Labor Representation in Governance as an Insurance Mechanism

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May 2018

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Abstract

We hypothesize that labor participation in governance helps improve risk sharing between employees and employers. It provides an ex-post mechanism to enforce implicit insurance contracts protecting employees against adverse shocks. Results based on German establishment-level data show that skilled employees of firms with 50% labor representation on boards are protected against layoffs during adverse industry shocks. They pay an insurance premium of 3.3% in the form of lower wages. Unskilled blue-collar workers are unprotected against shocks. Our evidence suggests that workers capture all the gains from improved risk sharing, whereas shareholders are no better or worse off than without codetermination.

Keywords: Risk-sharing, Employment insurance, Worker representation on corporate boards

JEL Classifications: J59, G34, G38

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Labor Representation in Governance as an Insurance Mechanism*

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Abstract

We hypothesize that labor participation in governance helps improve risk sharing between employees and employers. It provides an ex-post mechanism to enforce implicit insurance contracts protecting employees against adverse shocks. Results based on German establishment-level data show that skilled employees of firms with 50% labor representation on boards are protected against layoffs during adverse industry shocks. They pay an insurance premium of 3.3% in the form of lower wages. Unskilled blue-collar workers are unprotected against shocks. Our evidence suggests that workers capture all the gains from improved risk sharing, whereas shareholders are no better or worse off than without codetermination.

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The analysis of risk, hedging and risk-sharing have been at the core of finance research since the beginnings of modern finance, leading to a remarkable level of sophistication in the analyses of asset allocation, hedging of risky positions, and the pricing of risky securities. In contrast, the risk associated with human capital — a significant portion of wealth and arguably the largest source of risk, and most difficult to diversify for average workers—has received limited attention.[1] More recently, however, a new stream of research on finance and labor has demonstrated a critical role of human capital and employment risk in understanding risk-return trade-offs, indirect costs of financial distress, optimal capital structure, and restructuring.[2]

In this paper, we focus on sharing human capital risk between employers and employees. Our point of departure is implicit contract theory, which holds that the risk-neutral principals of the firm provide human capital protection to risk-averse employees against adverse shocks. Employees accept lower wages in return for insuring their employment and wages (Azariadis, 1975; Baily, 1974; Gordon, 1974; Rudanko, 2011). For employees to enter such implicit insurance contracts requires a means to monitor and enforce contracts when they need protection. Enforceability is essential for implicit insurance contracts to work. If workers accept lower wages today, how can shareholders commit not to lay them off in the future when it is in their best interest to do so? The literature has appealed to arguments based on reputation and investigated the worker-friendly preferences of family owners (e.g., Sraer and Thesmar, 2007; Ellul, et al., 2017).[3] To our knowledge, ours is the first study of a contractual mechanism to enforce implicit employment contracts.[4] Specifically, we hypothesize

[1] Exceptions are, e.g., Fama and Schwert (1977), Campbell (1996), and Jagannathan and Wang (1996).
[2] On risk-return trade-offs see Palacios-Huerta (2003) and Lustig and Van Nieuwerburgh (2008). On financial distress, optimal capital structure, and restructuring, see Berk, et al. (2010); Agrawal and Matsa (2013); Graham, et al. (2016); and Agrawal and Tambe (2016). These studies are selected from a much larger literature on finance and labor. We do not provide a systematic and comprehensive survey of this literature here.
[3] Azariadis (1975) assumes that firms that do not honor implicit contracts would "suffer a catastrophic loss in reputation" (p. 1187) and Rudanko (2011) invokes a similar assumption with the claim "equilibrium contracts are likely to be self-enforcing for a range of reasonable parameterizations." (pp. 2823–2824). Thomas and Worrall (1988) assume that agents can always renege on a contract, but at an exogenously imposed cost: they will be excluded from all trading in the future if they renege on a contract.
[4] Maybe the closest to our approach is Malcomson (1983), who argues unions enforce implicit contracts.

labor representation on corporate boards is such a mechanism to ensure employment guarantees will be honored.

In the 1970s, Germany led a trend towards more labor representation, and most countries now grant workers some degree of representation, at least in the form of work councils at the firm or establishment level. The world map in Figure 1 shows countries with labor representation at the firm level and/or at board level as of 2015.[5] By 2015, only two countries among the OECD countries - the United States and Singapore - did not grant workers any representation.

To test our hypothesis on labor representation on corporate boards, we examine the German system, which requires 50% employee representation on supervisory boards – hereafter referred to as parity-codetermination – when firms have more than 2,000 employees working in Germany. Germany offers a laboratory in which otherwise similar companies have different degrees of labor representation. In addition, the Institute of Employment Research (IAB) in Germany provides detailed, high quality panel data on employment and wages for all establishments located in Germany over our sample period 1990 to 2008.

Establishment level data allow cleaner identification devoid of false detection of insurance that can occur with firm level data. For example, if a firm fires workers in a plant—no employment protection—and adds the same number of different workers at another plant in a different industry, aggregate employment at the firm will be unchanged, making it appear as if the firm provided full employment protection. Establishment data also allow us to construct adverse industry shocks using the establishments of other, non-sample firms in the same industry.[6]

[5] Data are from the CBR-LRI dataset (Adams, et al., 2017) and refer to earlier years if data for 2016 were not available. Of the 117 countries covered, 27 grant workers board-level representation, albeit to varying degrees. Between 1970 and 2016, there were 28 changes of these laws in 19 countries, of which 22 led to more, and only 6 to lower, worker representation. Moreover, 92 countries provide workers with influence at the plant level or firm level through work councils. These laws were amended 78 times in 58 countries since 1970, leading to more influence for workers in 60 cases and less influence in 18 cases.

[6] See Section 2.3.2 for a detailed description of how industry shocks are defined and constructed.

Our initial identification strategy is a difference-in-differences approach. We are mindful of the fact that parity co-determination is a discrete function of the number of employees in Germany, which is loosely related to firm size. We employ three different strategies to separate the parity-codetermination effect from the effect of employment level in Germany: (1) the number of employees in Germany interacted with the shock indicator; (2) placebo tests using false thresholds different from the parity threshold of 2,000; and (3) regression discontinuity analyses around the 2,000-employee threshold for parity codetermination. All results support the same conclusions. We also check for the potential endogeneity of parity status and find no evidence that either workers or shareholders successfully manipulate the number of employees in Germany to maintain or avoid the parity status. It appears the parity status is largely a result of employment decisions driven by business and operational considerations.

We find white-collar and skilled blue-collar workers of parity-codetermined firms are protected against layoffs during adverse industry shocks, while those working for non-parity firms are not. Surprisingly, unskilled blue-collar workers of the same parity firms are unprotected from layoffs during industry shocks. We attribute this observation to the absence of unskilled blue-collar worker representation on supervisory boards. We could not identify a single unskilled blue-collar worker among labor representatives, which underscores the importance of worker participation in governance for an effective implementation of implicit employment insurance contracts.

From the evidence on employment protection alone, we cannot infer that firms honor implicit insurance contracts against adverse industry shocks. Parity codetermination may result in employee entrenchment sufficient to prevent layoffs (e.g., Pagano and Volpin, 2005; Atanassov and Kim, 2009); employees may not need to pay an insurance premium in the form of lower wages for the increase in job security. There are no theoretical reasons to believe that codetermination makes employees just strong enough to enforce an implicit insurance contract, without causing employee entrenchment. Therefore, whether codetermination leads to an insurance relationship or employee entrenchment is an empirical question. We find that workers with vocational and higher educational qualifications, two categories that cover most skilled blue-collar and white-collar workers, receive significantly lower wages at parity-codetermined firms. Their average wage concession is about 3.3%, supporting the notion of a quid pro quo in exchange for employment insurance.

Are the wage differentials between parity-codetermined firms and non-parity firms sufficient to compensate shareholders for risks they bear in providing insurance? Providing insurance is costly, because it increases operating leverage. We find parity firms' operating profits and valuation suffer more during shock periods compared to non-parity firms. While we observe that employees make concessions, this concession may still be too low to compensate shareholders for the additional risk. Short of a structural model of the costs of reduced flexibility to firms, we cannot directly infer whether the wage concessions are adequate.[7] We investigate this issue indirectly by relating the parity status to firm performance as measured through the cycle over the non-shock and shock periods. We find no significant relation between parity codetermination and operating performance or shareholder value, which is in line with the overall conclusions reached in the literature on the effect of German codetermination on firm performance.[8] On average, parity firms perform no worse or better than non-parity firms. Hence, whatever efficiency gains arise from improved risk sharing, workers seem to capture all these gains, making the minimum wage concessions that allow shareholders to be no worse off, but also no better off than without codetermination. The lack of shareholder gains may explain why firms do not voluntarily adopt codetermination, an observation often used to support arguments against worker participation in governance (Jensen and Meckling, 1979).

The fact that shareholders do not lose from parity codetermination may also be the reason why we do not find evidence for shareholders manipulating the number of employees in Germany so as to evade parity status. Parity codetermination seems to be, if anything, a second-order consideration

[7] Guiso, et al. (2005) provide a calibration based on a parameterization of Lucas's tree model following Clark, et al. (1994). They estimate that workers would be indifferent between the partial insurance the authors find for Italian firms, and uninsured consumption that is on average 9% higher than insured consumption.

[8] Renaud (2007) surveys 13 studies investigating the impact of codetermination on company performance using different methodological approaches, sample constructions, and performance variables. The overall evidence seems inconclusive, suggesting no systematic relation between parity codetermination and firm performance.

from the point of view of shareholder value, so that other, first-order business and operating considerations drive firms' employment decisions in Germany.

Our paper makes multiple contributions. We contribute to the literature on implicit insurance contracts by focusing on how such contracts are enforced.[9] This literature has relied mostly on reputation as an enforcement mechanism, which is fragile when companies undergo ownership changes (Shleifer and Summers, 1988). To our knowledge, we are first to think of codetermination as an enforcement mechanism in an implicit insurance relationship. The closest theoretical contribution in this context is Malcomson (1983), who argues that unions may act as an enforcement mechanism for implicit wage contracts. An earlier literature on life-long employment in Japanese firms views the security of long-term employment relationships as mechanisms to facilitate greater investments in firm-specific human capital (Hashimoto and Raisian, 1985; Abraham and Houseman, 1989). However, these papers do not specify how firms commit to protecting long-term employment relationships. Faleye, et al. (2006) and Kim and Ouimet (2014) analyze the implications of employee stock ownership in the United States, but do not explore the possibility that employee equity ownership might help enforce implicit contracts.

A strand of the family firm literature argues that it is easier for family firms to commit to implicit contracts because their managers have a longer time horizon.[10] Sraer and Thesmar (2007) show that family firms in France insure workers against employment shocks; however, they recognize that even with the long-term perspective provided by dynastic management, family-owners face incentives to renege on prior promises to insure workers during downturns. To explain their finding, they rely on preferences "irrationally tying top management to employees" (p. 729). Our argument is based on an explicit mechanism and self-interest and not on the preferences of decision-makers. Ellul, et al. (2017) find evidence of employment insurance in family firms for a cross-section of countries. However,

[9] See Azariadis and Stiglitz (1983) and Rosen (1985) for early surveys of this literature.
[10] Bach and Serrano-Velarde (2015) argue that it is family links between CEOs and their successors that enhances firms' ability to commit to implicit contracts.

insurance by family firms seems only partial; workers accept higher fluctuations in wages and are not covered against shocks originating from banking crises, when access to capital dries up. Similarly, D'Aurizio and Romano (2013) show that family firms in Italy do not insure workers employed in locations further away from company headquarters.

Ours is also the first to provide a quantitative estimate for the insurance premium based on establishment-level data within the context of the *quid pro quo* of the implicit insurance relationship.[11] Guiso, et al. (2005) examine a matched employee-firm panel of Italian firms and show that firms protect workers against wage shocks; by contrast, our study analyzes employment shocks. In addition, Guiso et al. lack a benchmark and a control group of firms that provide less or no insurance. Similarly, Beaudry and DiNardo (1991) provide evidence for the implications of implicit-contracting models for wage dynamics over the business cycle, but without looking at enforcement mechanisms or insurance premiums.

In a broader context, our paper provides evidence that balances critical views on labor representation in governance. The critics argue it is detrimental to the interests of investors, because sharing control rights with labor through representation on the board can have a harmful impact on firm performance. We find German codetermination improves risk sharing without hurting firm performance, suggesting a gain to employee welfare without damaging shareholders.

1. Theoretical Considerations and Hypothesis Development

In the simplest version of the insurance argument, diversified, risk-neutral investors (firms and entrepreneurs) insure risk-averse workers against firm-level shocks by promising them a wage that does not vary with their productivity from period to period. In most models, insurance affects wages as well as the employment status of workers (e.g., Azariadis, 1975; Baily, 1974; Holmstrom, 1983; and Gamber, 1988). Workers give up a portion of their wages in return for protection against adverse

[11] Ellul, et al. (2017) estimate this premium to be 6% based on firm-level data.

shocks to wages and employment and receive wages that are sometimes above and sometimes below their marginal product.

The insurance provided to workers shifts employment risk from workers to investors, but an effective risk transfer requires a commitment device. Workers first give up a portion of their wages. Later they have to count on firms' honoring contracts in the event of adverse shocks. The theoretical literature on implicit contracts simply assumes that firms have the ability to commit to long-term contracts. However, commitment is crucial. Workers often have to locate close to the firm or make investments in firm-specific human capital well before the firm has to honor its side of the bargain, which makes them vulnerable to breaches of implicit contracts (Shleifer and Summers, 1988). We argue parity-codetermination serves as a commitment device by allowing workers to influence employment decisions.

> ***Hypothesis 1:*** *Parity-codetermination provides an ex-post enforcement mechanism, which ensures workers receive protection against adverse shocks to employment and wages.*

This hypothesis explicitly incorporates employment guarantees, which imply that firms do not fire workers even when layoffs are ex-post efficient. If workers and firms could engage in frictionless bargaining, they would always agree to sever the employment relationship ex-post by negotiating suitable transfers, which makes ex-post inefficient employment of workers unsustainable. Models with employment insurance implicitly rule out such frictionless bargaining between firms and workers.[12] Specifically, our hypothesis relies labor market frictions such as mobility costs (Baily, 1974; Beaudry and DiNardo, 1991) or search frictions (Rudanko, 2011), so that firms do not have to pay the competitive market wage in every period. It also requires the assumption that firms have better access to capital markets than workers and therefore enjoy a privileged position to insure

[12] Without frictions in the labor market, only partial insurance is feasible, because workers always receive pay increases if their marginal product rises above their wage. Harris and Holmstrom (1982) and Thomas and Worrall (1988) discuss contracting problems in such a setting.

workers; otherwise, workers could insure themselves directly through capital markets (e.g., Berk and Walden, 2013).[13]

When firms act as insurers to workers, they enter a quid-pro-quo relationship, whereby workers receive job guarantees in return for an insurance premium. The standard form of compensating firms in the implicit contracting literature is through lower wages. We hypothesize that only parity-codetermined firms provide insurance to workers, so only parity-codetermined firms receive insurance payments from workers in the form of lower wages:

> ***Hypothesis 2:*** *Firms with parity-codetermination pay on average lower wages than non-parity firms.*

Lower wages are not necessarily the only way to compensate shareholders, but in all likelihood, lower wages are the most efficient way to provide an insurance premium because they do not distort the allocation of resources.

2. Institutional Background, Data, and Empirical design

2.1 The German Governance System and Wage-Setting Process

Germany has a two-tier board system. The management board (*Vorstand*) manages day-to-day operations and the supervisory board (*Aufsichtsrat*) monitors the management board; approves key strategic decisions; and appoints and dismisses management board members, including the CEO, and decides their compensation. The two boards are strictly separated and no member of one board can be a member of the other for the same company at the same time.

The structure, size, and composition of the supervisory board is regulated by the German stock corporation act (*Aktiengesetz*) and the codetermination act (*Mitbestimmungsgesetz*) as well as other laws. The regulation requires that half of the supervisory board members are worker representatives for corporations with more than 2,000 employees working in Germany. For corporations with more

[13] Berk and Walden (2013) argue that firms insure workers' human capital risk and investors spread this risk by investing in diversified portfolios. Workers could insure themselves directly through participation in capital markets. However, Berk and Walden show indirect insurance through firms is sufficiently close to being optimal so that workers prefer it to direct participation in capital markets even if the costs of direct participation are small.

than 500 up to 2,000 employees in Germany, one-third of the members of the supervisory board have to represent workers. Worker representatives are elected by the company's workers. Depending on the size of the supervisory board, two or three seats of the worker representatives are reserved for union representatives. One seat is always reserved for a representative from middle management (*leitende Angestellte*).[14] The chairman of the board casts the deciding vote in case of a tie.

Wages in German firms are often set through collective bargaining agreements between trade unions and employers' associations. Some firms are covered by sectoral agreements, whereas others negotiate firm-level agreements. Agreements are binding on all workers as German law forbids discriminatory wage policies that disadvantage non-union members. However, firms still have discretion over wages through firm-level wage agreements and the way in which they categorize workers, resulting in significant variation in wages across firms (Kohn and Lembcke, 2007). Unionization in Germany is generally not high and is also falling. Based on survey data, Schnabel and Wagner (2007) estimate it to be about one-third in 1992, declining to around 20% in 2004. Coverage by unionized wage agreements also has declined (Hassel, 1999). See Institutional Details in the Online Appendix for more information on German codetermination and collective bargaining.

2.2 Data

Employment and wage data at the establishment level are obtained from the Institute of Employment Research (IAB). The IAB is the research organization of the German employment agency, the *Bundesagentur für Arbeit* (BA). The BA collects worker and employer contributions to unemployment insurance and distributes unemployment benefits. All German businesses are required to report detailed information on employment and wages to the BA. The individual-level data is aggregated at the establishment level, made anonymous, and offered for scientific use by the IAB. An establishment is any facility having a separate physical address, such as a factory, service station,

[14] For firms with more than 2,000 and up to 10,000 employees, the supervisory board has 12 seats, of which six are shareholder representatives, three are from the firm's workers, one is from middle management, and two are from unions. For firms with up to 20,000 (over 20,000 employees), the size of the supervisory board increases to 16 (20) and the number of representatives from each constituency becomes: shareholders 8 (10), workers 5 (6), middle management 1 (1), and union representatives 2 (3).

restaurant, or office building. The IAB owns detailed establishment level data on industry, location, employment, employee education, age, nationality, and wages. The data are in the form of establishment-level statistics, such as sums, medians, quartiles, and averages on wages and employment according to different classifications and breakdowns.

We use the first three-digits of the NACE code, which identifies 224 separate economic sub-sectors (groups).[15] The NACE (Rev. 1.1) classification is available from the IAB database only for 2003 and afterwards. We assign an establishment's NACE (Rev. 1.1) classification in 2003 to all its prior sample years. Some establishments may have changed their industry classification prior to 2003, in which case they would receive new establishment IDs. To avoid assigning incorrect industry codes, we drop all establishments changing industry classifications over time in the entire IAB database, as well as establishment-year observations with missing information on industry classification. These screens yield approximately 33.4 million establishment-year observations on approximately 3.5 million establishments for the sample period 1990 through 2008.

Our sample firms are drawn from all companies included in the two main German stock market indices, DAX and MDAX, at any point over the 19-year period from 1990 to 2008. There are 184 such firms, for which we hand-collect data on the composition of the supervisory board from annual reports and *Hoppenstedt* company profiles. We do not include non-listed firms, because information on worker representation on their boards is usually unavailable. Stock market data comes from *Datastream*, balance sheet and accounting data from *Worldscope*.

At our request, the IAB matched our sample of listed firms with their establishment-level database using an automatic procedure, based on company name and address information (city, zip code, street, and house number). All cases not unambiguously matched by the automatic matching

[15] The industry classification is based on the Statistical Classification of Economic Activities in the European Community (NACE), a six-digit industry classification. NACE is similar to NAICS (North American Industry Classification System). The first four levels are the same for all European countries. The IAB database contains different versions of the NACE classification. We use NACE Revision 1.1, which is based on the International Standard Industrial Classification (ISIC Rev. 3) of the United Nations.

procedure are checked by hand to avoid mismatching. This procedure results in 284,538 establishment-years matched to 2,168 firm-years for 142 of the 184 firms. Of these, 1,461 firm-years (67.4%) are subject to parity codetermination, 442 (20.4%) subject to one-third codetermination, and the remaining 265 firm-years are not covered by any codetermination arrangement.

The matching was performed for 2004, 2005, and 2006. Firms are dropped if they do not exist during the period 2004 through 2006, because we cannot match them to the IAB data. IAB does not have a firm identifier, which is why manual matching is necessary. All establishments are matched only once to our sample firms and, if establishments were sold prior to 2004, they do not enter our sample because IAB cannot match them. This matching procedure does not allow us to identify changes in establishment ownership after 2006. (At the time of matching establishments to firms, establishment data was not available for 2007 or 2008) Thus, if an establishment belonging to a parity (non-parity) firm is sold to a non-parity (parity) firm in 2007 or 2008, it will be treated as if it still belonged to a parity (non-parity) firm after the sale. This will blur the distinction between parity and non-parity status of the establishment and potentially lead to attenuation bias working against finding significant results.

The IAB distinguishes employees by occupational status. The three most important groups are unskilled blue-collar workers, skilled blue-collar workers, and white-collar employees.[16] The IAB also reports three different qualification levels at each establishment by educational and vocational qualifications: (1) Low-qualified employees possess neither an upper secondary school graduation certificate as their highest school qualification nor a vocational qualification. (2) Qualified employees have either an upper secondary school graduation certificate as their highest school qualification or a vocational qualification. (3) Highly-qualified employees have a degree from a specialized college of

[16] Other groups are employees in vocational training, home workers, master craftsmen, and part-time employees. We do not analyze these groups of employees because they usually form only a small fraction of employees and are present in relatively few establishments.

higher education or a university degree.[17] Unfortunately, over our sample period an increasing number of firms stopped reporting information on qualifications, leading to a steady increase in the number of employees with unknown qualifications. If firms' decision not to report their employees' qualification is random, the increasing trend in the number of employees with unknown qualifications should not bias our results.

We include only establishments with more than 50 employees. These establishments almost always have works councils, which have significant information rights as well as the right to demand compensation for dismissals and may exert some influence at the establishment level (see Wiedemann, 1980). Nonetheless, our results are robust to an alternative sample construction with a cutoff point at 10 employees. While fiscal years of German firms are mostly from January to December, establishment years for IAB data are from July to June. We therefore lag all variables from *Worldscope* by six months relative to IAB years. Effectively, we assign year-end values from *Worldscope* to June 30 information on employment and wages of the next year.

Table 1 provides summary statistics. Monetary units are normalized to 2005 Euros. Panels A and B provide statistics at the establishment and firm level, respectively. All accounting and market variables are taken from *Worldscope* and *Datastream*. None of the firm level variables in Panel B are from IAB. Variable definitions and data sources are listed in Table 2. Our sample selection criteria tilt the sample towards large, mature firms (mean firm age: 85 years) with high book-value leverage and low betas. About two-thirds of firm-year observations are for parity-codetermined firms. The distributions of sales, market capitalization and the number of establishments are highly skewed; the mean exceeds the 75th percentile for all three size measures.

[17] In Germany, a relatively small fraction of students obtains an upper secondary school degree (high school, Abitur), although the fraction rose from 31% in 1992 to 45% in 2008. The typical career path in Germany is to leave school after tenth grade to enter vocational training. In 2009, 57.8% of the German population had such a vocational qualification, while 27.8% had none of these qualifications. IAB classifies the former as qualified employees and the latter as low-qualified employees (see Hethey-Maier and Seth, 2010).

2.3 Research Design

Our empirical strategy is to compare how a negative shock affects employee layoffs by establishments owned by parity-codetermined firms differently from those owned by firms with one-third or no labor representation on the supervisory board. Then we compare establishment-level wages between parity and non-parity firms through the cycle of the shock and non-shock period. To cross-check robustness of results based on a particular empirical design, we employ three different strategies: difference-in-differences estimations, placebo tests, and regression discontinuity analyses.

2.3.1 Specification

The difference-in-differences analysis relies on exogenous, negative industry shocks to employment and on the dummy variable *Parity*, which is one in any establishment-year when the firm has 50% worker representation on the supervisory board, and zero otherwise. Our definition of *Parity* is based on actual board data and is not inferred from employment data. The baseline specification is:

$$y_{ijkt} = \alpha_t + \alpha_i + \gamma X_{ijkt} + \delta Parity_{jt} + \theta Shock_{kt} + \beta Parity_{jt} \times Shock_{kt} + \epsilon_{ijkt} \,. \qquad (1)$$

The dependent variable, y_{ijkt}, is the log change in employment or the log of the median daily wage, where *i* indexes establishments, *j* indexes firms, *k* indexes industries, and *t* indexes time. $Parity_{jt}$ is the parity dummy, $Shock_{kt}$ is the shock dummy defined in the next section, and ϵ_{ijkt} is an error term. We control for year fixed effects, α_t, and establishment fixed effects, α_i. Establishment fixed effects absorb firm fixed effects, and control for unobservable time-invariant firm and establishment characteristics. The vector of time-varying control variables, X_{ijkt}, includes the logarithm of the number of employees working in Germany, the logarithm of total sales, firm leverage, and establishment age.

We cluster standard errors at the firm level. Abadie, et al. (2017) argue that the requirement to cluster follows from the sampling strategy and the experimental design. We follow a two-stage sampling process, in which we identify first firms and then match establishments to firms. The argument of Abadie et al. suggests clustering at the first-stage variable for such a sampling process; in our case the firm level. A different approach would cluster observations at the industry-year level, the level at which we define shocks. We present our results with standard errors clustered at the firm level and provide a robustness check with clustering at the industry-year level in Tables A–5 (Panel A) and A–6 (Panel A) in the Online Appendix. Standard errors and significance levels are not much affected by our choice of clustering.

We shall refer to establishment-year observations in which the firm has 50% worker representation as parity observations and to all others, including those with one-third representation, as non-parity observations.[18] For brevity and ease of exposition, we will mostly, and somewhat imprecisely, distinguish between "parity firms" and "non-parity firms," to avoid the more cumbersome expression "establishment-year observations." This focus on parity establishments helps to preserve the sample size of non-parity establishments, which is smaller than that of parity establishments. Table 1, Panel B shows 67.4% of our sample establishment-years are owned by parity firms.

For employment analysis, the coefficient of main interest is the β on the interaction of *Parity* and *Shock*. It measures the differential impact industry shocks have on employment of parity and non-parity establishments. Our hypothesis predicts $\beta > 0$; parity establishments maintain higher levels of employment after an industry-wide shock than non-parity establishments. For wages, the regression does not contain *Shock* and δ is of main interest. Our hypothesis predicts $\delta < 0$; employees of parity firms pay insurance premiums in the form of lower wages.

2.3.2 Definition of Shocks

We identify adverse employment shocks using a drop in employment by non-sample German firms and foreign firms with establishments in Germany. The main requirement is that shocks are exogenous and sufficiently correlated with the economic environment of our sample firms so that

[18] Gorton and Schmid (2004) and several of the studies surveyed by Renaud (2007) also use parity codetermination as their main variable for labor representation.

they warrant major adjustments. We do not require that shocks cannot be anticipated. A suitable definition of shocks requires that *Shock* has a significant impact in equation (1). If the estimate of θ is not significant, then either the definition of *Shock* is unsuitable and the assumed shock has no impact on non-parity establishments, or workers at non-parity establishments are also insured. In either case we cannot infer whether workers at parity establishments receive more insurance from estimates of β. We do not define shocks based on employment changes in other European countries because Germany follows a different business cycle and the correlations between employment at our sample firms and other European firms are too weak. For example, during 2011 to 2012, the German economy was booming while most other European countries were in, or on the verge of, a recession.

Employment shocks are defined at the industry level. An industry is subject to a shock if non-sample firms' establishments located in Germany belonging to the same 3-digit NACE-code industry as a whole suffer a decrease of at least 5% in employment. These establishments may belong to either German or foreign firms. Our sample firms are likely to be under economic pressure to decrease their payroll when other non-sample firms in the same industry lay off 5% or more of their workers. We use the 5% threshold to ensure that shocks are strong enough to have a material effect and frequent enough to permit identification. Results based on a lower threshold of 2.5% are similar. [19]

Notice that these non-sample German and foreign firms are used only to define shocks, not as controls. The non-sample firms include many large non-listed, family owned, or foreign firms with establishments located in Germany.[20] The mean (median) total sales and the number of employees of the largest 100 non-sample firms used to identify shocks are €10.2 bn (€7.0 bn) and 33,500 (19,700)

[19] We experimented with two other definitions of shocks. The first alternative makes shocks comparable across industries with different cash-flow volatilities by scaling shocks with the standard deviation of the industry-growth rate of employment, so that a lower cut-off applies to more volatile industries. The results are qualitatively similar, but statistically weaker. The second alternative uses sales growth or growth in operating income of firms from other European countries to define industry-level shocks. These analyses mostly yield insignificant estimates on the shock variable (insignificant θ in equation (1)).

[20] Examples include Bosch, Aldi, Boehringer Ingelheim, Edeka, Rewe Group, Haniel, Shell Germany, BP Germany, Ford, Coca Cola, Procter & Gamble, Dow Chemical, Pfizer, IBM, Hewlett-Packard, ExxonMobil, Vodafone, Gazprom Germania, Sanofi-Aventis Germany, Telefónica Germany, and Fujitsu.

in 2006, respectively. These numbers are quite comparable to the corresponding numbers for our sample firms in 2006, which are €11.7 bn (€2.0 bn) and 38,700 (9,200), respectively. Ultimately, if the non-sample firms were unsuitable to define industry shocks, attenuation bias would move the coefficient of *Shock* in equation (1) towards zero and render it insignificant.

In defining industry shocks, we exclude 5% declines in non-sample firms' employment followed by an immediate recovery in the subsequent year, because other firms in the industry do not react to such shocks. Shocks followed by immediate recovery render the coefficient θ in equation (1) insignificant (see Panel B of Table A–5 in the Online Appendix). One scenario in which this may happen has some non-sample firms lose market share or exit the market, and other firms in the industry subsequently increase employment to take the space left by declining or exiting firms. We exclude these very short-lived shocks, because they provide no identification. On the other hand, sufficiently severe and long-lasting shocks will require all firms, including parity-firms, to adjust their workforce to avoid bankruptcy (Gamber, 1988; Guiso, et al., 2005). We therefore end a shock period after four years, again, because permanent shocks cannot provide identification. Thus, a shock period ends either after a resumption of growth, or after four consecutive years of non-positive growth, whichever occurs first. We also define a shock over a two-year interval. Table A–1 in the Online Appendix illustrates how *Shock* is defined for four possible sequences of employment growth over five years.

To get a feel for how our definition identifies employment shocks during our sample period, we estimate OLS regressions of the shock dummy as the dependent variable using the four-year or two-year definitions of shock. The independent variables are year dummies, and the regression does not have an intercept. Adjusted R^2s are around 18%, indicating that much of the variation in shocks is industry-specific and is not driven by the business cycle. Figure A–1 in the Online Appendix plots the year dummy coefficients in the regressions and German unemployment rate by year. The two shock definitions are highly correlated with each other and move with the unemployment rate. We report estimation results based on the four-year interval. With this definition, 5.8% of establishment-years of the sample firms are in shock periods. Baseline results using the two-year interval are similar.

3. Empirical Results

We begin with an investigation of how layoffs at establishments owned by parity firms differ from those owned by non-parity firms when the industry suffers a negative shock to employment. We then compare wages between parity and non-parity firms.

3.1 Employment

Table 3 reports estimation results with different combinations of control variables. The results are consistent with the insurance hypothesis. The coefficients on *Shock* × *Parity* in the first two columns imply parity-codetermined firms retain about 8% more employees in comparison to non-parity firms during shock periods. The majority of non-parity firms in our sample have one-third worker representation. Hence, the coefficients reflect, to a large extent, the incremental impact of moving from one-third-codetermination to parity-codetermination, and to a lesser extent the impact of moving from no employee representation to parity codetermination.

As expected, *Shock* has a significantly negative coefficient, which implies non-parity firms suffer a sharp decline in employment. We perform a standard t-test for the restriction that the coefficients on *Shock* and *Shock* × *Parity* add up to zero, which would indicate full insurance. In no specification can we reject the null hypothesis that the coefficients on *Shock* and *Shock* × *Parity* have the same magnitude with opposite signs. It appears employees working for parity firms are fully protected against negative industry shocks.

3.1.1 Identification Issues

To address identification issues, we first check the validity of the parallel trends assumption, the possible confounding effects of the level of employment in Germany (hereafter, the employment level), and whether we find similar results in placebo tests. Then we examine the potential endogeneity of the parity status and conduct regression discontinuity analysis. In addition, we examine changes in parity status due to industry shocks, cross-industry effects, and robustness to various alternative specifications.

Parallel Trends. In our difference-in-differences estimation, the outcome variable of the control group is used to calculate the expected counterfactual, assuming that parity and non-parity firms have the same time trend if there are no industry shocks. Thus, we check whether the log change in the number of employees of parity and non-parity firms were following the same trend before an industry shock. Figure 2 plots log change in the number of employees separately for our sample of parity and non-parity firms from four years prior to three years after the first shock year, where the first year of the shock is defined as event year zero. We only include establishments in industries without a shock during the four years prior to year zero. To ensure that changes in the sample do not affect trends in employment, we require that, during the event period, all establishments have data available for all years and do not change their parity status.

Figure 2 shows remarkably parallel trends in average employment growth of parity and non-parity establishments until year –1, and a sharp divergence in year 0. The sharp drop in employment of non-parity establishments in year 0 vis-à-vis parity establishments, which show no significant decline in employment, reflects what we find in Table 3: Incomplete employment protection for non-parity establishments vs. full protection for workers of parity establishments.[21]

Parity and firm-level employment. An important concern for our specification is that *Parity* is defined with respect to the number of employees in Germany. Note that only the number of employees in Germany is relevant for whether a firm is subject to parity codetermination. Hence, *Parity* is only loosely related to firm size.[22] We separate the *Parity* effect from the employment level effects in Column (3) of Table 3 by including interactions of *Shock* with the log of the total number of

[21] If parity firms hold on to more employees during negative shock periods, they may hire fewer new employees in expansion periods. Thus, the trends may not to be strictly parallel. However, Figure 2 suggests that this effect is too subtle to carry to noticeable weight.
[22] The correlations between the parity indicator and standard measures of firm size are 0.15, 0.16, and 0.27 for, respectively, firms' total assets, market capitalization, and sales.

employees in Germany, *LogFirmEmployees*, and *LogFirmEmployees*^{2}. If *Parity* has no unique effect other than just being an indicator for having more than 2,000 employees in Germany, then the coefficient on *Shock x Parity* should become insignificant if we interact *Shock* and the employment level variables. However, the coefficient of *Shock* x *Parity* remains positive and significant, albeit at a lower statistical significance. The parity status per se has a significant positive effect on employment during shock periods.[23]

We also allow the impact of *LogFirmEmployees* and its higher-order terms to be different for parity and non-parity firms in Column (4) by interacting these terms with *Parity* and (1 – *Parity*). None of the coefficients of employment level variables is significant (For brevity, the coefficients are not reported.) The coefficient on the interaction of *Shock* and *Parity* remains positive and significant.

Placebo tests. Another way to test the unique effect of parity-codetermination during industry shocks is to conduct placebo tests using false parity dummies. We reestimate Columns (1) and (2) in Table 3, separately for parity and non-parity firms, with *Placebo* replacing *Parity* in the regressions. The thresholds for the placebo dummy, *Placebo,* equal the median number of employees in Germany, which is 1,318 for non-parity firms and 10,458 for parity firms. The results are presented in Table A–2 in the Online Appendix. Coefficients of both, *Placebo* and *Shock* x *Placebo,* are statistically zero for both subsamples, demonstrating that the 2,000-threshold is unique and the treatment effect is real.

We take this placebo test idea further by reestimating specification (2) in Table 3, with *Placebo* equal to one if employment exceeds a threshold that varies from 1,500 to 2,500 in increments of 50, i.e., 1,500, 1,550, 1,600, …, 2,500. Our hypothesis predicts that the 2,000 threshold is uniquely relevant. The more we move the cut-off above (below) 2,000, the more firm-years we misclassify as non-parity (parity) firms, which creates attenuation bias and reduces the coefficients on *Parity* and *Shock x Parity*. For example, if *Placebo* is based on 2,500 employees, all establishments belonging to firms with a number of employees in Germany between 2,000 and 2,500 are classified as non-

[23] The coefficients on the interaction terms of *Shock* with the employment level variables suggest employment protection increases with the employment level in Germany at a decreasing rate.

parity, even though they are parity codetermined. Hence, if the parity effect is unique, we should find both coefficient estimates and significance levels peak at 2,000, and decline as we move the cut-off further away from 2,000.

Figure 3 presents the results for the estimated coefficients on the interaction term (right-hand axis, gray line) and the corresponding t-statistics (left-hand axis, black line). As expected, coefficients and significance levels both peak around the 2,000 threshold. Notice that if our results were driven by the employment level effect alone, without a separate effect of *Parity*, no noise would have been added at *Placebo* thresholds greater than 2000, and the coefficient would have increased (or at least plateaued out) as *Placebo* increases further away from 2000.

Endogeneity of parity status. *Parity* may be endogenous, as employers and employees may attempt to influence their firm's parity status through influencing the number of employees in Germany. Workers may want to keep the number above 2,000 to maintain parity status, whereas shareholders may attempt to keep the number below 2,000 to maintain non-parity status.[24] Such attempts would lead to an accumulation of firms around the 2,000 threshold.

To investigate whether there is an unusual concentration of firms located right below or above the 2,000 employee threshold, we first plot a histogram of the frequency of the distribution of all sample firm-year observations with 500 to 3,500 workers employed in Germany in Figure A–2 of the Online Appendix. It shows there are more firms with fewer employees. More important, it shows scattered and minor peaks throughout the whole range of 500 to 3,500, but does not show an unusual concentration around the 2,000 threshold.

To conduct a more formal test, we first use the McCrary, 2008 test, which uses a smoothed version of the histogram, testing for a discontinuity of the density at the 2,000 threshold. Figure 4

[24] Shareholder-directors' resistance to crossing the 2,000 threshold, if any, cannot be due to their concern about losing their board seats, because when a non-parity firm crosses the threshold board size increases to accommodate the additional employee representatives. In our sample, when a firm becomes parity-codetermined, on average, its board size increases by five seats, leading to one more shareholder and four more employee representatives.

shows the smoothed density estimate, which allows for a discontinuous jump of the density at the threshold. Different estimates are obtained depending on the range of data included; we use ranges from 1,500 to 2,500 employees and expand it symmetrically; the largest interval is from zero to 4,000 employees. In all cases the estimate of the discontinuity is smaller than its standard error; we cannot reject that the density is continuous at 2,000 at any conventional significance level.

To be cautious, we conduct two additional tests. The first involves a non-parametric regression of employment growth on the employment level. If firms perceive parity codetermination as very costly, they may sacrifice growth if employment is just below the 2,000 threshold. Employees, on the other hand, may resist contraction more if the level of employment is just above the 2,000 threshold. Hence, we expect a negative relation between the number of employees and employment growth below the threshold, and a positive relation above the threshold. Figure 5 plots a non-parametric regression allowing the slope of the regression of employment growth on employment to change arbitrarily with the level of employment. We find no evidence in support of the manipulation hypothesis. Although never significant, the regression is upward sloping just below the threshold, and downward sloping just above the threshold, the opposite of what we would expect if workers or shareholders would influence the number of employees to prevent board composition from changing.

For the second test, we tabulate the frequency of firms crossing various employment level thresholds from below or above by counting how many times our sample firms cross 1,500, 1,600…up to 2,500 in the increment of 100. If shareholders prevent crossing the 2,000 threshold from below, and workers prevent crossing the 2,000 threshold from above, then we should observe fewer crossings over 2,000 compared to all other thresholds. Table A–3 in the Online Appendix reports the tabulation results, which show more or less even distribution across all thresholds without a noticeable difference for the 2,000 threshold.

In sum, all analyses lead to the same conclusion: There is no indication that firms cluster around the 2,000 threshold or manipulate the likelihood of crossing the threshold, either upwards or downwards. It appears business and operational considerations are the dominant factor in deciding the employment level. There is no evidence that either workers or shareholders influence employment decisions to suit their interests in terms of board representation. As we shall show later, parity codetermination has no discernible impact on firm profitability or value; hence, firms are unlikely to alter investment decisions just to avoid codetermination. Whether a firm is just above or just below the 2,000-threshold seems rather random.

Regression discontinuity analysis. The above conclusion allows us to perform regression discontinuity (RD) analysis. Testing whether our conclusions hold locally around the 2,000-employee cut-off point improves the covariate balance relative to difference-in-differences analysis. However, it comes at the cost of lowering the power of the tests stemming from smaller sample size: RD analysis has to be restricted to industry-shock periods, which drastically reduces our sample size to 5.8% of establishment-years in our sample.

We regress employment in shock periods on *Parity* using a local linear regression:

$$y_{ijkt} = f\left(FirmEmployees_{it}\right) + \delta Parity_{jt} + \eta_{jt}, \tag{2}$$

where $f(\ )$ is a kernel-weighted local polynomial of degree one. Table 4 reports the results and Figure 6 plots local polynomial regressions around the 2,000 threshold with 95% confidence intervals in dashed lines.[25] We show results for the optimized bandwidth based on Imbens and Kalyanaraman, 2012 as well as results from doubling or halving the optimal bandwidth. We apply a sharp regression discontinuity design, which is appropriate here, because firms never elect to be parity-codetermined if they are below the 2,000 threshold.

In spite of the big drop in sample size, the results show a statistically significant upward jump of employment residuals above the 2,000 cutoff for the optimal bandwidth, consistent with the shock influencing employment at non-parity firms more strongly than at parity firms. Results remain

[25] We cannot report sample averages for each bin, as is customary for local polynomial plots, because the IAB, our data provider, does not allow us to report moments of samples with fewer than 30 observations.

significant if we increase the bandwidth, but not if we halve it, which is equivalent to a further reduction in sample size that is already very small.

Continuous measures of industry shocks. Sraer and Thesmar, 2007 and Ellul, et al., 2017 study implicit employment contracts by family-firms using continuous measures of shocks. To check the robustness of our conclusions to continuous measures of shocks, we re-estimate our baseline specifications with measures of shocks similar to those used in Sraer and Thesmar, 2007 and Ellul, et al., 2017.

First, we follow Sraer and Thesmar, 2007 and define a new variable $LogMeanIndEmpl_{it}$, the logarithm of the industry mean of contemporaneous employment of non-sample firms in the 3-digit NACE industry firm *i* belongs to, excluding the employment of firm *i* itself.[26] We interact $LogMeanIndEmpl_{it}$ with the *Parity* indicator, in keeping with our earlier analysis. Here, *Parity* takes the place of indicators for family involvement in the family-firms literature.[27] Table 5 (Panel A) shows the results. Note that with this definition of shocks, our hypothesis predicts opposite signs: a positive sign on $LogMeanIndEmpl_{it}$ and a negative sign on the interaction with *Parity*. The coefficient on the inaction term is negative and highly significant in all specifications. Specifications controlling for year fixed effects (Columns 3 and 4) shows coefficients of 0.29 and 0.36 on *LogMeanIndEmpl* , a range of elasticity estimates quantitatively similar to those in Sraer and Thesmar, 2007. With this definition, insurance in parity firms manifests in a reduction of the sensitivity to industry-level shocks of about 50% for parity firms.

We also reestimate our baseline regressions following Ellul, et al., 2017, who use first differences and total log changes as “shock” variable, a somewhat different approach from Sraer and Thesmar, 2007. Specifically, we regress the log change in employment on *LogChangeIndEmpl*, the log change

[26] We use employment to construct a continuous measure of shocks instead of sales used in Sraer and Thesmar, 2007 because the IAB does not provide data on sales at the establishment level.

[27] Our specification replicates equation (4) of Sraer and Thesmar (2007). Their specification in Table 7, column (3) closely resembles our specification in Table 5 (Panel A), column (3), which includes year and establishment fixed effects.

in employment of non-sample firms' establishments in the same industry. Table 5 (Panel B) reports the results, which imply that parity firms' response to industry-level employment changes are only about one-third of that of non-parity firms. In sum, our conclusions based on discrete shocks are robust to continuous measures of industry shocks.

An alternative sample construction and alternative definitions of shock. We reestimate the regressions in Table 3 using other alternative specifications and report the results Table A–5 of the Online Appendix. Specifically, in Panel C we include all establishments with more than 10 employees. In Panel D we define *Shock* by a 2.5% drop in industry-wide employment among non-sample firms instead of the 5% drop. In Panel E we use the two-year interval in defining *Shock* instead of the four-year interval. In Panel F we control for outliers by truncating establishment-years with employment growth in the 1st and 99th percentile. Results are robust to these alternative specifications.

Changes in parity status due to industry shocks. An industry shock may cause a sufficient drop in employment to make a parity firm become a non-parity firm. If both happen simultaneously but we observe the drop in employment after the change in parity status, we will incorrectly attribute the employment change to non-parity firms, underestimating the decline in employment among parity firms. To check this possibility, we first read annual reports of all firms changing their status from parity to non-parity.[28] In almost all cases, worker representatives stay on the supervisory board until the annual shareholder meeting the year after the number of employees in Germany drops below 2,000; hence, our findings are highly unlikely to be affected by the simultaneous drop in employment and change in parity status.

We are also concerned about establishments switching from parity to non-parity status that were in an industry shock the year of or the two years before the change in parity status. A big drop in

[28] The loss of employees causing the changes in parity status is mostly due to major asset sales. In most cases, the annual reports mention sales of certain parts of the firm, which are intended to change the focus of operations.

employment in those establishments could cause the change in parity status. Our sample contains 236 establishments switching from parity to non-parity status. Of the 236 establishments, only 23 were in an industry shock in the year of or the two years before the parity status change. We drop all observations (49) belonging to these 23 establishments for three years after the switch, and reestimate Columns (1) to (3) of Table 3. The reestimation results, presented in Table A–4 of the Online Appendix, are robust.

Cross-industry effects. Firms operating in multiple industries may react to a shock in one industry by transferring workers to establishments in non-shock industries (Tate and Yang, 2016). Such transfers would be recorded as employment losses in establishments affected by a shock, lowering the coefficient on *Shock x Parity,* and as employment gains in establishments unaffected by the shock, not affecting the coefficient on *Shock x Parity*. Hence, the net effect would be to bias our results to finding less insurance. In spite of this potential bias, we find full insurance for parity firms.[29]

3.1.2 Local Labor Market Conditions and Positive Industry Shocks

In this section, we extend our hypothesis to consider how the insurance effect of parity codetermination depends on local labor market conditions and how parity and non-parity firms differ in their response to positive industry shocks.

Local labor market conditions. We expect implicit employment insurance be more prevalent when employment risk is greater. Labor markets are local and migration across local labor market has declined (Molloy, et al., 2017). Accordingly, employees become more vulnerable to losing their jobs if employers have more local monopsony power (Benmelech, et al., 2018). Hence, we hypothesize that if one employer accounts for a larger fraction of employment in an industry in their local labor market, demand for insurance will be greater and codetermination as an enforcement mechanism will have greater impact on preventing layoffs. To test this prediction, we adapt the research design of

[29] Employee transfers between establishments belonging to the firm and same industry (i.e., moving employees from one establishment to another in the same industry) will cancel out and will not be observed. They are not of concern because such transfers are unlikely to be a response to industry shocks.

Benmelech, et al., 2018 and measure employer concentration for each county-industry-year for the 401 counties and 222 NACE industries (3-digit level) by calculating the Hirschman-Herfindahl index for the employment shares of local employers. We then define the indicator variable *Herfindahl*, which is one if the *HHI* is above the median across all counties and industries, and zero otherwise.

We estimate triple-differences using the same specifications as Table 3 and report the results in Table 6. The variable of main interest here is the triple interaction of *Shock*, *Parity*, and *Herfindahl*. Its coefficient measures whether and how the effect of parity codetermination on insuring employment mechanism depends on employer concentration at the industry level. The effect is economically strong, with coefficient values around 0.05, and statistically significant at the 10% level in all specifications. From the interaction of *Shock* and *Herfindahl*, we also observe that higher employer concentration increases the vulnerability of employees to shocks, although this effect is not significant. These results imply that the insurance effect of parity codetermination is greater, the more vulnerable employees are to the market power of local employers, providing further corroborating evidence in support of our hypothesis.

Positive shocks. Since our hypothesis is about insurance against adverse industry shocks, it does not yield direct predictions about how positive industry shocks affect employment of parity firms differently from non-parity firms. However, it is plausible that when parity firms know it is difficult to lay off workers because of implicit contracts enforceable through parity codetermination, they will be more cautious to add workers when business conditions call for more employees. Similarly, if they have retained workers after negative shocks in the past, they are less likely to add new workers at the same rate as non-parity firms. The provision of job security is tantamount to increased firing costs in a dynamic model with adjustment costs. Based on calibrations of such a model (Bentolila and Bertola, 1990), we expect parity firms to be less responsive to positive shocks than non-parity firms.

To test this prediction, we compare employment growth in parity and non-parity establishments when the industry as a whole experiences positive growth in employment. The positive growth indicator, *Pos. Growth*, is equal to one when non-sample firms' establishments located in Germany belonging to the same industry as a whole increase their work force. We reestimate Table 3 with *Pos. Growth* in the place of *Shock* and report the results in Panel G of Table A–5. The results are consistent with our conjecture. While non-parity establishments increase the number of employees by about 4%, parity establishments show a significantly smaller increase, roughly half those of non-parity establishments.

3.1.3 Employment by Occupational Status

The estimation results based on all employees may mask important heterogeneity across different types of employees. Table 7 therefore reestimates the employment regressions separately for each skill level. We include the same set of control variables as in specifications (1) and (2) in Table 3. For white-collar workers and skilled blue-collar workers, the coefficient on *Shock x Parity* is positive, economically large, and statistically significant for both specifications. The results for unskilled blue-collar workers are in sharp contrast; both specifications show an insignificant coefficient on *Shock* × *Parity* and the sum of *Shock* and *Shock* × *Parity* is significantly negative at the ten percent level. Unlike white-collar and skilled blue-collar workers, there is no evidence these workers are protected against an industry-wide decline in employment.

To explain this surprising finding, we hand collect information on the occupational status and the educational and vocational qualification of labor representatives on supervisory boards for 1990, 1999, and 2008, the beginning, the middle, and the end of our sample period, for all firms which provide the relevant information in their annual reports. Table 8, Panel A, categorizes labor representatives into unskilled blue-collar, skilled blue-collar, white-collar, and union representatives. The occupational status of union representatives is usually not reported, so they form a separate category in the table. In Panel B we categorize labor representatives as low-qualified, qualified, and highly-qualified. Here we exclude union representatives, because information on union representatives' educational/vocational qualifications is usually not reported. Panel C shows the occupational status and qualification categories are closely related.

These tabulations reveal a striking phenomenon: We do not find a single unskilled blue-collar or low-qualified worker among the labor representatives for any firm in any year. Although companies not included in this tabulation may have unskilled blue-collar or low-qualified workers on their boards, the numbers in Table 8 reveal the lack of real representation of unskilled blue-collar or low-qualified workers.[30] The implicit insurance contract works only for those who have their own kind represented on the board, an important enforcement mechanism to ensure employers honor the contract. There appears to be an agency problem between unskilled workers and their representatives on supervisory boards. Why unskilled workers cannot remedy this lack of proper representation is puzzling and warrants further investigation, which would require an in-depth analysis of the election process of worker representatives, an issue beyond the scope of this paper.

3.2 Wages

The protection against layoffs may not be the result of implementing implicit insurance contracts. The employment protection could be due to employee entrenchment through worker-management collusion (Pagano and Volpin, 2005; Atanassov and Kim, 2009). Whereas the implicit-insurance hypothesis predicts lower wages (Hypothesis 2), the entrenchment hypothesis predicts, if anything, the opposite. Such entrenched employees are unlikely to accept lower wages. Thus, we relate wages to *Parity*, the coefficient of which measures the wage difference between parity-codetermined and non-parity firms. We use the median wage at each establishment, because the IAB provides only the first quartile, the median, and the third quartile wages. We use two sets of control variables: (1) the same control variables as in Column (2) in Table 3; and (2) these variables plus the number of employees in the establishment, the median employee age, and the percentage of white-collar employees. Prior research suggests the additional control variables help explain average employee

[30] Interestingly, the IAB's tabulation based on a random sample shows a disproportionately large percentage of foreign workers in the unskilled blue-collar worker category. Whereas Germans represent 93% of skilled blue-collar workers and 96% of white-collar workers, they represent only 80% of unskilled blue-collar workers, with the rest being foreign workers.

wages (e.g., Oi and Idson, 1999; Brown and Medoff, 1989. We take logs of all level variables when estimating regressions.

3.2.1 Wages for All Employees

The first two columns in Table 9 report estimation results for all employees. The coefficients on *Parity* are negative and highly significant. The point estimate in column (2) indicates employees of parity-codetermined firms receive on average about 3.3% lower wages, consistent with Hypothesis 2 and implicit contract theory.

We cannot completely rule out that employees offer wage cuts in exchange for other benefits we do not observe; for example, for better working conditions. While possible, we believe our explanation is more parsimonious. There is no reason to believe that other aspects of the labor contract, such as working conditions, safety, etc. change abruptly at the 2,000 threshold of employees in Germany. Working conditions are negotiated with work councils at the establishment level and usually not part of board-level negotiations. The discontinuous change at the 2,000 threshold documented through the placebo tests and regression discontinuity analysis above speaks more in favor of a quid pro quo in exchange for employment insurance.

Estimated coefficients on controls are mostly consistent with intuition. Older employees and employees working in older establishments and establishments with a greater proportion of white collar workers are paid more. However, the number of employees in establishments is associated with lower wages. This is somewhat surprising given the finding in Brown and Medoff (1989) that an increase in the number of employees is associated with higher wages. The difference could be due to differences in sample and specification. Our sample is at the establishment level, heavily skewed towards large firms, and our regression contains a number of other firm size variables, whereas Brown and Medoff, 1989 rely on firm-level data, include small businesses, and their specification contains fewer size controls.

We do not include *Shock* and *Shock* × *Parity* in the regressions, because we are primarily interested in the difference in average wages between parity and non-parity firms. For consistency with the employment regressions, we include *Shock* and *Shock* × *Parity* in the wage regressions and report results in Table A–6, Panel B, Columns (1) and (2). The results are robust; employees of parity firms receive 3.0% to 3.1% lower wages during non-shock periods and the difference is significant. The coefficients on *Shock* are insignificant. The *Shock* × *Parity* term shows positive but insignificant coefficients. These results confirm our prior that German wages are sticky and do not respond to industry shocks in significant ways.

3.2.2 Wages by Qualification

The IAB does not provide the wage data broken down by occupational status. It provides only the breakdown by qualifications. To see how the classification based on qualifications corresponds to occupational status, at our request, IAB cross-tabulated the percentage of employees belonging to each type of occupational status and qualification based on a random sample of 2% of all employees covered by its database between 1975 and 2008 (Sample of Integrated Labour Market Biographies). The tabulation is shown in Table 8, Panel C. Most highly-qualified workers tend to be white collar workers; most qualified workers are either white collar or skilled blue collar workers; and most low-qualified workers are unskilled blue collar workers.

Columns (3) through (8) in Table 9 report separate estimates for employees grouped by qualification.[31] Coefficients on *Parity* range from –2.9% to –3.4% for all three qualification levels. The coefficients are highly significant for the qualified and highly-qualified groups, suggesting that skilled blue collar and white collar employees of parity firms receive significantly lower wages. For low-qualified employees, the coefficient on *Parity* is not significant, even though the size of the coefficient is similar. This group of employees has larger standard errors, probably because roughly one-fourth belongs to skilled blue-collar or white-collar workers (see Table 8, Panel C).

[31] To see how well the qualification categories correspond to the occupational categories for the regression purpose, we repeat the employment regressions using the breakdown by educational and vocational qualifications and report the estimation results in Table A-7 in the Online Appendix. The results are qualitatively similar to those based on occupational status in Table 7. The statistical significance of the coefficient on *Shock x Parity* is lower for highly-qualified and qualified workers because of the imperfect correlation between occupational status and qualification.

3.2.3 Identification Issues

Placebo tests. As in the employment analysis, we reestimate the wage results while replacing the parity dummy with placebo dummies. The results are reported in Table A–8 of the Online Appendix, separately for parity and non-parity firms. Coefficients on placebo dummies are statistically zero for all four regressions.

We also repeat placebo regressions for all employees with *Placebo* equal to one if employment exceeds a threshold that varies from 1,500 to 2,500 in increments of 50. Figure 7 shows the regression coefficients on *Placebo* (right axis, gray line) and the t-statistic (left axis, black line). The lowest values are reached around the 2,000 threshold, supporting the conclusion that the relevance of this threshold is genuine.[32]

Regression discontinuity analysis. We also test whether our conclusions regarding wages hold locally around the 2,000-employee cut-off with regression discontinuity analyses. We follow the same steps as in the employment analysis. Unlike the employment analysis, however, we do not see a drop in sample size because the estimation is done for the entire cycle of shock and non-shock periods. Results are reported in Table 10, with Figure 8 plotting local polynomial regressions around the 2,000 threshold with 95% confidence intervals in dashed lines. The drop in wages above the 2,000 threshold is significant for the optimal bandwidth, implying that parity firms' employees are paid less than those at non-parity firms. The estimate for the insurance premium at the optimal bandwidth is about 8%, much larger than those estimated via the difference in differences approach, because the RD analyses do not contain the control variables used in the difference-in-differences approach.

Alternative sample constructions. As with the employment regressions, we reestimate our baseline regressions while (1) lowering the threshold of the minimum number of employees in an establishment for inclusion in our sample to 10 employees and (2) truncating the sample at the 1st

[32] The figure does not show a sharp drop at 2,000 because we gradually "misclassify" more and more establishments as we move the placebo threshold further away from the true threshold.

and 99th percentile of employment growth. The reestimation results reported in Panels C and D of Table A–6 are robust.

4. Operating Risks and Firm Performance

In this section, we examine how the employment insurance affects firm risk and performance over the cycle of non-shock and shock periods. These tests require regression estimation at the firm level, so we redefine our shock measure as *FirmShock*, the proportion of a firm's employees working in establishments in industries for which *Shock = 1. FirmShock* is a weighted average of *Shock* in a given firm-year, ranging between 0 and 1. For example, if 60% of a firm's employees work in industries in which *Shock* equals 1, and the remaining 40% work in industries not subject to a shock in that year, then *FirmShock* equals 0.6.

4.1 Operating Leverage

Providing job guarantees limits firms' ability to reduce payroll in response to changes in technology, consumer taste, or general business conditions. Then wages become more fixed costs rather than variable costs, increasing operating leverage. To test this prediction, we relate *Parity* to two different measures of operating leverage. First, we follow Mandelker and Rhee, 1984 and Chen, et al., 2011 and use the elasticity of a firm's operating income (after depreciation) with respect to sales, the Mandelker and Rhee degree of operating leverage (MRDOL). We estimate it for firms with positive EBIT using a time-series regression of log (EBIT) on log (Sales) using the five most recent annual observations. Second, we use the Novy-Marx, 2011 operating leverage measure, which is cost of goods sold plus selling, general and administrative expenses, scaled by the book value of assets (NMOL).

For each measure of operating leverage, we use two different specifications, one with year- and industry fixed effects; the other with year- and firm fixed effects. The control variables are similar to Chen, et al., 2011. Results are reported in Table 11, Panel A without control variables. The coefficient on *Parity* is positive and statistically significant, regardless of the specification. It appears employment insurance through parity codetermination increases operating leverage.

The higher operating leverage implies that parity firms will suffer greater relative reductions in profitability and valuation from industry shocks than non-parity firms. To test these implications, we estimate difference-in-differences in ROA and the logarithm of Tobin's Q, our measures of profitability and valuation, respectively. ROA is defined as EBITDA/Total Assets to avoid changes in depreciation and amortization methods during our sample period affecting estimation results. Tobin's Q is defined as the market value of common equity plus total assets minus the book value of common equity divided by total assets. Our main interest is again the coefficient of *FirmShock* × *Parity*, which we expect to be negative for *ROA* and *Q*.

The estimation results are reported in Table 11, Panel B, Columns (1) and (3). The coefficient on *FirmShock* × *Parity* is significant and negative in both specifications. The economic magnitude is also large. The estimates for ROA show that profitability of parity-codetermined firms falls by 3.6% more if all employees of a firm are affected by a shock. This number is substantial; Table 1 shows that the mean (median) ROA of all firm-year observations in the sample is 7.5% (6.9%) and the coefficient on *FirmShock* for non-parity firms is only –3.0%. The incremental decline in Tobin's Q for parity firms is 9.2% if all employees are affected by a shock, which is larger than the decline of 7.5% for non-parity firms. These estimates for ROA and Tobin's Q suggest that parity codetermination more than doubles the negative impact of shocks on profitability and valuation relative to non-parity firms.

4.2 Are Shareholders Properly Compensated for Bearing the Risk?

These results imply that the employment insurance increases shareholder risk—the risk of getting hurt more during industry downturns. Is the insurance premium in the form of lower wages sufficient to compensate shareholders for the risk? More generally, are shareholders worse or better off as the result of parity codetermination? We investigate this issue by reestimating the *ROA* and *Q* regressions while omitting *FirmShock* and *FirmShock* x *Parity* and report the re-estimated coefficients of *Parity* in Columns (2) and (4) of Table 11, Panel B.

The coefficient on *Parity* is insignificant for both *ROA* and *Q*, implying operating profits and shareholder value through the cycle are unrelated to parity codetermination. This conclusion is in line with previous findings in the literature on German codetermination.[33] It appears workers capture all the efficiency gains from the improved risk-sharing arrangement through the implicit insurance contract with firms; however, they do pay an insurance premium that is on average sufficient to cover the risks of providing the insurance. Put differently, the wage concessions workers make to firms seem to be just sufficient to compensate shareholders for higher operating leverage.

5. Conclusion

We find parity-codetermined firms provide skilled employees greater protection against layoffs during adverse industry shocks. There are two alternative explanations. According to the insurance hypothesis, parity-codetermination serves as an enforcement mechanism to ensure firms honor implicit insurance contracts, whereby workers receive protection against adverse shocks in return for accepting lower wages. The alternative explanation, employee entrenchment, suggests the worker control rights bestowed by parity-codetermination allow employees to prevent layoffs when the industry suffers adverse shocks, but without offering adequate concessions in the form of lower wages.

We distinguish the two competing explanations by examining the wage differential between parity and non-parity firms and by analyzing firm valuation and performance. We find employees of parity firms receive significantly lower wages relative to those working for non-parity firms. Our firm performance analyses indicate that shareholders do not gain or lose from improved risk sharing and workers reap all the benefits from this arrangement. Both findings favor the insurance hypothesis. However, only skilled blue-collar and white-collar workers benefit from employment insurance,

[33] Of the four studies surveyed by Renaud (2007) that use either Tobin's Q or the market-to-book ratio, two find negative effects and the other two find no effect of worker representation. In a study not covered by Renaud (2007), Petry (2018) performs an event study around the transition dates when firms announce changes in their parity status and finds a negative impact of 2.3% of changing from non-parity to parity status. This seems to contradict Baums and Frick (1999), who also conduct an event study based on 28 court cases that addressed the power of unions and workers on codetermined supervisory boards and find no impact on firm value.

whereas unskilled blue-collar workers do not. We attribute this finding to the lack of real representation of unskilled blue-collar workers on supervisory boards.

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Figure 1: World map of labor representation

This figure plots labor representation in works councils and corporate boards around the world. We categorize countries in three groups (1) no labor representation (light gray), (2) only work councils (gray), and (3) work councils and board representation (dark gray). This classification is based on variables 30 and 31 in the CBR-LRI data set provided by the Centre for Business Research at the University of Cambridge, UK. (See Adams, et al., 2017 andhttps://www.repository.cam.ac.uk/handle/1810/262929 for a documentation.) For each country we use the latest information available (for most countries that is 2015). The figure does not display more granular information about the power of work councils and the degree of board representation available in the data set.

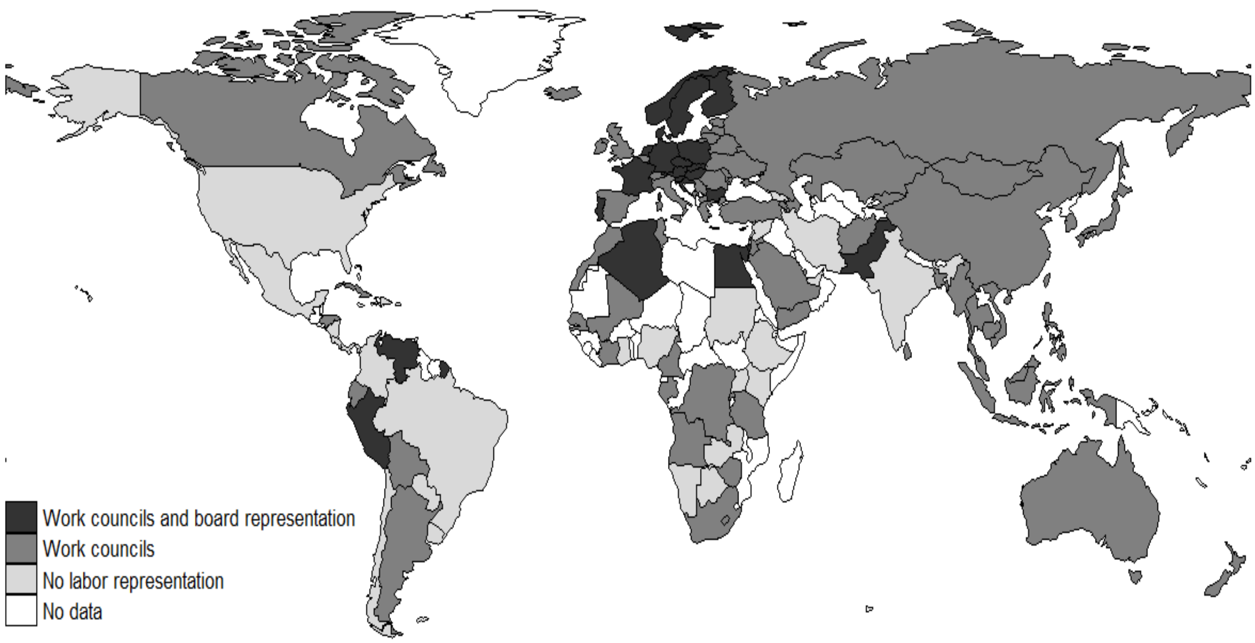

Figure 2: Pre- and post-shock trends in employment changes of establishments of parity and non-parity firms

This figure plots trends in average log changes of employment for establishments of parity (black line) and non-parity firms (gray line) in event time. Time = 0 is the first year of an industry shock. We require Shock = 0 for all years before Time = 0. Only establishments with more than 50 employees and data available for the complete event period are included. Establishments switching parity status during the event period are excluded.

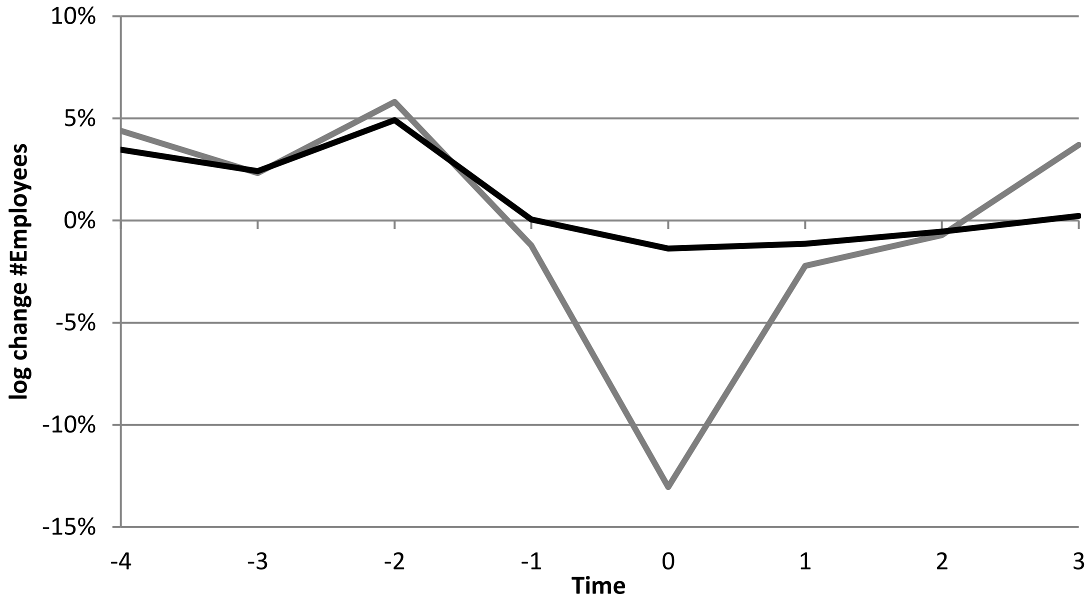

Figure 3: Employment – placebo tests

This figure shows the coefficients on the interaction term *Placebo x Shock* (right-hand axis, gray line) and the corresponding t-statistics (left-hand axis, black line) from estimating Column (2) in Table 3 with *Placebo* instead of *Parity*. We vary the placebo threshold in steps of 50 from 1,500 to 2,500 and let *Placebo* be equal to one for any establishment of a firm with employment in Germany above the corresponding placebo threshold.

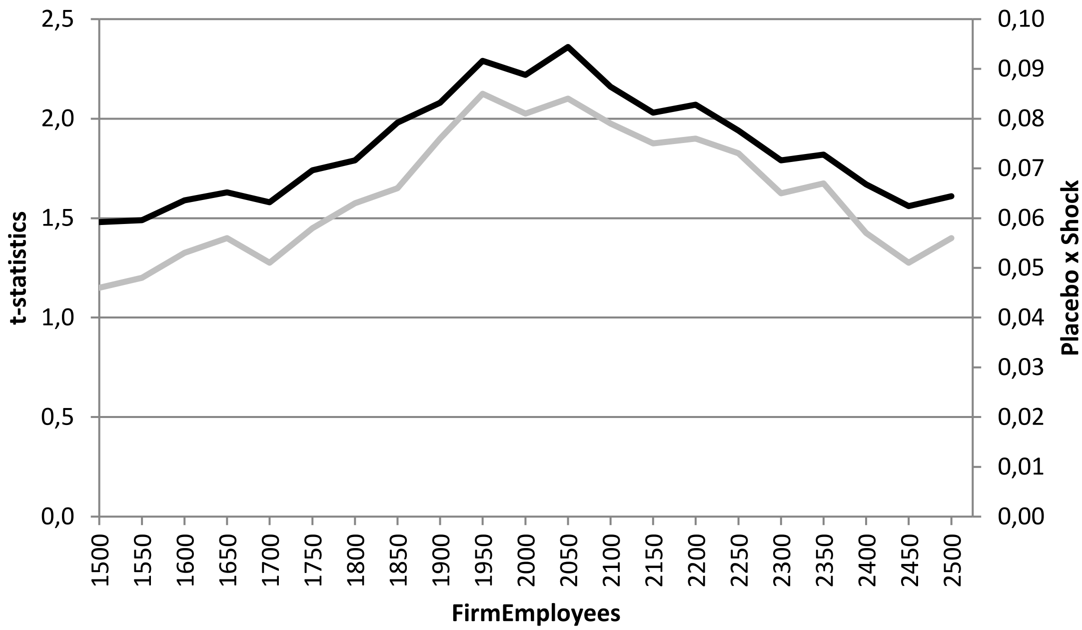

Figure 4: Distribution of firms by number of employees (density plot)

The figure shows the histogram and estimated density of all firm-year observations for which the number of employees in Germany is between 0 and 4,000 with breakpoint at 2,000 employees. The density and the confidence band are estimated using the McCrary, 2008 estimator using the default bin size of 59 and bandwidth of 904.

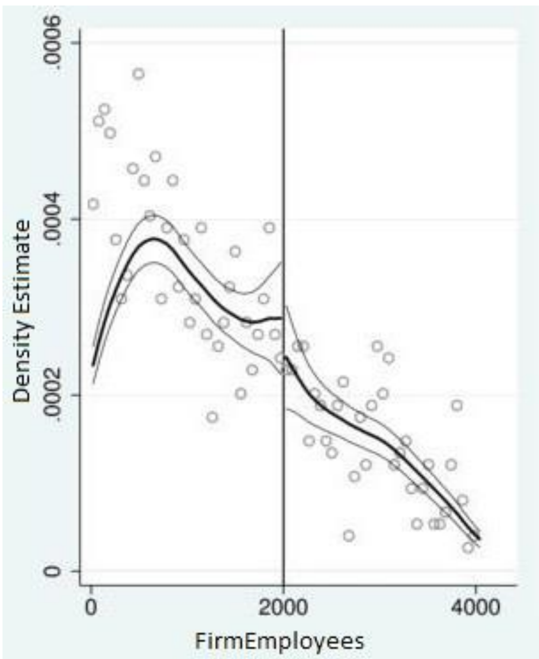

Figure 5: Employment growth around the 2,000-employee cutoff

This figure shows the result of a local polynomial regression with local-mean smoothing (and 95% confidence band) of firm employment growth on the number of firm employees between 1,500 and 2,500 in Germany. Epanechnikov kernel with bandwidth 50 are used.

Figure 6: Discontinuity at the 2,000-employee cutoff: Employment

This figure shows the results of two local polynomial regressions for log change in employment during shock periods around the parity cutoff of 2,000 employees.

Figure 7: Wages – placebo tests

This figure shows the coefficients on *Placebo* (right-hand axis, gray line) and the corresponding t-statistics (left-hand axis, black line) from estimating Column (2) in Table 9 with *Placebo* instead of *Parity*. We vary the placebo threshold in steps of 50 from 1,500 to 2,500 and let *Placebo* be equal to one for any establishment of a firm with employment in Germany above the corresponding placebo threshold.

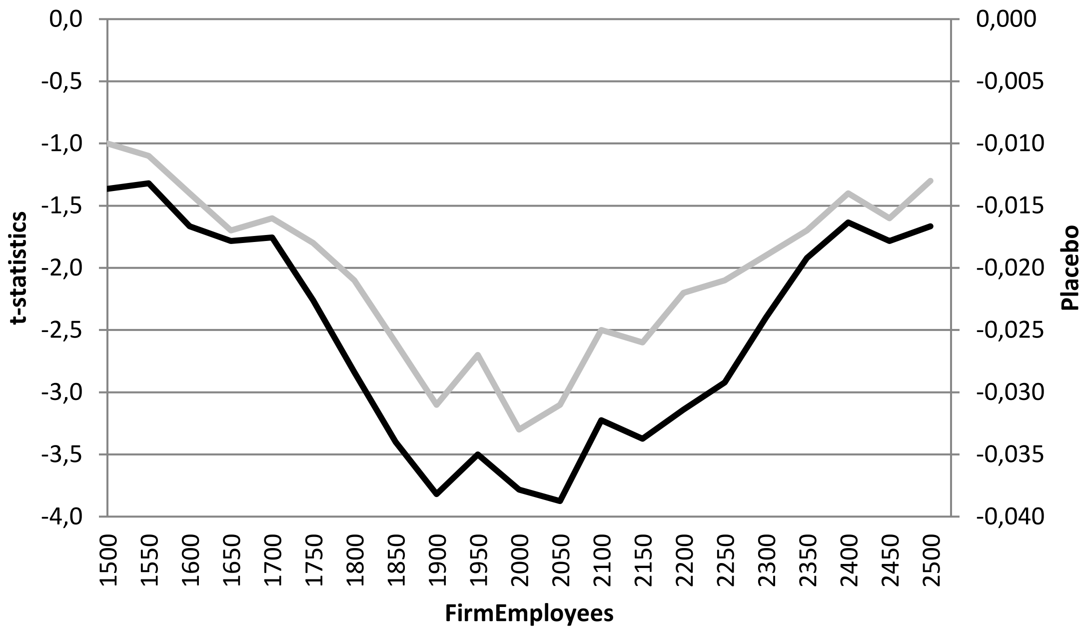

Figure 8: Discontinuity at the 2,000-employee cutoff: Wages

This figure shows the results of two local polynomial regressions for log median daily wage around the parity cutoff of 2,000 employees.

Table 1: Descriptive statistics

This table presents descriptive statistics for all key variables used in this paper. Panel A reports summary statistics at the establishment level. N is the number of establishment-years the respective variable is available. Only establishments with more than 50 employees are used. *DailyWageP50LQ* is the median daily gross wage for low-qualified employees. *DailyWageP50Q* is the median daily gross wage for qualified employees. *DailyWageP50HQ* is the median daily gross wage for highly qualified employees. Monetary units are normalized to 2005 euros. Panel B reports summary statistics at the firm level. N is the number of firm-years the respective variable is available.

Panel A

Variable	Mean	Median	Std	Min	P25	P75	Max	N
#Employees	517.47	148	2099.29	51	81	346	61,380	54,042
#Unskilled	97.14	5	700.35	0	0	31	32,733	54,042
#Skilled	103.32	10	584.98	0	0	49	19,658	54,042
#WhiteCollar	223.80	64	894.00	0	31	148	29,084	54,042
DailyWageP25	81.73	76.66	27.98	1.02	61.20	97.99	214.42	53,956
DailyWageP50	94.23	88.38	32.59	7.66	69.56	113.53	228.92	53,956
DailyWageP75	108.76	104.68	34.87	7.66	81.01	132.69	228.92	53,956
DailyWageP50LQ	82.50	77.52	29.07	1.87	61.99	99.04	781.59	44,783
DailyWageP50Q	93.11	88.53	30.25	7.66	70.37	110.98	199.33	53,811
DailyWageP50HQ	124.56	126.03	34.84	0.60	99.96	150.47	335.52	40,459
EstAge	15.64	16	9.880	0	6	24	33	54,042
MedianEmplAge	38.84	39	4.973	17	36	42	60	54,042
RatioWhiteCollar	0.477	0.446	0.297	0	0.228	0.746	1	54,042
Shock	0.058	0	0.233	0	0	0	1	52,756
HHI	0.405	0.304	0.314	0.007	0.145	0.563	1	54,042

Panel B

Variable	Mean	Median	Std	Min	P25	P75	Max	N
Beta	0.678	0.620	0.467	–3.198	0.324	0.997	3.002	1,832
FirmAge	84.5	86	53.3	0	36	124	259	1,989
Leverage	0.392	0.358	0.273	0	0.169	0.582	0.996	2,052
MCap (bn €)	5.382	0.976	12.327	0.003	0.297	4.141	198.186	2,002
NetPPE (bn €)	2.451	0.288	7.337	0	0.092	1.349	74.003	2,057
Parity	0.674	1	0.469	0	0	1	1	2,168
ROA	0.074	0.069	0.096	–1.152	0.031	0.110	0.671	1,926
Sales (bn €)	8.663	1.793	17.950	0.005	0.591	7.694	157.5	2,064
TobinsQ	1.546	1.224	1.010	0.454	1.054	1.602	12.529	1,991
FirmShock	0.048	0	0.177	0	0	0	1	2,126
#Establishments	26.045	6	56.827	1	3	20	497	2,075

Table 2: Variable definitions

This table defines all variables used in this paper. Board data are taken from *Hoppenstedt company profiles* and annual reports. Employment and wage data are from the *IAB Establishment History Panel*. Accounting data is taken from *Worldscope* and market data from *Datastream*. The numbers in brackets refer to *Worldscope* items, taken from the *Worldscope Data Definition Guide*.

Variable	**Description**	**Source**
#Employees	Total number of employees in the establishment	IAB
#Skilled	Number of skilled (blue-collar) employees (at least vocational training)	IAB
#Unskilled	Number of unskilled (blue-collar) employees (no formal qualification)	IAB
#WhiteCollar	Number of white-collar employees (at least vocational training)	IAB
Beta	CAPM beta estimated over the prior calendar year using daily returns	Datastream
EstAge	Age of the establishment in year	IAB
FirmEmployees	Sum of all employees across all establishments of the firm in Germany	IAB
FirmAge	Age of the firm in year	Worldscope
HHI	Herfindahl-Hirschman Index calculated using #Employees of all establishments with more than 50 employees in the same county and industry (3-digit NACE code).	IAB
Leverage	= Total debt [03255] / (total debt + common equity [03501])	Worldscope
MCap	Market capitalization [08001]	Worldscope
MedianEmplAge	Median age of all employees in the establishment	IAB
NetPPE	Net property, plant and equipment [02501]	Worldscope
Parity	= 1 if 50% of all members of the company's supervisory board are classified as employee representatives	Hoppenstedt, annual reports
PPE Dummy	=1 if NetPPE [02501] declines by more than 15%	Worldscope
RatioWhiteCollar	= #WhiteCollar / #Employees	IAB
ROA	= $EBITDA_t$ [18198] / ((total $assets_t$ [02999] + total $assets_{t-1}$)/2)	Worldscope
Sales	= Net sales or revenues [01001] in 2005 Euros	Worldscope
Shock	= 1 if employment in non-sample establishments in the same industry (3-digit NACE-code) as the establishment decreases by more than 5% with no increase in employment in the following year. A detailed description of the definition is provided in Section 2.3.2.	IAB
DailyWageP25	1^{st} quartile of gross average daily wage for all full-time employees in 2005 Euros	IAB
DailyWageP50	Median of gross average daily wage for all full-time employees in 2005 Euros	IAB
DailyWageP75	3^{rd} quartile of gross average daily wage for all full-time employees in 2005 Euros	IAB
TobinsQ	= (market capitalization [08001] + total assets [02999] – common equity [03501]) / total assets	Worldscope

Table 3: Employment changes – all employees

This table presents OLS estimation results with the log change in employment as the dependent variable. Only establishments with more than 50 employees are included. Column (4) includes six additional variables not shown on the table: *LogFirmEmployees × Parity*, *LogFirmEmployees × (1 – Parity)*, *LogFirmEmployees*^{2} *× Parity*, *LogFirmEmployees*^{2} × *(1 – Parity)*, *LogFirmEmployees*^{3} × *Parity*, and *LogFirmEmployees*^{3} × *(1 – Parity)*. T-statistics are reported in parentheses. Standard errors are clustered at the firm level. The table also reports the p-value for the t-test that *Shock + Shock × Parity = 0*, except in Column (3), which includes interaction terms of *Shock* with *LogFirmEmployees* and *LogFirmEmployees*^{2}.

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.082	0.081	0.064	0.083
	(2.37)	*(2.22)*	*(1.80)*	*(2.46)*
Shock × LogFirmEmployees			0.264	
			(1.86)	
Shock × LogFirmEmployees^2			–0.013	
			(–1.79)	
Shock	–0.096	–0.095	–1.323	–0.097
	(–3.30)	*(–3.05)*	*(–2.05)*	*(–3.44)*
Parity	–0.044	–0.042	–0.039	8.651
	(–1.39)	*(–1.39)*	*(–1.27)*	*(3.34)*
LogEstAge	–0.594	–0.594	–0.594	–0.594
	(–10.8)	*(–10.7)*	*(–10.8)*	*(–10.8)*
LogSales	0.005	–0.012	–0.014	0.006
	(0.22)	*(–0.08)*	*(–0.10)*	*(0.30)*
Leverage	–0.016	–0.018	–0.020	–0.017
	(–0.34)	*(–0.44)*	*(–0.48)*	*(–0.36)*
LogFirmEmployees	0.054	0.147	0.137	
	(1.46)	*(0.81)*	*(0.75)*	
LogSales^2		0.000	0.000	
		(0.10)	*(0.13)*	
LogFirmEmployees^2		–0.005	–0.005	
		(–0.47)	*(–0.42)*	
adj. R^2	0.174	0.174	0.175	0.175
Observations	48,896	48,896	48,896	48,896
t-test: Shock × Parity + Shock = 0	0.502	0.508		0.507
Year F.E.	Yes	Yes	Yes	Yes
Add. Employment Polynomials	No	No	No	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table 4: Employment change - Regression discontinuity analysis

This table presents results for a kernel regression using a triangular kernel and the optimized Imbens and Kalyanaraman, 2012 bandwidth (bw = 1). The dependent variable is log change in employment. We modify the optimized bandwidth by factors of 0.5 and 2 to check robustness. A sharp regression discontinuity design is assumed, where the treatment variable (Parity) changes from one to zero at 2,000 employees (firm level). The z-statistics for the coefficient estimates are reported in parentheses below the estimates.

Dependent variable	**Log change in employment**
Parity (bw = 1)	0.148
	(2.02)
Parity (bw = 0.5)	0.092
	(1.10)
Parity (bw = 2)	0.134
	(1.95)
Observations	314

Table 5: Employment changes - Continuous shock measure

This table presents OLS estimation results with (1) log employment as the dependent variable in Panel A, and (2) the log change in employment as the dependent variable in Panel B. Only establishments with more than 50 employees are included. Panel A adapts the methodology of Sraer and Thesmar, 2007 (see their equation (4) and Table 7). We use the log of contemporaneous average employment of non-sample firms' establishments in the same industry (NACE, *LogMeanIndEmpl)* instead of our *Shock* definition. Panel B adapts the methodology of Ellul, et al., 2017 (see their Table 2). We use the contemporaneous log change in employment of non-sample firms' establishments in the same industry (*LogChangeIndEmpl)* instead of our *Shock* definition. T-statistics are reported in parentheses. Standard errors are clustered at the firm level.

Panel A

Dependent variable	**log number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
LogMeanIndEmpl	0.492	0.480	0.286	0.359
	(5.68)	*(5.90)*	*(3.00)*	*(3.74)*
Parity × LogMeanIndEmpl	–0.172	–0.172	–0.141	–0.209
	(–2.12)	*(–2.15)*	*(–1.94)*	*(–2.51)*
Parity	0.620	0.623	0.527	0.645
	(1.09)	*(1.11)*	*(0.91)*	*(1.12)*
LogEstAge		–0.020	0.112	0.113
		(–0.57)	*(3.58)*	*(3.70)*
adj. R^2	0.909	0.909	0.912	0.912
Observations	54,042	54,042	54,042	54,042
t-test: LogMIE × Parity + LogMIE = 0	0.036	0.037	0.295	0.262
Establishment F.E.	Yes	Yes	Yes	Yes
Year F.E.	No	No	Yes	No
Year × Parity F.E.	No	No	No	Yes

Panel B

Dependent variable	**log change in number of employees**			
	(1)	(2)	(3)	(4)
LogChangeIndEmpl	0.740	0.598	0.424	0.453
	(3.03)	*(2.52)*	*(2.00)*	*(2.91)*
Parity × LogChangeIndEmpl	–0.681	–0.522	–0.271	–0.295
	(–2.38)	*(–2.04)*	*(–2.04)*	*(–2.71)*
Parity	–0.051	0.070	–0.001	0.406
	(–4.06)	*(1.53)*	*(–0.06)*	*(7.82)*
LogEstAge		–0.344	–0.596	–0.596
		(–9.50)	*(–11.51)*	*(–11.34)*
adj. R^2	0.084	0.153	0.179	0.180
Observations	50,134	50,134	50,134	50,134
t-test: LogCIE × Parity + LogCIE = 0	0.549	0.441	0.151	0.147
Establishment F.E.	Yes	Yes	Yes	Yes
Year F.E.	No	No	Yes	No
Year × Parity F.E.	No	No	No	Yes

Table 6: Employment changes and local labor market conditions

This table repeats the analysis of Table 3 with additional interaction terms using *Herfindahl*. *Herfindahl* is a dummy variable equal to one if the employment *HHI* in the establishment's industry-county is above the sample median. The employment *HHI* is calculated using all establishments with more than 50 employees in the same county and industry (3-digit NACE code). The controls are not reported to preserve space. T-statistics are reported in parentheses. Standard errors are clustered at the firm level.

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.052	0.051	0.045	0.048
	(1.95)	*(1.88)*	*(1.79)*	*(1.94)*
Shock × LogFirmEmployees			0.245	
			(1.69)	
Shock × $\text{LogFirmEmployees}^2$			–0.012	
			(–1.60)	
Shock	–0.170	–0.168	–1.338	–0.166
	(–2.57)	*(–2.50)*	*(–2.98)*	*(–2.56)*
Herfindahl	0.076	0.075	0.075	0.075
	(2.91)	*(2.94)*	*(2.91)*	*(2.91)*
Parity × Herfindahl	–0.001	–0.001	–0.001	–0.002
	(–0.19)	*(–0.19)*	*(–0.20)*	*(–0.22)*
Shock × Herfindahl	0.103	0.102	0.114	0.099
	(1.39)	*(1.37)*	*(1.60)*	*(1.35)*
Parity × Shock × Herfindahl	0.047	0.047	0.060	0.044
	(1.72)	*(1.71)*	*(1.98)*	*(1.68)*
Parity	–0.040	–0.039	–0.035	8.626
	(–1.30)	*(–1.30)*	*(–1.17)*	*(3.36)*
adj. R^2	0.175	0.175	0.175	0.175
Observations	48,896	48,896	48,896	48,896
Year F.E.	Yes	Yes	Yes	Yes
Add. Employment Polynomials	No	No	No	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table 7: Employment change – white-collar, skilled blue-collar, and unskilled blue-collar employees

This table presents OLS estimation results with the log change in the number of white-collar, skilled blue-collar, or unskilled blue-collar employees as the dependent variable. Only establishments with more than 50 employees are included. All regressions control for year and establishment fixed effects. T-statistics are reported in parentheses. Standard errors are clustered at the firm level. The table also reports the p-value for the t-test that *Shock + Shock × Parity = 0.*

Dependent variable	**White collar employees**		**Skilled blue collar employees**		**Unskilled blue collar employees**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Shock × Parity	0.076	0.076	0.137	0.133	0.005	0.006
	(2.23)	*(2.14)*	*(2.79)*	*(2.76)*	*(0.12)*	*(0.14)*
Shock	–0.082	–0.082	–0.112	–0.108	–0.063	–0.064
	(–2.63)	*(–2.50)*	*(–2.63)*	*(–2.57)*	*(–2.19)*	*(–2.29)*
Parity	–0.051	–0.050	–0.108	–0.104	0.030	0.029
	(–1.57)	*(–1.59)*	*(–2.41)*	*(–2.92)*	*(1.06)*	*(1.01)*
LogEstAge	–0.633	–0.633	–0.467	–0.467	–0.503	–0.503
	(–10.79)	*(–10.83)*	*(–8.07)*	*(–8.08)*	*(–12.95)*	*(–12.97)*
LogSales	0.021	–0.005	0.023	–0.019	–0.001	–0.018
	(0.85)	*(–0.03)*	*(1.15)*	*(–0.14)*	*(–0.06)*	*(–0.15)*
Leverage	0.077	0.074	–0.035	–0.041	–0.043	–0.043
	(0.80)	*(0.82)*	*(–0.76)*	*(–0.88)*	*(–1.94)*	*(–1.80)*
LogFirmEmployees	0.062	0.154	0.088	0.355	0.087	0.016
	(1.37)	*(0.81)*	*(2.44)*	*(2.51)*	*(2.15)*	*(0.08)*
$LogSales^2$		0.001		0.001		0.000
		(0.13)		*(0.31)*		*(0.13)*
$LogFirmEmployees^2$		–0.005		–0.014		0.004
		(–0.43)		*(–1.77)*		*(0.33)*
adj. R^2	0.171	0.171	0.064	0.065	0.076	0.076
Observations	48,896	48,896	48,896	48,896	48,896	48,896
t-test: Shock × Parity + Shock = 0	0.709	0.719	0.406	0.397	0.098	0.091

Table 8: Employee representatives by occupational status and qualification

Panel A reports the occupational status, and Panel B the educational and vocational qualification of labor representatives on supervisory boards. We hand collected this information for all sample firms existing in 1990, 1999, and 2008, the beginning, the middle, and the end of our sample period. The tabulation is based on 97, 231, and 229 labor representatives of 15, 35, and 48 sample firms, respectively, which report the relevant personal information in annual reports. To follow the structure of the IAB data, we categorized labor representatives in Panel A into (1) unskilled blue collar, (2) skilled blue collar, (3) white collar, and (4) union representatives. The occupational status of union representatives is usually not reported, but their occupational status tends to be white collar. Panel B categorizes labor representatives into (1) low-qualified, (2) qualified, and (3) highly qualified. In Panel B, union representatives are excluded because their qualification is usually not reported. Panel C presents how the classification based on occupational status corresponds to the breakdown by educational and vocational qualification. It is based on a random sample of 2% of all employees covered by the IAB database between 1975 and 2008 ("Sample of Integrated Labour Market Biographies").

Panel A

Occupational status	1990	1999	2008
Unskilled blue collar	0.0%	0.0%	0.0%
Skilled blue collar	42.3%	22.5%	22.3%
White collar	36.1%	54.5%	56.3%
Union representative	21.6%	22.9%	21.4%
Sum	100.0%	100.0%	100.0%
Number of labor rep's	97	231	229
Number of firms	15	35	48

Panel B

Qualification	1990	1999	2008
Low-qualified	0.0%	0.0%	0.0%
Qualified	86.8%	92.1%	59.4%
Highly qualified	13.2%	7.9%	40.6%
Sum	100.0%	100.0%	100.0%
Number of labor rep's	76	178	180
Number of firms	15	35	48

Panel C

	Unskilled blue collar	Skilled blue collar	White collar	Sum
Low-qualified	15.5%	2.2%	2.5%	20.2%
Qualified	9.8%	25.6%	36.6%	72.0%
Highly qualified	0.1%	0.1%	7.7%	7.9%
Sum	25.4%	27.9%	46.8%	100.0%

Table 9: Wages – all, highly-qualified, qualified, and low-qualified employees

This table presents OLS estimation results with median wages of all, highly qualified, qualified, or low-qualified employees as the dependent variable. The wage variables are defined as the log of median gross average daily wage for (1) all full-time employees, (2) with higher educational qualifications, (3) with educational/vocational qualifications, (4) without educational/vocational qualifications. Only establishments with more than 50 employees are included. All regressions control for year and establishment fixed effects. The t-statistics are reported in parentheses. Standard errors are clustered at the firm level.

Dependent variable	**All employees**		**Highly-qualified**		**Qualified**		**Low-qualified**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Parity	–0.031	–0.033	–0.031	–0.031	–0.029	–0.032	–0.033	–0.034
	(–3.10)	*(–3.78)*	*(–2.49)*	*(–2.56)*	*(–3.01)*	*(–3.70)*	*(–1.59)*	*(–1.66)*
LogEstAge	0.049	0.049	0.059	0.060	0.050	0.051	0.031	0.030
	(3.55)	*(3.42)*	*(6.18)*	*(6.06)*	*(3.61)*	*(3.49)*	*(1.90)*	*(1.79)*
LogSales	–0.212	–0.194	–0.022	–0.018	–0.244	–0.229	–0.058	–0.046
	(–2.27)	*(–2.37)*	*(–0.37)*	*(–0.32)*	*(–2.70)*	*(–2.85)*	*(–0.60)*	*(–0.52)*
Leverage	–0.024	–0.023	0.005	0.006	–0.016	–0.015	–0.075	–0.073
	(–0.93)	*(–0.90)*	*(0.23)*	*(0.29)*	*(–0.65)*	*(–0.64)*	*(–2.88)*	*(–2.85)*
LogFirmEmployees	0.032	0.067	–0.062	–0.053	0.039	0.065	0.016	0.023
	(0.34)	*(0.77)*	*(–1.04)*	*(–0.89)*	*(0.44)*	*(0.78)*	*(0.14)*	*(0.22)*
$LogSales^2$	0.006	0.005	0.001	0.001	0.006	0.006	0.002	0.002
	(2.49)	*(2.61)*	*(0.73)*	*(0.68)*	*(2.83)*	*(3.00)*	*(0.90)*	*(0.85)*
$LogFirmEmployees^2$	–0.003	–0.004	0.004	0.003	–0.003	–0.003	–0.002	–0.002
	(–0.48)	*(–0.73)*	*(0.94)*	*(0.88)*	*(–0.49)*	*(–0.64)*	*(–0.27)*	*(–0.28)*
Log#Employees		–0.033		–0.009		–0.032		–0.015
		(–4.04)		*(–1.58)*		*(–3.80)*		*(–1.84)*
LogMedianEmplAge		0.185		0.071		0.194		0.206
		(3.98)		*(2.45)*		*(5.65)*		*(4.86)*
RatioWhiteCollar		0.153		0.022		0.072		0.047
		(2.89)		*(1.03)*		*(1.59)*		*(0.75)*
adj. R^2	0.942	0.945	0.825	0.826	0.926	0.929	0.800	0.801
Observations	51,205	51,205	38,670	38,670	51,060	51,060	42,336	42,336

Table 10: Wages - Regression discontinuity analysis

This table presents results for a kernel regression using a triangular kernel and the optimized Imbens and Kalyanaraman, 2012 bandwidth (bw = 1). The dependent variable is log median daily wage. We modify the optimized bandwidth by factors of 0.5 and 2 to check robustness. A sharp regression discontinuity design is assumed, where the treatment variable (Parity) changes from zero to one at 2,000 employees (firm level). The z-statistics for the coefficient estimates are reported in parentheses below the estimates.

Dependent variable	**Log median daily wage**
Parity (bw = 1)	–0.079
	(–1.95)
Parity (bw = 0.5)	–0.148
	(–2.58)
Parity (bw = 2)	–0.039
	(–1.26)
Observations	4,962

Table 11: Firm-level regressions: Operating leverage, effects on *ROA* and *Q*

Panel A presents OLS estimation results with (1) the Mandelker and Rhee, 1984 measure of operating leverage (MRDOL) and (2) the Novy-Marx, 2011 measure of operating leverage (NMOL) as the dependent variable. Following Mandelker and Rhee, 1984 we estimate for firms with positive EBIT the degree of operating leverage using a time-series regression of log(EBIT) on log(Sales) using the five most recent annual observations. Novy-Marx, 2011 defines operating leverage as cost of goods sold plus selling, general and administrative expenses, scaled by the book value of assets. Control variables (LogAssets, LogTobinsQ, FixedAssets/Assets, Leverage, LogFirmAge, and FirmEmployees/Assets) are not reported to conserve space. The t-statistics are reported in parentheses. Standard errors are clustered at the firm level. Panel B presents OLS estimation results with (1) ROA and (2) log Tobin's q as the dependent variable. The *FirmShock* variable is defined as the weighted average of *Shock* across all establishments in a firm-year. All regressions control for year and firm fixed effects. The t-statistics are reported in parentheses. Standard errors are clustered at the firm level.

Panel A

Dependent variable	**MRDOL**		**NMOL**	
	(1)	**(2)**	**(3)**	**(4)**
Parity	0.177	0.135	0.297	0.127
	(2.16)	*(1.79)*	*(4.67)*	*(2.60)*
adj. R^2	0.146	0.241	0.736	0.913
Observations	1,442	1,442	953	953
Year F.E.	Yes	Yes	Yes	Yes
Industry F.E.	Yes	No	Yes	No
Firm F.E.	No	Yes	No	Yes

Panel B

Dependent variable	**ROA**		**LogTobinsQ**	
	(1)	**(2)**	**(3)**	**(4)**
FirmShock × Parity	–0.036		–0.092	
	(–2.59)		*(–1.80)*	
FirmShock	–0.030		–0.075	
	(–2.40)		*(–1.70)*	
Parity	0.003	–0.003	0.031	0.025
	(0.29)	*(–0.38)*	*(1.58)*	*(1.27)*
LogFirmAge	–0.005	–0.006	–0.037	–0.038
	(–0.75)	*(–0.83)*	*(–2.26)*	*(–2.32)*
LogSales	–0.029	–0.029	–0.747	–0.757
	(–0.65)	*(–0.67)*	*(–8.39)*	*(–8.53)*
Leverage	–0.056	–0.055	–0.249	–0.249
	(–5.15)	*(–5.06)*	*(–9.72)*	*(–9.72)*
LogFirmEmployees	–0.031	–0.030	0.277	0.280
	(–1.85)	*(–1.76)*	*(6.32)*	*(6.39)*
$LogSales^2$	0.001	0.001	0.018	0.018
	(1.36)	*(1.38)*	*(8.38)*	*(8.53)*
$LogFirmEmployees^2$	0.002	0.002	–0.019	–0.019
	(1.32)	*(1.25)*	*(–5.95)*	*(–6.03)*
adj. R^2	0.545	0.543	0.682	0.681

Observations	1,734	1,734	1,885	1,885

Online Appendix to:

Labor Representation in Governance as an Insurance Mechanism

Institutional Details

1. Labor Representation in Germany

The *Bundestag*, the lower house of the German parliament, passed the codetermination act on March 18, 1976, with only 22 votes against. However, several large corporations and the association of employers were dissatisfied and challenged the law in the German constitutional court, which decided in favor of the law in 1979. After the ruling the debate subsided. As witnessed by the fierce debate at the time of the passage of the codetermination laws in 1976, parity-codetermination was much more controversial and of a major concern to shareholders and managers than one-third representation.

The annual shareholder meeting elects the shareholder representatives on the supervisory board. All board members have one vote each in electing the chairman and the vice chairman of the board. If no member of the board receives two-thirds of the votes, the chairman is elected only by the shareholder representatives and the vice chairman by the employee representatives.

The law applies only to corporations required to have a supervisory board, i.e. companies registered as a stock corporation (*Aktiengesellschaft*), as a limited partnership (*Gesellschaft mit beschränkter Haftung*), or some hybrid forms that also have a supervisory board. Other legal forms such as general partnerships, which are not required to have a supervisory board, are not subject to codetermination. German law provides for extensive safeguards of codetermination if a German firm adopts the new form of a European stock corporation (Societas Europea, SE). So far, the large German corporations that have adopted the SE form (Allianz, BASF, and Fresenius) shrunk the supervisory board, but retained parity codetermination. See Gorton and Schmid, 2004 for other technical details on German codetermination and also for the slightly different arrangements in the coal and steel industry, which makes up only a small part of our sample.

Bank representation on supervisory boards and bank equity holdings in German non-financial firms are high at the beginning of our sample period, but decline to levels comparable to those found in the U.S. shortly after 2000, which is about the middle of our sample period (Dittmann, et al., 2010).

2. Collective Bargaining in Germany

Hassel, 1999 reports that in 1995, 53.4% of plants were covered by sectoral wage agreements, 8.2% by company agreements, and 38.4% were not covered at all. Addison, et al., 2010 show the number of employees covered by collective agreements declined during the 2000s. Although their sample may not be fully comparable to that of Hassel, 1999, Addison et al. show that 47.3% of German plants had sectoral agreements in 2000, a number that drops to 35.4% by 2008. Firm-level agreements were almost stable with 2.5% in 2000 and 2.7% in 2008, whereas the plants not covered by any collective bargaining agreement increased from 50.1% in 2000 to 61.9% in 2008. These data suggest significant variation in wages across firms, even though many firms may not be able to respond quickly to adverse shocks.

Figure A–1: Distribution of shocks

This figure presents results for OLS regressions with two different industry shock dummies based on two-year and four-year intervals as the dependent variable. The independent variables are year dummies, the constant is omitted. The plots show the regression coefficients of the year dummies and the annual German unemployment rate (based on the dependent civilian labor force). Data for 1990 is based solely on West Germany, because data for East Germany is only available starting 1991.

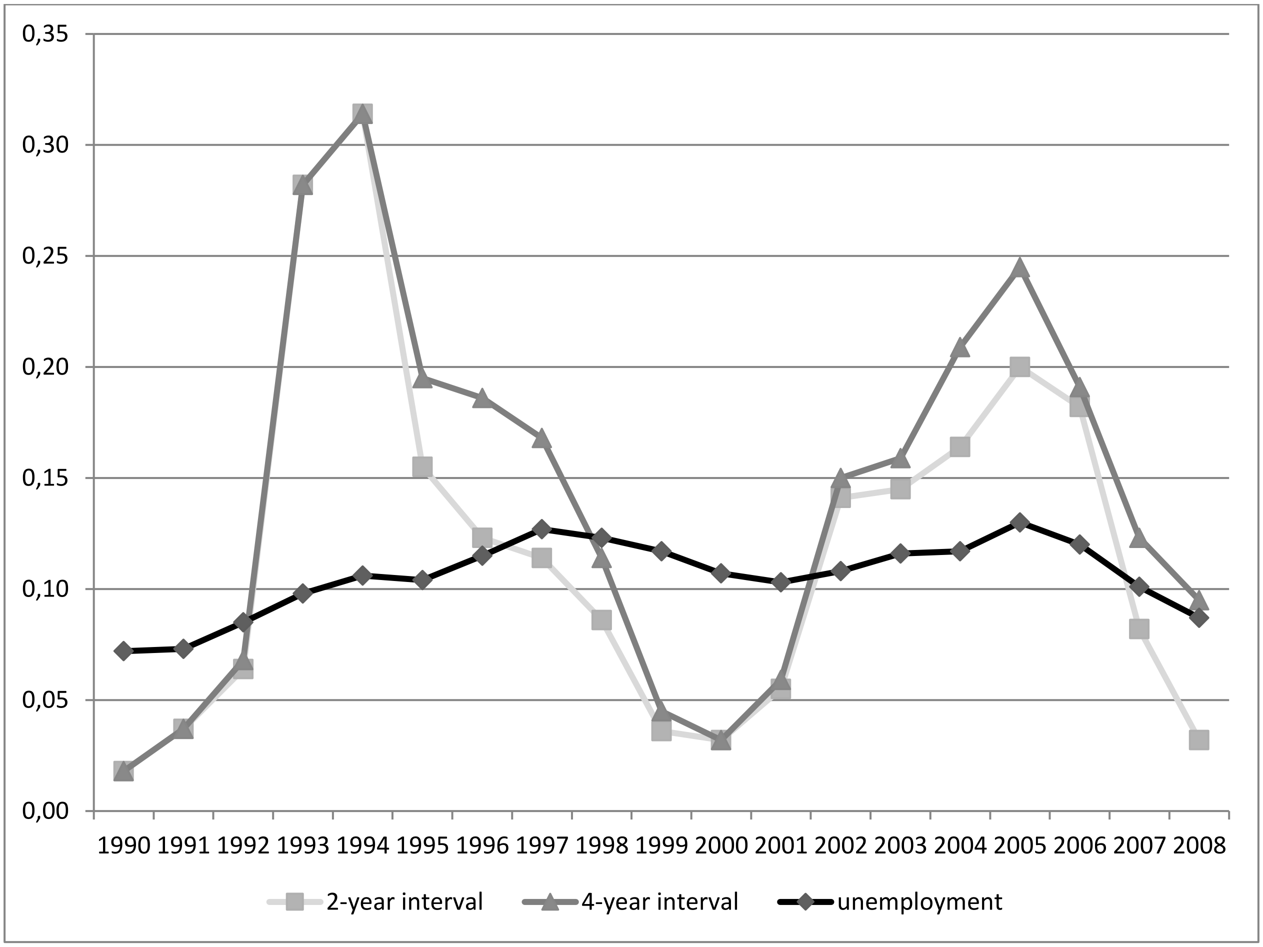

Figure A–2: Distribution of firms by number of employees (histogram)

The figure shows a histogram of the frequency distribution of all firm-year observations for which the number of employees in Germany is between 500 and 3,500. Bin width is set to 100.

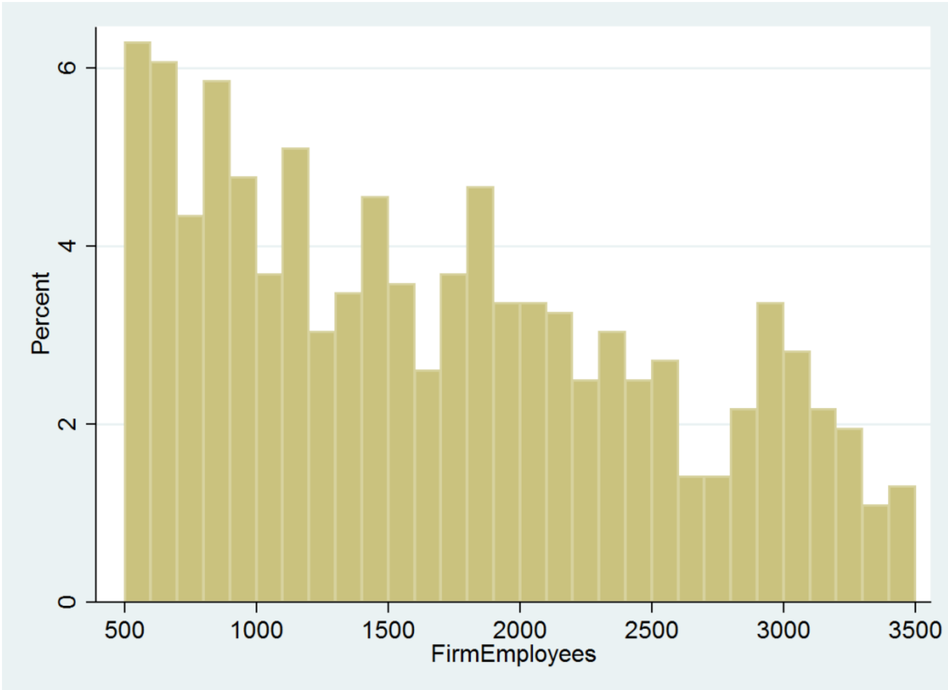

Table A–1: Definition of Shock

This table illustrates our definitions of Shock using four different sequences of employment growth. In case A Shock = 1 for years 1 and 2, and also for year 3, because there is no recovery in year 3. In case B there are no shock years because of the positive employment growth in year 2. In cases C and D, Shock = 1 for years 1, 2, 3, and 4, because there is no recovery before year 5. The table also shows the definition of the Shock dummy with a two-year interval.

	T	1	2	3	4	5
Case A	Employment growth	–6%	–2%	0%	+2%	–1%
	Shock (4-year interval)	1	1	1	0	0
	Shock (2-year interval)	1	1	0	0	0
Case B	Employment growth	–10%	+2%	0%	+2%	–1%
	Shock (4-year interval)	0	0	0	0	0
	Shock (2-year interval)	0	0	0	0	0
Case C	Employment growth	–10%	–2%	0%	–2%	–1%
	Shock (4-year interval)	1	1	1	1	0
	Shock (2-year interval)	1	1	0	0	0
Case D	Employment growth	–10%	–2%	0%	–5%	–1%
	Shock (4-year interval)	1	1	1	1	0
	Shock (2-year interval)	1	1	0	1	1

Table A–2: Placebo regressions: Employment change

This table repeats the analysis of Table 3 (models (1) and (2)) for two subsamples: parity and non-parity firms. The *Parity* dummy is replaced by a *Placebo* dummy, which is one for all parity (non-parity) establishments with above median *FirmEmployees* for the respective subsample and zero otherwise. The dependent variable is the log change in employment. For further details please see Table 3.

	log change in number of employees			
Subsamples	**Parity firms**		**Non-parity firms**	
	(1)	**(2)**	**(3)**	**(4)**
Shock × Placebo	–0.026	–0.027	0.013	–0.017
	(–0.68)	*(–0.70)*	*(0.19)*	*(–0.26)*
Shock	–0.091	–0.090	–0.112	–0.109
	(–3.21)	*(–3.14)*	*(–2.92)*	*(–2.93)*
Placebo	–0.021	–0.018	–0.002	0.016
	(–0.51)	*(–0.49)*	*(–0.03)*	*(0.28)*
LogEstAge	–0.586	–0.585	–0.752	–0.749
	(–9.77)	*(–9.80)*	*(–8.02)*	*(–7.52)*
LogSales	0.004	0.076	–0.020	–0.661
	(0.17)	*(0.47)*	*(–0.67)*	*(–1.65)*
Leverage	–0.021	–0.019	–0.045	–0.058
	(–0.49)	*(–0.48)*	*(–0.49)*	*(–0.67)*
LogFirmEmployees	0.072	0.131	0.134	0.455
	(2.16)	*(0.67)*	*(2.17)*	*(1.39)*
$LogSales^2$		–0.002		0.016
		(–0.41)		*(1.61)*
$LogFirmEmployees^2$		–0.003		–0.023
		(–0.30)		*(–1.04)*
adj. R^2	0.175	0.175	0.244	0.245
Observations	45,608	45,608	3,288	3,288
Year F.E.	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table A–3: Crossing different employment levels

This table reports the number of crossings for different employment levels in Germany from 1,500 to 2,500 employees.

	Crossings	
Threshold (#Employees)	from below	from above
1,500	28	14
1,600	30	14
1,700	28	14
1,800	29	16
1,900	30	17
2,000	28	17
2,100	28	15
2,200	26	14
2,300	24	13
2,400	27	16
2,500	27	14

Table A–4: Employment change – change in parity status

This table repeats the analysis of Table 3 (models (1) to (3)) excluding establishment-years of firms changing from parity to non-parity status if the establishment's industry is in industry shock the year of or the two years before the change. The dependent variable is the log change in employment. For further details please see Table 3.

Dependent variable	**log change in number of employees**		
	(1)	**(2)**	**(3)**
Shock × Parity	0.081	0.080	0.076
	(2.35)	*(2.19)*	*(2.01)*
Shock × LogFirmEmployees			0.260
			(1.82)
Shock × $LogFirmEmployees^2$			–0.013
			(–1.75)
Shock	–0.095	–0.094	–1.304
	(–3.24)	*(–2.98)*	*(–2.00)*
Parity	–0.040	–0.038	–0.036
	(–1.25)	*(–1.23)*	*(–1.15)*
LogEstAge	–0.595	–0.595	–0.595
	(–10.78)	*(–10.75)*	*(–10.75)*
LogSales	0.005	–0.013	–0.015
	(0.24)	*(–0.09)*	*(–0.10)*
Leverage	–0.015	–0.017	–0.019
	(–0.31)	*(–0.41)*	*(–0.45)*
LogFirmEmployees	0.054	0.152	0.141
	(1.46)	*(0.83)*	*(0.77)*
$LogSales^2$		0.000	0.000
		(0.11)	*(0.13)*
$LogFirmEmployees^2$		–0.005	–0.005
		(–0.48)	*(–0.44)*
adj. R^2	0.174	0.174	0.174
Observations	48,847	48,847	48,847
Year F.E.	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes

Table A–5: Employment change: Other robustness tests

This table reestimates the baseline regressions in Table 3 to test robustness to the following alternative specifications: Panel A uses standard errors clustered at the year x industry level; Panel B examines the effects of negative shocks (threshold of –5%), conditional on a recovery (positive employment growth) in year t+1; Panel C includes all establishments with more than 10 employees; Panel D defines *Shock* using a threshold of 2.5% reduction in industrywide employment among non-sample firms; Panel E defines *Shock* using two-year intervals; Panel F truncates the sample at the 1st and 99th percentile of employment growth; Panel G examines employment growth in parity and non-parity establishments when non-sample firms in the same industry experience positive growth in employment. The dependent variable and the control variables are the same as in Table 3 but not reported. For further details please see Table 3.

Panel A: Year x industry clustering

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.082	0.081	0.064	0.083
	(2.21)	*(2.17)*	*(1.78)*	*(2.23)*
Shock	–0.096	–0.095	–1.323	–0.097
	(–3.06)	*(–3.10)*	*(–1.94)*	*(–3.10)*
Parity	–0.044	–0.042	–0.039	8.651
	(–1.47)	*(–1.46)*	*(–1.31)*	*(2.51)*
adj. R^2	0.174	0.174	0.175	0.175
Observations	48,896	48,896	48,896	48,896

Panel B: Transitory shocks followed by an immediate recovery

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.019	0.019	–0.038	0.011
	(0.47)	*(0.46)*	*(–0.72)*	*(0.28)*
Shock	–0.026	–0.026	–0.588	–0.018
	(–0.96)	*(–0.92)*	*(–1.00)*	*(–0.76)*
Parity	–0.040	–0.038	–0.035	8.607
	(–1.26)	*(–1.26)*	*(–1.13)*	*(3.28)*
adj. R^2	0.173	0.173	0.173	0.174
Observations	48,896	48,896	48,896	48,896

Panel C: A sample including establishments with more than 10 employees

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.061	0.057	0.047	0.058
	(2.04)	*(1.96)*	*(1.85)*	*(1.99)*
Shock	–0.070	–0.067	–0.828	–0.068
	(–2.04)	*(–1.93)*	*(–2.35)*	*(–1.97)*
Parity	–0.060	–0.059	–0.057	1.367
	(–1.20)	*(–1.09)*	*(–1.05)*	*(0.76)*
adj. R^2	0.131	0.131	0.131	0.131
Observations	115,565	115,565	115,565	115,565

Panel D: Shocks defined by a –2.5% threshold reduction in employment

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.044	0.044	0.048	0.039
	(2.70)	*(2.55)*	*(1.96)*	*(2.33)*
Shock	–0.055	–0.055	–0.033	–0.054
	(–2.26)	*(–2.27)*	*(–1.79)*	*(–2.35)*
Parity	–0.008	–0.007	–0.013	8.303
	(–0.28)	*(–0.24)*	*(–0.34)*	*(3.16)*
adj. R^2	0.174	0.174	0.175	0.175
Observations	48,896	48,896	48,896	48,896

Panel E: Shock defined by two-year intervals

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.072	0.071	0.063	0.075
	(2.16)	*(2.11)*	*(1.95)*	*(2.30)*
Shock	–0.098	–0.097	–0.658	–0.100
	(–3.62)	*(–3.69)*	*(–0.75)*	*(–3.78)*
Parity	–0.046	–0.044	–0.045	8.759
	(–1.48)	*(–1.48)*	*(–1.55)*	*(3.36)*
adj. R^2	0.175	0.175	0.176	0.176
Observations	48,896	48,896	48,896	48,896

Panel F: Excluding outliers of employment growth

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Shock × Parity	0.086	0.084	0.063	0.087
	(2.46)	*(2.26)*	*(1.79)*	*(2.54)*
Shock	–0.099	–0.097	–1.299	–0.099
	(–3.32)	*(–2.99)*	*(–2.04)*	*(–3.45)*
Parity	–0.043	–0.041	–0.038	8.549
	(–1.33)	*(–1.36)*	*(–1.24)*	*(3.27)*
adj. R^2	0.177	0.177	0.177	0.177
Observations	47,939	47,939	47,939	47,939

Panel G: Positive employment growth

Dependent variable	**log change in number of employees**			
	(1)	**(2)**	**(3)**	**(4)**
Pos. Growth × Parity	–0.0250	–0.0250	–0.0120	–0.0200
	(–2.26)	*(–2.27)*	*(–1.98)*	*(–2.35)*
Pos. Growth	0.0440	0.0440	–0.3760	0.0390
	(2.70)	*(2.55)*	*(–1.00)*	*(2.33)*
Parity	–0.0080	–0.0070	–0.0140	8.3030
	(–0.28)	*(–0.24)*	*(–0.40)*	*(3.16)*
adj. R^2	0.174	0.174	0.175	0.175
Observations	48,896	48,896	48,896	48,896

Table A–6: Wages: Other robustness tests

This table reestimates the baseline regressions in Table 9 to test robustness to the following alternative specifications: Panel A uses standard errors clustered at the year x industry level. Panel B of this table is identical to Table 9 except that it also includes *Shock*, *Shock* × *Parity*, and *LogFirmEmployees³* as well as the interaction terms of *Shock* × *LogFirmEmployees*, *Shock* × *LogFirmEmployees²*, and *Shock* × *LogFirmEmployees³* in columns (2), (4), (6), (8). All other control variables not reported are the same as in Table 9. The dependent variable is the log of median wages of all employees, highly-qualified, qualified, or low-qualified employees. Panel C reestimates the regressions of Table 9 with a sample including all establishments with more than 10 employees. Panel D truncates the sample at the 1st and 99th percentile of employment growth. Control variables are the same as in Table 9 but not reported. For further details see Table 9.

Panel A: Year x industry clustering

Dependent variable: Median wage of…	**All Employees**		**Highly-qualified employees**		**Qualified employees**		**Low-qualified employees**	
Specification	**All establishments with more than 10 employees**							
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Parity	–0.031	–0.033	–0.033	–0.034	–0.029	–0.032	–0.031	–0.031
	(–4.04)	*(–4.68)*	*(–1.96)*	*(–2.07)*	*(–3.77)*	*(–4.36)*	*(–2.71)*	*(–2.74)*
adj. R^2	0.942	0.945	0.800	0.801	0.926	0.929	0.825	0.826
Observations	51,205	51,205	42,336	42,336	51,060	51,060	38,670	38,670
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Additional controls

Dependent variable: Median wage of…	All Employees		Highly-qualified employees		Qualified employees		Low-qualified employees	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Shock × Parity	0.017	0.007	0.007	–0.001	0.016	0.004	0.026	0.007
	(1.38)	*(0.39)*	*(0.33)*	*(–0.05)*	*(1.34)*	*(0.22)*	*(1.89)*	*(0.33)*
Shock	–0.013	1.562	–0.001	–0.459	–0.012	1.503	–0.024	1.696
	(–1.20)	*(1.63)*	*(–0.07)*	*(–0.45)*	*(–1.11)*	*(1.63)*	*(–2.00)*	*(1.85)*
Parity	–0.031	–0.030	–0.033	–0.032	–0.030	–0.029	–0.028	–0.026
	(–3.52)	*(–3.36)*	*(–2.60)*	*(–2.52)*	*(–3.30)*	*(–3.10)*	*(–1.54)*	*(–1.46)*
LogFirmEmployees	–0.614	–0.601	–0.175	–0.187	–0.506	–0.493	–1.120	–1.099
	(–1.74)	*(–1.73)*	*(–0.80)*	*(–0.87)*	*(–1.43)*	*(–1.41)*	*(–2.42)*	*(–2.35)*
$\text{LogFirmEmployees}^2$	0.074	0.073	0.018	0.019	0.062	0.061	0.130	0.128
	(1.79)	*(1.79)*	*(0.70)*	*(0.77)*	*(1.49)*	*(1.49)*	*(2.31)*	*(2.25)*
$\text{LogFirmEmployees}^3$	–0.003	–0.003	–0.001	–0.001	–0.002	–0.002	–0.005	–0.005
	(–1.84)	*(–1.85)*	*(–0.57)*	*(–0.64)*	*(–1.55)*	*(–1.56)*	*(–2.23)*	*(–2.19)*
Shock × LogFirmEmpl.		–0.523		0.149		–0.507		–0.580
		(–1.69)		*(0.45)*		*(–1.71)*		*(–1.96)*
Shock × LogFirmEmpl.^2		0.056		–0.016		0.055		0.063
		(1.73)		*(–0.46)*		*(1.77)*		*(2.02)*
Shock × LogFirmEmpl.^3		–0.002		0.001		–0.002		–0.002
		(–1.73)		*(0.48)*		*(–1.79)*		*(–2.04)*
adj. R^2	0.944	0.947	0.825	0.828	0.927	0.931	0.801	0.803
Observations	51,205	51,205	38,670	38,670	51,060	51,060	42,336	42,336

Panel C: A sample including establishments with more than 10 employees

Dependent variable: Median wage of…	All Employees		Highly-qualified employees		Qualified employees		Low-qualified employees	
Specification	**All establishments with more than 10 employees**							
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Parity	–0.033	–0.034	–0.035	–0.035	–0.033	–0.035	–0.036	–0.036
	(–2.62)	*(–3.11)*	*(–2.79)*	*(–2.98)*	*(–2.45)*	*(–2.86)*	*(–1.73)*	*(–1.80)*
adj. R^2	0.942	0.946	0.868	0.869	0.927	0.93	0.829	0.83
Observations	124,217	124,217	63,927	63,927	123,175	123,175	69,254	69,254
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Panel D: Excluding outliers of employment growth

Dependent variable: Median wage of…	All Employees		Highly-qualified employees		Qualified employees		Low-qualified employees	
Specification	**All establishments with more than 10 employees**							
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Parity	–0.031	–0.034	–0.033	–0.035	–0.029	–0.032	–0.030	–0.030
	(–3.14)	*(–3.77)*	*(–1.60)*	*(–1.69)*	*(–3.02)*	*(–3.66)*	*(–2.41)*	*(–2.48)*
adj. R^2	0.944	0.947	0.802	0.803	0.927	0.931	0.828	0.829
Observations	50,180	50,180	41,489	41,489	50,038	50,038	37,896	37,896
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A–7: Employment change regressions by qualification

This table repeats the employment regressions using the breakdown by educational and vocational qualifications. The log change in employment for each qualification category is the dependent variable. Only establishments with more than 50 employees are included. The t-statistics for the coefficient estimates are reported in parentheses below the estimates. Standard errors allow for clustering at the firm level.

	Highly-qualified		**Qualified**		**Low-qualified**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Shock × Parity	0.041	0.046	0.078	0.078	–0.014	–0.013
	(1.59)	*(1.65)*	*(2.06)*	*(1.97)*	*(–0.15)*	*(–0.15)*
Shock	–0.055	–0.057	–0.081	–0.081	–0.075	–0.077
	(–1.73)	*(–1.71)*	*(–2.30)*	*(–2.14)*	*(–1.93)*	*(–1.91)*
Parity	–0.001	0.001	–0.065	–0.063	0.030	0.029
	(–0.03)	*(0.02)*	*(–1.76)*	*(–1.84)*	*(1.03)*	*(0.98)*
LogEstAge	–0.405	–0.404	–0.743	–0.744	–0.500	–0.500
	(–5.16)	*(–5.16)*	*(–17.41)*	*(–17.40)*	*(–13.15)*	*(–13.17)*
LogSales	0.016	0.045	0.022	–0.024	0.002	–0.014
	(0.67)	*(0.23)*	*(0.79)*	*(–0.13)*	*(0.14)*	*(–0.11)*
Leverage	0.021	0.022	0.007	0.003	–0.048	–0.049
	(0.47)	*(0.53)*	*(0.10)*	*(0.04)*	*(–2.16)*	*(–2.00)*
LogFirmEmployees	0.067	0.176	0.085	0.205	0.083	0.019
	(1.34)	*(0.85)*	*(2.09)*	*(1.06)*	*(1.92)*	*(0.10)*
$LogSales^2$		–0.001		0.001		0.000
		(–0.14)		*(0.23)*		*(0.13)*
$LogFirmEmployees^2$		–0.006		–0.006		0.003
		(–0.48)		*(–0.57)*		*(0.28)*
adj. R^2	0.082	0.082	0.181	0.181	0.076	0.076
Observations	48,896	48,896	48,896	48,896	48,896	48,896
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table A–8: Placebo regressions: Wages

This table repeats the analysis of Table 9 (models (1) and (2)) for two subsamples: parity and non-parity firms. Instead of the *Parity* dummy we use a *Placebo* dummy, which is one for all parity (non-parity) establishments with above median *FirmEmployees* for the respective subsample and zero otherwise. The dependent variable is the log of median wage of all employees. For further details please see Table 9.

	Median wage of all employees			
Subsamples	**Parity firms**		**Non-parity firms**	
	(1)	**(2)**	**(1)**	**(2)**
Placebo	–0.002	0.003	0.006	0.008
	(–0.17)	*(0.21)*	*(0.39)*	*(0.52)*
LogEstAge	0.051	0.050	–0.001	0.000
	(3.39)	*(3.26)*	*(–0.03)*	*(–0.03)*
LogSales	–0.195	–0.165	–0.140	–0.160
	(–1.72)	*(–1.66)*	*(–1.02)*	*(–1.14)*
Leverage	–0.025	–0.024	–0.032	–0.035
	(–0.90)	*(–0.86)*	*(–0.90)*	*(–0.98)*
LogFirmEmployees	0.019	0.048	–0.113	–0.097
	(0.15)	*(0.43)*	*(–1.01)*	*(–0.85)*
$LogSales^2$	0.005	0.005	0.004	0.004
	(1.97)	*(1.94)*	*(1.04)*	*(1.15)*
$LogFirmEmployees^2$	–0.002	–0.003	0.008	0.008
	(–0.29)	*(–0.43)*	*(1.01)*	*(0.97)*
Log#Employees		–0.034		–0.016
		(–4.15)		*(–0.82)*
LogMedianEmplAge		0.188		0.057
		(3.95)		*(0.99)*
RatioWhiteCollar		0.153		0.052
		(2.66)		*(0.62)*
adj. R^2	0.954	0.957	0.973	0.973
Observations	47,483	47,483	3,722	3,722
Year F.E.	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

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