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Working Paper

Labor in the Boardroom

IZA Discussion Papers, No. 12799

Provided in Cooperation with:
IZA – Institute of Labor Economics

Suggested Citation: Jäger, Simon; Schoefer, Benjamin; Heining, Jörg (2019) : Labor in the Boardroom, IZA Discussion Papers, No. 12799, Institute of Labor Economics (IZA), Bonn

This Version is available at:
https://hdl.handle.net/10419/215195

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ISSN: 2365-9793

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ABSTRACT

Labor in the Boardroom*

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 certain firms incorporated after August 1994 but locked it in for the older cohorts. In sharp contrast to the canonical hold-up hypothesis – that increasing labor's power reduces owners' capital investment – we find that granting formal control rights to workers *raises* capital formation. The capital stock, the capital-labor ratio, and the capital share all increase. Shared governance does not raise wage premia or rent sharing. It lowers outsourcing, while moderately shifting employment to skilled labor. Shared governance has no clear effect on profitability, leverage, or costs of debt. Overall, the evidence is consistent with richer models of industrial relations whereby shared governance raises capital by permitting workers to bargain over investment or by institutionalizing communication and repeated interactions between labor and capital.

JEL Classification: J0, J53, J54

Keywords: industrial relations, corporate governance, codetermination, investment

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* We thank Daron Acemoglu, David Autor, David Card, Jonathan Cohn, Robert Gibbons, Lawrence Katz, Patrick Kline, Bentley MacLeod, Jim Poterba, Fabiano Schivardi, Antoinette Schoar, Andrei Shleifer, Sebastian Siegloch, David Sraer, Jean Tirole, Margarita Tsoutsoura, and John Van Reenen for useful discussions, as well as audiences at Columbia University, Columbia Micro Macro Labor Conference, CRC BerlinWorkshop on the Future of Labor, CREST, CSEF Conference on Finance, Labor, and Inequality, EIEF, INSEAD, IZA, MIT Organizational Econ Lunch, NBER Corporate Finance Meeting, NBER Summer Institute Labor Studies, Stanford SIEPR Workshop, Stockholm University, UC Berkeley, UMass Amherst, UBC, U Mannheim, and U Montreal. For sharing their legal expertise, we thank Andreas Engert, Thomas Hoffmann, Marcus Lutter, Sebastian Sick, and Holger Spamann. Karl Aspelund, Nikhil Basavappa, Jonathan Cohen, Dominik Jurek, René Livas, Enrique Perez, and Martina Uccioli provided outstanding research assistance. We thank the MIT Sloan Good Companies, Good Jobs Initiative, the UC Berkeley Institute for Research on Labor and Employment, and theWashington Center for Equitable Growth for financial support.

1 Introduction

A fundamental question societies face is whether and how to involve workers in decision-making at their workplace. Many countries, particularly in continental Europe, grant workers formal authority in firms' decision-making (Hall and Soskice, 2001). 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, although policy proposals to mandate worker-elected directors have been considered.[1] The consequences of granting workers such authority and voice remain highly debated. For example, worker participation may help overcome coordination issues and improve information flows (Hirschman, 1970; Freeman and Medoff, 1985; Freeman and Lazear, 1995), foster long-term employment relationships, or facilitate the enforcement of implicit contracts (Malcomson, 1983; Hogan, 2001). However, by the influential hold-up hypothesis (Grout, 1984), granting workers control rights will raise worker bargaining power and thereby discourage capital formation, as capitalists anticipate that labor will grab a larger share of the fruits from investments (Jensen and Meckling, 1976, 1979). Compelling evidence to adjudicate between these views is scant due to the absence of experiments randomizing shared governance across firms.

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 representatives elected by their workforce. Stock corporations may be traded on an exchange, but only few are listed (12% in our sample). 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 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). A 1994 reform abruptly abolished worker-elected directors in *newly incorporated* stock corporations, so that these firms were formally completely controlled by their shareholders unless reaching a threshold of 500 employees. Importantly, the cohort-based reform permanently locked in shared governance in the incumbent firm cohorts incorporated before the reform for the rest of their lifecycle.

The sharp law change permits a difference-in-differences design comparing, first, stock

[1] In the US, two federal bills proposed in 2018, the *Accountable Capitalism Act* and the *Reward Work Act*, would mandate that 40% or 1/3, respectively, of the directors of large companies be worker representatives.

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)).[2] 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.

We assess the validity of the design in four steps. First, a McCrary (2008) test of the density of incorporations around the reform cutoff suggests that firms did not manipulate incorporation dates, e.g., by delaying incorporation around the reform cutoff date or switching corporate forms. Second, we find no evidence for compositional shifts by industry or legal form in our analysis sample. Both pieces of evidence are consistent with survey evidence that incorporations were not deterred by one-third codetermination.[3] Third, several institutional features support the research design. For example, grandfathered firms cannot escape the mandate through simple re-incorporation. In addition, the arbitrary grandfathering cutoff date has been challenged by shareholders, suggesting that the mandate binds in older corporations. The courts – including the Federal Constitutional Court – have upheld the law's constitutionality. Finally, we conduct a series of placebo analyses, counterfactually assuming that the reform had taken place four or eight years later than it actually did and drawing corresponding new samples of firm cohorts, to rule out differential trends or lifecycle patterns by legal form driving our estimates.

The first outcome we study is board composition, starting by verifying that the reform shifted worker presence on corporate boards. Shared governance also sharply raises female supervisory board representation (15ppt or 43%) and sharply reduces the share of nobility-title holders (1.4ppt or 60%), a proxy for social-economic status and network capital (Bourdieu, 1986). In an attenuated way, these composition effects also pass through

[2]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.

[3]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. Rather, the surveyed firms generally accept shared governance, in particular because of the information and specific knowledge about the firm that worker representatives bring to the board room, and oppose the abolition of shared governance. In a survey sampling stock corporations founded between 1994 and 1996, Schawilye, Gaugler, and 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.

to the executive board – which the supervisory board appoints.

Second, and a central to our study, is the effect of shared governance on the production process, with a particular focus on capital formation. Most importantly, we find that firms with shared governance have about 40 to 50% larger fixed (long-term) capital stocks – sharply contradicting the disinvestment predicted by the hold-up and agency cost views of shared governance (see, e.g., Jensen and Meckling, 1979). The positive effects on capital formation even outpace a small increase in employment, leading the capital/labor ratio to increase. We further document a large and significant increase in the capital share of 8ppt (control mean: 0.30). Thus, shared governance shifts firms *toward* a more capital-intensive mode of production.

One mechanism raising capital intensity may be that owners strategically substitute into labor-substituting capital to offset worker participation. However, we find, if anything, a positive albeit statistically insignificant effect on employment, inconsistent with the substitution channel. Moreover, we find some evidence pointing towards a labor-complementing-capital interpretation, as the workforce under shared governance shifts from lower- into higher-skilled worker groups.

Another possible mechanism resulting in higher capital shares is that firms outsource labor-intensive steps of the value added chain. However, the share of sales produced in-house (value added over revenue) increases significantly by about 16ppt (control mean: 0.43). Moreover, the share of workers in occupations classified as outsourceable (namely food services, security, cleaning, and logistics as in Goldschmidt and Schmieder, 2017) if anything increases, albeit only marginally significantly.

Does the additional capital reflect only unproductive amenities demanded by workers (such as nice break rooms) rather than directly productive capital inputs (such as machines)? Our analysis of productivity suggests that this is not the case: we find that shared governance increases labor productivity, as indicated by a 16 to 21% increase in value added per employee, and detect no effects on total factor productivity.

We also analyze and detect no effects on the exit rates among the universe of initial incorporations, suggesting selective attrition is unlikely to explain our results.

Third, we turn to wages, the theoretical transmission mechanism through which hold-up would discourage capital investments, whereby labor would grab a larger share of the value-added pie once capital is installed. We start by studying administrative earnings outcomes without composition adjustment, finding at best small and insignificant wage increases in shared governance firms (compared to control group firms). For median wages, we find statistically insignificant point estimates between 2.0 and 3.4%, with similar results for the mean wage. Turning to the wage structure within the firm, we do not

find evidence for wage compression, e.g., by comparing the 25th and 75th percentile. In light of potential composition effects in the workforce, we then isolate firm's wage policies by analyzing pay premia in form of firm fixed effects estimated off movers between establishments controlling for worker effects (Abowd, Kramarz, and Margolis, 1999; Card, Heining, and Kline, 2013). Here, we find small positive effects on pay premia with point estimates between 0.5 and 1.2% , with confidence intervals ruling out pay premia effects above 5%.

As a direct test of the wage-bargaining mechanism at the core of hold-up, we directly estimate and compare rent-sharing elasticities between firm-level value added per worker and wage premia in firms with and without shared governance. We document similar elasticities of about 0.09 – indicating that worker representation does not raise worker bargaining power over wages, or perhaps that labor cannot or does not exploit its formal authority in the realm of wage setting. With this small elasticity, the increase in value added per worker of about 20% would predict a small 2% wage effect, which is within the confidence interval for our estimated wage effect.

In a final step, we turn to firms' financials and the capital side of income, documenting that shared governance does not appear to reduce firms' external-finance capacity. There is no clear effect on profitability or measures of financial constraints. Leverage is unchanged, although interest payments over debt are slightly reduced, albeit not statistically significantly so. Lower interest payments could reflect an associated collateral channel or worker preferences for safer projects. However, our data does not contain comprehensive information on dividend payouts to owners.

While our evidence is inconsistent with the disinvestment prediction of the canonical hold-up mechanism, we present several alternative mechanisms that can account for our main findings. As supervisory boards are directly responsible for larger strategic and financial decisions, or indirectly act as the principals of the executive board, shared governance can plausibly be thought of as increasing worker bargaining power in corporate decision-making beyond wages. Extending a basic hold-up model to one in which workers also participate in input decisions (following Manning, 1987), we show that increasing worker bargaining power in these choices *increases* capital investment. Therefore, our simple model extension can potentially account for our main results of higher capital. We also show that our model would predict only small wage increases, within the confidence intervals for our wage effects, through a rent-sharing mechanism. Our model also accommodates the interpretation that the additional capital formation could reflect yet another agency conflict, if worker representatives push for investments to keep cash flow inside the firm at the expense of dividend payouts. We also discuss richer and more optimistic

models, in which shared governance may crowd in investment in a mutually preferable way, either by facilitating cooperation, by institutionalizing communication, or through repeated interactions between labor and capital (Van der Ploeg, 1987).

Our finding that worker participation in firm governance raises capital formation contributes to the empirical literature on hold-up. The existing evidence is mixed, perhaps due to econometric challenges (Menezes-Filho and van Reenen, 2003). Several studies document a negative effect of unionization on investment (see, e.g., Connolly, Hirsch, and Hirschey, 1986; Hirsch, 2004) while others find no evidence for hold-up effects (see, e.g., Machin and Wadhwani, 1991; Card, Devicienti, and Maida, 2014). More directly relevant to our study, two studies analyze the role of the broader institution of codermination on investment in Germany. First, in a correlational analysis, Addison et al. (2007) find that establishments with works councils, i.e. shop-floor codetermination, do not have lower investment than those without them. Second, Rapp et al. (2019) find positive investment effects of worker board representation during the financial crisis based on a propensity score matching strategy among listed firms.

Our quasi-experimental research design also contributes to a small set of empirical studies on the effect of shared corporate governance on broader outcomes (reviewed in Addison, 2009; Scholz and Vitols, 2019), where most of the existing studies consider firms with and without codetermination, controlling for or matching on observable characteristics. Notable exceptions are Gorton and Schmid (2004), Lin, Schmid, and Xuan (2018), Kim, Maug, and Schneider (2018), and Redeker (2019), all of which use the parity (50/50) codetermination threshold at 2,000 employees, comparing larger firms with one-half employee representation on the supervisory board to smaller ones with one-third representation.[4] By contrast, we analyze a policy change that circumvents potential endogeneity concerns related to employment as an assignment variable. Moreover, our design analyzes a *persistent* change in shared governance rules rather than transitory exposure around employment cutoffs. Finally, our empirical variation has a baseline of *no* worker-elected directors in the control group, as opposed to the intensive margin shift from one third to parity representation (with shareholders still retaining the tie-breaking vote).

At a broader level, our paper contributes to the empirical literature on the effects of board composition such as on the effects of gender quotas (Bertrand and Schoar, 2003; Ahern and Dittmar, 2012; Matsa and Miller, 2013; Eckbo, Nygaard, and Thorburn, 2016; Ferrari et al., 2016; Bertrand et al., 2018; Hwang, Shivdasani, and Simintzi, 2018; Maida and Weber, 2019). A crucial difference between gender quotas and the setting we study,

[4]Svejnar (1981) analyzes the wage effects of the *introduction* of parity codetermination to industries without any codetermination in 1951 and 1976. Gurdon and Rai (1990) study the 1976 reform based on a survey of 63 firms.

board-level codetermination, is that worker representatives on the board are typically non-managerial workers who are *elected* by the non-managerial workforce rather than appointed by shareholders. Finally, our study relates to analyses of firm-level unionization (DiNardo and Lee, 2004; Lee and Mas, 2012; Frandsen, 2013).

In Section 2, we present and 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 assesses exit and specifically bankruptcy effects. In Section 7, we report on the effects on production outcomes, including our core result on capital formation. Section 8 studies the distributional consequences for wages, rent sharing, profitability and external finance. The last section concludes.

2 How Might Shared Governance Affect Firms?

We now discuss mechanisms through which shared governance affects firm and worker outcomes. Our point of departure is the influential hold-up view (Grout, 1984), formalized in Section 2.1, according to which shared governance discourages firms' investment by raising worker bargaining power in wage setting. Foreshadowing our main empirical results for positive capital effects, we present a hold-up model variant in Section 2.2 that additionally allows workers to participate in operational decisions, specifically investment. Here, shared governance can *raise* investment, overturning the basic hold-up prediction. This example is meant to demonstrate the fragility of the basic hold-up prediction rather than serving as our leading alternative model. We additionally review a broader set of alternative perspectives on the effects of shared governance in Section 2.3.

2.1 The Hold-Up View

By the hold-up hypothesis (Grout, 1984), institutions that give control rights to workers and hence increase workers' bargaining power strengthen their capacity to *extract rents*. Anticipating that labor will grab a larger share of the fruits from their investments, capitalists reduce investment. 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 and Snower, 1989), including the specific institution of shared governance (Jensen and Meckling, 1979).[5]

[5]Jensen and Meckling (1979) focus on the hold-up channel of codetermination: "Upon gaining control of the firm the workers will begin 'eating it up' by transforming the assets of the firm into consumption

In the version of the basic hold-up model we present below (following Grout, 1984), 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, and we derive the general case with endogenous labor in Appendix Section A.2. In our two-stage setting, capital K is purchased in stage 1 at price c, becomes productive at stage 2, and thereafter fully depreciates. There is no discounting between the two stages.

Stage 2: Wage Bargaining Hold-up emerges because wages, which are bargained over in stage 2, depend on output and the capital stock – which are predetermined at that point. Specifically, wages w are 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 and Ashenfelter, 1986; Abowd and Lemieux, 1993).[6] 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 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:

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

or personal assets." Regarding the broader consequences of codetermination, they write: "It will become difficult for the firm to obtain capital in the private capital markets. As this continues some firms will simply go out of business and others will reach the point where the returns on investment are so high that even given the horizon bias further reduction in the capital of the firm makes the workers worse off. 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."

[6] As hold-up works through inside-value/rent sharing, this specification of the outside option in Nash bargaining is inconsequential for our focus, and therefore evades the ongoing debate about its theoretical and empirical deficiencies (Hall and Milgrom, 2008; Jäger et al., 2019).

Hold-up emerges because the firm makes investment decisions anticipating wage rule (2). The wage is a function of K for two reasons. First, K affects average output, of which share ϕ goes into the wage. But second, K also boosts the firms' outside option by c' per unit of capital, therefore 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, a "tax" that reduces investment incentives. 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 ($F_K = c$), when the wage effect consideration on its own would call for the same capital level as in the wage-taking case. *Underinvestment*, in the form of $F_k > c$ compared to the efficient level ($F_K = c$), emerges as long as $\phi > 0$ (workers have some wage bargaining power) and $c' < c$ (capital is at least partially sunk).

As a result, shared governance may exacerbate hold-up and reduce investment to the extent that the institution manifests itself through a higher level of worker bargaining power ϕ in wage negotiations:

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

2.2 The Fragility of the Disinvestment Prediction of Hold-Up

We now show that a simple but plausible extension that models worker participation as occurring also in bargaining over inputs, besides bargaining over wages, can overturn the underinvestment result. Specifically, we separate the decisions in a model of sequential bargaining: rather than a unilateral firm decision, in stage 1 the firm and the workers now jointly determine the capital stock by bargaining. As in Manning (1987), on whom our application to shared governance draws, we permit the wage and investment bargaining to feature different worker bargaining power parameters, with ι applying to bargaining

over investment:

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

where stage-1 investment choices are again made anticipating wage rule (2), the structure of which remains unchanged 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 $c' < c$. In that case, modeling shared governance as an increase in ϕ distorted investment further downward.

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. Shared governance may therefore alternatively be viewed as an increase in ι. To foreshadow why this channel will lead to higher rather than lower investment, note that here workers care about the capital choice K solely because of its effect on wages. 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 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 choice ι overturns the Grout (1984) underinvestment result ($F_K > c$) to *overinvestment* if $c' < c$ (then $F_K = c' < c$).

The general bargained capital level K^* under $\iota \in [0, 1]$ is given by:

$$F_K(K^*, \bar{L}) = c - (c - c') \times \left[\frac{(\iota - \phi)(F(K^*, \bar{L}) - b\bar{L} - c'K^*) + \iota(c' - c)K^*}{(1-\phi)(F(K^*, \bar{L}) - b\bar{L} - c'K^*) + \iota(c' - c)K^*}\right]. \tag{9}$$

Here, K^* depends on ι as follows:

$$\frac{dK^*}{d\iota} = \frac{-(c-c')(F - b\bar{L} - c'K^* + (c' - F_K)K^*)}{(1-\phi)[F_{KK}(F - b\bar{L} - c'K^*) + (F_K - c')^2] - (c-c')[F_K - c' + \iota F_{KK}K^*]} \tag{10}$$

This expression (which we formally evaluate in Appendix A.1) is positive, so K^* is increasing in ι, as long as $\phi > 0$ and $c' < c$.[7] If given a chance, workers will bargain to raise capital investment, as they will benefit in stage-2 wage bargaining from the higher production. Hence, increasing worker bargaining power in operational decisions such as capital choices may mitigate hold-up and lead to efficient investment, and even overinvestment.

In sum, this extended model of labor-capital interaction serves as a proof of concept that a given institution boosting workers' control rights need not crowd out, and can in fact crowd in, investment.

Hold-Up and Profits Importantly, in this particular model, hold-up is still active; in fact, the prospect of wage bargaining drives workers' push for more investment. Moreover, here the increase in investment 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), perhaps explaining why at least individual capitalists may not voluntarily adopt codetermination even if doing so may increase efficiency. Our focus on the capital effects thereby echoes the broader debate between Jensen and Meckling (1979) and Levine and Tyson (1990); Freeman and Lazear (1995).[8] (Some alternative views reviewed in Section 2.3 would predict an increase in profitability.)

Employment and Capital-Labor Ratios In Appendix Section A.2, we also derive the comparative statics with endogenous choice of labor L, determined by bargaining along with capital. If labor and capital are complements ($F_{LK} > 0$), the effects of changes in ϕ or ι on labor have the same signs as the respective effects on capital (and opposite for $F_{LK} < 0$). Importantly, the model can therefore rationalize positive or neutral effects of increases in ι on labor. In Appendix Section A.3, we further show that the capital-labor ratio will increase with ι as long as the capital-labor complementarity is not too large. These predictions will be qualitatively consistent with our empirical evidence. Related models

[7] 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 ($F_K = c$).

[8] Moreover, our model and empirical design considers individual firms' capital decisions at the intensive margin. Entry and exit may imply additional aggregate capital effects at the extensive margin. Our empirical assessment of effects on entry and legal form choices (Section 4.3) as well as on exit effects (Section 6) does not detect extensive-margin patterns.

with an insider/outsider perspective, in which worker representatives might advocate to keep employee numbers low in order to increase the capital-labor ratio and to reap ensuing rents (Lindbeck and Snower, 1989; Johnson, 1990), would predict a decline in employment – in contrast to our findings.

Dynamic Aspects There are several alternative theoretical resolutions of the hold-up problem that shift the structure of bargaining. For example, under simultaneous rather than sequential bargaining over wages and investment in the first stage, the parties can neutralize hold-up and also reach the efficient investment level (Crawford, 1988). Here however, shifts in bargaining power from shared government would not affect the always-efficient capital choice, unless shared governance would shift the regime from inefficient to efficient bargaining. Similarly, 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.

Capital Markets Firms might strategically increase debt to counter hold-up problems in response to worker bargaining power increases (Matsa, 2010; Baldwin, 1983; Dasgupta and Sengupta, 1993; Subramaniam, 1996). Alternatively, worker representatives may prefer safer projects, hence lowering capital costs and permitting higher leverage and investment, consistent with the negative industry-level association between unionization and bond yields (Chen, Kacperczyk, and Ortiz-Molina, 2011) and the positive firm-level relationship between employee representation and leverage in Germany (Lin, Schmid, and Xuan, 2018).

Corporate Governance Given that workers still only hold a *minority* position on the board, in principle capital could always outvote labor. However, anecdotal evidence documents that codetermined supervisory board make most decisions unanimously (Gold, 2011; Steger, 2011). A potential explanation is that the minority position incentivizes worker representatives to become moderates and seek cooperation with shareholder representatives (Thelen, 1991). Even then, the increased diversity of objectives on a codetermined board could decrease managerial accountability (Tirole, 2001, 2010, p. 59-60). Managers and workers may also collude to further corporate decisions, in particular in

form of higher investment, that leave shareholders worse off. Together, they may try to transform cash flow into illiquid corporate assets rather than dividends, and engage in empire-building (as in the agency conflict mechanism in Jensen and Meckling, 1976). (We will not be able to study dividends or stock prices in our data, since most of our stock corporations are unlisted.) More broadly, increasing worker bargaining power has also been hypothesized to lead to an entrenchment of incumbent workers, perhaps at the expense of outsiders (Lindbeck and Snower, 1989; Pagano and Volpin, 2005; Atanassov and Kim, 2009). In contrast, monitoring through worker representatives could also be more stringent than through shareholder representatives; for example, if executives wield more influence over shareholder directors (Hermalin and Weisbach, 1998). Similarly, worker-elected directors could have longer horizons and more at stake compared to outside shareholder directors with limited incentives, or may act to counterbalance short-termism among executives (who may have an incentive to forsake investments in order to boost current earnings, as in Stein, 1989).

Incomplete Contracts Shared governance could facilitate relational contracts (Baker, Gibbons, and 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 and Hart, 1981; Malcomson, 1983; Tirole, 1986; Freeman and Lazear, 1995), leading workers to disregard information from management.[9] More generally, worker representation might lead firms to honor implicit contracts and thus resolve hold-up problems leading to underinvestment by workers (Hogan, 2001; Englmaier and Segal, 2012), as with firm-specific training (Becker, 1962; Hashimoto, 1981) or back-loaded compensation (Lazear, 1979; Gibbons and Murphy, 1992).[10] 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).

[9]In fact, several observers have associated the cyclical stability of the German labor market with "employment pacts" between firms and worker representatives aimed at preventing bankruptcy or layoffs (Dustmann et al., 2014; Bellmann, Gerner, and Laible, 2016), perhaps facilitated by codetermination (Rehder, 2003; Kim, Maug, and Schneider, 2018).

[10]See also Acemoglu and Pischke (1999a,b); Acemoglu, Aghion, and Violante (2001) for similar arguments related to unions, and Shleifer and Summers (1988) for inefficient distribution between workers and shareholders in the presence of implicit contracts. More generally, MacLeod and Malcomson (1993) analyze contracts that may generate efficient investment with two-sided specific investments.

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; Freeman and Medoff, 1985; Kochan et al., 2019). Worker voice could also have direct productivity-enhancing effects by fostering information flows and cooperation.[11] Finally, voice might increase job satisfaction and performance via workers' perception about procedural justice (Greenberg and Folger, 1983).

3 Institutional Context and the 1994 Reform of Shared Governance

We describe shared corporate governance in Germany and the 1994 reform we study, which sharply reduced worker representation on corporate boards in certain kinds of newly incorporated firms. We also briefly review wage setting institutions.

3.1 Shared Corporate Governance in Germany

Corporate Governance in Germany Like many other countries in continental Europe, Germany has a two-tier board system with a supervisory and an executive board, illustrated in Figure 1. 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.

Shared Governance Two legal institutions allow for a direct participation of workers in their employer's decision-making: worker representatives on the supervisory board and works councils.[12] The variation we study concerns mandates for worker representatives on the supervisory board, an institution introduced in the early years of the Federal Republic

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

[12] 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.

of Germany.[13] Worker representatives are elected by the firm's non-managerial workforce in general, secret, equal and usually direct elections, organized by works councils (*Betriebsräte*); board representatives frequently also serve on the works council.[14] Importantly, workers are formally involved in governance but are not de jure residual claimants of profits as, e.g., in employee-owned firms (Kruse, Freeman, and Blasi, 2010; Pencavel, 2013), so they potentially benefit from investments but not directly from higher profits or payouts. Once elected, the worker representatives are co-equal directors with the shareholder representatives and directly involved in, e.g., selection of the executive board and other larger strategic and financial decisions. 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 board seats, which range from zero to full parity, and vary by the company's legal form and size (employee count) and founding date. (While ownership structure is another factor, exempting family-owned and state-owned firms, which we drop these firms from our sample.) Our variation is in form of a mandate for zero or one third worker-elected directors, illustrated in Figure 1 Panels (a) and (b). Our variation applies by legal type among 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)*) or individual merchants had always been exempted from *any* board representation. In contrast, the rules for stock corporations were sharply changed by a 1994 reform on the basis of incorporation date, as we describe below.

Finally, in firms with 501 to 2,000 employees, workers elect one third of the seats no

[13]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.

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

matter the legal form. (Our research design does not condition on firm size and hence is intent-to-treat, as discussed in Section 4.2.) In very large firms with more than 2,000 employees, workers elect 50% of the supervisory board 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 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. Figure 2 and Table 1 illustrate the changes in the mandate induced by the 1994 reform. 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.[15] Figure 1 Panel (a) illustrates corporate governance in these corporations without the worker-director 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

[15]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. In our sample of stock corporations, which are generally larger, still only 14% of firms reach the 500 employee threshold (but if starting out smaller were still subject to the reform-induced differential along their growth path after entry). 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, and 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. We also analyze codetermination at the corporate group level as well as firm's subsidiary status (see Appendix Table D.2 and associated table note).

in all stock corporations (up to 500 employees), *including* existing ones, to harmonize rules between stock corporations and LLCs. 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.[16]

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.[17] 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 abolition of the shared governance mandate as described above, the 1994 law included several other changes (e.g., rules for use of profits and for 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.[18] In principle, such

[16]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*).

[17]See, for example, Raiser, Veil, and 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, including due to switching costs. On aggregate, such evasion behavior would show up as an increased exit rate in our survival analysis, which we do not detect.

[18]For example, the Frankfurter Allgemeine Zeitung, perhaps the leading newspaper of record in Germany, commented at the time that the proposed law change could be considered a mere placebo, were it not for

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 (but there may have been common effects on both older and younger stock corporations, which our difference-in-differences design would net out when studying the sample of firms in their mature stages).

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, and 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, and Schönberg, 2009; Kügler, Schönberg, and Schreiner, 2018), and the prevalence of opening clauses has risen (Brändle, Heinbach, and 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 8.1. Moreover, there is considerable between-firm dispersion in wage premia even within industries (Card, Heining, and Kline, 2013), and idiosyncratic shocks to firms, e.g., corporate tax changes or labor supply shocks, affect wages (Fuest, Peichl, and Siegloch, 2018; Jäger and Heining, 2019).

4 Data and Empirical Methodology

We describe the data as well as our difference-in-differences methodology. Our main firm-level data set covers the early 2000s onward – the median year firms are first observed in our sample is 1999, five years after the 1994 law change (see Appendix Table D.1 and

the changes to worker representation on the board ("Nicht nur weiße Salbe", *Frankfurter Allgemeine Zeitung*, May 27, 1994, p. 13).

Appendix Figure C.1). Our design thus measures treatment effects for firms in mature stages, although we will supplement our analysis of some outcomes with data for the earlier years.

4.1 Data

Our analysis relies on information from four data sources, with further details also on variable construction in Appendix Section B. First, our main data set is firm-level financial and production data from Bureau van Dijk (BvD). Second, we draw on matched employer-employee data merged to the BvD firm-level data. Third, we study a comprehensive panel of firm entry and exit from 1991 onward in the Mannheim Enterprise Panel. Fourth, to check compliance with the board composition mandate, we draw on the Hoppenstedt Aktienführer.

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

Firm Panel Data: Bureau van Dijk (BvD) Our main data source is firm-level panel data on balance sheets and income statements from BvD's Amadeus and Orbis Historical data sets, the largest available data set 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. Going forward, we will refer to our merged data set as our Bureau van Dijk (BvD) data set.

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. We report all procedures in detail Appendix Section B.2. Specifically, 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% 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 incorporation date – by dropping firms wholly owned explicitly by one family or by individuals that share the same last name.[19]

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 a histogram of firm-year observation in our sample in Appendix Figure C.1, with 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% (see our summary in Appendix Section B.1.1 and for a detailed description Schild, 2016; Antoni et al., 2018, , who also describe the linking process). We implement several validation exercises to check the quality of the matched data and report results in Appendix Figure C.3.

Survival Analysis of All Entrants: Mannheim Enterprise Panel (MUP) We further draw on firm panel data from the Mannheim Enterprise Panel (MUP) provided by Zentrum für Europäische Wirtschaftsforschung (ZEW) and described in Bersch et al. (2014). The MUP data permits us to comprehensively study incorporations and exits from 1991

[19]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., Gottschalk et al., 2014). 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.

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 effects in the early years beyond survival.

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, where we stack firm-level panel data:

$$Y_{ft} = \alpha + \sigma \cdot \mathbb{1}(\text{IncDate}_f < 0) \times \text{StockC}_f + \gamma \cdot \mathbb{1}(\text{IncDate}_f < 0) + \delta \cdot \text{StockC}_f + X'_{ft}\beta + \epsilon_{ft}, \tag{11}$$

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 in Section 4.3.

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% 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.6 to C.14. 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.

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 our key outcome variables, we will also show 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% of shareholder firms in our sample cross the 500 threshold. Correspondingly, IV estimates for the effect of shared governance would scale up our intent-to-treat effects by about 14%. 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.

Rank and Distribution Specifications While our outcome variables will usually be continuous firm level variables, we will additionally report linear probability models for each of our key outcomes 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), which will additionally include specifications with the percentile rank within a year-by-legal form cell as the outcome variable. This additional approach permits us to rule in or out meaningful effect magnitudes on top of our continuous outcomes (for which the effect estimates oftentimes come with large confidence intervals, perhaps due to dispersed firm-level outcomes with heavy tails). We will also include nonparametric plots of the cumulative distribution functions of these core outcome variables by treatment and control group (legal form and incorporation time).

4.3 Threats to Identification

We now test for potential threats to identification of $\hat{\sigma}$ as the causal effect of mandated worker presence on the board.

Strategic Delay of Incorporation: McCrary Test First, 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 3, 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 Second and relatedly, more firms may substitute into the legal form of stock corporation after the reform than LLCs. Figure 3 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.3. 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. 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 Third, 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 (11). Figure 4 (detailed effects in Appendix Table D.4) reports these treatment effects for our main analysis sample in the BvD data. The reform did not statistically significantly affect the firm composition with regards to any individual industry. The coefficients are also jointly insignificant in an F-test ($p = 0.91$). In addition to actual industry selection effects of the reform, our test also rules out spurious composition effects from, e.g., business cycles or trends around the reform cutoff (see, e.g., Geroski, 1995; Geroski, Mata, and Portugal, 2010).

Having ruled out industry composition effects in our main analysis sample of firms observed in the BvD data, we now also assess composition effects among 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 ($p < 0.01$). Potential reasons why these mild composition effects do not show up in our BvD analysis sample are perhaps because the MUP data set does not permit us to impose our sample restrictions (government and family ownership, and the employment minimum of ten employees). Alternatively, exits may quickly iron out initial composition differences.

Placebo Reforms Fourth, in Appendix Tables D.5 through D.10 we estimate treatment effects of placebo reform four and eight years after the actual reform, for each outcome variable. We counterfactually assume that the reform and cutoff rule occurred on August 10, 1998 or August 10, 2002, respectively, and consider analogously chosen new samples of shareholder and LLC cohorts incorporated within a two-year bandwidth around each of these two reforms.[20] This exercise sheds light on the potential threat of spurious findings due to trends (e.g., if stock corporations capital intensity grew cohort-by-cohort by more than in the control legal forms) or differential lifecycle paths (e.g., if stock corporations incorporated earlier generally have more capital-intensive modes of production than stock corporations incorporated slightly later, compared to the same difference for LLCs).

5 Composition of Supervisory and Executive Boards

In a first step, we study the effect of the reform on the share of worker representatives on the corporate board and then turn to characteristics of supervisory board members, as well as of executive board members. This step not only serves as an intervention check but also documents a channel by which the institution may shape corporate governance and ultimately affect firm outcomes.

5.1 Validation Test: Realized Shifts in Worker Representation

We verify that the reform shifted worker representation by incorporation date. Figure 5 shows the 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 listed corporations. We restrict the sample to stock corporations founded between 1989 and 1999 for which board composition data is reported.[21] 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 whom the reform changed the rules), there is a stark difference: workers comprise, on average, 29% of the supervisory board of stock corporations incorporated during or before 1994. In sharp contrast, workers comprise only around 3% of the supervisory board of stock corporations founded during

[20]The placebo effects build on, and can be interpreted through, the logic of randomization inference under the assumption that the timing of the reform cutoff was random (as in Ganong and Jäger, 2018).

[21]Specifically, we only consider firm-year observations for which 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. In addition, we only rely on data from the 1990s due to a structural break in reporting in 2000.

or after 1995. The non-negative worker share in these corporations is likely due to a small amount of measurement error, e.g., because 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 discernible difference in worker representation – both around one third – confirming that the comparison is not driven by differential reporting or data quality after 1994.

5.2 Board Demographics

We now analyze the effects on the demographic composition of the supervisory and executive boards (see Section 3 for an overview of the institutions). Our research design builds on variation in worker presence on the supervisory board. We seek to understand effects on the composition of the supervisory board to understand which demographic groups will be more likely to be represented under shared governance. The analysis of the supervisory board also serves as an intervention check, allowing us to assess whether the reform indeed affected board composition.

We report the estimates on board composition in Table 2, based on specification (11). Panel A reports the results for supervisory boards; Panel B does so for executive boards. The 1% symmetric winsorization, two-year bandwidth is our main specification, and we report additional variants with smaller and larger bandwidths in Appendix Figures C.6 through C.14. We use the BvD board data set offering a snapshot of board members between 2016 and 2018, with information on names and gender. (Board member information is missing for 99% of observations in the earlier waves.) We then additionally study the same outcomes on the executive board to assess whether shared governance affects manager selection at the highest corporate level – a natural transmission channel, as the supervisory board appoints and controls executives.

The BvD data set also does not differentiate worker and shareholder representatives, so we cannot separate composition effects into direct effects from the worker representatives differing in their composition, and indirect or spillover effects by which the composition of shareholder representatives may shift. Due to data limitations, these demographic characteristics are the only relevant other board-level outcome variables we can study. [22]

[22] The limited scope of the data do not permit us to credibly study executive tenure or turnover, while board size is capped based on size-dependent rules in Germany. These high-level executives are also not differentiated in our administrative data (due to coarse manager occupation codes and capped earnings observations).

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%.[23] 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.5 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 Next, we document the effect on members holding nobility titles, a marker of status and social or network capital (Bourdieu, 1986). We classify board members on the basis of their names ("*von*", "*zu*", "*Graf*", "*Gräfin*","*Baron/in*", "*Freiherr/-frau*"). Only 0.1% of the population has such a title. Columns (3) and (4) of Table 2 show that shared governance reduces this number by about 4ppt (SE 0.03), and the share by 1.4ppt (SE 0.007), a 60% decrease since only 2.3% of supervisory board members in control group stock corporations have titles of nobility. On the executive board, we find a sharp reduction in any presence by 3ppt (SE 0.014) relative to a 6% baseline, a relative effect of -50%. Similarly, the share of aristocratic executives is reduced by about 0.1ppt from a control group mean of 0.4%, although the estimate is not statistically significant.

As with all outcomes, we also test for, but do not find, any corresponding placebo effects for placebo reforms in later years and for different firm cohorts (Appendix Tables D.5 and D.6).

Academic Titles In Germany, doctorate degrees are regularly listed as titles in names, as in our data. Besides the academic qualifications, professional doctorates are sought as and are indicative of a position in the economic elite (Hartmann and Kopp, 2001). In the average control stock corporation, about 23% of supervisory board members hold doctorates or (likely largely nominal) professorial positions ("*Dr.*" and "*Prof.*"). In columns (5) and (6) of Table 2 Panel A, we find no significant effect on the probability of at least one supervisory board member holding a doctorate (insignificant 3ppt increase), nor the share of supervisory board members (insignificant 3ppt decrease). On the executive board (Panel B), we find marginally significant positive effects on the indicator outcome of 8ppt (SE 0.05) and on the continuous share of about 3ppt (SE 0.017). One potential interpretation, according with our findings for nobility among executives below, is that

[23] 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% female employment (§ 76 II 4 *BetrVG* 1952).

worker representatives push for executives with more formal qualifications rather than network capital.

6 Firm Survival

We next document no evidence for effect of shared governance on firm exit. We draw on the Mannheim Enterprise Panel containing comprehensive information on all incorporations in Germany from 1991 onward (while our main BvD data will consist of panels from future years). Figure 6 Panel (a) plots the survival probability of stock corporations and LLCs separately by incorporation date, and separately for firms incorporated within a two-year window before the reform ("Old") and those incorporated two years after ("Young"). Across groups, around 50% of firms still exist 20 years after incorporation. We find slightly lower survival rates for younger firms, and, separately, 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 (years elapsed since incorporation). 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. In sum, we do not find evidence for differential attrition as a function of board-level representation of workers.

We further identify the effects on the probability of experiencing a bankruptcy, the only other exit-related variable the MUP data offer. This category is not only a subcategory of exits. Alternatively, non-bankruptcy exits can occur, such that the firm was re-incorporated or merged with another firm. We plot the effect on the bankruptcy exit rate using blue squares in Figure 6 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.

7 Production and Capital Intensity

Below, we present the evidence on the effects of shared governance on production and capital intensity, among our core outcome variables. We again present estimates at the two-year bandwidth as our main specification, and report additional variants with smaller and larger bandwidths and winsorization levels in Appendix Figures C.8 and C.9.

As discussed in Section 4.2, for our key outcomes, 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). These tables also include a specification with the percentile rank within a year-by-legal form cell as the outcome variable in the first column. These specifications will allow us to rule in or out magnitudes of effects beyond those implied by our continuous outcomes, which will oftentimes imply effects with large confidence intervals, perhaps due to dispersed firm-level outcomes with heavy tails. In Appendix Figure C.15, we complement these regression analyses with nonparametric plots of cumulative distribution functions by treatment and control group (legal form and incorporation time). The key outcome variables for these additional distributional specifications are employment, fixed assets, value added per worker, fixed assets per worker, capital share, and value added over revenue.

Appendix Tables D.14 and D.15 show robustness to excluding firms in former East Germany, where firms were restructured in the early 1990s.

7.1 Firm Scale: Output and Inputs

We start with measures of production scale on the output and input sides, reporting effects in Table 3. These measures of firm scale will be dispersed and the estimates will come with wide confidence intervals. They are nevertheless a natural starting point as firm size also serves as a marker of productivity in many models (see, e.g., Lucas, 1978; Melitz, 2003). Our main and more precisely estimated results will be related outcomes normalized within the firm and thus less scale-dependent, which the rest of the empirical section will study.

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. This result also informs our subsequent interpretation of effects on production inputs.

Employment We consider both employment in the BvD data and the administrative data from IAB. The employment concepts differ as BvD reports a head count of all employees, including ones abroad, while the IAB data only includes employment subject to German social security. The latter employment concept tends to yield relatively lower levels, but

is also more relevant for determination of shared governance (see, e.g., Annuß, 2019, DrittelbG § 3, Rn. 2).

We first analyze effects on log employment and find small, positive, statistically insignificant effects summarized in columns (3) and (4) of Table 3. In our most fine-grained specifications, we find effects of 0.05 in both the BvD and IAB data (SEs 0.13 and 0.10, respectively). While the point estimates in the BvD data range between 0.05 and 0.13 between specifications, the IAB data reveal estimates between 0.04 and 0.07. In all cases, the confidence interval includes zero. Similarly, Appendix Table D.11 shows no significant effects on the distribution of employment, and the confidence interval for the effect on the percentile rank of employment ranges from -4.5 to 5.4.

In a further step, we draw on the Orbis-ADIAB data to provide a time series of employment in the four firm groups. Appendix Figure C.5 plots employment relative to incorporation both as an inverse hyperbolic sine transformation (Panel (a)) and in levels (Panel (b)). For each firm (among those incorporated between 1990 and 1999 and still active during the later years during the Orbis-IAB linkage), we generate a balanced panel of its IAB-based employment aggregate, which we set to zero if no IAB employment is recorded. The figures show that employment in older stock corporations, mandated to have worker-elected directors regardless of size, grows more rapidly on average after incorporation, ultimately ending up somewhat larger than slightly younger shareholder firms not subject to the mandate, although our econometric analysis shows that we cannot detect statistically significant differences.

In columns (5) and (6) of Table 3, we additionally 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).[24] About 14% of firms in the post-cutoff-date-incorporation group cross this threshold in either data set. (This small number also implies that an IV interpretation would only slightly scale up our reduced-form effects in our intent to treat design.) Positive treatment effects on this outcome would indicate that new stock corporations might have sought to avoid their size-dependent codetermination mandate by remaining small. In the data, we find no evidence for such a pattern, with confidence intervals ruling out even small positive effects. In the most fine-grained specifications, we find a point estimate of 0.007 (SE 0.033) in the BvD data and of 0.017 (SE 0.031) in the IAB data. Consistent with survey evidence in Albach et al. (1988), we thus conclude that firms do not appear to strategically avoid shared governance by remaining below the 500-employee threshold.

[24]See also our analysis of corporate structure and codetermination at the corporate group level in Appendix Table D.2 and the discussion in the detailed table note.

Interpreted through the lens of our model in Section 2.1, the findings on employment suggest a positive capital-labor complementarity. In addition, the results are hard to square with rent-seeking models in which higher worker bargaining power results in lower employment levels (Lindbeck and Snower, 1989; Johnson, 1990).

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 the effect on log fixed assets are around 0.43 to 0.47 with confidence intervals excluding zero, stable across specifications, which 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.[25] Importantly, our placebo analysis in Appendix Table D.7 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).

These estimates on capital formation are a central result of our analysis, implying that either shared governance does not depress capital formation through hold-up, or, if it does, that the hold-up mechanism is ultimately dominated by positive counteracting forces crowding in investment, generating a large positive net effect. We will devote much of the rest of the paper to unpacking and understanding this result.

Intermediate Inputs The larger (though noisily estimated) value-added effects compared to revenue are perhaps indicative of more in-house production and less outsourcing when worker representatives are on the board. Indeed, we find a large reduction in a key measure

[25] In Appendix Table D.11, we document a marginally significant increase in the percentile rank of fixed assets of 4.8 (SE 2.6), allowing us to rule out effects on the average percentile rank below -0.24. We dissect the effects on the distribution of fixed assets further in the remaining columns. For fixed assets below the 75th percentile threshold, we find positive but small and noisily estimated higher probabilities of the treated firms exceeding any given percentile threshold of the control group distribution; for example, a 5ppt (SE 3ppt) effect of exceeding the 25th percentile (holding 234,220 Euro in fixed assets). Yet the largest and most precisely estimated effect is a 8ppt (SE 4ppt) higher probability of exceeding the 75th percentile (corresponding to around 7 million Euro in fixed assets).

of intermediate input use through which outsourcing may show up in our BvD data, namely intermediate inputs, reported in the last column of Table 3. Concretely, we find changes in intermediate inputs of between -1.16 to -0.7 (log).[26] Juxtaposted with the large but noisily estimated increase in value added and with no clear effects on total revenue, this result points towards a shift in the mode of production towards more in-house production and less outsourcing. We will provide more granular analyses of potential shifts in the production function below, studying firm-level accounting data and worker-level data to study the occupational structure below in Section 7.3.

7.2 Productivity, Capital Intensity and the Capital Share

We next dissect the positive 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). Some capital may not necessarily be directly productive but serve as amenities (e.g., nicer break rooms), or assets serving as a store of value not used in production (e.g., undeveloped land), or simply book values of assets may not reflect market productive values. Below, we therefore study additional outcomes that corroborate the productive capital effects.

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.[27] Finally, the placebo analyses in Appendix Table D.8 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.

[26] We find negative but substantially smaller and not statistically significant effects on intermediate inputs for placebo reforms in 1998 and 2002 (see D.7), making it unlikely that trends or lifecycle patterns by legal form could fully explain the large effects we find in 1994. A caveat to the interpretation of intermediate inputs is that the variable is not well filled in our data.

[27] Appendix Table D.12 shows that, on average, worker board representation increases value added per worker by 6 to 9 percentile ranks, with standard errors around 3 across specifications. Further analyses reveal that the effect manifests itself as a shift of nearly the entire productivity distribution to the right, starting with the treated firms having a 7ppt (SE 4ppt) higher probability of exceeding the 25th percentile of the control group distribution (which lies at 31,630 Euro of value added per worker), through the 90th percentile (a 12ppt higher probability with a 4ppt SE, at 163,000 Euro).

Capital-Labor Ratio Next, we juxtapose the value-added per worker effect with that on fixed assets per worker. We find that 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)). By reading the value added and fixed assets per worker assets together, the evidence thus points towards our aforementioned results reflecting an increase in productive capital.[28] Again, our placebo analyses in Appendix Table D.8 show substantially smaller and statistically not significant effects on the capital-labor ratio for placebo reforms in 1998 and 2002. Interpreted through the lens of our model in Section 2.1, the findings on the capital labor ratio suggest a positive but not too large capital-labor complementarity.

Total Factor Productivity In column (5) of Table 4, we further study effects on log total factor productivity (TFP, which we construct based on fixed assets as detailed in Appendix Section B.3). Firm-level TFP essentially stays flat, with zero or very small, noisily estimated negative effects. We would have expected the efficiency measure to be strongly negative if the additional capital in shared governance firms were unproductive (i.e. the reverse implication of our value added per workers analysis above). Moreover, the TFP result helps to adjudicate between different views of shared governance as the implications of some theories reviewed in Section 2 would have shown up through TFP. (As our TFP estimates have large confidence intervals, we cannot rule out these alternative theories.)

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).[29] In addition, the placebo analyses in Appendix Table D.8 show no effects on the capital share for placebo reforms in 1998 and 2002.

[28] Appendix Table D.12 shows that the capital-labor ratio increases by about 7 percentile ranks (SE 2.5). Mandating worker board representation increases the probability of exceeding the 25th percentile of the control group distribution (which lies at 3,360 Euro of fixed assets per worker) by 10ppt (SE 3ppt), with similar results for the median (12,050 Euro of fixed assets per worker), and also a 10ppt effect (SE 3ppt) of exceeding the 90th percentile cutoff (corresponding to a large level of around 360,000 Euro of fixed assets per worker). These analyses may rationalize the large quantitative effects given the skewed distribution of the continuous variables related to firm scale. Note also that here we find smaller point estimates when winsorizing at the 2 or 5% level (Appendix Figure C.9).

[29] Appendix Table D.13 shows an average increase of 8.4 to 9.6 percentile ranks (SE 3). This effect appears driven by a 14ppt (SE 5ppt) higher probability of the treated firms shifting from below to above the median capital share level of the control group distribution (i.e. 0.24), and a 15ppt higher probability (SE 5ppt) of exceeding the 75th percentile (i.e. 0.46).

Overall, these pieces of evidence are consistent with shared-governance firms having shifted towards a more capital-intensive mode of production. This core result is, again, in sharp contrast to the disinvestment predicted by the basic hold-up view of shared governance.

7.3 Outsourcing, Skill Structure, and Turnover

Next, we shed further light on firms' mode of production, tracing effects of shared governance on outsourcing and in-house production use. We also studyd the structure of labor demand, such as workers' skill, occupational structure, tenure, and separations. Except for in-house production, we draw these worker-level outcomes from our IAB matched employer-employee data merged to BvD firm data.

In-House Production Before moving to effects on the structure of the workforce, we study effects on outsourcing using firm-level accounting data in column (7) of Table 4. Higher capital intensity could emerge because firms respond to labor power by outsourcing stages of the value added chain that may be particularly labor-intensive.[30] We study this possibility by estimating effects on the firm-level ratio of value added to revenue, as a proxy for in-house production. This share of sales produced in-house actually *increases* by 12 to 17ppt (SE 6 to 7ppt), compared to a control mean of 0.43, and is statistically significant across all specifications.[31] From this perspective, outsourcing of labor-intensive tasks is unlikely to explain the increased capital intensity of shared governance firms.

We also assess placebo reforms in 1998 and 2002 and find substantially smaller and statistically not significant effects (see Appendix Table D.8).

At a broad level, the finding that shared governance appears to raise in-house production is consistent with anecdotal evidence for worker representatives opposition to international offshoring and support of expansion of investments in existing plants. Out- and insourcing decisions are frequently subject to negotiations between firms and worker representatives. For example, the car manufacturer Opel recently conceded to insource

[30] Another mechanism raising capital intensity may be that owners strategically substitute into labor-substituting capital to offset worker participation. However, we have, if anything, already documented a positive albeit statistically insignificant effect on employment, inconsistent with the substitution channel.

[31] In our complementary analysis in Appendix Table D.13, we document an increase in the percentile rank between 7.1 and 7.8 (SE between 3.2 and 4.2). The effect is particularly pronounced around the margin of the 75th and 90th percentile of the control group distribution. We find an 11.5ppt (SE 5.5) increase in the probability of crossing the 75th percentile of the control distribution (where value added makes up a share of 57% of revenue). We find a 14.2ppt (SE 4.7) increase in the probability of crossing the 90th percentile (74% of revenue). The confidence interval is between 5 and 23.4ppt. We find substantially smaller and insignificant effects for crossing the lower percentiles of the control group distribution.

previously outsourced production steps following pressure from worker representatives calling for higher investments to preserve existing production units.[32] Speaking about the goals that worker representatives can achieve in corporate boards, Reiner Hoffmann, the President of the German Trade Union Confederation, considers worker representatives on the board to be crucial for "well-balanced decisions" when it comes to outsourcing.[33]

Outsourceable Occupations For outsourcing to explain the capital intensity, the marginal task not outsourced in shared governance would need to be substantially more capital-intensive than the average task rather than following the pattern of outsourcing of labor-intensive tasks. We can directly test for this outsourcing channel in our matched employer-employee data by classifying occupations associated with outsourcing of labor-intensive tasks in Germany (cleaning, food services, security, and logistics as in Goldschmidt and Schmieder, 2017). This result is reported in column (1) of Table 5, where we dissect the skill distribution of the workforce. While not precisely estimated (t-statistics of around 0.6-1.6), the share of such labor-intensive outsourceable occupations exhibits positive point estimates, thereby, if anything, pointing to less rather than more labor-intensive outsourcing in shared governance firms.

These finding corroborate our result on in-house production above. Echoing our findings for non-negative employment effects, this more fine-grained investigation of the occupational structure with respect to outsourceable occupations is also inconsistent with the insider/outside view of worker participation (reviewed in Section 2.3).

Skill and Occupational Structure In the remaining columns of Table 5, we more broadly study the effects of shared governance on the skill and occupational structure of the firms. In columns (2) through (4), we find some evidence for a shift from lower- into higher-skilled worker groups, perhaps consistent with a shift towards capital-complementing skill types.[34] We find evidence that shared governance lowers the share of low-skilled workers by about 1.1 to 1.4ppt, implying an 8 to 11% reduction. The effects are not statistically significant in any specification. Our estimates also allow us to rule out effects below -3.2ppt. The decreased share of low-skilled workers appears to be offset by a roughly

[32]Source: "IG Metall vermisst weiter Investitionen bei Opel", *Frankfurter Allgemeine Zeitung*, October 30, 2019.

[33]Source: Magazin Mitbestimmung, 07/2016, *Hans Böckler Stiftung*.

[34]The analysis uses the employer-reported education measure, which distinguishes (i) low-skilled workers with no vocational training (in baseline making up 13% of the workforce of stock corporations); (ii) medium-skilled workers with a finished school degree and a vocational qualification (baseline of 58%); and (iii) high-skilled workers with a degree from a university or university of applied sciences (*Fachhochschule*) (baseline of 28%).

equal increase in higher-skilled workers, although the effects are not precisely estimated.

In columns (5) to (7) of Table 5, we study the occupation structure following the commonly used Blossfeld (1987) categorization of German occupations. We find positive point estimates consistent with shifts towards skilled manual labor (qualified manual occupations, technicians, and engineers), and perhaps accompanying the more capital intensive production, although the confidence intervals for each specification include zero.

Overall, the suggestive effects on skill composition could follow from the strong positive effects on fixed assets through the channel of capital-skill complementarity. We cannot separately disentangle whether worker participation in firm governance may have separate, direct effects on these outcomes.

Tenure and Separations Finally, in Table 6, 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% effect in logs, which is less precisely estimated. Zooming in to year-to-year separation rates, we find a small and statistically insignificant effects of 1 to 2ppt off a baseline of 0.20 in column (3). These results would be surprising with basic intuitions that more capital or skills should result in longer tenure (Oi, 1962), that giving workers voice may reduce exit (Hirschman, 1970; Freeman, 1980) or perhaps job valuation by employees, or that shared governance may reduce layoff risk (Kim, Maug, and Schneider, 2018). Given that tenure can serve as a measure of job quality (although we cannot differentiate quits and layoffs), the absence of positive effects may suggest that shared governance does not measurably increase retention and perhaps job valuation by employees.[35] We decompose the effects in columns (3) to (5), where we 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). This pattern, though imprecisely estimated, may be consistent with insider/outside dynamics (Lindbeck and Snower, 1989) or with with higher-tenured workers' skills being complemented by the capital boost, or, in light of our results to less oursourcing, with firms perhaps shifting towards a higher-turnover workforce.

[35]See also Frick (1996), Backes-Gellner, Frick, and Sadowski (1997) and Jäger, Schoefer, and Zweimüller (2019) for evidence on the relationship between works councils and worker turnover and layoff decisions.

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

Lastly, we study the division of income generated by the firms. In Section 7.2, we already studied the division of value income between labor and capital, and documented that shared governance raises the capital share, along with capital intensity in production. We now turn to distributional outcomes within each factor. We start with wages, drawing on the Orbis-ADIAB administrative data. The wage response is the transmission mechanism through which hold-up discourages capital investments, whereby labor grabs a larger share of the value-added pie once sunk capital is installed. In contrast to the hold-up view, we find neither wage increases (or decreases) nor increased rent sharing – consistent with, and in fact helping rationalize, our first key finding that capital formation is not curbed in shared governance firms. We then turn to capital income, studying profitability as well as other financial outcomes including measures of external finance capacity.

8.1 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 and Medoff, 1985, p. 82-85), perhaps also indirectly affected by informal norm establishment, as in the case of unions (see, e.g., DiNardo, Hallock, and Pischke, 2000; Western and 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 7 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 Fixed 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 drawn from Abowd, Kramarz, and Margolis (1999) specifications with worker and firm effects, thereby netting out worker selection (as we documented composition effects in Section 7.3). Concretely, we calculate pay premia analogous to Card, Heining, and Kline (2013) for the

period between 1990 and 2009. Thanks to the structure of our data we can directly estimate the pay premia using *firms* rather than establishments as the workplace unit (drawing on the entire Orbis-ADIAB data set). We find a 0.012 effect of shared governance on the firm premium with standard errors of 0.023, allowing us to rule out that firm pay premia increased by more than 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 and Medoff, 1985; Saez, Schoefer, and Seim, 2019, p. 82-85). We study log wages at the 25th, 50th, and 75th percentile at the firm level. We report outcomes in columns (3) through (5) of Table 7 and 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 in the specifications with industry-year effects, with slightly larger effects at the 75th compared to the 25th percentile. As an additional measure of pay compression, we also investigate whether shared governance affects the share of wages above the social security earnings cap. About 11% of workers in post-reform-incorporated stock corporations have earnings above the social security earnings cap. This share is not affected, with a point estimate of 0.012 (SE 0.010, allowing us to reject increases above 0.032). Finally, as a measure of within-firm inequality, we consider the log ratio of the 75th percentile to the 25th percentile wage within the same firm in column (7). Across all specifications, we find a small positive effect 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.

Executive Pay Executive pay is a firm outcome over which the supervisory board has direct formal control, and plays an important role in corporate governance as a solution to or as an example of agency conflicts (e.g., Bebchuk and Fried, 2003). Unfortunately, our administrative data miss executive compensation due to the social security cap, and executives are not clearly identified in the occupational code. We sourced a data set on executive compensation (a survey by Kienbaum Consulting covering 4,055 German firms), but due to the very small sample size of matched observations incorporated even within a five-year window around the 1994 reform (fewer than 200), we were unable to estimate effects on executive compensation with any meaningful precision.

8.2 Rent Sharing

We explore rent sharing by assessing whether shared governance affects the cross-sectional relationship between firm-level wages and productivity. Rent sharing therefore also pro-

vides a direct measure of the very 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 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 7 Panel (a), we first plot the relationship in the whole sample independent of legal form and incorporation date. We find a slope of 0.091 (SE 0.004), implying that a 10% increase in value-added per worker is associated with a 0.91% increase in wages. 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), indicating that we can rule out increases in the rent-sharing elasticity of more than 0.05. The DiD estimate is the coefficient on the interaction between value added per worker and an indicator for stock corporations incorporated before August 10, 1994 in a model where we also include base effects of value added interacted with cohort and legal form and base effects for these two indicators.

In conclusion, we find no evidence for workers capturing a larger part of value added per worker when they have board representation – consistent with and perhaps underlying in the first place the absence of hold-up patterns in capital formation.

Back-of-the-Envelope Calculation: Which Wage Effects Would One Have Expected? Finally, we also assess whether the wage and value added effects that we found 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. We also found a rent sharing elasticity of 0.091, well within the range documented

in Card et al. (2018) and Jäger et al. (2019). Taken together, these estimates would imply an effect of $0.216 \times 0.091 = 0.019$ on wages through a rent-sharing channel, supposing worker bargaining power over wages is unaffected by worker board representation. As Table 7 showed, our actual, estimated effect on pay premia of 0.012 (SE 0.023) is close to this implied wage pass-through of the productivity effect, and the confidence interval includes the model prediction.

In other words and in the framing of our formal conceptual framework in Section 2, our small wage effects do not necessarily imply that workers did not benefit from the larger capital stock and the larger surplus pie through the standard wage bargaining channel. More precisely, this mechanism implies small positive wage effects due to a low worker bargaining power in wage setting or equivalently the empirically small rent-sharing elasticities. However, our wage findings do suggest that shared governance does not change the degree of rent sharing at least at the firm level, such that workers in codetermined firms do not grab a larger share of the larger value-added pie.

8.3 Profitability and External Finance

We close our distributional analysis with the effect of shared governance on profits (our measure of the income of firm owners) as well as potential effects on sources of external finance more generally, studying debt and its costs, and proxies for external finance capacity and constraints.

Profitability The long-standing debate about the appropriateness of shared governance has centered around profitability: by the influential argument by Jensen and Meckling (1979), firm owners would voluntarily adopt shared governance if it were profitable. We consider 3x2 measures of profitability: EBITDA (earnings before interest, tax, depreciation and amortization), EBIT (earnings before interest and taxation) and net income (after interest, depreciation, amortization and taxation, i.e. the profit concept ultimately available to pay out to owners), each divided by either revenue ("profit margin"), or divided by total assets ("return on assets").

Table 8 reports varied effects on these profitability measures (Appendix Table D.9 presents placebo reform 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 in all 16 specifications. 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 effects on net

income over assets. Measured by the different profit outcomes we consider, the evidence does not suggest that shared governance lowers profits.

Debt Structure and Leverage In Table 9, we report effects on various financial outcomes (Appendix Table D.10 presents placebo form analyses). If anything, we find a negative effect of the average cost of debt, measured as interest payment over face value of debt, of three 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. This result suggests that external finance suppliers do not charge shared governance firms a premium – although we also document no increase in leverage (and only an insignificant decrease in liabilities over total assets, perhaps also driven by the denominator in light of the increase in assets). Together, the findings are consistent with shared governance firms perhaps running less risky operations (as perhaps preferred by labor representatives), or higher collateral levels as would be expected by 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; Baldwin, 1983; Dasgupta and Sengupta, 1993; Subramaniam, 1996; Lin, Schmid, and Xuan, 2018), although our reduced-form net effects may mask various offsetting channels and hence cannot cleanly test for one specific mechanism.

Indicators of Debt Capacity and Financial Constraints We complement the analysis of leverage and costs of debt by studying effects on the probability of being in the top 50% or top 20% 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, and Lundstedt, 2004; Farre-Mensa and 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. A complication in interpreting these indices is that they are direct functions of assets and other variables we have shown to be affected by treatment.

Liquid Assets In column (5) of Table 9, 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).[36]

Overall, shared governance does not appear to lower shareholder profits (and, if anything, appears to raise net income) or appear to reduce firm's external finance capacity. While it would be fruitful to measure market values and actual dividend payments, the BvD data does not contain these outcomes (and our stock corporations are typically not publicly traded). Our analysis therefore leaves unmeasured the extent to which these profits are distributed to shareholders rather than, for example, kept and reinvested in the firm. We discuss these questions in the conclusion section below.

9 Conclusion

We have studied a natural experiment in form of a 1994 reform in Germany that preserved worker representation on supervisory boards for some cohorts of corporations, while abolishing it for their slightly younger peers incorporated on or after August 10, 1994. The discontinuity generated by the reform lends itself to a difference-in-differences design and provides an empirical answer to the long-standing debate on how expanding worker voice and formal authority in corporate decisions may affect firm and worker outcomes.

Our core result is that shared governance is associated with an increase in capital formation – sharply contradicting the predicted disinvestment following the hold-up and agency cost views of shared governance (see, e.g., Grout, 1984; Jensen and Meckling, 1979). At the same time, we do not find increases in wages in these firms, nor more rent sharing.

While the evidence is inconsistent with the classical hold-up mechanism as a dominant channel, we offer an extended model of bargaining over inputs that could account for our findings. Here, boosting worker bargaining power over investment *increases* capital formation and ameliorates or even overturns the underinvestment problem. A prediction of such a model is that workers prefer higher capital investments, consistent with anecdotal evidence as well as stated objectives of worker representatives, for example:

> [S]hared governance per se opposes short-term shareholder interests. The focus is on the long-term safeguarding of the company through investments and innovations with participation of the employees [...].[37]
>
> Berthold Huber, 2004, VP of IG Metall (Metalworker's Union) at the time

[36]Our results are consistent with Redeker (2019) who finds a reduction in cash-holding when firms cross the 2,000-employee threshold requiring them to adopt parity codetermination.

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

Worker Board Representative, Deputy Chairman Siemens

Relatedly, worker representatives may take a longer-term perspective. Alternatively, their mandated presence on boards may crowd in investment by facilitating cooperation and long-term contracts between owners, managers and the workforce, perhaps by institutionalizing communication channels and repeated interactions. This reading of worker representation as a cooperative institution that helps overcome coordination issues could hence also rationalize our findings. While we do not provide direct evidence adjudicating between these specific channels, we have documented a sharp rejection of the disinvestment prediction of the canonical hold-up view, which, at least in the context of this institution and this reform, failed to hold up on net.

While we do not find negative profitability effects, in our data we cannot definitively study the effects of the additional capital formation on shareholder welfare, or whether it reflects yet another agency conflict inside the firm. For example, worker participation may lock in resources into fixed capital at the expense of dividend payouts to the owners, to ultimately raise or stabilize compensation, or to secure employment stability for entrenched insiders. Workers may also aggravate agency conflicts of an imperfectly incentivized management engaging in empire building and hence overinvestment. Such an interpretation may explain why at least individual capitalists may not voluntarily adopt codetermination (Jensen and Meckling, 1979; Levine and Tyson, 1990; Freeman and Lazear, 1995). A broader question is whether the additional capital stock brings the economy closer to social efficiency, for example if investment is inefficiently low in a market economy (Lancaster, 1973; Acemoglu, 2001).[38]

We close with some reflections on the institutional context of our experiment. Our cohort-based difference-in-differences design assigns a permanent corporate governance regime from firm entry onward, addressing the longer-run institutional scenario rather than an alternative experiment that would impose shared governance onto already-existing companies. The effects of such reforms without grandfathering may differ from ours. Moreover, our experiment occurred against the backdrop of and perhaps interacted with existing establishment-level worker participation through works councils, an institution with a long history in Germany and the second lever of shared governance besides board-level representation. On one hand, an interaction may increase effects of shared corporate

[38]Our model, following Manning (1987), provides one variant of the standard hold-up model in which shared governance increases workers' decision rights over capital choices. Hold-up is still active at the wage setting stage, and this very prospect of wage bargaining drives workers' pushing for more investment, thereby lowering shareholder profits. Increases in worker bargaining power over *capital* can improve efficiency in the presence of bargaining over *wages* (which would lead to underinvestment on its own following Grout, 1984), while lowering profits (and even, when the increases are sufficiently large, leading to overinvestment).

governance through information sharing, or by providing the worker-supervisors with leverage beyond their vote and voice on the board. On the other hand, the incremental effect of supervisory board seats may duplicate some channels by which works councils already affect firm outcomes.

An open question, naturally beyond the scope of our analysis, is whether our main findings rejecting the hold-up disinvestment prediction may be a result specific to the overall cooperative labor relations in the German context. A similarly intriguing possibility is whether the shared governance institution we study contributed to more cooperative labor relations in Germany (Thelen, 1991) or even served to "tame" labor: since the capital side retains the majority of seats, it could always outvote radical labor representatives voicing excessive demands. Therefore, labor representatives may have to be moderates in order to wield influence through coalition-building with shareholders, a dynamic plausibly consistent with the kind of effects we document in our natural experiment.

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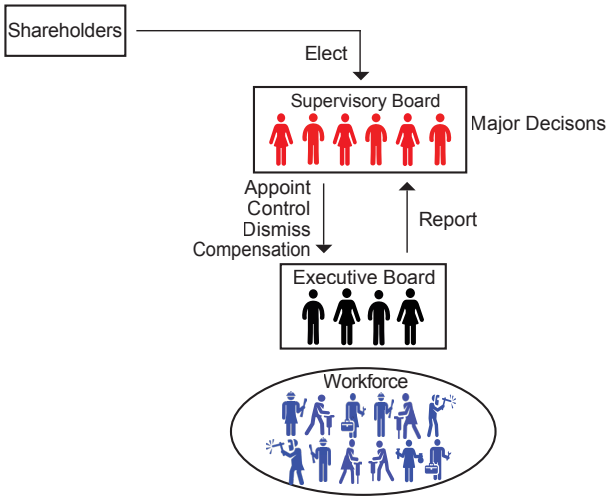
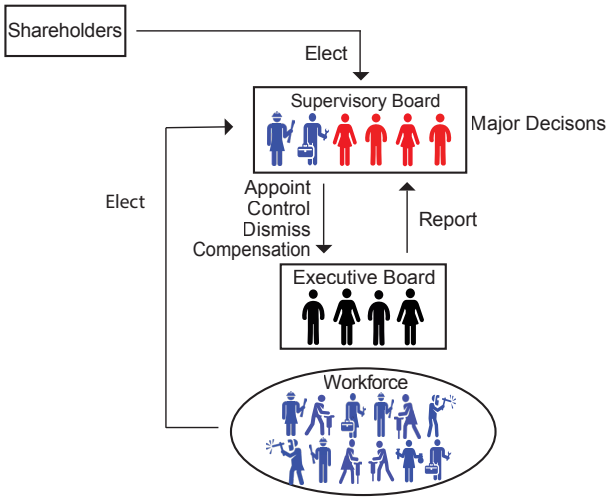
Figures

Figure 1: Corporate Governance and Worker Representation on Supervisory Board

(a) No Worker Representation on Supervisory Board



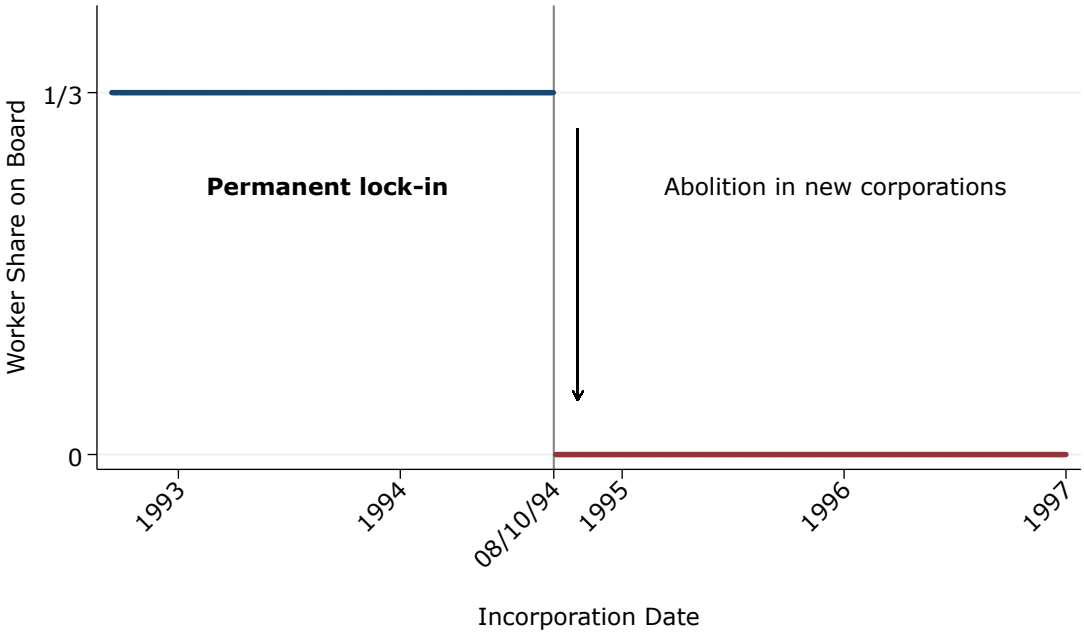
(b) One-Third Worker Representation on Supervisory Board



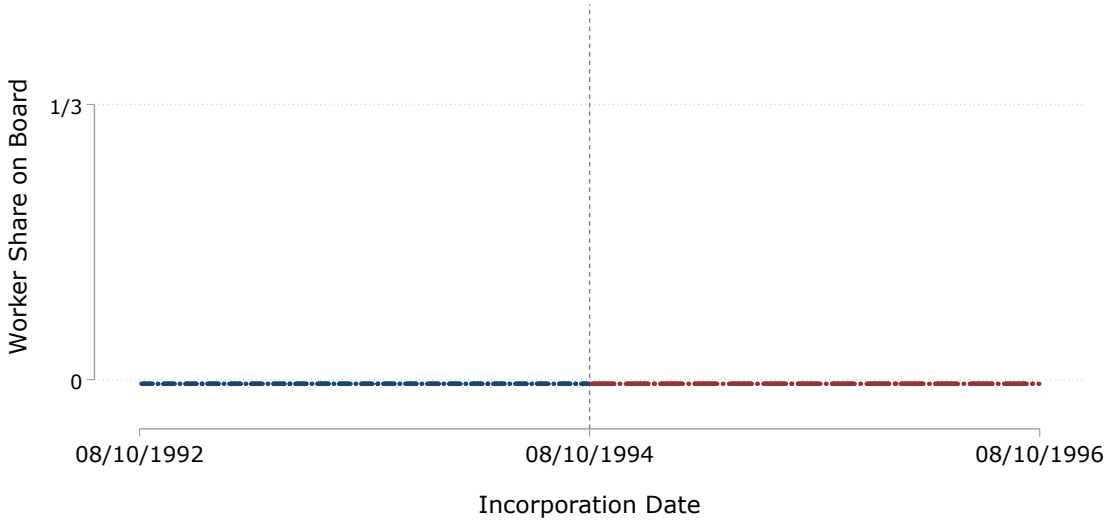
Note: The figure illustrates the supervisory board composition and its election in German corporations with and without worker-elected supervisory board directors. Stock corporations incorporated on or after August 10, 1994 as well as limited liability companies (LLCs) have no worker representatives on the supervisory board, as illustrated in Panel (a), unless they regularly employ more than 500 workers. Stock corporations incorporated before August 10, 1994 have one-third worker representatives on the supervisory board, as illustrated in Panel (b), even when they employ fewer than 500 workers. Regardless of incorporation date, firms between 500 and 2,000 employees are mandated to have one-third worker-elected directors and firms with more than 2,000 employees have equal representation of worker and shareholder representatives on the supervisory board. See Section 3 for additional information.

Figure 2: Shared Governance on Supervisory Board by Incorporation Date

(a) Stock Corporations



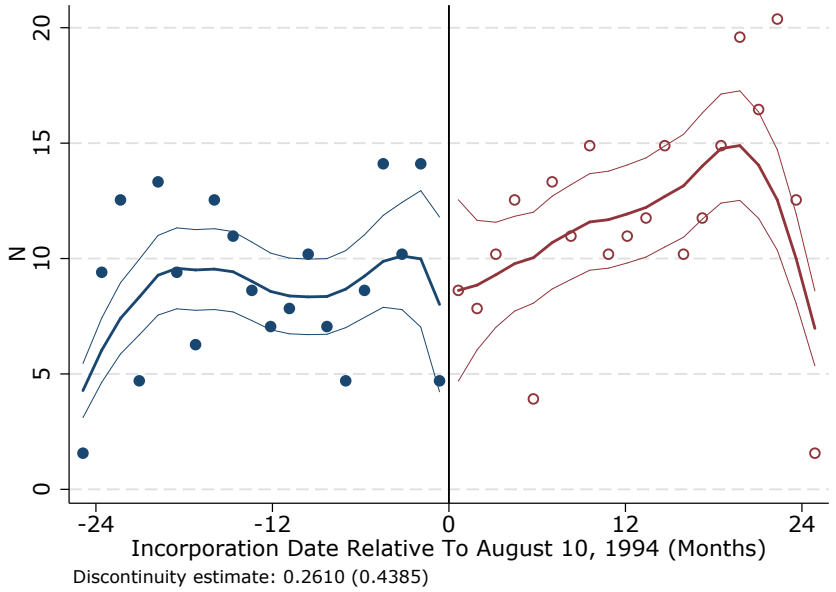
(b) LLCs



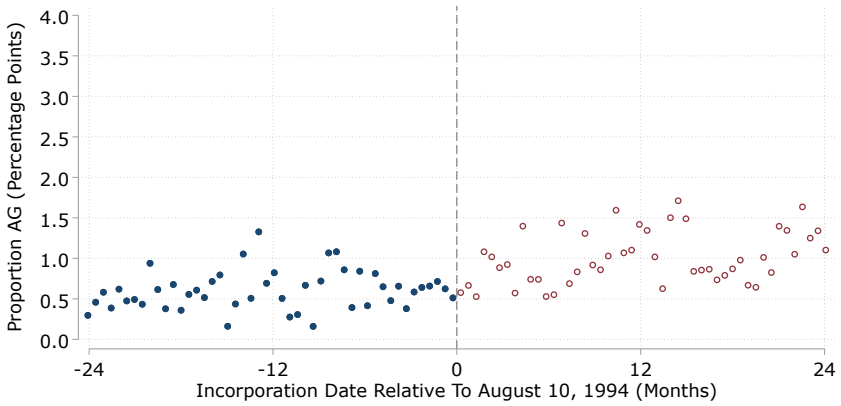
Note: The figure illustrates the mandates for shared governance by incorporation date in corporations with fewer than 501 employees (see Table 1 and Appendix Figure C.2 for rules for larger firms). Stock corporations incorporated before August 10, 1994 are mandated to have one-third worker representation on the supervisory board. Stock corporations incorporated on or after August 10, 1994 cannot have workers on the supervisory board if they have fewer than 501 employees. The rules for LLCs follow those for stock corporations incorporated on or after August 10, 1994, and were not changed as part of the reform. See Section 3 for additional information.

Figure 3: Frequency of Incorporation Around Reform Cutoff Date and Selection Into Stock Corporation Status

(a) Frequency of Incorporation and McCrary (2008) Test (Sample: Stock Corporations)

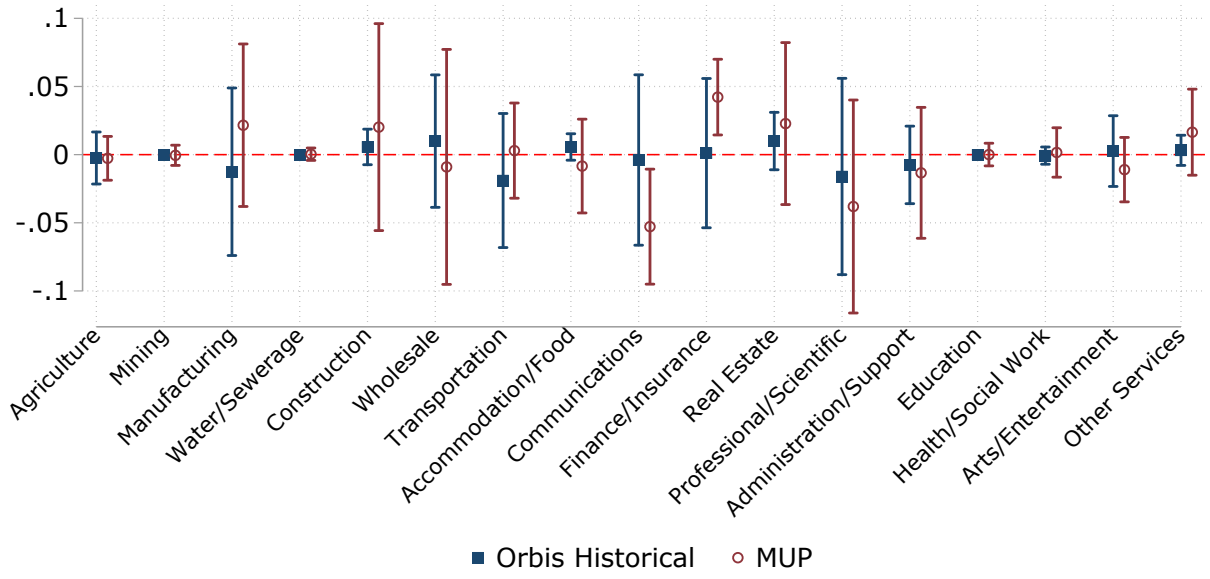


(b) Selection Into Stock Corporation Status



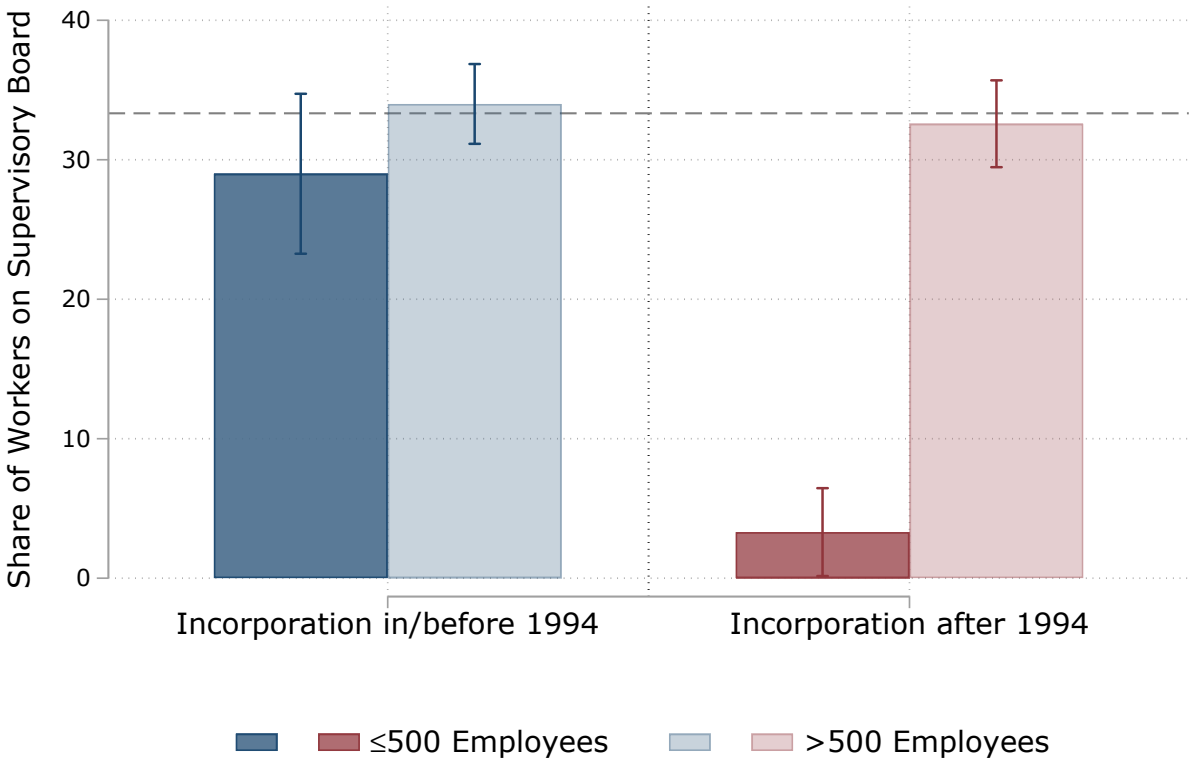
Note: Drawing on data from the Mannheim Enterprise Panel, Panel (a) plots the frequency of the incorporation of stock corporations around the August 10, 1994 cutoff date, which has been normalized to zero. The mandate for shared governance was relaxed for stock corporations incorporated after the cutoff date. The figure also 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 around the reform. Specifically, it plots the average of an indicator function for stock corporation status in a sample of all corporations by incorporation date based on BvD data. See also Appendix Table D.3, which reports global specifications testing whether the choice to incorporate as a stock corporation changed after the 1994 reform.

Figure 4: Balance of Industry Composition



Note: The figure plots difference-in-differences coefficients and associated 95% confidence intervals for specifications as in (11) using BvD and MUP data. Each coefficient is from a specification using an indicator for the respective industry as an outcome variable and can be interpreted as the effect of shared governance on the share of stock corporations incorporating in that industry (relative to LLCs). We use NACE Rev. 2 Classification 1 industry designations. 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.4 reports the differences-in-differences estimates in this figure.

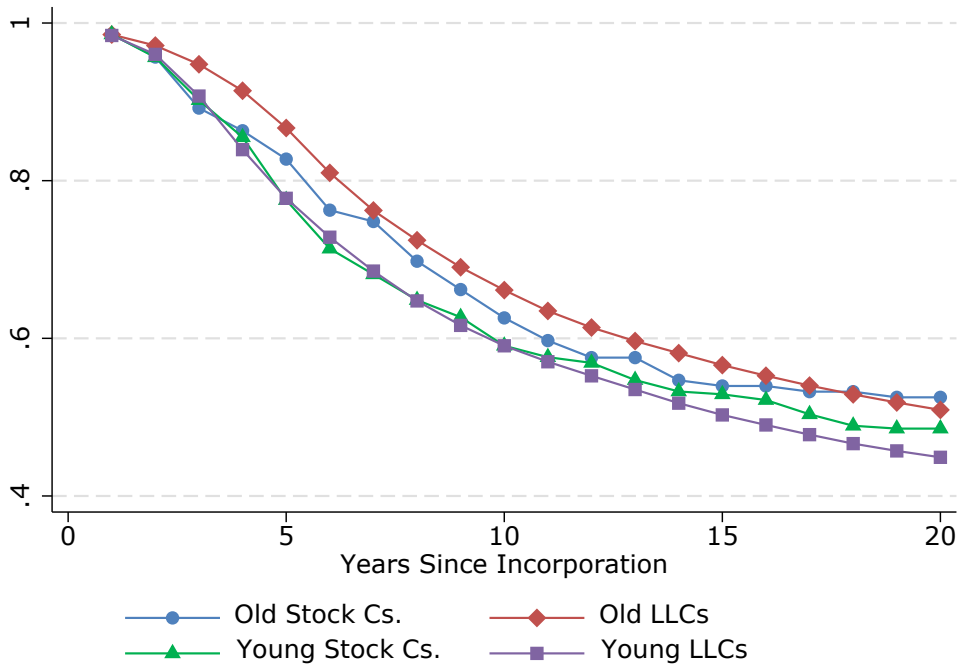
Figure 5: Share of Workers on the Supervisory Board



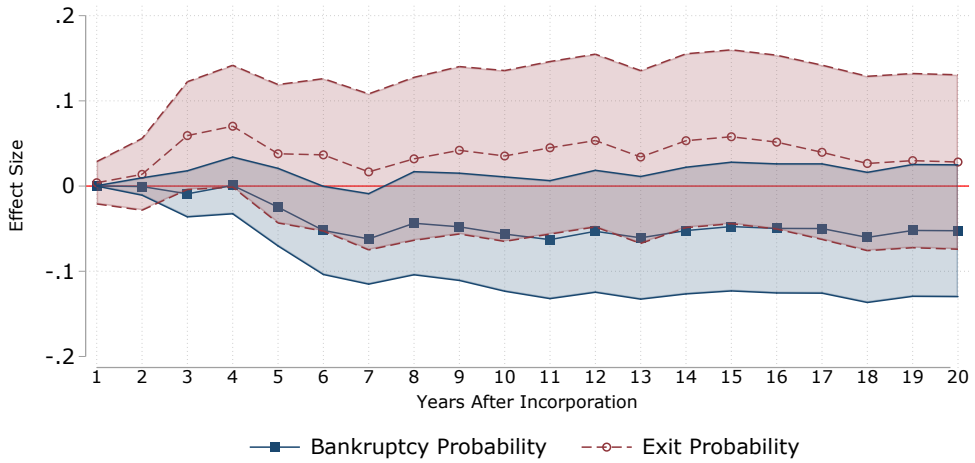
Note: The figure shows the share of workers on the supervisory board of stock corporations based on data from the Hoppenstedt Aktienführer. We restrict the sample to stock corporations founded between 1989 and 1999 for which board composition data and the incorporation year is reported. The two columns on the left report statistics for corporations with at most 500 employees, and the two columns on the right report those with more than 500 employees. The navy-colored bars represent corporations incorporated during or before 1994, the maroon-colored bars represent corporations incorporated during or after 1995. The dashed horizontal line at 33.33% indicates the mandated worker share under one-third codetermination.

Figure 6: 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

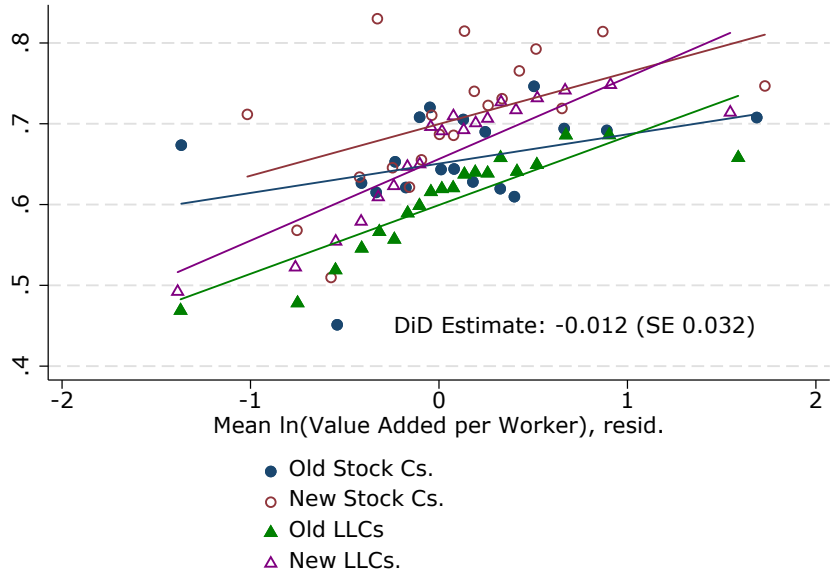


Note: The figure is based on the Mannheim Enterprise Panel. Panel (a) shows survival probabilities of firms incorporated within a two-year window of August 10, 1994 separately for firms incorporated before or after the cutoff date and for shareholder and LLCs. The running variable is time since incorporation 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.

Figure 7: Rent-Sharing: Firms' Pay Premia and Value-Added per Worker

(a) Rent-Sharing

(b) Rent-Sharing By Legal Form and Incorporation Date



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.

Tables

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

	Stock Corporations		LLCs	
Firm Size	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	
≥ 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.

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	0.158**	0.053	-0.039	-0.013*	0.029	-0.037
Industry FE	(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	0.044	-0.004	-0.029**	-0.001	0.083*	0.032*
Industry FE	(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 (11). 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.6 and C.7 for the specification with industry fixed effects at additional bandwidths and winsorization levels, and Appendix Tables D.5 and 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. Robust standard errors are reported in parentheses; we do not cluster here as we only have one observation per firm. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of Shared Governance on Firm Scale

	Log Revenue	Log Value Added	Log Emp (BvD)	Log Emp (IAB)	$\mathbb{1}$(Emp> 500) (BvD)	$\mathbb{1}$(Emp> 500) (IAB)	Log Fixed A.	Log Tang. A.	Log Intermediate Inputs
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Diff-in-Diff	0.537*	0.079	0.132	0.072	0.015	0.018	0.434**	0.193	-0.872*
	(0.300)	(0.223)	(0.133)	(0.140)	(0.032)	(0.033)	(0.219)	(0.244)	(0.514)
DiD	-0.090	0.037	0.108	0.040	0.013	0.018	0.427*	0.183	-1.158**
Year FE	(0.211)	(0.212)	(0.131)	(0.108)	(0.032)	(0.033)	(0.226)	(0.243)	(0.492)
DiD	0.290	0.113	0.096	0.052	0.013	0.017	0.466**	0.214	-0.708
Industry FE	(0.304)	(0.232)	(0.132)	(0.104)	(0.033)	(0.031)	(0.204)	(0.227)	(0.451)
DiD	-0.101	0.091	0.051	0.050	0.007	0.017	0.472**	0.229	-1.015**
Industry-Year FE	(0.199)	(0.198)	(0.127)	(0.104)	(0.033)	(0.031)	(0.212)	(0.219)	(0.429)
Control Mean: Stock Cs.	14.018	14.978	4.288	4.071	0.142	0.137	13.726	12.605	14.695
", LLCs	11.059	14.790	3.354	3.326	0.022	0.023	12.506	12.200	14.825
N, Firm-Years	207,418	40,066	278,878	289,348	278,878	289,348	114,844	113,291	22,834
N, Stock Cs.	529	246	616	298	616	298	360	360	163
N, LLCs	40,046	8,334	45,801	20,268	45,801	20,268	24,625	24,411	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 (11). 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.8 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.14 for robustness checks excluding former East German states, 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 clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 Year FE	35.112***	0.159	70.470***	0.434**	0.010	0.071**	0.159**
	(12.075)	(0.166)	(26.207)	(0.185)	(0.227)	(0.032)	(0.062)
DiD Industry FE	39.671***	0.218	71.535***	0.436**	-0.034	0.079***	0.167***
	(11.153)	(0.233)	(24.333)	(0.191)	(0.179)	(0.029)	(0.058)
DiD Industry-Year FE	40.064***	0.216	72.547***	0.487***	-0.030	0.081***	0.119**
	(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 (11). 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.9 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; Appendix Table D.15 for robustness checks excluding former East German states; 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 are clustered at the firm level are reported in parentheses. Stars denote statistical significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect of Shared Governance on Skill Structure (Matched Employer-Employee Data)

	Outsourceable (FSCL) % (1)	Low-Skilled % (2)	Med-Skilled % (3)	High-Skilled % (4)	Qualified Manual % (5)	Qualified Service % (6)	All Managers % (7)
Diff-in-Diff	0.014	-0.011	0.013	0.001	0.031	0.004	-0.014
	(0.009)	(0.010)	(0.026)	(0.028)	(0.026)	(0.007)	(0.011)
DiD Year FE	0.011	-0.011	0.009	0.001	0.030	0.001	-0.008
	(0.008)	(0.009)	(0.022)	(0.022)	(0.026)	(0.003)	(0.006)
DiD Industry FE	0.007	-0.013	0.011	0.004	0.035	0.001	-0.007
	(0.011)	(0.009)	(0.019)	(0.019)	(0.024)	(0.003)	(0.006)
DiD Industry-Year FE	0.007	-0.014	0.010	0.005	0.034	0.001	-0.007
	(0.011)	(0.009)	(0.019)	(0.019)	(0.024)	(0.003)	(0.006)
Control Mean: Stock Cs	0.019	0.125	0.582	0.276	0.100	0.043	0.093
", LLCs	0.133	0.115	0.738	0.121	0.268	0.019	0.030
N, Firm-Years	126,519	126,519	126,519	126,519	126,509	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 and Schmieder, 2017). We further consider (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 (11). 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.10 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of Shared Governance on Tenure (Matched Employer-Employee Data)

	Tenure	Log Tenure	Separations			
			All	<4 Yrs Tenure	4-9 Yrs Tenure	>9 Yrs Tenure
	(1)	(2)	(3)	(4)	(5)	(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 (11). 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.11 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: 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 (11). 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.13 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: 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 (11). 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.12 for the specification with industry-year fixed effects 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: 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 (11). 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.14 for specifications at additional bandwidths and winsorization levels, and Appendix Table D.10 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 ϕ and ι

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 $S^2(K, \bar{L}) = F(K, \bar{L}) - b\bar{L} - c'K$. [39]

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^*:

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

[39] 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

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

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

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

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

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), $\frac{S_K^{1F}}{S^{1F}} = -\frac{\iota}{1-\iota}\frac{S_K^{1W}}{S^{1W}}$. Therefore, C becomes:

$$\begin{aligned}
C &= \left[\frac{S_K^{1W}}{S^{1W}} - \frac{S_K^{1F}}{S^{1F}}\right] && \text{(A.20)}\\
&= \left[\frac{S_K^{1W}}{S^{1W}} + \frac{\iota}{1-\iota}\frac{S_K^{1W}}{S^{1W}}\right] && \text{(A.21)}\\
&= \frac{1}{1-\iota}\frac{S_K^{1W}}{S^{1W}} && \text{(A.22)}\\
&= \frac{1}{1-\iota}\frac{\phi S_K^2}{\phi S^2} && \text{(A.23)}\\
&> 0. && \text{(A.24)}
\end{aligned}$$

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:

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

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:

$$\frac{dL^*}{dK} = -\frac{F_{LK}}{F_{LL}}. \tag{A.28}$$

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.[40]

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

[40] 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.

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 40).

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.

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, $\text{sign}(\frac{d\frac{K}{L}}{d\psi}) = \text{sign}(\frac{dK^*}{d\psi})$, 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 $\frac{\partial\pi}{\partial K}\frac{\partial K}{\partial\phi} \geq 0$.[41]

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

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

So overall, we can consider three cases. For $\iota = 0$, $\frac{\partial \pi}{\partial K}|_{K=K^*} = 0$ (see Footnote 41), 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 to reverse it: $\left|-S^2\right| > \left|\frac{\partial \pi}{\partial K}\frac{\partial K}{\partial \phi}\right|$.[42]

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 $\frac{\partial \pi}{\partial K}|_{K=K^*} \leq 0$ (see Footnote 41) and $\frac{\partial K}{\partial \iota} > 0$ per Equation (A.25) (which extends to the context with endogenous labor). Finally, $\frac{\partial \pi}{\partial L} = (1-\phi)S^2_L = 0$ by FOC in Equation (A.27). So we have (for $\iota > 0$):[43]

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

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

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

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

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), $\frac{S^{1F}_K}{S^{1F}} = -\frac{\iota}{1-\iota}\frac{S^{1W}_K}{S^{1W}} = -\frac{\iota}{1-\iota}\frac{S^2_K}{S^2}$, that $S^{1W}_{KK} = \phi S^2_{KK}$ and that $S^{1F}_{KK} = (1-\phi)S^2_{KK}$. After some replacements and rearrangement, the condition becomes:

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

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

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

[43]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:

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% 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% 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% 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% 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% 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%. 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 ±.
   - We convert the following designations to 100%:
     - 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%.
   - 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% of all shares owned by one last name, then we designate

it as fully family-owned. If it has at least 50% 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% (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, and 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 Wirtschaftsforschung (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.[44] 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.

[44]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/.

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
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% 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% but no more than 1%.

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*):[45]

- 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)

[45]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.

- 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% 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% 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:

- 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.[46]

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 (Müller-Glöge et al., 2019, DrittelbG § 1 Rn. 8).

[46]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.

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

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

- Cost of Debt = $\frac{\text{Interest Paid}}{\text{Debt}}$
- Leverage = $\frac{\text{Debt}}{\text{Debt+Shareholders' funds}}$
- KZ Index
  $= -1.001909(\frac{\text{Profit after Tax (before Extraordinary Items) + Depreciation}}{\text{Lagged Tangible Fixed Assets}})$
  $+ 0.2826389(\frac{\text{Total Assets - Capital + Market Value of Equity}}{\text{Total Assets}})$
  $+ 3.139193(\frac{\text{Long Term Debt + Current Loans}}{\text{Long Term Debt + Current Loans + Capital/Shareholder Fund}})$
  $- 39.3678(\frac{\text{Dividends}}{\text{Lagged Tangible Fixed Assets}})$
  $- 3.139193(\frac{\text{Cash}}{\text{Lagged Tangible Fixed Assets}})$
  - We exclude dividends, which are not included in the BvD data.
- HP Index = -0.737(Log (Inflation Adjusted) Total Assets)
  $+ 0.043$(Log (Inflation Adjusted) Total Assets)$^2 - 0.040$(Yeas since Incorporation as AG)
- WW Index
  $= -0.091(\frac{\text{Profit after Tax (before Extraordinary Items) + Depreciation}}{\text{Total Assets}})$
  $- 0.062$(Dummy for Positive Dividend)
  $+ 0.021(\frac{\text{Long Term Debt}}{\text{Total Assets}})$
  $- 0.044$(Log Total Assets)
  $+ 0.103$(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
$$= -1.32$$
$$- 0.407(\text{Log (Inflation Adjusted) Total Assets})$$
$$+ 6.03(\frac{\text{Total Shareholder Funds and Liabilities - Shareholders Funds}}{\text{Total Assets}})$$
$$- 1.43(\frac{\text{Working Capital}}{\text{Total Assets}})$$
$$+ 0.0757(\frac{\text{Current Liabilities}}{\text{Current Assets}})$$
$$- 2.37(\frac{\text{Profit (Loss) for Period}}{\text{Total Assets}})$$
$$- 1.83(\frac{\text{Profit before Taxes + Depreciation}}{\text{Total Shareholder Funds and Liabilities - Shareholders Funds}})$$
$$+ 0.285(\text{Indicator for (Lagged Profit for Period + Two Period Ago Profit for Period)} < 0)$$
$$- 1.72(\text{Indicator for (Total Shareholder Funds and Liabilities - Shareholders Funds)} > \text{Total Assets})$$
$$- 0.521(\frac{\text{Profit for Period - Lagged Profit for Period}}{\text{Abs(Profit for Period) + Abs(Lagged Profit for Period)}})$$
- 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 $= \mathbb{1}\{\text{Other Shareholders Funds} \geq 0.5 * \text{Capital}\}$
- Retained Profit Share$_t$ $= \dfrac{\text{Other Shareholders Funds}_{t+1} - \text{Other Shareholders Funds}_t}{\text{Profit for Period}_t}$
- Retained Profit Share Excluding Profits$_t$
$$= \frac{\text{Other Shareholders Funds}_{t+1} - \text{P/L for Period}_{t+1} - \text{Other Shareholders Funds}_t + \text{P/L for Period}_t}{\text{P/L for Period}_t}$$
- Retained Earnings$_t$ $= \dfrac{\text{Other Shareholders Funds}_t}{\text{Total Assets}_t}$

- 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:

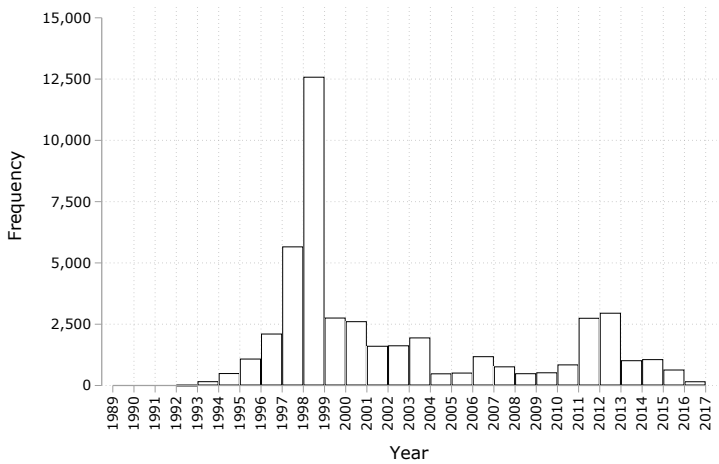
1. For each 2-digit NACE industry i and year t, we calculate the total wage bill and total value-added and divide the first by the second. Call this α_{it}.
2. Within i, we replace any $\alpha_{it} \geq 1$ with the highest α_{it} among all t that is less than 1.
3. We calculate the industry-specific 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:

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

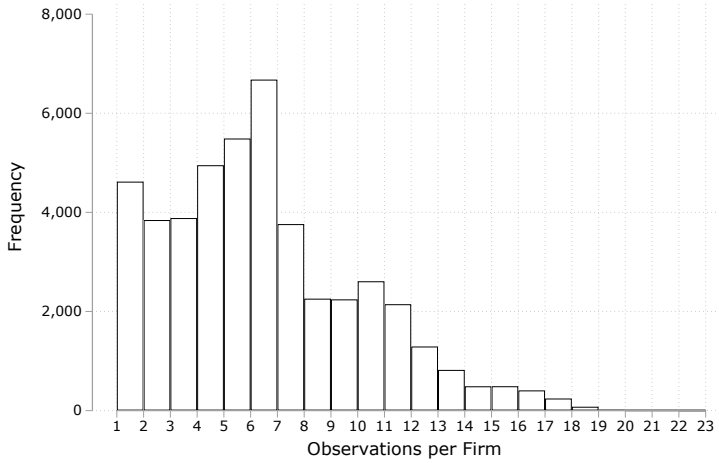
C Additional Figures

Figure C.1: Distribution of Observations in the Bureau van Dijk Data

(a) Distribution of Year of First Observation

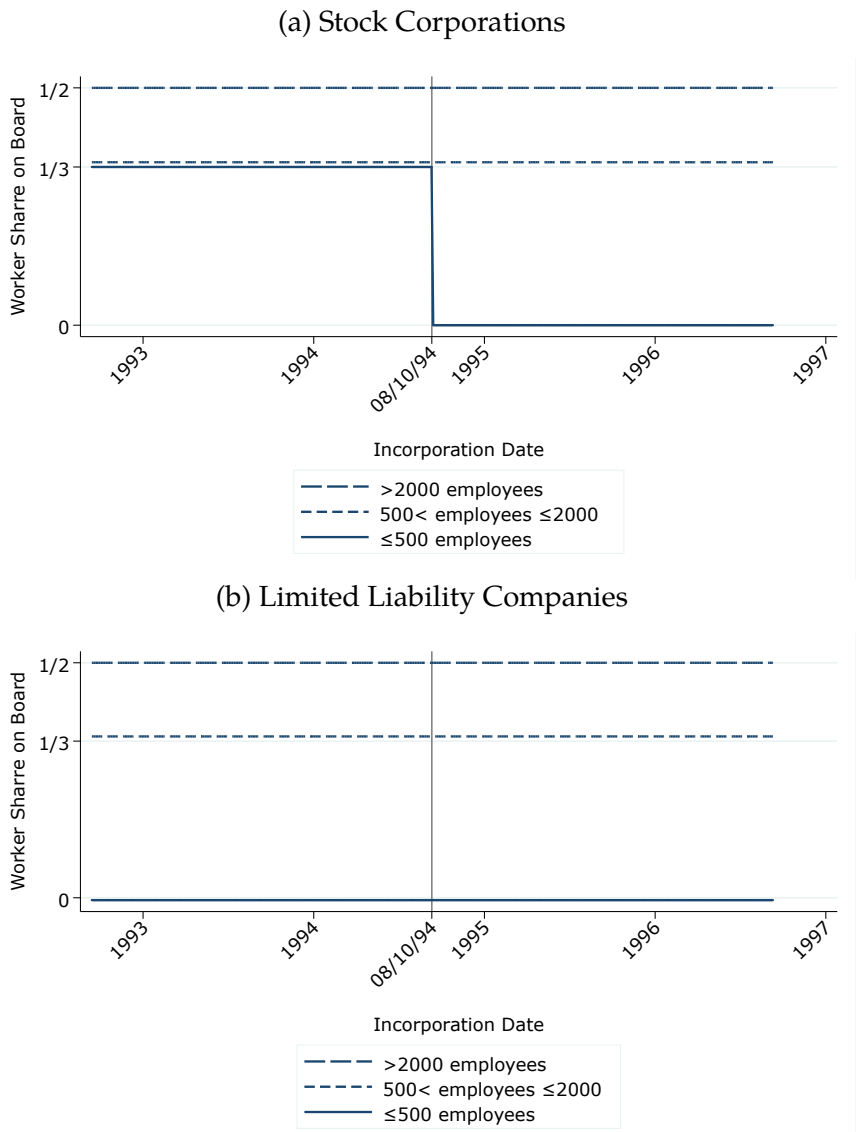


(b) Distribution of Number of Firm-Year Observations



Note: The figure reports the distribution of the first year and the number of years firms are observed in our BvD data set. The sample is restricted to stock corporations (*AGs*) and limited liability companies (*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. See Appendix Table D.1 for additional summary statistics.

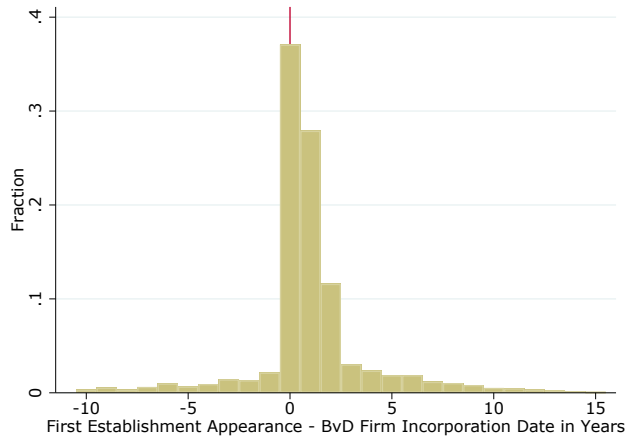
Figure C.2: Mandates for Shared Governance (Worker-Elected Directors) on Supervisory Board by Incorporation Date



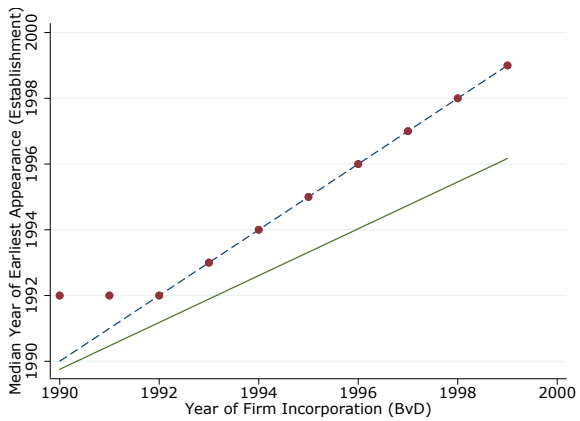
Note: The figure illustrates the share of worker-elected directors on the supervisory board by incorporation date as mandated by codetermination law (*MitbestG* and *DrittelbG*). Stock corporations incorporated before August 10, 1994 are mandated to have one-third worker representation on the supervisory board and parity if they have more than 2,000 employees. Family firms with fewer than 500 employees are exempt from shared governance unless they reach 500 employees. Stock corporations incorporated on or after August 10, 1994 cannot have workers on the supervisory board if they have fewer than 500 employees and are mandated to have one-third worker representation on the supervisory board between 500 and 2,000 employees and parity if they have more than 2,000 employees. The rules for LLCs broadly resemble those for stock corporations incorporated on or after August 10, 1994. See Section 3 for more information.

Figure C.3: Validation Exercises in Administrative IAB Data

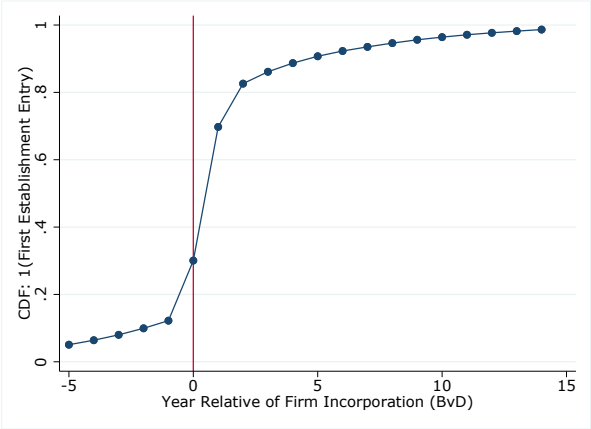
(a) Histogram of First Appearance Date of IAB Establishments vs. Firms' BvD Incorporation Dates



(b) First Appearance Date of IAB Establishments vs. Firms' BvD Incorporation Dates

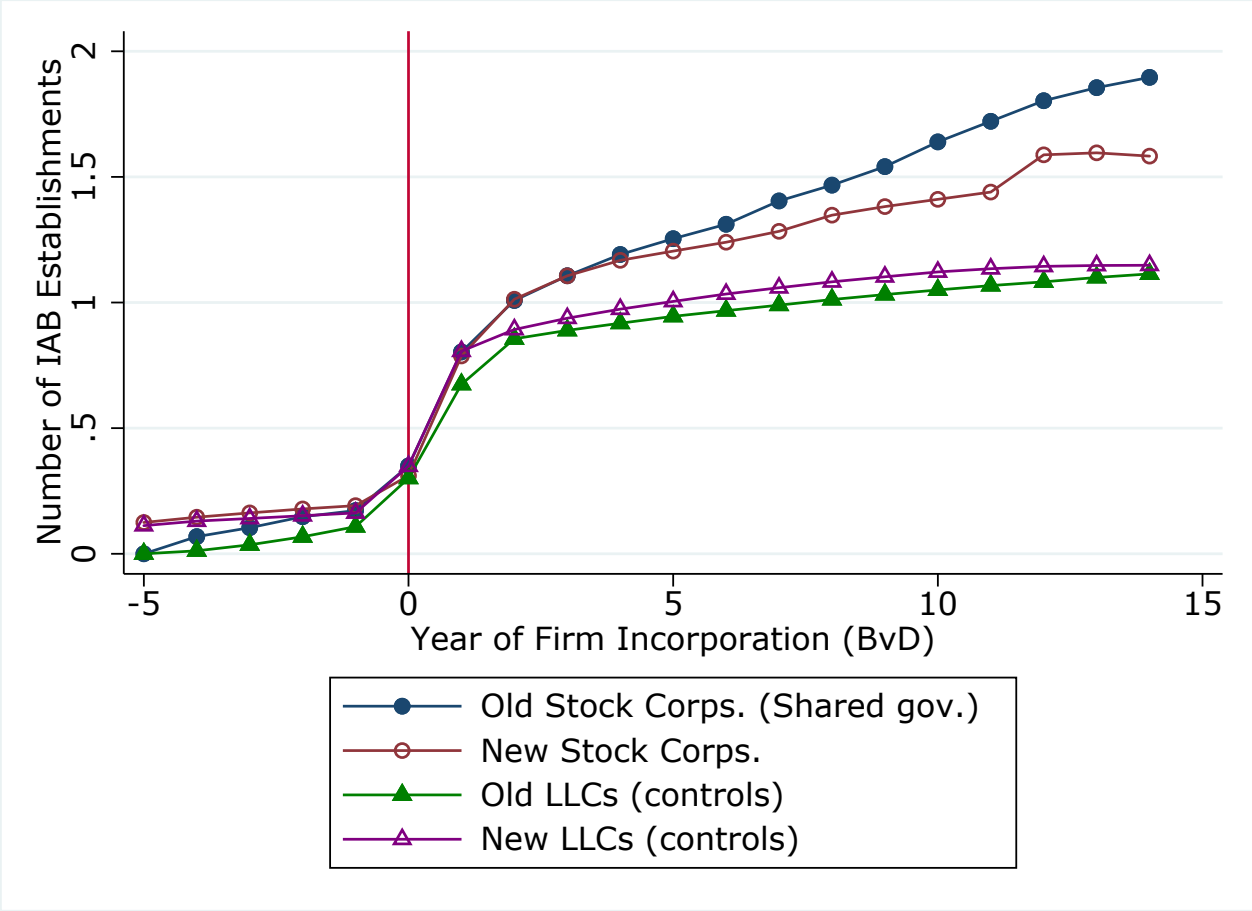


(c) IAB Establishment Entry Around BvD Incorporation Date



Note: The figure is based on the Orbis-ADIAB data and shows data for firms incorporated in the 1990s. Panel (a) plots a histogram of the date of the first appearance of an establishment in the administrative data relative to a firm's incorporation date as reported in BvD Orbis (based on the Commercial Register). For more than 50% of firms, the first occurrence of an establishment in the administrative IAB data is within a year of the BvD incorporation date. There is a small tail of establishments that appear in the administrative data before the legal incorporation. This could be due to the fact that establishments can keep the same establishment number even if the legal form or ownership of the firm changes or due to mismeasurement in the BvD incorporation date. There is a larger tail of first appearances *after* the legal incorporation. This could be due to the fact that the match between firm records (including the incorporation date) and establishments only occurs for the years between 2006 and 2014. As a consequence, we will miss establishments that had existed at some point before 2006 but closed by 2006, e.g., due to a firm's location change. In Panel (b), we provide a binned scatter plot of the first appearance date of establishments (median) in the administrative data against firms' BvD incorporation dates, along with the slope of the regression line. The figure also includes the diagonal in dashed blue. From 1990 to 1992, the average first appearance date in the administrative data is about a year or two after the legal incorporation. From 1993 onward, the mean first appearance date in the administrative data tracks the legal incorporation date very closely. In Panel (c), we plot the cumulative distribution function of establishment entry around the incorporation year. There is a sharp jump from about 0.1 to 0.7 from the year before legal incorporation to the year after.

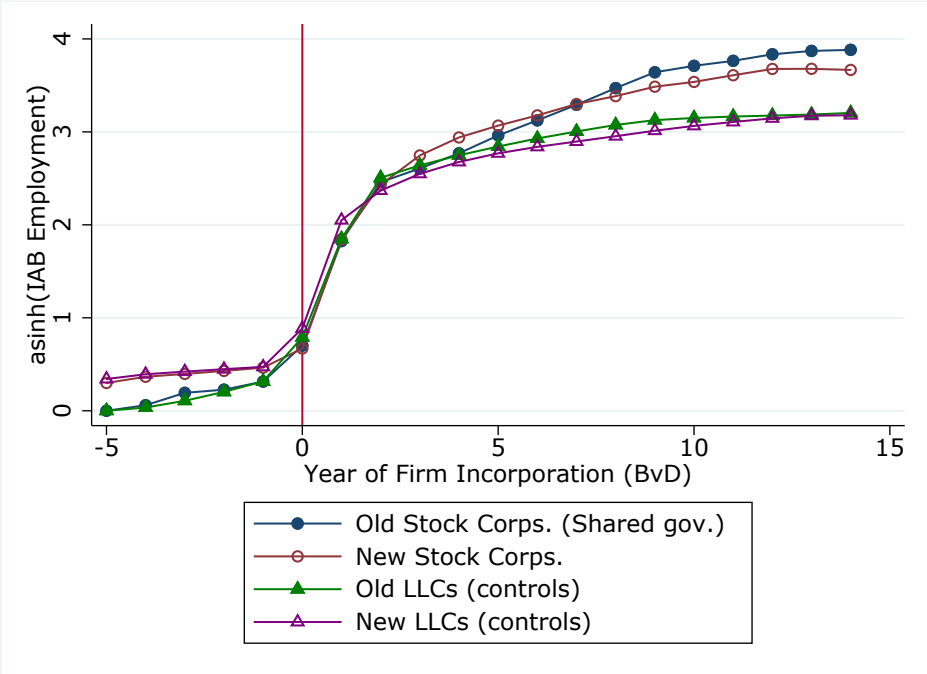
Figure C.4: BvD Firms' Number of IAB Establishments Relative to BvD Incorporation Date



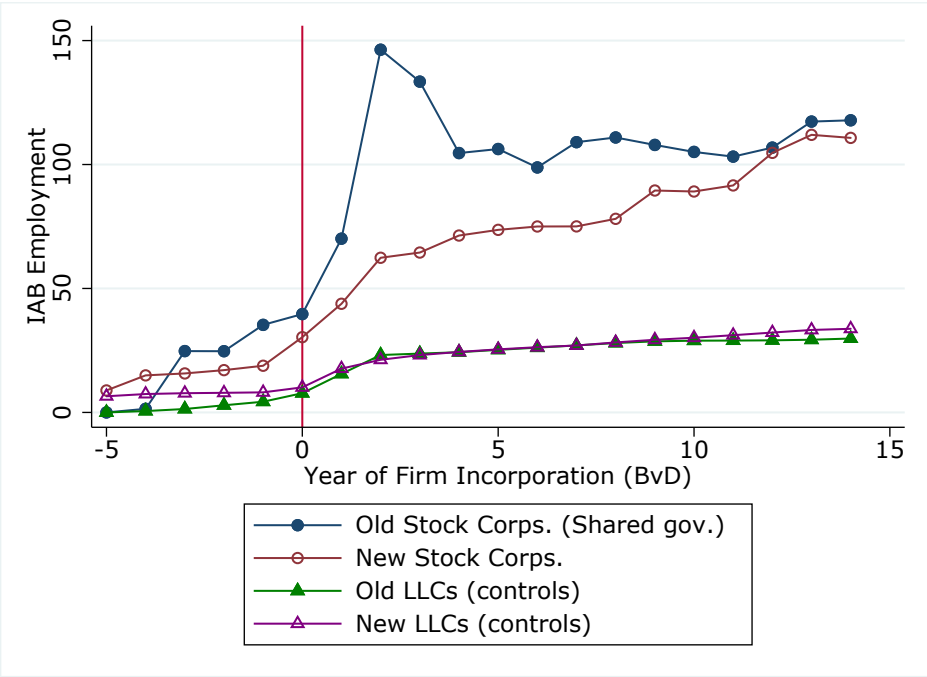
Note: The figure is based on the linked BvD Orbis-ADIAB data and shows the average number of establishments for firms incorporated in the 1990s. The navy line with solid circles plots the number of establishments for stock corporations incorporated before August 10, 1994 and the maroon line with hollow circles plots the number for stock corporations incorporated after the cutoff date. The green line with solid triangles and purple line with hollow triangles do so for old and new LLCs respectively.

Figure C.5: BvD Firms' Sum of IAB Employment Relative to BvD Incorporation Date

(a) arcinsh(IAB Employment)

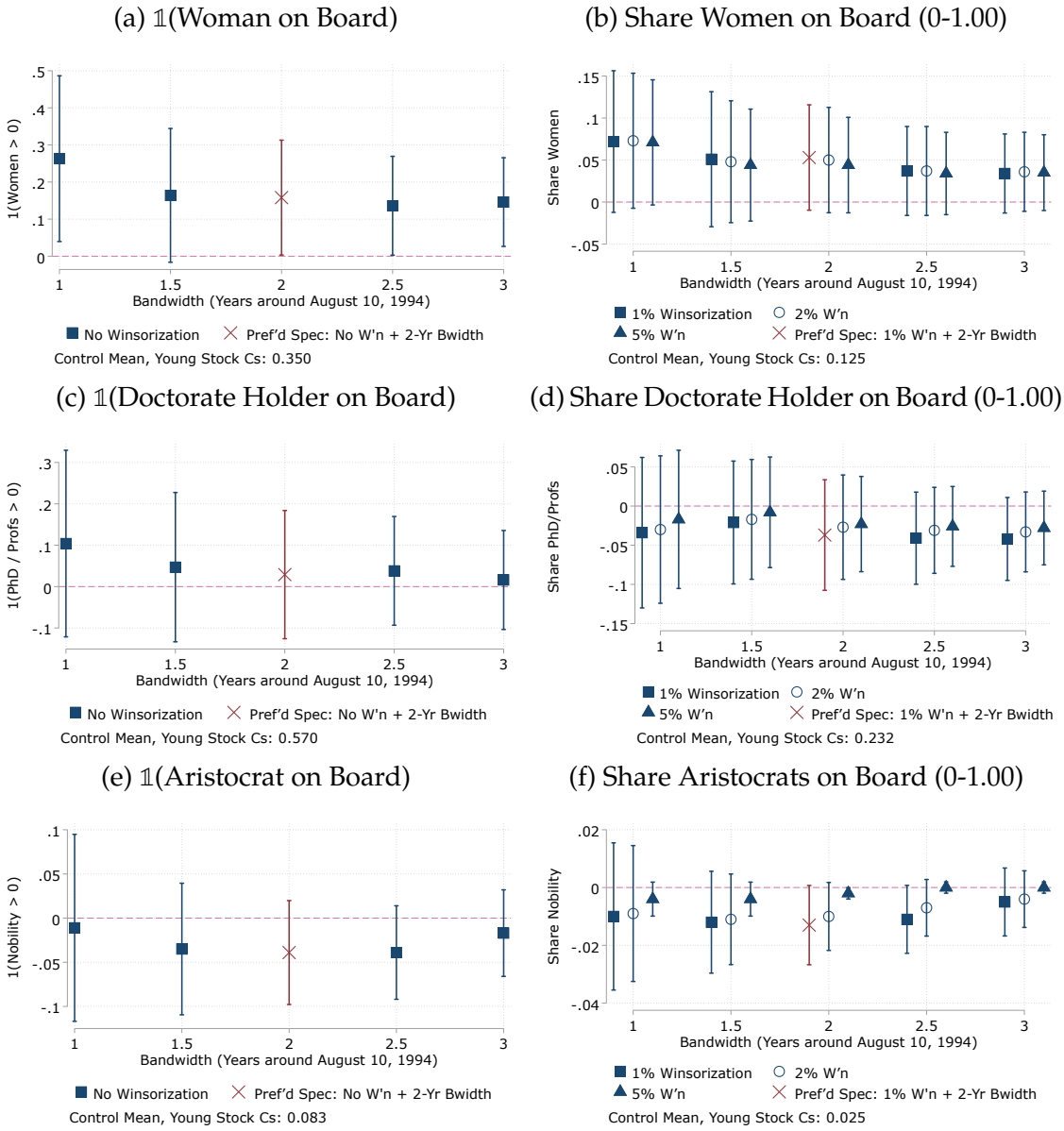


(b) Level of IAB Employment



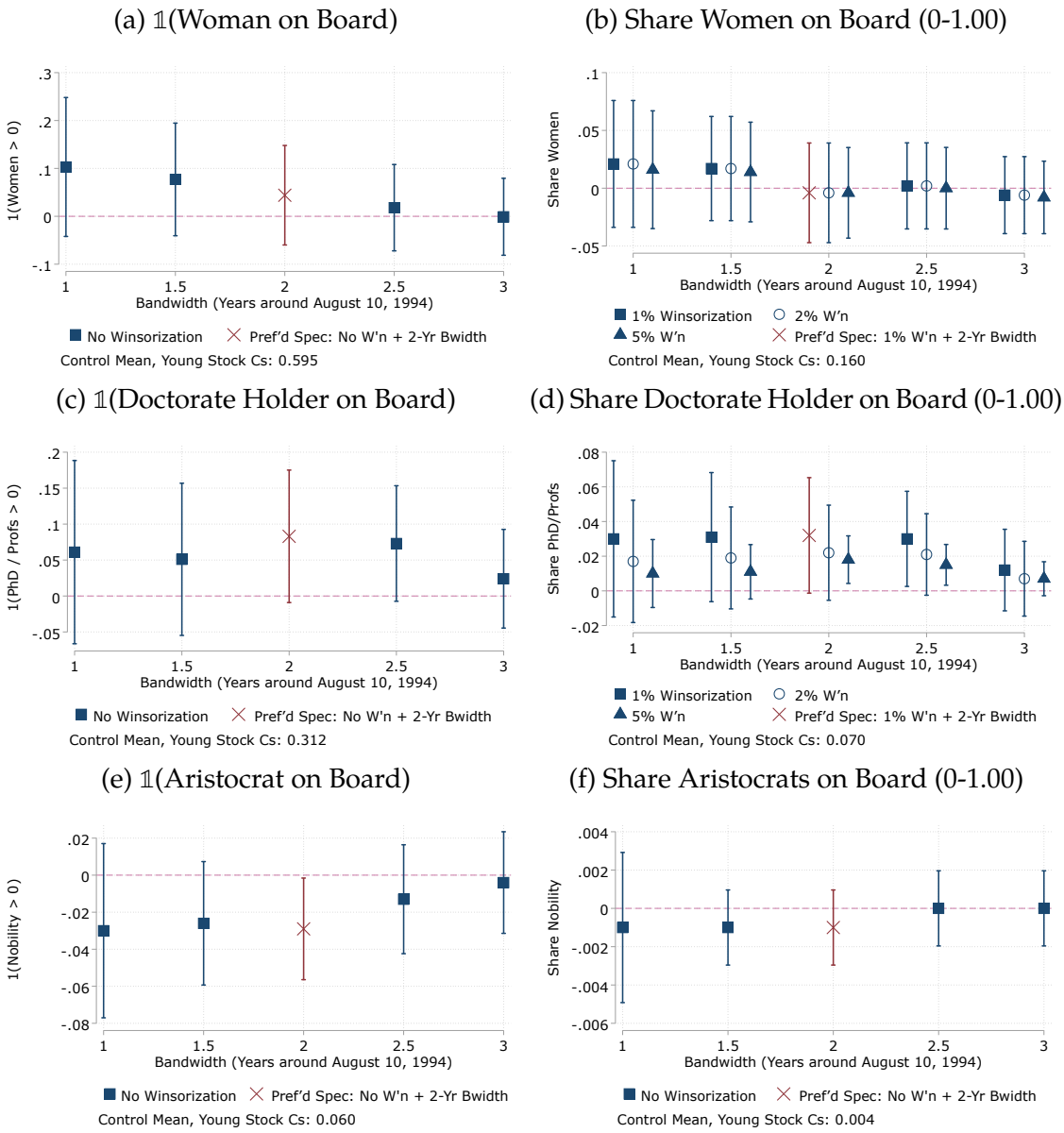
Note: The figure is based on the Orbis-ADIAB data and shows employment data for firms incorporated in the 1990s. Panels (a) and (b) plot employment as arcinsh(employment) and in levels, respectively. The navy line plots employment for stock corporations incorporated before August 10, 1994 and the maroon line employment for stock corporations incorporated after the cutoff date. The green line with solid triangles and purple line with hollow triangles do so for old and new LLCs respectively.

Figure C.6: Effect of Shared Governance on Supervisory Board Composition



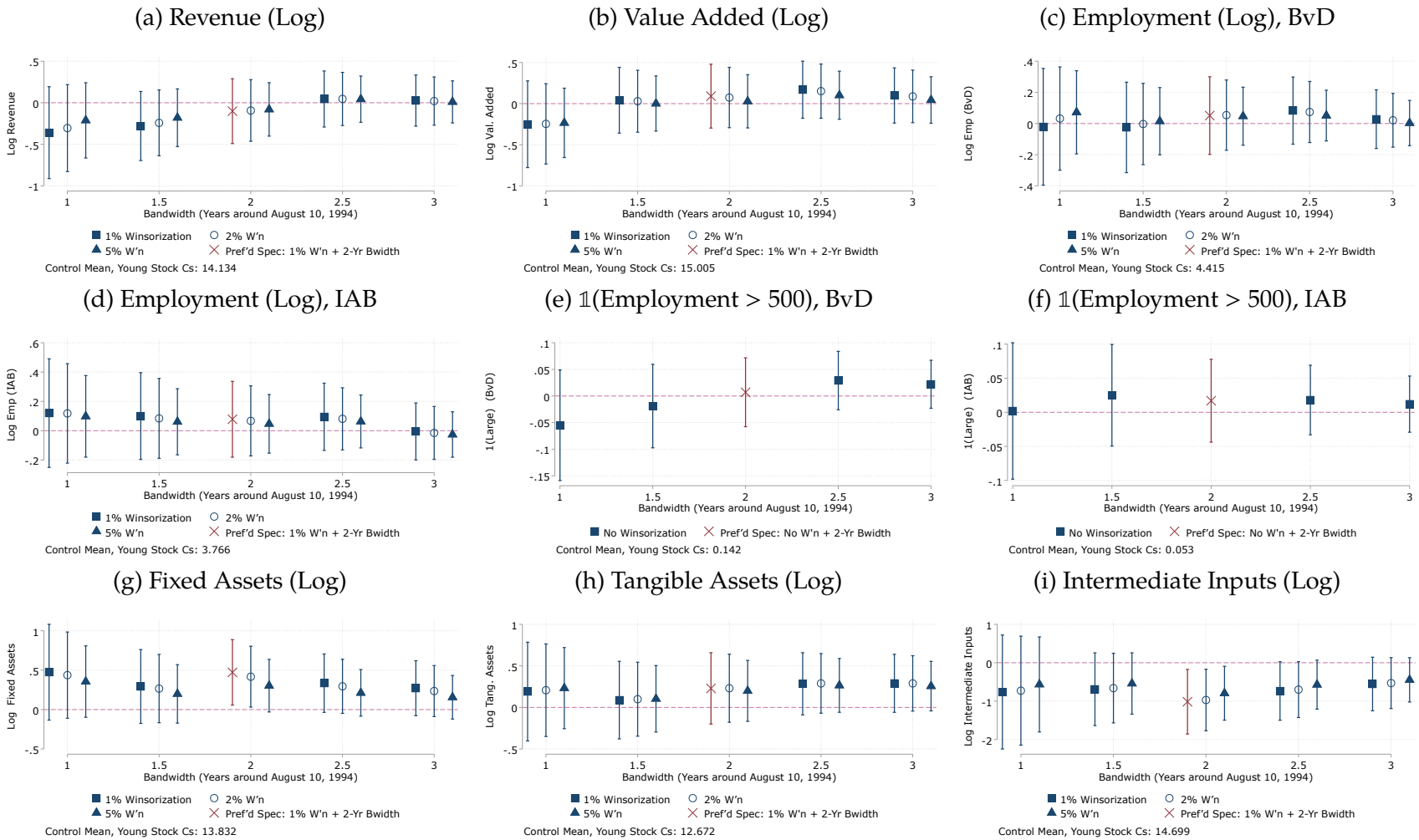
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on supervisory board composition at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry fixed effects. 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.

Figure C.7: Effect of Shared Governance on Executive Board Composition



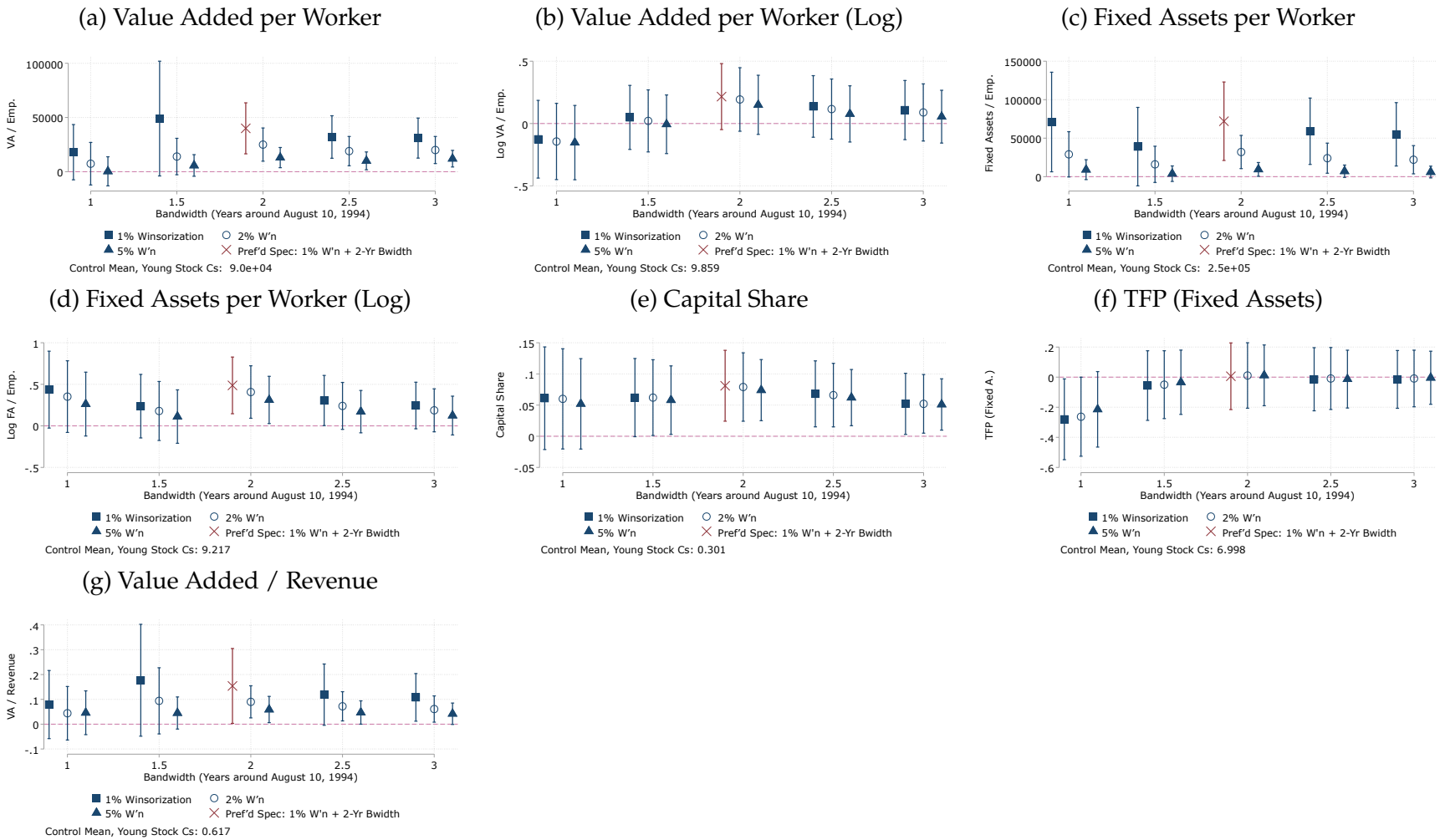
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on executive board composition at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry fixed effects. The 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.

Figure C.8: Effect of Shared Governance on Firm Scale



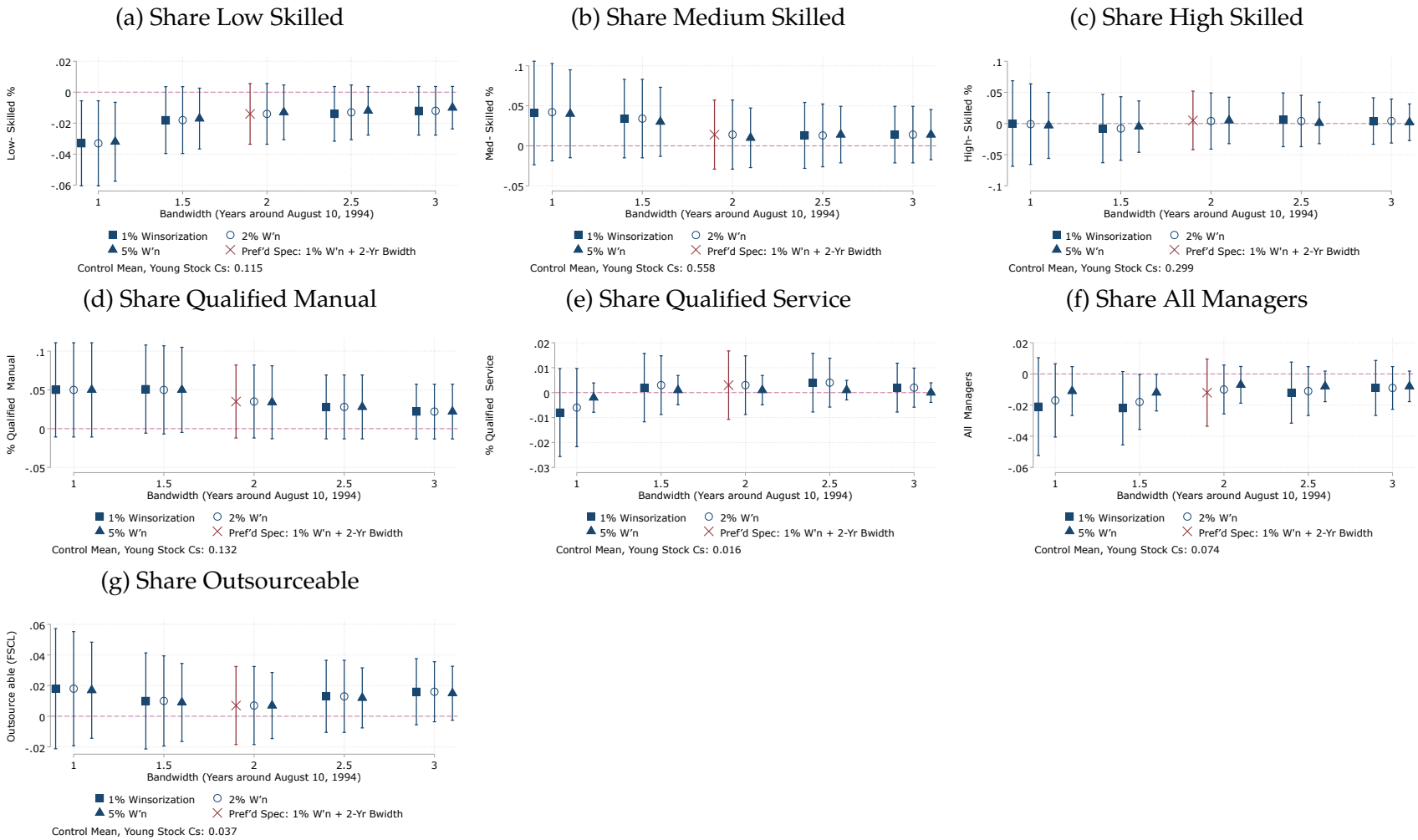
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on firm scale at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 1% 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.

Figure C.9: Effect of Shared Governance on Productivity and Capital Intensity



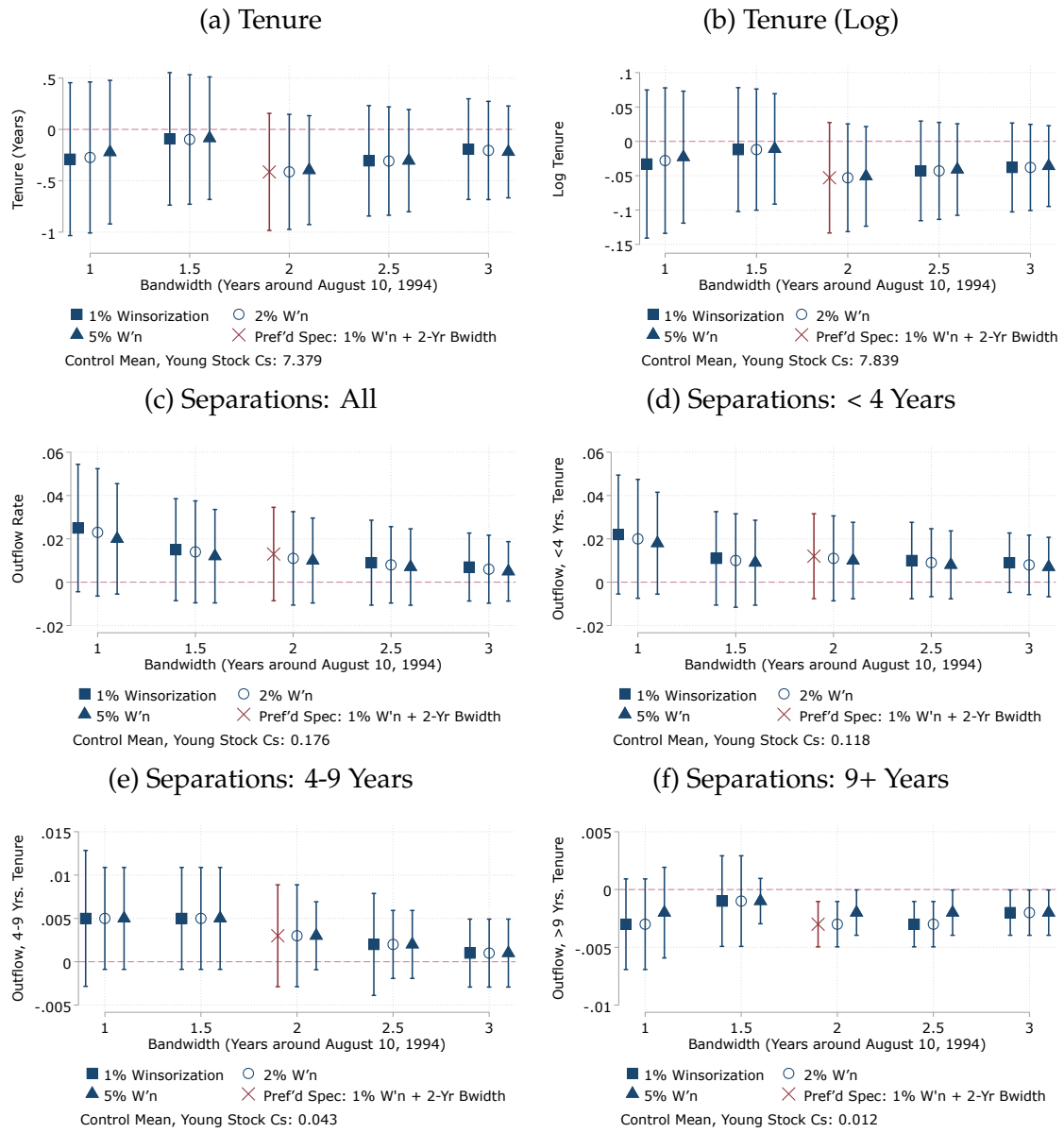
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on productivity at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry-by-year fixed effects. The square 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.

Figure C.10: Effect of Shared Governance on Skill Structure (Matched Employer-Employee Data)



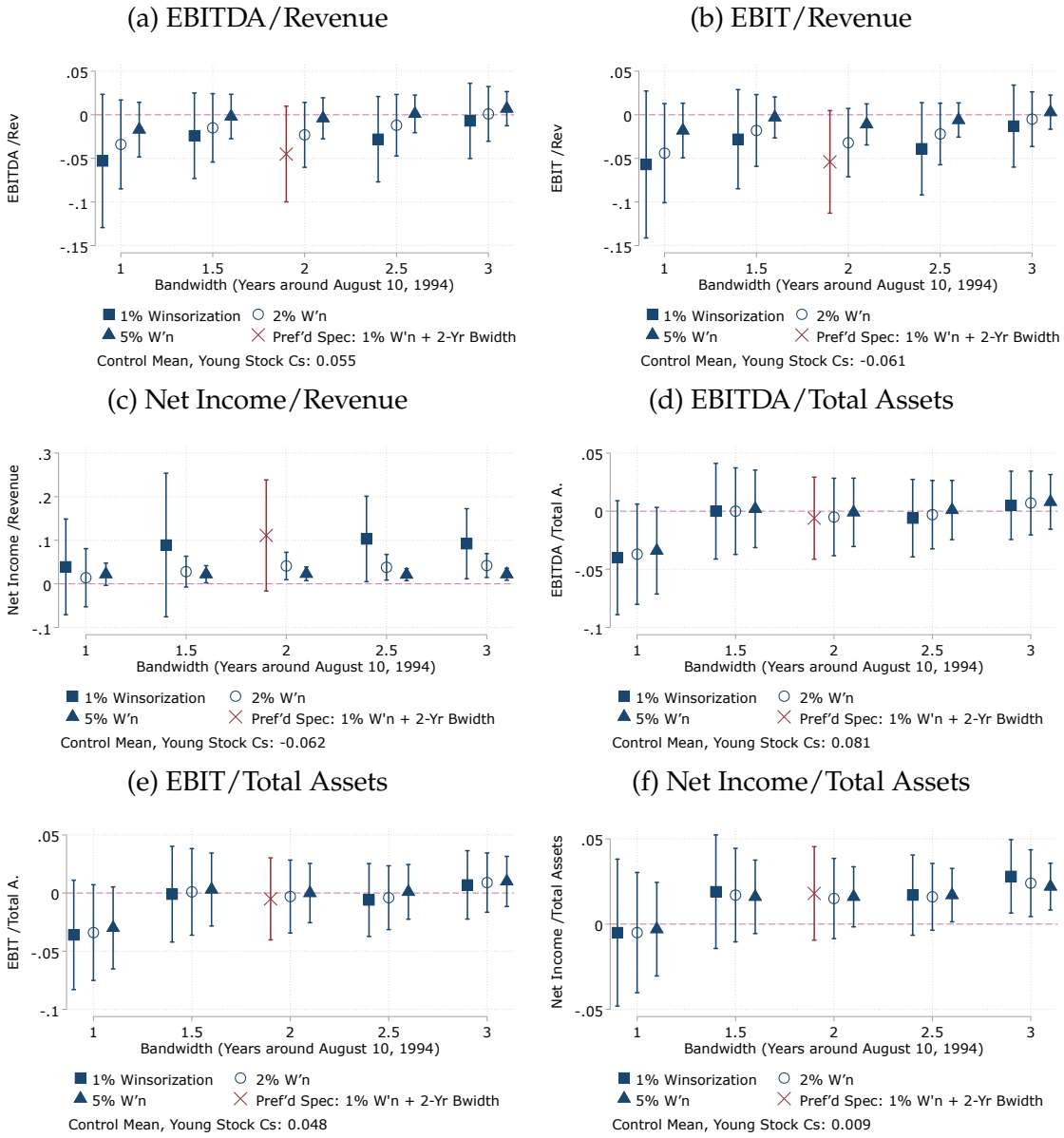
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on skill structure at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. All specifications include industry-by-year fixed effects. The square 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.

Figure C.11: Effect of Shared Governance on Tenure (Matched Employer-Employee Data)



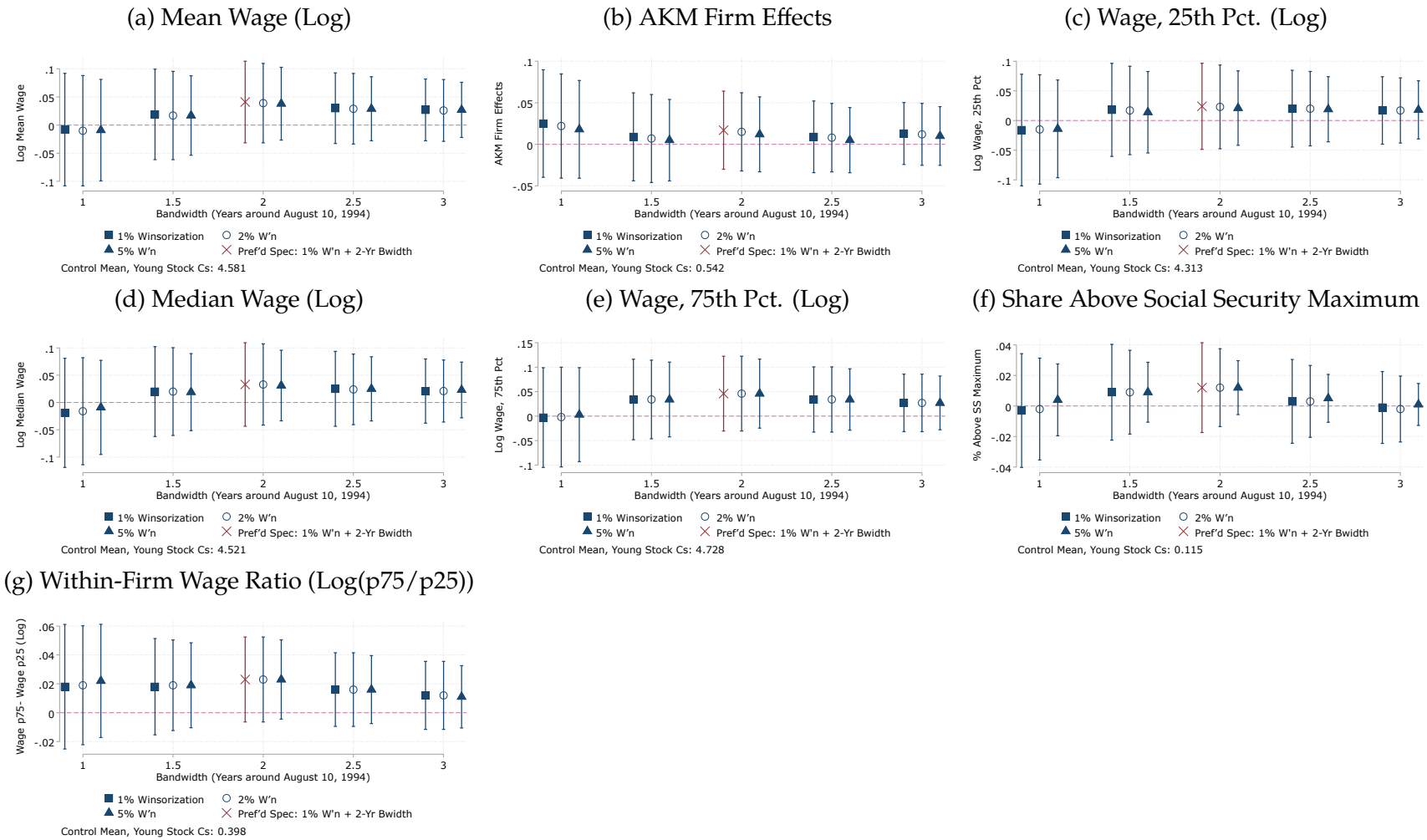
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on tenure at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 1% winsorization 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.

Figure C.12: Effect of Shared Governance on Profitability



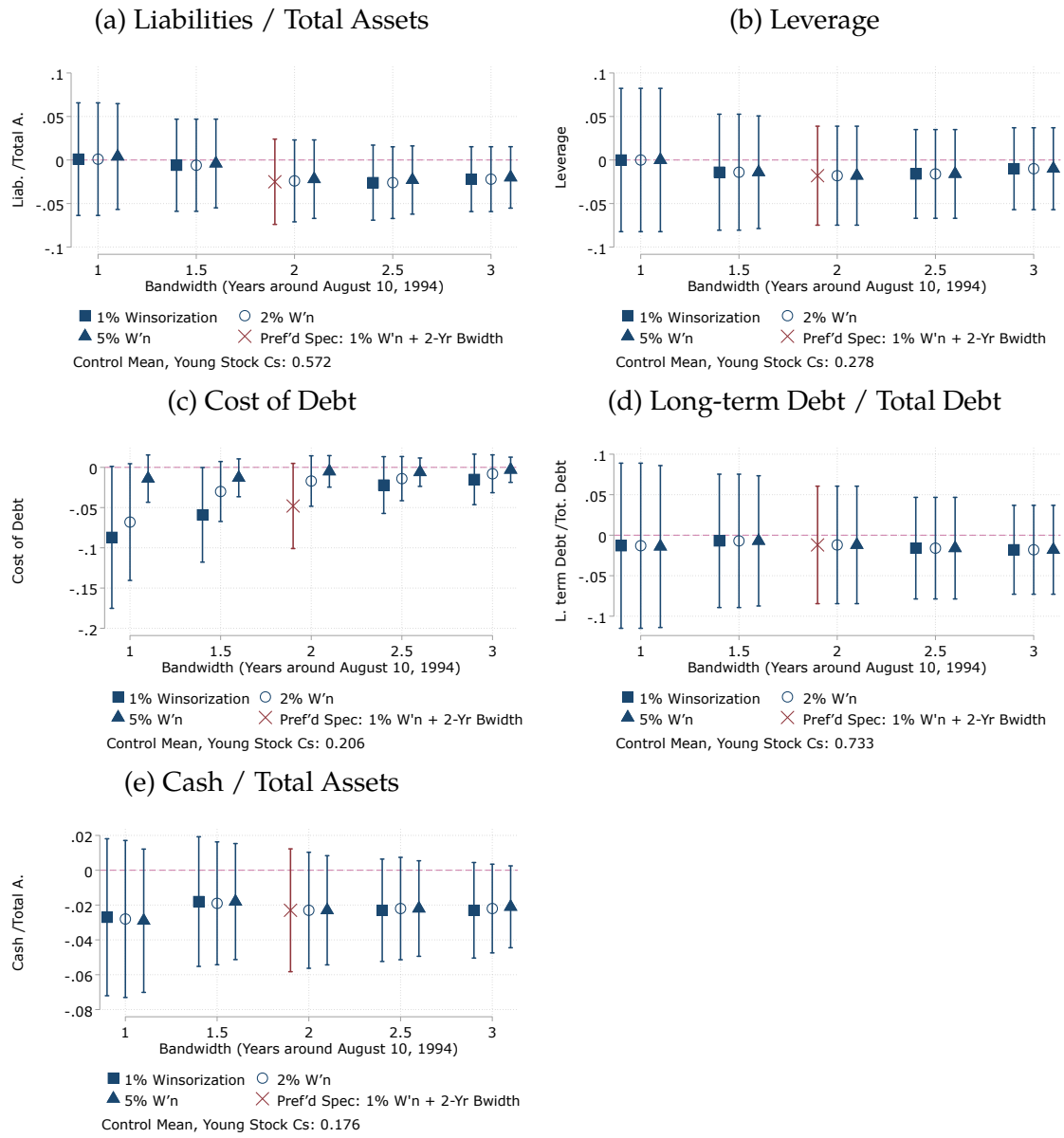
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on profitability at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our preferred 2-year bandwidth and 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.

Figure C.13: Effect of Shared Governance on Wages (Matched Employer-Employee Data)



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.

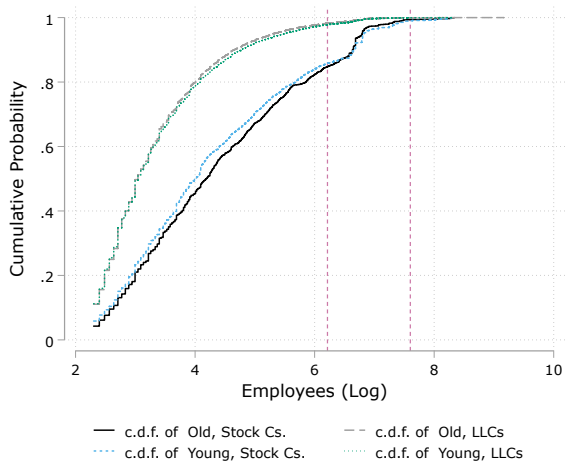
Figure C.14: Effect of Shared Governance on Capital Structure, Leverage, and Cost of Debt



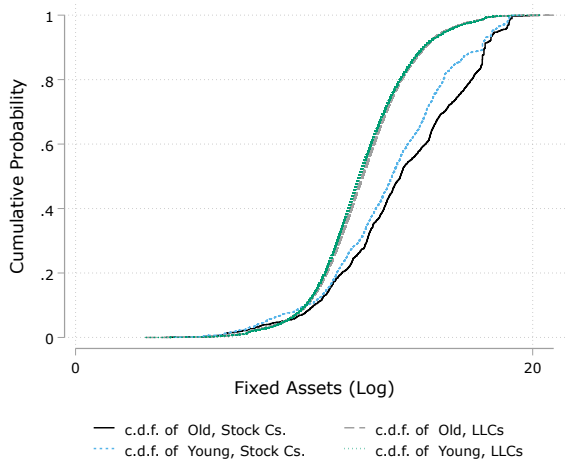
Note: The figure plots difference-in-differences estimates of the reduced-form effect of shared governance on capital structure, leverage, and cost of debt at different bandwidths of incorporation dates relative to August 10, 1994 and different winsorization levels. The square maroon marker denotes our 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.

Figure C.15: Cumulative Distribution Functions of Selected Outcomes

(a) Employment (Log)



(b) Fixed Assets (Log)



(c) Value Added per Worker (Log)

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. See Appendix Figure C.1 for the distribution of firm-years in the sample.

Table D.2: 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 (11). 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 and 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.

Table D.3: Differential Trends for Incorporation of Stock Corporations

	(1) $\mathbb{1}$(Incorporated as AG)	(2) $\mathbb{1}$(Incorporated as AG)
Incorporation Date	0.0023**	0.0019*
	(0.0011)	(0.0011)
$\mathbb{1}$(Post-Reform)	0.0001	0.00001
	(0.0021)	(0.0021)
Inc. Date $\times$ $\mathbb{1}$(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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.4: 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 (11). Column (1) reports the basic specification from equation (11), and column (2) includes quarter-of-incorporation fixed effects. We visually report the estimates from column (1) in Figure 4. See Appendix Section B.2 for details on the sample construction.

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

	𝟙(Women > 0) (1)	Share Women (2)	𝟙(PhD/Profs > 0) (3)	Share PhD/Profs (4)	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 (11) 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 (11). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6: **Placebo Reforms in 1998 and 2002**: Effect on Executive Board Demographic Composition

	𝟙(Women > 0) (1)	Share Women (2)	𝟙(PhD/Profs > 0) (3)	Share PhD/Profs (4)	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 (11) 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 (11). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: **Placebo Reforms in 1998 and 2002**: Effect on Firm Scale

	Log Revenue (1)	Log Value Added (2)	Log Emp (BvD) (3)	$\mathbb{1}$(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 (11) 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 (11). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8: **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 (11) 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 (11). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.9: **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 8. Panels A and B replicate our DiD specification in (11) 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 (11). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10: **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 9. Panels A and B replicate our DiD specification in (11) 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 (11). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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	1.678	-0.012	0.004	0.019	0.024	-0.006
Year FE	(2.581)	(0.017)	(0.030)	(0.041)	(0.038)	(0.025)
DiD	1.006	-0.023	-0.012	0.005	0.021	-0.010
Industry FE	(2.550)	(0.018)	(0.030)	(0.040)	(0.038)	(0.025)
DiD	0.428	-0.016	-0.011	-0.0002	0.017	-0.014
Industry-Year FE	(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	4.377	0.002	0.042	0.019	0.076*	0.034
Year FE	(2.707)	(0.019)	(0.032)	(0.042)	(0.041)	(0.032)
DiD	4.758*	-0.0008	0.042	0.019	0.074**	0.040
Industry FE	(2.477)	(0.019)	(0.032)	(0.040)	(0.037)	(0.029)
DiD	4.759*	0.007	0.051	0.031	0.082**	0.039
Industry-Year FE	(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 (11), 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

	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: 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.

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

	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: 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.

Table D.14: Effect of Shared Governance on Firm Scale **Excluding Former East Germany**

	Log Revenue (1)	Log Value Added (2)	Log Emp (BvD) (3)	$\mathbb{1}$(Emp> 500) (BvD) (4)	Log Fixed A. (5)	Log Tang. A. (6)	Log Intermediate (7)
Diff-in-Diff	0.846***	0.040	0.243*	0.023	0.441*	0.193	-0.725
	(0.314)	(0.236)	(0.138)	(0.034)	(0.231)	(0.260)	(0.555)
DiD	0.110	0.019	0.214	0.021	0.433*	0.184	-0.992*
Year FE	(0.226)	(0.228)	(0.137)	(0.034)	(0.239)	(0.259)	(0.531)
DiD	0.466	0.136	0.206	0.022	0.515**	0.252	-0.563
Industry FE	(0.325)	(0.253)	(0.138)	(0.035)	(0.214)	(0.236)	(0.490)
DiD	0.067	0.131	0.159	0.016	0.533**	0.295	-0.866*
Industry-Year FE	(0.214)	(0.216)	(0.134)	(0.035)	(0.225)	(0.231)	(0.452)
Control Mean: Stock Cs	13.963	15.059	4.258	0.142	13.720	12.613	14.709
", LLCs	10.989	14.770	3.350	0.022	12.465	12.143	14.785
N, Firm-Years	185,554	35,135	254,730	254,730	101,819	100,415	18,882
N, Stock Cs	495	222	580	580	329	329	143
N, LLCs	36,863	7,515	42,591	42,591	22,032	21,835	5,315

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 (11). 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 outside of former East Germany. We *exclude* firms whose address is in any of the following NUTS-1 regions (the *Bundesländer* of former East Germany and Berlin, i.e. both East and West Berlin): Mecklenburg-Vorpommern, Brandenburg, Berlin, Sachsen-Anhalt, Thüringen, or Sachsen. 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 and Appendix Figure C.8 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.15: Effect of Shared Governance on Productivity and Capital Intensity **Excluding Former East Germany**

	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	51.043**	0.110	52.896**	0.330	0.086	0.067**	0.241**
	(22.909)	(0.254)	(25.001)	(0.202)	(0.293)	(0.034)	(0.121)
DiD	42.913**	0.141	53.367**	0.391**	0.118	0.066**	0.229**
Year FE	(16.657)	(0.182)	(25.237)	(0.196)	(0.235)	(0.033)	(0.116)
DiD	45.350***	0.205	57.308**	0.399**	-0.030	0.076**	0.254**
Industry FE	(14.014)	(0.248)	(23.816)	(0.198)	(0.190)	(0.031)	(0.117)
DiD	42.251***	0.218	57.429**	0.465**	-0.012	0.075**	0.158*
Industry-Year FE	(13.766)	(0.146)	(25.320)	(0.184)	(0.120)	(0.032)	(0.090)
Control Mean: Stock Cs	84.536	9.926	96.063	9.186	6.871	0.294	0.444
", LLCs	68.749	10.485	32.182	8.964	7.650	0.255	0.360
N, Firm-Years	35,135	35,135	102,911	101,819	33,282	34,203	23,149
N, Stock Cs	222	222	329	329	217	225	204
N, LLCs	7,515	7,515	22,244	22,032	7,001	7,406	6,270

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 (11). 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 outside of former East Germany. We *exclude* firms whose address is in any of the following NUTS-1 regions (the *Bundesländer* of former East Germany and Berlin, i.e. both East and West Berlin): Mecklenburg-Vorpommern, Brandenburg, Berlin, Sachsen-Anhalt, Thüringen, or Sachsen. 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 and Appendix Figure C.9 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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	0.004	-0.022	0.081	0.057	-0.036	0.101
Year FE	(0.041)	(0.061)	(0.061)	(0.053)	(0.057)	(0.078)
DiD	0.011	-0.042	0.093*	0.038	-0.058	0.012
Industry FE	(0.039)	(0.057)	(0.055)	(0.054)	(0.056)	(0.055)
DiD	0.020	-0.026	0.096*	0.041	-0.056	0.008
Industry-Year FE	(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	0.090**	-0.025	0.088*	0.029	0.026	0.075
Year FE	(0.040)	(0.043)	(0.050)	(0.043)	(0.042)	(0.062)
DiD	0.097***	-0.030	0.113**	0.025	0.007	0.040
Industry FE	(0.038)	(0.043)	(0.047)	(0.043)	(0.040)	(0.044)
DiD	0.101**	-0.027	0.108**	0.017	0.020	0.026
Industry-Year FE	(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 and Wu (2006), Kaplan and Zingales (1997), and Hadlock and 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 (11). 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.