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Abstract

We estimate the effects of worker voice on productivity, job quality, and separations. We study the 1991 introduction of a right to worker representation on boards or advisory councils in Finnish firms, designed primarily to facilitate workforce-management communication. The reform only affected firms with at least 150 employees, permitting a difference-in-differences design to analyze its causal effects. Consistent with information sharing theories, worker voice slightly raised labor productivity, firm survival, and capital intensity. In contrast to the exit-voice theory, we find 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). A 2008 introduction of shop-floor representation had similarly limited effects.

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Voice at Work*

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1 Introduction

Workers can influence the decision-making of their employer either through *exit*—by quitting—or through *voice*—giving feedback. Worker voice has been hypothesized to reduce turnover (Hirschman, 1970) and increase productivity by improving information flows, coordination, and workforce-management cooperation (Malcomson, 1983; Freeman and Lazear, 1995). Yet, there is little *causal* evidence on how organizational cultures of worker voice affect firm and worker outcomes.[1] While many countries mandate some form of worker voice (Hall and Soskice, 2001; Jäger, Noy, and Schoefer, forthcoming), these voice mandates are typically bundled with other rules—such as strong labor regulations or re-allocations of formal corporate decision-making power—making the effects of *voice* difficult to disentangle. As one prominent example, Garicano, Lelarge, and Van Reenen (2016) estimate negative effects of a complex set of regulations kicking in at a firm size threshold in France, but primarily attribute them to firing regulations rather than the works council mandate. Similarly, the large existing literature studying German codetermination is unable to separate the effects of giving workers voice from the effects of re-allocating authority to workers (Jensen and Meckling, 1979). An ideal experiment to surgically uncover the causal effects of worker voice *alone* would institutionalize information exchange between managers and workers *without* simultaneously reallocating formal decision rights or increasing regulatory burdens.

We exploit two natural experiments in Finland to study the causal effects of worker voice. Our main design draws on the 1991 introduction of a right to worker board representation. Relative to its Scandinavian peers, Finland was a late arrival to board representation, after the advent of modern administrative micro data.[2] Most

[1]Important recent evidence from field experiments in India and China suggests that specific measures improving worker voice boost productivity or worker satisfaction, e.g., by introducing anonymous surveys and participatory meetings at specific firms (Adhvaryu, Molina, and Nyshadham, 2021; Adhvaryu, Gade, Molina, and Nyshadham, 2021; Cai and Wang, 2021; Levy Paluck and Wu, 2021).

[2]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. As an exception, Svejnar (1981) studies the wage effects of an introduction of parity codetermination in 1951 in the iron, steel and mining sector using industry-level data in Germany. Jäger, Schoefer, and Heining (2020) study the abolition of board-level representation in certain newly established firms in

importantly, Finnish implementation of board representation is, by design and in practice, primarily a voice institution, providing exceptionally limited shifts in formal authority to workers. Specifically, the reform provided a right to representation in firms with 150 or more workers, but explicitly left the concrete implementation to be negotiated between workers and the firm. The most common *de facto* form of representation is through an advisory council established by mutual agreement (see also Lekvall et al., 2014). As a fallback option, if no agreement is reached, workers have a right to elect representatives who take 20% of the seats on either the board of directors, the board of supervisors, or the management body—with the specific body selected by the firm. Survey and in-depth interviews we conducted with worker representatives confirm that they view their role as about improving communication, information sharing, and cooperation, but do not believe their role comes with direct decision-making power.

The 1991 reform permits a difference-in-differences research design, comparing 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 robustness checks such as restricting our sample to firms closer to the cutoff or dropping firms very close to it. As another robustness check, we also study a 2008 expansion of shop-floor representation (an institution similarly focused on information and consultation) to small firms, ruling out that this institution independently affects worker and firm outcomes, or even masks effects of the 1991 reform by already fulfilling workers' demands for voice (see also Keskinen, 2017). (Finnish shop-floor representation is much weaker than other European examples, such as German works councils or Swedish and Norwegian shop-floor representatives, who hold some veto powers.)

As the first takeaway, our study provides no support for the exit-voice hypothesis, which predicts that worker voice reduces voluntary turnover and increases job quality from the perspective of workers. We find small, statistically insignificant reductions in annual job-to-job (proxying for voluntary) transitions of about 0.7 percentage points. Turning to other measures of job quality, we find no clear effects on a revealed-preference measure of firm quality based on worker flows across

Germany in 1994, with a focus on wage effects.

firms (Sorkin, 2018). As worker representatives may prioritize non-wage amenities (Freeman and Medoff, 1985), we study worker health and workplace safety, finding precisely estimated zero effects on sickness spells. By contrast, we find some positive point estimates on subjective job quality in the Finnish Quality of Work Life Survey, of about 0.15 (SE 0.09) to 0.18 (SE 0.08) standard deviations.

These limited effects on job satisfaction contrast with the increase in job satisfaction and reduction in quit rates estimated in field experiments in the Indian garment industry and Chinese manufacturing sector, where voice is enhanced through technology (anonymous surveys) rather than through worker representatives (Adhvaryu, Molina, and Nyshadham, 2021; Adhvaryu, Gade, Molina, and Nyshadham, 2021; Cai and Wang, 2021). Differences in pre-existing cultures of worker involvement may explain the different findings.

Though we detect no effects on voluntary turnover, we find a small reduction in involuntary separations into nonemployment of about 2 percentage points (14%)—consistent with representatives' stated core objectives being avoiding layoffs and maintaining employment stability. These results are also consistent with theories of implicit contracts under which worker voice prevents layoffs by reducing information asymmetries about negative shocks (Malcomson, 1983), and with existing evidence suggesting that board representation reduces dismissals, among Danish, Norwegian, and Swedish firms (Gregorič and Rapp, 2019) or German firms (Kim, Maug, and Schneider, 2018).

As our last measure of workers' job quality, we find a marginally significant increase in composition-adjusted wages (following Abowd, Kramarz, and Margolis, 1999) of 1.6 percent, and can rule out effects above 3.6 percent; we can rule out increases in the labor share above 0.5 percentage points. We also find some evidence consistent with pay compression, with small wage gains concentrated among lower earners within the firm. Hence, we do not find evidence that workers experience wage cuts as compensating differentials for potential amenity gains, such as increased job stability (Baily, 1974; Azariadis, 1975). Instead, our results are more in line with theories about wage compression following worker representation and information sharing (Western and Rosenfeld, 2011). While we study a pure information-sharing institution, our findings complement a literature asking whether stronger formal codetermination rights permit workers to push for higher wages, which has produced mixed evidence studying, e.g., board-level codetermination in Germany (Kim, Maug, and Schneider, 2018; Gorton and Schmid, 2004; Jäger, Schoefer, and Heining, 2020; Redeker, 2019) and Norway (Blandhol, Mogstad, Nilsson, and Vestad, 2020) or German works councils (Hirsch and Mueller, 2020).

As a second takeaway, we do find small positive effects of worker voice on firm performance—consistent with an important strand of the literature (Freeman and Medoff, 1985; Freeman and Lazear, 1995).[3] We find moderate increases in labor productivity of 0.067 (SE 0.031) in our preferred specification. Similarly, we find moderately positive but statistically insignificant effects on firm survival, total factor productivity, and the capital-labor ratio. We find precisely estimated zero effects on the profit margin. We also find no evidence for avoidance in the form of bunching below the size threshold that would be predicted if the regulation were costly—as in the important case of Garicano, Lelarge, and Van Reenen (2016) in France, where works council thresholds go along with heavy labor regulation.

This second takeaway, about firm performance, complements existing work documenting positive effects of (vertical) information sharing within firms (see, e.g., Impink, Prat, and Sadun, 2021) and on management practices that involve workers in decision-making (see, e.g., Ichniowski, Shaw, and Prennushi, 1997; Bloom and Van Reenen, 2011; Cai and Wang, 2021; Levy Paluck and Wu, 2021). Our causal estimates also complement existing evidence on the positive correlation between cooperation or trust within firms and productivity (see, e.g., Krueger and Mas, 2004; Bloom, Sadun, and Van Reenen, 2012; Hjort, 2014). We also contribute to the literature on codetermination, which studies institutions that combine worker voice with formal decision-making rights for workers, and has found some evidence for positive productivity effects (Addison, 2009; Jäger, Schoefer, and Heining, 2020; Jäger, Noy, and Schoefer, forthcoming). Our study uniquely provides quasi-experimental evidence from the introduction of a pure voice institution, independent

[3] A separate literature cautions that increasing worker voice, to the extent that it goes along with shift in bargaining power, may instead have negative effects, e.g., by exacerbating agency conflicts or discouraging capital formation (Jensen and Meckling, 1979).

of simultaneous changes in labor market regulation or shifts in formal authority.

Outline In Section 2, we describe the reform and institutional context. Section 3 presents the research design and the data. Results on separations and job quality are presented in Section 4. Section 5 analyzes effects on firm performance. In Section 6, we study the 2008 reform of shop-floor representation. Section 7 concludes.

2 Institutional Context and Reform

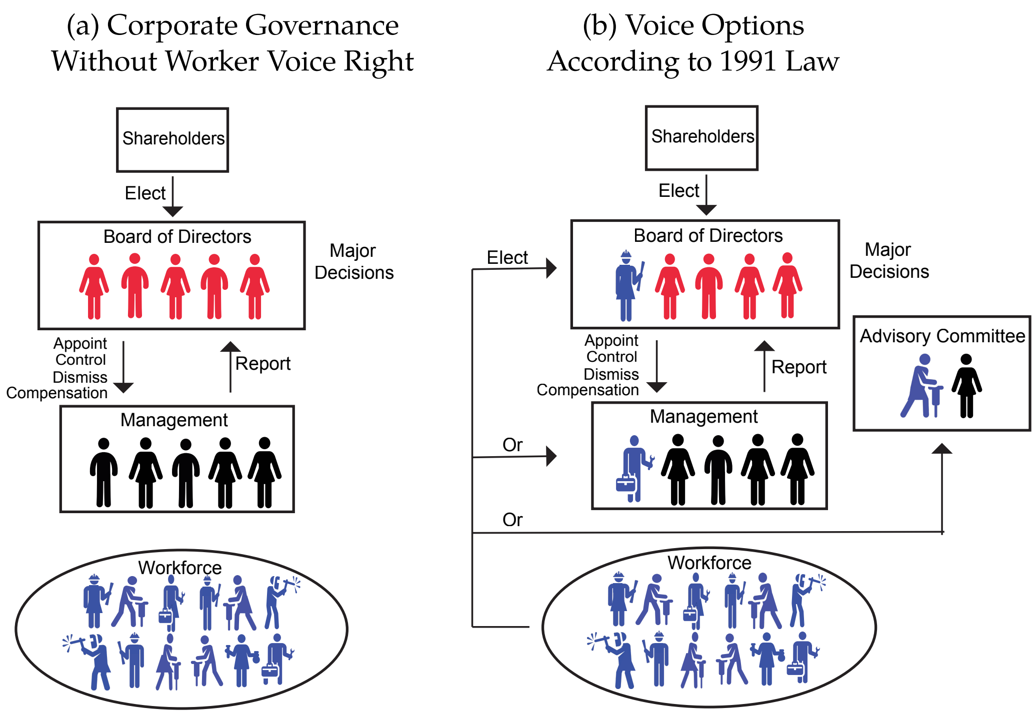
We describe corporate governance in Finland, the 1991 introduction of a right to worker voice, and additional worker voice and wage setting institutions.

Corporate Governance in Finland Finnish companies follow the Nordic single-tier board structure and typically feature a board of directors elected by the general meeting of the shareholders (and, rarely, a dual board structure with a supervisory board). Lekvall et al. (2014) Appendix B reviews corporate governance in Finland. Figure 1 Panel (a) illustrates this board structure, without worker voice. 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 general meeting of the shareholders sets the compensation for the board.

The 1991 Reform Until 1991, Finnish workers lacked 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 a right to firm-level voice 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 legislation was the result of a political compromise, with employer associations opposing it, while the Social Democrats called for a lower threshold of 30 employees (Marttila, 2016, p. 224). The law allowed for worker representation by mutual agreement starting on January 1, 1991, and then installed the statutory right to board-level representation starting with the first general meeting held after July 1, 1992. It has been in place without major changes since 1991.

Figure 1 Panel (b) illustrates corporate governance with board-level voice. Worker

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 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 (with the firm choosing which body), as well as the more common case of implementation of worker representation in an advisory committee.

voice is typically set up through an agreement between the firm and 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. Statutorily, workers 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.

Besides setting a default of 20% board representation, the law explicitly permits flexibility in the organization of representation, unlike in other countries such as Germany. We draw on three existing surveys of worker representatives, as well as a survey of worker representatives and phone survey of HR managers we conducted in 2020, to understand how the institution operates in practice. Our review of the survey evidence suggests two conclusions.

First, *de facto*, worker representation primarily operates through agreements that establish "advisory councils" for workers to sit on or that appoint workers to boards without granting them equal voting rights. Appendix Table A.1 shows that among the 50-60% of firms that take up worker representation following the 1991 law, only about one-quarter follow the statutory provisions. The rest organize worker representation in a roughly equal split across membership in the board of directors, membership in the management board, and membership elsewhere (e.g., in advisory councils); among firms that appoint workers to a board, only half report that worker representatives have the same rights as other board members. Consistent with our findings, Lekvall et al. (2014) and Thomsen, Rose, and Kronborg (2016) find that formal worker board representation with the same rights as shareholders is rare among listed firms in Finland, although observers note this may be due to a late 2000s decline. Overall, the institutional setup increases voice through advisory, consultation, and information rights, rather than providing formal authority via voting power (such as mandatory board representation as in German stock corporations) or veto rights (as in the case of German works councils).

Second, in line with these formal institutional details, surveys and interviews of worker representatives indicate the institution's primary function is to facilitate information-sharing and cooperation rather than to boost worker power. We conducted interviews and surveys of worker representatives in collaboration with a major Finnish union in 2020 to characterize worker representatives' views on the institution and their role therein. A total of 111 respondents participated in our survey (20% response rate, see Appendix D.3 for a detailed overview of the survey and interview sample).

We first asked representatives about their goals in the role. Panel (a) of Figure 2 illustrates that the most common goals are good working conditions and the avoidance of redundancies or layoffs, followed by good salaries, employment stability, higher investment, and less outsourcing. However, as we illustrate in Panel (b) of Figure 2, the worker representatives ascribe to themselves only limited decision-making power in these core areas. The three domains in which worker representatives report the highest degrees of influence are the improvement of cooperation between management and employees, improvements in working conditions, and investments in worker wellbeing. However, even in these domains, only a minority of respondents (25-40%) believe they can exert influence. Meanwhile, fewer than 20% of respondents believe they can affect wage-setting decisions, and virtually no respondents (<5%) believe they can affect strategic decisions about production, outsourcing, or investment.

We also report challenges that worker representatives face in Panel (c) of Figure 2 and find some indication that a lack of cooperation or trust from management is a core obstacle to representatives' efficacy. In Panel (d), we report workers representatives' expectations for what would happen to a number of economic outcomes if worker representation were introduced in a firm. Here, we find that 20-45% expect positive effects on productivity, profits, investment, and wages, while 40-60% expect no effect and only a small minority (<5%) expect negative effects.

We complement the survey evidence with in-depth interviews we conducted with worker representatives from five major companies. In the interviews, representatives emphasize the information-sharing benefits of the institution, as is evident in the following representative quotes from each interview:

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

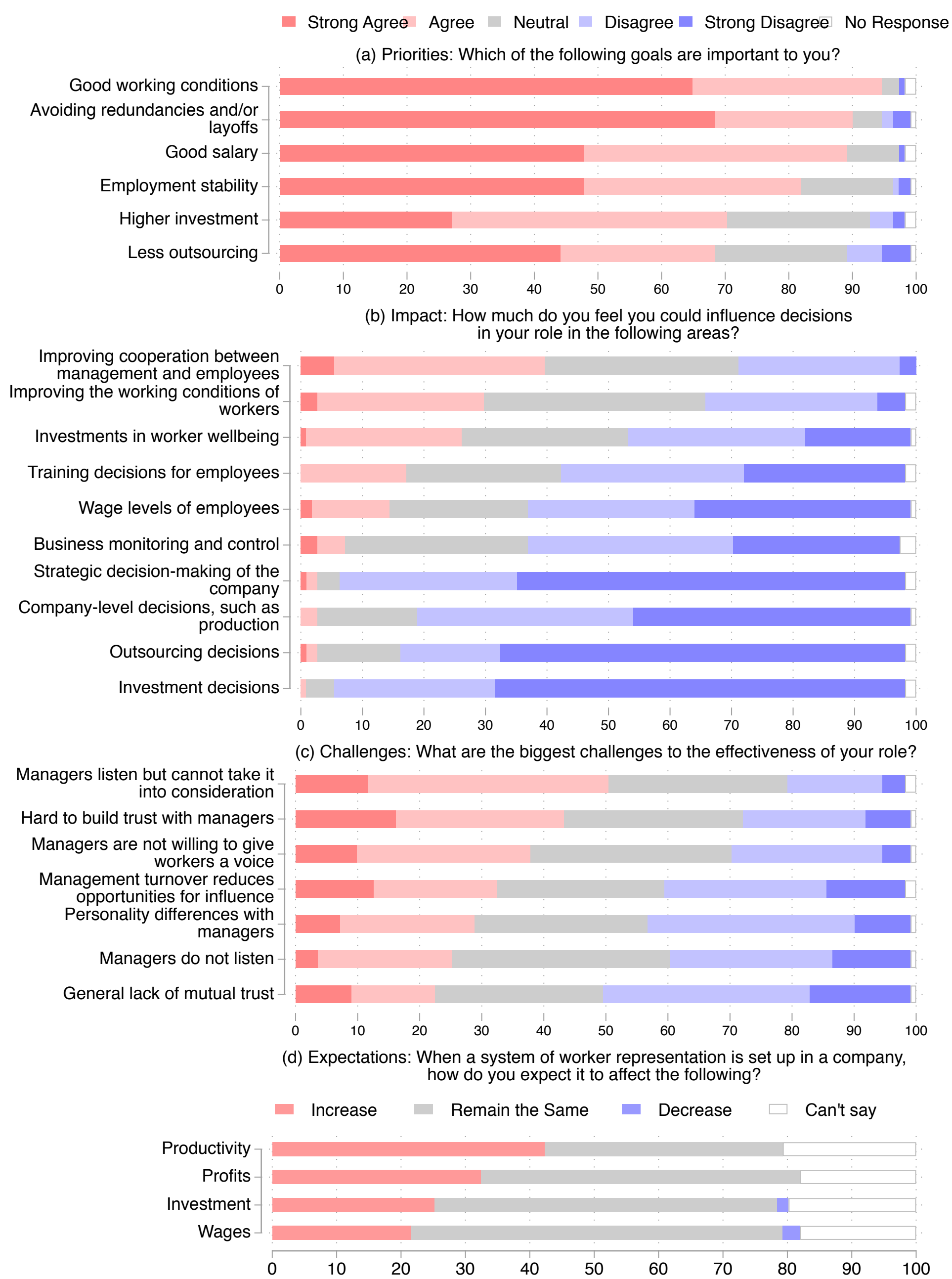
> It also often feels that the members of the management group want to talk to me because they feel that they are separated from the employees and want to hear my opinions. [...] I feel that I am a link between the employees and the management group.
>
> [...] I can bring the personnel's thoughts and ideas to the management team very freely. And bring different types of thinking from employees.
>
> [The role] improves information flow in the company, and giving people access to information makes it possible for them to influence matters.

Third, Finnish worker representatives appear to have weak decision-making influence not only in absolute terms, but also relative to worker representatives in other European countries. Figure 3 Panel (a) plots country-level mean responses to a question from the European Company Survey that asks worker representatives to assess their influence on the most important recent decision in their establishment. It shows that Finland ranks near the bottom, while countries like Germany or Sweden (that allocate codetermination powers to representatives) rank near the top.[4]

Overall, our survey and interview evidence indicates that the institution approximate a pure information-sharing and voice institution, rather than bundling voice with actual co-decision-making rights as in, e.g., Germany.

[4]We also plot responses to a question asked of workers, rather than worker representatives, about their own self-assessed voice, which we will discuss in the Conclusion.

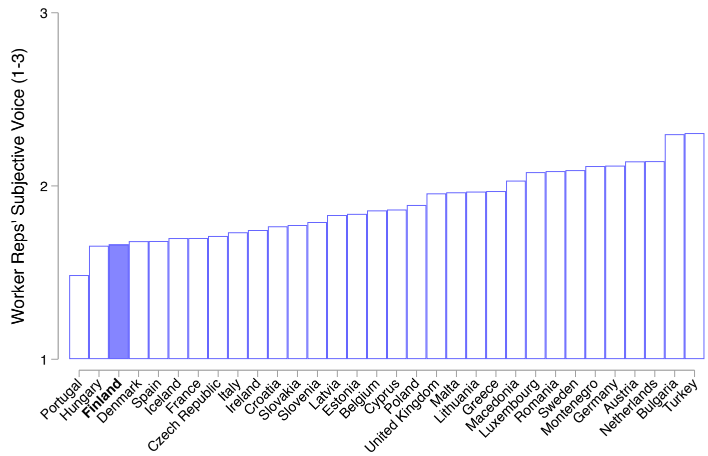
Figure 2: Survey Evidence on Worker Representatives' Perspectives



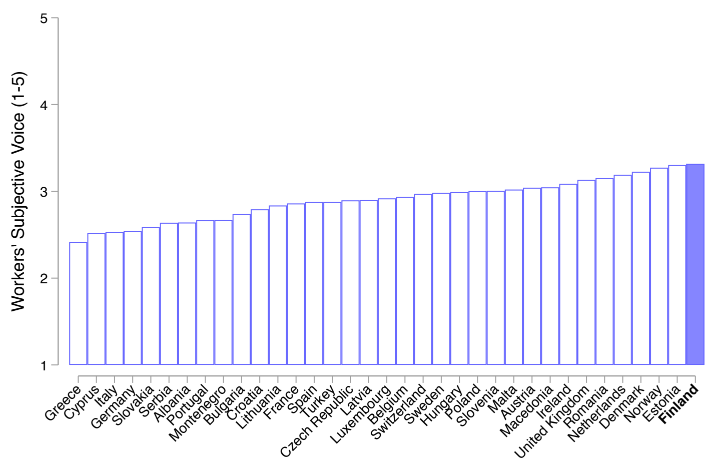
Note: The panels plot responses from our 2020 survey of 111 worker representatives in cooperation with a major trade union federation. For further details and statistics, see Appendix D.3 and Appendix Table A.1.

Figure 3: Formal and Informal Voice—Finland vs EU

(a) Worker Representatives' Self-Assessed Voice



(b) Workers' Self-Assessed Voice



Note: Panel (a) draws on data from the 2013 European Company Survey, which sampled several hundred establishments with $\geq$ 10 employees in each European country in 2013. It surveyed managers and (if present) worker representatives in those establishments. Here, we plot country-level mean responses to Q38 from the worker representative survey: representatives who report that managers in their establishment made a major decision in the past 12 months are asked how much they were able to influence that decision, on a 1-3 point scale ("no influence"/"some influence"/"strong influence"). Sample sizes vary across countries due to differences in the prevalence of worker representation in each country. The sample for Finland is 564 establishments, and among other countries the average sample size is 228 establishments (SD 138, minimum 41). Panel (b) draws on the 2015 European Working Conditions Survey, which covered a random sample of several hundred workers (employed and self-employed) in each European country in 2015. We draw on Q61 (c), (d), (e), and (n), which asked respondents on a 1-5 point scale how much they agree with the following statements: "I am consulted before objectives are set for my work," "I am involved in improving the work organisation or work processes of my department or organisation," "I have a say in the choice of my work colleagues," and "I can influence decisions that are important for my work." We take the average response to these four questions within each person, then take country-level means and plot them here. The sample size is 792 workers for Finland, and among other countries the average sample size is 1,026 workers (SD 455, minimum 553).

Other Worker Voice Institutions Several additional channels for worker voice exist in Finnish workplaces (Eurofound, 2020). First, sectoral collective bargaining agreements often provide for the election of shop-floor representatives. Besides company-level collective bargaining, these representatives have a variety of information and consultation rights, which entail the power to *delay* implementation but leave ultimate decision-making power to employers. In companies with at least 20 employees where no collective bargaining agreement guarantees 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 collective bargaining). A 2008 amendment lowered the threshold from 30 employees to 20; we leverage this reform in Section 6. Additionally, establishments with at least 10 employees must elect a health and safety representative. Coverage is high, especially in large firms. According to the 2009 European Company Survey (authors' own tabulations), 99% (100%) of Finnish establishments above 150 employees have a shop-floor representative (health and safety representative).

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 occupation- and job-level wage floors, which are rarely directly binding, as most employees receive pay premia above the floors (Uusitalo and Vartiainen, 2009). The local bargaining parties can agree to deviate from wage increases negotiated in collective agreements and can even negotiate pay cuts. Firm-specific pay policies, with profit-sharing arrangements and links between wages and productivity, have become increasingly common since the 1990s, with more than half of white-collar and about a third of blue-collar workers receiving some form of performance pay (Snellman, Uusitalo, and Vartiainen, 2003; Uusitalo and Vartiainen, 2009). Consistent with such firm-specific wage flexibility, in Section 4.5, we find that the Finnish firm-level rent sharing elasticity of wages to value added per worker, and the share of worker-level wage variation explained by firm wage policies, are in line with international estimates (Card, Cardoso, Heining, and Kline, 2018; Jäger, Schoefer, Young, and Zweimüller, 2020).

The 1990s Recession Between 1990 and 1993, Finland experienced a deep recession and a currency devaluation. Finland had become dependent on trade with the Soviet Union, and the Union's dissolution hit Finnish exporting industries (Gorodnichenko, Mendoza, and Tesar, 2012) and raised energy prices. Finland also experienced a credit crunch in 1992 (Gulan, Haavio, and Kilponen, 2014). The recovery in the mid-1990s was accompanied by a sectoral reallocation, with manufacturing mostly recovering, retail, construction remaining depressed, and the service sector expanding (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 effects of the 1991 introduction of a right to board-level worker voice 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 threshold. We illustrate results in nonparametric plots of average outcomes by firm group from 1988 through the 1991 reform and beyond. Additionally, our regression models for outcomes y_{it} of firm i (equally weighted) in year t are:

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

The coefficients of interests are ψ_k^{Treated} for $k \geq 1991$, which capture the effect of the right to worker voice in the post-reform period compared to the pre-reform year, 1990, for which we normalize the coefficient to zero ($\psi_{1990}^{\text{Treated}} = 0$). Pre-period effects capture potential pre-trends. Year effects, ψ_k, net out common trends or year-specific shocks. We also report average post-reform and pre-reform (rather than year-specific) treatment effects, namely the coefficient on $\mathbb{1}[\text{Emp}_{1988} \geq 150] \times \mathbb{1}[t \geq 1991]$ estimated with respect to base year 1990, and an analogous pre-reform effect covering 1988 and 1989. Standard errors are clustered at the firm level.

Potential Confounders and Control Variables A bias would arise if firms in different size categories were on different trajectories absent the reform, e.g., if the recession affected large and small firms differently. We implement several strategies to control for such confounders. First, as described below, our baseline specification draws on a local firm sample around 150 employees. Second, we also report even more local specifications using narrower bandwidths around the 150 employee threshold. Third, we control for the primary amplifier of the Finnish recession identified in Gorodnichenko, Mendoza, and Tesar (2012) by including industry-year effects (NACE Level 1, i.e., letters), as the recession was inherited from Russia and mediated by industry-specific trade exposure. Fourth, in our most granular and overall preferred specification, we add firm fixed effects to gauge potential differential attrition. Finally, we also assess whether firms selectively bunch above or below the policy threshold.

Data We use several firm and worker data sets from Statistics Finland. We winsorize all continuous outcome variables at the 1% level, and CPI-adjust nominal variables to 2010 EUR (inconsequential for our estimates due to year effects). First, our matched employer-employee data draws on the individual-level FOLK Employment Relationship Data. It reports the length, in days, of the employee-employer relationship by calendar year. We merge the dataset to the FOLK Basic panel reports demographics (gender, age, education) and annual labor and capital income from 1988 to 2017. Second, for firm financials, we draw on the Financial Statement Data Panel, which contains accounting data 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. Third, we merge on the Quality of Work Life Survey, an employee 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. 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).

Employment Measure The employment concept relevant for the representation threshold is the number of employees excluding temporary and seasonal workers. We construct employment on the 31st of December each year, dropping workers with fewer than 91 days of contracted work in the year or with zero earnings.

Summary Statistics We report control means in the base year 1990 and 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. 50.2% of employment is in firms above the cutoff.

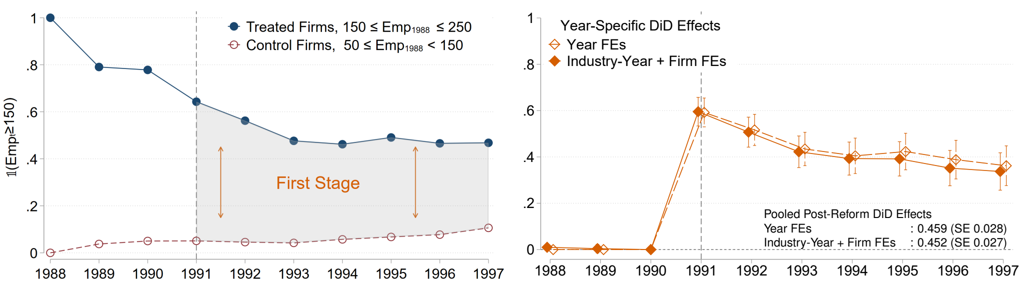
Sample In our main specification, we draw on all firms with 1988 employment in a 100-employee bandwidth around the threshold, i.e., between 50 and 149 employees (control group) and 150 and 250 (treatment group). In robustness checks, we also use smaller bandwidths as well as "donut hole" specifications that drop firms very close to 150 to assess the role of measurement error or limited persistence around the cutoff. We do not restrict the sample to be balanced; instead, we compare results with and without firm effects, which mitigate the impact of attrition, and study survival as an outcome.

Intent-To-Treat, Persistence, and First Stage Our DiD design is intent-to-treat as we use pre-treatment employment in 1988 to assign firms into groups (as concurrent employment can be affected by the treatment). In Figure 4, we gauge the persistence of this assignment.[5] 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 employment. Some convergence occurs, such that by 1991, 5% and 64% of firms in the control and treatment groups are above the threshold and hence will be subject to the worker right to board-level

[5] This analysis draws upon the matched employer-employee data matched with the firm-level accounting data. We confirm robustness to using the former data only in Appendix Figure A.3.

Figure 4: Persistence of Treatment Assignment

(a) Fraction of Firms with Employment ≥ 150 (b) DiD: Fraction of Firms with Worker Voice Right



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 representation (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.

representation. The post-reform gap between the two time series captures the treatment differential.

We also offer an instrumental variables (IV) interpretation with a first stage for scaling the reduced-form effects we later report. Formally, the treatment is a worker right to representation, i.e., $D_{it} = \mathbb{1}[\text{Emp}_{it} \geq 150] \cdot \mathbb{1}[t \geq 1991]$, which is a function of concurrent employment Emp_{it} (precisely, we will use end-of-year employment of the previous year, consistent with the practice we describe above). Figure 4 Panel (b) reports the year-specific coefficients from the difference-in-differences specification with this treatment indicator D_{it} as the outcome—normalizing the difference to zero in 1990. Here, we find coefficients of about 0.6 in 1991 and coefficients stabilizing at about 0.4 starting in 1993. For the first stage that averages over the post-reform period through 1997, we find a coefficient of 0.459 (SE 0.028). Hence, an IV interpretation of our 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 conclusions. This IV interpretation captures the effect of the right to representation: even in firms that do not formally take-up the institution, communication may increase in response to the right and to potential negotiations that do not result in board representation but other forms of worker voice. Since the first-stage effect in this framework captures not just the persistence of the sorting but also potential causal effects on employment, we focus on reduced-form rather than IV effects below.

4 Effects on Job Quality

We estimate the effects of the reform on separations, a revealed-preference ranking of firms from the perspective of workers, and several other measures of job quality, including wages.

4.1 Separations

A key prediction of Hirschman (1970) is that improving stakeholders' voice will reduce their exit, by enabling them to change their institution from within; here, voice may enable workers to affect workplace design (wages and amenities). Separations also serve as a catch-all revealed-preference measure of the relative attractiveness of an employer (Krueger and Summers, 1988). Finally, reducing *involuntary* layoffs is an outcome 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 (but not subsequent ones).

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. It encompasses direct job-to-job transitions, a proxy for voluntary quits, as well as separations into nonemployment, a proxy for involuntary layoffs. The baseline annual separation rate is 0.25 in the treatment group in 1990, the pre-reform year. In our plots, year t separations denote separations in calendar year t from the original employer on December 31st of year $t-1$.

We report results in Figure 5 Panel (a), the levels, and Panel (b), the year-specific DiD effects, and in Column (1) of Table 1, the DiD effect pooled over all post-reform years. Pre-trends are flat and parallel in the groups before the 1991 reform; the pooled pre-period estimate relative to 1990 is -0.007 (SE 0.013), so that we cannot reject the parallel trends assumption in any specification. In our most basic specification, the treatment effect is -0.018 (SE 0.014). Since the post-reform period includes a recession, we additionally include year-specific industry effects, since industry exposure was a large mediating factor (see Section 2). The estimates remain stable at -0.013 (SE 0.014), 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), the treatment effect is -0.029 (SE 0.013). Our preferred specification hence indicates reductions in overall separation rates by 2.9 percentage points (12% relative to the 1990 control mean), and lets us rule out reductions in the separation rate of more than 5.5 percentage points (22%).

As with all other outcome variables, we further confirm robustness to more local bandwidths or to excluding observations around the 150-employee threshold. We report these results in Panels (a) and (b) of Appendix Figure A.5. At smaller bandwidths, potential biases from size-specific shocks may be less relevant; however, sample size falls, so SEs increase (and the first stage falls).

Job-to-Job Transitions We separately study job-to-job transitions, as proxies for voluntary separations and hence for the relative attractiveness of the employer.[6] Out of the baseline any-separation rate of 0.225, job-to-job transitions are 0.097 (38.5%).

Figure 5 Panels (c) and (d) and Column (2) of Table 1 report an effect of -0.012 (SE 0.012) in our specification controlling for industry-year effects. From the baseline of

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

0.097, these effects imply 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.[7] We can rule out small positive or negative point effects outside of the 95% confidence interval spanning -0.027 and 0.013. Effect sizes are robust to other bandwidths or varying donut holes (see Appendix Figure A.5). 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 5 Panels (e) and (f) and in Column (3) of Table 1. This outcome captures layoff risk or job stability—mechanically corresponding to the residual between overall separations and job-to-job transitions. We find negative effects of -0.006 (SE 0.008) without controls and -0.022 (SE 0.007) when controlling for industry-year and firm fixed effects. Estimates are robust to varying the bandwidth and the size of the donut hole (see Appendix Figure A.5 Panels (e) and (f)). Our estimates thus indicate small reductions in separations into nonemployment.

Robustness to Alternative Separation Definitions As Appendix Table A.2 illustrates, our results are robust to removing high-turnover workers that worker representatives may not represent as insiders (those with at most one year of tenure and those outside of ages 20 to 55), and to removing spurious exits due to employer ID relabeling or mergers and acquisitions.

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 2 percentage points in annual separation rates into nonemployment, suggesting reduced layoffs and

[7] 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, through the lens of a monopsony framework in which separations are related to firms' relative wages. Bassier, Dube, and Naidu (forthcoming) estimate an elasticity of job-to-job separations to firm-level wage premia of 4 (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 corresponds to the 7% separations reduction.

increased job security or entrenchment of workers.

4.2 Revealed-Preference Job Quality Based on Worker Flows

We also study a revealed-preference measure of job quality designed by Sorkin (2018), which extends the PageRank algorithm to labor market flows. The measure recursively defines a firm quality index such that "good firms hire from other good firms and have few workers leave." We create the index separately in the pre-reform (1988-1990) and post-reform (1992-1997) periods, and detail our implementation in Appendix D.1. We report DiD effects on whether the treatment group firms increased their ranking (the firm value, normalized to have zero mean and unit standard deviation in the pre-period) relative to control group firms after the reform. Results are reported in Column (4) of Table 1. 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.6). 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 also study worker health as an outcome, thereby testing the long-standing hypothesis that worker representatives aim to improve workplace safety (Freeman and Medoff, 1985). Our administrative outcome variable is the fraction of workers with sickness leaves (of more than ten working days, e.g., receiving benefits after a long illness or an accident). Sickness leave benefits are coded in the same category as maternity leave benefits; to isolate the former, we zoom into workers older than 40, and male workers. The firm-level outcome is the fraction of the sample with such spells. Both 1990 treatment group base rates are around 0.08. We report results in Columns (5) and (6) of Table 1. 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 parallel pre-trends, 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 can rule out even small improvements of this measure of worker health and safety.

4.4 Survey-Based Subjective Job Quality and Labor Relations

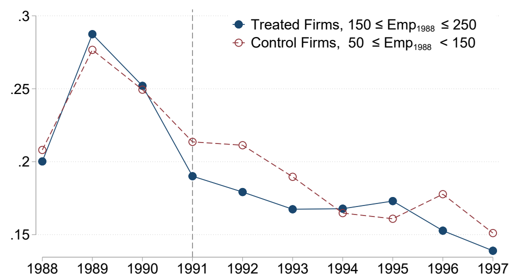
We close our analysis of job quality with subjective measures reported directly by workers, merging the Finnish Quality of Work Life Survey into our administrative data. We conduct difference-in-differences analyses drawing on the 1990 wave and the first post-reform wave in 1997. As the survey is not 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.

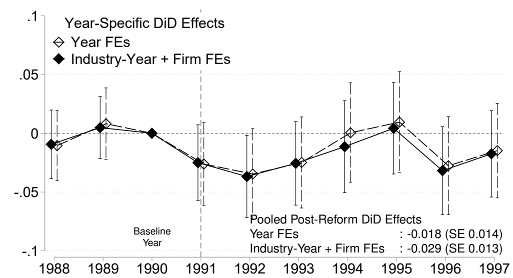
Subjective Job Quality For our analysis of subjective job quality, we draw on 21 survey items, such as whether problems at work make it hard to focus at work, and whether one's supervisor is supportive and encouraging (all variables listed in Appendix D.2). We report results for this job quality index in Column (4) of Table 1, finding a moderate increase 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.

Figure 5: Effects on Job Separations

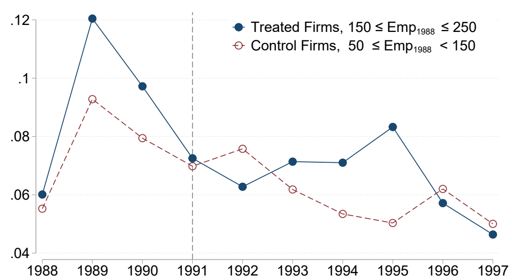
(a) Any Separation: Levels



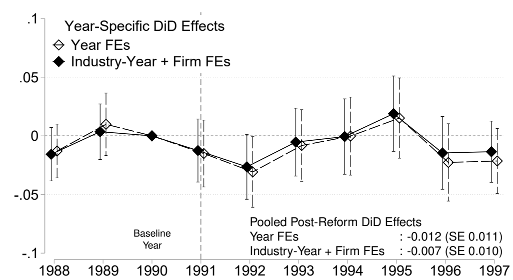
(b) Any Separation: DiD



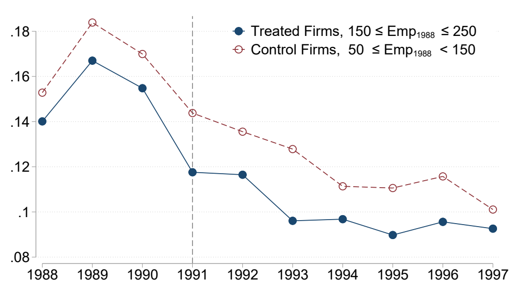
(c) Job-to-Job Separation: Levels



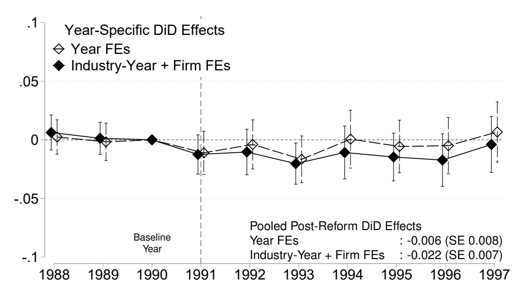
(d) Job-to-Job Separation: DiD



(e) Separation into Nonemployment: Levels



(f) Separation into Nonemployment: DiD



Note: The figure displays the effects of a right to worker voice 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 1. We report robustness analyses in Appendix Table A.2.

Table 1: 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)
DiD: Year FEs								
Treatment	-0.018	-0.012	-0.006	-0.043	-0.002	-0.001	0.182**	0.063
(1991-1997)	(0.014)	(0.011)	(0.008)	(0.105)	(0.003)	(0.003)	(0.084)	(0.083)
Pre-Period	-0.014	-0.008	-0.005		0.007*	0.005		
(1988-1989)	(0.015)	(0.012)	(0.007)		(0.004)	(0.004)		
DiD: Industry-Year FEs								
Treatment	-0.013	-0.010	-0.002	-0.049	-0.002	-0.002	0.146*	0.063
(1991-1997)	(0.014)	(0.011)	(0.008)	(0.104)	(0.003)	(0.003)	(0.088)	(0.089)
Pre-Period	-0.014	-0.010	-0.004		0.006	0.004		
(1988-1989)	(0.014)	(0.011)	(0.007)		(0.004)	(0.003)		
DiD: Year and Firm FEs								
Treatment	-0.027**	-0.006	-0.021***	-0.053	-0.002	-0.002		
(1991-1997)	(0.013)	(0.010)	(0.007)	(0.107)	(0.003)	(0.003)		
Pre-Period	-0.008	-0.009	0.000		0.008*	0.004		
(1988-1989)	(0.013)	(0.011)	(0.007)		(0.004)	(0.004)		
DiD: Industry-Year and Firm FEs								
Treatment	-0.029**	-0.007	-0.022***	-0.065	-0.002	-0.001		
(1991-1997)	(0.013)	(0.010)	(0.007)	(0.104)	(0.003)	(0.003)		
Pre-Period	-0.007	-0.009	0.002		0.007	0.003		
(1988-1989)	(0.013)	(0.011)	(0.007)		(0.004)	(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 5. We report robustness analyses in Appendix Table A.2. The post-period for the Job Quality and Labor Relations Quality outcomes draws on the 1997 wave; the previous wave is 1990.

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 1 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.

4.5 Wages

We now study whether the institution affects wages. Wages are an important attribute of job quality for which worker representatives advocate, compensating differentials may mean that wages decrease in response to improvements in amenities shifting, or the institution may affect wages by boosting worker bargaining power.

Mean Wages We start by studying the firm-level mean of raw worker-level log wages, and report results in Figure 6 Panels (a) and (b) and Column (1) of Table 2. 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 on firm fixed effects (wage policies) and worker fixed effects (capturing the permanent earnings potential of a worker) as well as cubic controls for potential experience interacted with education groups. We estimate AKM specifications in rolling three-year windows and use observations from t, $t+1$, and $t+2$ to calculate outcomes for period t.

We report results in Figure 6 Panels (c) and (d) and Column (2) of Table 2. 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 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, 2021). We study effects on deciles of the within-firm wage distribution and report pooled post-period DiD estimates in Figure 6 Panel (e), as well as in Appendix Table A.3. 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.

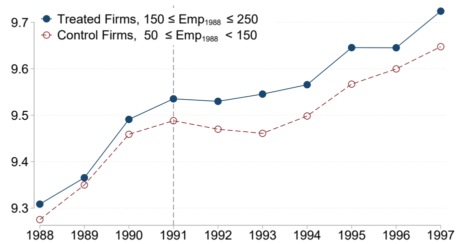
Executive Compensation Since Finnish boards set executive compensation, we also ask whether having workers on the board influences the firms' wage structure at the very top. Executive pay can reflect agency issues (Bertrand and Mullainathan, 2003), which worker representation may curb or exacerbate. 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, 2020), compensation in the Finnish data is not capped at a social security maximum. Appendix D.4 describes this variable construction in detail. The rightmost estimates in Figure 6 Panel (e) and Columns (10) and (11) of Appendix Table A.3 show 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 (not necessarily from the 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.

Rent Sharing We next study rent sharing elasticities, capturing potential shifts in wage setting indicative of higher worker bargaining power. We measure rent sharing using the cross-sectional relationship between firm-level composition-adjusted wage (AKM) policies and value 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, which is hypothesized to increase following boosts to worker authority (Grout, 1984; Jäger, Schoefer, and Heining, 2020). In Figure 6 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). 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, permitting us to rule out moderate boosts to wages from this channel.

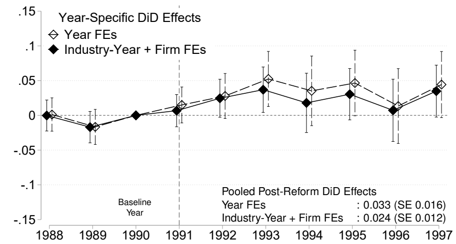
The Labor Share We also report effects on the firm-specific labor share (the wage bill divided by value added), in Table 2 Column (3). We find small negative and statistically insignificant effects with estimates of -0.010 (SE 0.014) in our basic specification and -0.022 (SE 0.014) in our preferred specification, allowing us to rule out even small increases in the labor share above 0.5 percentage points (compared to an average labor share in our treatment group of 0.576 in 1990).

Figure 6: Effects on Wages

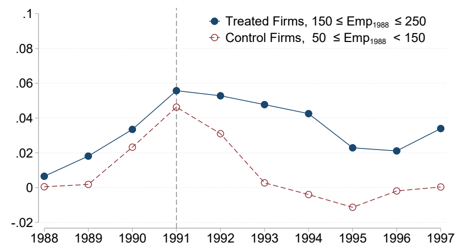
(a) Mean Log Wage: Levels



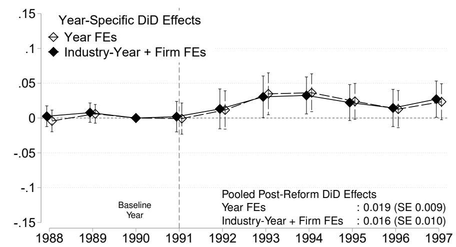
(b) Mean Log Wage: DiD



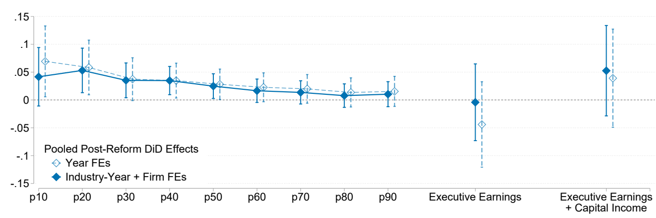
(c) AKM Pay Premium: Levels



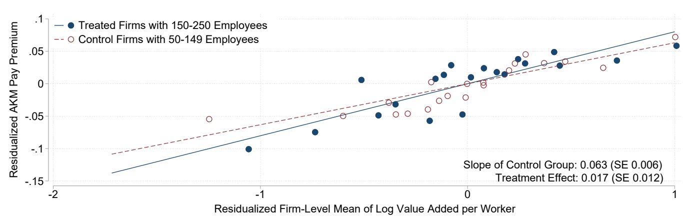
(d) AKM Pay Premium: DiD



(e) Within-Firm Wage Percentiles and Executive Compensation



(f) Rent Sharing



Note: The figure displays the effects of a right to worker voice 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 2). We report robustness analyses in Appendix Figure A.6. Panel (e) reports DiD effects on different within-firm percentiles of the wage distribution as well as (log) executive earnings (see also Table A.3). Panel (f) reports the relationship between firms' AKM pay premium and average log value added in the post-reform period (controlling for industry effects).

Table 2: Effects on Wages

	Mean Log Wage (1)	AKM Pay Premium (2)	Labor Share (3)
DiD: Year FEs			
Treatment	0.033**	0.019**	-0.010
(1991-1997)	(0.016)	(0.009)	(0.014)
Pre-Period	-0.006	-0.000	0.016
(1988-1989)	(0.013)	(0.008)	(0.011)
DiD: Industry-Year FEs			
Treatment	0.017	0.016*	-0.006
(1991-1997)	(0.015)	(0.009)	(0.014)
Pre-Period	-0.015	0.001	0.012
(1988-1989)	(0.013)	(0.008)	(0.011)
DiD: Year and Firm FEs			
Treatment	0.035***	0.019*	-0.018
(1991-1997)	(0.013)	(0.010)	(0.014)
Pre-Period	-0.003	-0.000	0.015
(1988-1989)	(0.010)	(0.007)	(0.011)
DiD: Industry-Year and Firm FEs			
Treatment	0.024**	0.016*	-0.022
(1991-1997)	(0.012)	(0.010)	(0.014)
Pre-Period	-0.007	0.001	0.012
(1988-1989)	(0.009)	(0.007)	(0.011)
1990 Average (Control):	9.459	0.023	0.600
1990 Average (Treated):	9.491	0.033	0.576
N, Firm-Years (Control):	8,684	7,089	5,056
N, Firm-Years (Treated):	1,839	1,489	1,256

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 visually in Figure 6. We report robustness analyses in Appendix Figure A.6.

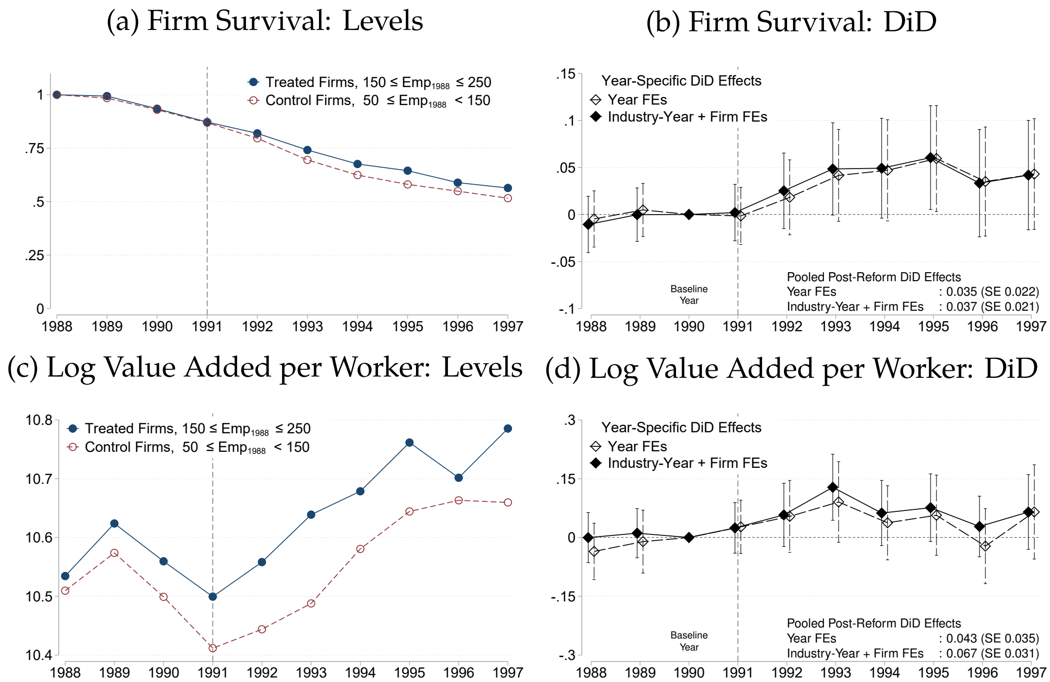
Summary We have found only small positive, if any, effects on wages of granting workers a right to worker voice. Our estimates leave room for small increases in pay premia and are consistent with a small amount of pay compression.The point estimates are within the range of estimates in the reform-based DiD design building on the repeal of board representation in some new firms in Germany (Jäger, Schoefer, and Heining, 2020). 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.

5 Effects on Firm Performance

We now turn to measures of firm performance. A large body of literature posits that worker voice may increase productivity, e.g., by lowering turnover (Hirschman, 1970; Freeman, 1980), or by facilitating information flows and cooperation and hence mitigating coordination and contracting problems (Freeman and Medoff, 1985; Freeman and Lazear, 1995).

Survival As a 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 (1) of Table 3 and in Panels (a) and (b) of Figure 7, 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 post-period effects starting 1992 are positive, at 0.035 (SE 0.022) without controls and 0.037 (SE 0.021) with 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.8.

Figure 7: Effects on Firm Performance



Note: The figure displays the effects of a right to worker voice on firm survival and labor productivity and capital. 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 and in Table 3. We display other firm performance outcomes in Appendix Figure A.7 and robustness analyses in Appendix Figure A.8.

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. This finding also implies that any composition effects through 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 (2) and Figure 7 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, 2020). Considering fixed assets as our capital proxy, we report effects on the capital-labor ratio in Column (3) of Table 3. Our basic specification gives a 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 Heining, 2020). 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.[8] 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, wages. The 95% confidence interval allows us to rule out negative effects below -0.059. The upper bound permits us to rule out positive capital effects above 0.129 (potentially accounting for some of the labor productivity effect).

Total Factor Productivity We next study log total factor productivity (TFP).[9] We

[8]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.

[9]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.

report these effects in Table 3 Column (4). 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 driving total factor productivity increases.

Profitability We have already documented in Section 4.5 that the labor share, if anything, marginally decreased. We now study the profit margin, net income (earnings after depreciation, interest, and taxation) divided by revenue. We find a precisely estimated effect of 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.[10]

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.10. 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 a right to worker voice imposed net costs, as in response to costly labor regulations or tax incentives studied in Garicano, Lelarge, and Van Reenen, 2016; Benzarti and Harju, forthcoming).

[10] We have also experimented with capital expenditure (investment) and dividends as outcome variables, which are available in Finnish data only starting in 1994, and hence cannot be studied in our DiD design. However, they could be studied in a regression discontinuity (RD) design. In Appendix C, we report an RD design for capital expenditure and dividends 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 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 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 potential firm selection around the cutoff. These concerns motivate our DiD design in the first place.

Table 3: Effects on Firm Performance

	Firm Survival (1)	Log Value Added per Worker (2)	Capital Intensity (3)	Total Factor Productivity (4)	Profit Margin (5)
DiD: Year FEs					
Treatment	0.035	0.043	0.099	-0.038	0.006
(1991-1997)	(0.022)	(0.035)	(0.078)	(0.060)	(0.008)
Pre-Period	0.000	-0.024	0.063	0.028	0.005
(1988-1989)	(0.015)	(0.033)	(0.073)	(0.060)	(0.005)
DiD: Industry-Year FEs					
Treatment	0.037*	0.039	0.055	0.018	0.005
(1991-1997)	(0.021)	(0.034)	(0.075)	(0.049)	(0.008)
Pre-Period	-0.005	-0.014	0.032	0.018	0.005
(1988-1989)	(0.015)	(0.032)	(0.072)	(0.048)	(0.006)
DiD: Year and Firm FEs					
Treatment	0.035	0.068**	0.037	0.057	-0.000
(1991-1997)	(0.022)	(0.031)	(0.049)	(0.036)	(0.008)
Pre-Period	0.000	-0.023	-0.039	0.018	0.004
(1988-1989)	(0.015)	(0.028)	(0.046)	(0.035)	(0.005)
DiD: Industry-Year and Firm FEs					
Treatment	0.037*	0.067**	0.035	0.063*	-0.001
(1991-1997)	(0.021)	(0.031)	(0.048)	(0.034)	(0.008)
Pre-Period	-0.005	-0.016	-0.044	0.027	0.002
(1988-1989)	(0.015)	(0.028)	(0.047)	(0.034)	(0.005)
1990 Average (Control):	0.930	10.499	10.059	6.115	-0.005
1990 Average (Treated):	0.935	10.560	10.220	5.959	-0.010
N, Firm-Years (Control):	12,648	4,979	5,037	4,979	5,049
N, Firm-Years (Treated):	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 firm performance results visually in Figure 7 and Appendix Figure A.7.

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 in 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.

6 Effects of Shop-Floor Voice Institution: 2008 Expansion of Shop-Floor Representation in Small Firms

One potential explanation for the limited effects of the 1991 introduction of board-level representation is that the institution may simply duplicate (or be weaker than) a pre-existing worker voice institution at the shop-floor level. We described this institution in Section 2, and now test this explanation by estimating shop-floor representation's effects on the same set of outcomes, leveraging a reform that made it mandatory in certain firms starting in 2008.

The Reform Figure 8 Panel (a) visualizes the reform. Before 2008, the Cooperation Act mandated shop-floor representation in firms with at least 30 employees. A 2007 reform lowered the threshold to 20 employees.[11] 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). To our knowledge, there exists only one empirical (DiD) study of the reform, 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

[11] 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 a large share of those firms (see Suomen Yrittäjät, 2010, Figure 2).

with 2005 employment of 10 to 19, and of 30 to 39.

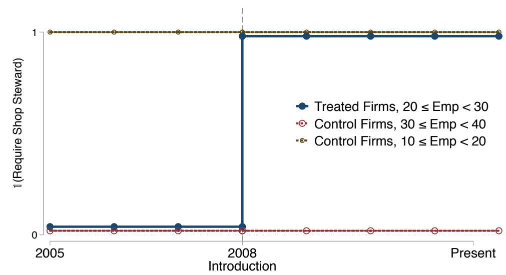
Results Figure 8 Panel (b) reports a first stage for the firm being subject to the shop-floor representation mandate. For each of the three groups, it plots the share with employment at least 30 (if before 2008) or at least 20 (starting 2008, post-reform). We see a considerably sharper relative increase in the treatment group compared to either control group, 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 (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.4 and A.5 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.

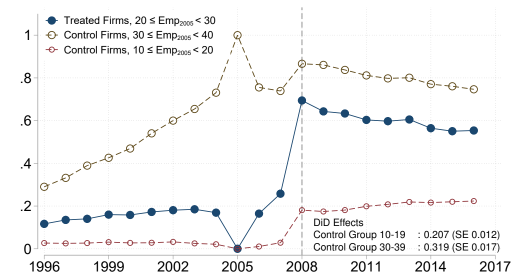
Implications The at best small effects of the 2008 expansion of shop-floor representation imply, first, that the baseline presence of shop-floor representation is unlikely to explain the limited effects of board-level representation following the 1991 reform. Second, as a substantive result in its own right in an area where causal estimates have been elusive, we find that shop-floor representation too has limited effects on worker and firm outcomes.

Figure 8: Effects of Alternative Worker Voice Institution: Shop-Floor Representation Reform

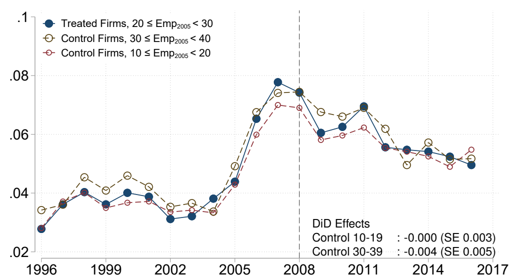
(a) Timeline



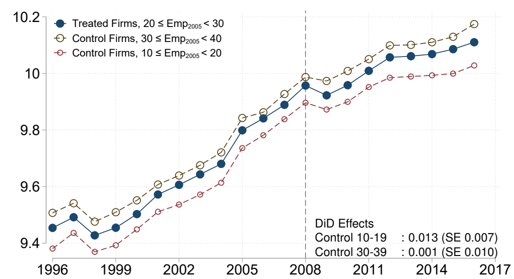
(b) First Stage: Fraction of Firms with Shop-Floor Representation Mandate By Law



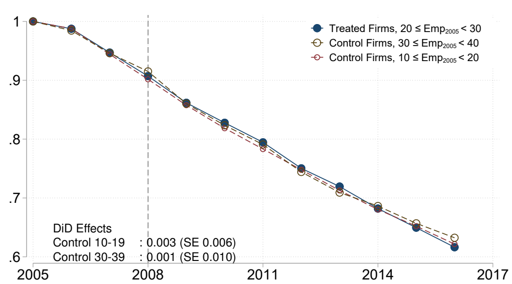
(c) Job-to-Job Separation



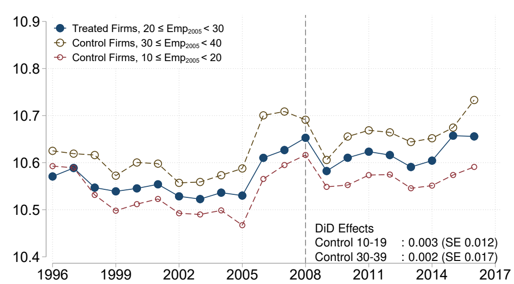
(d) Mean Log Wages



(e) Firm Survival



(f) Log Value Added per Worker



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.4 and A.5. We display other outcomes in Appendix Figure A.9.

7 Conclusion

Our quasi-experimental design studying the size-based introduction of a right to worker voice in Finnish firms has revealed that board-level minority representation of workers, and alternative negotiated implementations, led to small positive effects on firm performance. On the worker side, we have found some evidence for positive effects on job security and subjective job quality, even though most dimension of job quality were not improved. Most importantly, we have found no reductions in job-to-job separations, where the rubber meets the road for the exit-voice theory (Hirschman, 1970).

Do these limited effects suggest that voice does not matter? Not necessarily. Our findings need to be evaluated in light of high baseline levels of worker involvement in Finnish firms—both formal and informal, and perhaps irrespective of whether firms are subject to statutory worker voice rights.[12] While Finland ranked near the bottom of Europe in terms of worker representatives' authority in Figure 3 Panel (a), it is ranked the highest in Europe in terms of *workers'* (i.e., not their representatives') self-assessed ability to exercise voice in their workplaces, as Figure 3 Panel (b) illustrates using data from the 2015 European Working Conditions Survey. High baseline levels of informal voice may render statutory voice requirements redundant. This explanation could also help reconcile our findings with recent evidence for more positive effects of worker voice on worker satisfaction and productivity from India and China (Adhvaryu, Molina, and Nyshadham, 2021; Adhvaryu, Gade, Molina, and Nyshadham, 2021; Cai and Wang, 2021; Levy Paluck and Wu, 2021), where cultures of worker involvement and voice may be less less ingrained. Such an interpretation would also leave the door open to different effects in more adversarial industrial relations systems such as the United States (as, e.g., hypothesized by Sadun, 2018), where workers demand more voice at work (Kochan, Yang, Kimball, and Kelly, 2019; Dube, Naidu, and Reich, 2021).

[12]In the European Company Survey, we find that managers report a moderate gap in consultation and involvement of workers when comparing firms with and without formal worker representatives (80% vs. 64% for consultation, 68% vs. 55% for involvement). Importantly, we note that worker consultation and involvement is high even in the absence of worker representation (which is broadly defined in the survey).

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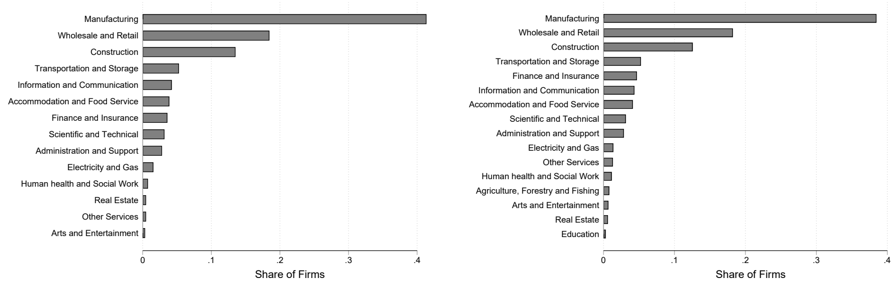
Online Appendix of:
Voice at Work

Jarkko Harju, Simon Jäger, and Benjamin Schoefer

A Appendix Figures

Figure A.1: Industry Composition

(a) Firm Sample in Financials Data | (b) Firm Sample in Matched Employer-Employee Data



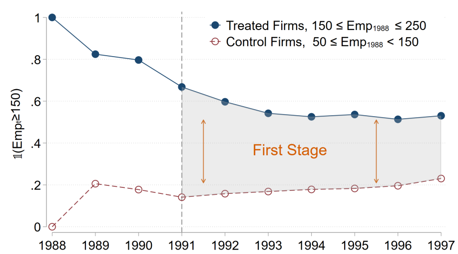
Note: The figure plots the industry composition of firms in our sample for the baseline year of 1990 (in which our control means of outcome variables are also specified, reported in the regression tables).

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

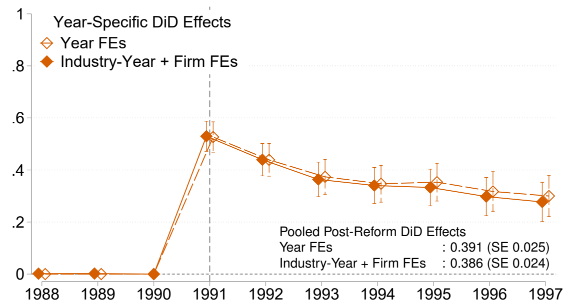
Note: The figure plots the standard deviation of individual log wages as well as of AKM firm effects over time. For the individual log wages, we both report the overall standard deviation as well as the standard deviation of individual wages of workers employed by firms in the largest connected sets (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 is estimation is based on the entire matched employer-employee data (rather than the firm size window for our main analysis).

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

(a) Fraction of Firms with Employment ≥ 150



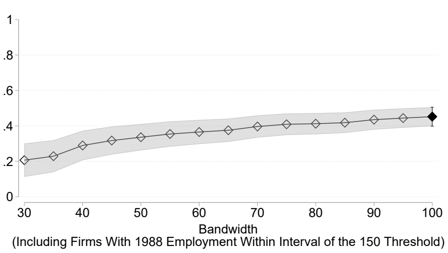
(b) DiD: Fraction of Firms with Worker Voice Rights



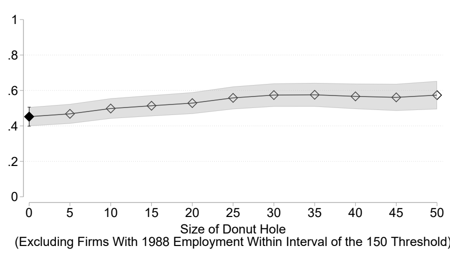
Note: The figure is a robustness check for Figure 4. Here, the sample is based on matched employer-employee data while Figure 4 restricts the sample to observations for which we also match firm data. For further details on the panels, see figure note to Figure 4.

Figure A.4: Fraction of Firms with Worker Voice Rights (Robustness Checks)

(a) Bandwidth Analysis



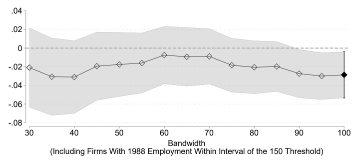
(b) Donuthole Analysis



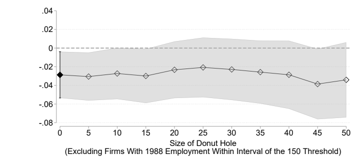
Note: The figure reports robustness checks for the DiD analysis in Figure 4. The outcome variable is an indicator for a firm being subject to worker voice rights governance (i.e. having at least 150 employees in the post-reform period). The figure plots DiD point estimates and 95% confidence intervals 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.5: Separations and Sickness (Robustness Checks)

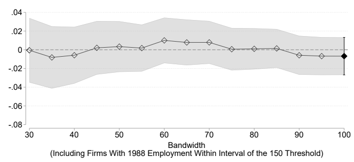
(a) Any Separation, Bandwidth



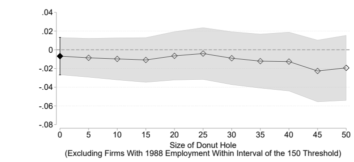
(b) Any Separation, Donuthole



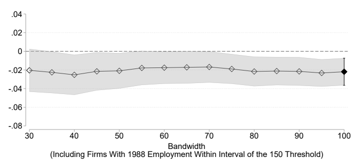
(c) Job-to-Job Separation, Bandwidth



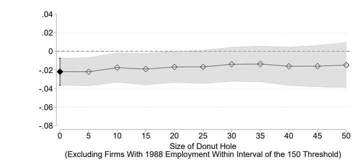
(d) Job-to-Job Separation, Donuthole



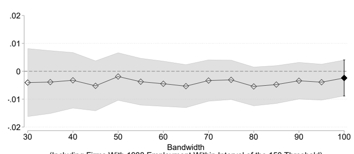
(e) Sep. into Nonemployment, Bandwidth



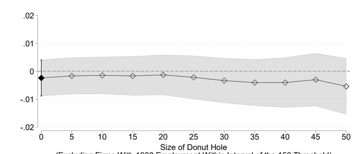
(f) Sep. into Nonemployment, Donuthole



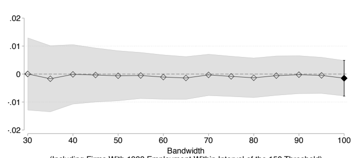
(g) Sickness, Older than 40, Bandwidth



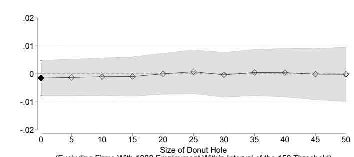
(h) Sickness, Older than 40, Donuthole



(i) Sickness, Male, Bandwidth



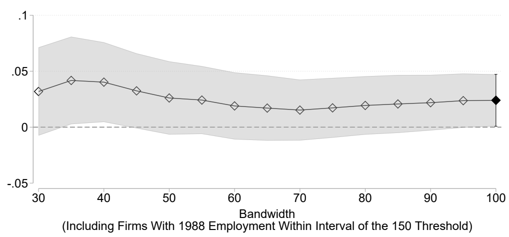
(j) Sickness, Male, Donuthole



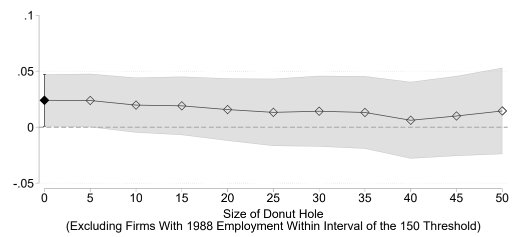
Note: The figure plots DiD point estimates and 95% confidence intervals, clustering standard errors at the firm level and including firm fixed effects and industry-year fixed effects. The left column varies the employment sample starting from a bandwidth of 10, 140-160, to a bandwidth of 100, our baseline 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 5 and Table 1.

Figure A.6: Wage Effects (Robustness Checks)

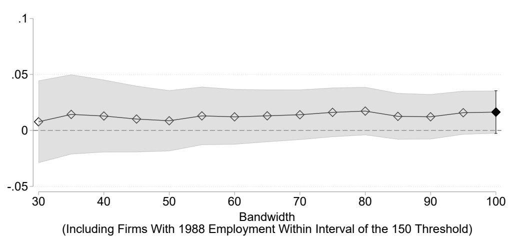
(a) Mean Log Wage, Bandwidth



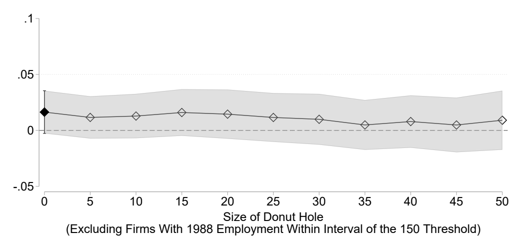
(b) Mean Log Wage, Donuthole



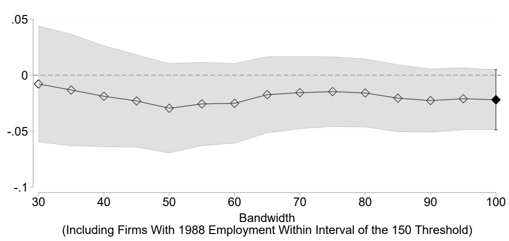
(c) AKM Pay Premium, Bandwidth



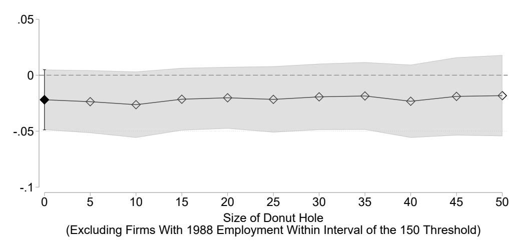
(d) AKM Pay Premium, Donuthole



(e) Labor Share, Bandwidth

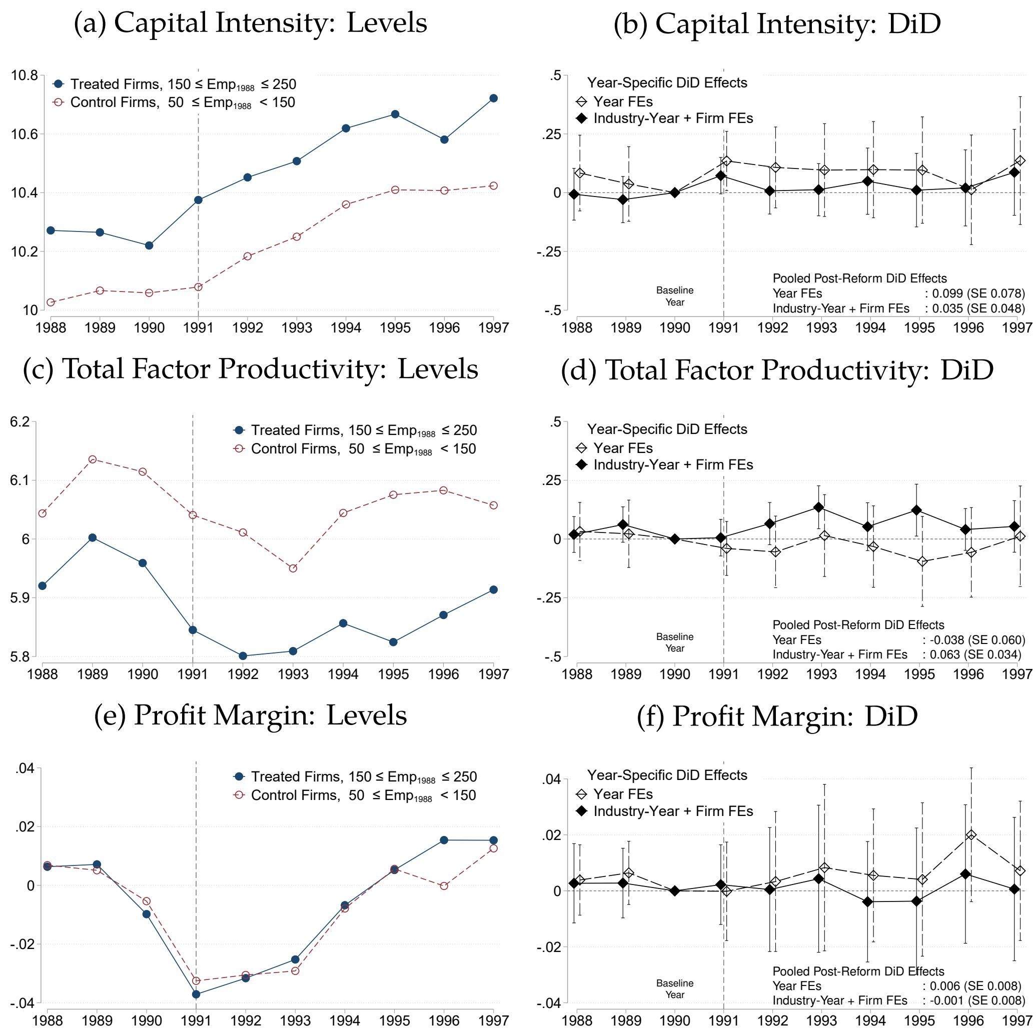


(f) Labor Share, Donuthole



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 6 and Table 2.

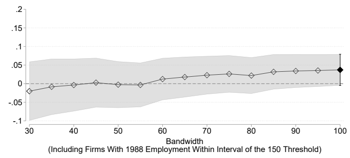
Figure A.7: Firm Performance (Additional Outcomes)



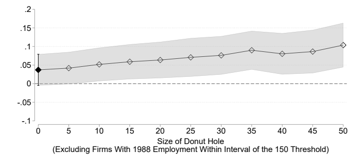
Note: The figure displays the effects of worker voice rights on capital intensity, total factor productivity and the profit margin. The left column displays the outcome in levels for treated firms (firms with employment between 150-250 in 1988) and for control firms (firms with employment between 50-149 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 7. We report the pooled post-reform effect in the bottom right corner of each plot and in Table 3.

Figure A.8: Firm Performance (Robustness Checks)

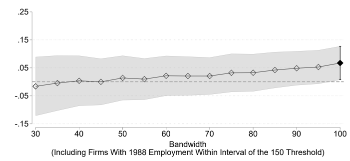
(a) Firm Survival, Bandwidth



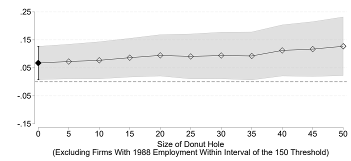
(b) Firm Survival, Donuthole



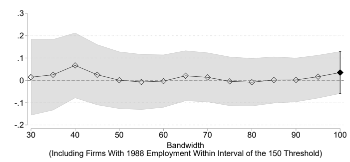
(c) Log Value Added per Worker, Bandwidth



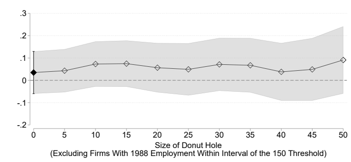
(d) Log Value Added per Worker, Donuthole



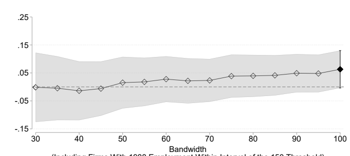
(e) Capital Intensity, Bandwidth



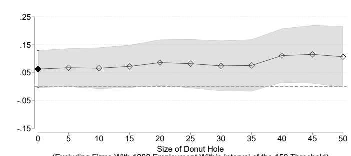
(f) Capital Intensity, Donuthole



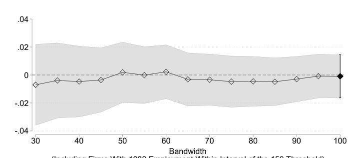
(g) Total Factor Productivity, Bandwidth



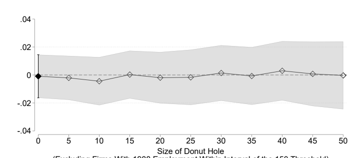
(h) Total Factor Productivity, Donuthole



(i) Profit Margin, Bandwidth



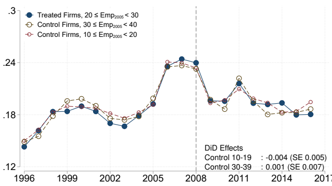
(j) Profit Margin, Donuthole



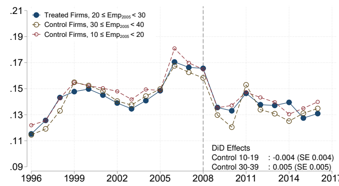
Note: The figure plots DiD point estimates and 95% confidence intervals clustering standard errors at the firm level and including firm fixed effects and industry-year fixed effects. The left column varies the employment sample starting from a bandwidth of 10, 140-160, to a bandwidth of 100, our baseline 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 7 and Table 3.

Figure A.9: Shop-Floor Representation Reform (Additional Outcomes)

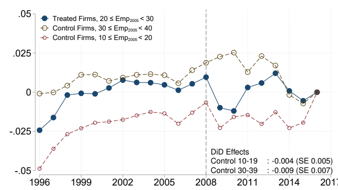
(a) Any Separation



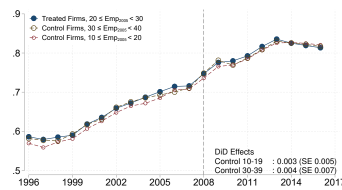
(b) Separation into Nonemployment



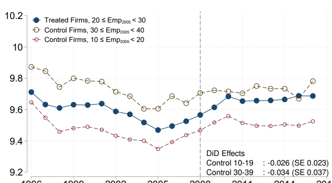
(c) AKM Pay Premium



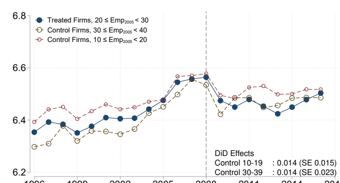
(d) Labor Share



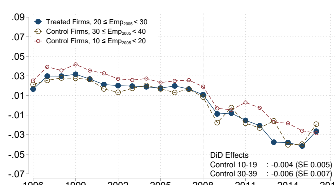
(e) Capital Intensity



(f) Total Factor Productivity



(g) Profit Margin



Note: The figure extends Figure 8 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 the each plot and in Tables A.4 and A.5.

B Appendix Tables

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

	2001	2017	2019	2020
	Panel (a): Do you have an administrative representative?			
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?			
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?			
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?			
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 ≥ 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 A.2: Effects on Separations and Sickness (Robustness Checks)

	Correcting Spurious ID Changes			With at Least One Year of Tenure			Employees Aged 20-55		
	Any Separation	Job-to-Job Separation	Separation into Nonemployment	Any Separation	Job-to-Job Separation	Separation into Nonemployment	Any Separation	Job-to-Job Separation	Separation into Nonemployment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
DiD: Year FEs									
Treatment	-0.018	-0.012	-0.006	-0.021	-0.015	-0.006	-0.021	-0.014	-0.007
(1991-1997)	(0.014)	(0.011)	(0.008)	(0.015)	(0.012)	(0.008)	(0.015)	(0.012)	(0.008)
Pre-Period	-0.000	-0.001	0.000	-0.003	-0.003	-0.000	0.000	-0.001	0.001
(1988-1989)	(0.014)	(0.011)	(0.007)	(0.014)	(0.012)	(0.007)	(0.014)	(0.012)	(0.007)
DiD: Industry-Year FEs									
Treatment	-0.012	-0.010	-0.002	-0.014	-0.013	-0.001	-0.014	-0.012	-0.002
(1991-1997)	(0.014)	(0.011)	(0.008)	(0.015)	(0.012)	(0.008)	(0.015)	(0.012)	(0.008)
Pre-Period	0.000	-0.001	0.001	-0.004	-0.003	-0.000	0.002	-0.001	0.003
(1988-1989)	(0.013)	(0.011)	(0.007)	(0.014)	(0.012)	(0.007)	(0.014)	(0.012)	(0.007)
DiD: Year and Firm FEs									
Treatment	-0.026**	-0.005	-0.021***	-0.026*	-0.008	-0.019**	-0.026**	-0.003	-0.023***
(1991-1997)	(0.013)	(0.010)	(0.007)	(0.014)	(0.011)	(0.008)	(0.013)	(0.011)	(0.007)
Pre-Period	-0.005	-0.008	0.003	-0.010	-0.010	0.000	-0.004	-0.007	0.003
(1988-1989)	(0.012)	(0.010)	(0.006)	(0.014)	(0.012)	(0.006)	(0.013)	(0.011)	(0.007)
DiD: Industry-Year and Firm FEs									
Treatment	-0.027**	-0.006	-0.022***	-0.024*	-0.006	-0.018**	-0.027**	-0.003	-0.024***
(1991-1997)	(0.013)	(0.010)	(0.007)	(0.013)	(0.011)	(0.008)	(0.013)	(0.011)	(0.008)
Pre-Period	-0.002	-0.006	0.004	-0.009	-0.009	-0.001	-0.001	-0.006	0.005
(1988-1989)	(0.012)	(0.010)	(0.006)	(0.013)	(0.012)	(0.007)	(0.013)	(0.011)	(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 5 and Table 1.

Table A.3: Effects on Within-Firm Wage Structure

	Log Wage in Within-Firm Wage Percentile									Executive	Executive Wage
	p10	p20	p30	p40	p50	p60	p70	p80	p90	(Log) Wage	& Capital Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
DiD: Year FEs											
Treatment	0.069**	0.058**	0.037*	0.035**	0.028**	0.023*	0.020	0.014	0.015	-0.044	0.039
(1991-1997)	(0.032)	(0.025)	(0.020)	(0.016)	(0.014)	(0.013)	(0.013)	(0.013)	(0.014)	(0.039)	(0.045)
Pre-Period	0.006	-0.003	-0.007	-0.004	-0.005	-0.006	-0.010	-0.015	-0.012	0.001	-0.020
(1988-1989)	(0.030)	(0.022)	(0.017)	(0.014)	(0.012)	(0.011)	(0.011)	(0.010)	(0.011)	(0.037)	(0.044)
DiD: Industry-Year FEs											
Treatment	0.045	0.034	0.017	0.016	0.012	0.010	0.010	0.005	0.009	-0.046	0.037
(1991-1997)	(0.031)	(0.024)	(0.019)	(0.015)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)	(0.039)	(0.045)
Pre-Period	-0.011	-0.015	-0.019	-0.016	-0.015	-0.014	-0.018*	-0.020**	-0.014	0.012	-0.015
(1988-1989)	(0.029)	(0.022)	(0.016)	(0.013)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)	(0.037)	(0.045)
DiD: Year and Firm FEs											
Treatment	0.055**	0.063***	0.045***	0.045***	0.035***	0.027**	0.024**	0.018	0.021*	0.008	0.067
(1991-1997)	(0.028)	(0.021)	(0.017)	(0.014)	(0.012)	(0.011)	(0.011)	(0.011)	(0.012)	(0.035)	(0.041)
Pre-Period	0.002	-0.005	-0.011	-0.005	-0.003	-0.000	-0.003	-0.006	-0.003	0.024	0.019
(1988-1989)	(0.026)	(0.018)	(0.013)	(0.010)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.025)	(0.031)
DiD: Industry-Year and Firm FEs											
Treatment	0.042	0.053***	0.035**	0.035***	0.025**	0.017	0.014	0.008	0.010	-0.004	0.053
(1991-1997)	(0.027)	(0.020)	(0.016)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.035)	(0.041)
Pre-Period	-0.003	-0.006	-0.014	-0.009	-0.007	-0.004	-0.007	-0.011*	-0.007	0.021	0.013
(1988-1989)	(0.026)	(0.018)	(0.012)	(0.010)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)	(0.026)	(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 2 for more information.

Table A.4: 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)
DiD: Year FEs							
Smaller Control Firms	-0.003	-0.005	0.002	-0.004	-0.000	0.229	0.344**
($10 \leq Emp_{2005} < 20$)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.167)	(0.153)
Larger Control Firms	-0.004	-0.006	0.001	-0.003	0.003	0.267	0.009
($30 \leq Emp_{2005} < 40$)	(0.008)	(0.005)	(0.005)	(0.004)	(0.004)	(0.213)	(0.199)
DiD: Industry-Year FEs							
Smaller Control Firms	-0.003	-0.004	0.001	-0.004	-0.000	0.211	0.286*
($10 \leq Emp_{2005} < 20$)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.165)	(0.154)
Larger Control Firms	-0.006	-0.006	0.000	-0.003	0.003	0.349	-0.027
($30 \leq Emp_{2005} < 40$)	(0.008)	(0.005)	(0.005)	(0.004)	(0.004)	(0.224)	(0.200)
DiD: Year and Firm FEs							
Smaller Control Firms	-0.004	-0.000	-0.004	-0.005*	0.001		
($10 \leq Emp_{2005} < 20$)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)		
Larger Control Firms	0.002	-0.004	0.006	-0.002	0.005		
($30 \leq Emp_{2005} < 40$)	(0.007)	(0.005)	(0.005)	(0.004)	(0.004)		
DiD: Industry-Year and Firm FEs							
Smaller Control Firms	-0.004	-0.000	-0.004	-0.005*	0.001		
($10 \leq Emp_{2005} < 20$)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)		
Larger Control Firms	0.001	-0.004	0.005	-0.002	0.005		
($30 \leq Emp_{2005} < 40$)	(0.007)	(0.005)	(0.005)	(0.004)	(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 8 and the remaining ones in Figure A.9. 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.5: 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)
DiD: Year FEs								
Smaller Control Firms	0.011	-0.002	0.002	0.004	0.012	0.040	-0.030	-0.003
($10 \leq \text{Emp}_{2005} < 20$)	(0.008)	(0.005)	(0.005)	(0.006)	(0.014)	(0.032)	(0.022)	(0.005)
Larger Control Firms	-0.004	-0.010	-0.002	0.002	0.040*	0.030	0.002	-0.002
($30 \leq \text{Emp}_{2005} < 40$)	(0.011)	(0.007)	(0.008)	(0.010)	(0.021)	(0.051)	(0.035)	(0.007)
DiD: Industry-Year FEs								
Smaller Control Firms	0.012	-0.001	0.002	0.003	0.010	0.026	-0.012	-0.002
($10 \leq \text{Emp}_{2005} < 20$)	(0.008)	(0.005)	(0.005)	(0.006)	(0.014)	(0.030)	(0.019)	(0.005)
Larger Control Firms	-0.005	-0.009	-0.003	0.001	0.027	0.010	0.003	-0.003
($30 \leq \text{Emp}_{2005} < 40$)	(0.010)	(0.007)	(0.008)	(0.010)	(0.021)	(0.049)	(0.031)	(0.007)
DiD: Year and Firm FEs								
Smaller Control Firms	0.012*	-0.003	0.004	0.004	-0.002	-0.025	0.009	-0.005
($10 \leq \text{Emp}_{2005} < 20$)	(0.007)	(0.005)	(0.005)	(0.006)	(0.012)	(0.023)	(0.015)	(0.005)
Larger Control Firms	0.001	-0.010	0.005	0.002	0.008	-0.033	0.018	-0.005
($30 \leq \text{Emp}_{2005} < 40$)	(0.010)	(0.006)	(0.007)	(0.010)	(0.017)	(0.037)	(0.023)	(0.007)
DiD: Industry-Year and Firm FEs								
Smaller Control Firms	0.013*	-0.004	0.003	0.003	0.003	-0.026	0.014	-0.004
($10 \leq \text{Emp}_{2005} < 20$)	(0.007)	(0.005)	(0.005)	(0.006)	(0.012)	(0.023)	(0.015)	(0.005)
Larger Control Firms	0.001	-0.009	0.004	0.001	0.002	-0.034	0.014	-0.006
($30 \leq \text{Emp}_{2005} < 40$)	(0.010)	(0.007)	(0.007)	(0.010)	(0.017)	(0.037)	(0.023)	(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 8 and the remaining ones in Figure A.9.

C Regression Discontinuity Design

As a complement to the difference-in-differences design studying the 1991 reform, 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. While we do not provide a detailed interpretation due to the *a priori* caveats noted in Footnote 10, the estimates broadly support the limited and small effects documented in the DiD design (and we can here additionally measure capital investment and dividends).

RD Specification Our regression model for the RD design is:

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

where y_{it} denotes the outcome of firm i in year t. The running variable N_{it-1} is the employment concept relevant to the codetermination law, counting all employees with more than 90 days of employment and positive earnings in a given year; we do not count short temporary job contracts. The RD design uses the same employment definition as the main analysis, namely a snapshot definition for December 31st of a given (previous) year, 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 β_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 5.

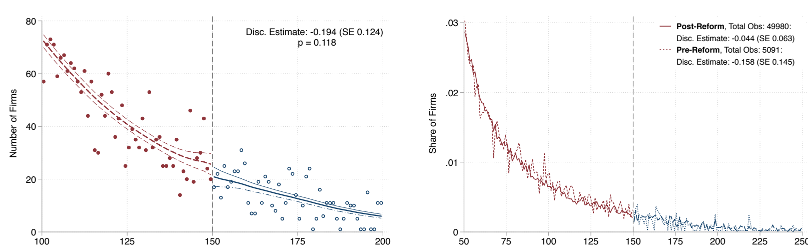
Bandwidth Choice and Inference Our main specification uses the bandwidth choice procedure in Calonico, Cattaneo, and Titiunik (2014), CCT in the following, with a triangular kernel. We cluster standard errors at the firm level.

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.10. 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 5, 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 the worker right to voice.

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 regressions 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.10: Density of Firm Size and McCrary Tests

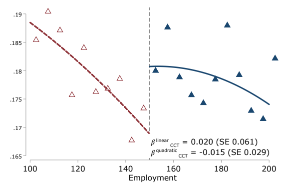
(a) McCrary: Pooled (b) McCrary: Pre and Post



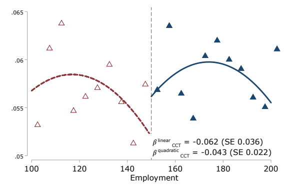
Note: The figure reports density plots and results of McCrary (2008) tests for continuity of the density at policy discontinuity of 150 employees. Panel (a) reports results for the pooled sample period from 1991 to 1997. Panel (b) reports the results separately for the pre-reform period, 1988-1990 (dashed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

Figure A.11: Regression Discontinuity Design

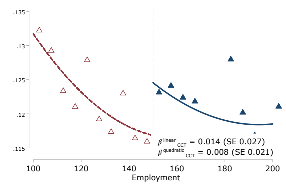
(a) Any Separation



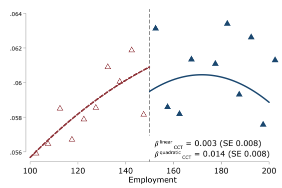
(b) Job-to-Job Separation



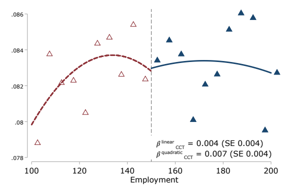
(c) Sep'n into Nonemp.



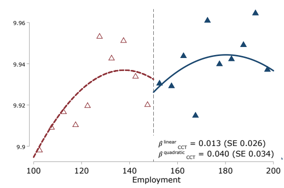
(d) Sickness Spell (Old)



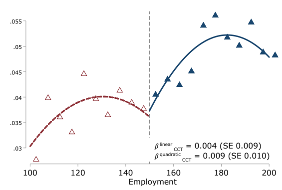
(e) Sickness Spell (Male)



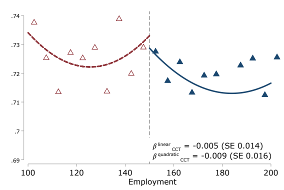
(f) Mean Log Wage



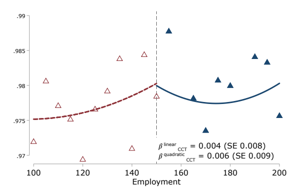
(g) AKM Pay Premium



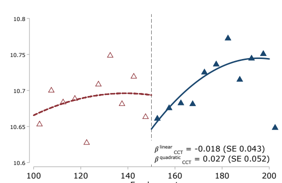
(h) Labor Share



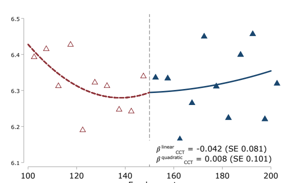
(i) Firm Survival



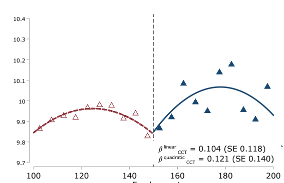
(j) Log Value Added Per Worker



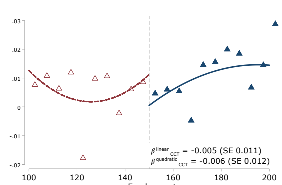
(k) Total Factor Productivity



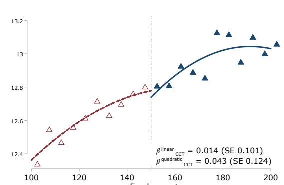
(l) Capital Intensity



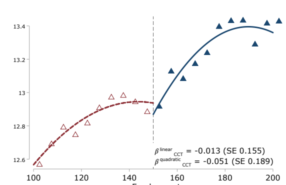
(m) Profit Margin



(n) Log Investments



(o) Log Dividends



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: 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, 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 λ_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

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

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 λ_f / N_f is constant across firms, Equation (A.2) yields the following relationship between firm common values:

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

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

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 1, 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 Columns (3) and (4) of Table A.6.

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 1. 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.6.

Table A.6: 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)
DiD: Year FEs				
Treatment	-0.043	-0.055	-0.013	0.004
(1991-1997)	(0.105)	(0.093)	(0.087)	(0.064)
DiD: Industry-Year FEs				
Treatment	-0.049	-0.072	0.021	0.019
(1991-1997)	(0.104)	(0.092)	(0.085)	(0.063)
DiD: Year and Firm FEs				
Treatment	-0.053	-0.056	-0.017	0.015
(1991-1997)	(0.107)	(0.100)	(0.084)	(0.067)
DiD: Industry-Year and Firm FEs				
Treatment	-0.065	-0.068	0.014	0.041
(1991-1997)	(0.104)	(0.098)	(0.082)	(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 1 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 voice right 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 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 Survey and Interviews of Employee Representatives

Our 2020 Survey The survey of employee representatives was developed and carried out in co-operation with the Industry Employees Association (Teollisuuden Palkansaajat or TP) and TP's co-operation group in September-October 2020. The Industry Employees TP is active in organizing training and education for firm-level employee representatives about their role and rights. Hence, the TP maintains a detailed list of the contact information of various employee representatives working in firms in different industries. As the TP does not have a separate list of worker representatives on boards or advisory councils, different types of employee representatives are mixed together in the list of contacts, including shop stewards as well as representatives of the European Works Councils (EWC). The contact list includes names, emails, and phone numbers of approximately 550 representatives.

We, in collaboration with TP, designed and developed a survey questionnaire and sent it out via email to these employee representatives. A total of 111 respondents participated in the survey, and thus the survey response rate was approximately 20%. Table A.7 below collects some descriptive statistics of the survey. Employee representatives who responded to the survey worked in very large companies, as the respondents estimated the firm to have an average of about 4,800 employees, but the dispersion in company size was very large, varying from 75 to 50,000 employees. Approximately 70% of the respondents were men and 80% were older than 45 years. A large share of respondents had worked for the firm the same firm for a long time—71% of the respondents for more than 15 years.

Furthermore, 30 of the respondents acted as worker representatives on boards or advisory councils in firms, the others were mainly shop stewards (43) and EWC representatives (22). The remaining respondents were either occupational health and safety representatives or represented staff in another role in the firm's institutions (16). The majority of all respondents, about 66%, worked in manufacturing firms. Two-thirds of the administrative representatives had served in their role more than three years and many of them also served as shop stewards (24) and EWC representatives (20). In addition, there was a large of variation also by union status: a larger share of the respondents were from the Trade Union Pro (27%), followed by the Confederation of Finnish Industry (18%) and the Association of Engineers (12%). 9% of the respondents were members of the Service Sector Trade Union and 7% were members of the Paper Association. The remaining respondents were evenly distributed among the various unions.

Existing Surveys In Appendix Table A.1, we also draw on results from existing surveys: one of 203 shop-floor representatives from the Finnish metalworks union, described in Sairo (2001), and two conducted in 2017 and 2019 by the Industry Employees TP Association, described in Teollisuuden Palkansaajat (2017, 2019). The

Table A.7: Survey Descriptives

	Mean	SD
Number of Employees	4833	7886
Male	0.68	0.49

Age in Years	No. obs
20-35	4
36-45	18
46-55	41
>55	48
Total	111

Years Working in the Firm	No. obs
0-5	2
6-10	12
11-15	18
>15	79
Total	111

latter surveys covered 288 and 164 firms with more than 150 employees, respectively, and surveyed worker representatives of various kinds. See the cited summary papers for further details.

Interviews We conducted five in depth interviews in June 2020 to learn about how the worker representation institution operates and is organized in different firms. We used the above-described list of employee representatives to contact them (via email) and asked if they would be willing to be interviewed on their role as a worker representative. We received 11 volunteers from whom we selected five, to represent different types of firms by size and industries. For these five representatives, we conducted in-depth interviews lasting approximately 30–60 minutes, in which we followed the structure of questions described below.

A broad interview structure:

- Background questions:
  - What is your history in the company? In which governing body do you work and how often do you meet?
  - How were you selected as an administrative representative?

- Do you feel that you can influence the company's operations?
- How do others in the management group see your role?
- What challenges do you face in influencing and participating in decision-making?
- Where have you succeeded in, for example, investments in employee training or well-being at work?
- Do you feel that you have been able to influence the company's layoffs or redundancies?
- Do you feel that this joint role as both as an employee representative and as an administrative representative strengthens your role in the company?
- Do you feel that the management group sees such a joint role negatively?
- Do you feel that you can influence investment decisions?
- What about decisions regarding levels of pay, do you feel you can influence these discussions?
- What is a typical management group meeting like?
- Do you feel that as an administrative representative you have succeeded in improving employee conditions?
- When it comes to influencing changes, do you think the formal management group is more important than informal relationships?
- If your company is, for example, considering outsourcing a service to another country, do you see that you could influence such a decision or participate in the discussion?
- If your company was considering layoffs, do you feel that you could influence such a decision or participate in the discussion?
- What else would you like to say about your role or what important points have we missed that could help us better understand the role of employee representation in company decision making?

D.4 Executive Compensation

We use individual-level occupation data (3-digit) to identify 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 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 the maximum total income measure (earned and capital income).

There are two caveats regarding the data. First, the occupation information is only available for a subset of years (1990, 1993, 1995, 2000 and 2004 onward). We address this issue by filling the data forward and backward 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.

Online Appendix References

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