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Contents

List of Figures

List of Tables

Abstract

We estimate the effects of a mandate allocating a third of corporate board seats to workers (*shared governance*). We study a reform in Germany that abruptly abolished this mandate for new firm cohorts but locked it in for incumbents. Rejecting the canonical hold-up prediction – that increasing labor's power reduces owners' investment incentives – we find *positive* effects on capital formation. Shared governance does not measurably raise wages or rent sharing, nor does it lower profitability or debt capacity. It lowers outsourcing. The evidence is consistent with richer models of industrial relations whereby shared governance institutionalizes communication and repeated interactions between labor and capital.

Zusammenfassung

Wir schätzen die Effekte einer Gesetzesauflage, nach welcher ein Drittel der Sitze im Aufsichtsrat eines Unternehmens durch Arbeitnehmer(-vertreter) zu besetzen sind (shared governance). Konkret untersuchen wir dabei eine Gesetzesreform in Deutschland, welche diese Auflage abrupt für neue Firmen aufhob, für bereits bestehende Firmen jedoch weiterbestehen ließ. Unsere Ergebnisse stehen dabei in starkem Widerspruch zur kanonischen Hold-up Hypothese, die von einem Rückgang der Investitionsanreize für Unternehmer bei steigenden Einfluss der Arbeitnehmer ausgeht. Stattdessen finden wir positive Effekte für die Kapitalbildung. Weiterhin führt Arbeitnehmerbeteiligung jedoch nicht zu höheren Lohnaufschlägen oder einer stärkeren Umverteilung von Monopolrenten. Darüber hinaus führt Arbeitnehmerbeteiligung nicht zu geringer Profitabilität oder Fremdkapitalaufnahme, jedoch wird Outsourcing reduziert. Die Ergebnisse sind konsistent mit komplexeren Modellen von Arbeitgeber-Arbeitnehmer-Beziehungen, in welchen Arbeitnehmerbeteiligung die wiederholte Interaktion zwischen den Produktionsfaktoren Kapital und Arbeit institutionalisiert wird.

JEL

Keywords

Acknowledgements

1 Introduction

A fundamental question societies face is how to involve workers in decision-making at their workplace. Many countries, particularly in continental Europe, grant workers formal authority in firms' decision-making. Such shared governance, or codetermination, institutions include worker-elected directors on company boards. By contrast, in many liberal market economies such as the United States, firms are legally controlled solely by their owners. An influential argument against shared governance is that granting workers control rights will raise their bargaining power and wages, and thereby discourage capital formation (Grout, 1984). Prominently, Jensen/Meckling (1979) lay out the disinvestment effects of codetermination as follows:

> [T]he workers will begin 'eating it up' [the firm] 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.

In contrast, worker participation may help overcome coordination issues and improve information flows (Hirschman, 1970; Freeman/Medoff, 1985; Freeman/Lazear, 1995), foster long-term relationships, or facilitate the enforcement of implicit contracts (Malcomson, 1983; Hogan, 2001). An ideal experiment adjudicating between these views would randomly impose shared governance on some firms, and prohibit it in others, and study the effects on capital formation and wages.

We provide quasi-experimental evidence on the effects of shared governance by studying a 1994 reform in Germany that sharply abolished worker-elected directors in certain firms, and permanently preserved the mandate in others. Before the law change, all stock corporations (*Aktiengesellschaften* and *Kommanditgesellschaften auf Aktien*) had to apportion at least one third of their supervisory board seats to worker representatives. These worker board representatives are primarily non-managerial workers, proposed as well as directly elected by the non-managerial workforce. In two-tier board settings such as Germany's, the supervisory board appoints, monitors, dismisses, and sets the compensation for the executive board. It is also directly involved in important decisions, such as large investments. Anecdotally, many decisions in the supervisory board are taken unanimously, with consensus between shareholder and worker representatives (Gold, 2011; Steger, 2011).

The 1994 reform abruptly abolished worker-elected directors in *new* stock corporations (unless firm size crossed a threshold of 500 employees). Importantly, the cohort-based reform locked in shared governance in the *incumbent* firm cohorts incorporated before the reform. The sharp law change

permits a difference-in-differences design comparing, first, stock corporations incorporated just before or after the August 1994 cutoff, and, second, their peer cohorts of untreated corporation types (*Gesellschaften mit beschränkter Haftung* (*GmbH*), which we will refer to as limited liability companies (LLCs)).[1] We implement this design by combining firm-, establishment-, and worker-level data: (i) financial and production data, ownership, and supervisory and executive board composition for public and private firms based on Bureau van Dijk (BvD) data sets, (ii) administrative matched employee-establishment data covering the universe of social security records merged to BvD firm-level data, (iii) a comprehensive data set of incorporations and exits, and (iv) additional data on board composition for listed firms. To assess the validity of the design, we (i) rule out manipulation of incorporation dates, (ii) rule out composition shifts at entry, (iii) argue that the grandfathering is binding (and note that the Federal Constitutional Court has upheld the grandfathering ruling against shareholder law suits), (iv) rule out selective attrition in the form of firm exit, and (v) estimate a series of placebo reforms.

Our first core result is on firm performance, with a particular focus on capital formation. Firms with shared governance have, if anything, a *larger* fixed (long-term) capital stocks and capital-labor ratios. The large point estimates of 40 to 50 percent come with wide confidence intervals, and so also accommodate smaller positive effects. Still, the confidence intervals exclude zero. Hence, the estimates reject the disinvestment predicted by the hold-up and agency cost views. We further document a large and significant increase in the capital share of 8ppt (control mean: 0.30).

One mechanism may be that owners strategically invest in labor-substituting capital to offset labor power. Speaking against this interpretation is a positive yet statistically insignificant effect on employment. Moreover, instead of codetermined firms outsourcing labor-intensive steps, the share of sales produced in-house increases by about 16ppt (control mean: 0.43) and we find no evidence for labor outsourcing. Finally, we find that shared governance increases value added per worker by 16 to 21 percent, with no effects on total factor productivity.

Our second focus is on wages. We test the theoretical mechanism whereby hold-up would discourage investment. We find small positive effects on composition-adjusted wages (Abowd/Kramarz/Margolis, 1999), with point estimates between 0.5 and 1.2 percent and confidence intervals ruling out pay premia above 5 percent. As a more direct test of the wage-bargaining mechanism at the core of hold-up, we estimate and compare rent-sharing elasticities in firms with and without shared governance. We document similar elasticities of wages to firm-level value added per worker, of about 0.09. With this small elasticity, the estimated effect on value added per worker of about 20 percent would predict a small 2 percent wage effect, which is within the confidence interval for our estimated wage effect.

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[1] German GmbHs are broadly comparable to private limited companies in the United Kingdom or LLCs in the United States. They differ from US LLCs in that they are formally corporations and in that their shares *cannot* be traded on a stock exchange.

In a final step, we turn to firms' financials and the capital side of income. We find no evidence for lower profitability, external-finance capacity, or leverage. These effects of codetermination may appear to be in tension with some evidence for negative profit effects of firm-level unionization (see, e.g., Lee/Mas, 2012). We speculate that although both institutions plausibly boost labor's bargaining power, codetermination institutionalizes more cooperative involvement of workers, whereas unionization may be more adversarial, in particular in the context of US industrial relations. Moreover, codetermination involves workers in broader corporate governance, including investment and financial decisions and control of executives, whereas firm-level unionization events may primarily reorganize the determination of wages and working conditions.

Our paper studies a large set of outcome variables suggested by the theoretical literature, oftentimes finding no effects. Despite this, we can reject the concern that our specific instances of significant effects are spurious, drawing on a joint significance test from a seemingly unrelated regressions model (Zellner, 1962), and our main results remain robust to accounting for multiple hypothesis testing (Romano/Wolf, 2005).

While our evidence is inconsistent with the disinvestment prediction of the hold-up view, richer views of corporate governance and labor-capital interaction can rationalize the results. As supervisory boards are directly responsible for large strategic and financial decisions, and for controlling the executive board, we show that, in an otherwise standard hold-up model, increasing worker bargaining power in corporate decisions beyond wages *increases* capital investment. The additional capital formation could still reflect yet another agency conflict, if worker representatives push for investments to keep cash flow inside the firm at the expense of dividend payouts. Alternatively, shared governance may crowd in investment by facilitating cooperation, by institutionalizing communication, or through repeated interactions between labor and capital.

Our paper is consistent with existing evidence for limited hold-up effects (see, e.g., Machin/Wadhwani, 1991; Addison et al., 2007; Card/Devicienti/Maida, 2014), even though some studies document negative investment effects of unionization (e.g., Connolly/Hirsch/Hirschey, 1986; Hirsch, 2004). In the context of codetermination via works councils, Addison et al. (2007) finds no association with lower investment. Rapp et al. (2019) find positive effects of board representation based on a propensity score matching strategy among listed firms. Our quasi-experimental research design also complements studies comparing firms with and without codetermination using size cutoffs. Specifically, Gorton/Schmid (2004), Lin/Schmid/Xuan (2018), Kim/Maug/Schneider (2018), and Redeker (2019) study the threshold at 2,000 employees (one-half vs. one-third employee representation). Compared to these studies, our variation has *no* worker-elected directors in the control group, whereas these designs compare one third compared to half representation (where shareholders still break ties). Moreover, we analyze a policy change that circumvents potential endogeneity concerns related to employment as an assignment variable. Lastly, our design analyzes a

persistent rather than transitory treatment around employment cutoffs.[2]

Finally, our paper contributes to the growing literature on how board composition affects firm outcomes (see, e.g., Ahern/Dittmar, 2012; Bertrand et al., 2018: for gender quotas). Indeed, we additionally find that worker representation boosts board representation of women (15ppt or 43%) while sharply reducing that of nobility-title holders (1.4ppt or 60%), a measurable proxy for social capital. Importantly, worker representatives are typically non-managerial workers who are elected by the non-managerial workforce rather than appointed by shareholders.

In Section 2, we discuss mechanisms through which shared governance may affect firm- and worker-level outcomes, with a focus on hold-up. In Section 3, we describe the institutional context and the reform. Section 4 presents our data sets and empirical strategy. Section 5 reports effects on board composition. Section 6, report effects on production outcomes, including our core result on capital formation. Section 7 studies the distributional consequences for wages, rent sharing, profitability and external finance. Section 8 addresses multiple hypothesis testing.

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[2] Svejnar (1981) analyzes wage effects of the introduction of parity codetermination to industries in 1951 and 1976. Gurdon/Rai (1990) study the 1976 reform based on a survey of 63 firms. See Addison (2009) and Scholz/Vitols (2019) for surveys of the literature on codetermination.

2 How Might Shared Governance Affect Firms?

Our point of departure is the hold-up view (Grout, 1984), according to which shared governance discourages investment by raising worker bargaining power in wage setting. We show that this disinvestment prediction is, however, fragile. When workers additionally participate in operational decisions, specifically investment, shared governance can *raise* investment. We then review broader perspectives on shared governance.

2.1 The Hold-Up View

Several authors have argued that unions and other forms of worker representation can be thought of as rent-extracting institutions (see, e.g., Grout, 1984; Lindbeck/Snower, 1989), and that shared governance is one of such institutions (Jensen/Meckling, 1979). Anticipating that labor will grab a larger share of the fruits from their investments, capitalists reduce investment. We formalize this view in a basic hold-up model (following Grout, 1984), in which the firm produces output with a decreasing returns to scale production function $F(K, \bar{L})$, with output prices taken as fixed and normalized to one. Labor $L = \bar{L}$ is fixed here for exposition (see Appendix Section A.2 for the case with endogenous labor). 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.

Stage 2: Wage Bargaining

Hold-up emerges because wages (bargained over in stage 2) depend on output and sunk capital. Specifically, wages w may be determined by Nash bargaining, with workers holding bargaining power ϕ:

$$w^* = \arg\max_w \{\phi \log S_2^W(w, \bar{L}, K) + (1 - \phi) \log S_2^F(w, \bar{L}, K)\}, \tag{1}$$

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/Ashenfelter, 1986;

Abowd/Lemieux, 1993).[3] 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 $c' \leq c$. Total surplus is $S_2 = S_2^W + S_2^F = F(K, \bar{L}) - b\bar{L} - c'K$. The Nash bargaining solution allocates surplus shares such that $S_2^W(w^*, \bar{L}, K) = \phi S_2$ or $S_2^F(w^*, \bar{L}, K) = (1 - \phi)S_2$, and therefore the Nash wage w^* is outside option b plus share ϕ of stage-2 surplus:

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

Hold-up emerges because the firm chooses investment anticipating wage rule (2). The wage is a function of K because K affects output, of which share ϕ goes into wages. Second, K boosts the firms' outside option by $c'K$, thereby lowering wages.

Stage 1: Capital Choice

In stage 1, firms make capital decisions unilaterally – a consequential assumption we relax in Section 2.2 –, maximizing expected profits: $\pi(w, \bar{L}, K) = F(K, \bar{L}) - w\bar{L} - cK$. Namely, the capitalist chooses K to equalize the marginal cost of purchasing it, c, with its marginal benefit (output net of wage effects):

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

Capital investment involves two considerations. First, as in the case of a wage-taking firm, the marginal unit of capital raises output by F_K, but comes at cost c. Second – the core of the hold-up mechanism – workers grab share ϕ of surplus in stage 2. At that stage, capital has value $c' \leq c$. Only if $c' = c$ (if capital can be resold at the original price) is investment first-best ($F_K = c$), i.e. when the wage effect consideration on its own would call for the same capital level as in the wage-taking case. *Underinvestment* ($F_k > c$) emerges as long as wage bargaining power $\phi > 0$ and capital is at least partially sunk ($c' < c$).

As a result, shared governance may exacerbate hold-up and reduce investment to the extent that it boosts worker bargaining power ϕ in wage negotiations:

$$\Rightarrow \frac{dK^*}{d\phi} = \frac{1}{F_{KK}(\bar{L}, K^*)}(c - c')\frac{1}{(1 - \phi)^2}. \tag{4}$$

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[3]   As hold-up works through inside-value/rent sharing, this specification evades the ongoing debate concerning the role of the outside option in wage bargaining (Hall/Milgrom, 2008; Jäger et al., 2019).

2.2 The Fragility of the Disinvestment Prediction of Hold-Up

We illustrate the fragility of the disinvestment prediction in a simple extension: worker participation involves also bargaining over inputs, besides wages. We draw on sequential bargaining (Manning, 1987), where in stage 1 the firm and the workers now jointly determine the capital stock, with worker bargaining power ι in investment decisions:

$$\max_K \{\iota \log S_1^W(w^*, \bar{L}, K) + (1 - \iota) \log S_1^F(w^*, \bar{L}, K)\},\tag{5}$$

where stage-1 investment choices are again made anticipating wage rule (2) in stage 2. The worker and firm surpluses entering first-stage bargaining are $S_1^W = \bar{L}w^* - b\bar{L}$, while $S_1^F = F(K, \bar{L}) - w^*\bar{L} - cK$. The previous case of the firm unilaterally setting capital is nested if $\iota = 0$, when underinvestment emerged whenever $\phi > 0$ and $c' < c$.

Shared governance may alternatively be viewed as an increase in ι. Indeed, the specific institution of codetermination gives workers a vote alongside capitalists in a series of corporate decisions, including those over strategically important investment decisions, and in the appointment and holding accountable of managers.

Intuitively, since workers care about the capital choice K solely because of its positive effect on wages without having to pay for it, worker say over capital in the form of higher ι will raise rather than lower investment. First consider the extreme case where workers have full bargaining power over inputs, i.e. $\iota = 1$. The optimization problem (5) now maximizes worker surplus, $\max_K \{\log S_1^W(w^*, \bar{L}, K)\}$, with the following first order condition:

$$\bar{L}\frac{\partial w^*}{\partial K} = 0\tag{6}$$

$$\Leftrightarrow \phi F_K(K^*, \bar{L}) - \phi c' = 0\tag{7}$$

$$\Leftrightarrow F_K(K^*, \bar{L}) = c' \leq c.\tag{8}$$

Workers' capital choice trades off the benefit – its marginal product – of which share ϕ goes to the worker, with the marginal cost – resale value c' – because each unit of capital boosts the firm's outside option in the form of $c'K$ in wage setting. Workers ignore direct capital costs c. The two extreme cases of $\iota = 0$ and $\iota = 1$ make clear that increasing worker bargaining power in capital choices ι overturns the Grout (1984) underinvestment result ($F_K > c$) to *overinvestment* ($F_K = c' < c$ if $c' < c$). If given a chance, workers push for capital formation, as they benefit in stage-2 wage setting from the higher production.[4]

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[4] The general bargained capital level K^* under $\iota \in [0, 1]$ is given by: $F_K(K^*, \bar{L}) = c - (c - c') \times \left[\frac{(\iota - \phi)(F(K^*, \bar{L}) - b\bar{L} - c'K^*) + \iota(c' - c)K^*}{(1 - \phi)(F(K^*, \bar{L}) - b\bar{L} - c'K^*) + \iota(c' - c)K^*}\right]$. Here, K^* depends on ι as follows: $\frac{dK^*}{d\iota} =$

Hold-Up and Profits

Here, hold-up is still active; in fact, the prospect of wage bargaining drives workers' push for more investment. Moreover, here the capital increase hurts capitalists *even if investment were to move closer to the first-best level*: profits are higher under $\iota = 0$ than under $\iota > 0$ (shown formally in Appendix Section A.3), consistent with at least individual capitalists not voluntarily adopting codetermination even if doing so could increase efficiency, echoing the broader debate between Jensen/Meckling (1979) and Levine/Tyson (1990); Freeman/Lazear (1995). Our empirical assessment in Section 4.3 does not detect entry and exit effects, which we sidestep here.

Employment and Capital-Labor Ratios

In Appendix Section A, we derive the comparative statics with endogenous labor L. If labor and capital are complements ($F_{LK} > 0$), the effects of ϕ or ι on labor and the capital-labor ratio have the same signs as those on capital.

Dynamic Aspects

Under simultaneous rather than sequential bargaining over wages and investment, the parties can neutralize hold-up and reach the efficient investment level (Crawford, 1988); shared governance could shift the regime from inefficient to efficient bargaining. 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.

2.3   Beyond Hold-Up: Broader Views of Shared Governance

We now review how shared governance may affect corporate decision-making and ultimately capital through channels beyond inputs and compensation.

$\frac{-(c-c')(F-b\bar{L}-c'K^*+(c'-F_K)K^*)}{(1-\phi)[F_{KK}(F-b\bar{L}-c'K^*)+(F_K-c')^2]-(c-c')[F_K-c'+\iota F_{KK}K^*]}$. This expression (which we formally evaluate in Appendix A.1) is positive, so K^* is increasing in ι, as long as $\phi > 0$ and $c' < c$. (If $\phi = 0$ (i.e the workforce has no power in setting the wage), then $w^* = b$ does not depend on K. For $\iota = 1$, any K^* is a solution, while for $\iota < 1$, efficiency emerges ($F_K = c$).)

Capital Markets

Firms might strategically increase debt to counter hold-up when labor power increases (e.g., Matsa, 2010). Alternatively, worker representatives may prefer safer projects, thus lowering capital costs and permitting higher leverage and investment, consistent with the negative industry-level association between unionization and bond yields (Chen/Kacperczyk/Ortiz-Molina, 2011) and the positive firm-level relationship between employee representation and leverage in Germany (Lin/Schmid/Xuan, 2018).

Corporate Governance

In principle, capital could always outvote labor. Yet codetermined supervisory boards appear to vote unanimously (Gold, 2011; Steger, 2011), suggesting that worker representatives compromise with shareholder representatives (Thelen, 1991). Such a cooperative equilibrium may arise with repeated interactions when labor representatives have an information advantage. Then, capitalists, who hold formal authority in the form of the majority of seats, may find it optimal to grant labor real authority (Aghion/Tirole, 1997), particularly in matters more important to labor than capital. Even then, the increased diversity of objectives in a codetermined board could decrease managerial accountability (Tirole, 2001: p. 59-60). Managers and workers may also collude to transform cash flow into illiquid corporate assets rather than dividends, and engage in empire-building (as in the agency conflict mechanism in Jensen/Meckling, 1976). (We will not be able to study dividends or stock prices in our data, since most of our stock corporations are unlisted.) Increasing labor power may entrench managers (Pagano/Volpin, 2005; Atanassov/Kim, 2009). In contrast, monitoring through worker representatives could also be more stringent, if executives wield more influence over shareholder directors (Hermalin/Weisbach, 1998). Worker-elected directors could have longer horizons and more at stake compared to outside shareholder directors, curbing short-termism.

Incomplete Contracts

Shared governance could facilitate relational contracts (Baker/Gibbons/Murphy, 2002) or enforcement of incomplete contracts. For instance, worker representatives could reduce information asymmetries, due to which management might always have the incentive to misinform workers about the firm's productivity or product demand states (Grossman/Hart, 1981; Malcomson, 1983), leading workers to disregard information from management. More generally, worker representation might lead firms to honor implicit contracts and thus resolve hold-up problems leading to underinvestment by workers (Hogan, 2001), as with firm-specific training or back-loaded compensation. The information channel is particularly relevant in Germany: the executive board is legally required to

report planned firm policy to the supervisory board, and the supervisory board, in its active advisory capacity, can demand reports from management (Lutter, 2001).

Collective Voice and Labor Relations

Finally, shared governance could operate as an institution of collective voice for the workforce, rather than voting with their feet and quitting (Hirschman, 1970; Freeman, 1980). Worker voice could also have direct productivity-enhancing effects by fostering information flows and cooperation.[5]

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[5] Relatedly, Ichniowski/Shaw (1999) document cross-country evidence on employee participation and productivity in the steel sector, and Freeman/Medoff (1985) and Black/Lynch (2001) argue that cooperative relations between labor and management are associated with positive productivity effects of U.S. unions.

3 Institutional Context and the 1994 Reform of Shared Governance

We describe shared corporate governance in Germany and its 1994 abolition in certain newly incorporated firms. We also review wage setting institutions.

3.1 Shared Corporate Governance in Germany

Corporate Governance in Germany

Like many other European countries, Germany has a two-tier board system, a supervisory and an executive board, illustrated in Figure 1 Panel (a). The executive board is the managing body and responsible for day-to-day business. The supervisory board – composed of representatives for shareholders and, in many cases, workers – is responsible for the selection, monitoring, auditing, compensation structuring, and dismissal of the executive board (§§ 84, 87 and 111 *AktG*). The German Corporate Governance Code advises that the supervisory board be involved in all decisions of fundamental importance to the company, e.g., strategic planning and larger financial decisions. Accordingly, corporate charters frequently prescribe thresholds above which investments need to be directly approved by the supervisory board.

Shared Governance

Two institutions allow worker participation in their employers' decision-making: worker representatives on the supervisory board and works councils.[6] The variation we study concerns mandates for the former, an institution introduced in the early years of the Federal Republic of Germany.[7] Worker representatives are elected by the firm's non-managerial workforce in general, secret, equal

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[6] Works councils have extensive consultation, information and codetermination rights in areas such as work hours, occupational safety, and organizational or staffing changes and can directly negotiate with the employer. The 1994 law change did not reform the institution of works councils.

[7] The historical context was favorable for shared governance because, while industry leaders had collaborated with the Nazi regime, the workers' movement was less tainted. Shared governance was also viewed as an acceptable compromise to many firm owners in light of nationalization episodes in the United Kingdom (McGaughey, 2016). In 1951 and 1952, two landmark acts in Germany mandated supervisory board parity in the mining and steel sectors for firms with more than 1,000 employees (1951), and the one-third mandate for other firms (1952) (exempting family firms, and non-stock-corporations with fewer than 500 employees). In the 1960s, the union movement began pushing for further expansion of worker representation, and the social-liberal coalition passed the 1976 codetermination law (*MitbestG*), which mandated parity also in non-mining/steel sectors for firms with more than 2,000 employees.

and usually direct elections, organized by works councils (*Betriebsräte*); board representatives frequently also serve on the works council.[8] Once elected, the worker representatives are co-equal directors with the shareholder representatives. All – or, for larger firms, the majority – of the worker representatives on the supervisory board must be employees of the firm. For larger firms with larger boards, the union can nominate additional external candidates (§ 7 *MitbestG*, § 4 *DrittelbG*). Though not required by law, a large share of worker-elected directors are union members (Addison, 2009). Unions and associated organizations also offer training programs for worker representatives on supervisory boards.

Worker Quotas by Firm Size and Legal Form

Table 1 describes the mandated worker shares of supervisory board seats, which range from zero to parity, and vary by the company's legal form and size (employee count), and founding date. Because family- and state-owned firms are exempt from codetermination, we drop these firms from our sample. Our variation is in the form of a mandate for zero or one-third worker-elected directors, illustrated in Figure 1 Panel (a). Our variation applies to firms with at most 500 employees. For these firms, rules were differentiated by legal form until 1994. Limited liability corporations (*Gesellschaften mit beschränkter Haftung*) and non-corporations (e.g., *Offene Handelsgesellschaften (OHG)*) in the same size category have always been exempted from *any* worker representation. In contrast, the 1994 reform changed the rules for stock corporations on the basis of incorporation date, as we describe below.

In firms with 501 to 2,000 employees, workers elect one third of the seats no matter the legal form. (Our intent-to-treat design does not condition on firm size, as discussed in Section 4.2.) In very large firms with more than 2,000 employees, workers elect 50 percent of the seats. The chairperson is generally a shareholder representative and can break ties. (There is full parity in the mining, coal, and steel industry sector.)

3.2   1994 Abolition of Shared Governance in New Stock Corporations

Since 1952, stock corporations had been required to have at least one-third worker representation on the supervisory board regardless of size. A 1994 reform of the Stock Corporation Law (*Aktiengesetz*) abruptly abolished this requirement for newly incorporated stock corporations while

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[8]   In firms with more than 2,000 employees, the managerial workforce also participates in the elections and sends at least on representative (§ 15 *MitbestG*).

preserving it in existing ones. The law was a result of last-minute political compromise and did not affect LLCs or other features of shared governance. Table 1 and Figure 1 Panel (b) illustrates these changes. These differences in the mandate continue to the present.

Abolition in Stock Corporations Founded after August 10th, 1994

The reform abolished the one-third mandate only for new corporations: those incorporated on or after August 10, 1994. As a consequence of the reform, new stock corporations *cannot* have any worker-elected board members, unless they grow very large. Upon having 501 employees, both cohort groups face the same one-third mandate.[9] Figure 1 Panel (a) illustrates corporate governance in these corporations without the mandate.

Political Compromise: Cohort-Based Differentiation by Incorporation Date

Importantly, the law locked in the worker representation mandate in already founded stock corporations. This cohort-specific grandfathering rule arose as a last-minute political compromise in late May 1994, between the conservative-liberal governing coalition, between Christian Democrats (CDU/CSU) and Free Democrats (FDP), and the center-left opposition Social Democrats (SPD), which held a majority in the upper chamber (*Bundesrat*). The conservative-liberal government had proposed to abolish shared governance in all stock corporations (up to 500 employees), *including* existing ones, to harmonize rules between stock corporations and LLCs and to deregulate and simplify the corporate and codetermination law. By contrast, the opposition had been in favor of maintaining shared governance for *all* stock corporations – new and old. A key rationale for the cohort-based compromise was that existing companies were believed to have already learned to operate under the mandate. Upon reaching the political compromise, the law was then promptly passed in both chambers in the subsequent weeks, and mandated a cutoff date of August 10, 1994, the day after the law's promulgation.[10]

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[9] For the vast majority of firms, the 500 employment cutoff is not binding, as only 0.02% of firms, and less than 35% of employment is in firms above this threshold. Among stock corporations, still only 14% of firms reach the 500 employee threshold. New stock corporations with fewer than 500 employees cannot formally have worker-elected board members as the corporate law leaves no room for choice (see, e.g., Raiser/Veil/Jacobs, 2015: § 1 Rn. 26, and § 23 (5) and § 96 AktG). In principle, LLCs could add additional worker representatives exceeding the fractions mandated by law, although anecdotal evidence suggests that this is not common. In any case, rules for LLCs were not changed by the 1994 reform.

[10] The initially proposed bill and compromise committee recommendation are reported in *Drucksache* 12/6721 and 12/7848, respectively (*Deutscher Bundestag, 1994*), the minutes of plenary proceedings in *Plenarprotokoll* 12/233 and 12/237 (*Stenographischer Bericht, Deutscher Bundestag, 1994*).

Rigidity of the Cohort-Based Lock-In

Notably, grandfathered stock corporations incorporated just before August 10, 1994 cannot simply escape the shared governance mandate by re-incorporating. Specifically, a change of legal form and temporally connected re-incorporation of an old stock corporation as an ostensibly new stock corporation does not invalidate the mandate for board representation of workers.[11] The 1994 grandfathering rule has been challenged in legal cases brought by shareholders of older corporations on the grounds that the arbitrary nature of the cutoff date violates the constitutional principle of equality. However, the courts have upheld the clause, including the Federal Constitutional Court as recently as 2014 (BVerfG, 09.01.2014, Az. 1 BvR 2344/11).

Secondary and Non-Grandfathered Elements of the Reform

In addition to the shared governance mandate, the 1994 law changed several other rules (e.g., use of profits, general shareholder meetings), *all of which applied regardless of the incorporation date, had no grandfathering, and were not cohort-specific – such that they would affect both cohort groups and hence be netted out by our first difference. Crucially, only the shared governance setup was grandfathered in for existing corporations. Moreover, the additional features of the 1994 reform were considered secondary to the abolition of shared governance by commentators.[12] In principle, such non-grandfathered features could still have effects in our research design if they affected the quantity or composition of post-1994 entrants. In Section 4.3, we will directly assess these potential confounders, and empirically find that the reform had no detectable effects on these margins. From that perspective, the broader motivation surrounding the reform to spur entrepreneurship among stock corporations is not borne out in the data (and in any case, our difference-in-differences design would net out any common effects on both older and younger stock corporations).

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[11] See, for example, Raiser/Veil/Jacobs (2015) § 1 Rn. 5. Re-incorporations as corporations according to European law (SE) also entail a grandfathering rule such that employee representation is preserved, even if the corporation adopts a unitary board structure (§ 21 (6) *SEBG*). In theory, re-incorporations as LLCs could undo the grandfathering rule (although LLCs can also opt to keep workers on the board). During our sample period, re-incorporations as an LLC require at least 75% of shareholder votes (§ 240 (1) *UmwG*), although additional requirements apply in certain cases (§ 242 *UmwG*). We did not identify cases where stock corporations switched corporate form to an LLC to evade the grandfathering rule. Likewise, the legal practitioners we consulted deemed this scenario unlikely due to, among other reasons, switching costs. On aggregate, such evasion behavior would show up as increased exits, which we do not detect in our survival analysis in Figure 3.

[12] For example, the Frankfurter Allgemeine Zeitung, a newspaper of record in Germany, considered the changes irrelevant except for the codetermination reform ("Nicht nur weiße Salbe", *Frankfurter Allgemeine Zeitung*, May 27, 1994, p. 13).

3.3 Wage Setting in Germany

Firm-level wage setting is crucial for the hold-up mechanism, as described in the model in Section 2.1, but would not be present if firms are wage-takers, such as if wages were rigidly fixed through collective bargaining at levels above the firm (as laid out by Acemoglu/Aghion/Violante, 2001: for the case of wage floors). In Germany, unions do negotiate with employer associations at the sectoral level, setting wage *floors*, work hours and working conditions. Yet, there is substantial scope for firm-specific deviations. Most importantly, covered employers can always deviate *upwards* (*Günstigkeitsprinzip*, § 4 (3) *TVG*). Moreover, the fraction of employment covered by collective bargaining agreements has decreased substantially (Dustmann/Ludsteck/Schönberg, 2009; Kügler/Schönberg/Schreiner, 2018), and the prevalence of opening clauses has risen (Brändle/Heinbach/Maier, 2011; Dustmann et al., 2014), allowing employers covered by an agreement to pay below-CBA wage and negotiate directly with works councils. Consistent with this evidence, we estimate evidence that firm-specific shocks to productivity affect wages in the German context in Section 7.1. Moreover, there is considerable between-firm dispersion in wage premia even within industries (Card/Heining/Kline, 2013), and idiosyncratic shocks to firms, e.g., corporate tax changes or labor supply shocks, affect wages (Fuest/Peichl/Siegloch, 2018; Jäger/Heining, 2019).

4 Data and Empirical Methodology

We describe the data as well as our difference-in-differences methodology.

4.1 Data

Our analysis relies on information from four data sources, with further details on variable construction in Appendix Section B.

Summary Statistics

Since all variables are potential outcome variables (as treatment is assigned at date of incorporation), we report control means in each regression column (separately for stock corporations and LLCs incorporated after the reform).

Firm Data: Bureau van Dijk (BvD)

Our main data source is firm-level panel data on balance sheets and income statements from BvD, the largest available data source for German firms. It is based on official company registers, company reports, and information from credit rating agencies. To construct the most comprehensive sample, we merge several versions of the BvD data: the Wharton Research Data Services Amadeus product (WRDS), Orbis Historical data (which includes some additional firms no longer active in the standard Amadeus/Orbis products), and additional historical tranches from the LMU-ifo Economics & Business Data Center (EBDC). We detail these sources and the merging procedure in Appendix Section B.1.1.

For our main analysis, we focus on stock corporations and LLCs incorporated from August 1992 through August 1996 – a symmetric two-year interval around the August 1994 reform cutoff. In addition to standard BvD data cleaning following Gopinath et al. (2017), we apply several sample restrictions motivated by the applicability of the mandate (mainly dropping family-owned stock corporations, state-owned enterprises, and firms in industries exempt from codetermination). We report all procedures in detail Appendix Section B.2.

Since the BvD data start to have sufficient coverage in the late 1990s, we will not observe outcome variables around the time of incorporation, but at more mature stages. We have around six firm-year observations for the median firm, and we generally use all available observations per firm for increased precision. We report additional summary statistics on the timing of our observations in Appendix Table D.1.

Matched Employer-Employee Data: Orbis-ADIAB

We study worker-level outcomes with administrative employer-employee data from IAB merged with BvD Orbis firm-level data. Based on the Orbis-ADIAB data, we measure effects on wages, pay premia, rent sharing, employment, worker turnover, as well as skill and occupational structure. The IAB data go back further in time than the BvD data (in principle back to 1975). The matching was conducted via establishment-level record linkage from 2006 to 2014. The match rate for stock corporations is the highest among all legal forms at 70.34 percent (see our summary in Appendix Section B.1.1 and Antoni et al., 2018).

Entrants & Exits: Mannheim Enterprise Panel (MUP)

We use firm panel data from the Mannheim Enterprise Panel (MUP Bersch et al., 2014) to comprehensively study incorporations and exits from 1991 onward (provided by Creditreform e.V., Germany's largest credit rating agency, based on official registers). However, the MUP data do not contain ownership information or comprehensive information on our core outcome variables, so we cannot apply our sample restrictions or study other outcomes in the early years.

Worker Supervisory Board Representation: Hoppenstedt Aktienführer

While our sample of BvD firms does come with board membership information (and is our main data set for our study of board-level outcomes), it does not differentiate between worker and shareholder representatives. To provide one intervention check that the reform shifts board composition, we draw on the Hoppenstedt Aktienführer covering all *listed* German firms and covering 1979 to 2015.

4.2 Empirical Methodology

Our identification strategy is to exploit the quasi-experiment induced by the 1994 reform, which generates a discontinuity in the mandated presence of workers on the supervisory board of stock corporations at the cutoff date for incorporation. We compare stock corporations incorporated before or after the cutoff date to LLCs (for which the rules were not changed) incorporated before or after the cutoff date.

Difference-in-Differences Regression Specification

We estimate the following difference-in-differences specification for outcome Y_{ft} of firm f in year t, stacking firm panel data:

$$Y_{ft} = \alpha + \sigma \cdot \mathbb{1}(\text{IncDate}_f < 0) \times \text{StockC}_f + \gamma \cdot \mathbb{1}(\text{IncDate}_f < 0) + \delta \cdot \text{StockC}_f + X'_{ft}\beta + \epsilon_{ft}, \quad (9)$$

where IncDate_f is firm f's incorporation date in event time (i.e. relative to August 10, 1994), and StockC_f is an indicator for stock corporations. The parameter of interest σ is the coefficient on the interaction of the indicator for incorporation before August 10, 1994 with the indicator for stock corporations, thereby capturing the effect of the law-mandated presence of workers on the supervisory board that was relaxed after August 10, 1994. The specification includes a baseline effect for incorporation before August 10, 1994, $\mathbb{1}(\text{IncDate}_f < 0)$, regardless of corporation type. This will capture, e.g., differences in the business cycle at the time of incorporation. The specification also includes a baseline effect for stock corporations, StockC_f, regardless of incorporation date, absorbing overall differences between stock corporations and LLCs.

Identification Assumption

Our identification assumption is thus *not* that stock corporations and LLCs do not differ: LLCs and stock corporations (see control means in our regression tables) will differ along a number of dimensions (including codetermination rules). Instead, our design relies on the assumption that the difference between slightly older versus younger stock corporations would not differ from the corresponding difference for LLCs, were it not for the 1994 reform that changed the codetermination mandate in young stock corporations (but left these rules unchanged for the three other groups). While we cannot test this assumption in our analysis sample (because of the reform), we implement placebo exercises to test for such differences in time periods without actual cohort- and legal form-specific reforms, described below.

Specifications

In our main specifications, we focus on corporations incorporated within two years before and after the reform, i.e. from August 10, 1992 through August 10, 1996. Unless reported otherwise, we winsorize all outcome variables at the 1 percent level (by year); financial variables are CPI-adjusted with base year 2015. We report results for other bandwidths (between one and three years around August 10, 1994) as well as other winsorization levels (2% and 5%) in Appendix Figures C.1 to C.9. We report specifications without or with control variables X_{ft} that include year effects, industry effects (2-digit NACE designations), and industry-by-year effects.

Multiple Hypothesis Testing

To account for the fact that we assess multiple hypotheses, we also estimate the model jointly for all BvD outcomes in a seemingly unrelated regressions model (Zellner, 1962), testing the joint significance of the treatment effects, i.e. $\sigma^{Y_1} = \sigma^{Y_2} = ... = 0$. We also implement the Romano/Wolf (2005) procedure and report significance levels accounting for multiple hypothesis testing and dependence between hypotheses in Section 8.

Sample Restrictions

We restrict our sample to corporations with 10 or more employees and implement further restrictions detailed in Appendix Section B.2.3 (largely excluding firm types who are legally exempt from codetermination). For all BvD outcome variables, have also confirmed robustness to excluding East Germany.

Intent-To-Treat Specifications

We do not condition on firm size and instead estimate intent-to-treat specifications, since firms incorporated on or after August 10, 1994 can become subject to the one-third mandate if they cross the 500-employee threshold. About 12 percent of shareholder firms in our sample do so. Hence, IV effects of shared governance would scale up our intent-to-treat effects by about 14 percent. Moreover, we will show that the treatment does not affect the probability of crossing this threshold.

Standard Errors

As treatment varies between firms but not within firms over time, we cluster standard errors at the firm level. As described above in Section 4.1, we use multiple firm-year observations per firm in the BvD data for increased precision.

Placebo Reforms

In Appendix Tables D.4 through D.9, we report treatment effects of placebo reforms four and eight years after the actual reform (August 10, 1998 or 2002), for each outcome variable, and consider analogously chosen samples incorporated within a two-year bandwidth around each placebo cutoff (in the the logic of randomization inference as in Ganong/Jäger, 2018). This exercise sheds light on spurious findings due to differential trends (if stock corporations capital intensity grew cohort-by-cohort by more than in LLCs) or lifecycle paths (if stock corporations of a given age are always more capital-intensive than slightly younger ones, compared to that difference within LLCs).

4.3   Threats to Identification

We test for threats to identification of $\hat{\sigma}$ as the causal effect of the codetermination mandate.

Strategic Delay of Incorporation: McCrary Test

Firms might delay incorporation date around the reform cutoff date. Our first check is a visual inspection of the incorporation frequency of stock corporations around the reform cutoff (Figure 2, Panel (a)). This analysis uses the Mannheim Enterprise Panel's comprehensive data on incorporations from 1991 onward. The figure reveals no evidence of a spike in incorporations after August 10, 1994, nor of a missing mass of incorporations leading up to the reform. In the same figure, we formally implement a McCrary (2008) test of continuity of the density against the alternative of a jump in the density function at the reform cutoff date, for which we find no evidence (estimate reported in the figure).

Several institutional features render the aforementioned two types of substitution unlikely a priori, as discussed in Section 3. The grandfathering was an unexpected political compromise, with no clear indication that strategic delay of incorporation would relax the firm's mandate. In addition, the legislative process was finalized within weeks of reaching the compromise, and mandated the day after the law's promulgation as cutoff date.

Composition of New Firms by Legal Form

Relatedly, more firms may substitute into the legal form of stock corporation after the reform than LLCs. Figure 2 Panel (b) plots an indicator for stock corporation legal form against time of incorporation in a sample of both LLCs and stock corporations. The probability of incorporating as a stock corporation did not change discontinuously around the reform cutoff date.

Next, we test less locally whether the reform affected substitution with regard to legal form in our sample window. To this end, we regress an indicator for incorporation as a stock corporation on a post-reform indicator, a time trend, and the interaction of the two, and report results in Appendix Table D.2. There was a small, secular trend towards incorporating as a stock corporation, but we do not detect a level shift or trend change in the post-reform period. These results are consistent with the survey evidence in Albach et al. (1988) that corporations did not view one-third codetermination as an impediment to their operation or incorporation.[13] More broadly, and as discussed in our institutional review in Section 3, this finding also implies that the reform overall, which in part aimed to encourage entrepreneurship in stock corporations, did not appear to have spurred a higher quantity of entrants into this legal form.

Selection Test: Industry Composition

Our design cannot test for selection by studying observables of firms because these are outcome variables potentially affected by the reform. Instead, we study one perhaps less mutable outcome determined at entry and hence indicative of selection, namely industry composition. We consider a firm's industry (17 industry NACE Level-1 codes) as binary outcome variables in specification (9). Figure 2 Panel (c) reports these treatment effects for our main analysis sample in the BvD data (detailed effects in Appendix Table D.3). The reform did not statistically significantly affect the firm composition; the coefficients are also jointly insignificant in an F-test ($p = 0.91$). This test also rules out spurious composition effects from, e.g., business cycles or trends around the reform cutoff. We also study composition effects in the universe of entrants, using the Mannheim Enterprise Panel (MUP). Here, we find no statistically significant effects for 15 of the 17 industries. The two exceptions are a reduction in communications and an increase in finance/insurance firms, and the F-test indicates significant effects ($p < 0.01$). These mild composition effects may not show up in our BvD analysis sample because the MUP data set does not permit us to impose our sample re-

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[13] In a survey of firms incorporated before the 1994 reform, Albach et al. (1988) find that codetermination in the supervisory board is generally not seen as an impediment to incorporation as a stock corporation and firms oppose the abolition of one-third shared governance. As advantages, firms cite information the worker representatives bring to the board room. In a survey sampling stock corporations founded between 1994 and 1996, Schawilye/Gaugler/Keese (1999) find that the top reasons for incorporating as a stock corporation are: (1) image and public relations concerns (high prestige of stock corporations), (2) raising capital, (3) corporate organization, (4) generational change and transfer of ownership.

strictions (government and family ownership, and the employment minimum of ten employees). Alternatively, exits may quickly iron out initial composition differences.

Attrition: Effects on Firm Survival

To rule out selective attrition – and as an outcome variable in its own right – we estimate effects on firm exit. Here, we draw on the comprehensive information on all incorporations in Germany from 1991 onward (from the Mannheim Enterprise Panel, while our main BvD data consist of survivors' panels from future years). Figure 3 Panel (a) plots the survival probability of stock corporations and LLCs separately by incorporation date, and by incorporation date within a two-year window before the reform ("Old") and after ("Young"). Across groups, around 50 percent of firms exist at age 20. We find slightly lower survival rates for younger firms, and for stock corporations compared to LLCs. Importantly, however, the survival rate is not *differentially* lower or higher for old stock corporations, locked into shared governance, compared to slightly younger stock corporations, when compared to the same cohort difference within LLCs. Panel (b) reports these difference-in-differences estimates on the exit probability (one minus survival probability) by firm age. The point estimates, averaged over a 20-years-post-incorporation period, indicate a 3.2ppt increase in the exit probability with a standard error of 3.6ppt, so the confidence interval confidently includes zero. We further identify the effects on the probability of experiencing a bankruptcy, the only other exit-related variable the MUP data offer, using blue squares in Figure 3 Panel (b). After about five years, we find negative point estimates of 3.6ppt (SE 2.5ppt), averaging the effects of shared governance on bankruptcy over a period of 20 years after a firm's incorporation. In sum, we do not find evidence for differential attrition from shared governance.

Validation Test: Realized Shifts in Worker Representation

We verify that the reform shifted worker representation by incorporation date. Figure 1 Panel (c) shows the empirical share of workers on the supervisory board by incorporation date and firm size. We draw on data from the Hoppenstedt Aktienführer, which lists supervisory board members and incorporation year for the subset of listed stock corporations. We restrict the sample to stock corporations founded between 1989 and 1999 for which board composition data is reported.[14] The left (navy-colored) and right (maroon-colored) bar pairs represent corporations incorporated during or before 1994, and, respectively, during or after 1995. For firms smaller than 500 employees (in dark shades, for which the reform changed the rules), there is a stark difference: workers hold 29 percent of the seats in stock corporations incorporated in or before 1994. In sharp contrast, workers

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[14] Specifically, we only consider firm-year observations with data on the role (chair person, worker representative, etc.) of individual supervisory board members is reported for at least one third of the supervisory board. We rely on data from the 1990s due to a structural break in reporting in 2000.

comprise only around 3 percent of the seats in stock corporations founded after 1994. The non-zero worker share is likely due to a small amount of measurement error as the employment concepts for codetermination and in the Hoppenstedt data might differ slightly. The lighter shades report analogous outcomes for very large firms, for whom the mandate did not change and for whom the data show no difference in worker representation – both around one third – confirming that the comparison is not driven by shifts in reporting or data quality after 1994.

5 Composition of Supervisory and Executive Boards

In a first step and reported in Table 2, we study the effects on the demographic composition of the supervisory boards (Panel A, also an intervention check) and executive boards (Panel B). Supervisory board effects also serve as an intervention check. Executive board effects assess whether shared governance affects manager selection at the highest corporate level – a natural transmission channel, as the supervisory board appoints and controls executives. We use the BvD board data set offering a snapshot of board members between 2016 and 2018, with information on names and gender. The BvD data set does not differentiate worker and shareholder representatives, so we cannot separate composition effects into direct effects, and indirect or spillover effects among shareholder representatives. (Board member information is missing for 99% of observations in the earlier waves. Board tenure or turnover cannot be credibly measured. Board size follows size-dependent regulations. Board members are not differentiated in our administrative data.) As with all outcomes, we also test for effects for placebo reforms (Appendix Tables D.4 and D.5). The 1% symmetric winsorization, two-year bandwidth is our main specification; additional bandwidths are studied in Appendix Figures C.1 through C.9.

Gender Composition

We find that shared corporate governance dramatically raises the probability of having at least one woman on the supervisory board by about 15 to 16ppt, relative to a control base of 35 percent. In part, this finding could be driven by codetermination law mandating that at least one worker representative ought to be a woman in firms with more than 50 percent female employment (§ 76 II 4 *BetrVG* 1952). In columns (1) and (2) of Table 2 Panel A, we also detect positive effects of about 5ppt on the share of female supervisory board members, which are not statistically significant. Our placebo analysis in Appendix Table D.4 reveals no corresponding effects for placebo reforms in 1998 and 2002. Turning to executive boards in Panel B, we find no statistically significant effects among executives.

Nobility Titles

Only 0.1 percent of the German population hold nobility titles, a measurable marker of socio-economic status or social capital (Bourdieu, 1986), but 2.3 percent of (control group) stock corporation supervisory board members do. Table 2 Column (4) shows that shared governance reduces

the share by 1.4ppt (SE 0.007), a 60 percent decrease from the 2.3 percent baseline. On the executive board, we find a sharp reduction in any presence by 3ppt (SE 0.014) relative to a 6 percent baseline, a relative effect of -50 percent. Similarly, the share of aristocratic executives is reduced by about 0.1ppt from a control group mean of 0.4 percent, although the estimate is not statistically significant.

Academic Titles

In Germany, doctorate degrees are regularly listed as titles in names, and are another marker of socio-economic status. About 23 percent of supervisory board members hold doctorates or (likely largely nominal) professorial positions ("*Dr.*" and "*Prof.*") in the control stock corporation. In columns (5) and (6) of Table 2 Panel A, we find no significant (3ppt) effect on the probability of at least one supervisory board member holding a doctorate, nor the share of supervisory board members (-3ppt). On the executive board (Panel B), we find marginally significant positive effects on any presence (8ppt (SE 0.05)) and on the share (3ppt (SE 0.017)) – perhaps consistent with worker representatives pushing for executives with more formal qualifications.

6 Production and Capital Intensity

We present the effects on production and capital intensity. Appendix Figures C.3 and C.4 present robustness checks with alternative bandwidths and winsorization levels.

Rank and Distribution Specifications

For our key outcomes (employment, fixed assets, value added per worker, fixed assets per worker, capital share, and value added over revenue), Appendix Tables D.11 through D.13 additionally report linear probability models for being above a series of percentile cutoffs (given by the distribution of the control firms, incorporated on or after the reform, of the same legal form). The first columns add a specification with the percentile rank within a year-by-legal form cell as the outcome variable. Appendix Figure C.10 adds nonparametric plots of cumulative distribution functions by treatment and control group (legal form and incorporation time). These transformed outcomes address the possibility of outliers driving our results. The results mirror those from our main specifications. For the rank outcomes, for example, we detect no effects on employment and find positive effects on fixed assets (marginally significant) and value added per worker, fixed assets per worker, the capital share and the share of sales produced in-house.

6.1 Firm Scale: Output and Inputs

We start with production scale on the output and input sides, reporting effects in Table 3.

Output

We report effects on log revenue and value added in columns (1) and (2) of Table 3. Once we add controls to net out year and industry factors, we cannot reject zero effects on these two scale margins, although the value added effects are positive throughout between 0.04 to 0.11 (with revenue point estimates being more volatile). That is, we find no evidence that shared governance leads to reductions in firm size.

Capital Inputs

We next study the effect of shared governance on capital input measures – our core test of the hold-up hypothesis, according to which increasing worker authority would lead to disinvestment. The BvD data does not report capital expenditures ("investment") but instead contains information on capital stocks. We start with fixed assets, which comprise tangible assets, such as buildings and equipment, and intangible assets, such as patents or trademarks. The point estimates for log fixed assets are around 0.43 to 0.47. Although wide, the confidence intervals exclude zero and stable across specifications, and allow us to rule out effects smaller than +0.056. Our distributional analysis is reported in Appendix Table D.11. We find that these large effects are rationalized by a higher probability of firms having very large fixed assets realizations, which given the skewed distribution of firm scale outcomes, implies large effects on mean asset levels. Importantly, our placebo analysis in Appendix Table D.6 shows no corresponding increases in fixed assets for placebo reforms in 1998 and 2002, implying that our estimates from the 1994 reform identify causal effects (rather than differential trends by legal form and incorporation date). We further study tangible assets, documenting a positive effect around 0.2 (albeit noisily estimated with SEs in the same magnitude) in column(4).

These non-negative capital effects are a central result of our analysis. Either shared governance does not depress capital formation through hold-up. Or, the hold-up mechanism is overturned by counteracting forces crowding in investment.

Employment

As the second core input, we consider employment both in the BvD (all employees including those abroad) and the IAB administrative data (employment subject to German social security, lower levels but more relevant for determination of shared governance, see Annuß, 2019: DrittelbG § 3, Rn. 2). We find small positive, statistically insignificant log employment effects summarized in columns (5) and (6) of Table 3. Our most fine-grained specifications yield point estimates of 0.05 in both the BvD and IAB data (SEs 0.13 and 0.10, respectively), with ranges 0.05–0.13 and 0.04–0.07, respectively. In columns (7) and (8), we study whether old stock corporations are more likely to cross the 500 employee threshold, above which all firm types become subject to shared governance (see Section 3).[15] The estimates rule out that new stock corporations seek to avoid codetermination by remaining small, consistent with survey evidence in Albach et al. (1988). Only 12 percent of the post-cutoff-date-incorporation firms cross this threshold, so an IV interpretation would only slightly scale up our reduced-form effects in our intent to treat design.

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[15] See also our analysis of corporate structure and codetermination at the corporate group level in Appendix Table D.10 and the discussion in the detailed table note. We find no evidence that old stock corporations are more likely to be part of a corporate group with codetermination at the group level.

Workforce Composition and Turnover

In Appendix Table D.14, we study skill and occupational structure of the workforce. In columns (1) through (3), document reductions in the share of low-skilled workers by about 1.1 to 1.4ppt (8 to 11%); while insignificant, our estimates allow us to rule out effects below -3.2ppt. The decreased share of low-skilled workers appears to be offset by a roughly equal increase in medium-skilled workers with an apprenticeship training. Columns (5) to (7) on the occupational structure reports positive point estimates on the share of skilled manual labor (qualified manual occupations, technicians, and engineers), although the confidence intervals for each specification include zero. In Appendix Table D.15, we report effects on tenure and separation rates. Columns (1) and (2) report negative effects of around half a year of tenure in a given cross section of workers (off a baseline of 7.8), which is marginally significant, and a 5 percent effect in logs, which is less precisely estimated. We find a small and statistically insignificant effects on annual separation rates of 1 to 2ppt off a baseline of 0.20 in column (3). In columns (3) to (5), where separately study year-ahead separation rates by tenure, and document a negative and economically large reduction in separation rates among higher-tenured workers (0.2-0.3ppt off a 1.8 base), whereas the decomposition reveals low-tenure workers to have slightly higher separation rates. In our sample, average tenure is high (7-8 years).

Intermediate Inputs, Outsourcing and In-House Production

Higher capital could emerge because firms respond to labor power by outsourcing labor-intensive tasks. The larger (though noisily estimated) value-added effects compared to revenue are indicative of less outsourcing. Indeed, we find a large reduction in intermediate inputs, reported in the last column of Table 3, of between -1.16 to -0.7 (log).[16]

To more directly study outsourcing, in column (7) of Table 4, we estimate effects on the share of sales produced in-house, the ratio of value added to revenue. This share increases by 12 to 17ppt (SE 6 to 7ppt), compared to a control mean of 0.43, and is statistically significant across all specifications. We also assess placebo reforms in 1998 and 2002 and find substantially smaller and statistically insignificant effects, in Appendix Table D.7. We also draw on the IAB matched employer-employee data, and find positive but insignificant estimates on the share of outsourceable occupations in column (7) of Appendix Table D.14 (following Goldschmidt/Schmieder, 2017: classifying as outsourceable occupations in cleaning, food services, security, and logistics).

The reduction of outsourcing is consistent with anecdotal evidence such as the car manufacturer

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[16] We find negative but substantially smaller, insignificant effects for placebo reforms in 1998 and 2002 (D.6), making it unlikely that trends or lifecycle patterns by legal form could fully explain the large 1994 effects. A caveat is that the variable is not well filled in our data.

Opel recently conceding to insource previously outsourced production steps following pressure from worker representatives for investment in existing plants.[17] Reiner Hoffmann, the President of the German Trade Union Confederation, described worker board representatives as crucial for "well-balanced decisions" when it comes to outsourcing.[18]

6.2   Productivity, Capital Intensity and the Capital Share

We next corroborate and dissect the non-negative effect of shared governance on capital formation, studying capital-labor ratios, capital shares and productivity measures, i.e. outcomes normalized by firm-specific scale variables, yielding estimates with more precision. We report these estimates in Table 4. The hold-up model concerns distortion of *productive* capital (e.g., machines) or intangible productive assets (e.g., patents) rather than financial ones or those improving amenities.

Productivity (Value Added per Worker)

To isolate this productive-capital effect in the data, we confirm positive and precisely estimated productivity effects on value added per worker of around 40,000 Euro in column (1) of Table 4. In logs, the effect is large (0.16-0.22, column (2)), although noisily estimated, with confidence intervals including zero across all specifications. The placebo analyses in Appendix Table D.7 show statistically not significant, negative effects on value added per worker for placebo reforms in 1998 and 2002, supporting the research design and substantiating the causal interpretation of our estimates of the 1994 reform.

Capital-Labor Ratio

Shared governance raises the capital-labor ratio by around 72,000 Euro per worker, or 0.4-0.5 in logs (both statistically significant, reported in columns (3) and (4)). Appendix Table D.7 shows substantially smaller and statistically insignificant effects of counterfactual reforms in 1998 and 2002.

Total Factor Productivity

In column (5) of Table 4, we study effects on firm-level log total factor productivity (TFP, detailed in Appendix Section B.3). We would have expected the efficiency measure to be strongly negative

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[17]   Source: "IG Metall vermisst weiter Investitionen bei Opel", *Frankfurter Allgemeine Zeitung*, October 30, 2019.
[18]   Source: Magazin Mitbestimmung, 07/2016, *Hans Böckler Stiftung*.

if the additional capital in shared governance firms were unproductive. We estimate zero or very small effects, which are however noisily estimated. The large confidence intervals also make it difficult to adjudicate those theories reviewed in Section 2 that would have predicted positive TFP effects.

Capital Share

We now study the firm-specific capital shares, calculated as one minus the wage bill divided by value added, in column (6) of Table 4. The income-based capital share serves as an independently computed measure of capital intensity not directly relying on – and therefore providing a validation check of – the BvD capital stock measures. Column (6) reports a large and statistically significant increase in the capital share, of around 7 to 8ppt (control mean: 0.30). In addition, the placebo analyses in Appendix Table D.7 show no effects on the capital share for placebo reforms in 1998 and 2002.

7 Dividing the Pie: Wages, Rent-Sharing, Profitability, and External Finance

Having studied the division of value added between capital and labor in Section 6.2, we turn to income distribution within each factor. We start with wages, as the hold-up view associates higher labor power with lower investment as labor grabs a larger share of the value-added pie. We find no increases in wages or rent sharing in shared governance firms, helping to rationalize the absence of disinvestment effects. We then turn to capital income, profits and financial outcomes.

7.1 Wages and Wage Structure

We begin by studying average wages and the wage distribution, as worker representation has been hypothesized to compress wages and reduce inequality inside the firm (see, e.g., Freeman/Medoff, 1985: p. 82-85), perhaps also indirectly affected by informal norm establishment, as in the case of unions (see, e.g., Western/Rosenfeld, 2011).

In our institutional review in Section 3.3, we clarify that the wage setting institutions, particularly in our study period, are characterized by a substantial degree of wage setting decentralization. Thus, there is scope for wage differentiation at the firm level.

Average Wages

Table 5 reports effects on average log worker earnings in the IAB data at the BvD firm level. We find point estimates ranging between 0.02 and 0.04 with standard errors of about 0.03. The confidence intervals include zero and allow us to reject effects on mean wages larger than 0.10.

AKM Firm Effects

The point estimates on the effects of mean wages could reflect actual pay premia as well as selection effects. Next, we analyze firm pay premia in specifications with worker and firm effects as in Abowd/Kramarz/Margolis (1999), thereby netting out worker selection. We do so based on data from 1990 to 2009, and estimate workplace effects at the firm level (rather than establishment level as in Card/Heining/Kline, 2013), drawing on the full Orbis-ADIAB data set. We find an effect of

shared governance of 0.012 on the firm premium with standard errors of 0.023, ruling out firm pay premia effects above 0.057.

Wage Structure

We also analyze effects on the firm-level wage structure, as average wage effects may mask effects on pay compression (see, e.g., Freeman/Medoff, 1985; Saez/Schoefer/Seim, 2019: p. 82-85). We study log wages at the 25th, 50th, and 75th percentile at the firm level, in columns (3) through (5) of Table 5. We find similar point estimates across these percentiles, ranging from 0.013 to 0.035 in the specification without controls to between 0.022 and 0.046 with industry-year effects, with slightly larger effects at the 75th than the 25th percentile. We also study the share of wages above the social security earnings cap. About 11 percent of workers in control stock corporations have earnings above the cap. This share is not affected, with a point estimate of 0.012 (SE 0.010, rejecting increases above 0.032). Finally, as a measure of within-firm inequality, we consider the within-firm log ratio of the 75th to the 25th percentile wage in column (7). We find a small positive effects of about 0.023 (SE 0.014) and can reject effects above 0.05. All in all, we find no evidence for effects on within-firm wage inequality.

7.2   Rent Sharing

We next assess whether shared governance affects rent sharing, studying the cross-sectional relationship between firm-level wages and productivity. This analysis provides a direct measure of the mechanism by which hold-up is hypothesized to occur. Here, we study persistent productivity differences across firms and relate them to composition-adjusted pay premia measures in the form of AKM firm effects (as in Card et al., 2018: Table 4). A firm's log value added variable is the within-firm average over all its observations, residualized by industry-year (3-digit NACE) fixed effects. By measuring the *cross-sectional* relationship *within* a given firm group (legal form and cohort) between wages at a particular firm (adjusted for composition and estimated with movers) and its productivity, the rent-sharing elasticities will also differ from our estimated treatment effect on labor shares, which compares payroll/value added ratios *across* these four firm groups.

Estimating the Average Firm-Level Rent-Sharing Elasticity for Germany

In Figure 4 Panel (a), we first plot the relationship in the whole sample independent of legal form and incorporation date. We estimate an elasticity of wages to value-added per worker of 0.091 increase in wages (SE 0.004). While no previous worker-and-firm-level rent-sharing estimates for

Germany have been reported (for lack of matched firm and worker data), the elasticity is similar to those documented in other countries (Card et al., 2018; Jäger et al., 2019) and elasticities based on establishment-level survey data in Germany (Gürtzgen, 2009).

Shared Governance and Rent Sharing

In Panel (b), we dissect the rent-sharing elasticity and find no detectable difference in rent sharing induced by shared governance. We find a DiD estimate of -0.012 (SE 0.032), ruling out increases in the rent-sharing elasticity of more than 0.05. The DiD estimate is the coefficient on the interaction between log value added per worker and an indicator for stock corporations incorporated before August 10, 1994. The model also includes base effects for cohort and legal form, each interacted with value added. Consequently, we find no evidence for workers capturing a larger part of output when they have board representation – a result consistent with and perhaps underlying the absence of hold-up patterns in capital formation.

Which Wage Effects Would One Have Expected?

Finally, we assess whether the wage and value added effects can be rationalized in a rent-sharing model, such as the one we outlined in Section 2. In our most fine-grained specification in Table 4, we found an effect on value added per employee of 0.216. Together with the rent-sharing elasticity of 0.091, it implies a wage increase of $0.216 \times 0.091 = 0.019$, supposing bargaining power over wages (i.e. the rent-sharing elasticity) is unaffected. Our actual estimated effect on pay premia of 0.012 (SE 0.023, Table 5) is close to this implied wage pass-through of the productivity effect, which lies within the confidence interval. That is, workers may benefit from the larger capital stock and larger pie through the standard rent-sharing channel, which in itself implies small wage gains. Yet, our group-specific rent-sharing estimates suggest that shared governance does not dramatically change the nature of wage setting.

7.3   Profitability and External Finance

We close our distributional analysis with profits and external finance.

agraphProfitability Observers such as Jensen/Meckling (1979) argue that firm owners would voluntarily adopt shared governance if it were profitable, and hence reject it if there are potential negative effects. We consider 3x2 profit measures: EBITDA (earnings before interest, tax, depreciation and amortization), EBIT (before interest and taxation) and net income (after interest, depreciation,

amortization and taxation, hence available to pay out), divided by either revenue ("profit margin"), or total assets ("return on assets").

Table 6 reports varied effects on these profitability measures (Appendix Table D.8 presents placebo analyses). When measured by EBIT(DA) over revenue, we find a reduction in the profit margin by about 5ppt. EBIT(DA) over total assets yields an order of magnitude smaller effects, insignificant throughout. For net income, we find positive point estimates across specifications and normalizations. For net income over revenue, they are statistically significant (marginally in two of the four positive specification) between 0.07 and 0.11. We find estimates of around 0.02 (SE 0.015) for net income over assets. Overall, we find no evidence of profit reduction.

Debt Structure and Leverage

Table 7 reports effects on various financial outcomes (placebo analyses in Appendix Table D.9). If anything, we find a negative effect of the average cost of debt, measured as interest payment over face value of debt, of 3 to 5ppt (baseline of 0.17 in control stock corporations), which is stable across most specifications although not statistically significant in all but marginally so in one. Hence, external finance suppliers do appear to charge shared governance firms a premium – while there is no increase in leverage (and only an insignificant decrease in liabilities over total assets, perhaps also driven by the denominator). Together, the findings are consistent with shared governance firms running less risky operations (as might be preferred by labor representatives or due to flexible wage polices as in, e.g. Schoefer, 2015), or higher collateral levels, which would be expected given the positive effects on fixed assets. The evidence also does not suggest that owners try to strategically lever up to shield free cash flow from wage bargaining (Matsa, 2010), although our reduced-form net effects cannot isolate one specific mechanism.

Debt Capacity and Financial Constraints

In light of potential effects of labor dynamics on financial constraints (Schoefer, 2015; Matsa, 2018), we complement the analysis of leverage and costs of debt by studying effects on being in the top 50 percent or 20 percent in terms of five indices of financial constraints, debt capacity and distress, constructed from BvD accounting variables, reported in Appendix Table D.16, with details and interpretation in the table note and with the variable construction detailed in Appendix Section B.3 (building on Hillegeist/Keating/Cram/Lundstedt, 2004; Farre-Mensa/Ljungqvist, 2016). Overall, we find no clear effects on measures of constraints and distress, consistent with our findings on capital formation, profits, leverage and realized exists and bankruptcies.

Liquid Assets

In column (5) of Table 7, we also check whether owners leave liquid assets in the firm, perhaps as an indication of a potential severity of free cash flow problems. We find an imprecisely estimated shift from cash (over total assets), perhaps reflecting a shift from liquid to fixed assets, or owners being less willing to store cash inside the firm (rather than in illiquid, fixed assets, consistent with Redeker, 2019).

Impacts on Shareholders

Overall, shared governance does not appear to lower profits or firms' external finance capacity. While it would be fruitful to measure market values and actual dividend payments, the BvD data set does not contain these outcomes (and our stock corporations are typically not publicly traded).

8 Multiple Hypothesis Testing

Since we estimate effects on a number of outcomes and do not reject zero effects along several dimensions, one may wonder whether the statistically significant effects actually represent spurious rejections of the null hypothesis. We address this concern in two ways.

First, we estimate the model jointly for all BvD outcomes in a seemingly unrelated regressions model (Zellner, 1962). Here, we can reject the null hypothesis that all effects are jointly equal to zero ($\sigma^{Y_1} = \sigma^{Y_2} = ... = 0$) with $p < 0.01$.

Second, we implement the Romano/Wolf (2005) procedure to account for multiple hypothesis testing and dependence between hypotheses. For the BvD outcomes, we report these associated significance levels in Appendix Table D.17.[19] A methodological challenge is that the power to reject false null hypotheses can be limited. Still, our main results on production remain largely significant to this stronger correction.

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[19] We cannot simultaneously implement the test on the IAB data, which are on a separate, secure server.

9 Conclusion

We study a reform in Germany that abolished shared governance in some firms but permanently preserved it in others. Studying a series of outcome variables motivated by long-standing theoretical hypotheses, we find that this institution does affect firm outcomes significantly (even after accounting for multiple hypothesis testing). One main result is that the data reject the prominent disinvestment prediction of the hold-up and agency cost views of shared governance (see, e.g., Jensen/Meckling, 1979), at least on net and in the specific context of our natural experiment. That is, if anything, we find that granting labor more formal authority resulted in positive rather than negative effects on capital formation. While the point point estimates are large, the wide confidence intervals are also consistent with smaller positive effects.

Instead of the hold-up view, our findings are consistent with richer perspectives on industrial relations and capital-labor interactions. A simple model extension highlighting the theoretical fragility of the hold-up view has workers be involved in investment decisions too. Here, shared governance can raise rather than lower capital formation. Alternatively, worker representatives may take a longer-term perspective than shareholders or executives. Or, shared governance may facilitate cooperation and long-term contracts between owners, managers and the workforce, perhaps by institutionalizing communication channels and repeated interactions. Our design does not adjudicate between these specific alternative channels. Overall, these richer views are also consistent with anecdotal evidence and stated objectives of worker representatives, for example:

> [S]hared governance per se opposes short-term shareholder interests. The focus [of shared governance] is on the long-term safeguarding of the company through investments and innovations with participation of the employees [...].[20]

> Berthold Huber, Worker Supervisory Board Representative, Siemens

The first sentence hints at another question our study leaves open: how shared governance affects shareholder welfare. While we do not find negative profitability effects, our data do not contain dividend payouts or firm valuations. For example, our capital estimates may reflect yet another agency conflict inside the firm such as facilitating managers' empire building, with resources being locked into fixed capital at the expense of dividends – such that capitalists may not voluntarily adopt codetermination (Jensen/Meckling, 1979).

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[20] Source: Frankfurter Allgemeine Zeitung, November 15, 2004, Nr. 267, p. 13, translation by authors.

We close by reflecting on the institutional context. Our cohort-based design assigns a permanent governance regime from firm entry onward. Reforms that impose shared governance onto incumbents may have different effects. Both our control and treatment groups may have establishment-level works councils, which may amplify the effects of the board mandate (e.g., through information sharing), or may duplicate and attenuate treatment effects. Lastly, our findings may also reflect the overall cooperative labor relations in the German context – and may hence differ from studies of more adversarial contexts such as firm-level unionization in the United States (as in Lee/Mas, 2012). It is also possible that shared governance itself contributed to more cooperative labor relations in Germany (Thelen, 1991).

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Figures

Figure 1.: Corporate Governance and Worker Representation on Supervisory Board

(a) One-Third Worker Representation
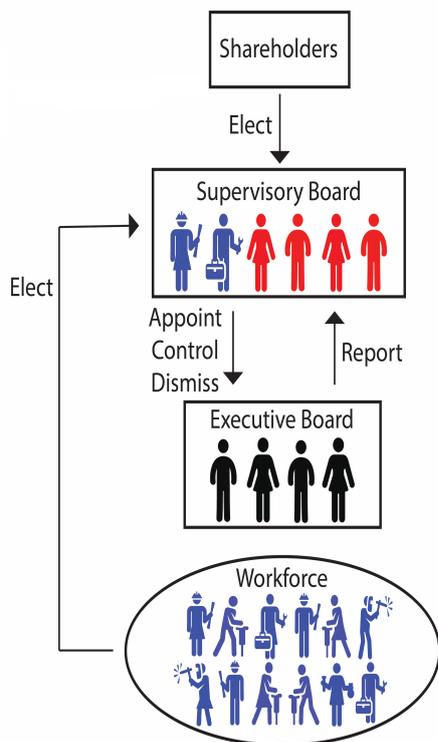
(b) Mandates by Legal Form and Incorporation Date
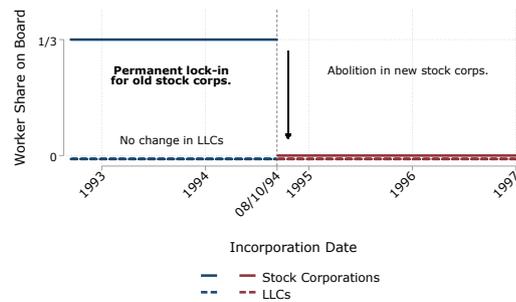
(c) Empirical Share of Worker Board Seats
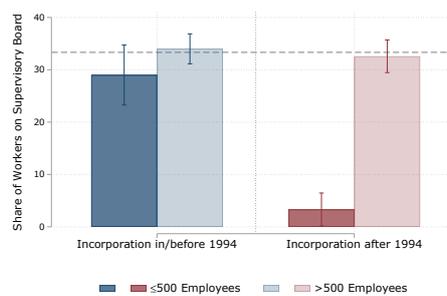
Note: The figure illustrates the supervisory board composition and election in German corporations with and without worker-elected supervisory board directors. Panel (a) illustrates corporate governance with one-third worker representation. Our paper studies variation in whether workers elect a third of the supervisory board members (here depicted in blue). Panel (b) shows the rules as a function of incorporation date and legal form of the firm. Stock corporations incorporated on or after August 10, 1994 as well as limited liability companies (LLCs) have no worker representatives on the supervisory board unless they regularly employ more than 500 workers. Stock corporations incorporated before August 10, 1994 have one-third worker representatives on the supervisory board even when they employ fewer than 500 workers. See Table 1 for rules for larger firms. Panel (c) shows the empirical share of worker seats in listed stock corporations founded between 1989 and 1999 for which the Hoppenstedt Aktienführer, by size and incorporation date. *Source*: Own illustrations and calculations.

Figure 2.: Frequency of Incorporation Around Reform Cutoff Date, and Selection

(a) Frequency of Incorporation and McCrary Test

Discontinuity estimate: 0.2610 (0.4385)
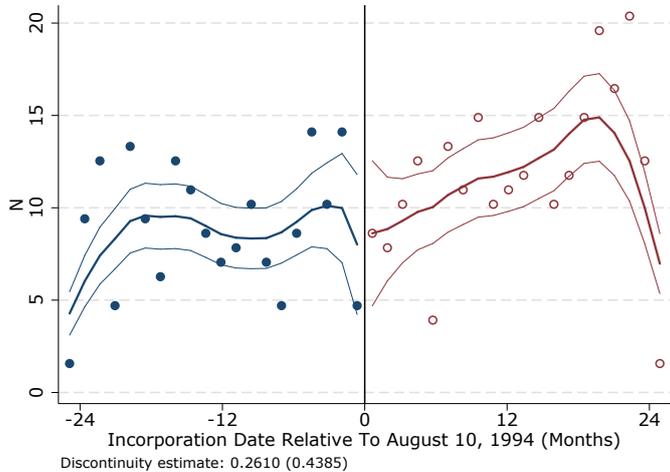
(b) Selection Into Stock Corporation Status

(c) Balance of Industry Composition

■ Orbis Historical   ○ MUP
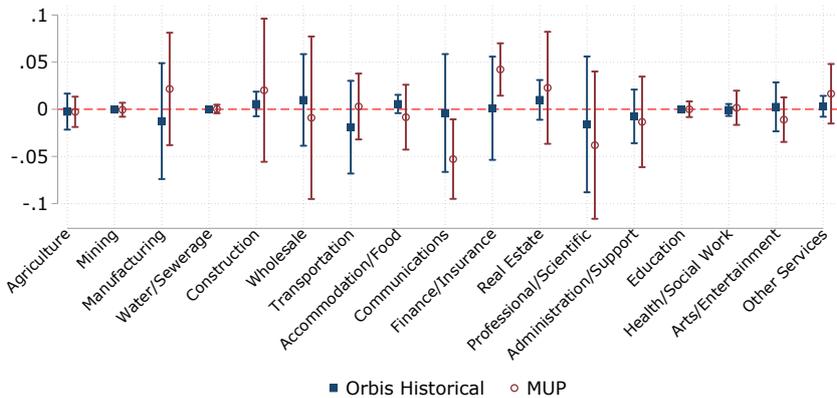
Note: Panel (a) plots the frequency of the incorporation of stock corporations around the August 10, 1994 cutoff date (Mannheim Enterprise Panel), after which shared governance mandate were relaxed. It reports the result of the McCrary (2008) test for a jump in the density at the discontinuity. Panel (b) visualizes the selection into stock corporation status as the share of stock corporation legal form in a sample of all corporations by incorporation date (BvD data), with a formal test in Appendix Table D.2. Panel (c) plots difference-in-differences coefficients and 95% confidence intervals for specifications as in (9) using BvD and MUP data with an an indicator for industry type (NACE Rev. 2 Classification 1) as an outcome variable. F-tests of joint significance show no statistically significant compositional changes ($p = 0.97$) for the BvD data but do show statistically significant changes for the MUP data ($p < 0.01$). Appendix Table D.3 reports the estimates.

Source: Own calculations.

Figure 3.: Firm Survival and Bankruptcy by Incorporation Date and Corporation Type

(a) Survival Probability by Group
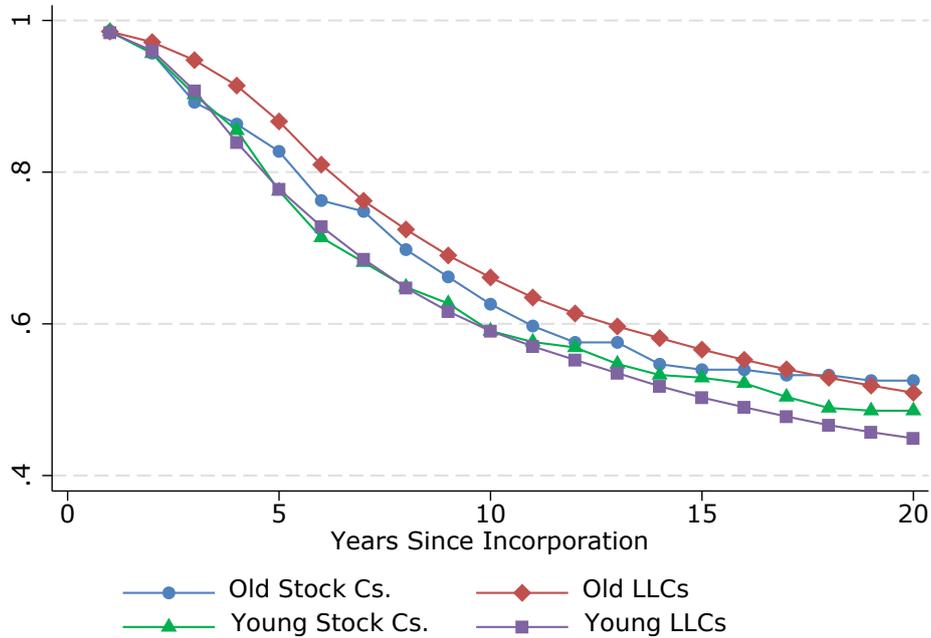
(b) Difference-in-Differences Estimates on Cumulative Exit and Bankruptcy
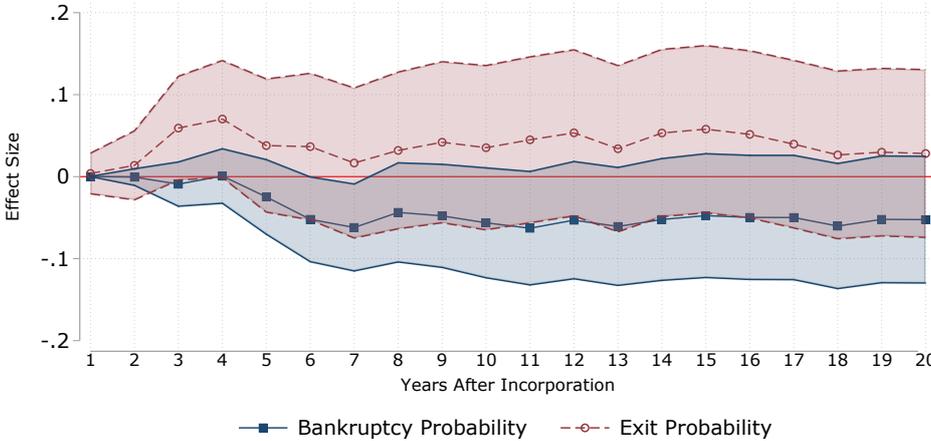
Note: The figure is based on the Mannheim Enterprise Panel. Panel (a) shows survival probabilities of firms incorporated within a two-year window of August 10, 1994 separately for firms incorporated before or after the cutoff date and for shareholder and LLCs. The running variable is time since incorporation in years. Panel (b) shows difference-in-differences point estimates and confidence intervals for cumulative bankruptcy probabilities and for cumulative firm exit probabilities at various years after incorporation.
Source: Own calculations.

Figure 4.: Rent Sharing: Firms' Pay Premia and Value-Added per Worker

(a) Rent Sharing

Slope: 0.091 (SE 0.004)

Mean ln(Value Added per Worker), resid.

(b) Rent Sharing By Legal Form and Incorporation Date

DiD Estimate: –0.012 (SE 0.032)

Mean ln(Value Added per Worker), resid.

- Old Stock Cs.
- New Stock Cs.
- Old LLCs
- New LLCs.
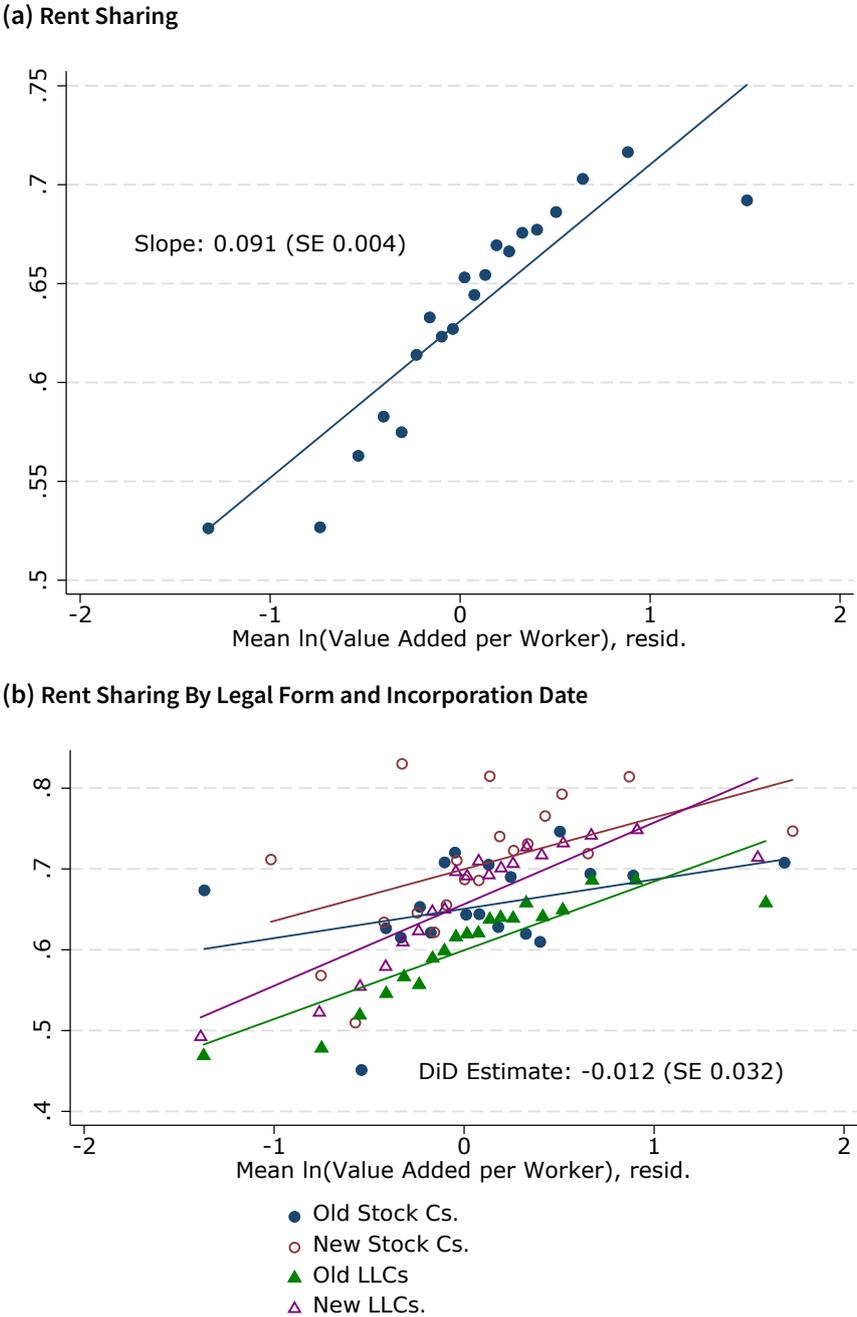
Note: The figure is based on the Orbis-ADIAB data and shows a binned scatter plot of firm's AKM pay premia plotted against ln(Value Added per Worker), which we residualize by year-industry (3-digit NACE) fixed effects.
Source: Own calculations.

Tables

Table 1.: Codetermination Rules: Share of Worker Representatives on Supervisory Board

Firm Size	Stock Corporations		LLCs	
	Incorporated		Incorporated	
	before 08/10/1994	on/after 08/10/1994	before 08/10/1994	on/after 08/10/1994
1 to 500	**1/3**	**0**	**0**	**0**
501 to 2000	1/3		1/3	
$\geq$ 2001	1/2*		1/2*	

Note: The table documents the share of worker representatives on the supervisory board by firm size, legal form, and incorporation date as mandated by codetermination law (*MitbestG* and *DrittelbG*). For firms with more than 2,000 employees, workers have 1/2 of the supervisory board seats although the chairperson, typically a shareholder representative, can break ties. In the mining, coal and steel industry, there is complete parity on the supervisory board between worker and shareholder representatives without tie-breaking by the chair. Stock corporations wholly owned by a family are exempt from the lock-in for smaller corporations incorporated before August 10, 1994. See Section 3 for more information.
Source: Own calculations.

Table 2.: Effect of Shared Governance on Demographic Composition of Corporate Boards

	$\mathbb{1}$(Women > 0) (1)	Share Women (2)	$\mathbb{1}$(Nobility > 0) (3)	Share Nobility (4)	$\mathbb{1}$(PhD/Profs > 0) (5)	Share PhD/Profs (6)
	Panel A: Supervisory Board					
Diff-in-Diff	0.145*	0.047	-0.038	-0.014**	0.029	-0.031
	(0.075)	(0.030)	(0.028)	(0.007)	(0.076)	(0.033)
DiD Industry FE	0.158**	0.053	-0.039	-0.013*	0.029	-0.037
	(0.079)	(0.032)	(0.030)	(0.007)	(0.079)	(0.036)
Control Mean: Stock Cs	0.350	0.125	0.083	0.023	0.570	0.231
″, LLCs	0.567	0.156	0.030	0.006	0.547	0.135
N, Firm-Years	726	726	726	726	726	726
N, Stock Cs	322	322	322	322	322	322
N, LLCs	404	404	404	404	404	404
	Panel B: Executive Board					
Diff-in-Diff	0.038	-0.002	-0.031**	-0.001	0.081*	0.032*
	(0.053)	(0.021)	(0.014)	(0.001)	(0.048)	(0.017)
DiD Industry FE	0.044	-0.004	-0.029**	-0.001	0.083*	0.032*
	(0.053)	(0.022)	(0.014)	(0.001)	(0.047)	(0.017)
Control Mean: Stock Cs	0.595	0.160	0.060	0.004	0.312	0.067
″, LLCs	0.425	0.186	0.012	0.001	0.071	0.023
N, Firm-Years	32,578	32,578	32,578	32,578	32,578	32,578
N, Stock Cs	366	366	366	366	366	366
N, LLCs	32,212	32,212	32,212	32,212	32,212	32,212

Note: The table reports the effect of shared governance on the outcomes reported in each column. We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction, Appendix Figures C.1 and C.2 for the specification with industry fixed effects at additional bandwidths and winsorization levels, and Appendix Tables D.4 and D.5 for checks on placebo reforms in 1998 and 2002. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Robust standard errors are reported in parentheses; we do not cluster here as we only have one observation per firm. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table 3.: Effect of Shared Governance on Firm Scale

	Log Revenue	Log Value Added	Log Fixed A.	Log Tang. A.	Log Emp (BvD)	Log Emp (IAB)	$\mathbb{1}$(Emp> 500) (BvD)	$\mathbb{1}$(Emp> 500) (IAB)	Log Intermediate Inputs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Diff-in-Diff	0.537*	0.079	0.434**	0.193	0.132	0.072	0.015	0.018	-0.872*
	(0.300)	(0.223)	(0.219)	(0.244)	(0.133)	(0.140)	(0.032)	(0.033)	(0.514)
DiD	-0.090	0.037	0.427*	0.183	0.108	0.040	0.013	0.018	-1.158**
Year FE	(0.211)	(0.212)	(0.226)	(0.243)	(0.131)	(0.108)	(0.032)	(0.033)	(0.492)
DiD	0.290	0.113	0.466**	0.214	0.096	0.052	0.013	0.017	-0.708
Industry FE	(0.304)	(0.232)	(0.204)	(0.227)	(0.132)	(0.104)	(0.033)	(0.031)	(0.451)
DiD	-0.101	0.091	0.472**	0.229	0.051	0.050	0.007	0.017	-1.015**
Industry-Year FE	(0.199)	(0.198)	(0.212)	(0.219)	(0.127)	(0.104)	(0.033)	(0.031)	(0.429)
Control Mean: Stock Cs	14.018	14.978	13.726	12.605	4.288	4.071	0.142	0.137	14.695
″, LLCs	11.059	14.790	12.506	12.200	3.354	3.326	0.022	0.023	14.825
N, Firm-Years	207,418	40,066	114,844	113,291	278,878	289,348	278,878	289,348	22,834
N, Stock Cs	529	246	360	360	616	298	616	298	163
N, LLCs	40,046	8,334	24,625	24,411	45,801	20,268	45,801	20,268	6,022

Note: The table reports the effect of shared governance on the outcomes related to firm scale. We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. Non-indicator outcomes are winsorized at the 1% level by year. See Appendix Section B for more information on the sample construction, Appendix Figure C.3 for the specification with industry-year fixed effects at additional bandwidths and winsorization levels, Appendix Table D.11 for rank and percentile robustness checks for employment and fixed assets, Appendix Table D.6 for checks on placebo reforms in 1998 and 2002. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table 4.: Effect of Shared Governance on Productivity and Capital Intensity

	Value Add. per Emp (1)	Log VA per Emp (2)	Fixed A. per Emp (3)	Log Fixed A. per Emp (4)	TFP (Fixed A.) (5)	Capital Share (6)	Value Added /Revenue (7)
Diff-in-Diff	34.897***	0.163	69.723***	0.400**	-0.009	0.071**	0.163**
	(11.789)	(0.241)	(25.951)	(0.195)	(0.282)	(0.032)	(0.066)
DiD	35.112***	0.159	70.470***	0.434**	0.010	0.071**	0.159**
Year FE	(12.075)	(0.166)	(26.207)	(0.185)	(0.227)	(0.032)	(0.062)
DiD	39.671***	0.218	71.535***	0.436**	-0.034	0.079***	0.167***
Industry FE	(11.153)	(0.233)	(24.333)	(0.191)	(0.179)	(0.029)	(0.058)
DiD	40.064***	0.216	72.547***	0.487***	-0.030	0.081***	0.119**
Industry-Year FE	(12.110)	(0.135)	(25.768)	(0.174)	(0.110)	(0.029)	(0.055)
Control Mean: Stock Cs	81.190	9.863	100.225	9.170	6.849	0.300	0.435
″, LLCs	67.363	10.472	34.436	8.991	7.629	0.257	0.360
N, Firm-Years	40,066	40,066	114,844	114,844	38,135	39,110	27,722
N, Stock Cs	246	246	360	360	240	249	227
N, LLCs	8,334	8,334	24,625	24,625	7,804	8,213	7,086

Note: The table reports the effect of shared governance on the outcomes related to productivity and capital intensity. We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. Non-indicator outcomes are winsorized at the 1% level by year. See Appendix Section B for more information on the sample construction. See Appendix Figure C.4 for the specification with industry-year fixed effects at additional bandwidths and winsorization levels; Appendix Tables D.12 and D.13 for rank and percentile robustness checks for value added per worker, fixed assets per worker, capital share, and value added / revenue; and Appendix Table D.7 for checks on placebo reforms in 1998 and 2002. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Standard errors are clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 5.: Effect of Shared Governance on Wages

	Log Mean Wage	AKM Firm Effects	Log Wage, 25th Pct	Log Med. Wage	Log Wage, 75th Pct	% Above SS Maximum	Within-Firm Wage Premium $\log(\frac{p75}{p25})$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Diff-in-Diff	0.022	0.005	0.013	0.023	0.035	0.009	0.023
	(0.037)	(0.024)	(0.038)	(0.041)	(0.040)	(0.016)	(0.016)
DiD	0.024	0.004	0.011	0.020	0.032	0.010	0.022
Year FE	(0.033)	(0.023)	(0.034)	(0.035)	(0.038)	(0.010)	(0.014)
DiD	0.035	0.011	0.024	0.034	0.048	0.012	0.023*
Industry FE	(0.033)	(0.023)	(0.033)	(0.034)	(0.037)	(0.010)	(0.014)
DiD	0.038	0.012	0.022	0.031	0.046	0.012	0.023
Industry-Year FE	(0.033)	(0.023)	(0.033)	(0.034)	(0.037)	(0.010)	(0.014)
Control Mean: Stock Cs	4.560	0.549	4.233	4.466	4.688	0.109	0.442
″, LLCs	4.313	0.463	4.089	4.252	4.419	0.045	0.326
N, Firm-Years	125,834	36,292	287,789	287,789	287,789	289,348	287,789
N, Stock Cs	285	235	298	298	298	298	298
N, LLCs	18,536	12,894	20,240	20,240	20,240	20,268	20,240

Note: The table reports the effect of shared governance on wages. We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. All outcomes are winsorized at the 1% level by year. See Appendix Section B for more information on the sample construction and Appendix Figure C.8 for specifications at additional bandwidths and winsorization levels. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Standard errors are clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table 6.: Effect of Shared Governance on Profitability

	EBITDA /Revenue (1)	EBIT /Revenue (2)	Net Income /Revenue (3)	EBITDA /Total A. (4)	EBIT /Total A. (5)	Net Income /Total A. (6)
Diff-in-Diff	-0.045	-0.050	0.068*	-0.0006	-0.003	0.018
	(0.029)	(0.033)	(0.037)	(0.019)	(0.018)	(0.016)
DiD	-0.042	-0.049	0.072**	-0.0003	-0.003	0.017
Year FE	(0.029)	(0.032)	(0.037)	(0.019)	(0.018)	(0.016)
DiD	-0.044	-0.050	0.072**	-0.001	-0.001	0.022
Industry FE	(0.029)	(0.032)	(0.037)	(0.019)	(0.018)	(0.015)
DiD	-0.045	-0.054*	0.111*	-0.006	-0.005	0.018
Industry-Year FE	(0.028)	(0.030)	(0.065)	(0.018)	(0.018)	(0.014)
Control Mean: Stock Cs	0.019	-0.024	-0.016	0.085	0.051	0.017
″, LLCs	0.070	0.037	0.012	0.142	0.095	0.054
N, Firm-Years	28,271	28,099	25,550	39,686	39,454	37,505
N, Stock Cs	236	236	234	254	253	252
N, LLCs	7,109	7,097	6,905	8,305	8,290	8,149

Note: The table reports the effect of shared governance on profitability. We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. Outcomes are winsorized at the 1% level by year. See Appendix Section B for more information on the sample construction, Appendix Figure C.7 for the specification with industry-year fixed effects at additional bandwidths and winsorization levels, and Appendix Table D.8 for checks on placebo reforms in 1998 and 2002. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 7.: Effect of Shared Governance on Capital Structure, Leverage, and Cost of Debt

	Liabilites /Total A. (1)	Leverage (2)	Cost of Debt (3)	Long-Term Debt /Total Debt (4)	Cash /Total A. (5)
Diff-in-Diff	-0.025	-0.007	-0.043	-0.005	-0.022
	(0.025)	(0.029)	(0.030)	(0.038)	(0.019)
DiD	-0.024	-0.003	-0.046	-0.011	-0.021
Year FE	(0.025)	(0.029)	(0.029)	(0.037)	(0.019)
DiD	-0.029	-0.023	-0.033	-0.013	-0.022
Industry FE	(0.024)	(0.029)	(0.028)	(0.037)	(0.018)
DiD	-0.025	-0.018	-0.048*	-0.012	-0.023
Industry-Year FE	(0.025)	(0.029)	(0.027)	(0.037)	(0.018)
Control Mean: Stock Cs	0.570	0.278	0.167	0.733	0.175
″, LLCs	0.671	0.372	0.117	0.821	0.158
N, Firm-Years	115,883	68,313	23,970	49,300	113,963
N, Stock Cs	360	330	219	290	361
N, LLCs	24,843	19,424	6,304	15,486	24,578

Note: The table reports the effect of shared governance on capital structure, leverage, and the cost of debt. We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. Outcomes are winsorized at the 1% level by year. See Appendix Section B for more information on the sample construction, Appendix Figure C.9 for specifications at additional bandwidths and winsorization levels, and Appendix Table D.9 for checks on placebo reforms in 1998 and 2002. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Online Appendix of:
Labor in the Boardroom

Simon Jäger, Benjamin Schoefer and Jörg Heining

A Theoretical Appendix

A.1 Baseline Hold-Up Model: Comparative Statics of Investment to Bargaining Power Parameters and

We here formally derive the properties of the comparative static of capital stock choice K^* to worker bargaining power parameters ϕ (in wage setting) and ι (in input choice).

Capital Choice

In period 1, the objective function in the bargaining is:

$$\max_{K} \{\iota \log S^{1W}(\phi, \bar{L}, K) + (1 - \iota) \log S^{1F}(\phi, \bar{L}, K)\}, \tag{A.1}$$

where the surpluses of the parties depend on period 2 Nash bargaining: $S^{1W}(\phi, \bar{L}, K) = \phi S^2(K, \bar{L})$ and $S^{1F}(\phi, \bar{L}, K) = (1 - \phi)S^2(K, \bar{L}) + (c' - c)K$, with $S^2(K, \bar{L}) = F(K, \bar{L}) - b\bar{L} - c'K$. [21]

The optimality condition for K is:

$$\iota \frac{S_K^{1W}}{S^{1W}} + (1 - \iota)\frac{S_K^{1F}}{S^{1F}} = 0. \tag{A.2}$$

Where the subscript K indicates the partial derivative of the function with respect to K. The second-order condition, a property we will use for the comparative statics below and the value of which we define as B, is:

$$\underbrace{\iota \left(\frac{S_{KK}^{1W}S^{1W} - S_K^{1W}S_K^{1W}}{S^{1W}S^{1W}} \right) + (1 - \iota) \left(\frac{S_{KK}^{1F}S^{1F} - S_K^{1F}S_K^{1F}}{S^{1F}S^{1F}} \right)}_{\equiv B} < 0. \tag{A.3}$$

The Effect of Worker Bargaining Power in Wage-Setting, ϕ, on K^*

To characterize the effect of K^* on ϕ totally differentiate first-order condition (A.2) with respect to K^* and ϕ in the neighborhood of K^*:

$$B \times dK^* + \underbrace{\left[\iota \left(\frac{S_{K\phi}^{1W}S^{1W} - S_K^{1W}S_\phi^{1W}}{S^{1W}S^{1W}} \right) + (1 - \iota) \left(\frac{S_{K\phi}^{1F}S^{1F} - S_K^{1W}S_\phi^{1F}}{S^{1F}S^{1F}} \right) \right]}_{\equiv A} d\phi = 0. \tag{A.4}$$

---

[21] Period 2 Nash bargaining allocates surplus so that $S^{2W}(w^*, \bar{L}, K) = \phi S^2(K, \bar{L})$ and $S^{2F}(w^*, \bar{L}, K) = (1 - \phi)S^2(K, \bar{L})$. Period 1 and period 2 surpluses are related as follows: $S^{1W} = S^{2W}$ and $S^{1F} = S^{2F} + (c' - c)K$.

And therefore,

$$\frac{dK^*}{d\phi} = \frac{A}{-B}. \tag{A.5}$$

By SOC (A.3), $-B > 0$. We will now evaluate A and hence the sign of $\frac{dK^*}{d\phi}$.

Note that

$$S^{1W} = \phi S^2 \qquad\qquad S^{1F} = (1-\phi)S^2 + (c'-c)K \tag{A.6}$$

$$S^{1W}_\phi = S^2 \qquad\qquad S^{1F}_\phi = -S^2 \tag{A.7}$$

$$S^{1W}_K = \phi S^2_K \qquad\qquad S^{1F}_K = (1-\phi)S^2_K + (c'-c) \tag{A.8}$$

$$S^{1W}_{K\phi} = S^{1W}_{\phi K} = S^2_K \qquad\qquad S^{1W}_{K\phi} = S^{1F}_{\phi K} = -S^2_K. \tag{A.9}$$

Therefore

$$S^{1W}_{K\phi}S^{1W} - S^{1W}_K S^{1W}_\phi = S^2_K \phi S^2 - \phi S^2_K S^2 = 0 \tag{A.10}$$

(the first parenthesis in A = 0). Recall also that from FOC (A.2), $\frac{S^{1F}_K}{S^{1F}} = -\frac{\iota}{1-\iota}\frac{S^{1W}_K}{S^{1W}}$. Note also that $S^{1F}_\phi = -S^{1W}_\phi$ and $S^{1F}_{K\phi} = -S^{1W}_{K\phi}$. Therefore, A becomes:

$$A = \left[\iota\left(\frac{S^{1W}_{K\phi}S^{1W} - S^{1W}_K S^{1W}_\phi}{S^{1W}S^{1W}}\right) + (1-\iota)\left(\frac{S^{1F}_{K\phi}S^{1F} - S^{1F}_K S^{1F}_\phi}{S^{1F}S^{1F}}\right)\right] \tag{A.11}$$

$$= (1-\iota)\left(\frac{S^{1F}_{K\phi}}{S^{1F}} - \frac{S^{1F}_K}{S^{1F}}\frac{S^{1F}_\phi}{S^{1F}}\right) \tag{A.12}$$

$$= -(1-\iota)\frac{S^{1W}}{S^{1F}}\left(\frac{S^{1W}_{K\phi}}{S^{1W}} + \frac{\iota}{1-\iota}\frac{S^{1W}_K}{S^{1W}}\frac{S^{1W}_\phi}{S^{1W}}\right) \tag{A.13}$$

$$= -(1-\iota)\frac{S^{1W}}{S^{1F}}\left(\frac{1}{1-\iota}\frac{S^2_K}{S^{1W}}\right) \tag{A.14}$$

$$= -\left[\frac{S^2_K}{(1-\phi)S^2 + (c'-c)K}\right]\frac{(1-\iota)\phi S^2}{(1-\iota)\phi S^2} \tag{A.15}$$

$$< 0, \tag{A.16}$$

provided that $\iota < 1, \phi > 0, S^2 > 0$.

Since $A < 0$ and $-B > 0$, we have now shown that

$$\frac{dK^*}{d\phi} < 0 \tag{A.17}$$

for any level of $\iota < 1$, provided that $\phi > 0$ and $S^2 > 0$.

The Effect of Worker Bargaining Power in Investment, ι, on K^*

We totally differentiate FOC (A.2) with respect to K^* and ι:

$$B \times dK^* + \underbrace{\left[\frac{S_K^{1W}}{S^{1W}} - \frac{S_K^{1F}}{S^{1F}}\right]}_{\equiv C} d\iota = 0. \tag{A.18}$$

so,

$$\frac{dK^*}{d\iota} = \frac{C}{-B}. \tag{A.19}$$

Again by SOC (A.3), $-B > 0$. We will now evaluate the sign of C, which determines the sign of $\frac{dK^*}{d\iota}$.

Recall that from FOC (A.2), $\frac{S^{1F}_K}{S^{1F}} = -\frac{\iota}{1-\iota}\frac{S^{1W}_K}{S^{1W}}$. Therefore, C becomes:

$$C = \left[\frac{S_K^{1W}}{S^{1W}} - \frac{S_K^{1F}}{S^{1F}}\right] \tag{A.20}$$

$$= \left[\frac{S_K^{1W}}{S^{1W}} + \frac{\iota}{1-\iota}\frac{S_K^{1W}}{S^{1W}}\right] \tag{A.21}$$

$$= \frac{1}{1-\iota}\frac{S_K^{1W}}{S^{1W}} \tag{A.22}$$

$$= \frac{1}{1-\iota}\frac{\phi S_K^2}{\phi S^2} \tag{A.23}$$

$$> 0. \tag{A.24}$$

Since $C > 0$ and $-B > 0$,

$$\frac{dK^*}{d\iota} > 0 \tag{A.25}$$

for any level of $1 > \phi > 0$. If $\phi = 0$, i.e workers have no power in setting the wage, then w^* is equal to b and does not depend on K. Therefore, for $\iota = 1$ any K is a solution, while for $\iota < 1$ we have efficiency ($F_K = c$) and K^* does not depend on ι ($\frac{dK^*}{d\iota} = 0$).

A.2 Endogenous Labor

Here, we relax the assumption of exogenous labor and assume instead that labor L is chosen contemporaneously to K with the same bargaining parameter ι. The stage 1 objective function in Nash bargaining is now

$$\max_{K,L}\{\iota \log S^{1W}(\phi, K, L) + (1 - \iota) \log S^{1F}(\phi, K, L)\}, \tag{A.26}$$

where stage-2 surplus is anticipated to be Nash bargained as above. Note that L only enters the surplus of the respective parties through aggregate period-2 surplus: $S^{1W}(\phi, K, L) = \phi S^2(K, L)$ and $S^{1F}(\phi, K, L) = (1-\phi)S^2(K, L) + (c'-c)K$, where $S^2(K, L) = F(K, L) - bL - c'K$. Hence, both parties will agree on choosing the optimal level of L regardless of bargaining powers, given by:

$$\iota \frac{S_L^{1W}}{S^{1W}} + (1-\iota)\frac{S_L^{1F}}{S^{1F}} = 0 \Leftrightarrow S_L^2 \left[\iota \frac{\phi}{U} + (1-\iota)\frac{(1-\phi)}{V} \right] \qquad \Leftrightarrow S_L^2 = 0 \qquad \Leftrightarrow F_K = b. \qquad \text{(A.27)}$$

L^* does not depend on ϕ or ι directly but only through K; for any change in K, L^* adjusts such that $F_L(K, L) = b$ and hence:

$$\frac{dL^*}{dK} = -\frac{F_{LK}}{F_{LL}}. \qquad \text{(A.28)}$$

Moreover, the results (A.17) on $\frac{dK^*}{d\phi} < 0$ and (A.25) on $\frac{dK^*}{d\iota} > 0$ continue to hold in the case with endogenous L. The formulae (A.5) and (A.19) still hold, with B now being a function of the Hessian of the objective function which we can again sign by appealing to the second order condition.[22]

As a result, employment effects inherit the qualitative properties of the capital effects in this extended setting as long as $F_{LK} > 0$. Going forward, we therefore consider the general setting with endogenous labor. Therefore, the results derived for capital effects with fixed labor above correspond to the partial effects $\frac{\partial K}{\partial \phi} = \frac{A}{-B}$ and $\frac{\partial K}{\partial \iota} = \frac{C}{-B}$ in the model with endogenous labor (with the seemingly fixed labor level set to the originally optimal one). However, the total capital effects $\frac{dK}{d\phi} = \frac{A}{-\tilde{B}}$ and $\frac{dK}{d\iota} = \frac{C}{-\tilde{B}}$ (while having the same sign as in the fixed-labor setting) also reflect endogenous adjustment in labor (with $\tilde{B}$ defined in Footnote 22).

A.3 Additional Comparative Statics: Capital-Labor Ratio, and Profits

We now derive the additional comparative statics of profit and the capital labor ratio, and do so in the aforementioned extended model with endogenous labor.

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[22] To see this, take the total derivative of the FOCs (A.2) – now with endogenous labor – and (A.27) with respect to L, K and the parameter of interest. Use the latter to replace dL as a function of dK in the former. This yields (A.4) and (A.18), with B being replaced by

$$\tilde{B} = \left[\frac{\partial^2 \Omega}{\partial L^2} \right]^{-1} \left[\frac{\partial^2 \Omega}{\partial K^2} \frac{\partial^2 \Omega}{\partial L^2} - \frac{\partial^2 \Omega}{\partial K \partial L} \frac{\partial^2 \Omega}{\partial L \partial K} \right]$$

where $\Omega(K, L; \phi, \iota) = \iota \log S^{1W}(\phi, K, L) + (1-\iota) \log S^{1F}(\phi, K, L)$ is the objective function of the bargaining. Note that $\tilde{B} < 0$ by SOC.

The Effect of Worker Bargaining Power on $\frac{K}{L}$

Denote the bargained capital-labor ratio by $R = \frac{K}{L}$. The effect of a parameter $\psi \in \{\phi, \iota\}$ on R is:

$$\frac{dR}{d\psi} = \frac{1}{L}\frac{dK}{d\psi} - \frac{K}{L^2}\frac{dL}{d\psi} \tag{A.29}$$

$$= \frac{1}{L}\left[1 + \frac{K}{L}\frac{F_{LK}}{F_{LL}}\right]\frac{dK}{d\psi}, \tag{A.30}$$

where the second equality uses (A.28). The capital-labor ratio will move in the same direction as capital, sign$(\frac{d\frac{K}{L}}{d\psi})$ = sign$(\frac{dK^*}{d\psi})$, if and only if $F_{LK} < -\frac{L}{K}F_{LL}$, that is if the complementarity between K and L is not too large for the labor response (to the capital increase) to outpace the capital response.

Profits and ϕ

Recall that profits $\pi(\phi, K, L) \equiv S^{1F}(\phi, K, L) = (1-\phi)S^2(K, L) + (c' - c)K$, where $S^2(K, L) = F(K, L) - bL - c'K$. The effect of ϕ on profits is given by

$$\frac{d\pi}{d\phi} = \frac{\partial \pi}{\partial \phi} + \frac{\partial \pi}{\partial K}\frac{\partial K}{\partial \phi} + \frac{\partial \pi}{\partial L}\frac{\partial L}{\partial \phi}. \tag{A.31}$$

First, $\frac{\partial \pi}{\partial \phi} = -S^2$ is the mechanical effect of ϕ i.e. a transfer of surplus from the firm to the workers holding (K, L) fixed.

Second, we consider $\frac{\partial \pi}{\partial K}\frac{\partial K}{\partial \phi}$. Here, we have already shown that $\frac{\partial K}{\partial \phi} < 0$ in Equation (A.17), a case that extends to the endogenous labor setting as shown in the previous section. Since $\frac{\partial \pi}{\partial K}|_{K=K^*} \leq 0$, we find that $\frac{\partial \pi}{\partial K}\frac{\partial K}{\partial \phi} \geq 0$.[23]

Finally, $\frac{\partial \pi}{\partial L} = (1-\phi)S_L^2 = 0$ by FOC (A.27), and therefore $\frac{\partial \pi}{\partial L}\frac{\partial L}{\partial \phi} = 0$.

So overall, we can consider three cases. For $\iota = 0$, $\frac{\partial \pi}{\partial K}|_{K=K^*} = 0$ (see Footnote 23), and we only have the mechanical effect: $\frac{d\pi}{d\phi} = \frac{\partial \pi}{\partial \phi} = -S^2 < 0$.

With $1 > \iota > 0$ there is some attenuation of the negative effect of ϕ on profits, but it is not sufficient

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[23] By FOC (A.2), $\iota\frac{S_K^{1W}}{S^{1W}} + (1-\iota)\frac{1}{\pi}\frac{\partial \pi}{\partial K} = 0$. When $\iota = 0$, it reduces to $\frac{\partial \pi}{\partial K} = 0$. When $\iota = 1$, it reduces to $S_K^{1W}(\phi, K, L) = 0$, which implies $S_K^2(K, L) = 0$ and then $\frac{\partial \pi}{\partial K} = (1-\phi)S_K^2(K, L) + (c' - c) = c' - c < 0$. When $\iota \in (0, 1)$, the FOC implies $\frac{\partial \pi}{\partial K} = -\frac{\iota}{1-\iota}\frac{\pi}{S^{1W}}S_K^{1W} < 0$ since $S_K^{1W}|_{K=K^*} = \phi S_K^2 = \phi(F_K - c') > 0$.

to reverse it: $\left|-S^2\right| > \left|\frac{\partial \pi}{\partial K}\frac{\partial K}{\partial \phi}\right|$.[24]

Finally, for $\iota = 1$, we again only have the mechanical effect, since the effect of ϕ on K^* is inconsequential, as workers always set inputs to maximize $S^{1W} = S^{2W} = \phi S^2$.

So we have

$$\frac{d\pi}{d\phi} = -S^2 \text{ for } \iota \in \{0,1\} \text{ and } -S^2 < \frac{d\pi}{d\phi} < 0 \text{ for } \iota \in (0,1). \tag{A.36}$$

Profits and ι

The effect of ι on profits is given by

$$\frac{d\pi}{d\iota} = \frac{\partial \pi}{\partial \iota} + \frac{\partial \pi}{\partial K}\frac{\partial K}{\partial \iota} + \frac{\partial \pi}{\partial L}\frac{\partial L}{\partial \iota}. \tag{A.37}$$

First, the direct mechanical effect on profits is zero i.e. $\frac{\partial \pi}{\partial \iota} = 0$. Second, evaluating $\frac{\partial \pi}{\partial K}\frac{\partial K}{\partial \iota}$, we find that $\frac{\partial \pi}{\partial K}\big|_{K=K^*} \leq 0$ (see Footnote 23) and $\frac{\partial K}{\partial \iota} > 0$ per Equation (A.25) (which extends to the context with endogenous labor). Finally, $\frac{\partial \pi}{\partial L} = (1-\phi)S_L^2 = 0$ by FOC in Equation (A.27). So we have (for $\iota > 0$):[25]

$$\frac{d\pi}{d\iota} < 0. \tag{A.38}$$

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[24] This derivation is easier using the notation $S_K^{1F} = \frac{\partial \pi}{\partial K}$.

$$\frac{d\pi}{d\phi} < 0 \iff -S^2 + S_K^{1F}\frac{\partial K}{\partial \phi} + S_L^{1F}\frac{\partial L}{\partial \phi} < 0 \iff -S^2 + S_K^{1F}\frac{S_K^2}{S^{1F}B} + 0 < 0 \iff B < \frac{S_K^{1F}}{S^{1F}}\frac{S_K^2}{S^2} \tag{A.32}$$

$$\iff \iota\left(\frac{S_{KK}^{1W}S^{1W} - S_K^{1W}S_K^{1W}}{S^{1W}S^{1W}}\right) + (1-\iota)\left(\frac{S_{KK}^{1F}S^{1F} - S_K^{1F}S_K^{1F}}{S^{1F}S^{1F}}\right) < \frac{S_K^{1F}}{S^{1F}}\frac{S_K^2}{S^2}, \tag{A.33}$$

where the second implication uses Equations (A.5) and (A.15), and in the second line we use the definition of B in (A.3). Recall that from FOC (A.2), $\frac{S_K^{1F}}{S^{1F}} = -\frac{\iota}{1-\iota}\frac{S_K^{1W}}{S^{1W}} = -\frac{\iota}{1-\iota}\frac{S_K^2}{S^2}$, that $S^{1W} = \phi S_{KK}^2$ and that $S_{KK}^{1F} = (1-\phi)S_{KK}^2$. After some replacements and rearrangement, the condition becomes:

$$S_{KK}^2\left(\frac{\iota\phi}{S^{1W}} + \frac{(1-\iota)(1-\phi)}{S^{1F}}\right) - \iota\left(\frac{S_K^2}{S^2}\right)\left(\frac{S_K^2}{S^2}\right) - (1-\iota)\left(-\frac{\iota}{1-\iota}\frac{S_K^2}{S^2}\right)\left(-\frac{\iota}{1-\iota}\frac{S_K^2}{S^2}\right) < -\frac{\iota}{1-\iota}\frac{S_K^2}{S^2}\frac{S_K^2}{S^2} \tag{A.34}$$

$$S_{KK}^2\left(\frac{\iota\phi}{S^{1W}} + \frac{(1-\iota)(1-\phi)}{S^{1F}}\right) - \frac{\iota}{1-\iota}\frac{S_K^2}{S^2}\frac{S_K^2}{S^2} < -\frac{\iota}{1-\iota}\frac{S_K^2}{S^2}\frac{S_K^2}{S^2}. \tag{A.35}$$

Given that at the optimum $S_{KK}^2 < 0$ and the parenthetical term is positive, the condition holds.

[25] Due to the envelope theorem $\frac{d\pi}{d\iota} = 0$ out of an initial level of $\iota = 0$.

B   Data Appendix

B.1   Data Construction

B.1.1   Versions of Bureau van Dijk Orbis Data

This section details the construction of our main data set. To construct the most comprehensive data set of firms' financial information, we draw on several versions of the Bureau van Dijk Orbis data set. **Bureau van Dijk WRDS** data sets are the Orbis data sets pulled from Wharton Research Data Services. **Orbis Historical** data sets have information on additional firms beyond those still included in the BvD data. **EBDC** data sets also have information on firms beyond the 10 years available from BvD and are based on data by the LMU-ifo Economics & Business Data Center (EBDC). **Dafne** is a database by Bureau van Dijk with additional information on German firms. Specifically, we draw on the following data sets: Interpreted through the lens of our model

1. **Orbis Historical, legal information**, which contains date of incorporation and corporation type,
2. **Orbis Historical, contact information**, which contains firm location,
3. **Orbis Historical, industry classification**, which contains various industry classifications, including NACE Rev. 2,
4. **Orbis Historical, financial information**, which contains data from income statements and balance sheets,
5. **Orbis Historical, ownership information**, which contains information on shareholders and ultimate owners,
6. **Bureau van Dijk WRDS, ownership**, which also contains information on shareholders and ultimate owners,
7. **Bureau van Dijk WRDS, industry classification**, which contains various industry classifications, including NACE Rev. 2,
8. **Bureau van Dijk WRDS, managers**, which contains information on members of supervisory and executive boards,
9. **EBDC, financial and contact information**, which contains the date of incorporation, corporation type, industry classifications, and information from income statements and balance sheets.
10. **Dafne, trade register entry information**, which contains the date of the firm's first entry into the German Trade Register (*Handelregister*) in the Dafne data set.

B.1.2   Preparing the Financial Data Sets

We begin by identifying the ID numbers of firms incorporated from 1990 through 1999 in both the Orbis Historical and EBDC financial data sets.

We then de-duplicate the financial data for these firms so that there is one observation per year for each firm:

1. Unconsolidated reports take precedence over consolidated reports.
2. If the firm-year has an unconsolidated statement with a consolidated companion (consolidation code: U2) and an unconsolidated statement without a consolidated companion (consolidation code: U1), take the latter.
3. If there are two unconsolidated statements of the same type, take the one that is filed as an annual report.
4. If there are still duplicates within firm-year, take the statement with the latest date in the year.

For the Orbis Historical financial data, we then merge the Orbis Historical and Bureau van Dijk WRDS industry classification files using the BvD ID, specifically the NACE Rev. 2 designations. If the industry classification is missing from the Orbis Historical file, we fill it in with the Bureau van Dijk WRDS file.

B.1.3   Pooling Orbis Historical and EBDC Financial Data Sets

We then pool the Orbis Historical and EBDC financial data. If a firm-year observation exists in both files and has non-missing information in both, we prioritize the (larger and better filled) Orbis Historical data.

For the industry classifications, this then means that our order of priority for industry classification is Orbis Historical, Bureau van Dijk WRDS, and then EBDC.

B.1.4   Incorporation Date Adjustment

Some firms have different incorporation dates in the Orbis Historical and EBDC data sets. In this case, we take the earlier incorporation date.

The 1994 reform of the Corporation Law stipulates that the incorporation date relevant to the worker representation mandate is the date of entry into the German Trade Register (*Handelsregister*). In the 1990s, the firm's date of trade register entry was often up to a few months after the establishment date of its charter (*Feststellung der Satzung*).

To use the most accurate legally relevant incorporation date, we replace the incorporation date in the Orbis Historical/EBDC data sets with the date of first trade register entry from the Dafne data set if the date of first trade register entry is within one year (365 days) of the firm's assigned incorporation date. If the Dafne date is more than a year before or after the incorporation date in the Orbis Historical/EBDC data sets, we assume that the first trade register entry date reported in the Dafne data set is not the true first entry date.

B.1.5 Board Composition Data

We use information on board composition from the Bureau van Dijk WRDS data set, which is a cross section from 2018 at the individual-position-firm level. We have access to a similar data set from Orbis Historical, but there were fewer firms and observations were often unfilled. After isolating firms incorporated in the 1990s, we take the following steps to adjust the data to the firm level:

1. We label any position with the words "Aufsichtsrat" or "Supervisory Board" as a supervisory board position. Individuals with both supervisory and non-supervisory positions make up only 0.15 percent of the data and are dropped. We can then aggregate the data to the firm-individual level, where each individual is either supervisory or non-supervisory.
2. We calculate **tenure** as the number of years between the individual's earliest appointment date and 2018.
3. We calculate **size** as the number of individuals in supervisory and non-supervisory positions.
4. We label individuals as **a PhD/professor** if their name contains "Prof", "Professor", "Doktor", or "Dr."
5. We label **aristocratic names** as those with "von", "v." "Graf", "Gräfin", "Baron", "Baronin", "Freiherr", "Frhr", "Freifrau", "Frfr", or "zu".
6. We identify **gender** from a gender indicator in the data set.
7. We then are able to aggregate to the firm level and thereby measure shares and presence of various groups in supervisory and non-supervisory boards.

B.1.6 Ownership Data

We use information on ownership, i.e. shareholders, from both the Bureau van Dijk Orbis Historical and the Bureau van Dijk WRDS data sets. The procedure to obtain the state and family ownership conditions in each data set is described below. Using the Orbis Historical data set, we additionally drop firms classified as branches from our analysis.

Bureau van Dijk Orbis Historical

We first obtain shareholder-subsidiary links, which are separated by year into eleven different files for the period 2007 to 2017. We consider both archived and active links and loop over each file.

We use the GUO 50 variable, which identifies the Global Ultimate Owner of the firm that directly or indirectly controls more than 50 percent of the voting stock, to identify shareholders classified as "Public Authorities, States, Governments". These are type-S shareholders in the Orbis Historical database. We then tag all firms whose domestic ultimate owner possessing more than 50 percent of the firm was a type-S shareholder at any point in time. Our state ownership restriction excludes these tagged firms from the analysis.

To construct the 100 percent family ownership variable, we consider **both direct and indirect ownership**, since a firm can assert the same codetermination exception through indirect ownership (i.e.

through an intermediate firm). We can observe the percentage of direct or indirect ownership by year associated to a shareholders BvD ID. First, we drop all firms not classified as AGs or GmbHs. We only consider global ultimate shareholder links classified as families or individuals and obtain their last name. In practice, this is usually the first word of the shareholder name, since the naming convention in the Orbis Historical Ownership files is to order last names first. There are two general exceptions to this that we identified. The first occurs when family names are listed as, e.g. "Familie Porsche". A second exception applies to last names beginning with the word "von". In both of these cases, we simply take the second word in the shareholder name to obtain shareholder last name.

We then aggregate the percentage of direct or indirect ownership by firm, year, and last name. To deal with rounding issues we compute direct or indirect ownership across all shareholders to see if the percentages either add to 100 exactly or to a number between 99.9 (inclusive) and 100. In the second case, if the total for same last name and the total for all shareholders add to the exact same number, we assume there was a rounding error and treat the firm as if it were 100 percent owned by a single family. This is consistent with the procedure we employed for the WRDS data below. We tag the firms whose aggregate direct or indirect ownership percentage by firm, year, and last name equal 100 percent. Our family ownership restriction excludes these tagged firms from the analysis.

In addition to the above, we tag firms classified as "Branch" independently of their status as shareholder or LLCs. These are type-Q shareholders in the Orbis Historical database. Our branch restriction excludes these tagged firms from the analysis.

Bureau van Dijk WRDS

After isolating firms incorporated between 1989 and 1999, we take the following steps to adjust the data to the firm level:

1. A variable contains the share that each shareholder owns in the firm. We convert the non-numerical designations:
   - We remove the symbols $>$, $<$, and $\pm$.
   - We convert the following designations to 100 percent:
     - WO (wholly owned)
     - VE (vessel), which does not appear in our ownership file
     - T (sole trader)
     - FC (foreign company), i.e. marking a foreign firm
   - We convert "NG" (negligible) to 0.01 percent.
   - We convert "MO" (majority-owned) and "CQP1" (50% + 1 share) to 50.01%.
2. We identify **state shareholders** as those with shareholder type S (public authorities, states, governments) or those with "KfW Bankengruppe" in their name. The KfW is a German state-owned development bank. We consider the total share owned by these shareholders as the **proportion state-owned** in the firm.
3. We define family ownership in two ways:

a) If there is only one shareholder, and that shareholder is of shareholder type I (one or more known individuals or families), then the firm is defined as fully family-owned.

b) Take the last name of all shareholders of shareholder type I (one or more known individuals or families). In practice, this is the last word of the shareholder name, since this is either an individual's last name or the family name only (e.g. "Familie Porsche"). Sum the shares owned by each last name for each firm. If a firm has at least 99.99 percent of all shares owned by one last name, then we designate it as fully family-owned. If it has at least 50 percent of all shares owned by one last name, we designate it as partially family-owned.

4. We then sum all shares owned by the state and by individuals, aggregating to the firm level.

B.1.7 Orbis-ADIAB

Next, we describe the construction of the Orbis-ADIAB data from IAB below.

Establishment-History-Panel (BHP) Data

The Establishment History Panel (Betriebs-Historik-Panel, BHP) data contains aggregations of individual social security records by establishment ID. It is composed of cross-sectional data sets since 1975 for West Germany and 1991 for East Germany. Every cross section contains all establishments in Germany with at least one employee subject to social security on June 30th. Since 1999, also establishments consisting solely of one marginal part-time employee are included. The BHP data contains information about the branch of industry and the location of the establishment. Furthermore, there is the number of employees liable to social security per establishment, as well as marginal part-time employees (since 1999), both in total and broken down by various demographic and skill categories.

Integrated Employment Biographies (IEB) Database

The Orbis-ADIAB database contains spells from the Integrated Employment Biographies (IEB), i.e. worker-level information, which for this merged data set is restricted to the years 1990 to 2014. The source is administrative records on employees from the notification process to the social security institutions in Germany as well as from internal processes of the German Federal Employment Agency. Every employer in Germany is obligated to submit at least once a year a notification on each of his employees to the social security institutions. Information submitted includes daily exact information on the start and the end date of employment, along with gender, educational attainment, (qualitative) information on full- or part-time work, occupation, place of residence, and the gross wages paid to the employee for the covered period, among others. If an employee is continuously employed all year, the recorded beginning and end dates of employment are January 1st and December 31st.

Linking the Data

Schild (2016) and Antoni et al. (2018) describe the linking process in detail. The data set was created by linking administrative employer-employee data at the establishment level with Orbis financial and production data at the firm level. In a first step, a cross-walk between BvD company IDs and BHP establishments and hence BHP ID was established by applying records linkage techniques based on firms' names, industry and other characteristics. The match rate for stock corporations, i.e. the legal form affected by the reform we study, is the highest among all legal forms at 70.34 percent (see Schild, 2016; Antoni et al., 2018: who also describe the linking process and the data set more generally). This BvD ID/establishment ID crosswalk is conducted for cross sections from 2006 to 2014. Based on the resulting crosswalk, additional waves of BHP establishment data for previous years were merged.

Preparation of the Linked Data

For the preparation of our final analysis data, we start with the Orbis component of the Orbis-ADIAB data.

1. We exclude all firms with an incorporation date before December 31, 1989. We keep the most recent incorporation date in case there are multiple entries per firm identifier.
2. Our version of the data includes two variables for the incorporation date. One only includes the year of incorporation, while the other contains more detailed information on this date. The detailed variable was extracted from a more recent version of the Orbis database. We restrict the sample to cases in which the year of incorporation in the more recent and detailed variable matches with the year information in the less detailed version of this variable.
3. For the purpose of applying our standard Orbis-based sample restrictions to the pre-Orbis years for which we have IAB matched employer-employee data but no Orbis data (recall that most variables in the Orbis part of the Orbis-ADIAB data are only populated as of 2006, so our ORBIS-ADIAB panel goes back earlier but only for the IAB variables), we extrapolate a given Orbis firm's earliest non-missing Orbis variables to these pre-2006 years. Then, we keep only the firms we observe in the Orbis Historical / WRDS / EBDC / Dafne data (detailed in Appendix Section B.1.1) after we apply our standard sample restrictions and cleaning procedures, which we detail below in Appendix Sections B.2.2 and B.2.3.
4. We aggregate information stemming from the BHP and IEB data to the firm-year level by BvD ID. (For the establishment-level variables, we weight by the establishment's share of total firm employment.)
5. We drop any spells from the worker-level data with earnings of less than 1 Euro per day. We also exclude spells indicating single or lump-sum payments.
6. In order to form occupational groups we rely on the classification introduced by Blossfeld (1987).
7. We construct the firm- and worker-level AKM effects by following Card/Heining/Kline (2013) but relying on the firm level rather than establishment-level information and drawing on information from 1990 to 2009. We also conduct this analysis on the basis of the fuller Orbis-ADIAB firm sample before restricting the sample to the firms observed in our main sample, described in Appendix Section B.1.1.

B.1.8 Additional Data Sources

We draw on two additional, separate data sources.

Firm Panel Data: Mannheim Enterprise Panel (MUP)

We draw on data from the Mannheim Enterprise Panel provided by Zentrum für Europäische Wirt-schaftsforschung (ZEW), Mannheim, a firm panel data set containing information on incorporations and exits (see Bersch et al., 2014: for detailed information). Comprehensive data on incorporations are provided by Creditreform e.V., Germany's largest credit rating agency, based on official registers and are available from 1991 onward for corporations.
We apply the same industry restrictions in the MUP data as in our overall sample as described below in Section B.2.3. Importantly, we cannot apply the same restrictions regarding state and family ownership since such information is not recorded in the data. In addition, we cannot restrict the analysis to firms above the 10 employee threshold as employment is not comprehensively recorded in the relevant sample years.

Hoppenstedt Aktienführer

We also draw on the Hoppenstedt Aktienführer covering all *listed* German firms from 1979 to 2015 including data on worker representatives on firms' supervisory boards.[26] We focus on consolidated statements from firms and drop state-owned enterprises.

B.2 Sample Construction

We describe how we construct our main analysis sample from the merged Orbis Historical / WRDS / EBDC / Dafne data set. Details are below. Broadly, certain nonprofit firms and media organizations are exempt from codetermination (§ 1 (2) *DrittelbG*), so we drop firms in pertinent sectors such as science, education, and charities along with nonprofit firms that we can identify through their legal form in the data. We also drop utilities, rail transportation, and other industries with heavy state involvement. In addition, we drop state-owned firms in other industries, defined as those where a public authority has more than a 50 percent voting share. We also drop the large, formerly state-owned national railway, postal, and telecommunications firms (and their subsidiaries) that were privatized in the mid-1990s (*Deutsche Bahn, Deutsche Post, Deutsche Telekom*). Even before 1994, the law had exempted stock corporations wholly owned by one family from one-third codetermination so that such firms were not affected by the 1994 reform. While family links between individuals are not listed in the data, we attempt to drop such family stock corporations – regardless of their

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[26] The historical Hoppenstedt Aktienführer data have been digitized through a project by the German Research Foundation (*DFG*) and were retrieved from https://digi.bib.uni-mannheim.de/aktienfuehrer/.

incorporation date – by dropping firms wholly owned explicitly by one family or by individuals that share the same last name.[27]

B.2.1 Corporation Type

Before cleaning, we keep all firms ever labeled as one of the following corporation types:

Stock corporations

  – *Aktiengesellschaft* (Public limited company)
  – *KGaA* (Limited partnership by shares)
  – *GmbH & Co. KGaA* (Limited liability company and partnership by shares)

Limited liability companies (LLCs)

  – *GmbH* (Limited liability company)
  – *GmbH & Co. KG* (Limited liability company and partnership)

Our standard analysis sample uses all observations where firms are labeled as one of these corporation types, but we keep all observations for all firms labeled as one of these corporations in their earliest observation and at their earliest (pre-trade register entry adjustment) incorporation date.

B.2.2 Sample Cleaning Procedure

After adding the board composition and ownership data sets, we construct our sample as described below, broadly following the criteria in Gopinath et al. (2017) where applicable to our data set. We deviate slightly from the cleaning procedure in Gopinath et al. (2017) in three ways. First, we generally set variable values to missing instead of dropping firm-year observations. Second, for the internal consistency of balance sheet information, we set each of the variable values in the numerator to missing if the values of the ratios are outside of the $[0.999, 1.001]$ interval, as opposed to dropping firm-year observations that are below the 0.1 percentile or above the 99.9 percentile of the distribution. Third, we also set fixed assets, added value, and wage bill to missing if zero or negative. We detail our sample cleaning procedure as follows:

1. Drop if number of months is fewer than 12 or observation year precedes incorporation year
2. Set total assets to missing if zero or negative
3. Set operating revenues to missing if zero or negative
4. Set employment to missing if negative
5. Set employment to missing if greater than 2 million
6. Set sales to missing if negative

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[27] The law's ownership-based definition of family firms is stricter than the typical ownership criterion for family firms based on more than 50% rather than 100% ownership (see, e.g., **?**). The extent to which we miss stock corporations that are wholly owned by one family (or by the state) will increase the share of never-takers in our sample.

7.   Set tangible assets to missing if negative
8.   Set fixed assets to missing if zero or negative
9.   Set added value to missing if zero or negative
10. To check for the for the internal consistency of balance sheet information, we generate the following ratios from BvD variables and set the variables in the numerator to missing if less than 0.999 or greater than 1.001, i.e. if the sum is more than 0.1 percent away from the composite value.

     a)   (Tangible assets + Intangible assets + Other fixed assets) / Fixed assets
     b)   (Stocks + Debtors + Other current assets) / Current assets
     c)   (Fixed assets + Current assets) / Total assets
     d)   (Capital + Other shareholders' funds) / Shareholders' funds
     e)   (Long-term debt + Other non-current liabilities) / Non-current liabilities
     f)   (Loans + Creditors + Other current liabilities) / Current liabilities
     g)   (Non-current liabilities + Current liabilities + Shareholders' funds) / Total shareholders' funds and liabilities
     h)   (EBIT + Depreciation) / EBITDA

11. Set shareholders' funds, total shareholders' funds and liabilities to missing if Total shareholders' funds and liabilities are less than Shareholder's funds
12. Generate the following ratio and set all variables in construction to missing if less than 0.9 or greater than 1.1

     a)   (Total shareholders' funds and liabilities - Shareholders' funds) / (Current liabilities + Non-current liabilities)
     b)   (Total assets - Current liabilities - Non-current liabilities) / Shareholders' funds

13. Set to missing if any of the following is negative:

     a)   Current liabilities
     b)   Non-current liabilities
     c)   Current assets
     d)   Loans
     e)   Creditors
     f)   Other current liabilities
     g)   Long-term debts

14. Set long-term debts and liability variables to missing if long-term debts are larger than total liabilities (Current liabilities + Non-current liabilities)
15. Set to missing if wage bill is negative or zero
16. Set to missing if intangible assets are negative
17. Set to missing if tangible assets are zero or missing
18. Set to missing tangible assets if tangible assets are larger than total assets
19. Set to missing if depreciation is negative
20. Construct operating expenses by subtracting EBIT from Operating revenue. Set operating revenue and EBIT to missing if this value is negative or at or above the 99th percentile.
21. Set PLAT and Extraordinary P/L to missing if Extraordinary P/L is exactly equal to PLAT
22. Generate the following ratios and set variables in the construction to missing if it's less than the 0.1th percentile or 99.9th percentile

     a)   Capital / Wage bill
     b)   Tangible assets / Shareholders' funds
     c)   Total assets / Shareholders' funds

23. Set to missing if Shareholders' funds are negative
24. Set other shareholders' funds to missing if Other shareholders' funds is less than the 0.1th percentile
25. Set operating revenue and material costs to missing if operating revenue - material costs are negative
26. Generate the following ratio and set variables in construction to missing if it's less than the 1st percentile or larger than 1.1
    a) Wage bill / (Operating revenue - Material costs)
27. Set current liabilities, non-current liabilities, long-term debts, and laons to missing if the fraction of total liabilities (Current liabilities + Non-current liabilities) composed of debt (Long-term debt + Loans) is greater than 0 percent but no more than 1 percent.

B.2.3 Sample Restrictions

After cleaning and variable construction, we drop the following industries that are either characterized by heavy state involvement or comprised of non-profit or media firms largely exempt from one-third codetermination (§ 1 (2) *DrittelbG*):[28]

- Electricity, gas, steam and air conditioning supply (NACE 35)
- Water collection, treatment and supply (NACE 36)
- Sewerage (NACE 37)
- Waste collection, treatment and disposal activities; materials recovery (NACE 38)
- Passenger and freight rail transport (NACE 491 and 492)
- Publishing: newspapers and magazines (NACE 5813)
- Broadcasters (NACE 60)
- Scientific Activities (NACE 72)
- Public administration and defence; compulsory social security (NACE 84)
- Education (NACE 85) excluding driving and flying schools (NACE 8553)
- Charities (NACE 87 and 88)
- Activities of membership organisations (NACE 94)
- Activities of households as employers of domestic personnel (NACE 97)
- Undifferentiated goods-and services-producing activities of private households for own use (NACE 98)
- Activities of extraterritorial organisations and bodies (NACE 99)

We then drop firms with more than 50 percent state ownership, as well as Deutsche Telekom, Deutsche Bahn, and Deutsche Post DHL (the formerly state-owned telecommunications, railway and postal service firms that were privatized in the mid-1990s), as well as the subsidiaries of these firms that we can identify in the data. To do so, we drop firms that have a Domestic Ultimate Ownership link indicating more than 50 percent ownership by a government entity.

In a similar fashion, we eliminate fewer than 100 firms from our analysis on the basis of one of the following criteria:

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[28] Specifically, § 1 (2) *DrittelbG* exempts enterprises that predominantly pursue political, coalitional (labor or employer representation), religious, charitable, educational, scientific or artistic goals as well as media organizations.

- Their links to Deutsche Telekom, Deutsche Bahn, or Deutsche Post DHL (where examples include "DB Station & Service Aktiengesellschaft", "Deutsche Telekom Strategic Investments GmbH", "Deutsche Post Grundstücks-Vermietungsgesellschaft mbH", etc.)
- Their contact information indicating their legal residence is outside of Germany (this drops exactly one firm in the Orbis data)
- Subsidiaries of large business groups that we identified ("Daimler AG", etc.)

We also drop stock corporations wholly owned by individuals with the same last name. The reason is that even before 1994, the law always exempted stock corporations wholly owned by one family from one-third codetermination so that such firms were not affected by the 1994 reform. We describe how we identify such family stock corporations in Appendix Section B.1 above.

We then exclude all remaining not-for-profit or firms in the data if we can observe their not-for-profit legal status in their names as non-profits are largely exempt from one-third codetermination (§ 1 (2) *DrittelbG*). In Germany, not-for-profit status can be inferred by observing a letter "g" prefixed to the corporation type "AG" or "GmbH". We thus exclude all firms where we can find either a "gAG" or "gGmbH" string in their name.[29]

Lastly, we drop all firms classified as branches by either the WRDS or the Orbis Historical sources, as well as firms with fewer than 10 employees as locked-in firms with very few employees are exempt from board-level codetermination (**?**: DrittelbG § 1 Rn. 8).

B.3   Variable Construction

B.3.1   Financial Variables

After cleaning, we construct the following financial variables.

- Debt = Loans + Long-term Debt
- Non-Debt Liabilities = Current Liabilities + Non-Current Liabilities - Debt
- Labor Share $= \frac{\text{Wage Bill}}{\text{Value Added}}$
- Net Cash Flow from Financial Activities

$$= \frac{\text{1-Year Change in Capital} + \text{1-Year Change in Debt}}{\text{Total Assets}}$$

- Cost of Debt $= \frac{\text{Interest Paid}}{\text{Debt}}$
- Leverage $= \frac{\text{Debt}}{\text{Debt} + \text{Shareholders' funds}}$
- KZ Index

$$= -1.001909 \left(\frac{\text{Profit after Tax (before Extraordinary Items)} + \text{Depreciation}}{\text{Lagged Tangible Fixed Assets}} \right)$$
$$+ 0.2826389 \left(\frac{\text{Total Assets - Capital} + \text{Market Value of Equity}}{\text{Total Assets}} \right)$$
$$+ 3.139193 \left(\frac{\text{Long Term Debt} + \text{Current Loans}}{\text{Long Term Debt} + \text{Current Loans} + \text{Capital/Shareholder Fund}} \right)$$

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[29] Only few firms carry the "gAG" prefix in our data, therefore our industry restrictions described above are more relevant for excluding firms not subject to codetermination.

$$-39.3678(\frac{\text{Dividends}}{\text{Lagged Tangible Fixed Assets}})$$

$$-3.139193(\frac{\text{Cash}}{\text{Lagged Tangible Fixed Assets}})$$

- – We exclude dividends, which are not included in the BvD data.

- HP Index $= -0.737(\text{Log (Inflation Adjusted) Total Assets})$
  $+ 0.043(\text{Log (Inflation Adjusted) Total Assets})^2 - 0.040(\text{Yeas since Incorporation as AG})$

- WW Index
  $= -0.091(\frac{\text{Profit after Tax (before Extraordinary Items) + Depreciation}}{\text{Total Assets}})$
  $- 0.062(\text{Dummy for Positive Dividend})$
  $+ 0.021(\frac{\text{Long Term Debt}}{\text{Total Assets}})$
  $- 0.044(\text{Log Total Assets})$
  $+ 0.103(\text{Average Industry (similar to 3 digit SIC) level growth in } \frac{\text{Turnover - Lagged Turnover}}{\text{Lagged Turnover}})$
  $- 0.035(\frac{\text{Turnover - Lagged Turnover}}{\text{Lagged Turnover}})$

  - – We exclude dividends, which are not included in the BvD data.

- Z-Score for Public Firms
  $= 0.012(\frac{\text{Working Capital}}{\text{Total Assets}})$
  $+ 0.014(\frac{\text{Other Shareholders Funds}}{\text{Total Assets}})$
  $+ 0.033(\frac{\text{EBIT}}{\text{Total Assets}})$
  $+ 0.006(\frac{\text{Market Value of Equity}}{\text{Total Shareholder Funds and Liabilities - Shareholders Funds}})$
  $+ 0.999(\frac{\text{Turnover}}{\text{Total Assets}})$

- Z-Score for Private Firms
  $= 0.717(\frac{\text{Working Capital}}{\text{Total Assets}})$
  $+ 0.847(\frac{\text{Other Shareholders Funds}}{\text{Total Assets}})$
  $+ 3.107(\frac{\text{EBIT}}{\text{Total Assets}})$
  $+ 0.420(\frac{\text{Shareholders Funds}}{\text{Total Shareholder Funds and Liabilities - Shareholders Funds}})$
  $+ 0.998(\frac{\text{Turnover}}{\text{Total Assets}})$

- Z-Score four variable for Private Firms
  $= 3.25 + 6.56(\frac{\text{Working Capital}}{\text{Total Assets}})$
  $+ 3.26(\frac{\text{Other Shareholders Funds}}{\text{Total Assets}})$
  $+ 6.72(\frac{\text{EBIT}}{\text{Total Assets}})$
  $+ 1.05(\frac{\text{Shareholders Funds}}{\text{Total Shareholder Funds and Liabilities - Shareholders Funds}})$

- O-Score
  $= -1.32$
  $- 0.407(\text{Log (Inflation Adjusted) Total Assets})$
  $+ 6.03(\frac{\text{Total Shareholder Funds and Liabilities - Shareholders Funds}}{\text{Total Assets}})$
  $- 1.43(\frac{\text{Working Capital}}{\text{Total Assets}})$
  $+ 0.0757(\frac{\text{Current Liabilities}}{\text{Current Assets}})$
  $- 2.37(\frac{\text{Profit (Loss) for Period}}{\text{Total Assets}})$

$$- 1.83 \left(\frac{\text{Profit before Taxes + Depreciation}}{\text{Total Shareholder Funds and Liabilities - Shareholders Funds}} \right)$$
$$+ 0.285 (\text{Indicator for (Lagged Profit for Period + Two Period Ago Profit for Period)} < 0)$$
$$- 1.72 (\text{Indicator for (Total Shareholder Funds and Liabilities - Shareholders Funds)} > \text{Total Assets})$$
$$- 0.521 \left(\frac{\text{Profit for Period - Lagged Profit for Period}}{\text{Abs(Profit for Period) + Abs(Lagged Profit for Period)}} \right)$$

- Dummy Low Reserves $= \mathbb{1}_{\{\text{Other Shareholders Funds}<0.1*\text{Capital}\}}$
- Dummy Negative Profit $= \mathbb{1}\{\text{Profit for Period} < 0\}$
- Dummy RE more than 1/2 CE $= \mathbb{1}\{\text{Other Shareholders Funds} \geq 0.5*\text{Capital}\}$
- $\text{Retained Profit Share}_t = \dfrac{\text{Other Shareholders Funds}_{t+1} - \text{Other Shareholders Funds}_t}{\text{Profit for Period}_t}$
- $\text{Retained Profit Share Excluding Profits}_t$
  $= \dfrac{\text{Other Shareholders Funds}_{t+1} - \text{P/L for Period}_{t+1} - \text{Other Shareholders Funds}_t + \text{P/L for Period}_t}{\text{P/L for Period}_t}$
- $\text{Retained Earnings}_t = \dfrac{\text{Other Shareholders Funds}_t}{\text{Total Assets}_t}$
- $\text{Average Debt Maturity}_t = \dfrac{\text{Long Term Debt}_t + \text{Loans}_t}{\text{Loans}_t}$

B.3.2 Firm-Level TFP Construction

Using the sample of firms incorporated five years around the reform cutoff date (i.e. 1989 to 1999), we keep all observations between 2005 and 2015 with non-missing values for industry classification, wage bill, and value-added. We apply the sample restrictions described in Appendix Section B.2.3. We then calculate industry-specific labor shares:
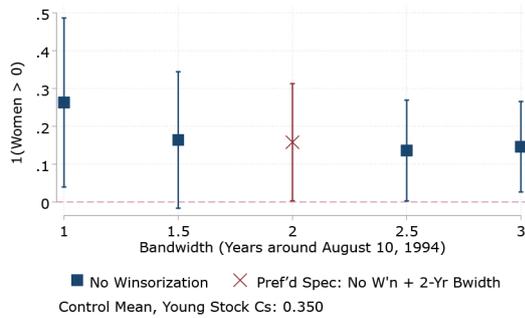
1. For each 2-digit NACE industry i and year t, we calculate the total wage bill and total value-added and divide the first by the second. Call this α_{it}.
2. Within i, we replace any $\alpha_{it} \geq 1$ with the highest α_{it} among all t that is less than 1.
3. We calculate the industry-specific average share α_i across all years t.
4. We then merge these industry-specific values back into the sample and calculate TFP based on fixed assets for every firm f of industry i and year t:

$$\text{TFP}_{ft} = \log(\text{Value Added}_{ft}) - \alpha_i \log(\text{Employment}_{ft}) - (1 - \alpha_i) \log(\text{Fixed Assets}_{ft})$$
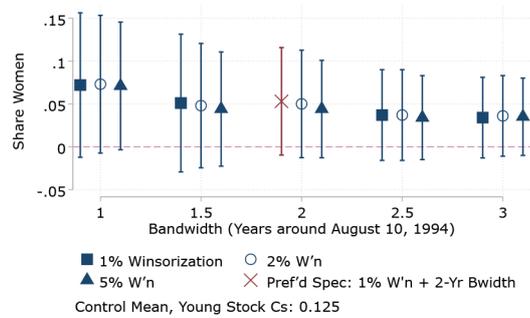
C   Additional Figures

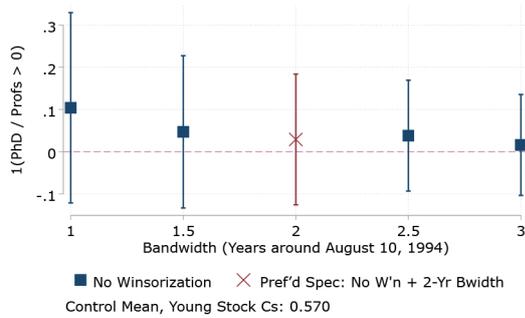**Figure C.1.: Effect of Shared Governance on Supervisory Board Composition**
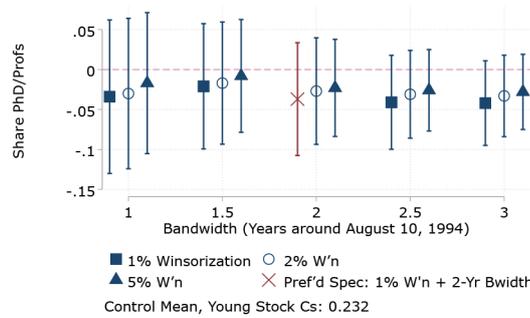
(a) 𝟙**(Woman on Board)**

Control Mean, Young Stock Cs: 0.350

(b) Share Women on Board (0-1.00)

Control Mean, Young Stock Cs: 0.125

(c) 𝟙**(Doctorate Holder on Board)**

Control Mean, Young Stock Cs: 0.570

(d) Share Doctorate Holder on Board (0-1.00)

Control Mean, Young Stock Cs: 0.232

(e) 𝟙**(Aristocrat on Board)**

Control Mean, Young Stock Cs: 0.083

(f) Share Aristocrats on Board (0-1.00)

Control Mean, Young Stock Cs: 0.025

Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on supervisory board composition at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry fixed effects. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. Indicator outcomes are not winsorized. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

Figure C.2.: Effect of Shared Governance on Executive Board Composition

(a) 𝟙(Woman on Board)

(b) Share Women on Board (0-1.00)
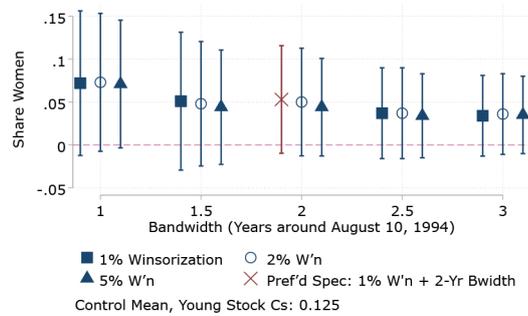
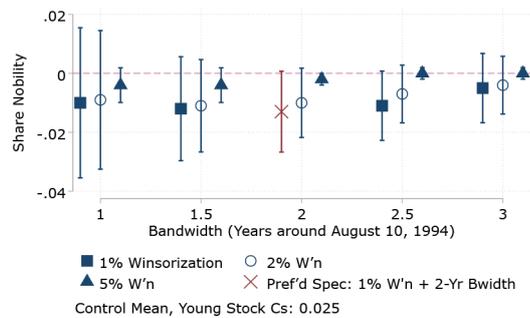**(c)** 𝟙(Doctorate Holder on Board)

(d) Share Doctorate Holder on Board (0-1.00)
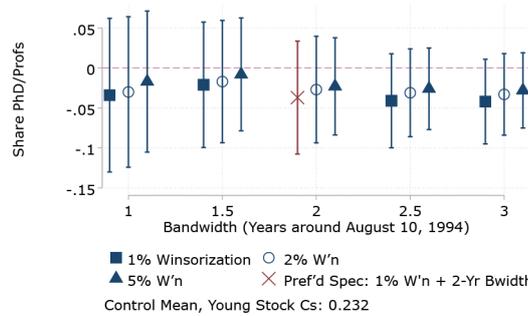
(e) 𝟙(Aristocrat on Board)
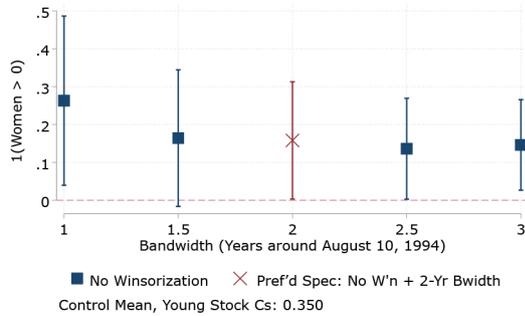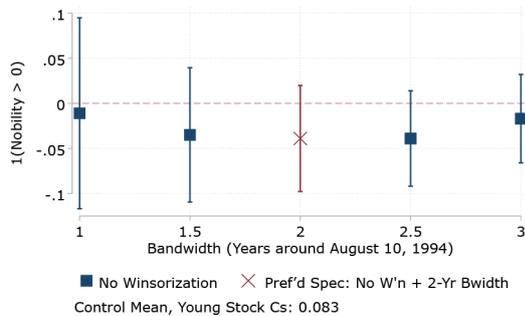
(f) Share Aristocrats on Board (0-1.00)
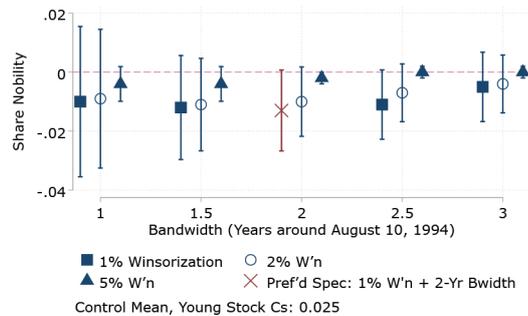
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on executive board composition at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry fixed effects. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. Indicator outcomes are not winsorized. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
*Source*: Own calculations.
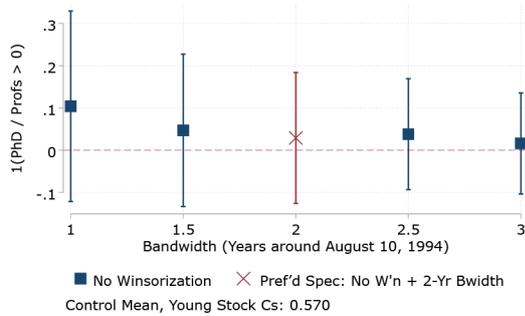
Figure C.3.: Effect of Shared Governance on Firm Scale

(a) Revenue (Log)

Control Mean, Young Stock Cs: 14.134

(b) Value Added (Log)

Control Mean, Young Stock Cs: 15.005

(c) Employment (Log), BvD

Control Mean, Young Stock Cs: 4.415

(d) Employment (Log), IAB

Control Mean, Young Stock Cs: 3.766

(e) $\mathbb{1}(\text{Employment} > 500)$, BvD

Control Mean, Young Stock Cs: 0.142

(f) $\mathbb{1}(\text{Employment} > 500)$, IAB

Control Mean, Young Stock Cs: 0.053

(g) Fixed Assets (Log)

Control Mean, Young Stock Cs: 13.832

(h) Tangible Assets (Log)

Control Mean, Young Stock Cs: 12.672

(i) Intermediate Inputs (Log)

Control Mean, Young Stock Cs: 14.699
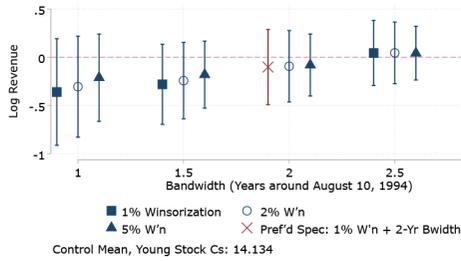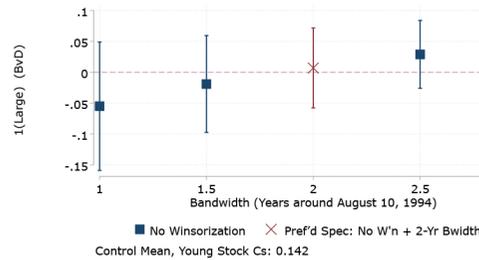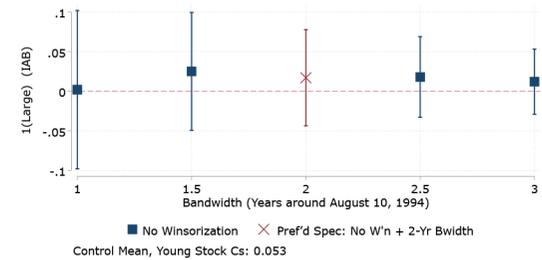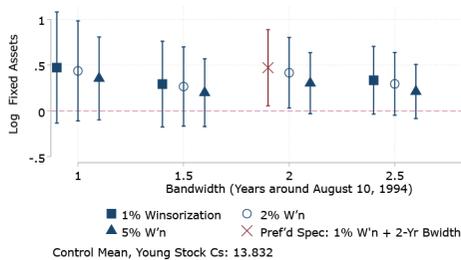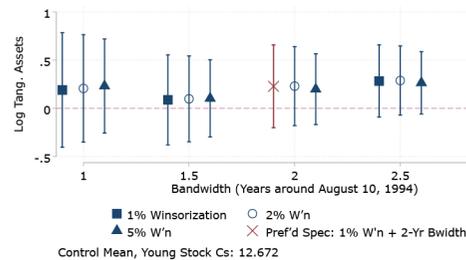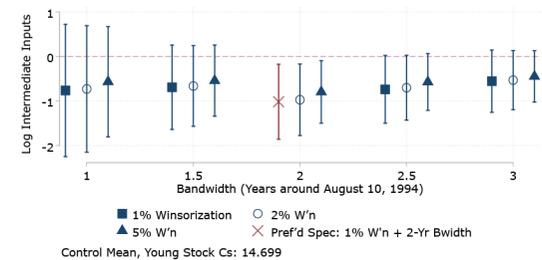
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on firm scale at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. Indicator outcomes are not winsorized. All specifications include industry-by-year fixed effects. The IAB label denotes outcomes from Orbis-ADIAB data. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

Figure C.4.: Effect of Shared Governance on Productivity and Capital Intensity

(a) Value Added per Worker

(b) Value Added per Worker (Log)
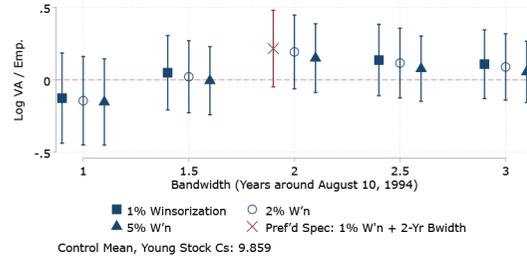
(c) Fixed Assets per Worker

(d) Fixed Assets per Worker (Log)
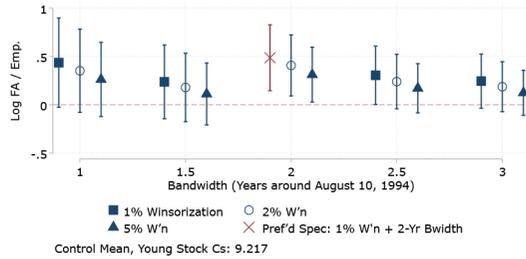
(e) Capital Share
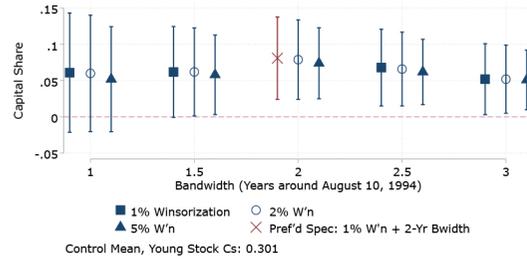
(f) TFP (Fixed Assets)
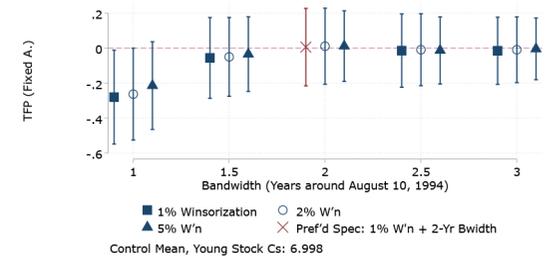
(g) Value Added / Revenue
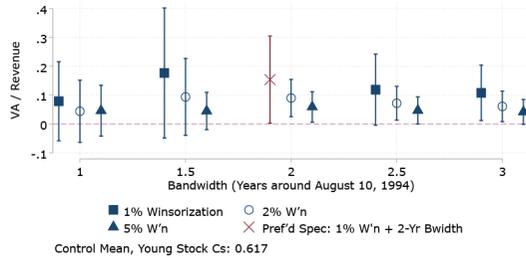
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on productivity at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry-by-year fixed effects. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

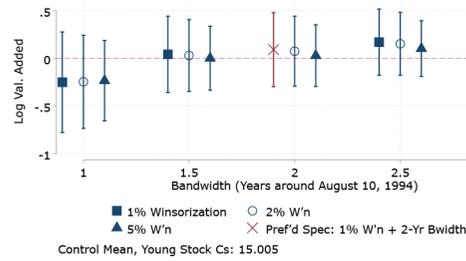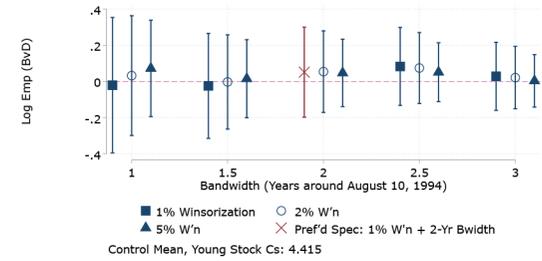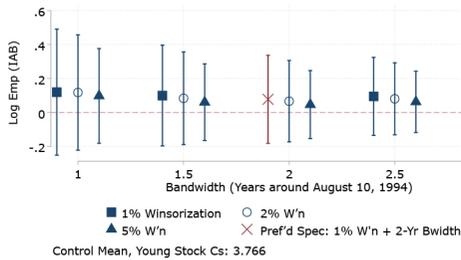## Figure C.5.: Effect of Shared Governance on Skill Structure (Matched Employer-Employee Data)

(a) Share Low Skilled

Control Mean, Young Stock Cs: 0.115

(b) Share Medium Skilled

Control Mean, Young Stock Cs: 0.558

(c) Share High Skilled

Control Mean, Young Stock Cs: 0.299
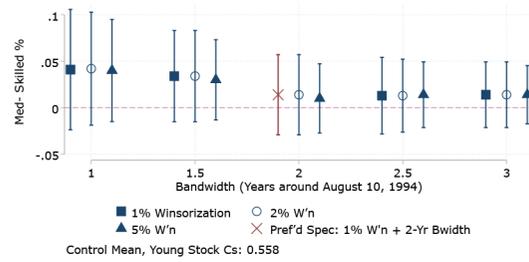
(d) Share Qualified Manual

Control Mean, Young Stock Cs: 0.132
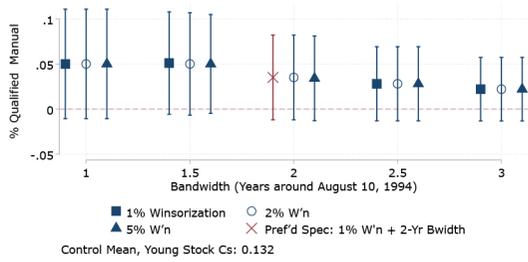
(e) Share Qualified Service
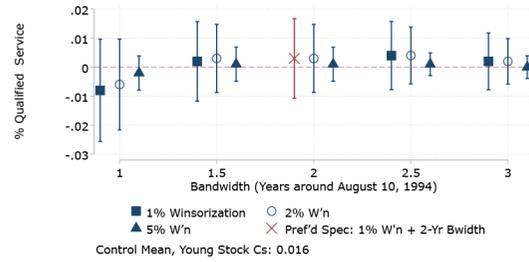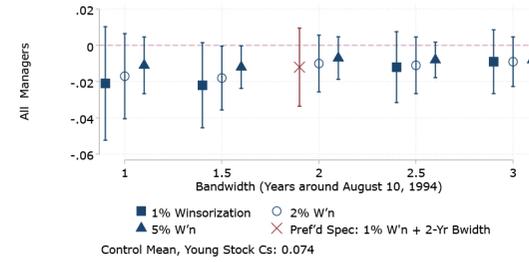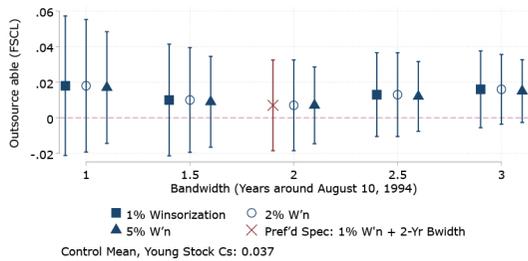
Control Mean, Young Stock Cs: 0.016

(f) Share All Managers

Control Mean, Young Stock Cs: 0.074

(g) Share Outsourceable

Control Mean, Young Stock Cs: 0.037

Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on skill structure at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry-by-year fixed effects. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

Figure C.6.: Effect of Shared Governance on Tenure (Matched Employer-Employee Data)

(a) Tenure
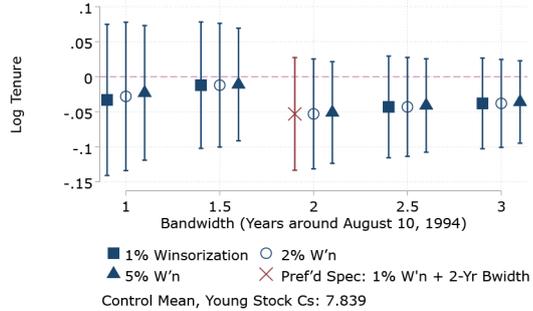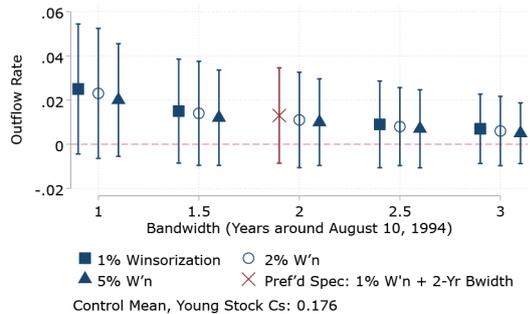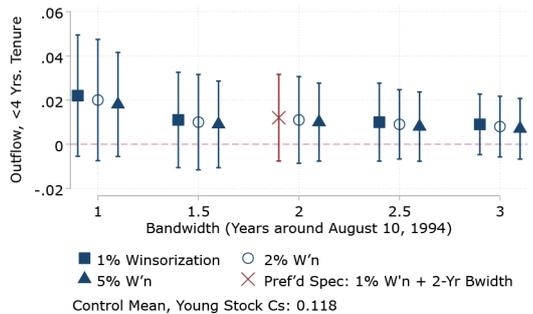
1% Winsorization　○ 2% W'n
▲ 5% W'n　✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 7.379

(b) Tenure (Log)

1% Winsorization　○ 2% W'n
▲ 5% W'n　✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 7.839

(c) Separations: All

1% Winsorization　○ 2% W'n
▲ 5% W'n　✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.176

(d) Separations: < 4 Years

1% Winsorization　○ 2% W'n
▲ 5% W'n　✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.118

(e) Separations: 4-9 Years

1% Winsorization　○ 2% W'n
▲ 5% W'n　✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.043

(f) Separations: $9+$ Years

1% Winsorization　○ 2% W'n
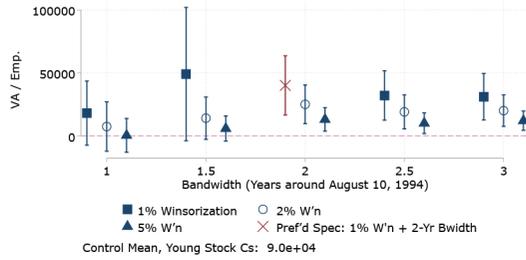▲ 5% W'n　✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.012
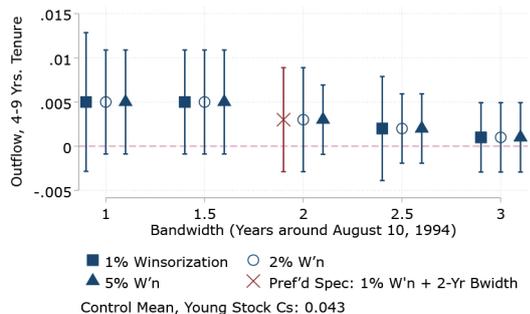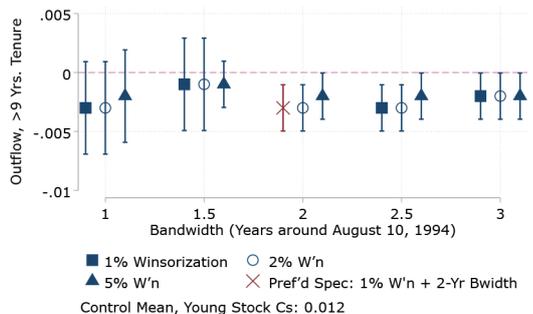
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on tenure at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. Indicator outcomes are not winsorized. All specifications include industry-by-year fixed effects. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

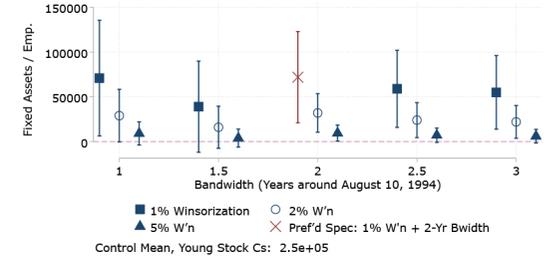## Figure C.7.: Effect of Shared Governance on Profitability

(a) EBITDA/Revenue
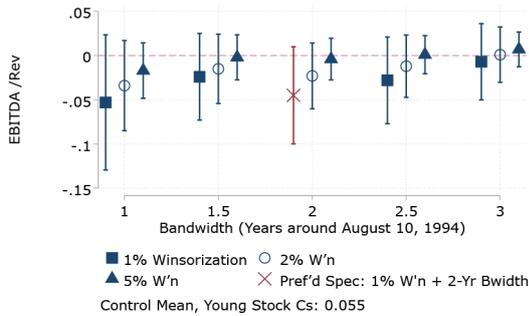
■ 1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.055

(b) EBIT/Revenue

■ 1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: -0.061

(c) Net Income/Revenue

■ 1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: -0.062

(d) EBITDA/Total Assets

■ 1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.081

(e) EBIT/Total Assets

■ 1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.048
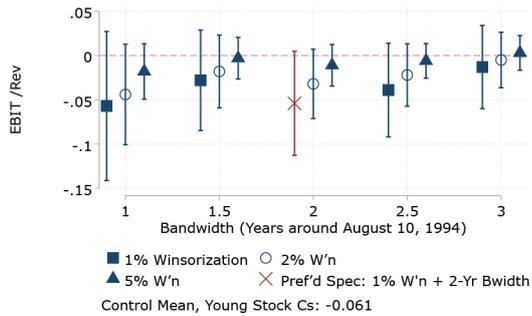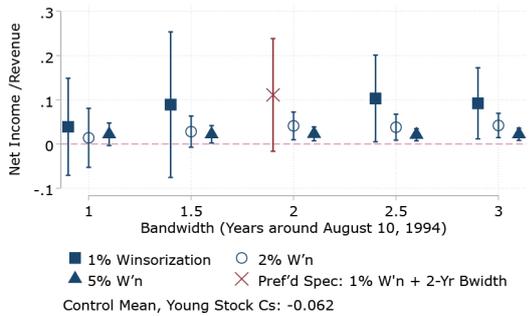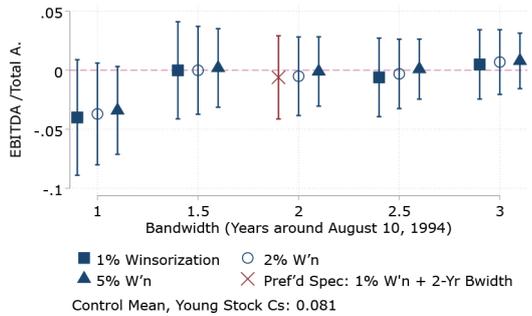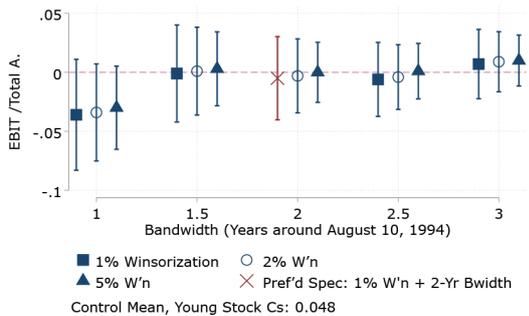
(f) Net Income/Total Assets
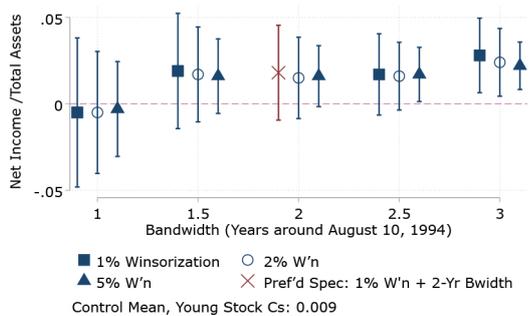
■ 1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.009
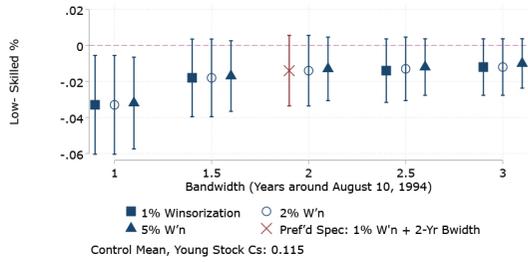
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on profitability at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. All specifications include industry-by-year fixed effects. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

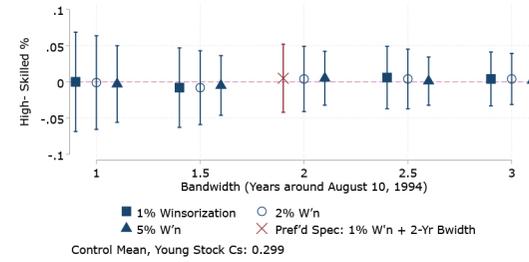**Figure C.8.: Effect of Shared Governance on Wages (Matched Employer-Employee Data)**

(a) Mean Wage (Log)
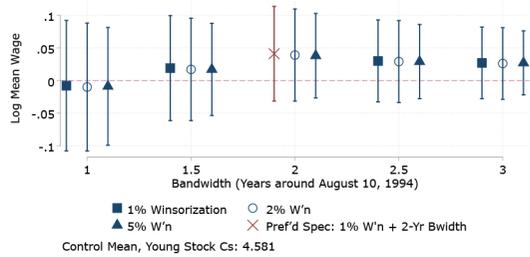
(b) AKM Firm Effects
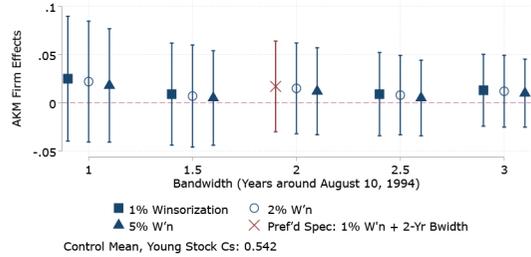
(c) Wage, 25th Pct. (Log)
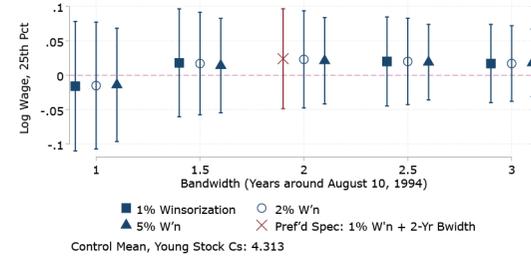
(d) Median Wage (Log)
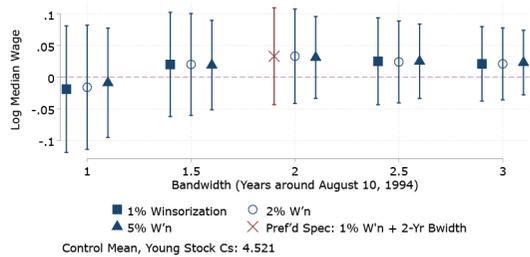
(e) Wage, 75th Pct. (Log)
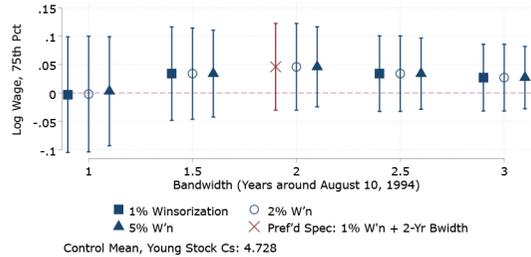
(f) Share Above Social Security Maximum
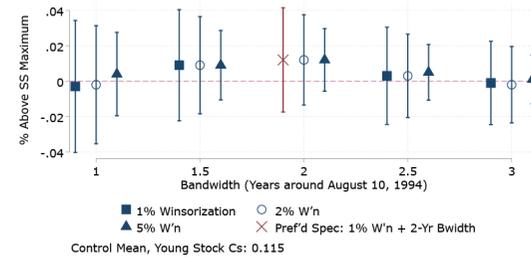
(g) Within-Firm Wage Ratio (Log(p75/p25))
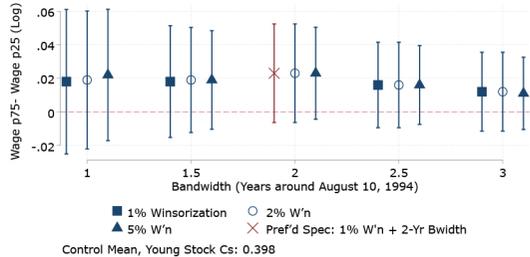
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on wages at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. All specifications include industry-by-year fixed effects. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

Figure C.9.: Effect of Shared Governance on Capital Structure, Leverage, and Cost of Debt

(a) Liabilities / Total Assets
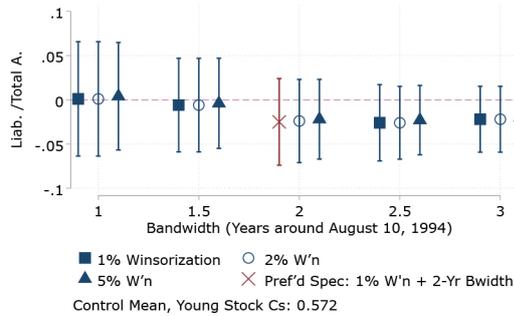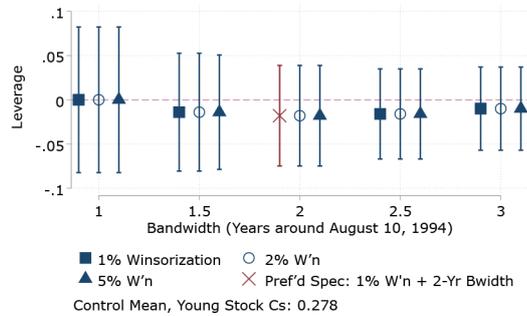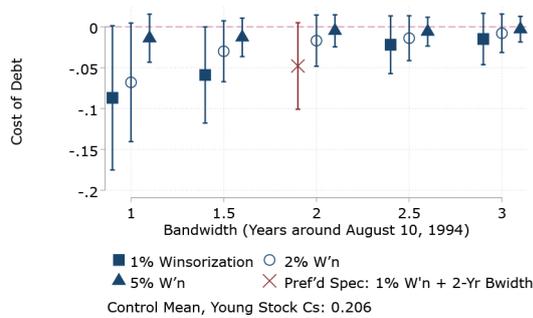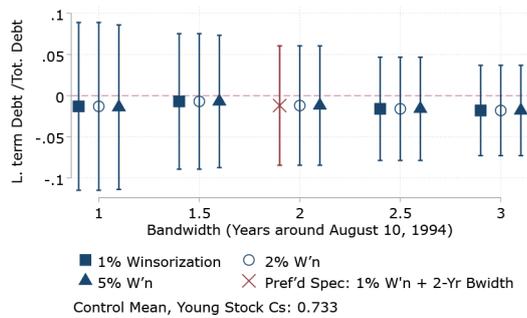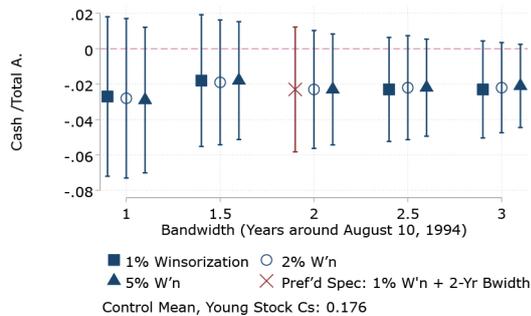
1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.572

(b) Leverage

1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.278

(c) Cost of Debt

1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.206

(d) Long-term Debt / Total Debt

1% Winsorization ○ 2% W'n
▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.733

(e) Cash / Total Assets

1% Winsorization ○ 2% W'n
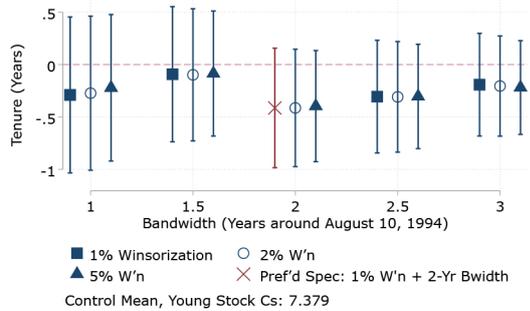▲ 5% W'n ✕ Pref'd Spec: 1% W'n + 2-Yr Bwidth
Control Mean, Young Stock Cs: 0.176

Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on capital structure, leverage, and cost of debt at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization specification. All specifications include industry-by-year fixed effects. The vertical bars denote confidence intervals based on standard errors clustered at the firm level.
Source: Own calculations.

Figure C.10.: Cumulative Distribution Functions of Selected Outcomes

(a) Employment (Log)
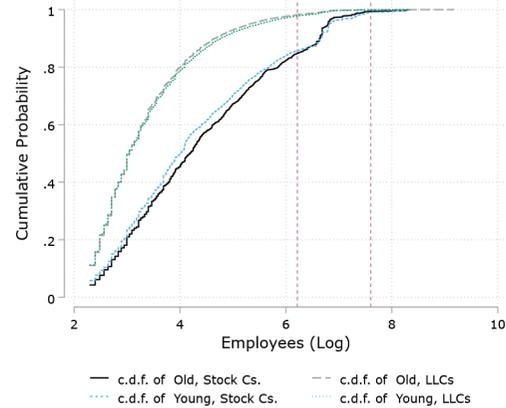
(b) Fixed Assets (Log)
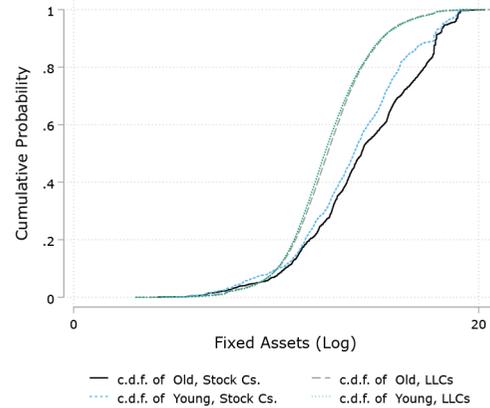
(c) Value Added per Worker (Log)
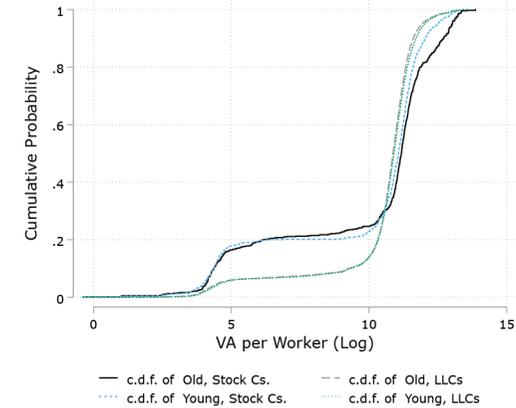
(d) Fixed Assets per Worker (Log)
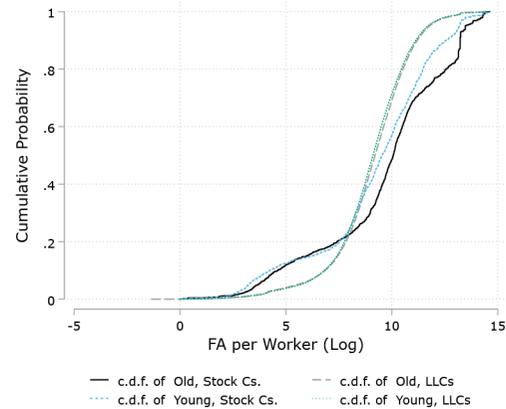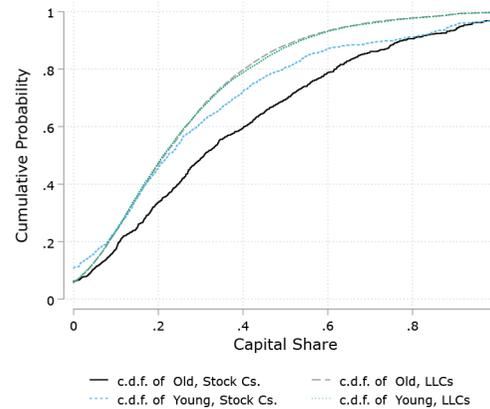
(e) Capital Share

(f) Value Added / Revenue
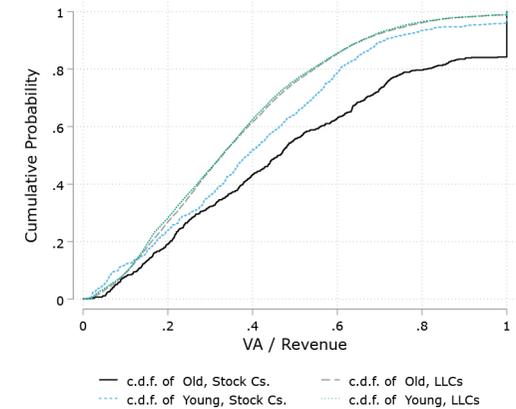
Note: The figures plot the CDFs by legal form and pre/post reform incorporation date for the key outcome variables employment, fixed assets, value added per worker, fixed assets per worker, capital share, and value added/revenue, the distributions of which we additionally study in a regression framework in Tables D.11-D.13. The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of August 10, 1994. The two vertical bars in Panel (a), which plots the employment distribution, denote the 500- and 2,000-employee cutoffs, for which one third and one half of supervisory board seats, respectively, are allocated to workers by law even in the control groups (LLCs, and stock corporations incorporated after the reform).
Source: Own calculations.

D   Additional Tables

Table D.1.: Observation Windows in the Bureau van Dijk Data

	Observations	Mean	10th Pctile.	25th Pctile.	50th Pctile.	75th Pctile.	90th Pctile.
First Year Observed	46,363	2001.93	1997	1998	1999	2006	2012
Last Year Observed	46,363	2009.51	2002	2003	2013	2015	2015
Observations per Firm	46,363	6.02	2	3	6	8	11
Calendar Year (Firm-Year Observations)	278,878	2005.70	1998	2000	2003	2012	2014

Note: The table documents the first and last appearance as well as the observations per firm for the firms in our BvD data set. The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. See Appendix Section B for more information on the sample construction.
Source: Own calculations.

Table D.2.: Differential Trends for Incorporation of Stock Corporations

	(1) 𝟙(Incorporated as AG)	(2) 𝟙(Incorporated as AG)
Incorporation Date	0.0023**	0.0019*
	(0.0011)	(0.0011)
𝟙(Post-Reform)	0.0001	0.00001
	(0.0021)	(0.0021)
Inc. Date $\times$ 𝟙(Post-Reform)	0.0011	0.0012
	(0.0018)	(0.0018)
Constant	0.0128***	0.0125***
	(0.0014)	(0.0014)
Industry FE	No	Yes
N, Firms	46,417	44,218
N, Stock Cs	616	574
N, LLCs	45,801	43,644
Adj. R^2	0.001	0.039

Note: This table reports estimates of whether the reform had an effect on firms' decision to incorporate as a stock corporation (AG). We test for differential trends before and after the reform by interacting an indicator for whether the firm incorporated post-reform with a continuous time trend variable (denominated in years) for incorporation date relative to August 10, 1994. The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. Column (1) reports the basic specification, and column (2) includes industry (i.e. 2-digit NACE designations) fixed effects. See Appendix Section B.2 for details on the sample construction. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.3.: 1994 Reform and Industry Composition of Stock Corporations

NACE Industry Classification	(1)	(2)	NACE Industry Classification	(1)	(2)
A: Agriculture, forestry, fishing	-0.002	-0.002	K: Financial and insurance activities	0.001	0.001
	(0.010)	(0.010)		(0.028)	(0.028)
Control Mean: Post-Reform Stock Cs	0.009	0.009	Control Mean: Post-Reform Stock Cs	0.114	0.114
″, Post-Reform LLCs	0.013	0.013	″, Post-Reform LLCs	0.009	0.009
B: Mining and quarrying	-0.0002	-0.0002	L: Real estate activities	0.010	0.010
	(0.0003)	(0.0003)		(0.011)	(0.011)
Control Mean: Post-Reform Stock Cs	0.0000	0.0000	Control Mean: Post-Reform Stock Cs	0.009	0.009
″, Post-Reform LLCs	0.0006	0.0006	″, Post-Reform LLCs	0.009	0.009
C: Manufacturing	-0.013	-0.012	M: Professional, scientific, and technical activities	-0.016	-0.016
	(0.031)	(0.031)		(0.037)	(0.037)
Control Mean: Post-Reform Stock Cs	0.154	0.154	Control Mean: Post-Reform Stock Cs	0.237	0.237
″, Post-Reform LLCs	0.196	0.196	″, Post-Reform LLCs	0.142	0.142
E: Water supply, sewerage, waste management/remediation	-0.0001	-0.0001	N: Administrative and support service activities	-0.008	-0.008
	(0.0001)	(0.0001)		(0.015)	(0.015)
Control Mean: Post-Reform Stock Cs	0.0000	0.0000	Control Mean: Post-Reform Stock Cs	0.034	0.034
″, Post-Reform LLCs	0.0001	0.0001	″, Post-Reform LLCs	0.027	0.027
F: Construction	0.006	0.006	P: Education	-0.0001	-0.0001
	(0.007)	(0.007)		(0.0002)	(0.0002)
Control Mean: Post-Reform Stock Cs	0.006	0.006	Control Mean: Post-Reform Stock Cs	0.0000	0.0000
″, Post-Reform LLCs	0.044	0.044	″, Post-Reform LLCs	0.0002	0.0002
G: Wholesale and retail trade; repair of motor vehicles	0.010	0.010	Q: Human health and social work activities	-0.0007	-0.0009
	(0.025)	(0.025)		(0.003)	(0.003)
Control Mean: Post-Reform Stock Cs	0.077	0.077	Control Mean: Post-Reform Stock Cs	0.003	0.003
″, Post-Reform LLCs	0.200	0.200	″, Post-Reform LLCs	0.012	0.012
H: Transporting and storage	-0.019	-0.019	R: Arts, entertainment, and recreation	0.003	0.003
	(0.025)	(0.025)		(0.013)	(0.013)
Control Mean: Post-Reform Stock Cs	0.083	0.083	Control Mean: Post-Reform Stock Cs	0.022	0.022
″, Post-Reform LLCs	0.171	0.171	″, Post-Reform LLCs	0.032	0.032
I: Accommodation and food service activities	0.006	0.006	S: Other services activities	0.003	0.003
	(0.005)	(0.005)		(0.006)	(0.006)
Control Mean: Post-Reform Stock Cs	0.0000	0.0000	Control Mean: Post-Reform Stock Cs	0.003	0.003
″, Post-Reform LLCs	0.029	0.029	″, Post-Reform LLCs	0.006	0.006
J: Information and communication	-0.004	-0.005	N, Firms	44,164	44,164
	(0.032)	(0.032)	N, Sh. Corp.	538	538
Control Mean: Post-Reform Stock Cs	0.160	0.160	N, Non-Sh. Corp.	43,626	43,626
″, Post-Reform LLCs	0.047	0.047	Joint P-Value	0.972	0.972

Note: This table reports estimates of the effect of shared governance on the industry composition of stock corporations. Formally, we use indicators for each NACE Rev. 2 Classification 1 industry code as outcomes for DiD specifications as in equation (9). Column (1) reports the basic specification from equation (9), and column (2) includes quarter-of-incorporation fixed effects. We visually report the estimates from column (1) in Figure 2 Panel (c). See Appendix Section B.2 for details on the sample construction.
Source: Own calculations.

Table D.4.: Placebo Reforms in 1998 and 2002: Effect on Supervisory Board Demographic Composition

	$\mathbb{1}$(Women > 0) (1)	Share Women (2)	$\mathbb{1}$(PhD/Profs > 0) (3)	Share PhD/Profs (4)	$\mathbb{1}$(Nobility > 0) (5)	Share Nobility (6)
			Panel A: Placebo Reform in 1998			
DiD	-0.089	-0.025	-0.065	0.019	-0.011	-0.006
Industry FE	(0.082)	(0.034)	(0.085)	(0.037)	(0.036)	(0.010)
Control Mean: Stock Cs	0.345	0.134	0.477	0.200	0.043	0.012
″, LLCs	0.575	0.162	0.475	0.144	0.036	0.006
N, Firm-Years	1,064	1,064	1,064	1,064	1,064	1,064
N, Stock Cs	794	794	794	794	794	794
N, LLCs	270	270	270	270	270	270
			Panel B: Placebo Reform in 2002			
DiD	-0.027	-0.046	0.104	0.050	-0.021	-0.0007
Industry FE	(0.081)	(0.033)	(0.082)	(0.037)	(0.036)	(0.009)
Control Mean: Stock Cs	0.390	0.151	0.457	0.181	0.077	0.021
″, LLCs	0.599	0.153	0.516	0.143	0.074	0.014
N, Firm-Years	1,037	1,037	1,037	1,037	1,037	1,037
N, Stock Cs	794	794	794	794	794	794
N, LLCs	243	243	243	243	243	243

Note: The table reports placebo analyses for the specifications for supervisory board composition reported in Table 2. Panels A and B replicate our DiD specification in (9) for placebo samples and placebo reforms on August 10, 1998 and 2002, respectively (rather than August 10, 1994, when the actual reform occurred). We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of August 10, 1998 on Panel A and within two years of August 10, 2002 on Panel B. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction. The control means refer to observations of firms incorporated *on or after* August 10, 1998 or August 10, 2002. Robust standard errors are reported in parentheses; we do not cluster here as we only have one observation per firm. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.5.: Placebo Reforms in 1998 and 2002: Effect on Executive Board Demographic Composition

	$\mathbb{1}$(Women > 0) (1)	Share Women (2)	$\mathbb{1}$(PhD/Profs > 0) (3)	Share PhD/Profs (4)	$\mathbb{1}$(Nobility > 0) (5)	Share Nobility (6)
			Panel A: Placebo Reform in 1998			
DiD	0.046	-0.005	0.023	0.004	-0.002	-0.000009
Industry FE	(0.036)	(0.015)	(0.030)	(0.010)	(0.012)	(0.001)
Control Mean: Stock Cs	0.598	0.182	0.291	0.071	0.042	0.004
″, LLCs	0.418	0.181	0.072	0.023	0.013	0.001
N, Firm-Years	33,435	33,435	33,435	33,435	33,435	33,435
N, Stock Cs	1,020	1,020	1,020	1,020	1,020	1,020
N, LLCs	32,415	32,415	32,415	32,415	32,415	32,415
			Panel B: Placebo Reform in 2002			
DiD	-0.020	-0.015	0.025	0.005	-0.009	-0.001
Industry FE	(0.035)	(0.017)	(0.028)	(0.012)	(0.011)	(0.001)
Control Mean: Stock Cs	0.516	0.181	0.209	0.065	0.023	0.003
″, LLCs	0.383	0.172	0.069	0.024	0.012	0.001
N, Firm-Years	29,074	29,074	29,074	29,074	29,074	29,074
N, Stock Cs	933	933	933	933	933	933
N, LLCs	28,141	28,141	28,141	28,141	28,141	28,141

Note: The table reports placebo analyses for the specifications for executive board composition reported in Table 2. Panels A and B replicate our DiD specification in (9) for placebo samples and placebo reforms on August 10, 1998 and 2002, respectively (rather than August 10, 1994, when the actual reform occurred). We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of August 10, 1998 on Panel A and within two years of August 10, 2002 on Panel B. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction. The control means refer to observations of firms incorporated *on or after* August 10, 1998 or August 10, 2002. Robust standard errors are reported in parentheses; we do not cluster here as we only have one observation per firm. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.6.: Placebo Reforms in 1998 and 2002: Effect on Firm Scale

	Log Revenue (1)	Log Value Added (2)	Log Emp (BvD) (3)	$\mathbb{1}(\text{Emp} > 500)$ (BvD) (4)	Log Fixed A. (5)	Log Tang. A. (6)	Log Intermediate (7)
				Panel A: Placebo Reform in 1998			
DiD	0.123	-0.215	0.136*	0.022	0.105	-0.172	-0.199
Industry-Year FE	(0.127)	(0.158)	(0.075)	(0.015)	(0.173)	(0.170)	(0.327)
Control Mean: Stock Cs	13.602	15.007	3.805	0.047	13.544	12.118	13.929
″, LLCs	12.435	14.859	3.404	0.026	12.411	12.061	14.819
N, Firm-Years	165,923	41,755	234,862	234,862	120,603	118,606	24,577
N, Stock Cs	1,323	514	1,559	1,559	891	880	325
N, LLCs	37,674	8,822	44,659	44,659	25,968	25,698	6,415
				Panel B: Placebo Reform in 2002			
DiD	-0.143	-0.308*	-0.082	-0.029	-0.121	-0.150	-0.189
Industry-Year FE	(0.159)	(0.175)	(0.095)	(0.022)	(0.181)	(0.168)	(0.468)
Control Mean: Stock Cs	16.071	15.691	3.809	0.083	13.523	12.518	15.030
″, LLCs	15.111	14.831	3.396	0.022	12.314	11.980	14.706
N, Firm-Years	75,294	36,733	137,504	137,504	115,764	113,833	21,638
N, Stock Cs	812	393	1,090	1,090	894	885	253
N, LLCs	22,566	8,259	31,438	31,438	26,089	25,751	6,012

Note: The table reports placebo analyses for the specifications reported in Table 3. Panels A and B replicate our DiD specification in (9) for placebo samples and placebo reforms on August 10, 1998 and 2002, respectively (rather than August 10, 1994, when the actual reform occurred). We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of August 10, 1998 on Panel A and within two years of August 10, 2002 on Panel B. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction. The control means refer to observations of firms incorporated *on or after* August 10, 1998 or August 10, 2002. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.7.: Placebo Reforms in 1998 and 2002: Effect on Productivity and Capital Intensity

	Value Add. per Emp (1)	Log VA per Emp (2)	Fixed A. per Emp (3)	Log Fixed A. per Emp (4)	TFP (Fixed A.) (5)	Capital Share (6)	Value Added /Revenue (7)
	Panel A: Placebo Reform in 1998						
DiD	-15.718	-0.170	21.094	0.092	-0.215**	0.008	0.055
Industry-Year FE	(13.569)	(0.116)	(26.512)	(0.144)	(0.101)	(0.027)	(0.087)
Control Mean: Stock Cs	128.807	10.511	141.021	9.551	7.047	0.352	0.648
″, LLCs	71.313	10.570	35.017	8.917	7.727	0.252	0.374
N, Firm-Years	41,755	41,755	121,971	120,603	41,183	40,750	30,660
N, Stock Cs	514	514	894	891	511	526	467
N, LLCs	8,822	8,822	26,219	25,968	8,683	8,640	7,687
	Panel B: Placebo Reform in 2002						
DiD	-14.677	-0.090	-7.301	-0.072	-0.069	-0.025	0.029
Industry-Year FE	(12.433)	(0.094)	(21.680)	(0.138)	(0.082)	(0.029)	(0.048)
Control Mean: Stock Cs	110.152	11.030	102.648	9.671	7.615	0.305	0.435
″, LLCs	67.581	10.657	35.690	8.898	7.880	0.245	0.378
N, Firm-Years	36,733	36,733	117,698	115,764	36,071	35,486	26,208
N, Stock Cs	393	393	902	894	391	396	344
N, LLCs	8,259	8,259	26,388	26,089	8,145	8,055	7,126

Note: The table reports placebo analyses for the specifications reported in Table 4. Panels A and B replicate our DiD specification in (9) for placebo samples and placebo reforms on August 10, 1998 and 2002, respectively (rather than August 10, 1994, when the actual reform occurred). We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of August 10, 1998 on Panel A and within two years of August 10, 2002 on Panel B. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction. The control means refer to observations of firms incorporated *on or after* August 10, 1998 or August 10, 2002. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.8.: Placebo Reforms in 1998 and 2002: Effect on Profitability

	EBITDA /Revenue (1)	EBIT /Revenue (2)	Net Income /Revenue (3)	EBITDA /Total A. (4)	EBIT /Total A. (5)	Net Income /Total A. (6)
	Panel A: Placebo Reform in 1998					
DiD	-0.031	-0.034	-0.330	-0.025	-0.020	-0.031**
Industry-Year FE	(0.036)	(0.040)	(0.223)	(0.017)	(0.016)	(0.014)
Control Mean: Stock Cs	-0.060	-0.115	-0.184	0.076	0.039	0.014
″, LLCs	0.061	0.028	0.010	0.134	0.085	0.050
N, Firm-Years	31,297	31,153	28,107	41,397	41,169	38,769
N, Stock Cs	495	498	497	547	549	544
N, LLCs	7,700	7,692	7,471	8,741	8,723	8,599
	Panel B: Placebo Reform in 2002					
DiD	-0.008	-0.009	-0.036	0.0007	0.005	0.004
Industry-Year FE	(0.020)	(0.021)	(0.044)	(0.018)	(0.017)	(0.014)
Control Mean: Stock Cs	0.022	-0.014	-0.007	0.094	0.049	0.023
″, LLCs	0.058	0.027	0.009	0.134	0.084	0.050
N, Firm-Years	26,501	26,419	23,987	35,844	35,726	34,233
N, Stock Cs	350	350	347	399	399	395
N, LLCs	7,109	7,107	6,943	8,132	8,126	8,058

Note: The table reports placebo analyses for the specifications reported in Table 6. Panels A and B replicate our DiD specification in (9) for placebo samples and placebo reforms on August 10, 1998 and 2002, respectively (rather than August 10, 1994, when the actual reform occurred). We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of August 10, 1998 on Panel A and within two years of August 10, 2002 on Panel B. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction. The control means refer to observations of firms incorporated *on or after* August 10, 1998 or August 10, 2002. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.9.: Placebo Reform in 1998 and 2002: Effect on Capital Structure, Leverage, and Cost of Debt

	Liabilites /Total A. (1)	Leverage (2)	Cost of Debt (3)	Long-Term Debt /Total Debt (4)	Cash /Total A. (5)
			Panel A: Placebo Reform in 1998		
DiD	-0.016	0.001	-0.010	0.059**	-0.006
Industry-Year FE	(0.020)	(0.024)	(0.019)	(0.025)	(0.014)
Control Mean: Stock Cs	0.564	0.284	0.143	0.729	0.194
″, LLCs	0.674	0.372	0.121	0.822	0.163
N, Firm-Years	121,921	71,239	23,752	49,584	119,463
N, Stock Cs	892	776	435	649	889
N, LLCs	26,221	20,291	6,377	15,896	25,889
			Panel B: Placebo Reform in 2002		
DiD	-0.033*	-0.027	-0.002	-0.033	0.009
Industry-Year FE	(0.018)	(0.025)	(0.020)	(0.029)	(0.014)
Control Mean: Stock Cs	0.646	0.372	0.125	0.773	0.171
″, LLCs	0.698	0.401	0.111	0.828	0.160
N, Firm-Years	117,658	67,994	21,781	48,312	115,044
N, Stock Cs	902	775	315	626	894
N, LLCs	26,384	20,365	6,131	16,009	26,016

Note: The table reports placebo analyses for the specifications reported in Table 7. Panels A and B replicate our DiD specification in (9) for placebo samples and placebo reforms on August 10, 1998 and 2002, respectively (rather than August 10, 1994, when the actual reform occurred). We report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of August 10, 1998 on Panel A and within two years of August 10, 2002 on Panel B. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction. The control means refer to observations of firms incorporated *on or after* August 10, 1998 or August 10, 2002. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.10.: Corporate Group Structure and Presence of Shared Governance at the Corporate Group Level

	(1)	(2)	(3)
Panel A: Part of	Corporate Group	Domestic Corp. Group	Corp. Group w/ > 2000 Emp.
Diff-in-Diff	0.084**	0.040	-0.005
	(0.036)	(0.038)	(0.021)
DiD	0.092**	0.047	-0.009
Industry FE	(0.037)	(0.039)	(0.022)
Control Mean: Stock Cs	0.579	0.532	0.107
″, LLCs	0.317	0.275	0.044
N, Stock Cs	452	452	452
N, LLCs	37,268	37,268	37,268
Panel B: Parent of	Corporate Group	Domestic Corp. Group	Corp. Group w/ > 2000 Emp.
Diff-in-Diff	0.045	0.022	0.008
	(0.038)	(0.038)	(0.010)
DiD	0.057	0.030	0.009
Industry FE	(0.039)	(0.039)	(0.011)
Control Mean: Stock Cs	0.358	0.340	0.026
″, LLCs	0.136	0.123	0.002
N, Stock Cs	452	452	452
N, LLCs	37,268	37,268	37,268
Panel C: Subsidiary of	Corporate Group	Domestic Corp. Group	Corp. Group w/ > 2000 Emp.
Diff-in-Diff	0.039	0.018	-0.014
	(0.035)	(0.034)	(0.019)
DiD	0.035	0.017	-0.019
Industry FE	(0.035)	(0.034)	(0.019)
Control Mean: Stock Cs	0.221	0.193	0.082
″, LLCs	0.181	0.153	0.042
N, Stock Cs	452	452	452
N, LLCs	37,268	37,268	37,268

Note: Panel A reports specifications with outcomes related to status as either parent or subsidiary of a corporate group. A corporate group is defined by a set of business entities ultimately owned (i.e. directly or indirectly) by one corporation with a higher than 50% ownership stake in the other business entities. The indicators for parent (Panel B) or subsidiary (Panel C) indicate that a firm is a subsidiary or a parent of a corporate group, respectively. (*The table note continues on the next page.*)

(*Table note continued from previous page.*) To shed light on codetermination at the corporate group level, we distinguish domestic groups (with a parent firm incorporated in Germany) and those that are ultimately owned by a firm outside of Germany. We also distinguish by corporate group employment of more than 2,000 employees. Domestic corporate group employment is defined as the sum of yearly employment aggregated across all German corporations within the corporate group (where the ultimate corporate owner can be located outside of Germany), regardless of their date of incorporation. We aggregate employment considering all types of firms to build the 2,000-employee indicator.

The table reports the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Robust standard errors are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In addition to the regression results, which we discuss below, the control means for the indicators in column (3) above are informative as they indicate whether firms may be subject to codetermination at the group level. Among the stock corporations in our sample, 10.7% are part of a corporate group with more than 2,000 domestic employees. These control means suggest that 10.7% of stock corporations in our sample incorporated after the 1994 reform are subject to parity codetermination at the corporate group, which kicks in above 2,000 employees. That is, a German corporate group is subject to parity codetermination at the group level if the aggregate domestic employment of business entities in the group exceeds 2,000 employees. Business entities are to be counted as part of a corporate group if the group is the ultimate owner of a majority of the shares (§ 5 *MitbestG*, § 17 *AktG*). Codetermination at the business entity level is not affected by the presence or absence of codetermination at the group level.

We cannot credibly calculate the presence of one-third codetermination at the corporate group level because a stricter legal standard for defining corporate groups applies there: business entities are only counted towards a corporate group for the purposes of one-third codetermination if they are completely integrated into the group (*Eingliederung*) or if a domination agreement of the group over the unit exists (§ 2 (2) *DrittelbG*). Domination agreements are empirically rare (e.g., Lieder/Hoffmann, 2017: find that 3 to 7% of stock corporations are governed by such agreements) and not reported in the data.

The regression results reveal a higher probability of being a part of a corporate group but not on membership in a domestic corporate group or in a group with more than 2,000 employees at domestic business entities. Across specifications, we do not find statistically significant effects and point estimates are close to zero with standard errors of about 2 to 4ppt.
Source: Own calculations.

Table D.11.: Effect of Shared Governance on Distribution of Employment and of Fixed Assets

	Rank	$\mathbb{1}$(Above 10th Percentile)	$\mathbb{1}$(Above 25th Percentile)	$\mathbb{1}$(Above 50th Percentile)	$\mathbb{1}$(Above 75th Percentile)	$\mathbb{1}$(Above 90th Percentile)
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Employment (BvD)			
Diff-in-Diff	1.710	-0.021	-0.001	0.017	0.023	-0.006
	(2.583)	(0.017)	(0.030)	(0.041)	(0.038)	(0.025)
DiD Year FE	1.678	-0.012	0.004	0.019	0.024	-0.006
	(2.581)	(0.017)	(0.030)	(0.041)	(0.038)	(0.025)
DiD Industry FE	1.006	-0.023	-0.012	0.005	0.021	-0.010
	(2.550)	(0.018)	(0.030)	(0.040)	(0.038)	(0.025)
DiD Industry-Year FE	0.428	-0.016	-0.011	-0.0002	0.017	-0.014
	(2.513)	(0.018)	(0.030)	(0.040)	(0.038)	(0.025)
Level at Percentile: Stock Cs	49.59	13.52	24.18	61.46	231.61	1,311.27
″, LLCs	49.99	10.55	13.46	22.71	47.94	120.78
N, Firm-Years	278,878	278,878	278,878	278,878	278,878	278,878
N, Stock Cs	616	616	616	616	616	616
N, LLCs	45,801	45,801	45,801	45,801	45,801	45,801
			Panel B: Fixed Assets			
Diff-in-Diff	4.449	-0.004	0.038	0.016	0.075*	0.034
	(2.708)	(0.020)	(0.032)	(0.042)	(0.040)	(0.032)
DiD Year FE	4.377	0.002	0.042	0.019	0.076*	0.034
	(2.707)	(0.019)	(0.032)	(0.042)	(0.041)	(0.032)
DiD Industry FE	4.758*	-0.0008	0.042	0.019	0.074**	0.040
	(2.477)	(0.019)	(0.032)	(0.040)	(0.037)	(0.029)
DiD Industry-Year FE	4.759*	0.007	0.051	0.031	0.082**	0.039
	(2.552)	(0.019)	(0.032)	(0.042)	(0.039)	(0.031)
Level at Percentile: Stock Cs	47.85	48.85	234.22	1,103.31	6,960.97	75,967.94
″, LLCs	49.31	14.28	43.72	177.44	824.06	5,617.32
N, Firm-Years	114,844	114,844	114,844	114,844	114,844	114,844
N, Stock Cs	360	360	360	360	360	360
N, LLCs	24,625	24,625	24,625	24,625	24,625	24,625

Note: The table reports the DiD effects of shared governance following specifications (9), with indicators for whether the underlying continuous outcome variable exceeds various percentiles in the control group in a year-by-legal-form cell. In the first column, we construct a rank variable by dividing the relative position of each firm (sorted in ascending order by each outcome) by the number of positions observed in its own year-by-legal-form cell, and then scaling this by a factor of 100. The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. Non-indicator outcomes are winsorized at the 1% level by year. See Appendix Section B for more information on the sample construction. For the first column, the level at percentile line refers to the control mean of the rank variable. For columns 2 to 6, this refers to the levels at cutoff percentile refer to the value of the underlying variable in the control group by firm legal type at each percentile cutoff. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table D.12.: Effect of Shared Governance on Distribution of Value Added per Worker and of Fixed Assets per Worker

	Rank (1)	$\mathbb{1}$(Above 10th Percentile) (2)	$\mathbb{1}$(Above 25th Percentile) (3)	$\mathbb{1}$(Above 50th Percentile) (4)	$\mathbb{1}$(Above 75th Percentile) (5)	$\mathbb{1}$(Above 90th Percentile) (6)
			Panel A: Value Added per Worker			
Diff-in-Diff	6.142**	-0.029	0.029	0.053	0.093**	0.093***
	(3.119)	(0.026)	(0.041)	(0.052)	(0.045)	(0.036)
DiD	6.025*	-0.022	0.034	0.055	0.089**	0.089**
Year FE	(3.111)	(0.026)	(0.041)	(0.052)	(0.045)	(0.036)
DiD	8.526***	-0.028	0.046	0.095*	0.123***	0.110***
Industry FE	(3.204)	(0.027)	(0.042)	(0.053)	(0.046)	(0.035)
DiD	8.909***	-0.013	0.072*	0.104*	0.133***	0.116***
Industry-Year FE	(3.276)	(0.027)	(0.042)	(0.054)	(0.047)	(0.036)
Level at Percentile: Stock Cs	48.56	19.98	31.63	46.93	77.77	163.00
″, LLCs	50.80	9.97	17.30	29.41	62.86	98.71
N, Firm-Years	40,066	40,066	40,066	40,066	40,066	40,066
N, Stock Cs	246	246	246	246	246	246
N, LLCs	8,334	8,334	8,334	8,334	8,334	8,334
			Panel B: Fixed Assets per Worker			
Diff-in-Diff	6.780***	0.004	0.076**	0.082**	0.027	0.091***
	(2.584)	(0.020)	(0.030)	(0.041)	(0.040)	(0.033)
DiD	6.713***	0.009	0.080***	0.084**	0.026	0.088***
Year FE	(2.580)	(0.020)	(0.030)	(0.041)	(0.040)	(0.033)
DiD	7.360***	0.009	0.084***	0.089**	0.033	0.097***
Industry FE	(2.387)	(0.019)	(0.030)	(0.039)	(0.037)	(0.031)
DiD	7.391***	0.016	0.097***	0.097**	0.036	0.095***
Industry-Year FE	(2.455)	(0.019)	(0.030)	(0.040)	(0.039)	(0.033)
Level at Percentile: Stock Cs	46.72	1.49	3.36	12.05	64.46	359.24
″, LLCs	49.23	0.59	1.60	5.23	18.42	60.65
N, Firm-Years	116,018	116,018	116,018	116,018	116,018	116,018
N, Stock Cs	360	360	360	360	360	360
N, LLCs	24,850	24,850	24,850	24,850	24,850	24,850

Note: See note for Appendix Table D.11.
Source: Own calculations.

Table D.13.: Effect of Shared Governance on Distribution of Capital Share and Value Added / Revenue

	Rank (1)	$\mathbb{1}$(Above 10th Percentile) (2)	$\mathbb{1}$(Above 25th Percentile) (3)	$\mathbb{1}$(Above 50th Percentile) (4)	$\mathbb{1}$(Above 75th Percentile) (5)	$\mathbb{1}$(Above 90th Percentile) (6)
			Panel A: Capital Share			
Diff-in-Diff	8.440**	-0.016	0.027	0.107**	0.133**	0.059*
	(3.461)	(0.015)	(0.039)	(0.054)	(0.052)	(0.035)
DiD	8.348**	-0.008	0.034	0.112**	0.133**	0.057
Year FE	(3.447)	(0.013)	(0.039)	(0.055)	(0.052)	(0.035)
DiD	9.636***	-0.013	0.043	0.125**	0.144***	0.064*
Industry FE	(3.134)	(0.015)	(0.038)	(0.049)	(0.048)	(0.034)
DiD	9.617***	-0.001	0.053	0.142***	0.148***	0.065*
Industry-Year FE	(3.158)	(0.014)	(0.038)	(0.049)	(0.050)	(0.035)
Level at Percentile: Stock Cs	46.77	0.02	0.10	0.24	0.46	0.72
″, LLCs	50.02	0.03	0.10	0.21	0.37	0.57
N, Firm-Years	39,110	39,110	39,110	39,110	39,110	39,110
N, Stock Cs	249	249	249	249	249	249
N, LLCs	8,213	8,213	8,213	8,213	8,213	8,213
			Panel B: Value Added / Revenue			
Diff-in-Diff	7.740*	0.025	-0.021	0.039	0.119**	0.143***
	(4.155)	(0.031)	(0.055)	(0.068)	(0.060)	(0.048)
DiD	7.637*	0.034	-0.014	0.042	0.117**	0.141***
Year FE	(4.141)	(0.031)	(0.055)	(0.068)	(0.060)	(0.048)
DiD	7.864**	0.021	-0.022	0.043	0.123**	0.148***
Industry FE	(3.172)	(0.028)	(0.043)	(0.052)	(0.053)	(0.045)
DiD	7.123**	0.027	-0.023	0.045	0.115**	0.142***
Industry-Year FE	(3.269)	(0.028)	(0.046)	(0.053)	(0.055)	(0.047)
Level at Percentile: Stock Cs	46.87	0.08	0.23	0.41	0.57	0.74
″, LLCs	49.79	0.12	0.21	0.35	0.53	0.69
N, Firm-Years	27,722	27,722	27,722	27,722	27,722	27,722
N, Stock Cs	227	227	227	227	227	227
N, LLCs	7,086	7,086	7,086	7,086	7,086	7,086

Note: See note for Appendix Table D.11.
Source: Own calculations.

Table D.14.: Effect of Shared Governance on Skill Structure (Matched Employer-Employee Data)

	Low-Skilled % (1)	Med-Skilled % (2)	High-Skilled % (3)	Qualified Manual % (4)	Qualified Service % (5)	All Managers % (6)	Outsourceable (FSCL) % (7)
Diff-in-Diff	-0.011	0.013	0.001	0.031	0.004	-0.014	0.014
	(0.010)	(0.026)	(0.028)	(0.026)	(0.007)	(0.011)	(0.009)
DiD	-0.011	0.009	0.001	0.030	0.001	-0.008	0.011
Year FE	(0.009)	(0.022)	(0.022)	(0.026)	(0.003)	(0.006)	(0.008)
DiD	-0.013	0.011	0.004	0.035	0.001	-0.007	0.007
Industry FE	(0.009)	(0.019)	(0.019)	(0.024)	(0.003)	(0.006)	(0.011)
DiD	-0.014	0.010	0.005	0.034	0.001	-0.007	0.007
Industry-Year FE	(0.009)	(0.019)	(0.019)	(0.024)	(0.003)	(0.006)	(0.011)
Control Mean: Stock Cs	0.125	0.582	0.276	0.100	0.043	0.093	0.019
″, LLCs	0.115	0.738	0.121	0.268	0.019	0.030	0.133
N, Firm-Years	126,519	126,519	126,519	126,509	126,519	126,519	126,519
N, Stock Cs	285	285	285	285	285	285	285
N, LLCs	18,578	18,578	18,578	18,578	18,578	18,578	18,578

Note: The table reports the effect of shared governance on the skill structure of firms. Outsourceable occupations refer to the share of workers in food, security, cleaning and logistics occupations Goldschmidt/Schmieder (2017). We further consider employer-reported education measures: (i) low-skilled workers with no vocational training, (ii) medium-skilled workers with a finished school degree and a vocational qualification, and (iii) high-skilled workers with a university degree and reports the results of DiD specifications as in (9). Qualified manual and service occupations follow the Blossfeld (1987) classification. The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction and Appendix Figure C.5 for the specification with industry-year fixed effects at additional bandwidths and winsorization levels. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.15.: Effect of Shared Governance on Tenure (Matched Employer-Employee Data)

	Tenure (1)	Log Tenure (2)	All (3)	<4 Yrs Tenure (4)	4-9 Yrs Tenure (5)	>9 Yrs Tenure (6)
Diff-in-Diff	-0.529*	-0.068	0.018*	0.017*	0.004	-0.003**
	(0.309)	(0.043)	(0.011)	(0.010)	(0.003)	(0.001)
DiD	-0.499*	-0.064*	0.015	0.015	0.004	-0.003**
Year FE	(0.284)	(0.038)	(0.010)	(0.009)	(0.002)	(0.001)
DiD	-0.371	-0.048	0.010	0.011	0.003	-0.003**
Industry FE	(0.265)	(0.036)	(0.010)	(0.009)	(0.002)	(0.001)
DiD	-0.388	-0.050	0.010	0.010	0.003	-0.002*
Industry-Year FE	(0.265)	(0.036)	(0.010)	(0.009)	(0.002)	(0.001)
Control Mean: Stock Cs	7.769	7.887	0.198	0.134	0.046	0.018
″, LLCs	8.361	7.941	0.176	0.121	0.040	0.014
N, Firm-Years	126,519	126,519	110,490	110,490	110,490	110,490
N, Stock Cs	285	285	280	280	280	280
N, LLCs	18,578	18,578	18,344	18,344	18,344	18,344

Note: The table reports the effect of shared governance on worker tenure at firms. We consider (i) low-skilled workers with no vocational training, (ii) medium-skilled worker with a finished school degree and a vocational qualification, and (iii) high-skilled workers with a university degree and report the results of DiD specifications as in (9). The sample is restricted to stock corporations (*AGs*) and limited liability companies (LLCs, *GmbHs*) with 10 or more employees incorporated within two years of the reform date of August 10, 1994. We use 2-digit NACE designations for industry fixed effects. See Appendix Section B for more information on the sample construction and Appendix Section B for more information on the sample construction and Appendix Figure C.6 for the specification with industry-year fixed effects at additional bandwidths and winsorization levels. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Standard errors clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.16.: Effect of Shared Governance on Indices for Financial Constraints and Distress

	HP Index (1)	KZ Index (2)	Z Score, 5 Vars (3)	Z Score, 4 Vars (4)	O Score (5)	WW Score (6)
			Panel A: $\mathbb{1}$(Above Median)			
Diff-in-Diff	-0.0002	-0.029	0.085	0.060	-0.037	0.094
	(0.040)	(0.062)	(0.061)	(0.053)	(0.057)	(0.078)
DiD Year FE	0.004	-0.022	0.081	0.057	-0.036	0.101
	(0.041)	(0.061)	(0.061)	(0.053)	(0.057)	(0.078)
DiD Industry FE	0.011	-0.042	0.093*	0.038	-0.058	0.012
	(0.039)	(0.057)	(0.055)	(0.054)	(0.056)	(0.055)
DiD Industry-Year FE	0.020	-0.026	0.096*	0.041	-0.056	0.008
	(0.041)	(0.058)	(0.057)	(0.056)	(0.058)	(0.055)
Control Mean: Stock Cs	0.504	0.507	0.508	0.507	0.509	0.509
″, LLCs	0.500	0.500	0.500	0.500	0.500	0.501
N, Firm-Years	116,059	28,314	27,103	37,925	28,657	19,426
N, Stock Cs	361	237	227	244	228	219
N, LLCs	24,856	6,904	6,921	8,083	6,608	5,866
			Panel B: $\mathbb{1}$(Above 80th Percentile)			
Diff-in-Diff	0.089**	-0.028	0.095*	0.035	0.026	0.077
	(0.040)	(0.042)	(0.050)	(0.043)	(0.042)	(0.062)
DiD Year FE	0.090**	-0.025	0.088*	0.029	0.026	0.075
	(0.040)	(0.043)	(0.050)	(0.043)	(0.042)	(0.062)
DiD Industry FE	0.097***	-0.030	0.113**	0.025	0.007	0.040
	(0.038)	(0.043)	(0.047)	(0.043)	(0.040)	(0.044)
DiD Industry-Year FE	0.101**	-0.027	0.108**	0.017	0.020	0.026
	(0.040)	(0.044)	(0.049)	(0.044)	(0.040)	(0.043)
Control Mean: Stock Cs	0.206	0.211	0.213	0.210	0.212	0.214
″, LLCs	0.200	0.201	0.201	0.201	0.201	0.201
N, Firm-Years	116,059	28,314	27,103	37,925	28,657	19,426
N, Stock Cs	361	237	227	244	228	219
N, LLCs	24,856	6,904	6,921	8,083	6,608	5,866

Note: The table reports the effect of shared governance on financial distress risk (Altman (2000) z-score, and Ohlson (1980) o-score), and financial constraints (Whited/Wu (2006), Kaplan/Zingales (1997), and Hadlock/Pierce (2010) indices). See Appendix Section B.3 on their construction. The indices are split into indicators by median (Panel A) or 80th percentile (Panel B) in our baseline sample control group in a year-by-legal-form cell, with 1 indicating higher risk or constraints. We report the results of DiD specifications as in (9). The sample is corporations incorporated within two years of the reform. The control means refer to observations of firms incorporated *on or after* August 10, 1994. Our interpretation is mixed due to noisily estimated effects, except for significantly positive effects on the z-score (but only in the 5-variable variant for public firms, but no the 4-variable variant more appropriate for our largely private sample), and for the HP index if evaluated at the top-20% cutoff but not at the median. These effects necessarily reflect the increase in e.g. assets (which either enter quadratically or as denominators). Standard errors clustered at the firm level are in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table D.17.: Accounting For Multiple-Hypothesis Testing: Romano-Wolf p-values for BvD Outcomes

	No FE	No FE	Year FE	Industry FE	Industry-Year FE
Supervisory Board					
$\mathbb{1}(\text{Women} > 0)$	0.170				
Share Women	0.405				
$\mathbb{1}(\text{PhD/Profs} > 0)$	1.000				
Share PhD/Profs	0.882				
$\mathbb{1}(\text{Nobility} > 0)$	0.553				
Share Nobility	0.168				
Executive Board					
$\mathbb{1}(\text{Women} > 0)$	0.974				
Share Women	1.000				
$\mathbb{1}(\text{PhD/Profs} > 0)$	0.345				
Share PhD/Profs	0.222				
$\mathbb{1}(\text{Nobility} > 0)$	0.112				
Share Nobility	0.691				
Firm Scale					
Log Revenue	0.262	0.204	0.996	0.804	0.980
Log Value Added	1.000	0.996	1.000	0.970	0.980
Log Fixed A.	0.168	0.116	0.162	0.054	0.166
Log Tang. A.	0.968	0.898	0.920	0.804	0.769
Log Emp (BvD)	0.868	0.759	0.894	0.846	0.986
$\mathbb{1}(\text{Emp} > 500)$ (BvD)	0.998	0.986	0.998	0.970	0.994
Productivity					
Value Add. per Emp	0.022	0.008	0.008	0.002	0.014
Log VA per Emp	0.976	0.930	0.812	0.804	0.407
Fixed A. per Emp	0.028	0.012	0.016	0.006	0.044
Log Fixed A. per Emp	0.146	0.102	0.046	0.054	0.044
TFP (Fixed A.)	1.000	1.000	1.000	0.976	0.994
Capital Share	0.084	0.048	0.046	0.006	0.052
Value Added /Revenue	0.046	0.022	0.024	0.006	0.192
Profitability					
EBITDA /Revenue	0.449	0.349	0.397	0.407	0.407
EBIT /Revenue	0.455	0.355	0.357	0.383	0.371
Net Income /Revenue	0.254	0.200	0.132	0.136	0.407
EBITDA /Total A.	1.000	1.000	1.000	0.976	0.994
EBIT /Total A.	1.000	0.998	1.000	0.976	0.994
Net Income /Total A.	0.786	0.683	0.755	0.501	0.589
Capital Structure					
Liabilites /Total A.	0.868	0.759	0.812	0.681	0.769
Leverage	1.000	0.996	1.000	0.834	0.980
Cost of Debt	0.553	0.425	0.357	0.681	0.405
Long-Term Debt /Total Debt	1.000	0.998	0.998	0.970	0.994
Cash /Total A.	0.786	0.685	0.755	0.681	0.581

Note: This table reports the Romano-Wolf p-values following the procedure in Romano/Wolf (2005) and Clarke/Romano/Wolf (2019) for the BvD outcomes. We consider our difference-in-differences specifications with standard errors clustered at the firm level and different fixed effects. We cannot implement the procedure on the IAB and BvD data jointly since the data are on separate servers. The first column includes all BvD outcomes. For this set a separate joint significance test for all BvD outcomes building on Zellner (1962) rejects the null hypothesis of no effects of shared governance ($p = 0.0082$). The remaining columns report Romano-Wolf p-values for the BvD production outcomes for richer fixed effects and hence exclude board composition (which is not panel data).
Source: Own calculations.

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