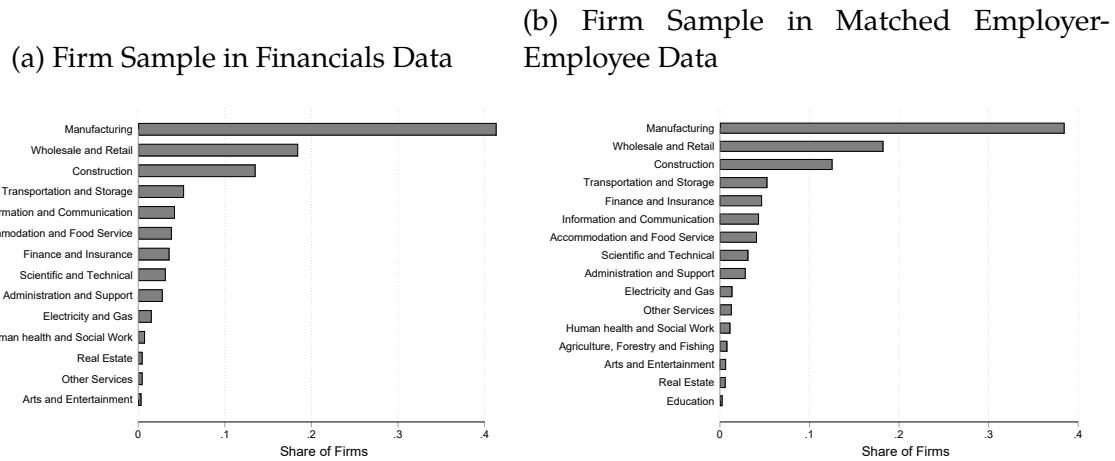
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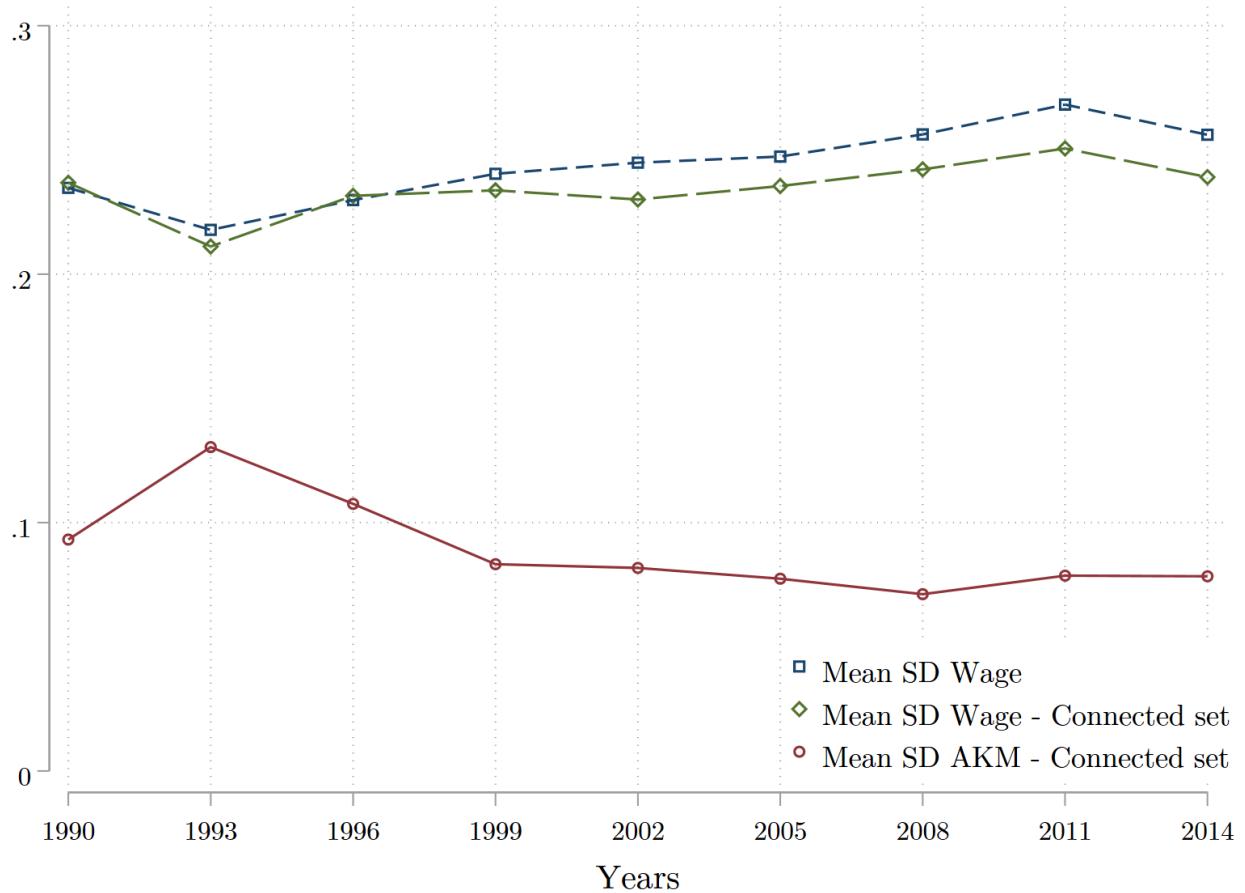
# A Appendix Figures

Figure A.1: Industry Composition



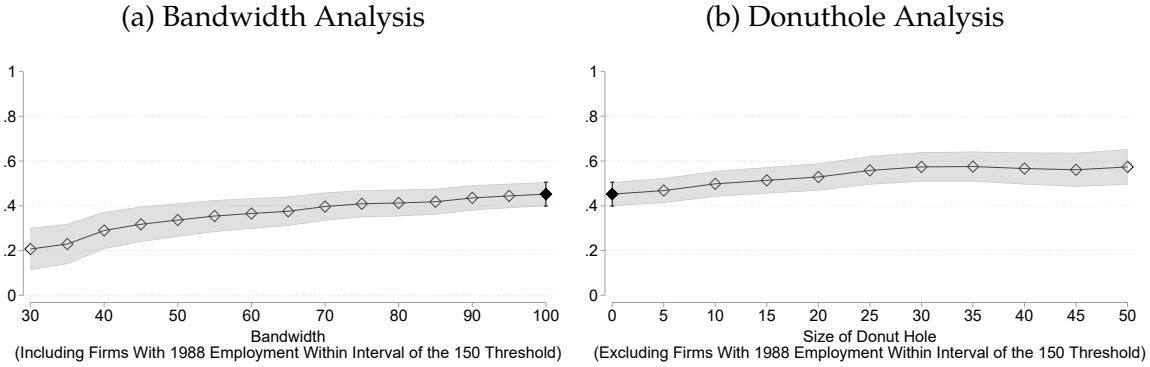
Note: The figure plots the industry composition of firms in our sample for the baseline year of 1990 (in which, e.g., our control means of outcome variables are also specified, reported in the regression tables).

Figure A.2: Standard Deviation of Log Wages and AKM Firm Effects Over Time



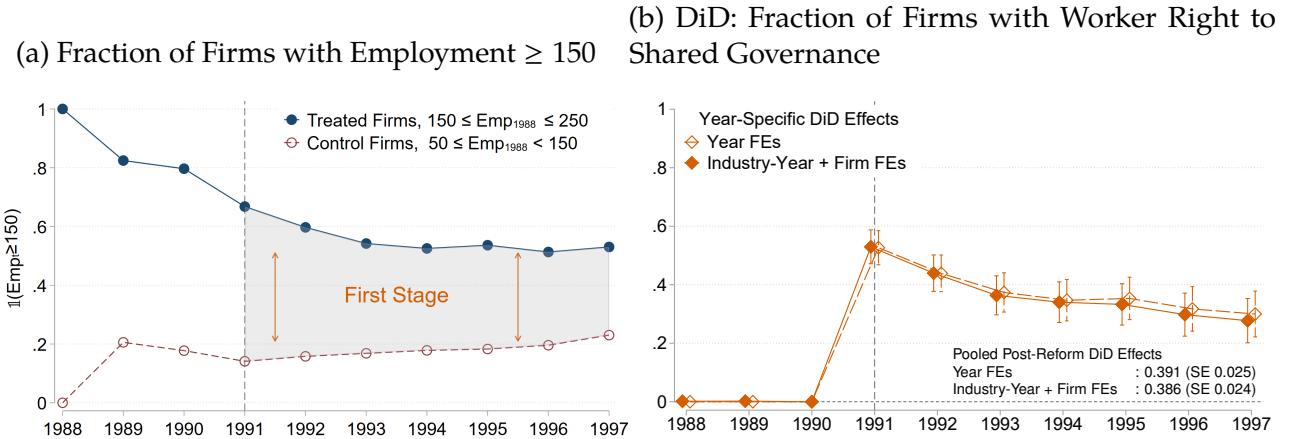
*Note:* The figure plots the standard deviation of individual log wages as well as of AKM firm effects over time. For the individual log wages, we both report the overall standard deviation as well as the standard deviation of individual wages of workers employed by firms in the largest connected sets (respectively for each three-year time window) from the AKM estimation. The AKM firm effects are estimated in three-year windows and we report the standard deviation for those firms in the largest connected set at each time horizon. The sample for this estimation is based on the entire matched employer-employee data (rather than the firm size window for our main analysis).

Figure A.4: Fraction of Firms with Worker Right to Shared Governance (Robustness Checks)



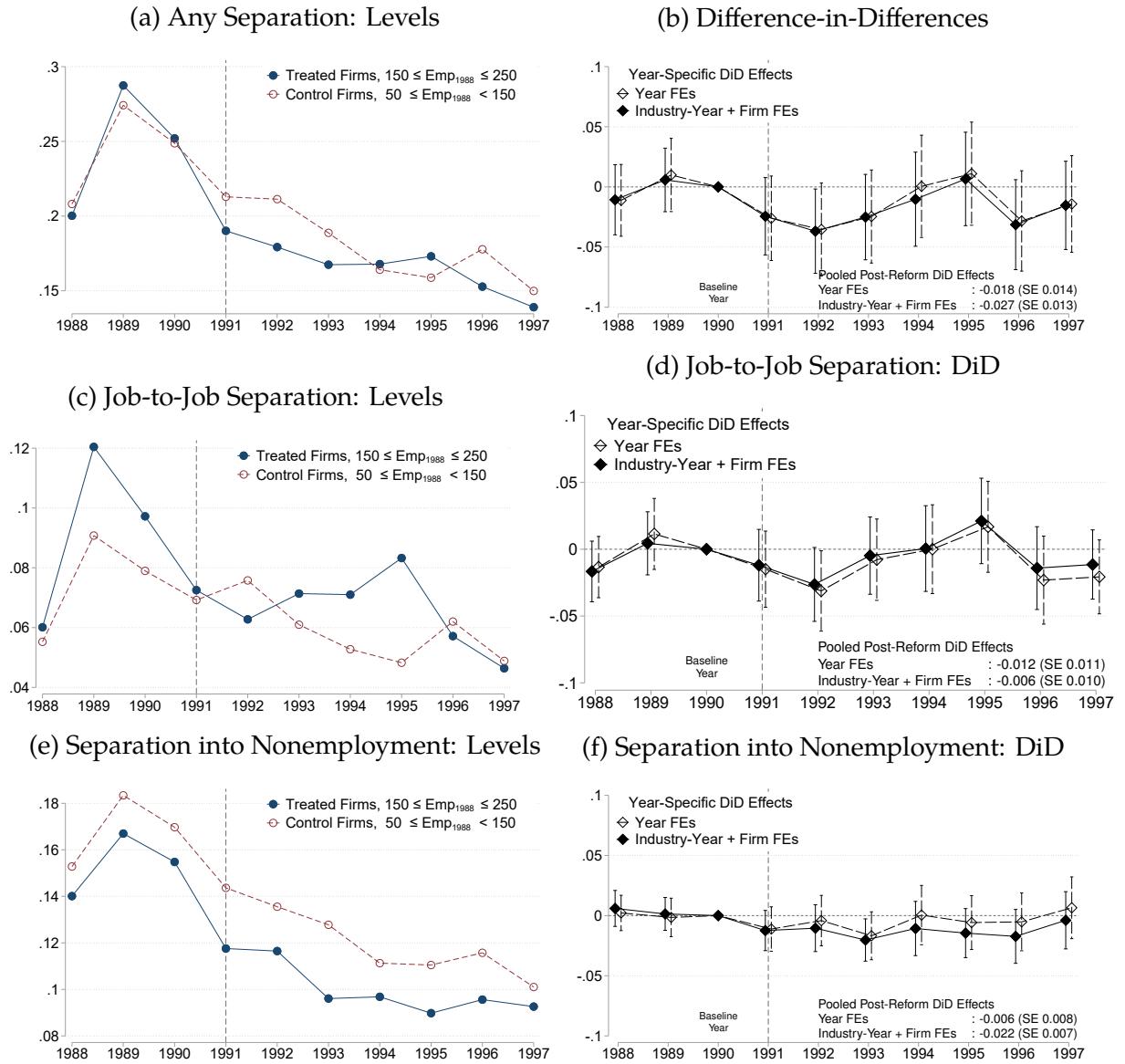
*Note:* The figure reports robustness checks for the DiD analysis in Figure 2. The outcome variable is an indicator for a firm being subject to the worker right to shared governance (i.e. having at least 150 employees in the post-reform period). The figure plots DiD point estimates and 95% confidence intervals clustering standard errors at the firm level and including firm fixed effects and industry-year fixed effects. The upper panel varies the employment sample starting from a bandwidth of 10, i.e. 140-160, to a bandwidth of 100, our baseline specification indicated by the solid black diamond. The lower panel displays a donuthole specification starting from a hole size equal to 0, our baseline indicated by the black solid diamond, to a hole size equal to 50, excluding firms with employment between 100-200.

Figure A.3: Persistence of Treatment Assignment, EE Sample



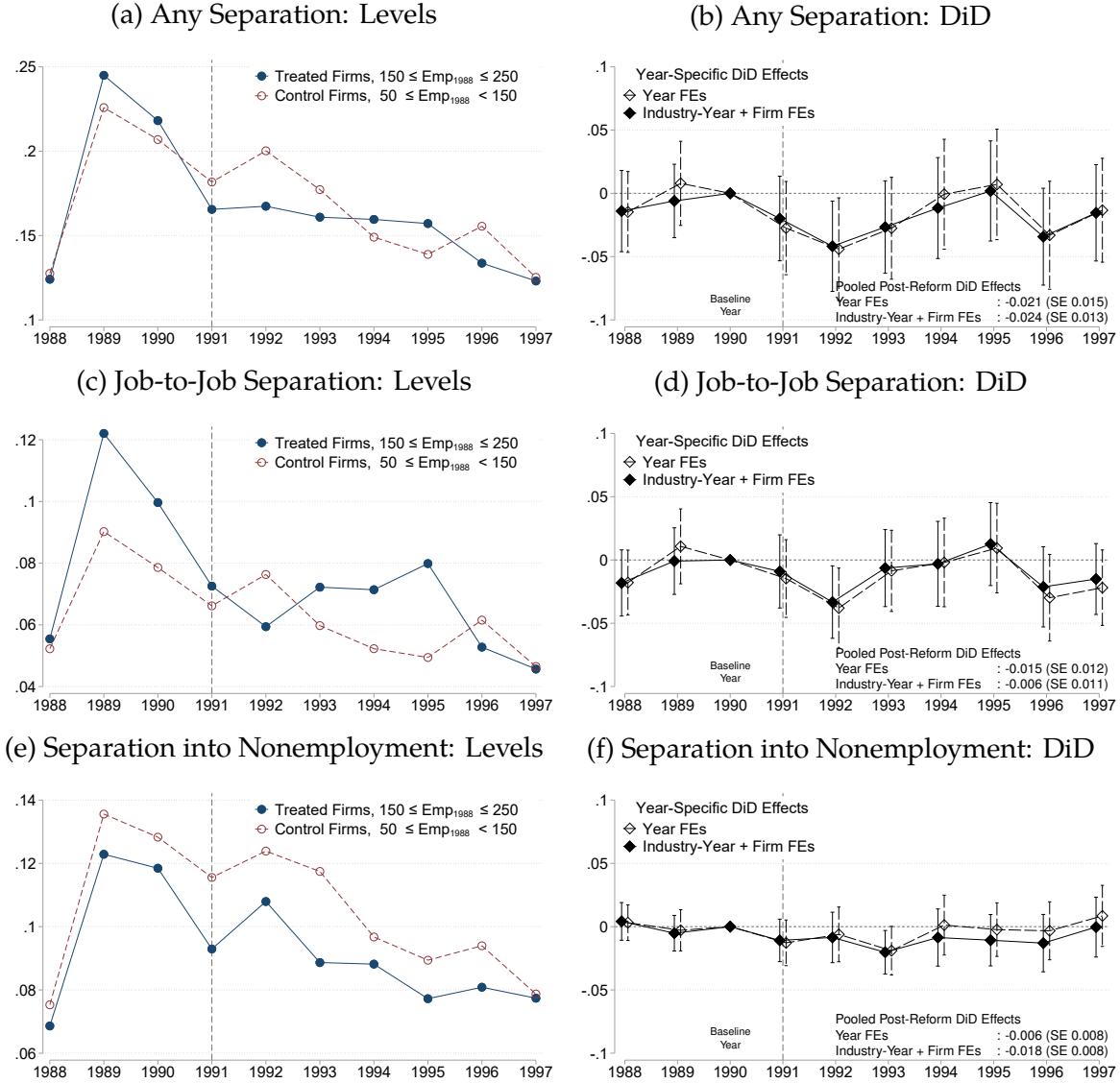
*Note:* The figure is a robustness check for Figure 2. Here, the sample is based on matched employer-employee data while Figure 2 restricts the sample to observations for which we also match firm data. For further details on the panels, see figure note to Figure 2.

Figure A.5: Separations for Employees Correcting for Spurious ID Changes



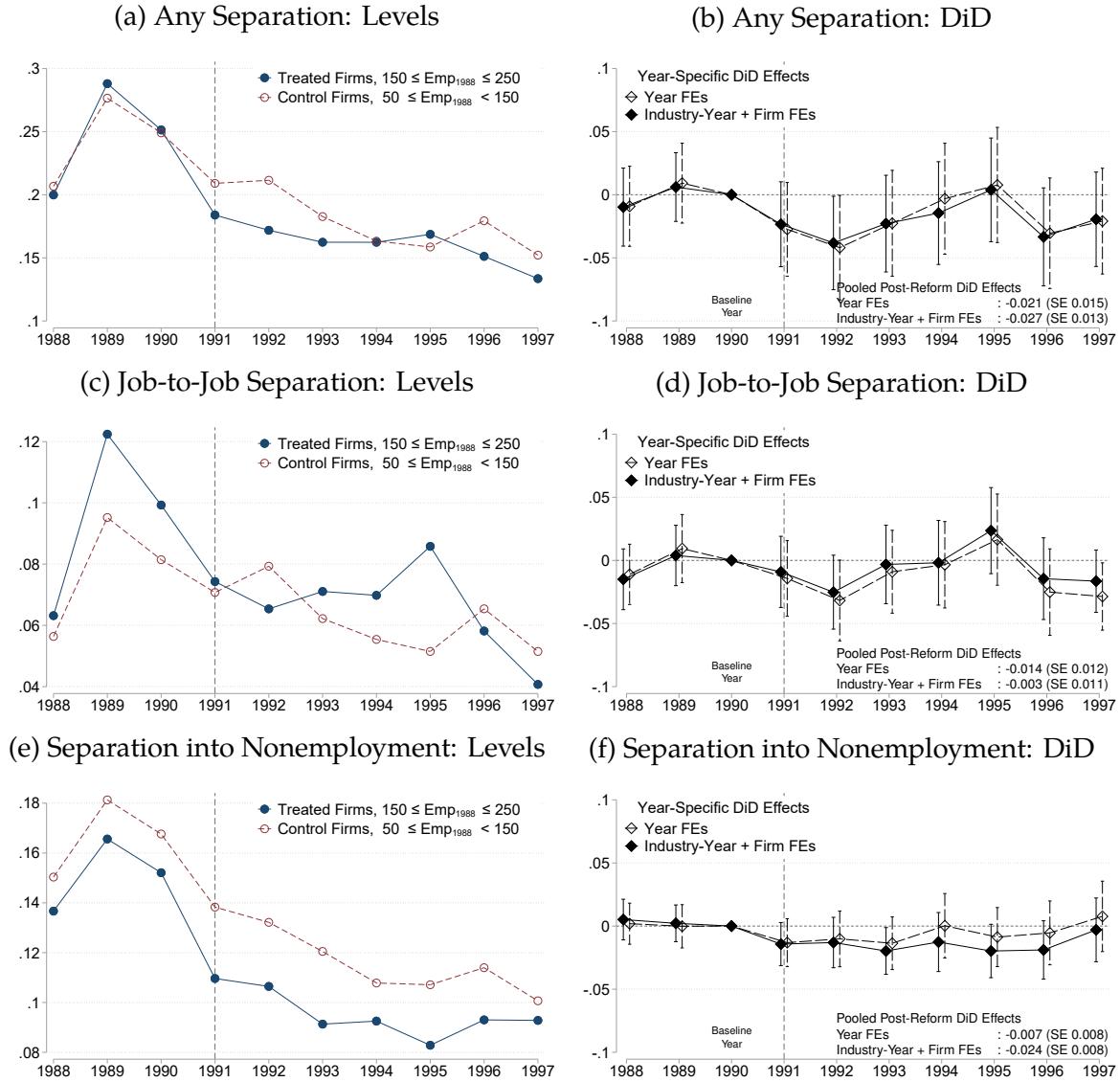
*Note:* The figure displays the effects of shared governance on separation rates for employees correcting spurious firm ID changes. Our procedure follows ideas in Hethey-Maier and Schmieder (2013) and identifies a spurious exit as one in which a group of at least 50% of a firm's employees exit and then are all subsequently employed at one firm in the subsequent year. In such a case, we recode the exit variable from 1 to 0. The left column displays the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 in 1988) over the period 1988-1997. The right column displays the difference-in-differences estimates decomposed by years normalized relative to baseline year 1990. The dashed vertical line in 1991 denotes the year the reform became active. In the right panels, the capped vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level; we report the pooled post-reform effect in the bottom right corner of each plot. We also report results in Columns (1) through (3) of Table A.1. We report full and unadjusted sample results in Figure 3 and Columns (1) through (3) of Table 2.

Figure A.6: Separations for Employees With at Least One Year of Tenure



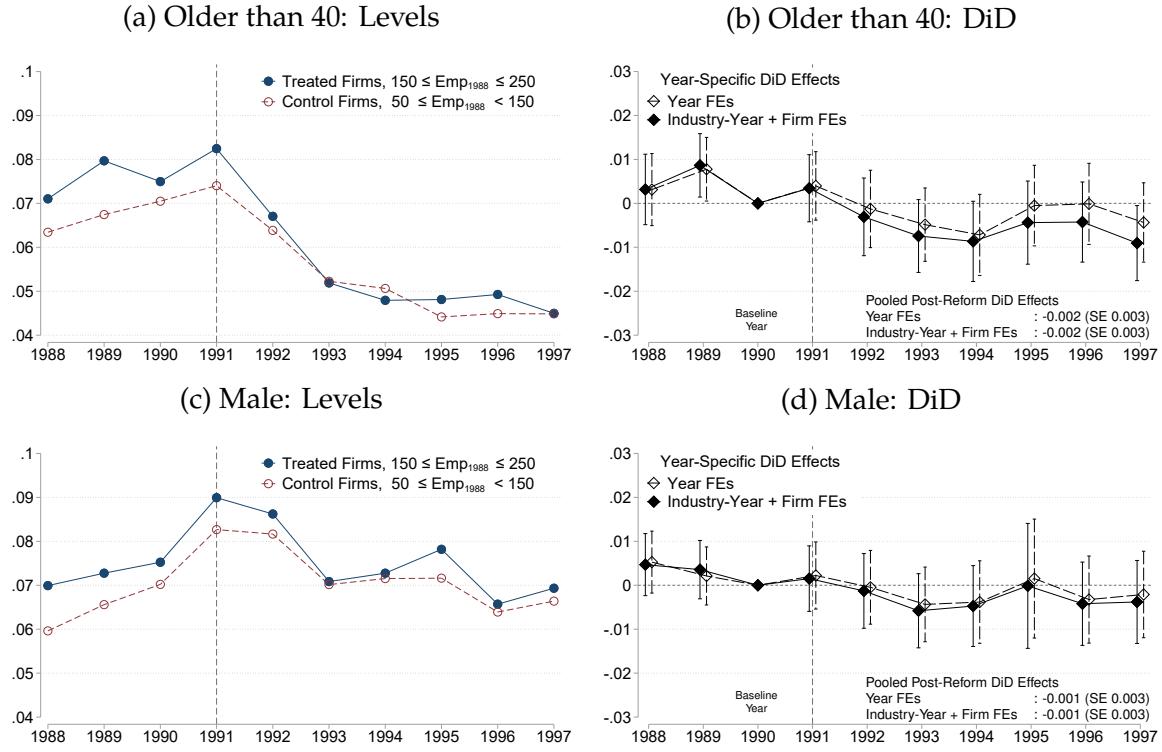
*Note:* The figure displays the effects of shared governance on separation rates for employees with at least one year of tenure. The left column displays the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 in 1988) over the period 1988-1997. The right column displays the difference-in-differences estimates decomposed by years normalized relative to baseline year 1990. The dashed vertical line in 1991 denotes the year the reform became active. In the right panels, the capped vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level; we report the pooled post-reform effect in the bottom right corner of each plot. We also report results in Columns (4) through (6) of Table A.1. We report full sample results in Figure 3 and Columns (1) through (3) of Table 2.

Figure A.7: Separations for Employees Aged 20-55



*Note:* The figure displays the effects of shared governance on separation rates for employees aged 20-55. The left column displays the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 in 1988) over the period 1988-1997. The right column displays the difference-in-differences estimates decomposed by years normalized relative to baseline year 1990. The dashed vertical line in 1991 denotes the year the reform became active. In the right panels, the capped vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level; we report the pooled post-reform effect in the bottom right corner of each plot. We also report results in Columns (7) through (9) of Table A.1. We report full sample results in Figure 3 and Columns (1) through (3) of Table 2.

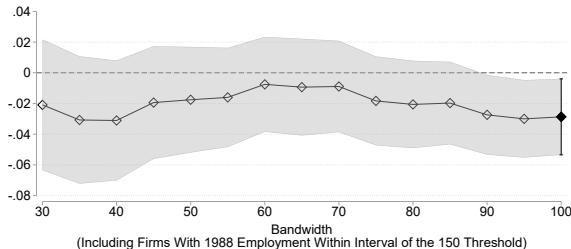
Figure A.8: Sickness: Fraction of Employees with Sick Benefits



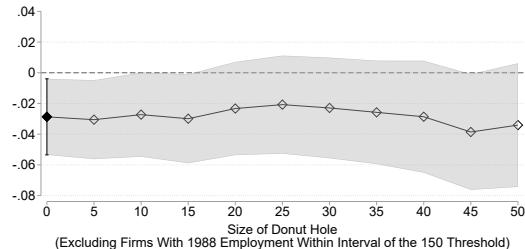
*Note:* The figure displays the effects of shared governance on sick benefits and maternity benefits. The left column displays the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 in 1988) over the period 1988-1997. The right column displays the difference-in-differences estimates decomposed by years normalized relative to baseline year 1990. The dashed vertical line in 1991 denotes the year the reform became active. In the right panels, the capped vertical bars denote 95% confidence intervals based on standard errors clustered at the firm level; we report the pooled post-reform effect in the bottom right corner of each plot. We also report results in Columns (5) and (6) of Table 2. We report robustness analyses in Figure A.9.

Figure A.9: Separations and Sickness (Robustness Checks)

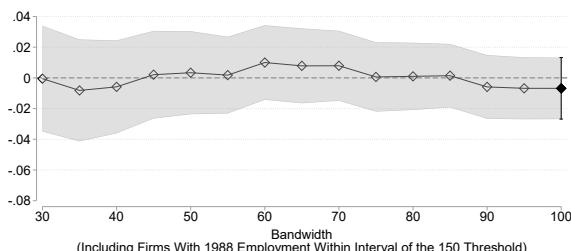
(a) Any Separation, Bandwidth



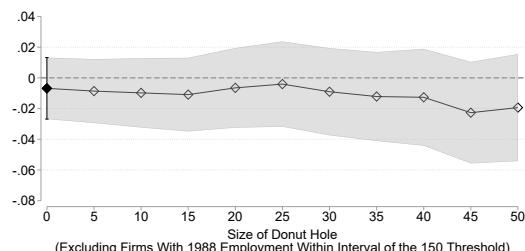
(b) Any Separation, Donuthole



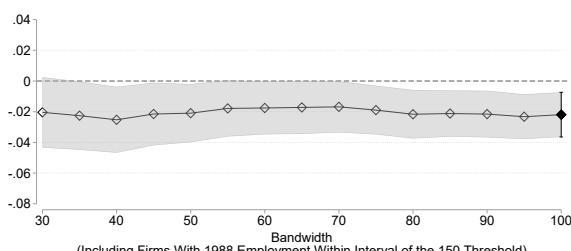
(c) Job-to-Job Separation, Bandwidth



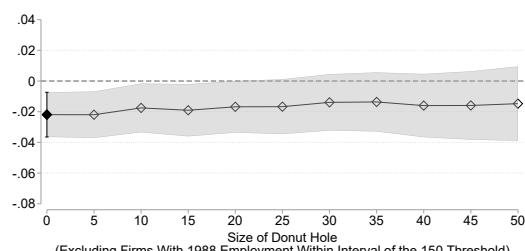
(d) Job-to-Job Separation, Donuthole



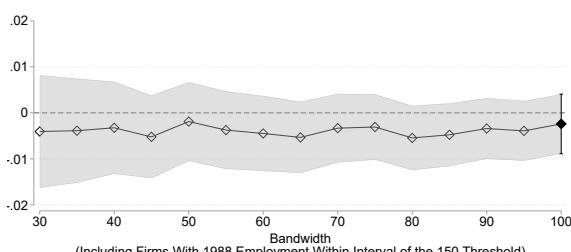
(e) Sep. into Nonemployment, Bandwidth



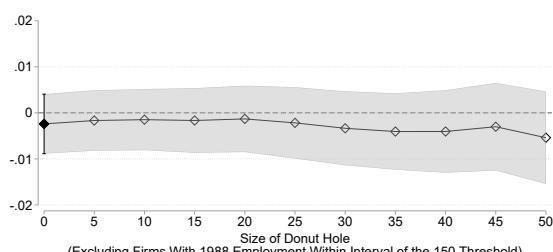
(f) Sep. into Nonemployment, Donuthole



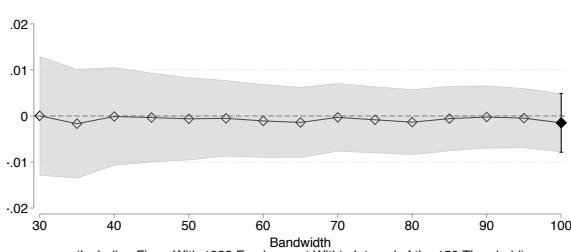
(g) Sickness, Older than 40, Bandwidth



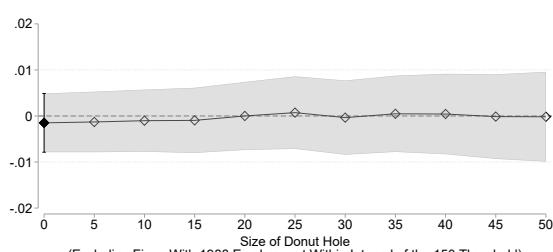
(h) Sickness, Older than 40, Donuthole



(i) Sickness, Male, Bandwidth

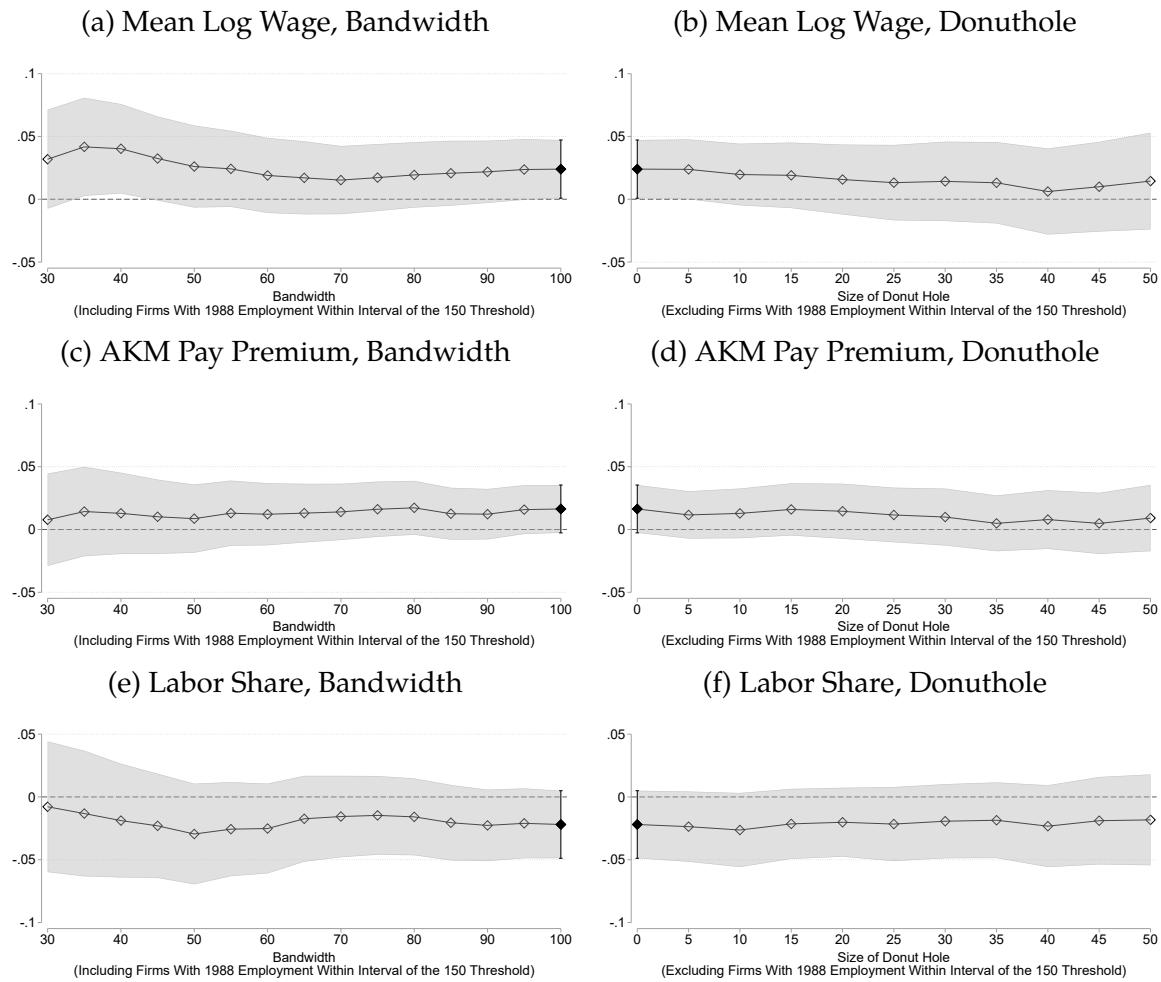


(j) Sickness, Male, Donuthole



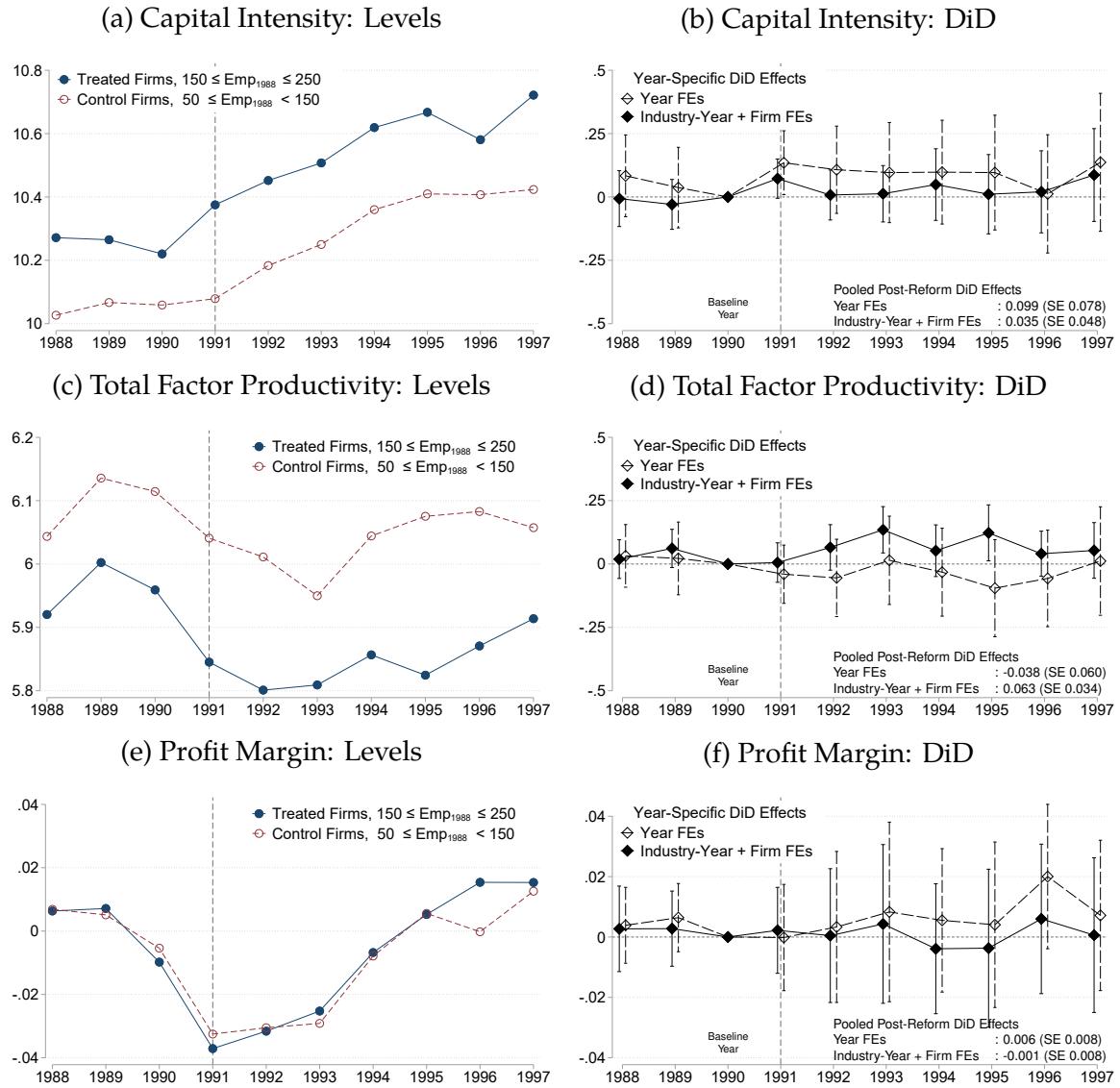
*Note:* The figure plots DiD point estimates and 95% confidence intervals, clustering standard errors at the firm level and including firm fixed effects and industry-year fixed effects. The left column varies the employment sample starting from a bandwidth of 10, 140-160, to a bandwidth of 100, our baseline specification indicated by the solid black diamond. The right column displays the donuthole specification starting from a hole size equal to 0, our baseline indicated by the black solid diamond, to a hole size equal to 50, excluding firms with employment between 100-200. We display the corresponding baseline results in Figure 3, Appendix Figure A.8 and Table 2.

Figure A.10: Wage Effects (Robustness Checks)



*Note:* The figure plots DiD point estimates and 95% confidence intervals, clustering standard errors at the firm level and including firm fixed effects and industry-year fixed effects. The left column varies the employment sample starting from a bandwidth of 10, 140-160, to a bandwidth of 100, our baseline specification indicated by the solid black diamond. The right column displays the donut hole specification starting from a hole size equal to 0, our baseline indicated by the black solid diamond, to a hole size equal to 50, excluding firms with employment between 100-200. We display the corresponding baseline results in Figure 4 and Table 3.

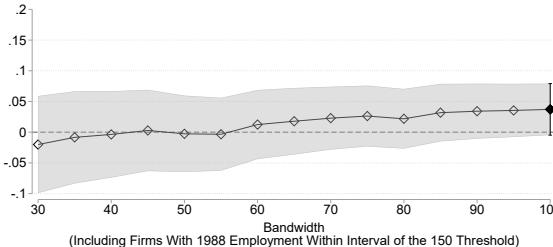
Figure A.11: Firm Performance (Additional Outcomes)



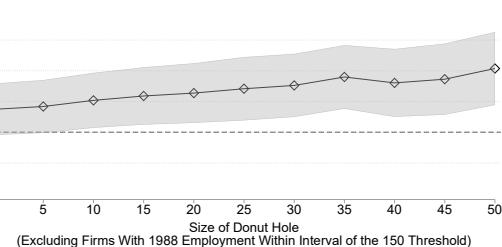
*Note:* The figure displays the effects of shared governance on capital intensity, total factor productivity and the profit margin. The left column displays the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 in 1988) over the period 1988-1997. The right column displays the difference-in-differences estimates decomposed by years normalized by baseline year 1990. Main results in Figure 5. We report the pooled post-reform effect in the bottom right corner of each plot and in Table 3.

Figure A.12: Firm Performance (Robustness Checks)

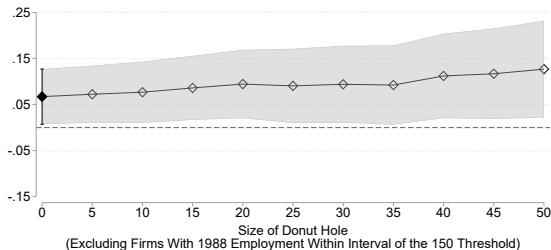
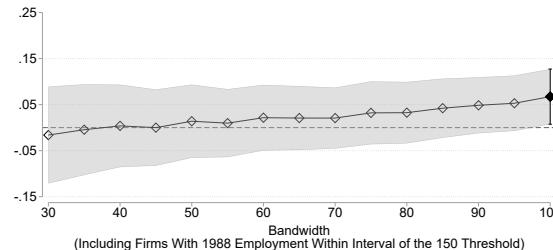
(a) Firm Survival, Bandwidth



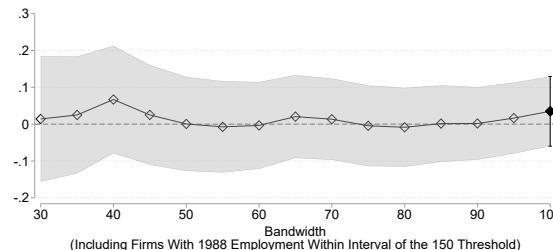
(b) Firm Survival, Donuthole



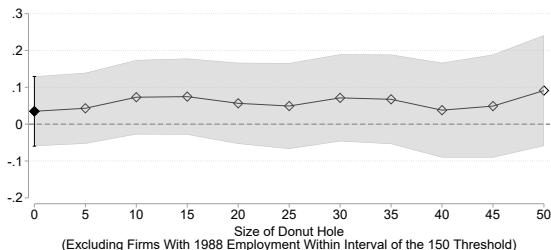
(c) Log Value Added per Worker, Bandwidth (d) Log Value Added per Worker, Donuthole



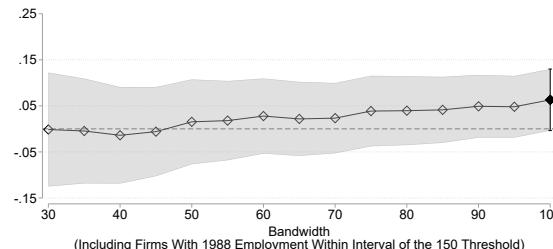
(e) Capital Intensity, Bandwidth



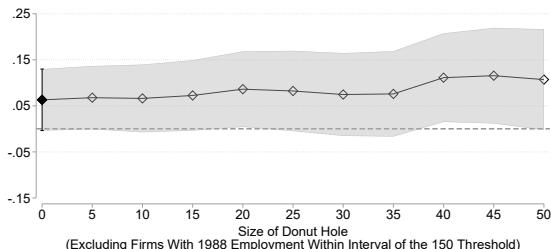
(f) Capital Intensity, Donuthole



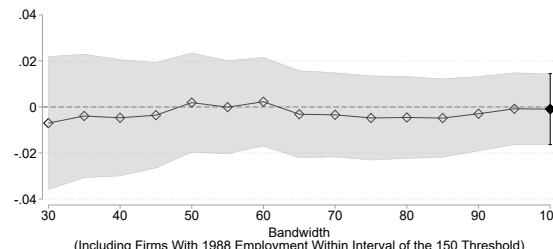
(g) Total Factor Productivity, Bandwidth



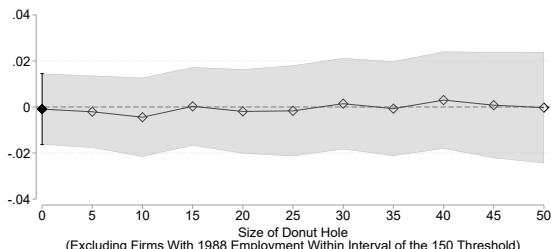
(h) Total Factor Productivity, Donuthole



(i) Profit Margin, Bandwidth

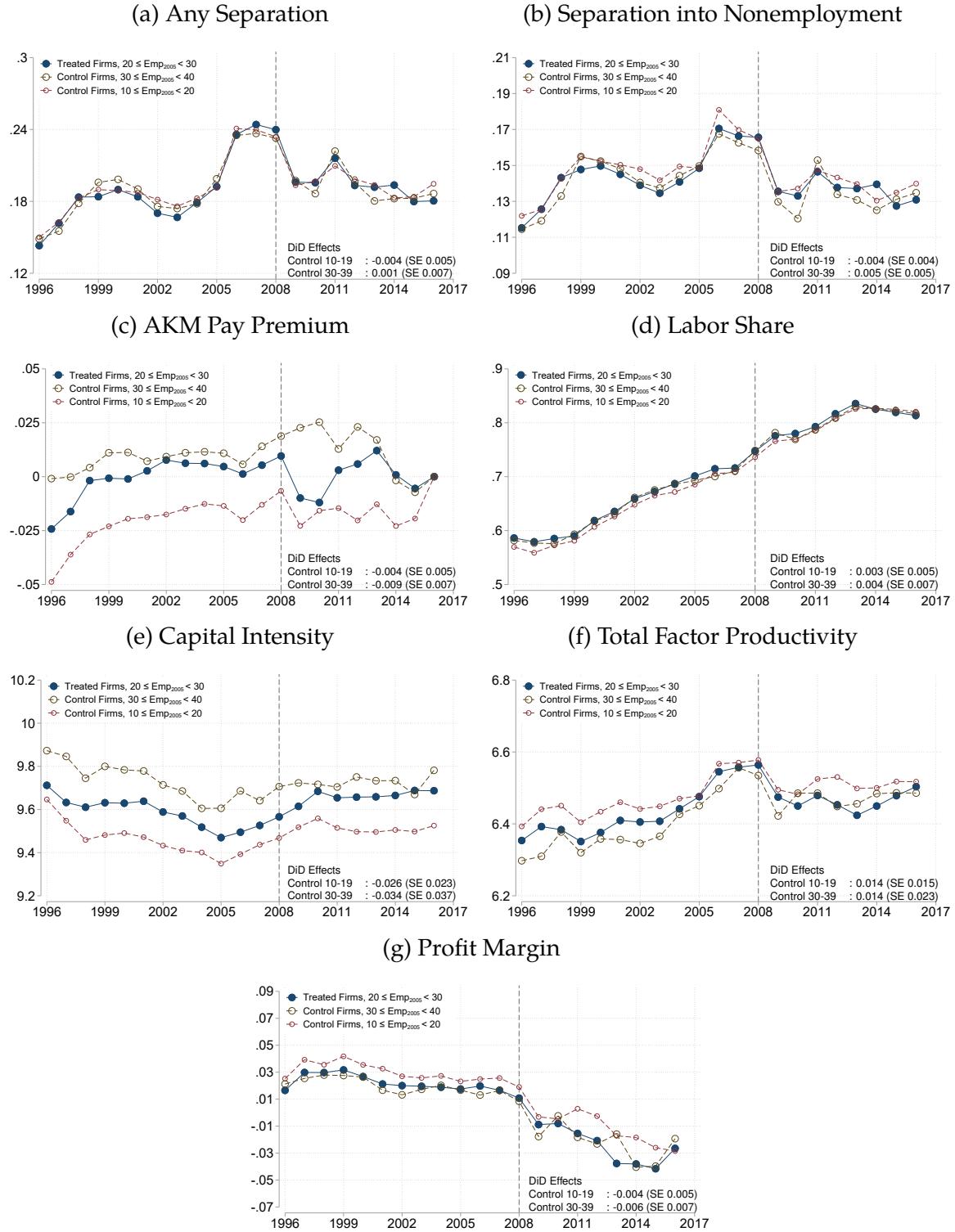


(j) Profit Margin, Donuthole



*Note:* The figure plots DiD point estimates and 95% confidence intervals clustering standard errors at the firm level and including firm fixed effects and industry-year fixed effects. The left column varies the employment sample starting from a bandwidth of 10, 140-160, to a bandwidth of 100, our baseline specification indicated by the solid black diamond. The right column displays the donuthole specification starting from a hole size equal to 0, our baseline indicated by the black solid diamond, to a hole size equal to 50, excluding firms with employment between 100-200. We display the corresponding baseline results in Figure 5 and Table 3.

Figure A.13: Shop-Floor Representation Reform (Additional Outcomes)



Note: The figure extends Figure 6 to other outcomes. It displays the effects of a 2008 reform that lowered the threshold for mandatory shop-floor representation from 30 to 20 employees. We report the pooled post-reform effect in each plot and in Tables A.3 and A.4.

## B Appendix Tables

Table A.1: Effects on Separations and Sickness (Robustness Checks)

	Correcting Spurious ID Changes			With at Least One Year of Tenure			Employees Aged 20-55		
	Any Separation (1)	Job-to-Job Separation (2)	Separation into Nonemployment (3)	Any Separation (4)	Job-to-Job Separation (5)	Separation into Nonemployment (6)	Any Separation (7)	Job-to-Job Separation (8)	Separation into Nonemployment (9)
<i>DiD: Year FEs</i>									
Treatment (1991-1997)	-0.018 (0.014)	-0.012 (0.011)	-0.006 (0.008)	-0.021 (0.015)	-0.015 (0.012)	-0.006 (0.008)	-0.021 (0.015)	-0.014 (0.012)	-0.007 (0.008)
Pre-Period (1988-1989)	-0.000 (0.014)	-0.001 (0.011)	0.000 (0.007)	-0.003 (0.014)	-0.003 (0.012)	-0.000 (0.007)	0.000 (0.014)	-0.001 (0.012)	0.001 (0.007)
<i>DiD: Industry-Year FEs</i>									
Treatment (1991-1997)	-0.012 (0.014)	-0.010 (0.011)	-0.002 (0.008)	-0.014 (0.015)	-0.013 (0.012)	-0.001 (0.008)	-0.014 (0.015)	-0.012 (0.012)	-0.002 (0.008)
Pre-Period (1988-1989)	0.000 (0.013)	-0.001 (0.011)	0.001 (0.007)	-0.004 (0.014)	-0.003 (0.012)	-0.000 (0.007)	0.002 (0.014)	-0.001 (0.012)	0.003 (0.007)
<i>DiD: Year and Firm FEs</i>									
Treatment (1991-1997)	-0.026** (0.013)	-0.005 (0.010)	-0.021*** (0.007)	-0.026* (0.014)	-0.008 (0.011)	-0.019** (0.008)	-0.026** (0.013)	-0.003 (0.011)	-0.023*** (0.007)
Pre-Period (1988-1989)	-0.005 (0.012)	-0.008 (0.010)	0.003 (0.006)	-0.010 (0.014)	-0.010 (0.012)	0.000 (0.006)	-0.004 (0.013)	-0.007 (0.011)	0.003 (0.007)
<i>DiD: Industry-Year and Firm FEs</i>									
Treatment (1991-1997)	-0.027** (0.013)	-0.006 (0.010)	-0.022*** (0.007)	-0.024* (0.013)	-0.006 (0.011)	-0.018** (0.008)	-0.027** (0.013)	-0.003 (0.011)	-0.024*** (0.008)
Pre-Period (1988-1989)	-0.002 (0.012)	-0.006 (0.010)	0.004 (0.006)	-0.009 (0.013)	-0.009 (0.012)	-0.001 (0.007)	-0.001 (0.013)	-0.006 (0.011)	0.005 (0.007)
1990 Average (Control):	0.249	0.079	0.170	0.207	0.079	0.128	0.249	0.081	0.168
1990 Average (Treated)	0.252	0.097	0.155	0.218	0.100	0.118	0.251	0.099	0.152
N, Firm-Years (Control):	8,635	8,635	8,635	8,235	8,235	8,235	7,988	7,988	7,988
N, Firm-Years (Treated):	1,833	1,833	1,833	1,787	1,787	1,787	1,684	1,684	1,684

Note: The table reports results of robustness checks for the separation outcomes analyzed in Figure 3 and Table 2. We plot outcomes in Appendix Figures A.5-A.7.

Table A.2: Effects on Within-Firm Wage Structure

	Log Wage in Within-Firm Wage Percentile									Executive	Executive Wage	
	p10 (1)	p20 (2)	p30 (3)	p40 (4)	p50 (5)	p60 (6)	p70 (7)	p80 (8)	p90 (9)	(Log) Wage (10)	& Capital Income (11)	
<i>DiD: Year FEs</i>												
Treatment (1991-1997)	0.069** (0.032)	0.058** (0.025)	0.037* (0.020)	0.035** (0.016)	0.028** (0.014)	0.023* (0.013)	0.020 (0.013)	0.014 (0.013)	0.015 (0.014)	-0.044 (0.039)	0.039 (0.045)	
Pre-Period (1988-1989)	0.006 (0.030)	-0.003 (0.022)	-0.007 (0.017)	-0.004 (0.014)	-0.005 (0.012)	-0.006 (0.011)	-0.010 (0.011)	-0.015 (0.010)	-0.012 (0.011)	0.001 (0.037)	-0.020 (0.044)	
<i>DiD: Industry-Year FEs</i>												
Treatment (1991-1997)	0.045 (0.031)	0.034 (0.024)	0.017 (0.019)	0.016 (0.015)	0.012 (0.013)	0.010 (0.013)	0.010 (0.013)	0.005 (0.013)	0.009 (0.014)	-0.046 (0.039)	0.037 (0.045)	
Pre-Period (1988-1989)	-0.011 (0.029)	-0.015 (0.022)	-0.019 (0.016)	-0.016 (0.013)	-0.015 (0.011)	-0.014 (0.010)	-0.018* (0.010)	-0.020** (0.010)	-0.014 (0.010)	0.012 (0.037)	-0.015 (0.045)	
<i>DiD: Year and Firm FEs</i>												
Treatment (1991-1997)	0.055** (0.028)	0.063*** (0.021)	0.045*** (0.017)	0.045*** (0.014)	0.035*** (0.012)	0.027** (0.011)	0.024** (0.011)	0.018 (0.011)	0.021* (0.012)	0.008 (0.035)	0.067 (0.041)	
Pre-Period (1988-1989)	0.002 (0.026)	-0.005 (0.018)	-0.011 (0.013)	-0.005 (0.010)	-0.003 (0.008)	-0.000 (0.007)	-0.003 (0.007)	-0.006 (0.007)	-0.003 (0.007)	0.024 (0.025)	0.019 (0.031)	
<i>DiD: Industry-Year and Firm FEs</i>												
Treatment (1991-1997)	0.042 (0.027)	0.053*** (0.020)	0.035** (0.016)	0.035*** (0.013)	0.025** (0.011)	0.017 (0.011)	0.014 (0.011)	0.008 (0.011)	0.010 (0.011)	-0.004 (0.035)	0.053 (0.041)	
Pre-Period (1988-1989)	-0.003 (0.026)	-0.006 (0.018)	-0.014 (0.012)	-0.009 (0.010)	-0.007 (0.008)	-0.004 (0.007)	-0.007 (0.007)	-0.011* (0.007)	-0.007 (0.007)	0.021 (0.026)	0.013 (0.032)	
1990 Average (Control):	8.528	9.062	9.333	9.501	9.622	9.722	9.820	9.932	10.110	10.508	10.806	
1990 Average (Treated):	8.576	9.116	9.384	9.538	9.649	9.744	9.841	9.954	10.125	10.709	11.077	
N, Firm-Years (Control):	8,684	8,684	8,684	8,684	8,684	8,684	8,684	8,684	8,684	6,766	6,773	
N, Firm-Years (Treated):	1,839	1,839	1,839	1,839	1,839	1,839	1,839	1,839	1,839	1,614	1,615	

Note: The table reports DiD effects on different percentiles of the within-firm wage distribution. See Table note for Table 3 for more information.

Table A.3: Effects on Separations and Measures of Job Quality, 2008 Reform

	Any Separation (1)	Job-to-Job Separation (2)	Separation into Nonemployment (3)	Sickness Spell (Older than 40) (4)	Sickness Spell (Male) (5)	Job Quality (z-score) (6)	Labor Relations Quality (z-score) (7)
<i>DiD: Year FEs</i>							
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	-0.003 (0.005)	-0.005 (0.004)	0.002 (0.004)	-0.004 (0.003)	-0.000 (0.003)	0.229 (0.167)	0.344** (0.153)
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	-0.004 (0.008)	-0.006 (0.005)	0.001 (0.005)	-0.003 (0.004)	0.003 (0.004)	0.267 (0.213)	0.009 (0.199)
<i>DiD: Industry-Year FEs</i>							
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	-0.003 (0.005)	-0.004 (0.004)	0.001 (0.004)	-0.004 (0.003)	-0.000 (0.003)	0.211 (0.165)	0.286* (0.154)
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	-0.006 (0.008)	-0.006 (0.005)	0.000 (0.005)	-0.003 (0.004)	0.003 (0.004)	0.349 (0.224)	-0.027 (0.200)
<i>DiD: Year and Firm FEs</i>							
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	-0.004 (0.005)	-0.000 (0.003)	-0.004 (0.004)	-0.005* (0.003)	0.001 (0.003)		
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	0.002 (0.007)	-0.004 (0.005)	0.006 (0.005)	-0.002 (0.004)	0.005 (0.004)		
<i>DiD: Industry-Year and Firm FEs</i>							
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	-0.004 (0.005)	-0.000 (0.003)	-0.004 (0.004)	-0.005* (0.003)	0.001 (0.003)		
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	0.001 (0.007)	-0.004 (0.005)	0.005 (0.005)	-0.002 (0.004)	0.005 (0.004)		
2007 Average (Treated Firms):	0.244	0.078	0.166	0.063	0.081	-0.236	-0.128
2007 Average (Smaller Control Firms):	0.240	0.070	0.170	0.058	0.075	-0.001	0.157
2007 Average (Larger Control Firms):	0.237	0.074	0.163	0.058	0.083	-0.114	-0.220
N, Firm-Years (Treated Firms):	15,074	15,074	15,074	14,795	14,624	353	399
N, Firm-Years (Smaller Control Firms):	46,035	46,035	46,035	44,453	43,451	569	610
N, Firm-Years (Larger Control Firms):	7,003	7,003	7,003	6,929	6,876	220	242

Note: The table reports DiD effects of the 2008 reform, which affected firms with 20 to 29 employees. We report estimates relative to two separate control groups of firms with employment of 10 to 19 and 30 to 39 employees, respectively, in 2005. The treatment group is defined as firms with 2005 employment of 20 to 29 employees. All point estimates are reported relative to 2007, the year for which we normalize the difference between treatment and the relevant control group to zero. Treatment indicates the DiD treatment effect in the post-reform period from 2008 to 2013. We report estimates using either smaller or larger firms as the comparison group. The first panel reports DiD results without additional control variables, the second panel includes industry-year effects (NACE Level 1), the third and fourth panel repeat the same specifications including firm effects. Standard errors clustered at the firm level are reported in parentheses. We plot some outcomes in Figures 6 and the remaining ones in Figure A.13. Since the last round of Quality of Work Life Survey prior to the reform was conducted at 2003, for Job Quality and Labor Relations Quality, the “2007 Average” corresponds to the 2003 wave; the post-period for the survey draws on the 2013 wave.

Table A.4: Effects on Measures of Firm Performance, 2008 Reform

	Mean Log Wage (1)	AKM Pay Premium (2)	Labor Share (3)	Firm Survival (4)	Log Value Added per Worker (5)	Capital Intensity (6)	Total Factor Productivity (7)	Profit Margin (8)
<i>DiD: Year FEs</i>								
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	0.011 (0.008)	-0.002 (0.005)	0.002 (0.005)	0.004 (0.006)	0.012 (0.014)	0.040 (0.032)	-0.030 (0.022)	-0.003 (0.005)
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	-0.004 (0.011)	-0.010 (0.007)	-0.002 (0.008)	0.002 (0.010)	0.040* (0.021)	0.030 (0.051)	0.002 (0.035)	-0.002 (0.007)
<i>DiD: Industry-Year FEs</i>								
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	0.012 (0.008)	-0.001 (0.005)	0.002 (0.005)	0.003 (0.006)	0.010 (0.014)	0.026 (0.030)	-0.012 (0.019)	-0.002 (0.005)
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	-0.005 (0.010)	-0.009 (0.007)	-0.003 (0.008)	0.001 (0.010)	0.027 (0.021)	0.010 (0.049)	0.003 (0.031)	-0.003 (0.007)
<i>DiD: Year and Firm FEs</i>								
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	0.012* (0.007)	-0.003 (0.005)	0.004 (0.005)	0.004 (0.006)	-0.002 (0.012)	-0.025 (0.023)	0.009 (0.015)	-0.005 (0.005)
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	0.001 (0.010)	-0.010 (0.006)	0.005 (0.007)	0.002 (0.010)	0.008 (0.017)	-0.033 (0.037)	0.018 (0.023)	-0.005 (0.007)
<i>DiD: Industry-Year and Firm FEs</i>								
Smaller Control Firms (10 ≤ Emp <sub>2005</sub> < 20)	0.013* (0.007)	-0.004 (0.005)	0.003 (0.005)	0.003 (0.006)	0.003 (0.012)	-0.026 (0.023)	0.014 (0.015)	-0.004 (0.005)
Larger Control Firms (30 ≤ Emp <sub>2005</sub> < 40)	0.001 (0.010)	-0.009 (0.007)	0.004 (0.007)	0.001 (0.010)	0.002 (0.017)	-0.034 (0.037)	0.014 (0.023)	-0.006 (0.007)
2007 Average (Treated Firms):	9.889	0.005	0.716	0.947	10.627	9.526	6.558	0.017
2007 Average (Smaller Control Firms):	9.838	-0.013	0.708	0.944	10.595	9.437	6.570	0.026
2007 Average (Larger Control Firms):	9.927	0.014	0.711	0.946	10.709	9.641	6.556	0.016
N, Firm-Years (Treated Firms):	15,254	14,048	12,605	18,536	12,176	12,365	11,836	12,307
N, Firm-Years (Smaller Control Firms):	46,607	42,191	38,112	57,190	36,848	37,254	35,883	37,327
N, Firm-Years (Larger Control Firms):	7,048	6,458	5,717	8,568	5,542	5,622	5,384	5,580

Note: The table reports DiD effects of the 2008 reform, which affected firms with 20 to 29 employees. We report estimates relative to two separate control groups of firms with employment of 10 to 19 and 30 to 39 employees, respectively, in 2005. The treatment group is defined as firms with 2005 employment of 20 to 29 employees. All point estimates are reported relative to 2007, the year for which we normalize the difference between treatment and the relevant control group to zero. Treatment indicates the DiD treatment effect in the post-reform period from 2008 to 2013. We report estimates using either smaller or larger firms as the comparison group. The first panel reports DiD results without additional control variables, the second panel includes industry-year effects (NACE Level 1), the third and fourth panel repeat the same specifications including firm effects. Standard errors clustered at the firm level are reported in parentheses. We plot some outcomes in Figures 6 and the remaining ones in Figure A.13.

## C Regression Discontinuity Design

As a complement to the difference-in-differences design studying the 1991 introduction of shared governance, we implement a more local regression discontinuity design comparing firms above and below the 150-employee threshold over a longer horizon of 25 years, from the introduction of the policy in 1991 to the end of our data in 2016. Overall, while we do not provide a detailed interpretation due to the *a priori* caveats reiterated below, the estimates broadly support the limited and small effects documented in the DiD design (and we can here additionally measure capital investment and dividends).

**A Priori Caveats** We reiterate our caveats flagged in Section 6. Compared to the DiD design made possible by the reform-based quasi-experiment, we view the RD design not as compelling as (i) there need not be a permanent policy discontinuity at 150 employees (due to firms above/below the cutoff moving in and out of transitory treatment, due to lagged or anticipation effects), (ii) the running variable is not sharply defined due to some discretion in the employment measure, and (iii) due to concerns of firm selection around the cutoff. These concerns have motivated our reform-based DiD design in the first place.

**RD Specification** Our regression model for the regression discontinuity design is:

$$y_{it} = \alpha + \underbrace{\beta_1 \mathbb{1}[N_{it-1} \geq 150]}_{\text{Worker Rep.}} + \beta_2(N_{it-1} - 150) + \beta_3 \mathbb{1}[N_{it-1} \geq 150](N_{it-1} - 150) + \nu_{t,J(i)} + \epsilon_{it}, \quad (\text{A.1})$$

where  $y_{it}$  denotes the outcome of firm  $i$  in year  $t$ . The running variable  $N_{it-1}$  corresponds to the employment concept relevant to the codetermination law. That is, it counts all employees with more than 90 days of employment and positive earnings in a given year; we do not count short temporary job contracts, such as seasonal workers. The regression discontinuity design uses the same employment definition as the main analysis, namely a snapshot definition for December 31st of a given (previous) year, which is the best approximation to the employment concept that triggers codetermination in the subsequent year. Hence, we match the outcomes variables of a given year to the employment number of the previous year. In addition to this linear specification, we also report results from a quadratic one.

Importantly, there are no other policy discontinuities, such as tax incentives or administrative burdens, that kick in at the 150 employee threshold. The coefficient of interest is  $\beta_1$  and captures the effect of the right to worker representation. To increase precision, our specification also includes industry-year effects,  $\nu_{t,J(i)}$ . Finally, we winsorize all continuous outcomes  $y_{it}$  at the 1% level.

**Tax and Accounting Data from Finnish Tax Administration (1994 to 2016)** We merge on firm-level tax and accounting data from the Finnish Tax Administration, which covers all firms from 1994 to 2016. This data set contains the additional variables (investments and dividends), which we use in our RD analysis, but not available in our DiD sample period, as discussed in Section 6.

**Bandwidth Choice and Inference** Our main specification uses the bandwidth choice procedure in Calonico, Cattaneo, and Titiunik (2014), CCT in the following, with a triangular kernel. We cluster standard errors at the firm level.

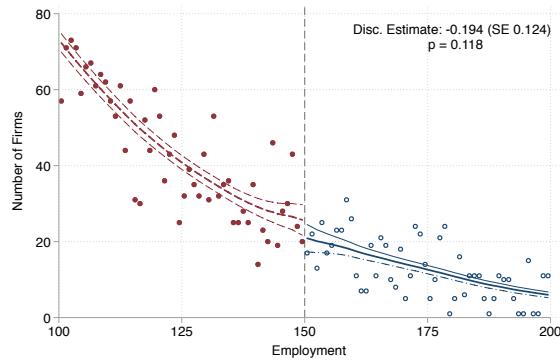
**McCrary Test** We implement a McCrary (2008) test for discontinuity of the density of firms at the 150 employee threshold and plot the density in Figure A.14. The corresponding McCrary (2008) test does not reject continuity of the density at 150 employees ( $p = 0.118$ ), among observations of the post-reform period (1991-1997, we find  $p = 0.485$  when considering the maximum post-reform horizon to 2016 to maximize observations and power).

As we discuss in Section 6, the absence of bunching to the left of the 150 threshold is already a substantial result, as it shows that firms do not manipulate their size to avoid, or seek, falling under the shared governance policy (thereby differing from, e.g., evidence for size-dependent regulations to distort firm size in France and Finland, see Garicano, Lelarge, and Van Reenen, 2016; Harju, Matikka, and Rauhanen, 2019).

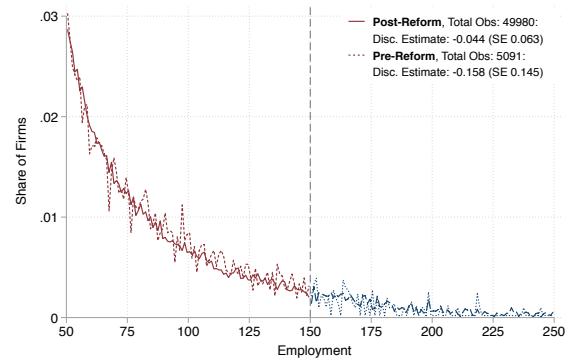
**Graphic Illustration** We visualize the data and research design using quadratic specifications and binned scatter plots. For consistency across graphs, we plot the same bandwidth of 50 employees around the threshold. As in our regression specifications, we use a triangular kernel around the policy discontinuity for weighting. We report the  $\widehat{\beta}_1$  and its standard error from our regression specification in the figures as well. These are overall quantitatively very similar. Potential differences between the RD effect visualized in the figure and the preferred regression specification arise from (i) differences in bandwidth (optimal CCT bandwidth vs. fixed), (ii) inclusion of industry and year effects, and (iii) the CCT bias-correction in the regressions.

Figure A.14: Density of Firm Size and McCrary Tests

(a) McCrary: Pooled

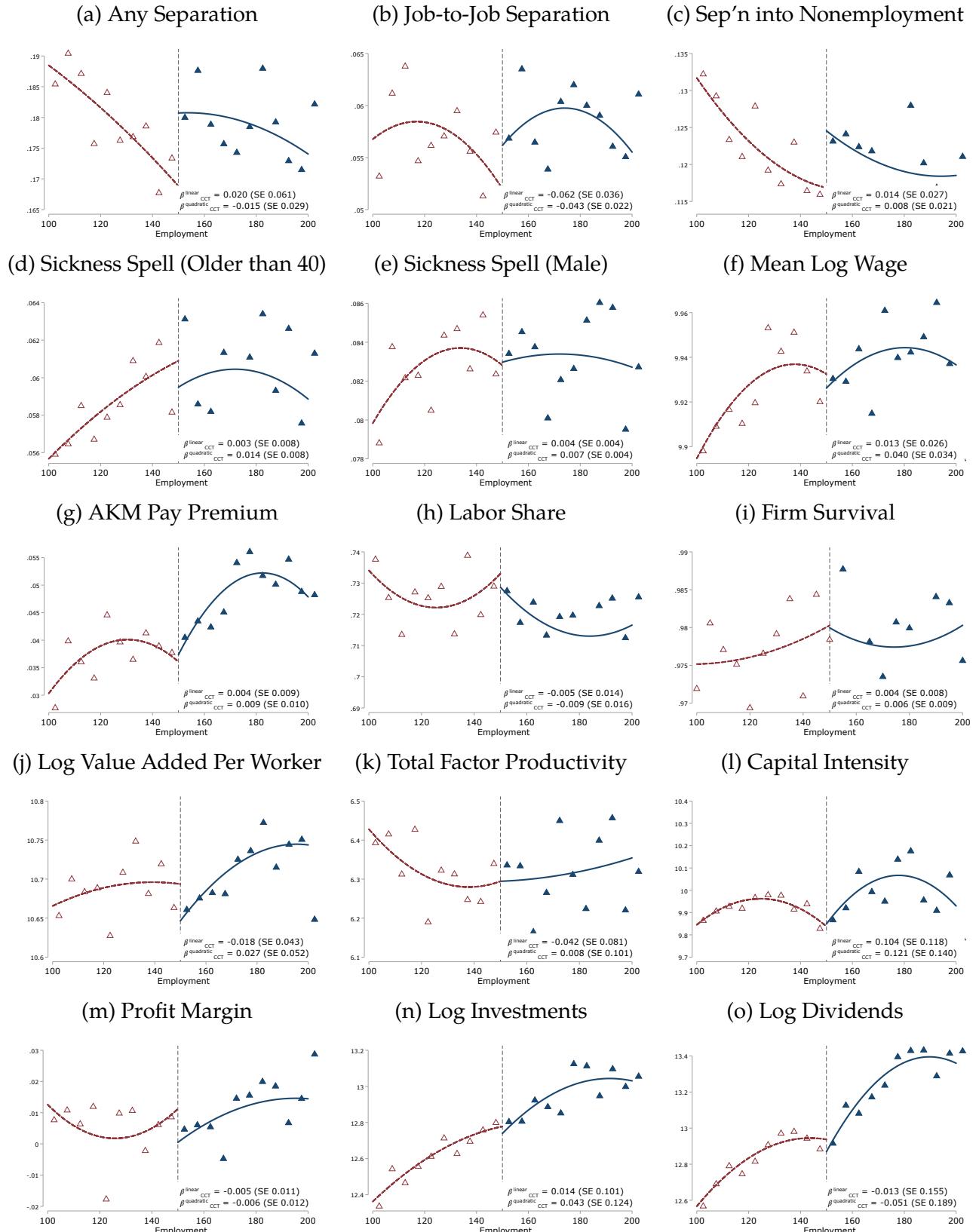


(b) McCrary: Pre and Post



*Note:* The figure reports density plots and results of McCrary (2008) tests for continuity of the density at policy discontinuity of 150 employees. Panel (a) reports results for the pooled sample period from 1991 to 1997. Panel (b) reports the results separately for the pre-reform period from 1988 to 1990 (dashed line) and the post-reform period from 1991 to 2016 (solid line).

Figure A.15: Regression Discontinuity Design



Note: The figure presents RD estimates based on the employment threshold of 150. Potential differences between the RD effect visualized in the figure and the preferred regression specification arise from (i) differences in bandwidth (optimal CCT bandwidth vs. fixed), (ii) inclusion of industry-year effects, and (iii) the CCT bias-correction in the regressions.

## D Data Appendix: Additional Details on Variable Construction

### D.1 Constructing a Revealed-Preference Index of Firm Value

We study a revealed-preference measure of job quality that uses the full information on the quantity and direction of job-to-job transitions. Specifically, we draw on the PageRank algorithm, used and extended by Sorkin (2018) to provide a revealed-preference ranking of US employers. We provide a summary of our implementation here.

#### D.1.1 The Sorkin (2018) Procedure

Let  $F$  be the set of firms in our firm ranking analysis. The procedure uses worker flows from employer  $g$  to employer  $f$ , denoted  $M_{fg}$ , to estimate relative job values and to assign a common value  $V_f$  to each employer  $f \in F$ . In the underlying decision model, in the spirit of an on-the-job search model, employed workers receive up to one outside offer each period. Let  $\lambda_f$  denote the probability workers receive an offer from firm  $f$ . After receiving an offer, workers choose whether to accept by comparing firm common values and independently drawn idiosyncratic utility shocks associated with staying and leaving. Specifically, incumbent employees receive a value of  $V_g + \nu_1$  from their current employer  $g$ , and a value of  $V_f + \nu_2$  from accepting a potential offer from an employer  $f$ . In this discrete choice setting, the number of workers switching from employer  $g$  to  $f$  is then given by

$$M_{fg} = \lambda_f N_g \Pr(\text{Accept} | \text{Offer}), \quad (\text{A.2})$$

where  $N_g$  is the number of workers employed at firm  $g$ . Under the assumption that utility shocks are drawn from a type I extreme value distribution,  $\Pr(\text{Accept} | \text{Offer}) = \frac{\exp V_f}{\exp V_f + \exp V_g}$ . Assuming in addition that the ratio of offers to firm size  $\lambda_f/N_f$  is constant across firms, Equation (A.2) yields the following relationship between firm common values:

$$\begin{aligned} \frac{M_{fg}}{M_{gf}} &= \frac{\lambda_f N_g \exp V_f}{\lambda_g N_f \exp V_g} \\ \frac{M_{fg}}{M_{gf}} &= \frac{\exp V_f}{\exp V_g} \\ \frac{\sum_{g \in F} M_{fg} \exp V_g}{\sum_{g \in F} M_{gf}} &= \exp V_f \quad \forall f \in F. \end{aligned} \quad (\text{A.3})$$

Intuitively, a firm's value is a weighted average of the values of the firms it hires from, where weights are given by the size of hiring flows relative to total exit out of the firm. We estimate firm values from the linear system these equations define using a power iteration algorithm, detailed below.

This stylized framework assumes that all job-to-job transitions are informative about workers' preferences and that firms extend offers at rates proportional to their size. Sorkin (2018) uses a richer model to relax these assumptions. In particular, he accounts for the fact that some separations are exogenous (e.g. resulting from layoffs) by down-weighting separations at contracting firms, while also allowing offer intensities to differ across employers. We adopt the more parsimonious approach, since Sorkin finds that three-quarters of job-to-job separations are endogenous, and relaxing the assumptions above does not qualitatively change his main findings.

Since information on firm values come from relative worker flows, the identification condition for this estimation procedure is that firms be strongly connected. (To be part of a strongly connected set, a firm must hire at least one worker from, and lose at least one worker to, other firms in the strongly connected set.) We estimate the values  $V_f$  separately in the windows before and after the reform (1988-1990, 1992-1997 respectively) in the largest strongly connected set of firms in each window.

### D.1.2 Defining Employer-to-Employer (EE) Transitions for the Sorkin (2018) Procedure

For this exercise, we take the following steps to define EE transitions (which deviate from our main definition of job-to-job transitions by maximizing the use of transitions on the spell level rather than annual perspectives). Following Sorkin (2018), we drop any firm whose median number of yearly non-singleton employees is below 10. Here, a non-singleton employee is one who appears at least twice within the period of the analysis.

One of the key assumptions of the methodology proposed by Sorkin (2018) is that the moves used to estimate the firm ranking should be driven by employees' preferences. That is, we need to identify worker-initiated EE transitions, at least up to a large and homogeneous proportion among moves across firms. One of our challenges lies with the structure of the dataset prior to 1995. Before 1995, our matched employer-employee dataset only recorded start date and end date, employer ID and employee ID of spells, together with an aggregated annual income of an employee. Thus, we cannot use wage-based strategies such as the one used in Sorkin (2018) to determine the dominant employer at a given period for an employee when spells overlap. To overcome this challenge, we posit that the longer a spell is, the more likely the employer is a dominant one to a given employee. Based on this assumption, we use the following rule to determine dominant employers.

- Spells shorter than 3 months are not regarded as dominant.
- When there are spells that are completely contained or almost completely contained (start no earlier than 30 days before or end no more than 30 days after) by other spells, the one with the longest duration is the dominant one.

Hence, an EE transition occurs when a worker ends a dominant spell and begins a dominant spell at a different employer within plus or minus 30 days (has a gap or an overlap of no more than 30 days).

### D.1.3 Computational Details

We now detail key steps toward constructing the firm ranks.

**Strongly Connected Sets** To find firms in strongly connected sets, we start by retaining all firms with both inflow and outflows. Following Sorkin (2018), we use Tarjan's algorithm to identify the largest strongly connected sets in the pre- and post-periods. We then estimate firm values for the firms within these sets, separately in the pre- and post-periods.

**Solving for Firm Values** Due to the high-dimensional linear system of Equations (A.3), we follow Sorkin (2018) and use the power iteration algorithm to approximate the solution, stopping when the difference in norms of  $\exp \vec{V}$  between two adjacent iterations is smaller than 0.001.

**Transformation of Firm Ranking Index** The estimated Page Ranks have a clear interpretation based on the on-the-job search model. In particular, the raw indices corresponds to the exponential of the firm value. In Table 2, we report DiD estimates for the effects on the estimated firm values,  $\hat{V}_f$ . To better interpret effect sizes, we transform firm values into a z-score, using the mean and standard deviation of the pre-period index. We perform this standardization using the pre-period distribution in both periods to avoid masking treatment effects. We also report the DiD estimates using the raw index  $\widehat{\exp(V_f)}$  as an outcome in Column (3) and Column (4) of Table A.5.

**Sample Selection** Since the estimable set of firm values before and after the reform do not perfectly overlap, we restrict our sample to firms belonging to the intersection of both sets for the result reported in Table 2. In this section, we additionally report the estimates of firm ranking value for the union of the two strongest connected sets in the pre- and post-period, in Column (2) and Column (4) of Table A.5.

Table A.5: Effects on Firm Ranking Index

	Firm Value Log Index z-score (Intersection) (1)	Firm Value Log Index z-score (Union) (2)	Firm Value Index z-score (Intersection) (3)	Firm Value Index z-score (Union) (4)
<i>DiD: Year FEs</i>				
Treatment (1991-1997)	-0.043 (0.105)	-0.055 (0.093)	-0.013 (0.087)	0.004 (0.064)
<i>DiD: Industry-Year FEs</i>				
Treatment (1991-1997)	-0.049 (0.104)	-0.072 (0.092)	0.021 (0.085)	0.019 (0.063)
<i>DiD: Year and Firm FEs</i>				
Treatment (1991-1997)	-0.053 (0.107)	-0.056 (0.100)	-0.017 (0.084)	0.015 (0.067)
<i>DiD: Industry-Year and Firm FEs</i>				
Treatment (1991-1997)	-0.065 (0.104)	-0.068 (0.098)	0.014 (0.082)	0.041 (0.067)
1990 Average (Control):	-0.008	0.014	0.000	0.006
1990 Average (Treated):	0.045	0.138	0.009	0.076
N, Firm-Years (Control):	4,402	4,584	4,403	4,585
N, Firm-Years (Treated):	1,409	1,416	1,409	1,416

*Note:* The table reports results of DiD specifications as in Equation (1). Column (1) uses the logarithm of the raw index as an outcome, and reports the effects for the intersection sample of firms between pre- and post-period strongest connected sets. Results of this column are also reported in Table 2 Column (4). Instead of the intersection set of firms, Column (2) uses the union of the two sets of firms. Column (3) and Column (4) report effects using the raw index in the intersection and union sets of firms, respectively, as outcome variables.

## D.2 Quality of Work Life Survey

For our analysis of the 1991 reform, we draw on the 1990 and 1997 waves of the Finnish Quality of Work Life Survey, merged with the administrative firm-level data, to assess effects of the reform on subjective measures of worker voice, labor relations and job quality. (For our analysis of the 2007 reform, we draw on the waves in 2003 and 2013, where we skip the 2008 wave, which is ambiguously timed as the 2007 reform became active in 2008.)

We construct measures of job quality and of the quality of labor relations using factor analysis. After selecting a set of variables (listed below) for a specific measure, we transform each variable into a z-score and apply principal factor analysis, extracting a single factor using the regression method. We then normalize the extracted factor into a z-score using the post-reform mean and standard deviation of firms in our sample without a worker right to shared governance in 1997 (i.e. firms with fewer than 150 employees) and normalize it such that higher values indicate higher worker voice, job quality, or quality of labor relations.

**Survey Items for Construction of Job Quality Index** We select the following variables, measured on Likert scales or as indicator variables and available in both waves we consider

(two per reform), for our construction of our job quality index:

1. It is hard to focus on home due to issues at work.
2. Do you have a fair wage compared to other jobs?
3. Do you feel unwilling or mentally tired to go to work?
4. Have you had a work related accident during past 12 months?
5. How boring is your job?
6. How physically demanding is your job?
7. How mentally demanding is your job?
8. How demanding the pace of work in your job?
9. Tasks are well organized at our firm.
10. There are too few employees in our firm for the tasks.
11. There is an open atmosphere and team spirit in our firm.
12. My supervisor supports and encourages me.
13. My supervisor inspires me.
14. Negative working conditions (17 sub-items).
15. Uncertainty related to: transfer to other tasks.
16. Uncertainty related to: furloughs.
17. Uncertainty related to: layoffs.
18. Uncertainty related to: unemployment.
19. Uncertainty related to: disability.
20. Opportunities to develop skills.
21. Importance of wage vs. content of work.

**Survey Items for Construction of Labor Relations Index** We select the following variables, measured on Likert scales or as indicator variables and available in all three waves we consider, for our construction of our labor relations index:

1. Do you belong to a labor union?
2. Conflicts between managers and employees in your working unit
3. Conflicts between employees in your working unit
4. Conflicts between different employee groups in your working unit
5. My supervisor supports and encourages me.
6. My supervisor actively interacts with employees.
7. My supervisor openly informs employees about all decisions.
8. My supervisor trusts the employees.
9. When do you usually receive information about changes in your work tasks? (1 = Already at the planning stage; 2 = Just before the actual change; 3 = When the change has been decided or after).
10. There is an open atmosphere and team spirit in our firm.

### D.3 Executive Compensation

We use individual-level occupation data (3-digit) to define chief executives and managing directors. Then we merge these data with annual wage and total earned and capital income for each individual, where earned income includes all wage income, benefits and pension income, and capital income contains dividend, entrepreneurial, rental and interest income, among others. Finally, we link these data with employee-employer level data to form firm-executive pairs. When we observe two or more executives for a single firm in a year, we select the highest-paid executive based on use the maximum total income measure for executive pay (earned and capital income).

There are two caveats regarding the data. First, the classification of occupations information is only available for a subset of years (1990, 1993, 1995, 2000 and 2004 onward). We address this issue by filling the data when we observe the same individual being an executive in the same firm over time. We also fill data backward for years 1988 and 1989 and assume that executives in 1990 are also executives in earlier years. Second, due to institutional reasons, in some cases it could be that chief executives are not actually employed with the firm and are thus missing from the employee-employer data set. However, our data still defines the highest paid executive for 91.2% of the firm-year pairs and thus offers a reasonable proxy for executive pay. We study wage income and total income (earned and capital income) separately as a measure of executive pay. In addition, we study the executive pay share that is the total income of all firm-level labor costs. For this measure, we draw on all executives at a firm.

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