Worker Voice and Shared Governance: Evidence from Finland*

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Abstract

We estimate the effects of a 1991 reform in Finland that introduced worker representation on board seats or in management bodies of firms with at least 150 workers. In regression-discontinuity specifications, we find that worker representation *increases* investment and the capital stock by 10-22%. We find no analogous effects at the policy discontinuity in the pre-reform period. Worker representation raises overall wages by about 5%, driven by a shift in worker composition rather than an increase in pay premia. We find no effects on efficiency (TFP), profitability, or dividend payouts. Overall, our evidence stands in sharp contrast to the disinvestment predictions of hold-up theories.

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1 Introduction

We study the firm-level effects of shared governance—granting workers formal decision rights in their firm, specifically granting workers the right to appoint representatives to company boards or management bodies alongside owners. We focus on the influential hold-up view, which provides a powerful caveat to any institutions that raise labor's bargaining power: such institutions might lead to higher wages at the expense of capital income, thereby depressing capitalists' willingness to invest, ultimately leading to a decline in the capital stock in the firm and the economy (Grout, 1984; Jensen and Meckling, 1979; Jäger, Schoefer, and Heining, 2019).[1] By this view, shared governance essentially acts as a distortionary tax on capital income.

Our empirical design compares variation across firms with and without shared governance in Finland arising from a policy discontinuity in firm size introduced by a 1991 reform. In firms with 150 or more workers, employees have a right to nominate worker representatives to participate in firm-level decision making. The rules affect all for-profit firms. No such right exists for workers in firms below that threshold. In firms meeting the policy threshold, the exact form of worker participation is in most cases negotiated through an agreement between workers and the firm. Absent an agreement, workers have a statutory right to nominate 20% of the representatives in either the board of directors, the board of supervisors, or the management body – with the specific body of representation, in turn, selected by the firm. Our reduced-form specifications do not condition on specific arrangements, and encompass three effects: the default statutory 20% representation (in turn of either of the three types), negotiated arrangements, and—even in firms without take-up—the threat arising from the unexercised right to representation.

We draw on firm and worker administrative data sets from tax and other sources to compare firms above and below the 150-employee threshold while the policy was active. Specifically, our main design is a regression discontinuity (RD) design. We also consider placebo specifications, counterfactually assuming that the policy discontinuity at 150 existed already in pre-periods, as well as at a placebo threshold of 100 employees. We validate our RD design by ruling out that firms strategically distort employment below the cutoff in a McCrary (2008) test, and assess the continuity of industry composition at the threshold. Importantly, we find no bunching of firms below the 150 employee threshold, indicating that firms do not appear to evade falling under the shared governance law – a property necessary

[1] For example, Jensen and Meckling (1979) describe the hold-up channel of shared governance as follows: "[T]he workers will begin 'eating it [the firm] up' by transforming the assets of the firm into consumption or personal assets. [...] It will become difficult for the firm to obtain capital in the private capital markets. [...] The result of this process will be a significant reduction in the country's capital stock, increased unemployment, reduced labor income, and an overall reduction in output and welfare."

for the validity of our identification strategy, as well as a result interesting in its own right.

Our first main result is that we find *positive* effects of shared governance on capital formation. Investment expenditures increase by about 25% and total assets by around 16%. These effects are driven by increases in fixed assets, which encompass investment goods such as buildings, machines, and equipment, rather than current (or financial) assets. Hence, we find that the disinvestment prediction of the hold-up view is not borne out in the Finnish context for this institution of shared governance.

Our empirical findings against disinvestment effects is consistent with evidence for positive capital effects of worker representation on company boards in Germany (Jäger, Schoefer, and Heining, 2019) and with observational evidence from comparing establishments with and without works councils (Addison et al., 2007).[2] Similarly, our results are also in line with Redeker (2019) who compares corporate savings of listed firms in Germany with one-third vs. parity board representation. At a broader level, our results are also consistent with the absence of hold-up patterns in firm-level dynamics in Italy (Card, Devicienti, and Maida, 2014).

In contrast to the hold-up view, richer views of industrial relations or theories of the second best might account for our findings. For instance, a richer model of shared governance with workers participating not only in wage setting but also in input decisions, such as over capital (Manning, 1987; Jäger, Schoefer, and Heining, 2019), delivers predictions consistent with our main findings. In fact, the Finnish institution comes with a broad range of company outcomes over which worker representatives may have direct influence, including investment decisions directly, but also by controlling the executives. Models of efficient, joint bargaining can predict investment to increase if shared governance induces such a regime shift (Crawford, 1988). Alternatively, shared governance may ameliorate other frictions such as coordination issues and imperfect information sharing (Freeman and Medoff, 1985; Freeman and Lazear, 1995), support implicit contracts or long-term interactions (Malcomson, 1983; Hogan, 2001). Our evidence does not distinguish these views. However, we cannot detect effects on total factor productivity (finding positive but not precisely estimated output effects), hence ruling out large effects on worker involvement to make firms more efficient.

[2] Jäger, Schoefer, and Heining (2019) is most closely related to our study, in that it aims to estimate the causal effects of shared governance on firm outcomes, with a focus on capital formation and the hold-up prediction. That paper also finds positive investment effects, studying a cohort-based reform in Germany that abolished shared governance in small firms but locked older cohorts into their pre-existing regime. The sample are stock corporations, and the institution is one-third supervisory board seat quotas for worker-elected representatives. By contract, the present paper studies a reform that imposes the institution onto incumbent firms, and the Finnish institution provides for more flexible implementation (permitting management involvement or other local arrangements besides board representation), and occurs in the context of a largely unitary board structure. The worker representatives are statutorily set to be 20% in the Finnish context, compared to the variation of 33% in the German reform.

Our second focus is on the consequences of shared governance for capital income and labor income. These outcomes are the transmission mechanisms in the theoretical views of hold-up: workers' are believed to bargain for a larger share of the firms' value added, leading to higher wages. Anticipating the lower capital share, owners will cut back on investment. Moreover, the theoretical debate as to the voluntary adoption of shared governance centers around the idea that it would hurt profits (Jensen and Meckling, 1979; Freeman and Lazear, 1995), an outcome we directly study.

On the labor income side, we find that shared governance raises firms' wage policies by moderate amounts. First, we document a 9% increase of total labor costs and a 5% increase in log wages. further identify firms' wage policies by implementing Abowd, Kramarz, and Margolis (1999) specifications, hence netting out workforce composition. Here, we also find that shared governance firms appear to attract workers with high earnings potential, such that raw wage effects are larger (yet this selection would not reflect actual pay premia). This wage effect does not arise from larger rent sharing, as we do not detect higher firm-level elasticity of worker wages to productivity shifts in shared governance firms. Perhaps this finding helps explain the absence of disinvestment effects, as hold-up requires workers' *share* of value added to increase, whereas unconditional wage increases would not distort marginal investment incentives. We further document evidence consistent with wage compression and our point estimates suggest a 3.5% reduction in the ratio of wages of workers at the 75th percentile to the ones of those at the 25th percentile. In addition, our point estimates suggest a negative effect in the share of payroll going to executives.

On the capital side, we find no change in the capital share of value added as well as an increase in capital income (cash flow, i.e., value added minus labor costs). In large part, this positive effect reflects the increased capital intensity of production. We further find a zero effect on net income per equity.

Finally, our data also permit us to directly study dividend payouts to firm owners. We can therefore directly study the additional investment that may in fact arise from worker representatives locking cash flow inside the firm in form of retained earnings, as could be consistent with, e.g., empire building on the part of managers colluding with workers. Here, we find positive point estimates close to zero, allowing us to rule out even small negative effects on the income received by shareholders. Hence, overall, we find no evidence that shared governance measurably hurts capitalists.

We dedicate our conclusion, Section 8, to reflecting on the particular institutional context, the external validity and implications of our findings. In Section 3, we describe the reform and the Finnish codetermination and wage setting institutions. Section 4.1 presents the research design and the data. We present the results on production outcomes in Section 5.1,

with a particular focus on capital and investment. Section 6 contains the results on labor and capital incomes.

2 The Hold-Up and Disinvestment View of Shared Governance

We here formally recap the influential view of holdup, by which unions and other forms of worker representation can be thought of as rent-extracting institutions (see, e.g., Grout, 1984; Lindbeck and Snower, 1989), and that shared governance is one of such institutions (Jensen and Meckling, 1979). In short, disinvestment emerges because capitalists anticipate that labor, once empowered with formal decision rights, will grab a larger share of output in the form of wages. That is, by this view, shared governance essentially acts just like a distortionary tax on profits.

The core of the hold-up mechanism is that wages are endogenous, specifically increase, in the firm's capital stock K, due to firm-level wage bargaining. Firms hence invests to maximize expected profits, $\pi(w, K) = F(K, \bar{L}) - w\bar{L} - cK$, taking into account this effect:

$$F_K(\bar{L}, K^*) = c + \bar{L}\frac{\partial w^*}{\partial K}, \tag{1}$$

where $F(K)$ is a decreasing returns production function taking labor $\bar{L}$ as given without loss of generality; prices are fixed at one.

The specific microfoundation of the wage setting rule is canonically Nash bargaining. Here, workers will grab share ϕ of surplus *after* capital is sunk – and only has value $c' \leq c$. In our two-stage setting, capital K is purchased in stage 1 at price c, becomes productive in stage 2, then fully depreciates. There is no discounting between periods.

The Nash wage w^* is outside option b plus share ϕ of stage-2 surplus:[3]

$$w^*(K, \bar{L}) = b + \phi\frac{1}{\bar{L}}(F(K, \bar{L}) - b\bar{L} - c'K). \tag{2}$$

[3]Formally, Workers have bargaining power ϕ, such that the Nash wage maximizes $w^* = \arg\max_w\{\phi \log S_2^W(w, \bar{L}, K) + (1-\phi)\log S_2^F(w, \bar{L}, K)\}$. where $S_2^W(w, \bar{L}, K) = \bar{L}(w - b)$ is the workforce's surplus in the second stage: the inside value of the relationship $\bar{L}w + (N - \bar{L})b$ minus the outside option, which is set as Nb, where b denotes some reduced-form flow value of members of the workforce not employed in the firm (unemployment insurance or wages at a reference competitive wage) and N is the total size of the labor entity bargaining with the firm at hand (as in union bargaining models, e.g., Brown and Ashenfelter, 1986; Abowd and Lemieux, 1993). Firm surplus is $S_2^F(w, \bar{L}, K) = F(K, \bar{L}) - w\bar{L} - c'K$ at the bargaining stage, when stage-1 capital expenditures cK are sunk and hence do not enter firm surplus directly. Instead, K enters stage-2 surplus as firms' alternative use of capital in the form of a reselling option at price $c' \leq c$.

The wage increases in K as K boosts output, of which workers grab share ϕ in the form of a higher wage bill. More subtly, K affects the firms' outside option $c'K$, all things equal lowering wages.

The firm anticipate wage setting rule (2). It will *underinvest* ($F_k > c$) if wage bargaining power $\phi > 0$ and capital is at least partially sunk ($c' < c$)L

$$F_K(\bar{L}, K^*) = c + \bar{L}\frac{\partial w^*}{\partial K} = c + \phi(F_K(\bar{L}, K^*) - c') = c + (c - c')\left[\frac{\phi}{1-\phi}\right]. \tag{3}$$

Shared governance – viewed as raising ϕ – entails disinvestment.

In the following sections **??**- **??**, we test the hold-up prediction – studying both investment and wage effects (along with other outcomes). We will find the opposite: positive investment effects. To understand our empirical results, in Section **??** we review alternative variants of capital-labor interaction as well as richer views of industrial relations that can account for our findings.

3 Institutional Context and the 1991 Reform

We provide an overview of corporate governance, the reform that introduced firm-level codetermination, and wage setting institutions in Finland.

Corporate Governance in Finland Finnish companies follow the Nordic board model and overwhelmingly feature a single-tier board structure with a board of directors elected by the general meeting of the shareholders (see, Lekvall et al., 2014, Appendix X, for a detailed overview of corporate governance in Finland).[4] We illustrate this governance structure in Figure 1 Panel (a). The board of directors is responsible for determining the strategy of the firm as well as for appointing, dismissing at will, and overseeing the managing director, who runs the firm on a day-to-day level and is *de jure* not an employee of the firm. The Finnish Corporate Governance Code advises that the majority of directors ought to be independent. In practice, most boards are exclusively comprised of non-executive directors and, in 2013, only 15% of listed firms had their managing director on the board of directors (Lekvall et al., 2014). Executives, including the managing director, must follow and implement instructions from the board of directors (Ringe, 2016). The general meeting of the shareholders sets the compensation for directors; the board of directors or a committee comprised of non-executive directors sets the compensation for the managing director.

[4]Firms can also choose to adopt a two-tier structure with a supervisory board, as in, e.g., Germany, even though very few do so.

Reply from Harri to the question about fiduciary duty (This is now proofread by Harri) Administrative bodies known by the law are subject to Commercial/Corpora

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

Shared Governance Following 1991 Reform As a consequence of the 1991 reform, workers in firms with at least 150 employees have a right to participate in the governance of their firms and to be involved in business and financial decisions.[5] The typical form of representation is through an agreement between the firm and worker representatives of at least two employee groups (manual, non-manual and managerial workers) representing a majority of employees. If no agreement is reached, workers have a statutory right to appoint representatives to the board of directors (or the supervisory board, in the less common dual board structure) or the management group, with the firm choosing between these two options. We illustrate representation on the board of directors as well as in the management group in Figure 1 Panel (b). Statutorily, between one and four worker-elected representatives can be elected and can make up 20% of the respective body (although, by agreement rather than default, firms can 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, and workforce wage setting and other employment-related matters such as strikes.

In a 2019 survey among union members conducted by the trade union federation (Teollisuuden palkansaajat, 2019), 50% of workers in firms with at least 150 employees reported that their firms featured formal forms of shared governance (see visualization in Figure 1 Panel (c)). Among those, 27% followed statutory provisions through the law rather than by agreement. Among firms with shared governance, 40% featured worker representatives

[5] In principle, as in any context, before the reform (and after the reform, if below the cutoff) firms could have voluntarily employed shared governance or installed formal or informal worker voice mechanisms including board representation of workers chosen by shareholders, for example.

on the management team, 26% on the board of directors, and 9% on the supervisory board. Other forms of representation that were reported include advisory boards or regular meetings between top-level management and worker representatives.

Survey and qualitative evidence suggests that worker representatives may influence a variety of firm outcomes. According to a survey among 203 worker representatives conducted by the Finnish Metalworkers' Union in 2001, a substantial share of representatives on the board of directors or the management team report exerting influence over a variety of decision-making processes in their respective firms (Sairo, 2001). In particular, 32% of surveyed management team representatives and 44% of board representatives reported having exerted moderate or high levels of influence over their firms' investment decisions. In an interview conducted by Michael Gold and reported in (Gold, Kluge, and Conchon, 2010), a worker board representative of a shipbuilding company in Turku, Finland, describes concrete examples of how he acts as an intermediary between the board and the workforce and exerts influence over decisions regarding production, workplace safety and environment, or outsourcing (see excerpts in Appendix C.1). He also describes that while he does not directly participate in worker pay negotiations his knowledge of the firm's economic situation acts both as a constraint on communication by management in negotiations with the workforce as well as an input into the negotiation strategy of the shop stewards (see, e.g., Freeman and Lazear, 1995, for a model in which such a reduction in information asymmetry may increase efficiency).

Wage Setting in Finland The Finnish labor market features a high coverage rate of collective bargaining that leaves substantial room for firm-specific wage setting. Unions and employer associations negotiate collective agreements that mandate wage floors at the occupation and job level. The wage floors are rarely directly binding as most employees receive pay premia above the floors (Uusitalo and Vartiainen, 2009). Firms can also deviate from wage increases negotiated in a collective agreement, and can even negotiate pay cuts with consent of the local bargaining parties. Dickens et al. (2007) report low downward nominal but high real, wage rigidity in Finland in an international comparison. Firm-specific pay policies, with profit-sharing arrangements and links between wages and productivity, have become increasingly common since the 1990s (Uusitalo and Vartiainen, 2009). Similarly, performance pay has become more common, with more than half of white-collar and about a third of blue-collar workers receiving some form of performance pay in 2000 (Snellman, Uusitalo, and Vartiainen, 2003). In Section 6, we directly estimate specifications relating changes in wages and productivity (value added per worker) at the firm level (see also Figure 6, panel (d)). On average, we find a rent-sharing elasticity of 0.051, i.e. in the lower end of the range

of estimates surveyed in the literature (where the average is around 0.10, as surveyed in Card et al., 2018; Jäger et al., 2019), and consistent with idiosyncratic productivity shifts affecting wages. In addition, we estimate Abowd, Kramarz, and Margolis (1999) specifications and find substantial between-firm variation in pay policies, accounting for a substantial share of the overall worker-level variation in wages, consistent with substantial scope for firm-level wage setting. Over the course of our sample window, we find that the standard deviation of AKM firm effects accounts for about half of the overall standard deviation in individual wages for most years (a slightly higher share compared to Germany during the same sampling period, Card, Heining, and Kline, 2013).[6]

4 Research Design and Data

Here, we describe our main research design, which exploits the policy discontinuity at the 150 employee threshold in the post-reform period. We also describe placebo discontinuity specifications in the pre-reform period as well a placebo threshold after the reform. Section 4.2 describes our data.

4.1 Regression Discontinuity Design

Our main research design estimates the causal effects of worker representation in firm governance in a regression discontinuity design comparing firms above and below the 150-employee threshold:

$$y_{it} = \alpha + \beta_1 \underbrace{\mathbb{1}[N_{it} \geq 150]}_{\text{Worker Rep.}} + \beta_2 \cdot (N_{it} - 150) + \beta_3 \mathbb{1}[N_{it} \geq 150](N_{it} - 150) + \gamma_t + \nu_{J(i)} + \epsilon_{it}, \quad (4)$$

where y_{it} denotes the outcome of firm i in year t. The running variable N_{it} corresponds to the employment concept relevant to the codetermination law. That is, it counts all employees with more than 90 days of employment and positive earnings in a given year; we do not count short temporary job contracts such as seasonal workers.

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

[6] We plot the standard deviation of log wages and AKM firm effects over time in Appendix Figure A.1. We find a small increase in the standard deviation of wages during the 1990s. To provide a benchmark for the level of wage dispersion (as measured by the standard deviation of log wages), Finland has about 25% lower dispersion compared to Germany up until the early 2000s at which point wage dispersion stagnated (but continued to rise by about 0.06 in Germany, cf. Card, Heining, and Kline, 2013)

specification also includes year effects, γ_t, and industry effects, $\nu_{J(i)}$. We separately show that the industry distribution is smooth at the policy discontinuity by using predicted outcomes based on leave-out means at the industry levels as outcomes that we report in each specification. Finally, we winsorize outcomes y_{it} at the 1% level.

Bandwidth Choice and Inference We estimate linear and quadratic specifications of (4). Our main specification uses the bandwidth choice procedure in Calonico, Cattaneo, and Titiunik (2014), CCT in the following, with a triangular kernel, and we also report results for specifications with a constant bandwidth of 50 employees around the threshold to maintain comparability across outcomes. 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 2 Panel (a). The corresponding McCrary (2008) test does not reject continuity of the density at 150 employees ($p = 0.498$).[7]

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

Graphic Illustration We visualize the data and research design using quadratic specifications and binned scatter plots. For consistency across graphs, we plot the same bandwidth of 50 employees around the threshold. As in our regression specifications, we use a triangular kernel around the policy discontinuity for weighting. We report the $\widehat{\beta_1}$ from our regression specification in the figures as well. These are overall quantitatively very similar. Potential small differences from the RD effect visualized in the figure and the preferred specification reported in the Table (and print in each respective figure) arise from (i) differences in bandwidth (optimal CCT bandwidth vs. fixed, which we also report in the Appendix Section), (ii) inclusion of industry and year effects, and (iii) the CCT bias-correction in the regressions.

Placebo Specifications We additionally estimate placebo specifications in the pre-reform period when no policy discontinuity existed at the 150 employee threshold. A caveat is that, because the pre-period data only covers a subset of the variables and only goes back

[7]Since we also implement placebo regression discontinuity specifications in the pre-reform period, we also plot the density in the pre-reform period from 1988 to 1990 in Appendix Figure A.2 ($p = 0.948$ for McCrary (2008) test).

to 1988, we have three years of observations in the pre-period; consequently, our pre-period specifications have wider standard errors. We therefore also add a placebo specification for a counterfactual policy discontinuity at 100 employees (following Ganong and Jäger, 2018). Finally, we estimate a placebo specification that uses the leave-out mean at the industry-year level for the respective outcome variable as outcome (e.g., the leave-out mean capital share in the relevant industry-year cell). Comparing the true effects to the ones on this predicted outcome allows us to detect the extent to which the effects we find in our non-placebo specification are driven by compositional changes of firms above or below the policy discontinuity.

4.2 Data

We use several administrative data sources on firm- and worker-level outcomes from Statistics Finland as well as from the Finnish Tax Administration. We CPI-adjust all nominal variables (measured in Finnish markka until 2001 and EUR thereafter) to 2010 EUR. We note that the CPI adjustment is completely inconsequential for our estimated treatment effects due to the inclusion of year effects; it matters only for the reporting of control means and the level of the outcome variables in our visualizations.

Summary Statistics In lieu of a separate summary statistics table, we report control means of each our outcome variables in each regression specification. Appendix Figure A.3 reports on the industry composition of our sample, the majority of our firm-year observations stem from manufacturing. In terms of the firm size distribution, the 150 employee cutoff is at the median with 49.4% of employment in firms above the cutoff.

Firm-level Accounting Data (1988 to 2017) Our main dataset, the Financial Statement Data Panel, contains firm-level accounting data from Statistics Finland. The data set is available from 1988 to 2017. The sample is based on Statistics Finland's own 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 the size category of personnel. 1994 onward the data cover the universe of firm population.

The variable content of the data set has also a break in 1994. Some of the variables are missing from the sample for the period before 1994, such as investments and dividends. However, many of the standard profit and loss account variables, e.g., net profits and wage bill are available also before 1994. Similarly, balance sheet information, for example, fixed and total assets and various debt categories are available consistently before and after 1994.

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. These data include all relevant variables in taxation such as sales, inputs, investments, depreciation, other deductions and taxable income.

Matched Employer-Employee Data (1987 to 2017) This dataset, the FOLK Employment Relationship Data, contains individual-level information that we match to the firm-level data sets. The data include both employee and employer identifiers and information about the length of employment relationships in days (spells) at the employee-employer level within a year. We first transform this data set to a long format by year. Then using employee identifiers we merge it with a data set (FOLK Basic panel) that includes all Finnish individuals and their background information such as gender, age, education, occupation group and annual total earned income in each year from 1988 to 2017. Finally, we merge all this information to the firm-level accounting and tax data mentioned above using firm identifiers.

Employment Measure The employment concept relevant for the shared governance threshold (and our running variable) is the number of employees excluding temporary and seasonal workers. We construct this analogue from our data on employee-employer job contract pairs. We exclude temporary and seasonal workers from the workforce by dropping all workers with fewer than 91 days of contracted work with a specific employer within a year and workers with no earned income in a a given year. Any mismeasurement will likely attenuate a potential treatment effect in our RD design, which assumes that we precisely measure the statutory employment concept in the running variable. Also, as our data set is in panel format, we can follow firms over time and identify firms with 150 employees or more in consecutive years. Based on our conversations with legal practitioners, the conditions to trigger the codetermination law are definitively met when a firm meets the 150 employee threshold in two consecutive years. Our running variable for the regression discontinuity design is contemporaneous employment in a given year. We also assess an indicator for having had at least 150 employees in the last two years. In Figure 2 Panel (b), we show that this probability changes discontinuously by 47.2 percentage points (SE 0.018) at the threshold. This indicator is necessarily zero for all firms firms below the threshold in the given year. An instrumental variables interpretation of our findings, using the two-year trigger described by legal practitioners, would thus lead one to roughly double the reduced-form estimates (i.e., scale up by 1/0.472).

Executive Compensation and Company Board Member Data (1994 to 2018) We draw on individual-level board-member data for all Finnish firms. On top of the individual-level identifier, these data include the status of the board member: CEO, regular board member and substitute member. These data are available from 1994 to 2018. We merge board data to individual-level (FOLK Basic panel) income data to measure executive compensation.

5 Production and Capital Intensity

We present the effects on production, capital intensity in Table 2. Appendix Tables A.3 through A.6 report additional specifications and outcomes .

5.1 Capital and Investment

Our core set of outcomes is investment, the capital stock, and capital intensity of the production process. These variables are of particular interest because the hold-up view of shared governance predicts negative effects on capital formation (Jensen and Meckling, 1979; Jäger, Schoefer, and Heining, 2019).

Investment As a first outcome we study investment (capital expenditure) in Figure 3 Panel (a) and columns (1) through (4) of Table 1. Visually, a clear discontinuity of log investment at the 150 employee threshold of about 0.2 is discernible. In column (1) of Table 1, we find a positive effect of 0.232 (SE 0.094) in the linear specification and 0.240 (SE 0.118) in the quadratic specifications with optimal bandwidth choice. Quantitatively, the estimates imply that investment is about a quarter high in firms in which workers have a right to participate in governance. We find no evidence for placebo effects at the 100 employee cutoff or for compositional changes as indicated by predicted outcomes shifting (unlike for assets, we cannot study effects on investment in the pre-period as this variable is not recorded then).

A potential concern with the previous specification is that log investment is only defined for strictly positive values. We first check whether the effects shroud or are driven by an extensive margin effect (e.g., if firms are less likely to invest on the extensive margin). In column (2) of Table 1, we instead show that, if anything, firms with shared governance are slightly *more* likely to have strictly positive capital expenditure with a point estimate of 0.014 in the linear (SE 0.015) and quadratic specifications (SE 0.017). To further probe the robustness to the inclusion of zero capital expenditure, we additionally also use an inverse hyperbolic transformation of investment and, due to the lumpy nature of investment, we consider a rolling average over three years. In column (3), we document positive estimates

of 0.169 (SE 0.087) and 0.218 (SE 0.102) for the transformed outcome variable. Our conclusions are thus robust to explicitly accounting for zero capital expenditure. We furthermore find no significant effects for predicted outcomes or when assessing estimates at the placebo discontinuity at 100 employees.

Finally, we do not detect any significant effects on the composition of investment between (i) machines and equipment, (ii) buildings and structures, and (iii) software, reported in Appendix Table A.2, suggesting that increases in investment arise across the board.

Capital Stocks We report effects on total assets in Figure 3 Panel (b) and in column (4) of Table 1. The figure shows a sharp and discernible change in total assets of about 0.1 at the policy discontinuity of 150 employees. We report estimation results in column (4) of Table 1 and find estimates of 0.148 (SE 0.065) in the linear specification and 0.146 (SE 0.079) in the quadratic specification. That is, shared governance leads to about a fifteen percent increase in total assets. We find substantially smaller or even negative point estimates of -0.032 (pre-period), 0.037 (placebo discontinuity at 100) and 0.006 (predicted outcomes).

We next decompose the asset effects in Figure 5 and columns (5) and (6) of Table 1 and show that the asset effects are driven by fixed rather than current assets. Fixed assets consist of tangibles (buildings and equipment) and intangibles (e.g., patents or trademarks), which our data do not further separate out. This is the closest measure to productive capital that our data allow us to measure. Here, we again document large positive effects of 0.233 (SE 0.106) and 0.277 (SE 0.123). We also investigate effects on fixed assets in the pre-period and find substantially smaller or even negative (pre-period) placebo estimates. We also study current assets, such as cash, inventory or accounts receivable, and find a substantially smaller effect and negative point estimates of -0.051 (SE 0.100) and -0.041 (SE 0.113), reported in column (6). The total asset effect we documented in column (4) is thus driven by fixed rather than current assets.

The positive effects on capital formation reject the disinvestment predicted by the hold-up view, implying that it either is quantitatively irrelevant for this specific institution, or that counteracting forces offset holdup by crowding in more investment.

5.2 Scale and Productivity

We next study with effects on production scale, studying output and input measures, including labor inputs. Having studied firm scale, which is also a marker of productivity in many models (see, e.g., Lucas, 1978; Melitz, 2003), we then estimate productivity effects directly.

Output We report effects on log revenue and value added in Table 2. We find positive point estimates of 0.064 (SE 0.046) in the linear specification and 0.063 (0.055) in the quadratic specification. We find comparable positive effects on value added of 0.07 (0.043) and 0.048 (SE 0.053) in the linear and quadratic specifications, respectively. For both outcomes, we find no tantamount positive effects in the pre-period or for predicted outcomes; however, we find sizable (but also not significant) estimates at the placebo discontinuity of 200 employees of about 0.05. Taking the linear specifications for the treatment effects, our estimates allow us to rule out that shared governance reduces revenue and output by more than 0.026 or 0.014, respectively.

We have further investigated whether firms with shared governance outsource fewer production activities (as documented in Jäger, Schoefer, and Heining, 2019, , e.g., for the share of revenue produced in-house), but found no evidence for such a mechanism, as indicated already by the quantitatively similar revenue and value added point estimates. We also report effects on log materials directly in column (3) of Table 2 and who positive but statistically not significant effects of about 0.11.

Tenure We next study tenure in column (4) of Table 2. Tenure or turnover may plausibly be affected by worker voice (Hirschman, 1970). Tenure is also a proxy for worker surplus and job quality capturing job attributes valued by workers beyond wages (which we will study below separately). We find an increase of 2.9 log points (SE 3.4) on tenure in the linear specification and 6.8 log points (SE 4.2) in the quadratic specification. Since our matched employer-employee data only go back to 1988, we have also studied effects from 2005 to 2016, when right-censoring of tenure ought to matter less, and find slightly larger effects of 0.038 (0.047) in the linear specification and 0.105 (SE 0.059), i.e., statistically marginally significant, in the quadratic specification.[8] Our results are thus consistent with small or moderate positive effects on tenure but we cannot reject a zero effect at the 5% level in any specification.

Education and Occupation Groups We next study the education and occupation structure and find no compositional shifts. Since we consider a total of eight different groups, we report results in Appendix Tables A.5 and A.6. We consider the share of employment in five education categories in Appendix Table A.5, namely (i) missing/lower-than-secondary education (control mean: 18.4%), (ii) secondary education (57.1%), (iii) post-secondary education (9.20%), (iv) bachelor's degree (11.5%), and (v) master's degree or PhD (2.63%). Across these categories, we detect no systematic effects, with point estimates close to zero and stan-

[8]See Appendix Table A.4 for these specification as well as using separation rates as outcome.

dard errors between 0.005 and 0.01. We next study broad occupational shifts in Appendix Table A.6. We consider the share of (i) manual workers (control mean: 41.0%), (ii) lower-level white-collar workers (21.4%), and (iii) upper-level white-collar workers (8.44%), as well as a category of missing occupation information (29.3%). Again, we find no compositional shifts, with point estimates ranging between -0.003 and 0.004 (SEs around 0.1).

Capital-Labor Ratios Building up to our analysis of labor productivity, we study the capital-labor ratio in column (5) of Table 2. Consistent with the effects on log capital, we find positive and significant effects on log fixed assets per worker of 0.208 (SE 0.104) in the linear and of 0.244 (SE 0.120) in the quadratic specification. We find smaller or even negative effects for the placebos in the pre-period and at 100 employees as well as the predicted outcomes.

Labor Productivity and Total Factor Productivity We next analyze value added per worker as a labor productivity measure. In Column (7) of Table 2, we document positive point estimates for value added per worker at 0.062 (SE 0.043) in the linear specification and of 0.043 (SE 0.051) in the quadratic specification. In both cases, the 95% confidence interval includes zero and we can reject effects more negative than -0.022 or -0.056, respectively.

5.3 Quantitative Evaluation: The Size of the Capital Effects

We find that granting workers shared governance rights leads to an increase of 0.23 increase in log investment and of 0.14 in log assets, driven by a 0.23 increase in fixed assets. We also find moderate value added effects. We now evaluate the size of these effects (i) within our study in a production framework, and (ii) by comparing our estimates to others in the literature. Drawing on basic economic theory, we also conduct a back on the envelope calibration approach, by (iii) assessing the implied losses implied by an interpretation of the capital effect as being a mere distortion, and (iv) asking which bargaining models could rationalize our findings.

Consistency of Output and Capital Effects Why is that we find large capital increases but small output per worker effects? The data and basic production theory offer a two-step explanation. First, we find no effects on total factor productivity, calculated as the firm-level Solow residual from a Cobb-Douglas production function with capital and labor, and an industry-level long-run labor income share. In column (7), we document point estimates of -0.03 (SE 0.064) and -0.04 (SE 0.075). In both cases, the confidence interval includes zero and, for the linear specification, ranges from -0.155 to 0.095 for the linear specification. Hence,

we find no evidence that worker voice raises efficiency. We also find relatively wide standard errors and large (but not statistically significant) placebo effects, suggesting limited power of our analysis total factor productivity. Our findings are broadly in line with Jones et al. (2010), who find no relationship between measures of employee involvement and productivity in a survey of Finnish manufacturing firms (see also Ichniowski et al., 1997; Black and Lynch, 2001, for evidence for positive productivity effects of employee involvement).

Second, the capital share in our sample is not large, around 0.207 on average (control mean of Table 4 column (1)), which also represents the elasticity of value added per worker to the capital-labor ratio (holding efficiency constant). Hence, a 20% increase in fixed assets times a capital share of 0.2 would lead to a 4% predicted increase of value added per worker, provided no TFP effect (as we find), close to our actual point estimates of 4.3% and 6.2% .

Capital Effects from the Literature The magnitude of our capital effects falls within the range of estimated firm-level investment responses to other policies or variation.

First, investment exhibits sizable responses to idiosyncratic cost shifts. In the context of Finnish firms, Benzarti and Harju (2020) find a 14 log point effect on total assets driven by a 18 log point increase in fixed assets in response to a 2.6ppt payroll tax increase in an RD design in firm size. Zwick and Mahon (2017) documents a 17.7 to 28.9 log point effect of bonus depreciation policy among U.S. firms (importantly including non-Compustat firms in the population given by tax data). Cingano, Manaresi, and Sette (2016) finds a 20-40% investment effect on firms exposed to bank loan supply shocks in Italy.

cite capital-labor substitution footnote (0.5-0.6, meta study), to clarify that purely price-driven ones are likely too low.

Second, of particular interest to our study is how corporate governance variation may affect investment. There is a literature on how individual executives have large effects on even large firms' investment policies. For example, in the context of Compustat firms, Bertrand and Schoar (2003) study managerial styles and find that the difference between a manager at the 25th percentile of the distribution of investment (to capital ratio) to one at the 75th percentile is 20 log points. Pan, Wang, and Weisbach (2016) find a 29% (6.9/24) predictable effect on investment over the course of a new CEO's tenure comparing the first three years to the later years. In the context of smaller, Danish firms, Bennedsen et al. (2007) employ an IV strategy in family firms that reveals a treatment effect of death-driven successions of CEOs by family members rather than outside directors reduces assets by 0.30 (SE 0.24).

Third, a key motivation of our study is the limited amount of quasi-experiment evidence for how firms' capital investment responds to shifts in the labor-capital interaction within the firm specifically in form of shared governance. Most related to the current paper, Jäger,

Schoefer, and Heining (2019) find about a 50% capital stock increase in firms subject to a mandate to shared governance in terms of mandatory board-level representation. These point estimates for the capital effect are therefore very similar to the present study. Second, in the context of U.S. firm-level unionization events, Schmalz (2018), using the close-election RD design of Lee and Mas (2012), finds an effect of 3ppt decline in the investment/sales ratio.In the conclusion Section 8, we discuss why generally the institution of shared governance may dramatically differ from this adversarial context.

Benchmark: Dispersion in the Capital-Labor Ratio Cross-Sectional: in our 100-200 sample (with industry-year FEs)

Within-firm: say take 10-year differences of a firm or 5-year or 3-year, and ask what rank switch this would imply of the K/L ratio

Ballparking the Implied Losses from a Pure Distortion View One useful way to assess the plausibility of the capital losses is to benchmark them against an interpretation by which they constitute pure distortions within a stable production function.

Profits $\pi(K,L) = F(K,L) - wL - rK$ are output $F(K,L)$ minus payroll wL and capital costs rK, where r is the rental rate or otherwise use cost of capital; factor prices are taken as given. Suppose that the control group's profit is maximized at $\pi(K^*, L^*)$. The treatment group chooses, due the institution we study, another level $\hat{K}$ and $\hat{L}$, which may deviate from the profit-maximizing levels. We apply a second-order approximzation of the resulting profit loss. The envelope theorem eliminates first-order terms. Our RD design in employment does not permit us to study employment effects, and we moreover did not find workforce composition effects. As an upper bound, we therefore consider a setting in which L is not moving, such that the capital adjustment fully passes through into distortions in the product mix without offsetting reoptimization of labor. This assumption eliminates the terms involving L, reducing the loss function to as simple quadratic function of the capital distortion:

$$\pi(\hat{K}, \hat{L}) - \pi(K^*, L^*) \approx \frac{1}{2} F_{KK} \cdot (\hat{K} - K^*)^2, \tag{5}$$

where we used $\pi_{KK} = F_{KK}$. Assuming Cobb-Douglas but not necessarily constant returns with scale parameter σ, $F(K,L) = K^{\alpha} L^{\sigma-\alpha}$ (such that profits could emerge with, e.g., barriers

to firm entry), we have:

$$\pi(\hat{K},\hat{L}) - \pi(K^*,L^*) \approx -\frac{1}{2}\alpha(1-\alpha)K^{*\alpha-2}L^{*\sigma-\alpha}\cdot(\hat{K}-K^*)^2 \tag{6}$$

$$\Leftrightarrow \frac{\pi(\hat{K},\hat{L}) - \pi(K^*,L^*)}{F(K^*,L^*)} \approx -\frac{1}{2}\alpha(1-\alpha)\cdot\left(\frac{\hat{K}-K^*}{K^*}\right)^2 \tag{7}$$

$$\approx -\frac{1}{2}0.2(1-0.2)\cdot 0.148^2 \approx 0.0018 \tag{8}$$

where we have used the capital share α in value added of around 0.2 and a capital effect of 0.148 from our broadest definition of the total asset effect. That is, the implied profit reduction from our capital effects is 0.18 percentage points in the profit-value added ratio, with the profit concept in accounting data most closely resembling earnings before interest and taxes but after depreciation, i.e. value added minus payroll minus depreciation.

translate into divide-by-revenue effects, we have the mean VA/Rev ratio in table! (outsourcing)

below, we check on profit effects. don't find them. TFP? swithcing alpha? etc.

As another benchmark, a positive TFP effect of 0.18% could fully offset the profit distortion from the capital effect, well within the (wide) confidence interval of our TFP effects.

Below in Section 6, we will directly study the effects of shared governance on profits.

TFP effect that could offset this

Implied ϕ, ι offsetting TFP effect to break even...? under estimate etc

"Scaling up the effects of investment subsidies on capital (perhaps from Finland?), this would be equivalent to a XXXppt subsidy on invesment?" "This corresponds to an XXX% of the gap in capital intensity comparing Finland to OECD average" "This corresponds to moving from the 50th percentile to the XXXth percentile of the industry distribution of capital (or between firms rather than across industries)"

6 Dividing the Pie: Wages, Rent Sharing, and Capital Income

We now study the distribution of the firm's value added between, and within, capital and labor.

6.1 Wages and Wage Structure

We start our analysis by studying wages. A central prediction of the hold-up view, and of wage bargaining models more generally, is that granting workers more authority will raise wages.

Total Labor Costs We study total labor costs at the firm level in Figure 6 and in column (1) of Table 3. Total labor costs are the sum of the wage bill and other labor costs such as payroll taxes and employee benefit programs. The econometric results reveal positive and statistically significant effects on log total labor costs of 0.087 (SE 0.033) in the linear specification and 0.087 (0.037) in the quadratic specification. We find no tantamount increase in the pre-period with a placebo estimate of 0.005 (SE 0.058); similarly, we find substantially smaller effects at the placebo threshold of 100 employees (0.016, SE 0.027) and using predicted outcomes (0.002, SE 0.019).

Worker-Level Wages We now turn to the firm's average worker-level log wages, starting with raw wages, which we show in panel (b) of Figure 6. Here, we find a positive effect of 0.050 (SE 0.024) in the linear specification and 0.051 (SE 0.028) in the quadratic specification for the effect of shared governance on log worker-level wages (Table 3, Column (2)). We find no comparable increase in the pre-period placebo estimates (0.009, SE 0.038), at the placebo discontinuity (0.015, SE 0.018) or using predicted outcomes (-0.005, SE 0.012). We also study the effect on log median wages in column (5) and find comparable estimates to the ones for average worker-level log wages, with point estimates of 0.044 (SE 0.023) and 0.042 (SE 0.028) for the linear and quadratic specifications, respectively.

Are Shared Governance Firms High-Wage Firms, or Do They Employ High-Wage Workers? What explains the positive effects of shared governance on raw average wages? Shared governance firms could either be high-wage firms, i.e., pay higher wage premia. Or, shared governance workforces may consist of high-wage workers, who would earn higher wages no matter their workplace due to, for example, higher skill. That is, the wage effects could be driven by firms' shifting wage policies, or may arise from worker composition (although our analysis of basic skill proxies in Section 5.2 did not indicate such effects). We now separate the wage effects into whether shared governance firms become high wage firms, or whether they hire high-wage workers. We use Abowd, Kramarz, and Margolis (1999) regression specifications on worker-level wages, using our matched employer-employee panel data with firm fixed effects (denoting composition adjusted firm pay premia) and worker fixed effects (capturing the permanent earnings potential of a worker) and 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 treatment assignment based on employment in t.[9]

Worker Selection: AKM Worker Effects To study the role of workforce composition in the raw wage effects, we report effects on AKM worker fixed effects in column (3) of Table 3, where we document a precisely estimated positive effect of 0.005 (SE 0.004) in the linear specification. We thus find evidence that shared governance leads firms to attract or retain workers with a higher permanent earnings capacity as proxied for by their AKM worker effect.

Isolating Firm Pay Premia: AKM Firm Effects The firm fixed effects are a simple estimate of the pay premia the firm extends to all employees irrespectively of their permanent earnings potential. They can represent differences in bargaining power,

We report effects on AKM firm pay premia in column (4) of Table 3 and document a small positive effect of 0.011 (SE 0.010) in the linear specification and 0.015 (SE 0.19) in the quadratic specification. Our results are thus consistent with small positive effects and the 95% confidence interval for the linear specification ranges from -0.013 to 0.041.

Together, our AKM effects appear inconsistent with the wage premium in the unadjusted worker-level wage effects, although we note that the *sum* of the worker and firm effects lie well within the confidence interval of the raw wage effect in column (3). Moreover, we suspect that the sample differences (our firm AKM effects are only estimated in a fraction of the sample, namely a connected set [and moreover only for a subset of sample years due to estimation speed ahead of the conference deadline.]

Rent Sharing The wage effects could be unconditional pay premia extracted similarly across all firms. Or, they may reflect workers extracting a larger share of the value-added pie in a bargaining setting. In fact, the workhorse model of hold-up assumes split-the-surplus rules like Nash bargaining, by which the pass-through of productivity into wages would identify the bargaining parameter (as in, e.g., Jäger et al., 2019) that is hypothesized to increase following increases in worker authority in corporate decisions (Manning, 1987).

To study for potential shifts in the wage setting process indicative of higher worker bargaining power in wage setting, we study effects on rent sharing, the cross-sectional relationship between firm-level wages and productivity, using the typical log-log specification.

[9] A caveat to our current analysis is that the AKM results are currently based on data up to 2004 due to computational reasons and will be updated with the results for the whole sample in the next days.

In Figure 6 Panel (d), we plot year-to-year changes in average log wages against changes in log value added per worker. For both firms above and below the 150 employee discontinuity, we find similar rent sharing coefficients of 0.051 (SE 0.004) above and of 0.050 (SE 0.003) below the policy discontinuity. In the Figure, we also report the population average in the overall Finnish economy with a rent-sharing elasticity of 0.051 (SE 0.001). Overall, we thus find no evidence that rent sharing increases as a consequence of giving workers rights to shared governance.

To further study rent sharing, we interact the right-hand side of specification (4) with log value-added per worker, and study whether productivity differences have a larger or smaller effect on log wages in firms affected by the shared governance reform:

$$\begin{aligned}\ln(w)_{it} = \alpha + \beta_1 \mathbb{1}[N_{it} \geq 150] + \beta_2 \cdot (N_{it} - 150) + \beta_3 \mathbb{1}[N_{it} \geq 150](N_{it} - 150) \\ + \rho_1 \mathbb{1}[N_{it} \geq 150] \cdot \ln(\mathrm{VA}_{it}/N_{it}) + \rho_2 \cdot (N_{it} - 150) \cdot \ln(\mathrm{VA}_{it}/N_{it}) + \\ + \rho_3 \mathbb{1}[N_{it} \geq 150](N_{it} - 150) \cdot \ln(\mathrm{VA}_{it}/N_{it}) + \gamma_t + \nu_{J(i)} + \epsilon_{it}.\end{aligned} \tag{9}$$

The coefficient of interest is ρ_1; positive values of ρ_1 indicate higher degrees of rent sharing in firms with worker representation following the reform. We report results in column (5) and find *negative* point estimates of -0.060 (SE 0.029) in the linear specification and of -0.026 (SE 0.019). Alas, we also find a sizable, and marginally significant effect in the pre-period placebo specification of -0.150 (0.086).

Overall, we therefore find no increases – if anything, decreases – in the degree to which workers benefit from productivity boosts at the firm. One implication is that the mechanism by which hold-up deters the fruits of capitalists' investment (higher output). Another substantive interpretation is that the wage effects appear to not be concentrated only in the most productive firms. These findings are consistent with previous empirical evidence for the absence of the wage channel hold-up (Card, Devicienti, and Maida, 2014; Jäger, Schoefer, and Heining, 2019).

Within-Firm Wage Structure Within-firm compression is one potential outcome of worker representation (as in the case of unions Freeman and Medoff, 1985; Western and Rosenfeld, 2011), in light of empirical evidence for firms sharing their rents unequally across worker types (Saez, Schoefer, and Seim, 2019; Drenik et al., 2020). We now directly study the within-firm ratio of log wages at the 75th to the 25th percentile, reporting results in column (6). We find negative point estimates of -0.036 (SE 0.022) and -0.035 (SE 0.025). This indicates a reduction by about 3.6% relative to a control mean of 0.78 (indicating that workers at the 75th percentile in firms at the cutoff have salaries 78 log points higher than

those at the 25th percentile). That is, we find some but noisily estimated evidence consistent with pay compression within the firm in shared governance firms.

Executive Compensation We identify executives of a company (specifically using the board-level dataset, as often the highest level executives are not formally employees), and study executive compensation. Table 3, column (7), reports an effect on log executive compensation of -0.043 (SE 0.053) in the linear and of 0.000 (SE 0.071) in the quadratic specification. We further express executive compensation as a share of a firm's total labor costs, we find a large reduction reduction with point estimates of -0.098 (SE 0.047) in the linear specification and of -0.048 (SE 0.060) in the quadratic one. Our results thus point towards a reduction of the share of payroll to executives, but are less consistently estimated than the direct worker-level wage effects.

6.2 Capital Income

We now study the capital side of income, which has been the focus of the theoretical discussion of the effects of shared governance. For example, Jensen and Meckling (1979) argue that shared governance reduces profitability, and argue that, in line with the shareholder value paradigm (Friedman, 1970), capitalists would voluntarily adopt shared governance if it were profitable. By contrast, Freeman and Lazear (1995) and Levine and Tyson (1990) provide arguments for why individual capitalists may not do so even if advantageous as a macro institution.

Capital Share We start with firm-specific capital shares in column (1), calculated as one minus the wage bill divided by value added, in Table 4. We find no effects on the capital share with estimates of 0.002 (SE 0.010) in the linear specification and -0.004 (SE 0.012) in the quadratic specification, allowing us to rule out even small changes.

Capital Income We next study capital income in column (2), i.e., value added minus payroll, hence the cash flow accruing to capital, i.e., firm owners and creditors. If anything, we find an increase of 13.9 log points (SE 5.2) and 2.9 log points (SE 10.3) for the linear and quadratic specifications, respectively.

Profitability We next study net income (i.e., earnings after depreciation, interest, and taxation) as a measure of profitability in columns (3) and (4). This corresponds to the residual that the firm can retain or pay out as dividends. Column (3) normalizes by (the book value of) equity ("return on equity"), leading to small positive point estimates of 0.020

(SE 0.168) and 0.029 (0.189). Relative to the control mean of 0.30, these effects are small, and the confidence intervals include zero. Column (4) normalizes net income by revenue, with effects of -0.006 (SE 0.007) in the linear specification and 0.017 (SE 0.009) in the quadratic specification relative to a control mean of 0.01. However, recall that we found positive but noisily estimated effects on revenue, the denominator. Our estimates therefore cannot rule out small declines in profitability.

Dividend Payouts to Firm Owners The analysis of profit effects revealed no clear patterns for reductions in profitability, and, if anything, suggest an increase in cash flow. However, net income measures the income that can potentially be paid out to owners, but it could also be retained and reinvested inside the firm. More importantly, firms with shared governance may actually pay out *less* than purely capital-controlled firms, and indeed our core results on positive investment effects may reflect yet another agency conflict akin to empire building on part of the workers Jensen and Meckling (1976).

We therefore directly study dividend payouts in logs in column (5), an indicator for positive dividends in column (6), and a zero-robust inverse hyperbolic sine transformation of a three-year moving average of dividend payouts in column (7). Throughout, we find positive point estimates on all measures of dividend payouts.

Finally, we study whether firms pay out any dividends in a given year. Column (5) of Table 4 reveals positive point estimates on the probability to pay out a dividend of 0.041 (SE 0.027) in the linear and of 0.029 (SE 0.03) in the quadratic specification.

Finally, in column (8) we revisit the negative point estimate on net income per revenue and find an increase in dividends as a fraction of revenue, a point estimate of 0.004 (SE 0.002), suggesting that the net income effect is not associated with reduced dividends.

Interest Expenditures Capital income, net of depreciation and taxes, can be divided between owners and lenders. In column (9) we study the effects on interest expenditures normalized by revenue. We find no evidence that shared governance firms pay lower or higher interest payments, with point estimates of 0.020 (SE 0.014) and 0.011 (0.008), which in principle could arise from labor-capital interactions through financial markets or frictions (Matsa, 2010; Chen, Kacperczyk, and Ortiz-Molina, 2011; Schoefer, 2015; Lin, Schmid, and Xuan, 2018; Matsa, 2018).

7 Mechanisms and Interpretation

To tease out the potential mechanisms of our finding and to ask whether the hold-up mechanism may be prevalent in pockts of the labor market, we analyze heterogeneity in the effect on wages and investment.

Effect Heterogeneity To further probe and understand our main findings, we now study heterogeneous treatment effects for the outcomes log investment and wages. Our research design lends itself primarily to studying heterogeneity by industry (as time-variying firm characteristics may be endogenous to the treatment). We estimate specification (4) separately in subsets of the sample. To do so, we first calculate characteristics at the 3-digit industry-by-year level and then split industries into those above and below the median. Importantly, we calculate characteristics using observations outside our estimation sample, thus excluding firms with employment between 100 and 200 employees, to avoid biasing our heterogeneity analysis (see Abadie, Chingos, and West, 2018, for related arguments in the context of randomized experiments).

We analyze four categories of heterogeneity variables. First, we study two wage flexibility measures and split industries according to the between-worker standard deviation of wages as well as the rent-sharing elasticity. Second, we consider two measures of market competition, the Herfindahl-Hirschman Index (HHI) of sales and the Lerner index (calculated as one minus the ratio of profits to sales). Third, we consider measures of job quality, studying the separation rate and firms' AKM pay premium. Lastly, we consider measures of capital intensity by studying heterogeneity by fixed assets per worker, value added per worker, and the capital share. We report effect heterogeneity for investment and wages in Tables 5 and 6 and Figure 7 and more detail on data construction in Appendix Section D.

We discuss interpretations below and separate our discussion of the heterogeneity analyses along two themes. First, in Section 7.1, we ask whether the hold-up mechanism may be masked in the particular setting we study. Second, in Section 7.2 we study which richer views of shared governance and industrial relations may rationalize our findings.

7.1 Salvaging Hold-Up: Masking Mechanisms

Wage Rigidity The hold-up problem may be muted in settings where wages are fixed or set through centralized bargaining. For example, Acemoglu (2001) provides a model in which wage floors can resolve the hold-up problem and induce more efficient investment. Relatedly, Levine and Tyson (1990) argued that the benefits of worker participation in firm governance are particularly large when wages are compressed. Studying shop-floor codetermination,

Freeman and Lazear (1995) argued that such codetermination institutions "fit better in labor relations systems where pay and other basic components of compensation are determined outside the enterprise (essentially bounding divisions of the rent) than in systems where firms set pay". Essentially, this setting amounts to setting working bargaining power to zero (or a low value) in the hold-up model.

We study two markers of wage flexibility and compared segments of the labor market differing by (i) how much firms share rents with workers as well as (ii) a direct measure of wage dispersion, the between-worker standard deviation of wages in an industry-year cell.

Labor-Substituting Capital One hypothesis is that boosts to worker bargaining power lead firms to shift investment into labor-substituting technologies. This richer view of hold-up would lead to a decline in employment at a given output level. While we cannot directly test for employment effects (as employment is our running variable), we have not found effects on the workforce composition in terms of observable skill-related characteristics. We also did not see a decline in wages (although perhaps bargaining and productivity effects may offset each other) Moreover, if anything we see an increase in output.

Capital Durability At the core hold-up model is the ex-post surplus that is created by sunk capital investments. Perhaps firms substitute into shorter-lived or easier to liquidate assets, as another response hold-up. We have additionally studied, in Appendix Table A.2 the composition of investment. We did not see a shift away from, for example, R%D, for example, which is plausibly very long-lived and also hard to liquidate.

The Capital Intensity and "Good Jobs"

Lease vs. Own One interpretation of the capital effects is that it reflects a switch from renting capital to owning capital. The hold-up channel would likely predict a switch to renting to begin with. Moreover, if anything, we find that materials go up as does revenue, making this masking of hold-up effects unlikely. We also did not find compositional effects of the investment categories.

7.2 Industrial Relations Views Beyond Hold-Up

Overcoming Hold-Up by Switching Bargaining Regimes One intriguing, but difficult to directly test for, way to view the positive investment effect is that it involves a shift in the regime to efficient bargaining, out of an inefficiently low investment regime for firms without shared governance, if switching to a regime of simultaneous rather than sequential

bargaining over capital and wages (Crawford, 1988). In repeated games without commitment, reputation building may help overcome hold-up and result in efficient investment levels (Van der Ploeg, 1987); shared governance may facilitate such repeated interactions and may thereby raise investment.

Alternatively, Jäger, Schoefer, and Heining (2019) build on Manning (1987) to argue that the effect of shared governance on investment depends on whether the institution is thought of as boosting workers' bargaining power in wage negotiations only, or whether it may more broadly involve workers on the board. If the former, then in fact workers can be viewed as offsetting the disinvestment effects by adding yet another agency conflict in the firm, perhaps leading to more investment as a second-best outcome (against the interest of capitalists).

In principle, our evidence for positive investment effects supports both of the aforementioned views. Moreover, the absence of negative profit or dividend effects speaks to the efficient rather than the joint bargaining interpretation. Moreover, the absence of clear positive wage effects or shifts in the rent sharing elasticities would not support the latter model.

Implicit Contracts and Coordination Problems Specifically and relatedly, by information sharing, shared governance may reduce information asymmetries underlying coordination and contracting problems, if, for instance, workers do not trust management's assessment of firm productivity or product demand states (Grossman and Hart, 1981; Malcomson, 1983). By having workers participate in firm management, shared governance may attenuate hold-up of workers by firms, as with firm-specific training or back-loaded compensation.

Corporate Governance, Management Quality, and Agency Conflicts Perhaps worker representatives collude with manages to engage in empire-building (Jensen and Meckling, 1976), or help entrench managers (Pagano and Volpin, 2005; Atanassov and Kim, 2009). In contrast, monitoring through worker representatives could also be more stringent, if executives wield more influence over shareholder directors (Hermalin and Weisbach, 1998). or curb manager's short-termism if their horizon is longer than that of outside shareholder directors.. Alternatively, the higher diversity of objectives could decrease managerial accountability (Tirole, 2001, p. 59-60). Finally, shared governance could affect the selection of managers.

Financial Markets One potential explanation for our effects me be that shared governance affects the cost of capital by selecting of safer projects, which in turn affects investment and drive the investment increases (e.g., bond yields and unionization are negatively correlated in the U.S., and employee representation is associated with higher leverage in Germany Chen, Kacperczyk, and Ortiz-Molina, 2011; Lin, Schmid, and Xuan, 2018). Or, firms respond to

hold-up by increasing leverage and removing free cash flow (e.g., Matsa, 2010).

The Firms' Competitive Environment

Cooperative Industrial Relations

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Information Channel and Collective Voice If workers have some informational advantage, capitalists may find it optimal to grant labor real authority (Aghion and Tirole, 1997), particularly in matters more important to labor than capital. More broadly, shared governance as an institution for collective voice could lower turnover (Hirschman, 1970; Freeman, 1980) and, by facilitating information flows and cooperation, improve productivity.[10] In Finland, shared governance has an explicit voice function, by, e.g., permitting the installation of advisory councils.

While we do find that output per worker increases, we cannot rule in or out clear effects on efficiency (TFP). We have also not found effects on separations, although we have found that low separation rates appear to amplify the positive investment effects.

- Add survey results, direct effects

8 Conclusion

We have studied a 1991 reform in Finland that introduced the right to shared governance as a function of a firm's employment size, generating a discontinuity at 150 employees. Overall, we have found that the policy variation in shared governance has yielded a striking rejection of the canonical hold-up view of codetermination: while we have found moderate positive wage effects largely reflecting worker selection, we have found that shared governance *boosts* capital formation, all with no negative effects on profits and dividends. What may account for the positive investment effects?

One plausible candidate is that workers may not, as the hold-up review assumes, solely and primarily be involved in wage setting. In fact, in the Finnish setting, worker representatives are explicitly barred from formal participation in wage setting. In practice, we suspect that

[10] Ichniowski and Shaw (1999) show that employee participation has a positive association with productivity in the steel sector across countries. For U.S. unions, Freeman and Medoff (1985) and Black and Lynch (2001) link cooperative labor-management relations with productivity.

through various indirect means, worker representations may well find wages to boost wages, say through facilitating appointments of labor-friendly managers (Bertrand and Schoar, 2003; He and le Maire, 2019), by making labor harder to replace, or by boosting labor's power more generally – consistent with the moderately positive effects on worker selection and on firms' pay premia. However, we have not found increases in rent-sharing elasticities; hence, shared governance does not appear to raise conventional bargaining power concepts.

Our evidence for positive capital effects is also consistent with richer views of labor relations. For example, if workers were involved in broader management decisions, as is in the case in Finland through board or management participation, models with bargaining over capital inputs would predict workers to call for higher capital formation (as in Manning, 1987; Jäger, Schoefer, and Heining, 2019). Similarly, efficient bargaining, if induced by shared governance, would lead to higher capital levels by overcoming hold-up (Crawford, 1988).

Finally, we note that the institution we study provides a relatively moderate boost of worker voice and codetermination, in fact giving *firms* the choice over the modality of shared governance – on the board or in the management group – workers will be involved in. Moreover, the policy explicitly permits the firm and its workforce to agree on alternative arrangements in lieu of this default implementation, such as advisory councils. Workers are granted a statutory right, but need not take it up. Hence, the particular institution may permit firms to bargain away shared governance exactly where and when it would be detrimental to efficiency. Relatedly, the share of seats to which workers have a statutory is limited to 20% – a low figure compared to, e.g., Germany where workers in firms with board-level codetermination make up between 33 and 50% of the supervisory board. As a cooperative institution with broader scope, shared governance may also differ in its effects from those of firm-level unionization in the US (see, e.g., Lee and Mas, 2012), which may represent a more adversarial institution and entail a more narrow focus on negotiation of wages and working conditions.

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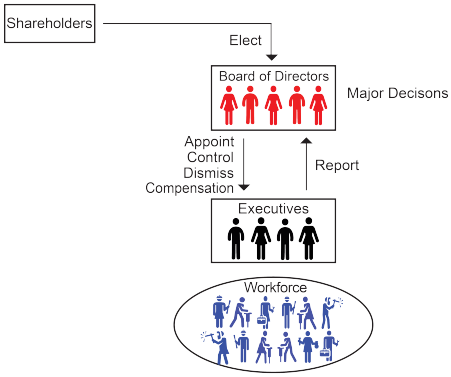
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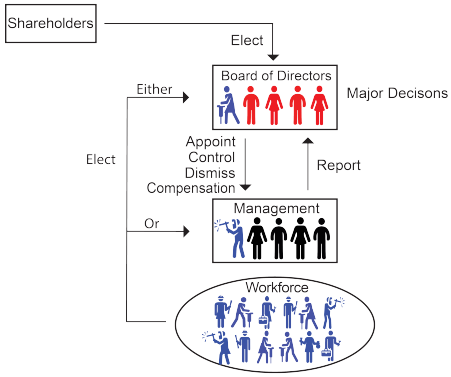
Figures

Figure 1: Corporate Governance and Worker Representation

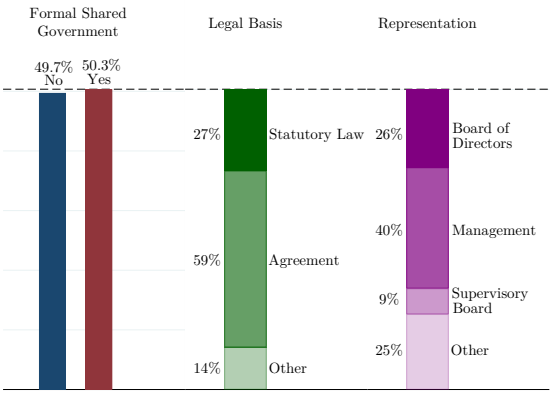
(a) Corporate Governance Without Worker Representation



(b) Corporate Governance With Statutory Worker Representation



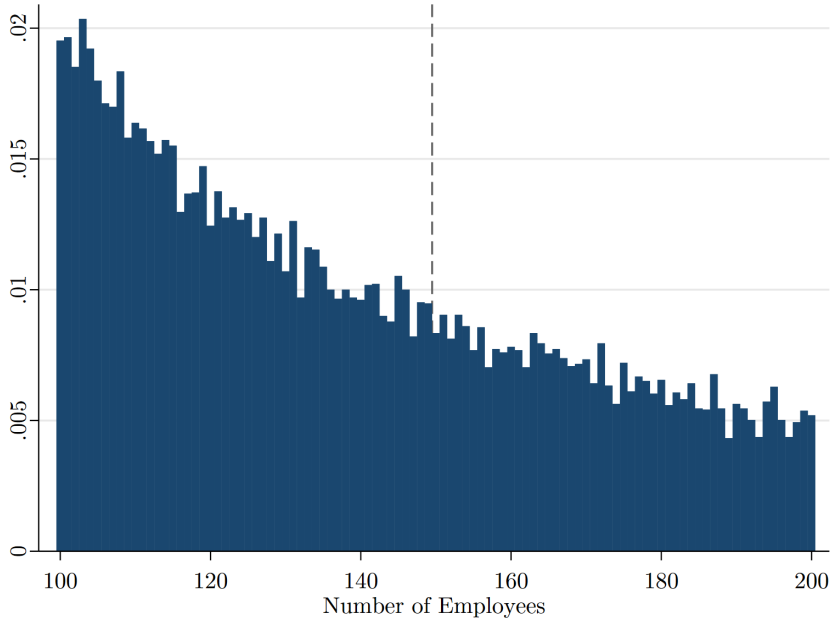
(c) Shared Governance in Firms With ≥ 150 Employees: Survey Evidence



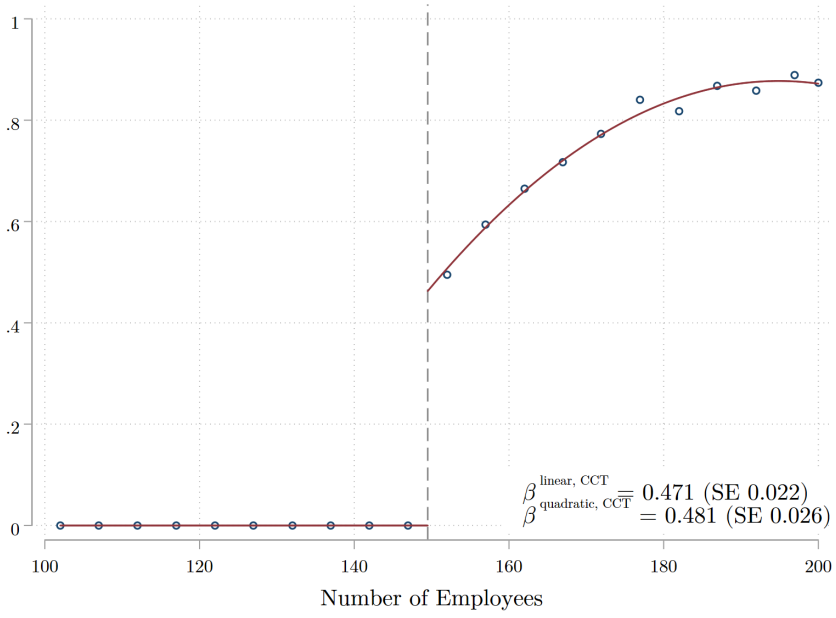
Note: The figure illustrates the governance structure of a Finnish firm with a unitary board structure. Panel (a) illustrates the governance structure without worker representation, which applied to firms before 1991 as well as to firms with fewer than 150 employees post-1991. Panel (b) illustrates the governance structure under the statutory provision of the codetermination law with 20% worker representation. We illustrate the cases where workers exercise their statutory right to elect representatives to either the board of directors or the firm's management group; under the statutory provisions, it is the firm's choice which of these bodies worker representatives are elected to. Here, we do not illustrate the less common case of a dual board structure and the option for worker representation on the supervisory board. Panel (c) reports results from a 2019 survey among union members (Teollisuuden palkansaajat, 2019). We focus on respondents in firms with at least 150 employees (the survey also had a few responses from workers in smaller firms). A small share of responses indicate that the respondent does not know the answer, in which case we omit it from our calculations. Examples of the "other" category for the legal basis include informal agreements or policies for worker representatives to have voice (but no vote) in board meetings. Examples of responses from the free-form "other" category for the body of representation include representation in two bodies, regular meetings between top management and worker representatives, and advisory boards.

Figure 2: Specification Checks for Regression Discontinuity Design

(a) Density of Firms' Employment



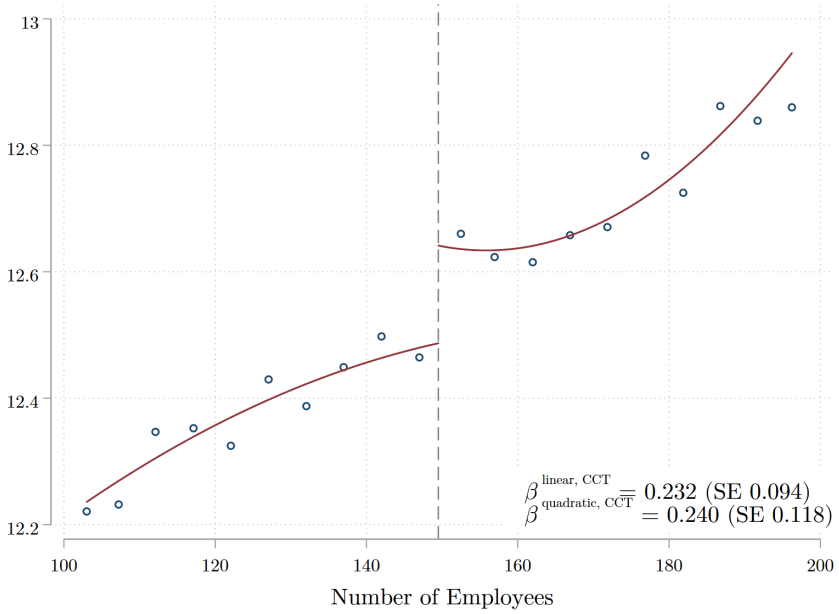
(b) Probability of Meeting Employee Threshold in Two Consecutive Years



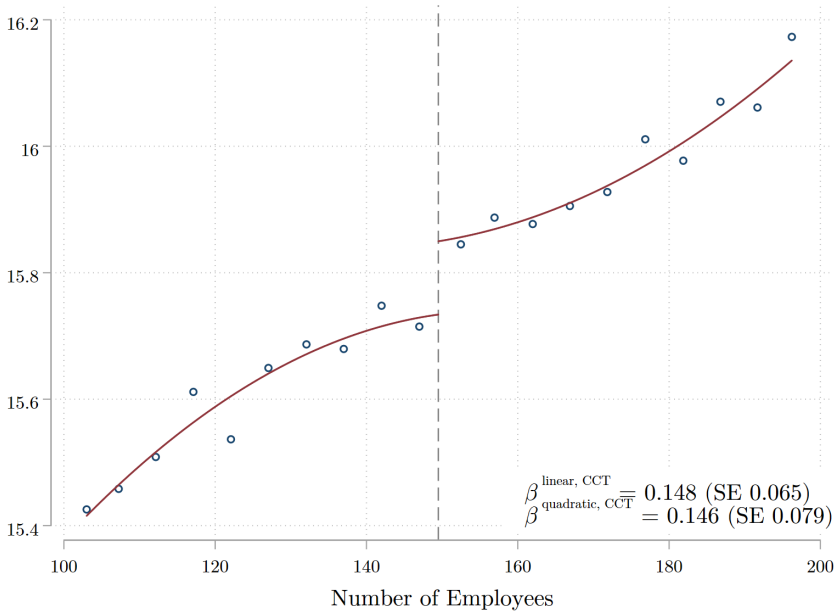
Note: Panel (a) plots the density of firms' employment around the 150 employee threshold in the post-reform period between 1992 and 2016. A McCrary (2008) test of the density at the 150 employee threshold does not reject continuity at the threshold ($p = 0.498$). Panel (b) plots an indicator function that is equal to one if a firm has crossed the 150 employee threshold in the past two years (as codetermination law binds when employment is at least 150 in two consecutive years).

Figure 3: Investment and Capital

(a) Log Investment



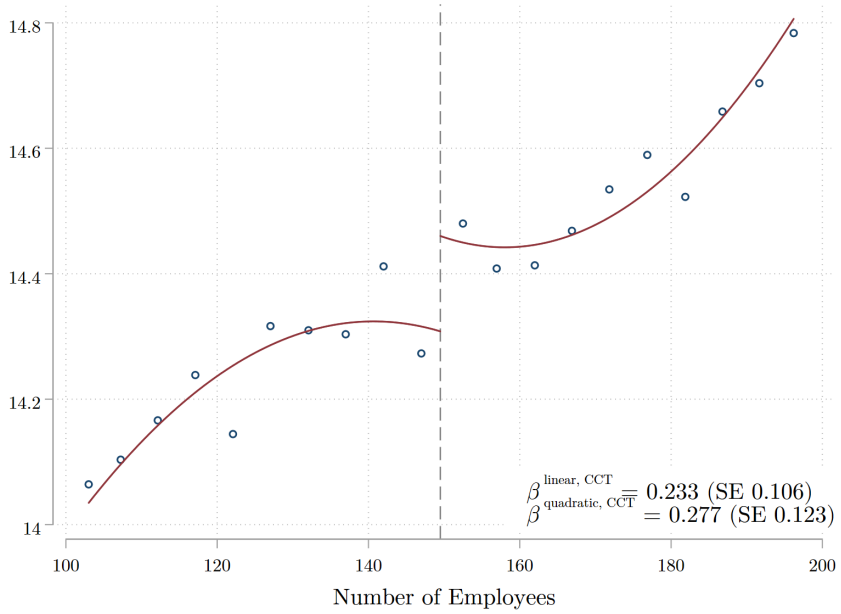
(b) Log Total Assets



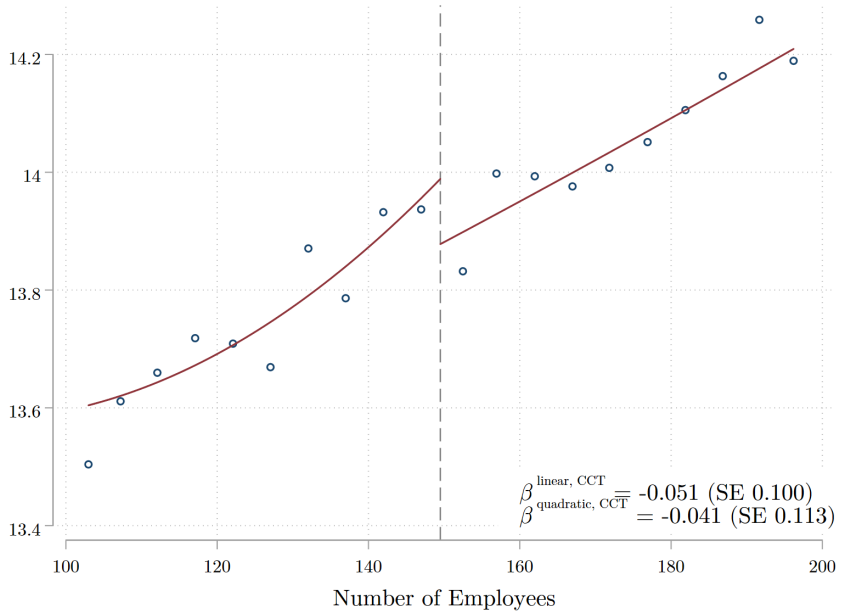
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure 4: Asset Composition: Fixed and Current Assets

(a) Log Fixed Assets



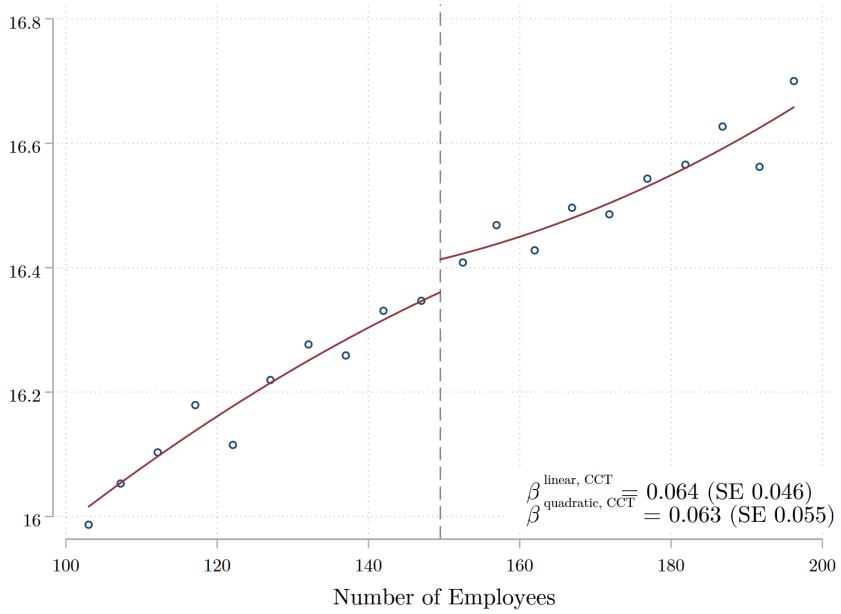
(b) Log Current Assets



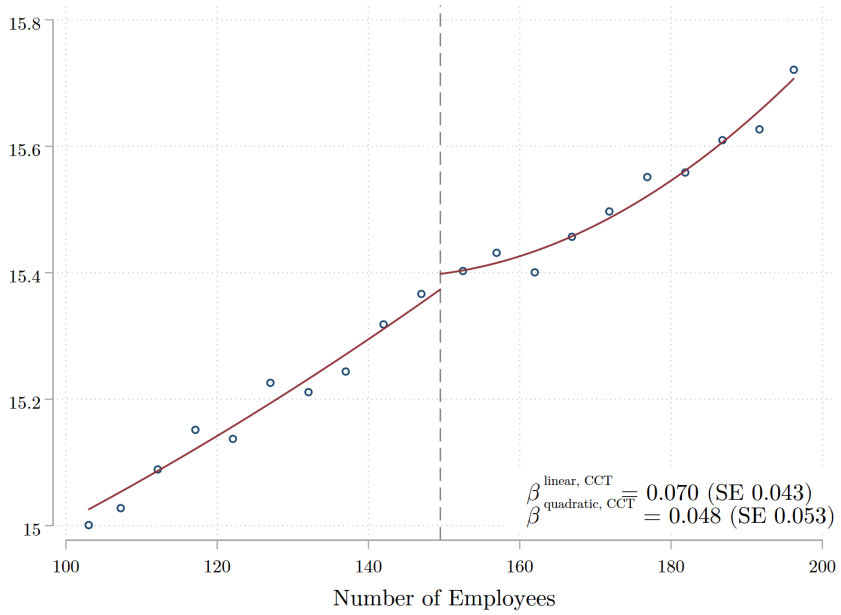
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure 5: Output

(a) Log Revenue



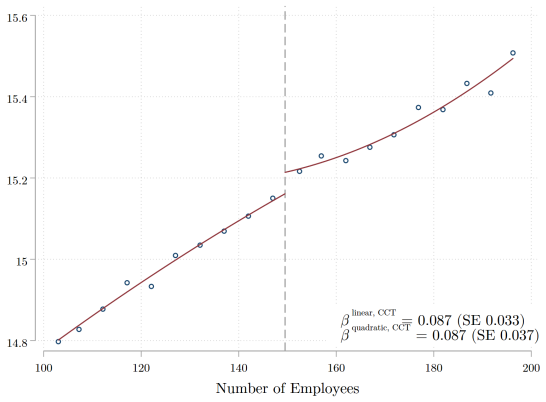
(b) Log Value Added



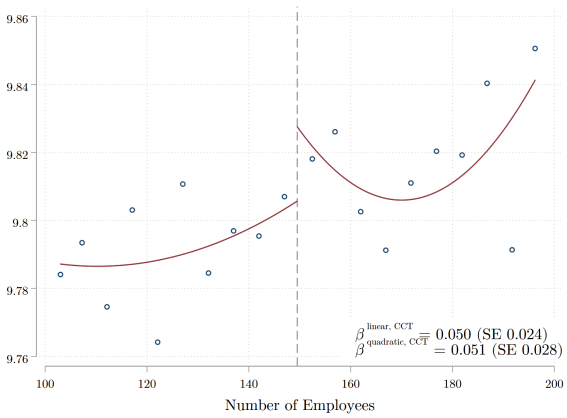
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 2.

Figure 6: Labor Income

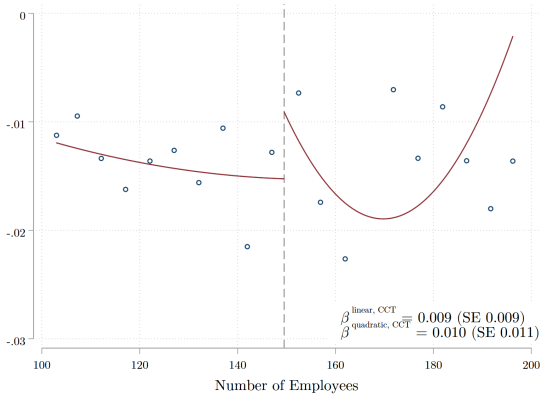
(a) Log Total Labor Costs



(b) Log Wage



(c) Firm AKM FE



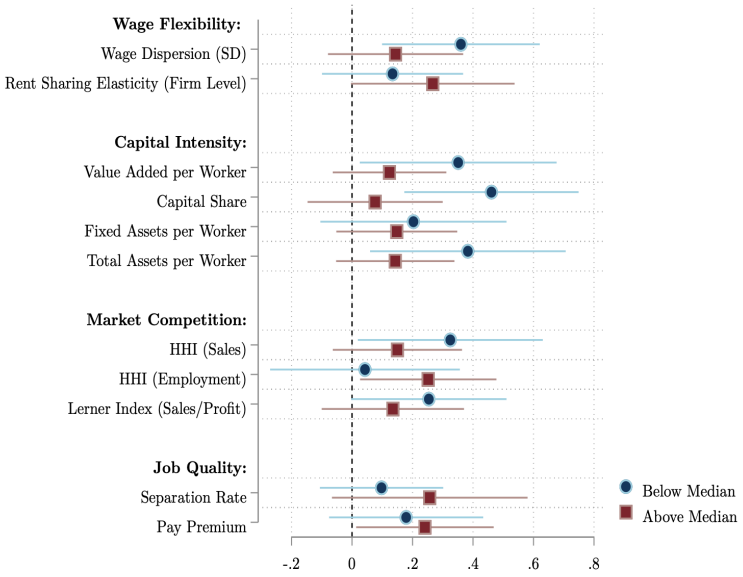
(d) Rent Sharing Elasticity



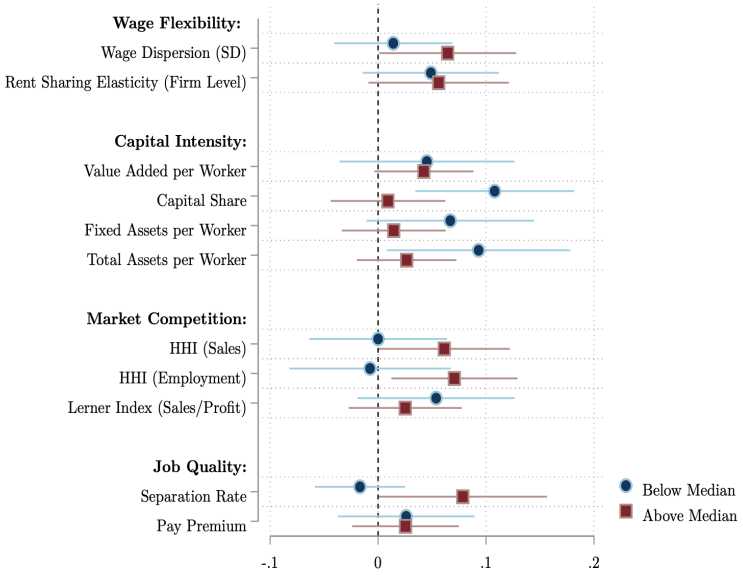
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. Panel (d) shows the relation between yearly increase in log value added per worker and average log wages for firms with and without the right of employee representation. We report the corresponding estimates in Table 3.

Figure 7: Heterogeneity

(a) Log Investments



(b) Log Wage



Note: The figure reports treatment effects of shared governance on log investments (panel (a)) and on log wages (panel (b)) for median splits of the sample according to industry characteristics. For example, the red circle in the first row of panel (a) reports the effect of shared governance on log investment in industries with an above-median (between-worker) standard deviation of wages. Industry characteristics are calculated separately for each year and based on a sample leaving out our baseline estimation sample of firms with employment between 100 and 200 employees. The specifications reported here are linear specifications with CCT bandwidth choice. We also report the results in Tables 5 and 6. We report quadratic specifications in Appendix Figure A.10 and describe our variable construction in Appendix D.

Tables

Table 1: Investment and Capital

	(1) Log Investment	(2) $\mathbb{1}$(Investment > 0)	(3) arcsinh (Inv.)	(4) Log Total Assets	(5) Log Fixed Assets	(6) Log Current Assets
Panel A: Treatment Specification (1992 - 2016)						
Linear Fit	0.232**	0.014	0.169*	0.148**	0.233**	-0.051
	(0.094)	(0.015)	(0.087)	(0.065)	(0.106)	(0.100)
Bandwidth	[111, 172]	[124, 196]	[113, 173]	[114, 175]	[118, 170]	[110, 186]
Effective Observations	12,147	12,528	12,874	13,098	10,947	11,530
Quadratic Fit	0.240**	0.014	0.218**	0.146*	0.277**	-0.041
	(0.118)	(0.017)	(0.102)	(0.079)	(0.123)	(0.113)
Bandwidth	[113, 179]	[107, 200]	[105, 179]	[109, 183]	[110, 177]	[100, 192]
Effective Observations	12,156	18,347	15,987	15,916	14,544	14,707
Control Mean	12.48	0.89	12.58	15.73	14.34	13.93
Total Observations	63,443	72,127	69,497	72,847	70,698	51,958
Panel B: Placebo Specifications (Linear Fit)						
Pre-Reform Period	—	—	—	-0.032	-0.083	-0.032
(1988 - 1991)	—	—	—	(0.137)	(0.150)	(0.188)
Placebo Discontinuity	-0.031	0.007	-0.021	0.037	0.070	0.036
(100 Employees)	(0.068)	(0.010)	(0.061)	(0.053)	(0.074)	(0.072)
Predicted Outcomes	0.040	0.001	0.028	-0.001	0.008	-0.020
(Industry Averages)	(0.044)	(0.006)	(0.049)	(0.052)	(0.064)	(0.089)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We further show results for log investment and log total assets in Figure 3 and for log fixed and current assets in Figure 4. We show additional specifications in Appendix Table A.1.

Table 2: Output and Productivity

	(1) Log Revenue	(2) Log Value Added	(3) Log Materials	(4) Log Years Tenure	(5) Log Fixed Assets per Worker	(6) Log Value Added per Worker	(7) Log Total Factor Productivity
Panel A: Treatment Specification (1992 - 2016)							
Linear Fit	0.064	0.070	0.107	0.029	0.208**	0.062	-0.030
	(0.046)	(0.043)	(0.110)	(0.034)	(0.104)	(0.043)	(0.064)
Bandwidth	[113, 182]	[115, 169]	[120, 182]	[106, 186]	[117, 171]	[118, 169]	[128, 182]
Effective Observations	14,348	11,746	9,114	18,436	11,537	10,553	9,773
Quadratic Fit	0.063	0.048	0.110	0.068	0.244**	0.043	-0.040
	(0.055)	(0.053)	(0.119)	(0.042)	(0.120)	(0.051)	(0.075)
Bandwidth	[103, 186]	[119, 178]	[98, 194]	[107, 180]	[111, 178]	[119, 179]	[120, 196]
Effective Observations	18,903	11,860	16,260	17,372	14,483	11,686	13,246
Control Mean	16.34	15.34	15.07	1.84	9.43	10.38	6.28
Total Observations	73,564	71,333	54,382	76,398	70,698	71,333	69,317
Panel B: Placebo Specifications (Linear Fit)							
Pre-Reform Period	-0.038	0.021	—	-0.111	-0.064	0.021	0.141
(1988 - 1991)	(0.094)	(0.084)	—	(0.084)	(0.142)	(0.080)	(0.118)
Placebo Discontinuity	0.054	0.045	0.006	0.046	0.061	0.041	-0.030
(100 Employees)	(0.044)	(0.035)	(0.079)	(0.029)	(0.073)	(0.033)	(0.048)
Predicted Outcomes	0.008	0.006	-0.001	-0.013	0.003	0.002	-0.013
(Industry Averages)	(0.040)	(0.026)	(0.079)	(0.024)	(0.063)	(0.025)	(0.050)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We further show results for log revenue and log total factor productivity in Figure 5. We show additional specifications in Appendix Table A.3.

Table 3: Labor Income

	(1) Log Total Labor Costs	(2) Log Wage	(3) Log Wage + Control	(4) Log Wage + Control + FE	(5) Log p75/p25	(6) Log Executive Wages	(7) Log Share Executive Wages
Panel A: Treatment Specification (1992 - 2016)							
Linear Fit	0.087***	0.055*	0.014	0.002	-0.036*	-0.056	-0.105**
	(0.033)	(0.030)	(0.024)	(0.005)	(0.022)	(0.052)	(0.048)
Bandwidth	[113, 172]	[103, 176]	[137, 169]	[138, 171]	[110, 172]	[110, 186]	[105, 177]
Effective Observations	13,281	2,713,152	1,071,938	1,032,480	14,850	8,828	9,249
Quadratic Fit	0.087**	0.058	0.007	0.002	-0.035	-0.001	-0.046
	(0.037)	(0.036)	(0.030)	(0.006)	(0.025)	(0.071)	(0.061)
Bandwidth	[102, 180]	[103, 183]	[131, 171]	[133, 174]	[102, 182]	[119, 188]	[121, 191]
Effective Observations	18,758	2,967,000	1,395,272	1,258,899	19,349	7,335	7,385
Control Mean	15.13	—	—	—	0.78	11.31	-3.87
Total Observations	75,679	8,462,220	3,564,113	3,193,160	76,398	39,919	39,919
Panel B: Placebo Specifications (Linear Fit)							
Pre-Reform Period	0.005	-0.061	-0.044	0.026*	0.050	—	—
(1988 - 1991)	(0.058)	(0.054)	(0.039)	(0.014)	(0.040)	—	—
Placebo Discontinuity	0.016	—	—	—	-0.027	0.104**	0.097**
(100 Employees)	(0.027)	—	—	—	(0.017)	(0.047)	(0.043)
Predicted Outcomes	0.001	—	—	—	0.009	-0.017	-0.008
(Industry Averages)	(0.022)	—	—	—	(0.012)	(0.015)	(0.015)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We further show results for log total labor costs, log wage, firm AKM fixed effects and rent sharing elasticity in Figure 6. We show additional specifications in Appendix Table A.7.

Table 4: Capital Income

	(1) Capital Share	(2) Log Capital Income	(3) Net Income over Equity	(4) Net Income over Revenue	(5) Log Dividends	(6) $\mathbb{1}$(Dividends > 0)	(7) arcsinh (Dividends)	(8) Dividends over Revenue	(9) Payout ratio	(10) Interest Payments over Revenue
Panel A: Treatment Specification (1992 - 2016)										
Linear Fit	0.002	0.129	-0.020	-0.006	0.159	0.041	0.181*	0.004*	-0.039	0.001
	(0.010)	(0.079)	(0.168)	(0.007)	(0.116)	(0.027)	(0.106)	(0.002)	(0.042)	(0.001)
Bandwidth	[108, 181]	[112, 179]	[126, 176]	[114, 183]	[108, 178]	[112, 174]	[118, 176]	[106, 173]	[114, 178]	[115, 185]
Effective Observations	15,909	11,720	9,712	14,064	5,762	13,710	5,804	14,504	12,951	14,091
Quadratic Fit	-0.004	0.024	-0.029	-0.017*	0.109	0.029	0.207*	0.004	-0.051	0.001
	(0.012)	(0.103)	(0.189)	(0.009)	(0.140)	(0.030)	(0.113)	(0.002)	(0.051)	(0.001)
Bandwidth	[119, 199]	[123, 183]	[114, 194]	[112, 179]	[112, 194]	[104, 194]	[103, 196]	[99, 189]	[104, 185]	[99, 196]
Effective Observations	14,429	9,307	15,836	14,003	5,846	19,217	9,873	19,530	17,299	21,417
Control Mean	0.207	13.64	0.30	0.015	12.59	0.38	12.12	0.014	—	0.012
Total Observations	72,537	59,571	72,766	71,447	28,289	72,127	37,539	67,857	68,390	71,447
Panel B: Placebo Specifications (Linear Fit)										
Pre-Reform Period	-0.014	-0.160	-0.710	-0.023	—	—	—	—	—	0.008
(1988 - 1991)	(0.027)	(0.198)	(0.555)	(0.017)	—	—	—	—	—	(0.005)
Placebo Discontinuity	0.008	0.057	-0.017	0.023***	0.036	-0.013	0.043	-0.001	0.000	-0.002
(100 Employees)	(0.008)	(0.068)	(0.094)	(0.007)	(0.095)	(0.020)	(0.082)	(0.002)	(0.032)	(0.001)
Predicted Outcomes	0.003	-0.000	0.008	0.001	0.009	0.002	-0.006	0.000	0.004	-0.000
(Industry Averages)	(0.004)	(0.045)	(0.019)	(0.001)	(0.045)	(0.005)	(0.042)	(0.001)	(0.005)	(0.001)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We show additional specifications in Appendix Table A.8.

Table 5: Log Investment

Panel A: Wage Rigidity and Capital Intensity

	(1)		(2)		(3)		(4)		(5)		(6)	
	Wage Dispersion (SD)		Rent Sharing Elasticity (Firm Level)		Value Added per Worker		Capital Share		Fixed Assets per Worker		Total Assets per Worker	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.360***	0.144	0.134	0.267*	0.351**	0.124	0.461***	0.076	0.203	0.148	0.383**	0.143
	(0.133)	(0.114)	(0.119)	(0.138)	(0.166)	(0.096)	(0.147)	(0.114)	(0.157)	(0.102)	(0.165)	(0.100)
Bandwidth	[115, 169]	[107, 181]	[113, 177]	[118, 174]	[108, 169]	[109, 193]	[119, 166]	[117, 193]	[109, 172]	[113, 180]	[103, 168]	[111, 180]
Effective Observations	3,643	9,393	6,496	4,724	4,750	9,302	4,437	6,383	4,521	8,128	5,027	8,560
Quadratic Fit	0.295*	0.166	0.130	0.316**	0.291	0.144	0.535***	-0.053	0.226	0.118	0.463**	0.116
	(0.172)	(0.139)	(0.149)	(0.157)	(0.205)	(0.121)	(0.161)	(0.151)	(0.183)	(0.124)	(0.193)	(0.124)
Bandwidth	[117, 180]	[98, 183]	[114, 185]	[105, 182]	[110, 179]	[108, 195]	[96, 174]	[119, 188]	[102, 180]	[115, 198]	[94, 176]	[114, 197]
Effective Observations	3,849	11,779	6,771	6,907	4,957	9,909	8,886	5,926	5,893	8,958	6,477	9,147
Total Observations	21,053	42,390	35,572	27,871	25,335	38,108	32,271	31,172	24,458	38,985	23,138	40,305
Control Mean	12.81	12.24	12.46	12.41	11.65	12.88	11.79	13.06	11.58	12.93	11.71	12.82

Panel B: Market Competition and Job Quality

	(1)		(2)		(3)		(4)		(5)	
	HHI (Sales)		HHI (Employment)		Lerner Index ($\frac{\text{Profits}}{\text{Sales}}$)		Separation Rate		Pay Premia	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.325**	0.150	0.043	0.252**	0.254*	0.135	0.098	0.257	0.179	0.241**
	(0.156)	(0.109)	(0.160)	(0.115)	(0.131)	(0.120)	(0.104)	(0.165)	(0.130)	(0.116)
Bandwidth	[105, 170]	[111, 183]	[113, 183]	[117, 173]	[114, 171]	[104, 185]	[112, 182]	[116, 175]	[108, 173]	[117, 183]
Effective Observations	4,275	9,377	3,404	7,779	5,465	8,097	7,642	4,478	6,699	5,614
Quadratic Fit	0.183	0.212	-0.064	0.319**	0.277	0.137	0.064	0.352*	0.179	0.246*
	(0.194)	(0.136)	(0.199)	(0.136)	(0.173)	(0.138)	(0.135)	(0.183)	(0.171)	(0.126)
Bandwidth	[116, 183]	[106, 182]	[116, 187]	[108, 179]	[118, 175]	[95, 198]	[118, 185]	[96, 182]	[115, 180]	[96, 197]
Effective Observations	3,603	10,163	3,353	10,015	5,193	10,553	7,067	7,609	5,987	9,944
Total Observations	21,443	42,000	20,278	43,165	31,624	31,819	37,068	26,375	33,268	30,175
Control Mean	12.26	12.52	12.43	12.44	12.13	12.75	12.76	11.91	12.12	12.76

Note: The table reports treatment effects of shared governance on log investment for median splits of the sample according to industry characteristics. For example, column (1) reports the effect of shared governance on log investment in industries with an above- or below-median (between-worker) standard deviation of wages. Industry characteristics are calculated separately for each year and based on a sample leaving out our baseline estimation sample of firms with employment between 100 and 200 employees. The specifications reported here are linear and quadratic specifications with CCT bandwidth choice. We also report the results in Figure 7 and A.10 and describe our variable construction in Appendix D.

Table 6: Log Wage

Panel A: Wage Rigidity and Capital Intensity

	(1)		(2)		(3)		(4)		(5)		(6)	
	Wage Dispersion (SD)		Rent Sharing Elasticity (Firm Level)		Value Added per Worker		Capital Share		Fixed Assets per Worker		Total Assets per Worker	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.014	0.065**	0.049	0.056*	0.045	0.042*	0.108***	0.009	0.067*	0.015	0.093**	0.026
	(0.028)	(0.032)	(0.032)	(0.033)	(0.041)	(0.023)	(0.037)	(0.027)	(0.040)	(0.025)	(0.043)	(0.024)
Bandwidth	[124, 185]	[114, 173]	[114, 171]	[124, 189]	[112, 176]	[121, 180]	[104, 168]	[116, 190]	[105, 175]	[113, 188]	[115, 172]	[109, 185]
Effective Observations	3,873	9,246	7,117	5,877	6,036	7,353	8,490	7,123	6,697	9,544	4,930	10,449
Quadratic Fit	0.008	0.060	0.044	0.070*	0.026	0.041	0.103**	0.016	0.066	0.021	0.112**	0.015
	(0.030)	(0.037)	(0.039)	(0.039)	(0.050)	(0.026)	(0.045)	(0.033)	(0.046)	(0.030)	(0.049)	(0.030)
Bandwidth	[100, 193]	[100, 181]	[110, 180]	[108, 188]	[109, 183]	[99, 188]	[109, 180]	[113, 193]	[102, 183]	[106, 191]	[108, 181]	[116, 190]
Effective Observations	6,932	13,606	8,404	8,227	6,955	12,693	8,402	7,798	7,676	11,606	6,405	9,419
Total Observations	24,064	52,334	41,911	34,487	32,846	43,552	40,214	36,184	31,865	44,533	30,425	45,973
Control Mean	10.004	9.71	9.73	9.89	9.51	10.00	9.70	9.92	9.57	9.96	9.52	9.98

Panel B: Market Competition and Job Quality

	(1)		(2)		(3)		(4)		(5)	
	HHI (Sales)		HHI (Employment)		Lerner Index ($\frac{\text{Profits}}{\text{Sales}}$)		Separation Rate		Pay Premia	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.000	0.061**	-0.007	0.071**	0.054	0.025	-0.017	0.079**	0.026	0.025
	(0.033)	(0.031)	(0.038)	(0.030)	(0.037)	(0.027)	(0.021)	(0.040)	(0.032)	(0.025)
Bandwidth	[103, 194]	[109, 173]	[100, 185]	[108, 170]	[113, 174]	[106, 187]	[105, 195]	[102, 178]	[102, 182]	[113, 195]
Effective Observations	6,546	10,210	5,677	10,976	6,987	9,182	11,344	8,078	10,206	7,983
Quadratic Fit	0.008	0.066*	0.001	0.070**	0.041	0.059*	-0.002	0.085*	0.032	0.050
	(0.039)	(0.035)	(0.046)	(0.035)	(0.044)	(0.036)	(0.028)	(0.045)	(0.038)	(0.032)
Bandwidth	[95, 186]	[96, 183]	[108, 199]	[107, 181]	[108, 182]	[114, 185]	[98, 183]	[95, 186]	[95, 191]	[106, 184]
Effective Observations	7,565	15,470	4,886	12,559	8,353	7,744	12,228	10,236	12,920	8,572
Total Observations	26,466	49,932	24,104	52,294	38,516	37,882	42,348	34,050	41,400	34,998
Control Mean	9.837	9.79	9.875	9.78	9.75	9.86	10.015	9.51	9.65	9.98

Note: The table reports treatment effects of shared governance on log wages for median splits of the sample according to industry characteristics. For example, column (1) reports the effect of shared governance on log investment in industries with an above- or below-median (between-worker) standard deviation of wages. Industry characteristics are calculated separately for each year and based on a sample leaving out our baseline estimation sample of firms with employment between 100 and 200 employees. The specifications reported here are linear and quadratic specifications with CCT bandwidth choice. We also report the results in Figure 7 and A.10 and describe our variable construction in Appendix D.

Online Appendix of:
Worker Voice and Shared Governance:
Evidence from Finnish Firms

Jarkko Harju, Simon Jäger, and Benjamin Schoefer

Contents

A Appendix Figures **50**

B Appendix Tables **60**

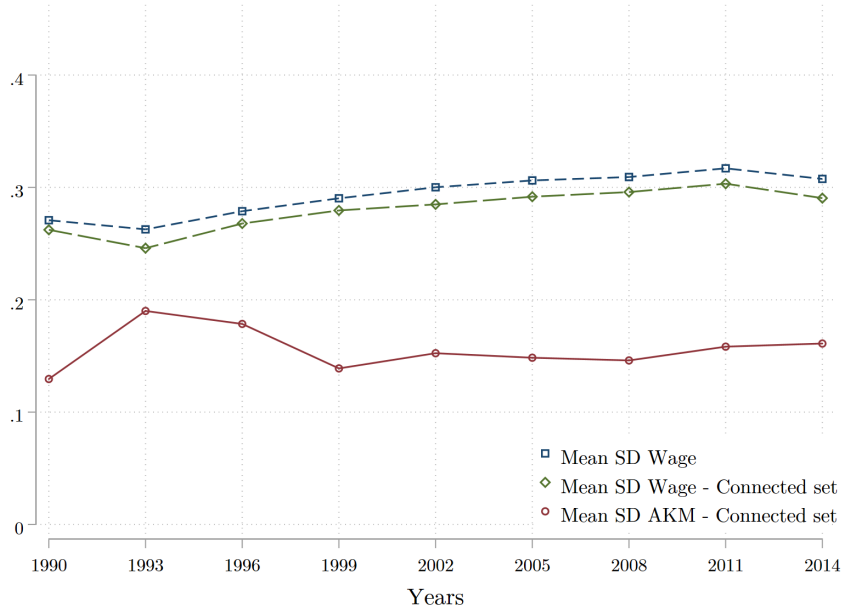
C Qualitative and Survey Evidence on Shared Governance in Finland **70**

C.1 Qualitative Evidence from Survey of Board Representative in Gold, Kluge, and Conchon (2010) 70

D Construction of Variables for Heterogeneity Analysis **72**

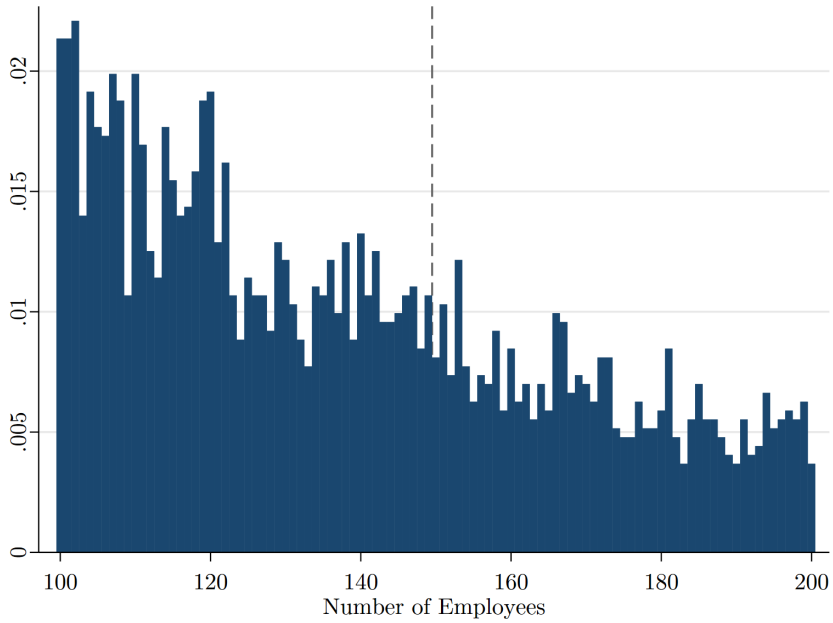
A Appendix Figures

Figure A.1: 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.2: Density of Firms' Employment (Pre-Reform Period)
(1988 to 1990, no policy discontinuity)



Note: The figures plot the density of firms' employment in the pre-reform period between 1988 and 1990, when no policy discontinuity at 150 existed. A McCrary (2008) test of the density at the 150 employee threshold does not reject continuity at the threshold ($p = 0.948$).

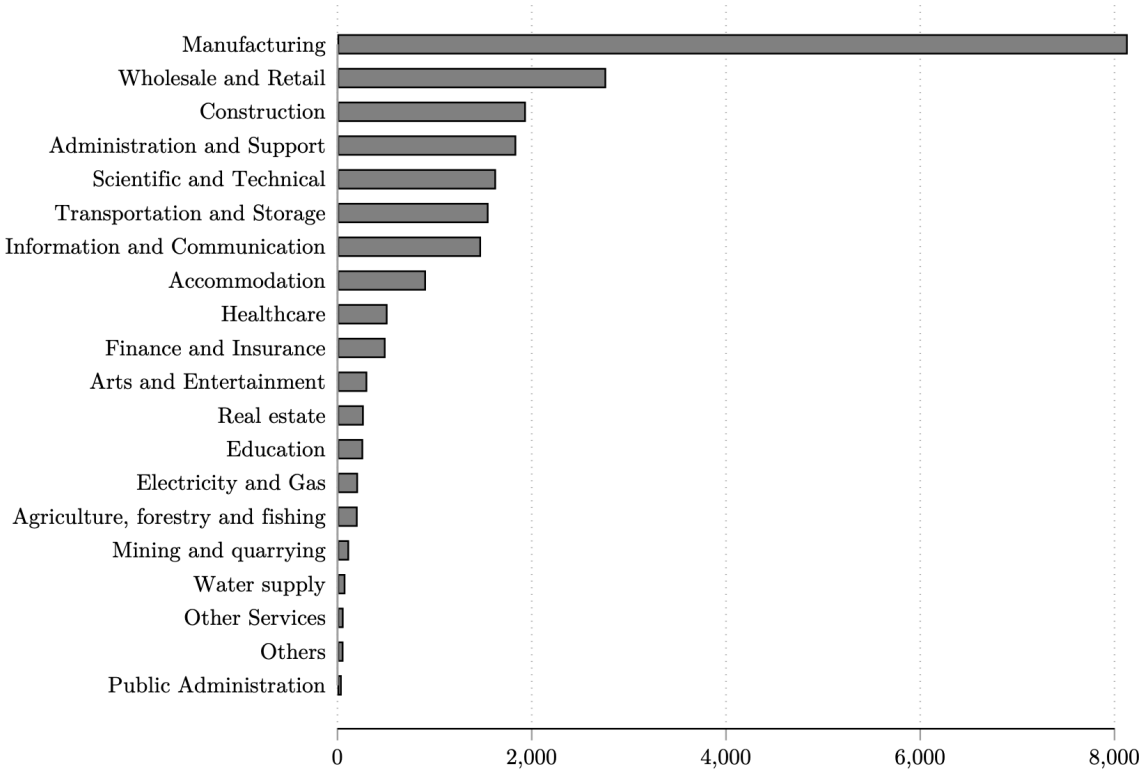
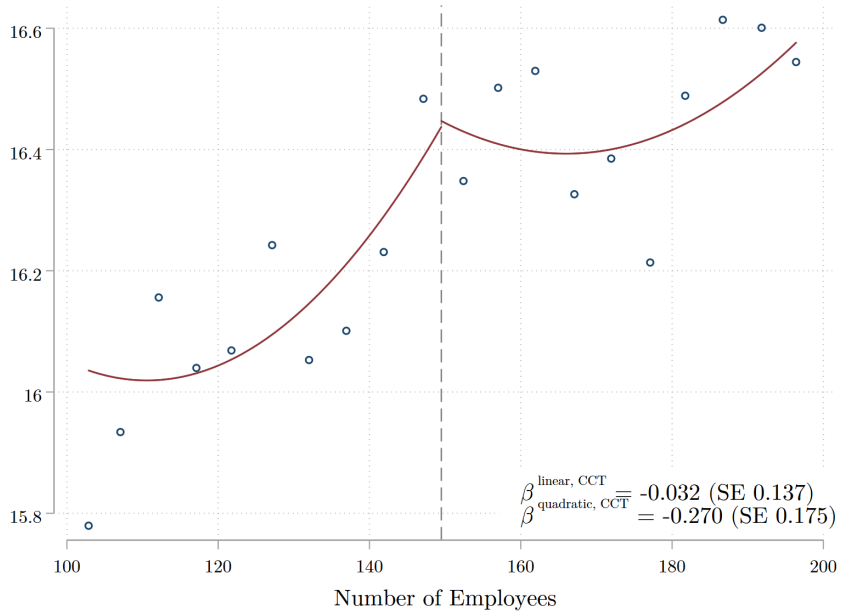


Figure A.3: Industry Composition

Note: The figure displays the number of firms by industry (Standard Industrial Classification TOL 2008) for firms with 100-200 employees in the post-reform period (1991-2016).

Figure A.4: Pre-Reform Period - Capital Income

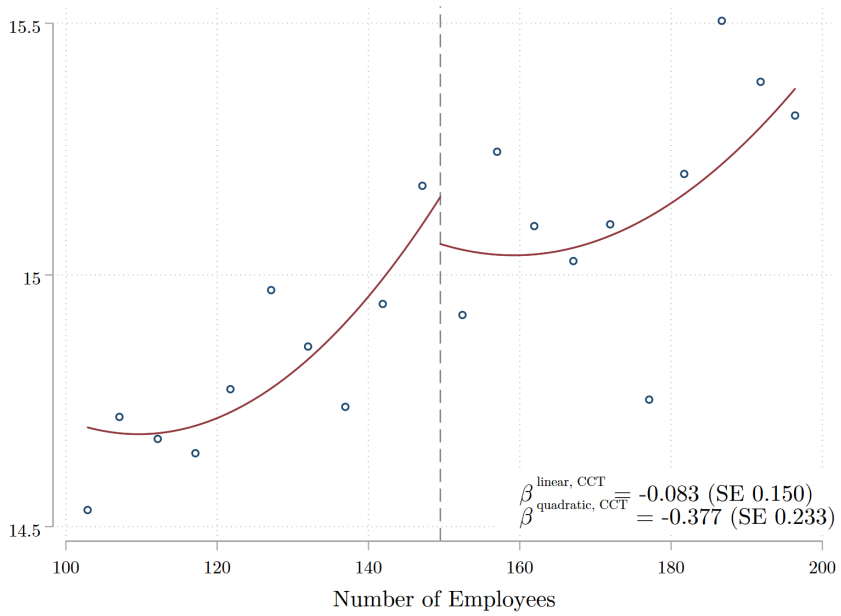
(a) Log Total Assets



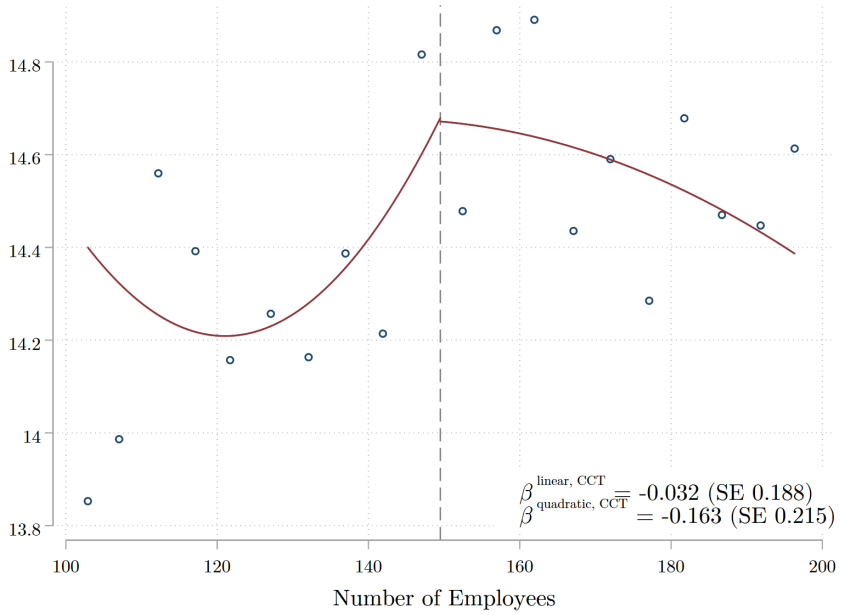
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure A.5: Pre-Reform Period - Capital Income

(a) Log Fixed Assets



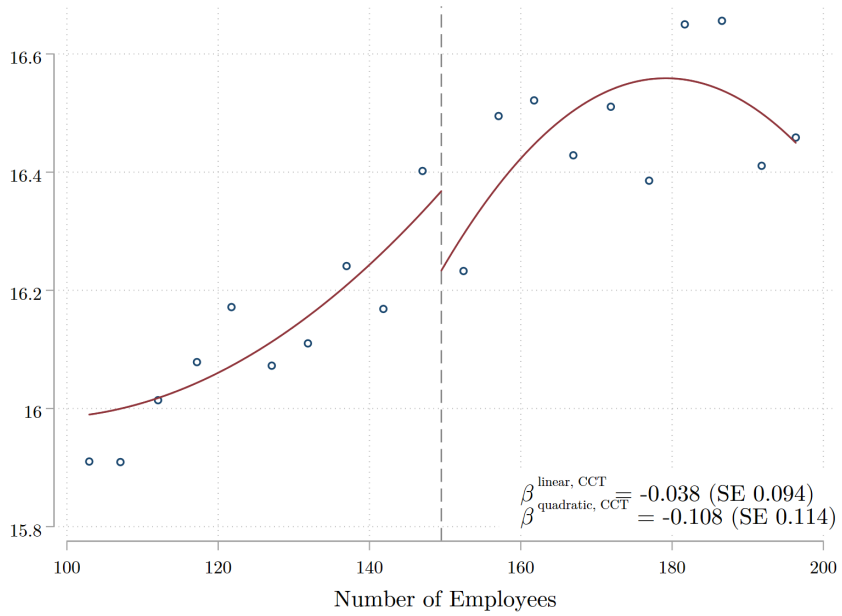
(b) Log Current Assets



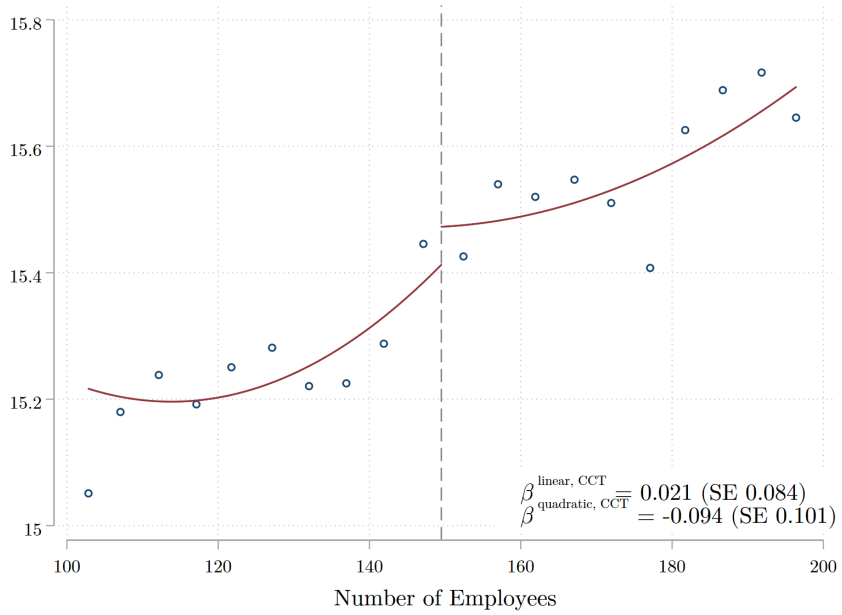
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 1.

Figure A.6: Pre-Reform Period - Output

(a) Log Revenue



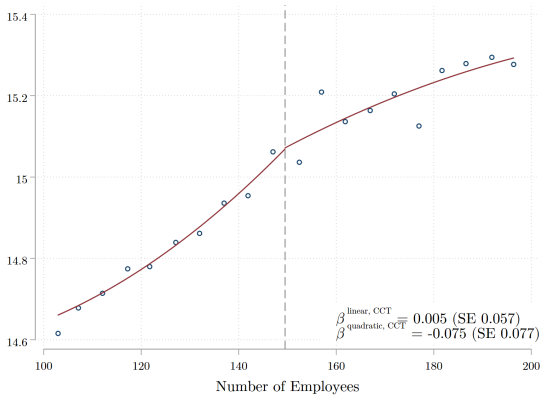
(b) Log Value Added



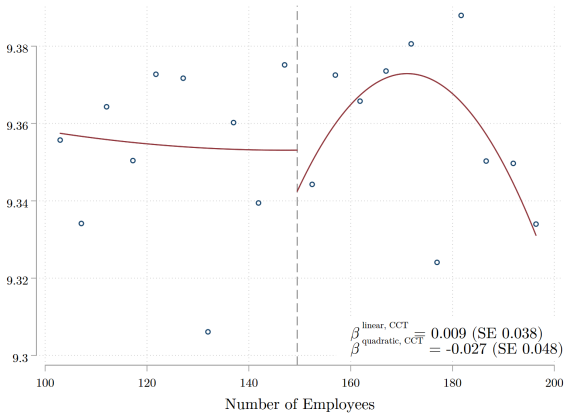
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 2.

Figure A.7: Pre-Reform Period - Output

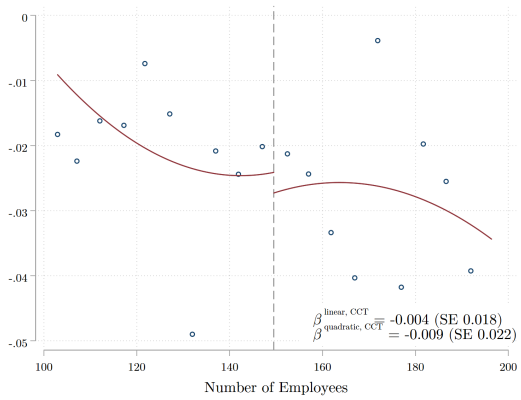
(a) Log Total Labor Costs



(b) Log Wage



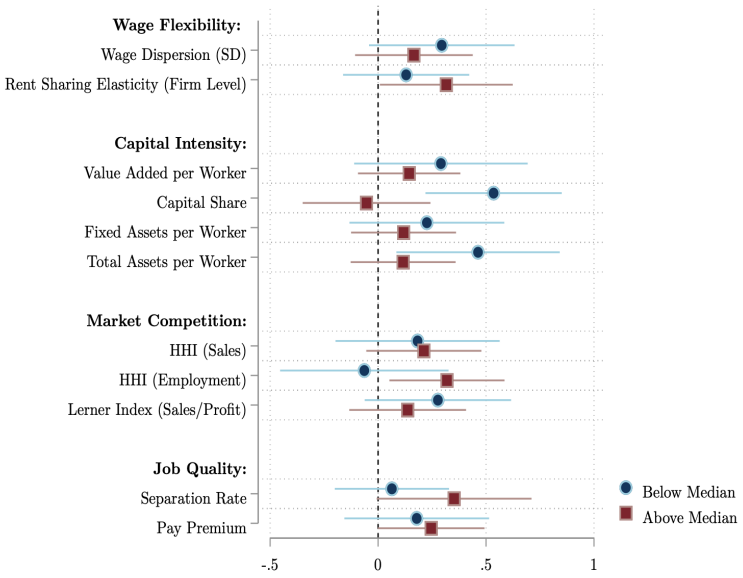
(c) Firm AKM FE



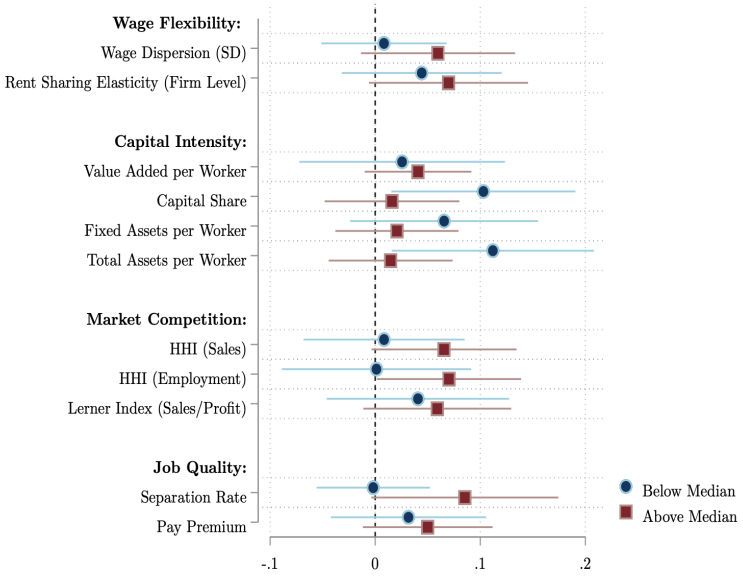
Note: The figures report binned scatter plot of the respective outcome variable plotted against employment. Firms with employment equal to or higher than 150 employees fall under the shared governance legislation giving workers a statutory right for participation in governance. The red lines report the predicted outcomes based on a quadratic specification. We report the corresponding estimates in Table 3.

Figure A.8: Heterogeneity - Quadratic Fit

(a) Log Investments



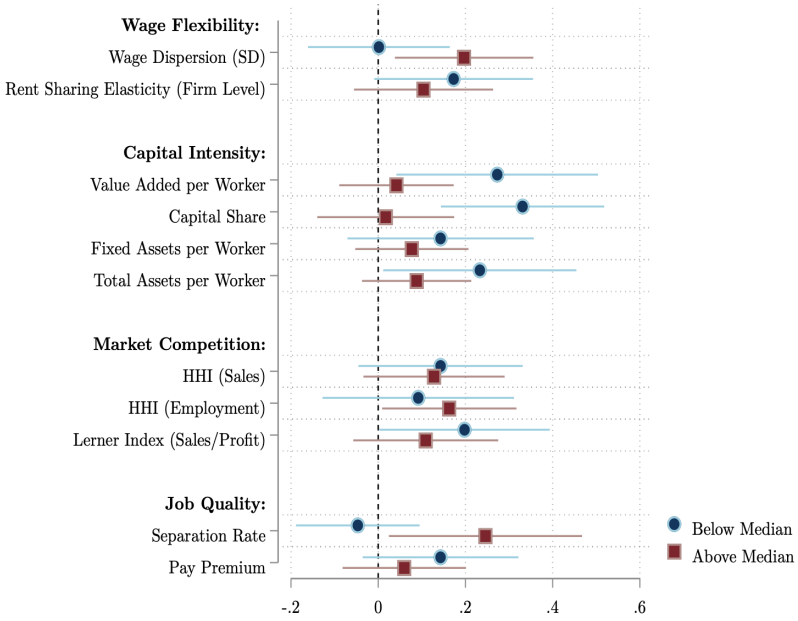
(b) Log Wage



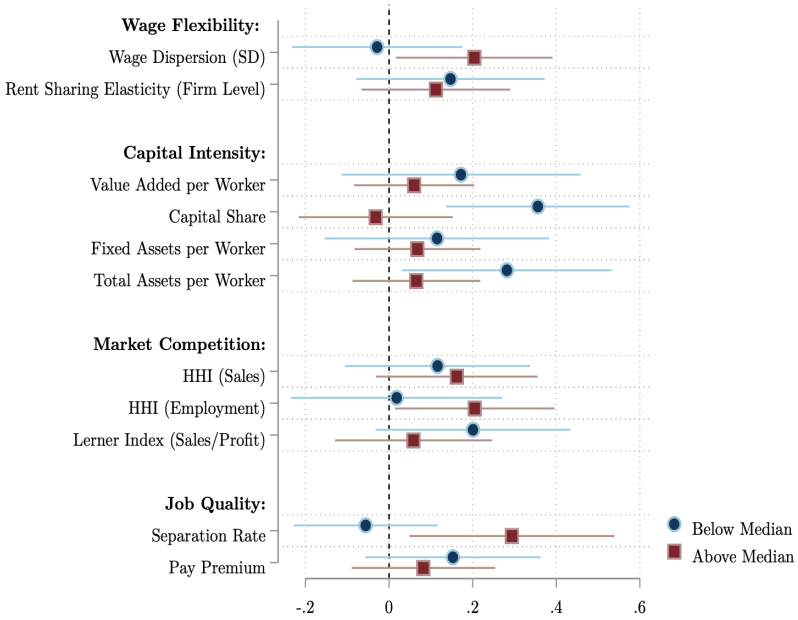
Note: The figure reports treatment effects of shared governance on log investments (panel (a)) and on log wages (panel (b)) for median splits of the sample according to industry characteristics. For example, the red circle in the first row of panel (a) reports the effect of shared governance on log investment in industries with an above-median (between-worker) standard deviation of wages. Industry characteristics are calculated separately for each year and based on a sample leaving out our baseline estimation sample of firms with employment between 100 and 200 employees. The specifications reported here are linear specifications with CCT bandwidth choice. We also report the results in Tables 5 and 6. We report linear specifications in Figure 7 and describe our variable construction in Appendix D.

Figure A.9: Heterogeneity - Log Total Assets

(a) Linear Fit



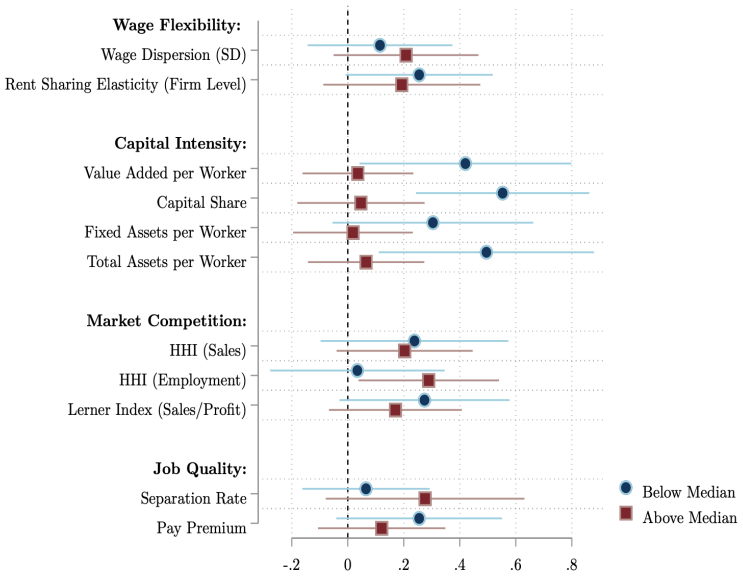
(b) Quadratic Fit



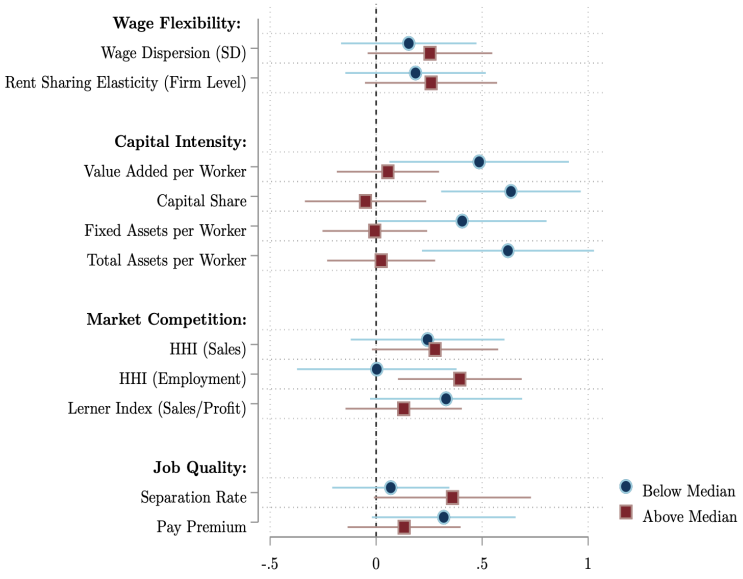
Note: The figure reports treatment effects of shared governance on log total assets for median splits of the sample according to industry characteristics. For example, the red circle in the first row of panel (a) reports the effect of shared governance on log investment in industries with an above-median (between-worker) standard deviation of wages. Industry characteristics are calculated separately for each year and based on a sample leaving out our baseline estimation sample of firms with employment between 100 and 200 employees. The specifications reported here are linear specifications with CCT bandwidth choice. We also report the results in Appendix Table A.9 and describe our variable construction in Appendix D.

Figure A.10: Heterogeneity - Log Fixed Assets

(a) Linear Fit



(b) Quadratic Fit



Note: The figure reports treatment effects of shared governance on log total assets for median splits of the sample according to industry characteristics. For example, the red circle in the first row of panel (a) reports the effect of shared governance on log investment in industries with an above-median (between-worker) standard deviation of wages. Industry characteristics are calculated separately for each year and based on a sample leaving out our baseline estimation sample of firms with employment between 100 and 200 employees. The specifications reported here are linear specifications with CCT bandwidth choice. We also report the results in Appendix Table A.10 and describe our variable construction in Appendix D.

B Appendix Tables

Table A.1: Investment and Capital Robustness

	(1) Log Investment	(2) $\mathbb{1}$(Investment > 0)	(3) arcsinh (Inv.)	(4) Log Total Assets	(5) Log Fixed Assets	(6) Log Current Assets
Fixed Bandwidth at 50 (Linear)	0.186**	0.009	0.147*	0.129*	0.165	-0.049
	(0.094)	(0.015)	(0.085)	(0.066)	(0.101)	(0.100)
Fixed Bandwidth at 50 (Quadratic)	0.207*	0.017	0.177*	0.144*	0.228*	-0.029
	(0.114)	(0.019)	(0.102)	(0.079)	(0.121)	(0.118)
No Controls	0.241**	0.020	0.183*	0.129	0.208	-0.114
	(0.103)	(0.017)	(0.102)	(0.082)	(0.127)	(0.138)
Industry-Year Fixed Effects	—	—	—	—	—	—
	—	—	—	—	—	—

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 1.

Table A.2: Investment Decomposition

	(1) Share Machinery and Equipment	(2) Share Buildings and Structure	(3) Share Software
Panel A: Treatment Specification (1992 - 2016)			
Linear Fit	-0.004	-0.006	0.015
	(0.017)	(0.009)	(0.017)
Bandwidth	[115, 177]	[115, 188]	[122, 174]
Effective Observations	11,746	12,721	9,492
Quadratic Fit	-0.013	-0.009	0.021
	(0.020)	(0.012)	(0.019)
Bandwidth	[98, 182]	[113, 193]	[105, 181]
Effective Observations	18,029	13,982	14,930
Control Mean	0.726	0.075	0.20
Total Observations	63,543	63,543	63,543
Panel B: Placebo Specifications (Linear Fit)			
Pre-Reform Period	—	—	—
(1988 - 1991)	—	—	—
Placebo Discontinuity	-0.005	0.008	-0.002
(100 Employees)	(0.013)	(0.008)	(0.012)
Predicted Outcomes	0.001	-0.002	0.001
(Industry Averages)	(0.004)	(0.003)	(0.004)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects. We report main estimates in Table 1.

Table A.3: Output and Productivity Robustness

	(1) Log Revenue	(2) Log Value Added	(3) Log Materials	(4) Log Years Tenure	(5) Log Fixed Assets per Worker	(6) Log Value Added per Worker	(7) Log Total Factor Productivity
Fixed Bandwidth at 50 (Linear)	0.058	0.043	0.107	0.034	0.158	0.044	0.021
	(0.048)	(0.041)	(0.106)	(0.035)	(0.099)	(0.040)	(0.059)
Fixed Bandwidth at 50 (Quadratic)	0.064	0.041	0.127	0.058	0.210*	0.039	-0.049
	(0.058)	(0.049)	(0.130)	(0.043)	(0.118)	(0.048)	(0.072)
No Controls	0.057	0.054	0.091	0.025	0.186	0.049	-0.088
	(0.060)	(0.051)	(0.145)	(0.042)	(0.124)	(0.051)	(0.083)
Industry-Year Fixed Effects	—	—	—	—	—	—	—
	—	—	—	—	—	—	—

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 2.

Table A.4: Separation Rates and Tenure

	(1) Separation Rate	(2) Log Avg. Tenure	(3) Log Avg. Tenure (1992-2004)	(4) Log Avg. Tenure (2005-2016)
Panel A: Treatment Specification (1992 - 2016)				
Linear Fit	-0.044	0.029	0.042	0.038
	(0.035)	(0.034)	(0.049)	(0.047)
Bandwidth	[119, 171]	[106, 186]	[118, 185]	[112, 179]
Effective Observations	11,233	18,436	6,476	8,053
Quadratic Fit	-0.066	0.068	0.039	0.105*
	(0.050)	(0.042)	(0.057)	(0.059)
Bandwidth	[110, 184]	[107, 180]	[108, 193]	[113, 176]
Effective Observations	15,847	17,372	8,779	7,537
Control Mean	0.24	7.74	7.77	7.72
Total Observations	74,077	76,398	35,208	41,190
Panel B: Placebo Specifications (Linear Fit)				
Pre-Reform Period	0.059	-0.111	—	—
(1988 - 1991)	(0.040)	(0.084)	—	—
Placebo Discontinuity	-0.006	0.046	0.026	0.065*
(100 Employees)	(0.015)	(0.029)	(0.038)	(0.038)
Predicted Outcomes	0.002	-0.013	-0.012	0.005
(Industry Averages)	(0.007)	(0.024)	(0.021)	(0.024)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects.

Table A.5: Workforce Composition - Education

	(1) Share Missing Educ.	(2) Share Secondary	(3) Share Post-Secondary	(4) Share Bachelor	(5) Share Master/PhD
Panel A: Treatment Specification (1992 - 2016)					
Linear Fit	0.009	-0.011	0.004	-0.000	0.003
	(0.006)	(0.010)	(0.005)	(0.006)	(0.008)
Bandwidth	[126, 187]	[122, 186]	[120, 196]	[116, 182]	[119, 175]
Effective Observations	7,779	8,616	9,760	9,375	7,732
Quadratic Fit	0.010	-0.011	0.004	-0.001	-0.002
	(0.007)	(0.011)	(0.005)	(0.007)	(0.009)
Bandwidth	[114, 199]	[112, 201]	[99, 204]	[105, 194]	[120, 187]
Effective Observations	11,279	11,847	15,749	12,948	8,657
Control Mean	0.145	0.47	0.124	0.153	0.111
Total Observations	50,186	51,693	50,699	51,053	46,366
Panel B: Placebo Specifications (Linear Fit)					
Pre-Reform Period	-0.044	-0.002	0.019	0.002	0.031**
(1988 - 1991)	(0.030)	(0.019)	(0.017)	(0.012)	(0.015)
Placebo Discontinuity	0.001	0.001	-0.002	-0.006	0.008
(100 Employees)	(0.004)	(0.008)	(0.004)	(0.005)	(0.007)
Predicted Outcomes	0.005	-0.000	0.002	-0.005	-0.002
(Industry Averages)	(0.004)	(0.006)	(0.003)	(0.003)	(0.004)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects.

Table A.6: Workforce Composition - Occupation

	(1) Share Manual	(2) Share Lower-Level	(3) Share Upper-Level	(4) Share Missing Occup.
Panel A: Treatment Specification (1992 - 2016)				
Linear Fit	0.003	0.004	0.001	-0.003
	(0.011)	(0.011)	(0.009)	(0.007)
Bandwidth	[107, 191]	[102, 187]	[113, 179]	[114, 180]
Effective Observations	11,884	13,684	9,839	9,793
Quadratic Fit	-0.001	-0.000	-0.001	-0.000
	(0.014)	(0.013)	(0.011)	(0.009)
Bandwidth	[104, 193]	[114, 204]	[106, 184]	[115, 188]
Effective Observations	12,790	11,590	12,123	10,276
Control Mean	0.394	0.283	0.159	0.175
Total Observations	49,382	51,411	49,399	51,474
Panel B: Placebo Specifications (Linear Fit)				
Pre-Reform Period	-0.061*	0.021	0.005	0.024
(1988 - 1991)	(0.034)	(0.026)	(0.021)	(0.020)
Placebo Discontinuity	0.009	-0.006	0.003	-0.001
(100 Employees)	(0.010)	(0.008)	(0.008)	(0.005)
Predicted Outcomes	0.005	-0.002	-0.001	-0.000
(Industry Averages)	(0.011)	(0.009)	(0.007)	(0.003)

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. The top panel reports local linear and quadratic specifications based on data from 1992 onwards, when the shared governance law has been active. We report three placebo specifications in the bottom panel. First, we report placebo specifications during the pre-1991 period before the law became active; the respective entries are empty ("—") when pre-reform data are not available. Next, we report placebo estimates for a (counterfactual) policy discontinuity at 100 employees. Finally, we assess the extent to which potential industry composition changes affect our estimates and use the leave-out mean of the outcome variable at the industry (NACE Level 1)-year level as outcome variable. Across all specifications, the reported coefficients and standard errors are based on the robust, bias-corrected procedure in Calonico, Cattaneo, and Titiunik (2014) with standard errors clustered at the firm level and industry and year fixed effects.

Table A.7: Labor Income Robustness

	(1) Log Total Labor Costs	(2) Log Wage	(3) Log Wage + Control	(4) Log Wage + Control + FE	(5) Log p75/p25	(6) Log Executive Wages	(7) Log Share Executive Wages
Fixed Bandwidth at 50 (Linear)	0.069**	0.047	0.020	0.005	-0.024	-0.050	-0.097**
	(0.032)	(0.030)	(0.017)	(0.004)	(0.021)	(0.053)	(0.048)
Fixed Bandwidth at 50 (Quadratic)	0.079**	0.056	0.028	0.006	-0.033	-0.002	-0.064
	(0.039)	(0.036)	(0.021)	(0.005)	(0.026)	(0.068)	(0.059)
No Controls	0.076*	—	—	—	-0.029	-0.083	-0.092*
	(0.039)	—	—	—	(0.024)	(0.054)	(0.052)
Industry-Year Fixed Effects	—	—	—	—	—	—	—
	—	—	—	—	—	—	—

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 3.

Table A.8: Capital Income Robustness

	(1) Capital Share	(2) Log Capital Income	(3) Net Income over Equity	(4) Net Income over Revenue	(5) Log Dividends	(6) $\mathbb{1}$(Dividends > 0)	(7) arcsinh (Dividends)	(8) Dividends over Revenue	(9) Payout ratio	(10) Interest Payments over Revenue
Fixed Bandwidth at 50 (Linear)	-0.001	0.117	0.005	-0.007	0.144	0.031	0.176*	0.003*	-0.036	0.001
	(0.010)	(0.079)	(0.149)	(0.007)	(0.120)	(0.026)	(0.102)	(0.002)	(0.041)	(0.001)
Fixed Bandwidth at 50 (Quadratic)	-0.008	0.050	0.006	-0.013	0.065	0.021	0.149	0.002	-0.061	0.001
	(0.012)	(0.094)	(0.194)	(0.009)	(0.144)	(0.032)	(0.122)	(0.002)	(0.052)	(0.001)
No Controls	0.006	0.167*	0.024	-0.006	0.108	0.040	0.129	0.004*	-0.038	0.001
	(0.011)	(0.089)	(0.174)	(0.007)	(0.142)	(0.028)	(0.122)	(0.002)	(0.042)	(0.001)
Industry-Year Fixed Effects	—	—	—	—	—	—	—	—	—	—
	—	—	—	—	—	—	—	—	—	—

Note: The table reports regression discontinuity specifications for the effect of having more than 150 employees—the legal threshold at which workers have a right for shared governance. Here, we report additional specifications as robustness checks. While the main results used the optimal bandwidth choice based on Calonico, Cattaneo, and Titiunik (2014) , rows 1 and 2 report results based on a fixed bandwidth of 50 employees around the discontinuity. The next row reports the linear specification but without control variables. The last row reports the linear specification with industry-by-year controls. Across all specifications, standard errors clustered are at the firm level. We report the main results in Table 4.

Table A.9: Log Total Assets

Panel A: Wage Rigidity and Capital Intensity

	(1) Wage Dispersion (SD)		(2) Rent Sharing Elasticity (Firm Level)		(3) Value Added per Worker		(4) Capital Share		(5) Fixed Assets per Worker		(6) Total Assets per Worker	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.002	0.197**	0.173*	0.104	0.273**	0.042	0.331***	0.017	0.143	0.077	0.233**	0.088
	(0.083)	(0.081)	(0.093)	(0.081)	(0.118)	(0.067)	(0.096)	(0.080)	(0.109)	(0.066)	(0.113)	(0.064)
Bandwidth	[117, 187]	[114, 174]	[113, 173]	[121, 194]	[114, 169]	[118, 182]	[114, 165]	[110, 174]	[110, 175]	[113, 187]	[109, 170]	[114, 192]
Effective Observations	4,546	8,828	7,073	6,450	4,752	7,902	6,120	6,748	5,492	9,336	5,126	9,997
Quadratic Fit	-0.029	0.204**	0.147	0.112	0.172	0.060	0.356***	-0.032	0.115	0.068	0.282**	0.065
	(0.104)	(0.096)	(0.115)	(0.091)	(0.146)	(0.073)	(0.112)	(0.094)	(0.137)	(0.077)	(0.128)	(0.078)
Bandwidth	[114, 194]	[105, 183]	[112, 181]	[98, 195]	[116, 180]	[103, 201]	[103, 174]	[108, 180]	[112, 184]	[110, 201]	[93, 178]	[114, 202]
Effective Observations	5,207	11,889	7,904	10,164	5,120	12,374	8,989	7,481	5,806	11,176	8,208	10,538
Total Observations	23,147	49,700	40,170	32,677	30,585	42,262	38,713	34,134	29,743	43,104	28,327	44,520
Control Mean	16.35	15.41	15.62	15.84	14.72	16.33	15.17	16.32	14.75	16.33	14.63	16.35

Panel B: Market Competition and Job Quality

	(1) HHI (Sales)		(2) HHI (Employment)		(3) Lerner Index ($\frac{\text{Profits}}{\text{Sales}}$)		(4) Separation Rate		(5) Pay Premia	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.143	0.128	0.092	0.163**	0.198**	0.109	-0.047	0.246**	0.143	0.060
	(0.096)	(0.083)	(0.112)	(0.079)	(0.100)	(0.085)	(0.072)	(0.113)	(0.091)	(0.072)
Bandwidth	[108, 177]	[112, 177]	[105, 179]	[115, 176]	[119, 167]	[110, 186]	[124, 191]	[119, 176]	[114, 179]	[112, 195]
Effective Observations	4,829	9,662	4,561	9,586	5,198	7,845	6,997	5,101	7,179	7,755
Quadratic Fit	0.116	0.162*	0.018	0.205**	0.201*	0.059	-0.056	0.294**	0.153	0.082
	(0.113)	(0.099)	(0.129)	(0.097)	(0.119)	(0.096)	(0.088)	(0.125)	(0.107)	(0.087)
Bandwidth	[106, 194]	[104, 182]	[99, 186]	[110, 178]	[114, 175]	[98, 187]	[119, 191]	[104, 183]	[103, 188]	[104, 191]
Effective Observations	5,474	12,567	5,490	10,887	6,589	10,392	7,784	7,589	10,263	9,013
Total Observations	24,584	48,263	23,148	49,699	37,313	35,534	40,695	32,152	39,559	33,288
Control Mean	15.51	15.83	15.77	15.71	15.53	15.94	16.28	14.93	15.24	16.26

Note:

Table A.10: Log Fixed Assets

Panel A: Wage Rigidity and Capital Intensity

	(1)		(2)		(3)		(4)		(5)		(6)	
	Wage Dispersion (SD)		Rent Sharing Elasticity (Firm Level)		Value Added per Worker		Capital Share		Fixed Assets per Worker		Total Assets per Worker	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.115	0.208	0.255*	0.193	0.420**	0.036	0.553***	0.047	0.304*	0.018	0.495**	0.066
	(0.132)	(0.132)	(0.134)	(0.143)	(0.193)	(0.101)	(0.158)	(0.116)	(0.183)	(0.109)	(0.196)	(0.106)
Bandwidth	[114, 173]	[119, 175]	[113, 170]	[125, 183]	[121, 169]	[109, 184]	[121, 164]	[109, 189]	[122, 172]	[120, 178]	[123, 168]	[113, 181]
Effective Observations	4,155	7,458	6,562	5,010	3,817	9,631	4,832	7,802	3,769	7,106	3,265	8,997
Quadratic Fit	0.154	0.254*	0.186	0.259	0.486**	0.056	0.636***	-0.050	0.406**	-0.006	0.622***	0.024
	(0.163)	(0.150)	(0.169)	(0.159)	(0.216)	(0.123)	(0.168)	(0.146)	(0.203)	(0.126)	(0.207)	(0.130)
Bandwidth	[113, 178]	[107, 182]	[116, 179]	[103, 183]	[106, 178]	[110, 186]	[97, 172]	[115, 189]	[106, 179]	[115, 188]	[98, 174]	[116, 186]
Effective Observations	4,620	10,937	6,655	8,293	6,203	9,709	10,006	6,833	5,903	8,828	6,707	8,973
Total Observations	22,897	47,801	39,241	31,457	28,795	41,903	36,955	33,743	27,998	42,700	26,616	44,082
Control Mean	15.01	13.98	14.29	14.38	13.17	14.99	13.62	15.06	13.03	15.102	13.16	14.97

Panel B: Market Competition and Job Quality

	(1)		(2)		(3)		(4)		(5)	
	HHI (Sales)		HHI (Employment)		Lerner Index ($\frac{\text{Profits}}{\text{Sales}}$)		Separation Rate		Pay Premia	
	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median
Linear Fit	0.238	0.203	0.034	0.289**	0.274*	0.170	0.065	0.276	0.255*	0.121
	(0.171)	(0.124)	(0.159)	(0.128)	(0.155)	(0.121)	(0.116)	(0.181)	(0.151)	(0.116)
Bandwidth	[121, 173]	[111, 172]	[109, 176]	[117, 171]	[119, 166]	[107, 187]	[119, 175]	[124, 176]	[121, 171]	[111, 184]
Effective Observations	3,129	9,344	3,997	8,163	4,906	8,434	6,486	4,112	5,351	7,269
Quadratic Fit	0.243	0.278*	0.003	0.395***	0.330*	0.130	0.069	0.360*	0.319*	0.132
	(0.185)	(0.152)	(0.192)	(0.149)	(0.183)	(0.140)	(0.141)	(0.189)	(0.173)	(0.136)
Bandwidth	[105, 187]	[106, 176]	[112, 187]	[105, 175]	[114, 173]	[97, 189]	[118, 179]	[101, 183]	[109, 178]	[99, 187]
Effective Observations	5,247	10,871	4,023	11,477	6,271	10,585	7,073	7,941	7,748	9,458
Total Observations	23,630	47,068	22,762	47,936	35,667	35,031	40,244	30,454	37,826	32,872
Control Mean	14.11	14.44	14.280	14.35	13.90	14.78	14.92	13.43	13.82	14.88

Note:

C Qualitative and Survey Evidence on Shared Governance in Finland

C.1 Qualitative Evidence from Survey of Board Representative in Gold, Kluge, and Conchon (2010)

To provide concrete examples of how worker representatives view their role and exert influence, we here report brief excerpts from an interview with Ari Rajamäki, a board representative at a shipbuilding corporation with shipyards in Turku, Helsinki, and Rauma. The interview was conducted by Michael Gold in 2009 and is reported as part of a book containing interviews with board-level employee representatives in various countries (Gold, Kluge, and Conchon, 2010, , p. 34–46).

Rajamäki had joined the company in 1975 and worked as an assembler. A member of the Finnish Metalworkers' Union, he was elected as board representative in 2005, representing 2,600 blue-collar workers. A second board representative, from the Union of Salaried Employees, represents the white-collar workers. A third, from the engineers' union (*Tekniikan Akateemiset TEK*), serves as an observer.

Below, we provide excerpts from the interview.

On the influence of the worker representatives on the board:

> *We are equal members of the board. [...] [We] sit around the same table and we have the same powers and responsibilities, but of course I know where the power lies. Of course, if we come to a vote, then we lose – but the Norwegians [the capital representatives from the business group] always seek consensus, and the former chairman of the board used to ask if anyone was against. If no one was against, then it was decided unanimously, but if you were against, then he goes, 'Why – what's the grounding?' So you have to be awake, you have to be able to make reasonable comments – and then you can have influence.*

Describing a concrete proposal to affect corporate decision-making, and illustrating pathways through which worker representatives may affect outsourcing or rent sharing:

> *Three years ago, our management in Norway discovered that we had a large surplus in our private pension fund, because it had been run very effectively. They proposed outsourcing it to an outside company. That came almost out of the blue on the board agenda, and I said I was against it. The chairman asked why. I said I had a feeling that it was our own money and comprised transferred salaries, and*

that, even though the proposal was legal – in Finland, the company has the legal right to that money – nevertheless it has to invest it according to very strict criteria and we bore the final risk. I asked to postpone the decision, which we did. I then consulted our union pension experts about the risks if we outsourced the fund, and we discussed the matter at three or four board meetings. The bidding came from several large Finnish pension fund companies, and I squeezed them for the type of contribution they were going to make to our fund because the shareholder representatives on the board were already anticipating outsourcing. We managed to pump one million Euros of that money into employee welfare with an advisor from the insurance company to advise on welfare and benefits. So we got that from the company, and the rest of the money of course went to the owners. But that came out of the blue, and you have to be awake if you are going to say anything at the board meeting. Of the one million Euros, we've used EUR200,000 a year over five years to develop our welfare, to train people, improve communications, promote a better working environment and so on. That was just one day's meeting – if I hadn't been awake, we wouldn't have received that money.

On his background and training:

With my background, I am most familiar with working environment issues, and perhaps some training issues as well, especially when it comes to blue-collar workers. But I've been making myself more and more familiar with financial issues because that's a key issue at board level. [...] Of course, we can use our union – they organise certain training courses. It's not actually a problem if you are interested in developing yourself, though of course you can't be an expert in all areas. [...] There was tailor-made training for the directors. The tailoring came from the union side, so I've taken part in that, but I don't recall any other. I haven't found the training issue is a big problem. It depends on how active you are yourself. You can get support from the union. Of course, you can request certain types of training, and of course we have been discussing this at our union general assembly, that this type of person needs special support or at least a contact person to discuss things with. That's because so often you get classified information, but you have to discuss it with somebody to work out your reactions.

On his role in transmitting information:

Yes, top management always looks to me, or my colleague, when there are difficult issues and they are not happy with developments – for instance, if we have had

a strike or are unable to agree certain issues, or there's some dispute over pay. They say 'can't you tell them that we have run out of money? That we can't award any pay rises?'. And so on. But we try to avoid that, because we are not carrying the can... If it's an employer decision, then it's the employer who has to take the responsibility. But of course, when there are very difficult times – we can't get any new orders, for instance – I've been at our union general assemblies and meetings explaining the management's point of view, or what plans they have, or perhaps what they would like to decide in the near future. So, in that sense, I guess I'm a middle man, or an intermediary, but of course the information flow goes both ways.

On his role in pay negotiations:

[Do you feel particularly concerned not to harm company performance?] Yes. I've decided that when it comes to pay issues, I'm not going to adopt a position, because I've seen all the calculations concerning, for instance, a ship order, and I know what they are based on. So I leave that totally to the negotiators, though of course we'll discuss it with the shop stewards. I just don't participate in the actual negotiations.

D Construction of Variables for Heterogeneity Analysis

Here, we describe the construction of variables used for our heterogeneity analysis in Section 7. We first calculate characteristics at the 3-digit industry-by-year level (except for rent sharing elasticity, see below) and then split industries into those above and below the median. For this calculation, we use observations outside our estimation sample, thus excluding firms with employment between 100 and 200 employees. Below, we explain in more detail the construction of variables that we use to split our data:

- Wage Flexibility Measures
  - Standard Deviation of Wages: We calculate the standard deviation of average worker wages at the firm-level by industry and year.
  - Rent Sharing Elasticity: We regress the annual firm-level average log wages on log value added per worker by 2-digit industry codes and split the sample using these coefficients to industries below and above the median. As there is a relatively small number of observations by 3-digit industry codes, we use 2-digit industry codes in this case.

- Measures of Product Market Competition
  - Herfindahl-Hirsch Index (Sales): We first calculate the sum of sales by industries and years. Then we divide firm-level sales by total sales and square it. Finally, we take the sum of these measures by industry-year and split the data to below and above the median.
  - Lerner Index: We divide the sum of Earnings Before Interests and Taxes (EBIT) by the sum of sales at the industry-year-level.
- Job Quality Measures
  - Separation Rate: We first calculate the sum of separated workers and then divide it by the total number of employees at the industry-year level.
  - AKM Pay Premia: We calculate the standard deviation of Firm-level AKM fixed effects at the industry-year level.
- Capital Intensity Measures
  - Fixed Assets per Worker: We calculate the sum of fixed assets and then divide it by the total number of employees at the industry-year level.
  - Value Added per Worker: We first calculate the sum of firm-level value added and then divide it by the total number of employees at the industry-year level.
  - Capital Share: We first divide the sum of labor costs by the sum of value added at the industry-year level. Then we split the data using one minus this measure to samples below and above the median.
- Measures of Corporate Governance

We analyze four categories of heterogeneity variables. First, we study two wage flexibility measures and split industries according to the between-worker standard deviation of wages as well as the rent-sharing elasticity. Second, we consider two measures of market competition, the Herfindahl-Hirschman Index (HHI) of sales and the Lerner index (calculated as one minus the ratio of profits to sales). Third, we consider measures of job quality, studying the separation rate and firms' AKM pay premium. Lastly, we consider measures of capital intensity by studying heterogeneity by fixed assets per worker, value added per worker, and the capital share. We report effect heterogeneity for investment and wages in Tables 5 and 6 and Figure 7 and more detail on data construction in Appendix Section D.