Labor Representation in Governance as an Insurance Mechanism*

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Abstract

We investigate how Germany's mandated 50% labor representation on supervisory boards affects layoffs and wages during adverse industry shocks. We hypothesize that parity-codetermination helps the implementation of implicit contracts that insure employees against adverse shocks. We estimate difference-in-differences in employment and wages using panel data at the establishment level. The results show white-collar and skilled blue-collar employees of firms with parity-codetermination are protected against layoffs during shock periods and pay an insurance premium of about 3% in the form of lower wages. Unskilled blue-collar workers lack real representation on the board, and they are not protected against shocks. The effects of insuring employees manifest in higher operating leverage and lower average profitability. We conclude that mandated parity codetermination implements an insurance mechanism, but also prevents employers from extracting adequate wage concessions from workers in return.

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1 Introduction

Worker participation in corporate governance varies across countries. While employees are rarely represented on corporate boards in most countries, Botero et al. (2004) state "workers, or unions, or both have a right to appoint members to the Board of Directors" (page 1349) in Austria, China, Czech Republic, Denmark, Egypt, Germany, Norway, Slovenia, and Sweden. Such board representation gives labor a means to influence corporate policies, which may affect productivity, risk sharing, and how the economic pie is shared between providers of capital and labor.

This paper focuses on risk-sharing between workers and the firm. Our point of departure is implicit contract theory, which holds that the risk-neutral principals of the firm provide job protection to risk-averse employees against adverse shocks. Employees, in turn, accept lower wages (Baily, 1974; Azariadis, 1975; Rudanko, 2011). Commitment to such implicit insurance contracts may require a means for employees to monitor and enforce the implementation, an aspect often taken for granted in the theoretical literature. We hypothesize labor representation on corporate boards provides an ex-post enforcement mechanism to ensure contracts will be honored when employees need protection.[1]

To test this hypothesis, 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. We choose the German case because it offers a laboratory in which companies that are similar on many dimensions nonetheless 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.

[1] Perotti and von Thadden (2006) postulate that electorates with concentrated wealth have a strong preference to protect human capital rents. Labor representation seems to be a more direct mechanism to achieve this then the indirect mechanism through designing a bank-based governance system.

Using a difference-in-differences approach, we find white-collar and skilled blue-collar workers of parity-codetermined firms are protected against layoffs during adverse industry shocks, but those of non-parity firms are not. Firms are considered to be in adverse industry shocks when other, out-of-sample firms in the same industry with establishments located in Germany decrease their work force by at least 5% with no increase in employment in the following year. Interestingly, unskilled blue-collar workers of the same parity firms are not protected from layoffs during industry shocks. The lack of job protection for unskilled blue-collar workers may be explained by the composition of labor representatives on the supervisory boards. The election process for worker representatives reserves some seats for union representatives and representatives of middle management, favoring skilled blue-collar and white-collar workers. Unskilled-blue-collar workers do not seem to have real representation on the boards championing their cause. We examine occupational status and qualifications of labor representatives of parity-codetermined firms providing the necessary personnel data in 2008 and find no unskilled blue collar workers or those with low educational qualifications.

We also find skilled blue-collar and white-collar workers are fully protected from wage cuts. However, this wage protection does not depend on whether firms are parity codetermined or not; rather, it reflects the downward rigidity in German worker wages, probably stemming from the prevalence of industry-wide collective bargaining agreements. The incremental insurance provided against wage cuts through parity-codetermination seems modest in comparison to the protection against layoffs.

The job protection for white-collar and skilled blue-collar workers does not necessarily imply the implementation of implicit insurance contracts. It could be due to greater worker influence arising from their representation on boards. If it is this influence, rather than insurance, that prevents layoffs during industry shocks, there is no reason to expect employees to pay an insurance premium in the form of lower wages. We find that workers with vocational and higher educational qualifications, two categories that cover most skilled blue-collar and white-collar workers, accept significantly lower wages at parity-codetermined firms. The wage concession is about 3.5%; it is 4.3% in counties with above-median unemployment.

To the extent that firms with parity-codetermination provide protection to their white-collar and skilled blue-collar workers against adverse shocks, their operating leverage should be higher. The higher operating leverage makes firms more vulnerable to industry shocks. We find parity-codetermined firms' profitability and valuation suffer more, and their stock price beta increases more during shock periods than firms without parity-codetermination. Parity-codetermined firms also engage in more major asset sales during shock periods, perhaps to avoid cut in payroll. These asset sales appear to be efficient and are followed by strong recoveries of profitability after the shock, hence, labor representatives at parity-codetermined firms do not seem to use their influence to block efficient restructurings.

These analyses provide an opportunity to address the controversy over whether mandated parity-codetermination is efficient. The insurance hypothesis predicts that parity codetermination is efficient because it improves risk-sharing, and shareholders share in the efficiency gains through lower wages. Jensen and Meckling (1979) argue the opposite: Mandatory codetermination is inefficient because workers' decision rights may guide the firm towards value-decreasing policies. As supporting evidence, they point out that firms rarely voluntarily invite worker representatives on the board. The German experience is similar; firms required to have one-third worker representation rarely adopt parity-codetermination.

To distinguish these arguments we analyze the average impact of parity codetermination on profitability and valuation, i.e., the impact as measured through the cycle over the non-shock and shock periods. We do not find a significant impact on valuation, but the impact on profitability is negative and significant. The ROA of parity-codetermined firms is on average 1.6% to 1.8% lower than that of non-parity firms. The insurance hypothesis is consistent with many, but not with all our findings. Mandated codetermination provides a commitment device and helps implement employment insurance, and workers do make wage concessions in return, but these concessions seem insufficient to enhance firms' profitability.

We subject our baseline specifications to a range of robustness checks and also show that the main results on employment and wages stand up in a regression discontinuity design, which performs a local analysis around the 2,000 employee trade-off.

The hypothesis that firms insure workers against shocks goes back at least to the implicit contracting models of Baily (1974) and Azariadis (1975). More recently, Guiso, Pistaferri, and Schivardi (2005) investigate a matched employee-firm panel of Italian firms and show that firms have a significant role for protecting workers against wage shocks. We add to these contributions by examining how workers are protected against employment shocks. In addition, we explicitly address the commitment problem inherent in the insurance hypothesis by comparing firms with 50% worker representation on the board with those that do not have such representation. In so far as German firms are concerned, insurance is not automatic. The insurance effects are most prevalent when workers have a 50% representation on the board. Even with such representation, not all workers are covered by the insurance; only workers who have their own kinds represented on the board benefit from the insurance.

Similar insurance mechanism may be at work for family firms. Sraer and Thesmar (2007) show that family firms in France insure workers against employment shocks and argue that family firms find it easier to commit to implicit contracts because their managers have a longer time horizon. In contrast to our findings, they also find that firms which provide additional insurance outperform their peers. Ellul, Pagano, and Schivardi (2013) find evidence of employment insurance for a cross-section of countries in contemporaneous work.[2] However, in their study, workers pay a price for employment insurance by accepting higher fluctuations in wages, which is not predicted by the implicit-contract argument.

[2] Bach and Serrano-Velarde (2013) provide evidence for the claim that family links between CEOs and their successors enhance firms' ability to commit to implicit contracts.

Our study is also related to the literature on employment protection; Addison and Teixeira (2003) survey that literature, which mostly follows the lead of Lazear (1990). This literature is concerned with the protection of workers through instruments such as severance pay and notice periods and how they impact employment and unemployment. A later strand of that literature examines cross-country differences in legal institutions protecting employment and worker rights (Botero et al., 2004). We examine an important employment protection mechanism overlooked by these firm-level or country-level studies – implementation of implicit insurance contracts through worker participation in governance, transferring employment risk from workers to firms.

There is also a large literature investigating the implications of German codetermination on firm profitability and valuation. 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.[3] Our analysis adds to this discussion by analyzing establishment-level data and by addressing a specific economic rationale for codetermination.

2 Theoretical considerations and hypothesis development

2.1. The insurance hypothesis

The insurance argument relies on two frictions: (1) firms have better access to capital markets than workers and therefore enjoy a privileged position to insure workers; (2) there is some friction in the labor market such as mobility costs (Baily, 1974) or search frictions (Rudanko, 2011), so that firms do not have to pay the market wage in a competitive labor market in every period.[4] In the simplest version of the insurance hypothesis, 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 workers'

3 A more recent event-study by Petry (2009) finds the codetermination has a negative effect on firm performance.

4 Berk and Walden (2013) argue that frictions in capital markets can be negligible. In their model, firms insure workers' human capital risk and investors then spread this risk by investing in diversified portfolios. Workers could insure themselves directly through participation in capital markets. However, 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.

productivity from period to period. In most models, insurance affects wages as well as the employment status of workers. Workers give up a portion of their wages in return for protection against adverse shocks to wages and employment and receive wages that are sometimes above and sometimes below their marginal product.[5]

The insurance provided to workers shifts employment risk from workers to investors, but an effective risk transfer requires a commitment device that ensures the promise will not be reneged. Workers who give up a portion of their wages have to count on firms' honoring contracts in the event of adverse shocks. The theoretical literature on the insurance hypothesis typically ignores this problem by assuming that firms are endowed with the ability to commit to long-term contracts.[6] However, workers may choose a location close to the firm and make investments in firm-specific human capital well before the firm has to honor its side of the bargain. Thus, workers making wage concessions are vulnerable to breaches of implicit contracts by the firm. We argue parity-codetermination serves as an ex-post enforcement device that ensures firms will honor their commitment to long-term employment contracts.

> ***Hypothesis 1:*** *Parity-codetermination is an ex-post enforcement mechanism that 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 not sustainable. Models with employment insurance implicitly rule out frictionless bargaining between firms and workers. This

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5 Papers that formalize aspects of this argument are Azariadis (1975), Baily (1974), Holmstrom (1983), and Gamber (1988). 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 this setting.

6 Azariadis (1975) assumes that firms which 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).

assumption is not unreasonable because workers can take collective actions in larger firms. Furthermore, ex-post renegotiations of long-term contracts cannot be frictionless, because of workers' limited knowledge of firms' productivity and firms' limited knowledge of workers' outside options.

When firms act as insurers to workers, they enter a quid-pro-quo relationship, whereby workers receive insurance and job guarantees in return for an insurance premium in the form of lower wages. We hypothesize that parity-codetermined firms will provide insurance to workers, whereas non-codetermined firms will not be able to commit to insurance for which workers will agree to lower wages:

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

Providing insurance and job guarantees limit firms' ability to reduce payroll in reaction to changes in technology, consumer taste, or general business conditions. This increases the fixed components of payroll, thereby increasing operating leverage. We therefore expect that the valuation and profitability of parity-codetermined firms respond more negatively to adverse shocks.

Hypothesis 3: *Parity-codetermined firms suffer larger reductions in profitability and valuation from adverse industry shocks than firms without parity-codetermination.*

If parity-codetermined firms' profitability reacts more negatively to adverse shocks, they need to find a way to honor their commitment to maintain the current payroll. The lower profitability and firm valuation will inhibit their ability to raise external capital, making them more cash constrained vis-à-vis non-parity firms. With limited access to external capital and less cash inflows, parity-codetermined firms may have to resort to major asset sales to finance the payroll.

Hypothesis 4: *Parity-codetermined firms engage in more major asset sales during adverse industry shocks than firms without parity-codetermination.*

2.2. Is mandated codetermination efficient?

If labor representation improves efficiency because it enhances risk sharing and shareholders participate in the efficiency gains through lower wages, then parity codetermination should be beneficial to shareholders and workers.[7] However, worker representatives may use their influence not only to protect implicit contracts, but also to prevent restructuring measures necessary for revitalizing the company (Atanassov and Kim, 2009) or to expand employment and thereby increase their power. Moreover, Jensen and Meckling (1979) point out firms almost never voluntarily provide workers with decision-making rights and conclude that labor representation on the board is inefficient and mandating it is likely to be harmful.

However, Levine and Tyson (1990) argue firms do not voluntarily invite worker representatives on the board because competition for talented workers creates externalities. They argue all firms would collectively benefit if they have labor participation in governance because it would provide workers with stronger incentives to enhance productivity.[8] However, such firms would also have compressed wage structures.[9] In smoothly functioning labor markets without mandatory labor representation, firms with labor representation will lose their best workers to firms without labor representation; hence, the equilibrium with labor representation will unravel and only an inferior equilibrium without labor representation will prevail.[10] Hence, they argue for mandatory worker representation for all firms as a

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7 Furubotn (1988) distinguishes between the European model, in which codetermination is legally mandated, and the "joint investment model," where shareholders and workers agree on codetermination as an efficient governance mechanism.

8 Levine and Tyson (1990) review the empirical evidence in support of the productivity benefits of worker participation. Fauver and Fuerst (2006) list more advantages of labor representation, such as reduced frictions and fewer strikes. Kim and Ouimet (2013) show employee stock ownership plans designed to improve worker incentives in general enhance productivity, benefiting both employees and shareholders.

9 Levine and Tyson (1990) provide three reasons why pay would be egalitarian in firms that enhance productivity through worker participation: (1) egalitarian pay is conducive to an atmosphere of trust; (2) bonuses for group work provide better incentives for cooperation than competition in "bonus tournaments"; (3) if worker participation in wage-setting extends to compensation, there will be "pressure to reduce high-end wages." (p. 212).

10 There is a broader literature that identifies frictions in labor markets to support long-term contracts. Baily (1974) provides a formal model of such a friction. In a recent theoretical analysis, Acharya, Pagano, and Volpin

means to remove this externality.[11] While Germany does have mandatory worker representation on the board, it does so only for a small minority of firms.

One counter argument against the proponents of mandated codetermination is that worker participation in governance may facilitate worker-management entrenchment. Pagano and Volpin (2005) develop a model in which management grants control rights to workers and pay above-market wages to garner their support in thwarting hostile takeover bids. Atanassov and Kim (2009) extend their argument and provide evidence of inefficient restructuring in countries that provide strong legal protection for workers. They argue when employees have sufficient voice in governance, managers of poorly performing firms may shift their allegiance from shareholders to workers, forming worker-management alliances to protect their jobs rather than shareholder value. German codetermination may help facilitate such worker-management alliances, as labor representatives have influence on top management appointment and retention decisions. Similarly, with mandatory employee participation in governance, managers are more likely to pursue a "quiet life" to avoid confrontations with employees, whom they work with on a daily basis (Bertrand and Mullainathan, 2003; Cronqvist et al., 2009).

These worker-management entrenchment hypotheses provide a negative prediction on firm performance. If it is entrenchment that provides workers protection against adverse shocks, employees are unlikely to offer wage concessions, and firms incur the costs of employment protection and suffer the ensuing inefficiencies without any matching benefits.

In sum, the improved incentives through worker participation predict productivity gains, whereas the worker-management entrenchment implies value loss. We are agnostic about how these two effects

(2010) show how different levels of frictions in the managerial labor market may enhance or undermine long-term contracts between firms and managers in which firms provide insurance to managers.

[11] These empirical predictions of the Levine and Tyson argument cannot be tested using German data because only a subset of German firms are required to have labor representation, while other German firms with fewer employees and foreign firms are not subject to codetermination.

offset each other, or whether one prevails over the other. As such, we have no prediction on how the co-determination affects firm performance and valuation. We would rather let the data speak.

3 Institutional background, data, and empirical design

3.1. Institutional background on the German governance system and wage bargaining process

Germany has a two-tier board system, where the management board (*Vorstand*) manages day-to-day operations and the supervisory board (*Aufsichtsrat*) supervises and monitors the management board, approves key strategic decisions, and appoints and dismisses management board members, including the CEO, and decides on 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. Direct board interlocks are also prohibited, so it is not possible for a supervisory board member of company A to also sit on the management board of company B if a member of the supervisory board of company B is already on the management board of company A. Individuals are not allowed to accumulate more than ten seats on the supervisory boards of different corporations. For this regulation, a chairmanship counts as two board seats.

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.[12] There is a minimum and a maximum number of seats dependent on the number of employees of the firm and its equity capital. The German stock corporation act (*Aktiengesetz*) requires that half of the supervisory board members are worker representatives for firms with more than 2,000 employees working in Germany. For firms with more 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

12 See Gorton and Schmid (2004) for more technical details, also on the slightly different arrangements in the coal and steel industry, which makes up only a very small part of our sample.

from middle management (*leitende Angestellte*).[13] The annual general shareholders' 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 chairman of the board has the casting vote in case of a tie. Bank representation on supervisory boards and bank equity holdings in German non-financial firms were high at the beginning of our sample period, but declined to levels comparable to those found in the U.S. shortly after 2000, about the middle of our sample period (see Dittmann, Maug, Schneider 2010). By law, establishments with more than 50 employees also have works councils, which represent workers on social and personnel matters, and which have significant information rights as well as the right to demand compensation for dismissals (Wiedemann 1980).

Wages in most German firms are set through collective bargaining agreements between trade unions and employers' associations.[14] Unions used to specialize in broadly-defined industries (e.g., metal, mining, banking, etc.), but several of these unions merged during our sample period. The wage contracts between unions and employers' associations are only binding on their respective members, but are generally extended to non-unionized workers. Firms not covered by binding wage agreements sometimes adopt unionized wage agreements or negotiate firm-level agreements with the unions in their firm. During our sample period it became more common for collective wage agreements to include opt-out clauses that allow firms not to apply the agreement in some circumstances, generally tied to poor business prospects of the firm. Then the workers of the firm may offer wage concessions to the firm to preserve their jobs.

[13] 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 from the firm's workers, one from middle management, and two from unions. For firms with up to 20,000 (over 20,000 employees), the size of the supervisory board increases to 16 (20). The number of representatives from the other constituencies becomes: shareholders 8 (10); workers 5 (6); middle management 1 (1), and union representatives 2 (3).

[14] See Guertzgen (2009) for a detailed discussion of the institutions of the German labor market.

3.2. Data

3.2.1. Data sources and sample construction

The 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.[15] 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. Stock market data comes from *Datastream*, balance sheet and accounting data from *Worldscope*.

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. This data is made anonymous and offered for scientific use by the IAB. An establishment is any facility reported by a company as having a separate physical address, such as a factory, service station, restaurant, or office building. The IAB owns detailed establishment level data on industry, location, employment, employee education, age, nationality, and wages, and provides these data in the form of establishment-level statistics, such as medians, quartiles, and averages on wages and employments according to different classifications and breakdowns.

The industry classification we use is based on the Statistical Classification of Economic Activities in the European Community (NACE), a six-digit industry classification. 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

[15] The DAX was introduced by Deutsche Börse in 1988 and consists of the 30 largest German stock companies trading on the Frankfurt Stock Exchange. The MDAX was introduced in 1994 and originally included 70 large to medium size German stock companies. Both indices together formed the DAX100, the index of top 100 listed German companies, until 2003. In 2003 Deutsche Börse reorganized its indices, reducing the size of the MDAX from 70 to 50 companies and replacing the DAX100 by the HDAX. The HDAX now includes 110 firms from the DAX, MDAX, and TecDAX, the newly introduced technology sector index. Our sample covers all firms included in the DAX 100 until 2003 and the 80 firms included in the DAX 30 and the MDAX after that.

the United Nations.[16] We use the first three-digits of the NACE code, which identifies 224 separate economic sub-sectors (groups). The NACE (Rev. 1.1) classification is available from the IAB database only for 2003 and afterwards. (The IAB reports different industry classifications; unfortunately, none is reported for the entire sample period.) 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.

At our request, the IAB matched our sample of listed firms with their establishment-level database using an automatic procedure; matching was based on company name and address information (city, zip code, street, and house number). Additionally, we provided IAB with names of major subsidiaries listed in the annual report of our sample firms in 2006. All cases not unambiguously matched by the automatic matching 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. 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. 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 belongs to a parity (non-parity) firm after the

[16] NACE is similar to NAICS (North American Industry Classification System), which is also based on ISIC.

sale. This will blur the distinction between parity and non-parity status of the establishment and potentially lead to attenuation bias, but not to spurious results.

3.2.2. Employee classification

The IAB distinguishes employees in different categories depending on their occupational status. The three most important groups are unskilled blue-collar workers, skilled blue-collar workers, and white-collar employees. 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.

The IAB also reports three different qualification levels at each establishment by educational and vocational qualifications: (1) Low-qualified employees neither possess an upper secondary school graduation certificate as their highest school qualification nor a vocational qualification. (2) Qualified employees either have 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 higher education or a university degree. In Germany, only a relatively small fraction of students obtains an upper secondary school degree (high school, Abitur), which allows them to enter a college or university. This fraction rose from 31% in 1992 to 45% in 2008. IAB classifies all employees who obtained a college or university degree as highly qualified. The typical career path in Germany is to leave school after tenth grade and to enter vocational training. In 2009, 57.8% of the German population had such a vocational qualification and IAB classifies these as qualified employees. In 2009, 27.8% of the German population had none of these qualifications. All employees who have neither an upper secondary school degree nor a vocational qualification are classified as low-qualified employees. (See Hethey-Maier and Seth, 2010). Unfortunately, over our sample period an increasing number of firms stopped reporting information on qualifications, either stating the qualification is unknown or not responding to the question. This trend leads to a steady increase in the number of employees with unknown qualifications.

For this reason, our employment analyses rely on the occupational status of unskilled blue-collar workers, skilled blue-collar workers, and white-collar employees. However, IAB does not report wage distributions according to occupational status, so our wage analyses rely on the breakdown by educational and vocational qualifications. We use the median daily wages of the three different qualification levels. 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.

To see how the classification based on educational and vocational qualifications corresponds to the breakdown by occupational status, IAB, upon our request, 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 1. Most highly-qualified workers tend to be white collar workers; most qualified workers, either white collar or skilled blue collar workers; and most low-qualified workers, unskilled blue collar workers. However, the reverse is not true. For example, only a small part of the white-collar workers, who make up close to half of the sample, is highly-qualified. Similarly, more than three quarters of the low-qualified workers are unskilled blue-collar workers, but not all unskilled blue-collar workers are low-qualified. More than a third of unskilled blue-collar workers are classified as either highly-qualified or qualified, presumably because they are not qualified for the job they currently hold or do a job that does not need a formal qualification.

Table A-2 in the Appendix shows the breakdown of the five most common nationalities in German workforce across the three categories of occupational status. It 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.

3.2.3. Composition of labor representatives

To examine the extent to which each type of workers is represented on the board, we hand collect information on the occupational status and the educational and vocational qualification of labor representatives on supervisory boards in 2008. Of 113 sample firms in 2008, 48 provide the relevant information for 229 labor representatives in their annual reports.

Table 2, Panel A, categorizes labor representatives as unskilled blue-collar, skilled blue-collar, white-collar workers, and union representatives. The occupational status of union representatives is usually not reported, although in most cases their occupational status is similar to white-collar employees. In Panel B we categorize labor representatives as low-qualified, qualified, and highly-qualified. We exclude all union representatives from this analysis because their qualification is usually not reported.

These tabulations reveal a striking phenomenon: not a single unskilled blue-collar or low-qualified workers among the 229 labor representatives. The labor representatives are either skilled blue-collar, white-collar, or union representatives. In terms of qualification, labor representatives are more or less evenly distributed between qualified and highly-qualified, but none belongs to the category of low-qualified workers. Although the tabulation is based on only 48 companies in 2008, leaving the possibility of other companies having unskilled blue-collar or low-qualified workers on their board, it illustrates the lack of real representation of unskilled blue-collar or low-qualified workers.

3.2.3. Descriptive statistics

Table 3 provides summary statistics. Monetary units are normalized to 2005 Euros. All variables are defined in Table A-1 in the Appendix. Panel A shows statistics at the establishment level, while Panel B is at the firm level. All accounting and market variables are taken from *Worldscope* and *Datastream*, as they are available only at the firm level. The IAB does not provide information on any of the firm level variables in Panel B. Establishment years for IAB data are from July to June, whereas fiscal years of German firms are mostly from January to December. 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 same year.

3.3. Research design

We hypothesize that labor representation in governance is an ex-post enforcement mechanism to ensure the implicit insurance contract will be honored. The insurance will soften or even remove the impact of an adverse shock that would otherwise require sacrifices from employees. Our empirical strategy is to compare how a negative shock affects employee layoffs and wages of parity-codetermined firms differently from those with one-third or no labor representation on the supervisory board. This comparison calls for a difference-in-differences approach.

The key independent variable is the dummy variable *Parity,* which is one in any firm-year when a firm is required to have 50% worker representation on the supervisory board, and zero otherwise. We shall refer to such observations as parity firms and to all others, including those requiring one-third representation, as non-parity firms. Following Gorton and Schmid (2004), we focus on the difference between parity-codetermined firms and non-parity firms, and do not distinguish between firms with one-third codetermination and those without worker representation.[17] The focus on parity-codetermination is also justified by the fierce debate over the codetermination laws at the time of its passage in 1976, which illustrates that parity-codetermination was much more controversial and of a major concern to shareholders and managers than one-third representation.[18] This definition of labor representation also helps to preserve the sample size of non-parity firms, which is smaller than that of parity firms; only very few firms in our sample have no labor representation. Table 3, Panel B shows

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17 Several of the contributions surveyed by Renaud (2007) also use the presence of parity codetermination as their main variable for labor representation.

18 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 (see also Petry (2009)).

67.4% of our sample firm-years are parity firms. We defer the discussion of some identification issues associated with the definition of *Parity* to Section 3.3.3.

3.3.1. Specification

We perform a difference-in-differences analysis, in which the exogenous intervention comes from exogenous, industry-level negative shocks to employment. Our baseline regression model is as follows:

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

The dependent variable, y_{ijkt}, is the logarithm of the number of employees or the logarithm of the median daily wage, where *i* indexes establishments, *j* indexes firms, *k* indexes industry, and *t* indexes time. $Parity_{jt}$ is the parity dummy, $Shock_{kt}$ is the shock dummy, defined in Section 3.3.2 below, and ϵ_{ijkt} is an error term. The main coefficient of interest is the β on the interaction of *Parity* and *Shock*. It measures the differential impact industry shocks have on employment or wages of parity and non-parity firms. When the dependent variable is the number of employees, for example, our hypothesis predicts $\beta > 0$; that is, parity firms maintain higher levels of employment after an industry-wide shock than non-parity firms. We focus on the difference between parity-codetermined firms and non-parity firms, and neglect the difference between one-third codetermination and the complete absence of worker representation on the board, in line with most of the literature on the subject (See Gorton and Schmid, 2004; Renaud, 2007).

We control for year fixed effects, α_t, and establishment fixed effects, α_i. X_{ijkt} is a vector of control variables, which include the logarithm of the number of employees working for a firm; the logarithm of sales; leverage; and establishment age. We control for firm size because parity-codetermination is mandatory for corporations with 2,000 or more employees working in Germany. We count the number of employees only in Germany because the requirement for parity-codetermination depends on the number of employees in Germany. All variables in monetary terms (e.g., sales and wages) are adjusted for inflation and stated in 2005 Euros.

Later we also estimate the baseline regression with measures of firm performance, beta, and asset sales as dependent variables. We use an accounting based measure of profitability, the return on assets, *ROA*, and a market value based measure of valuation, the logarithm of Tobin's Q, *LogTobinsQ*. In these regressions, we include firm fixed effects instead of establishment fixed effects and all variables are calculated at the firm level.

3.3.2. Definition of shocks

A key in any difference-in-differences approach is the identification of an exogenous shock. We identify shocks using a persistent drop in employment by out-of-sample German firms and foreign firms with establishments in Germany. The main requirement for a suitable definition of shocks is that they are exogenous and sufficiently correlated with the economic environment of our sample firms so that 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 firms, or workers at non-parity firms are also insured. In either case we cannot infer if workers at parity firms 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 from other EU countries and the correlations between employment at our sample firms and foreign firms is too weak. For example, during 2011-2012, 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. We aggregate the number of employees in all establishments located in Germany. An industry is subject to a shock if establishments belonging to out-of-sample firms in 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. When other firms in the same industry reduce the number of workers employed, our sample firms are also likely to be under economic pressure to decrease their payroll. Our test is whether the responses by parity firms differ from those of non-parity firms in our sample. We use the 5% threshold to ensure that shocks are strong enough to have a material effect and frequent enough to permit identification.[19]

A potential concern with using out-of-sample German and foreign firms to define shocks may be that the non-sample firms are too small in comparison to our sample firms. However, the non-sample firms used to define shocks include many large non-listed, family owned, or foreign firms with establishments located in Germany, e.g., 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. Furthermore, 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) in 2006, respectively. These numbers are reasonably close to the corresponding numbers for our sample firms in 2006, which are €11.7 bn (€2.0 bn) and 38,700 (9,200), respectively.

We do not include transitory shocks, which may reflect short-term fluctuations in demand for products and services, with no direct impact on firms' optimal payroll. Since our test requires shocks that are likely to lead to a reduction in payroll, we require that employment growth in an industry is not positive in the year following the initial shock. So we use persistent shocks to employment in non-sample firms and assume our sample firms are also under similar pressure to reduce payroll. *Shock* is equal to one in any given year when non-sample firms in an industry was subject to a persistent shock. We illustrate how *Shock* is defined with Table 4, which shows four possible sequences of employment growth over five years. Untabulated tests show that including transitory shocks leads to insignificant

[19] We experimented with two other definitions of shocks. The first alternative makes shocks comparable across industries with different cash-flow volatilities by defining the cut-off in terms of the standard deviation of the industry-growth rate of employment rather than a fixed cut-off in terms of a certain percentage of employment. 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)).

estimates for θ in equation (1). Transitory shocks therefore do not seem to impact our sample firms and do not qualify as shocks for our purpose.

A shock period is defined such that a decrease of 5% or more in employment triggers a shock period if the following year also shows a non-positive change in employment. If growth is positive in the subsequent year, then the shock is regarded as transitory and *Shock* = 0, even in the year where employment declines by more than 5%. We are not interested in positive shocks, because they do not help us to address our question. A shock period is defined over four years. A shock period ends after four consecutive years of non-positive growth or after a resumption of positive growth, whichever occurs first. *Shock* = 1 for the first year of a shock period and for up to three subsequent years as long as there is no recovery. Hence, Table 4 shows *Shock* = 1 for years 1 and 2, and also for year 3 in case A, because there is no recovery in year 3; no shock years in B, because there is positive employment growth in year 2; and *Shock* = 1 for years 1, 2, 3, and 4 in cases C and D. For better illustration Table 4 also shows the definition of the Shock dummy if we assume a two-year shock period. However, defining shock periods over two or three years instead of four years has no impact on the results. Based on the four-year shock period, 5.8% of establishment-months are in shock periods.

To get a feel for how the definition identifies employment shocks during our sample period, we estimate OLS regressions of the shock dummy as the dependent variable. The independent variables are year dummies with 1991 as the base year. We show the results for the four-year definition of shocks and also for one alternative definition with a two-year shock period. The year dummy coefficients in both regressions are plotted in Figure 1. Both shock definitions are highly correlated, with the four-year definition being somewhat more persistent. We observe peaks in 1994 (almost 40% of industries with *Shock*=1) and 2005 (25% of industries with *Shock*=1), which is consistent with the long economic downturns in Germany following the post-unification boom in the early 1990s (1990-1992) and the recession after the burst of the internet bubble in 2000-2001. The shock-periods appear longer because of the lag built into the definition of shocks. These regressions are reported in Table A-3 in the Appendix, which show adjusted R^2s of only around 8%, indicating that much of the variation in shocks is industry-specific and not driven by the business cycle. Since the longer interval may capture the persistence of industry employment downturns better, we report results based on the four-year interval. Results based on the two-year interval are similar, but unreported to conserve space.

We might suspect that the *Shock* variable is measured incorrectly for industries in which parity-codetermined firms account for a large proportion of the workforce. Then the hypothesized insurance of parity-codetermined firms might force the adjustment of employment in the industry on the non-parity firms, whose employment would then become correspondingly more volatile. However, across all industry-years in our sample, parity firms account on average only for just under 7% of the work force (median: 0.8%). Hence, the scope for incorrect measurement of shocks is very limited.

3.3.3. Identification issues

One identification concern is the potential endogeneity of *Parity*. Employers and employees may attempt to impact their firm's parity status through influencing the number of employees in Germany. Workers may want to keep the number of employees above 2,000 to obtain/maintain the parity status, whereas shareholders may attempt to keep the number of employees in Germany below 2,000 to prevent it. Such attempts may lead to an accumulation of firms around the 2,000 threshold of employees in Germany.

To investigate whether there is any unusual concentration of firms located right below or above the 2,000-employee threshold, we draw a histogram and a kernel density plot of the frequency of distribution of all firm-year observations with 500 to 3,500 workers employed in Germany in Figures 2 and 3. Both graphs show there are more firms with fewer employees, with scattered and minor peaks throughout the whole range of 500 to 3,500, but neither shows an unusual peak around the 2,000 threshold. Hence, there is no evidence for the conjectured behavior of firms to cluster either just below or just above the 2,000 threshold, which is inconsistent with the notion that either workers or shareholders influence firms' employment policies to affect the composition of the supervisory board in their favor.

Another important concern is that *Parity* may proxy for firm size. In particular, larger firms may better be able at protecting themselves against shocks, e.g., by procuring government aid. The *Parity* indicator is a non-linear function of the number of employees in Germany with a jump at 2,000. Thus, we control for the number of workers employed in Germany and sales revenue. We also include their square terms as control variables to account for possible non-linear impacts. In addition, we include an interaction of *Shock* and *LogFirmEmployees* to separate the effects associated with *Parity* from those associated with the number of employees. Finally, we offer a regression discontinuity specification, which performs a local analysis around the 2,000-employee cut-off to address this issue.

4 Empirical results

Our empirical analyses 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 conduct similar difference-in-differences analyses on wages, firm performance, systematic risk (beta), and asset sales.

4.1. Employment

We first estimate the baseline regression for all employees at the establishment level. In the next step, we analyze moderating factors that may influence workers' demand for insurance. Finally, we separate employees by occupational status into white-collar, skilled blue-collar, and unskilled blue-collar workers, and re-estimate the regression for each type. For employment regressions, we include only establishments with more than 50 employees. Inclusion of establishments with a small number of employees would increase noise and would give too much weight to small establishments; for example, for an establishment with only 10 employees, the loss of one employee accounts for 10% of the work force. Moreover, establishments with more than 50 employees almost always have works councils, which have only weak decision-making rights, but may exert some influence at the establishment level (see Wiedemann 1980). Excluding small establishments makes the sample homogenous in this respect. We obtain qualitatively similar but statistically weaker results if we include small establishments.

4.1.1. Baseline results on employment insurance

Table 5 reports estimation results for all employees with different combinations of control variables. Consistent with the insurance hypothesis, the first three columns show a positive, economically large, and statistically significant coefficient on *Shock × Parity.* Our baseline specification is column (3). It shows a coefficient of 0.146, which implies parity-codetermined firms retain 14.6% more employees in comparison to non-parity firms during shock periods. The majority of our sample non-parity firms have one-third worker representation on their supervisory boards.[20] Hence, the employment impact implied by the coefficient of *Shock × Parity* reflects 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 is highly significant in regressions (1) to (4). This implies non-parity firms suffer a sharp decline in employment. We perform an F-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, again, regardless of which controls are included. It appears employees working for parity firms are more or less fully protected against negative industry shocks. An industry-wide decline in employment, on average, leads to a significant reduction in employment among non-parity firms, but employees of parity firms are practically immune to layoffs during shock periods.

Columns (4) to (6) present robustness test results concerning the non-linear size effect and the interaction of *Shock* with *LogFirmEmployees*, respectively. Estimation results in column (4) show the results are robust to adding squared terms to control for a potential non-linear impact of size.

[20] Our sample contains 265, 442, and 1461 firm-year observations with no, one-third, and one-half worker representatives, respectively.

More important, columns (5) and (6) shows the interaction of *Shock* with *Parity* is robust to controlling for the interaction of *Shock* with *LogFirmEmployees*. The employment protection during shock periods is attributable to parity-codetermination, not to employment size. The coefficients on the interaction terms with employment size in column (6) suggest employment size has a non-linear negative effect on employment during shock periods; firms with more employees tend to lay off more workers but at a decreasing rate. The coefficient of *Shock* as a standalone variable becomes insignificant, probably because the new interaction terms absorb much of its effects.

We emphasize that our analysis is at the establishment level. The firms in our sample have multiple plants and many of them are diversified conglomerates, which may react to employment shocks in one industry by transferring workers between establishments of different industries. Such transfers do not affect our analysis. They would be recorded as employment losses in one establishment and employment gains in another establishment. Our regressions would not be affected by the transfer if the affected establishments are not in the same industry, because *Shock* is defined at the industry level. Transfers of employees between different establishments within the same industry are not likely as a response to industry shocks. Also, conglomerates have no particular advantage at within-industry transfers compared to stand-alone firms.

4.1.2. Worker representation by occupational status

The estimation results based on all employees mask important heterogeneity across different types of employees. If employees are protected from layoffs because the 50% employee representation on the supervisory board helps enforce implicit insurance contracts, the level of enforcement may depend on how each type of employees is represented. Since worker representatives are mostly drawn from the pool of skilled blue-collar workers and white-collar workers, the representatives may focus their efforts on protecting their own kind, namely, fellow skilled blue-collar and white-collar workers, rather than unskilled, less educated workers who have no effective representation on the board.

Table 6 re-estimates the employment regressions separately for each skill level. We include the same set of control variables as in specifications (2) to (4) in Table 5. For white-collar workers and skilled blue-collar workers, the coefficient on *Shock x Parity* is positive, economically large, and statistically significant for all specifications.[21] For unskilled blue-collar workers, by contrast, all specifications show a negative but insignificant coefficient on *Shock × Parity*. Unlike white-collar and skilled blue-collar workers, there is no evidence these workers are protected against an industry-wide decline in employment. We attribute this lack of job protection for unskilled workers to their lack of effective representation of supervisory boards.

4.2. Wages

The protection against layoffs during an industry-wide decline in employment among parity firms may not be the results of implementing implicit insurance contracts. It may simply be due to the influence employee representatives have in reducing or blocking layoffs when they make up 50% of supervisory boards. To distinguish the insurance hypothesis from the control rights hypothesis, we examine the relation between wages and parity-codetermination. According to the insurance hypothesis, workers receive lower wages in return for job security, i.e., they pay an insurance premium. By contrast, if parity firms provide job security without wage concessions, then the protection against adverse industry shocks may be attributed to the power bestowed onto employees by mandatory codetermination.

To distinguish these two hypotheses, we estimate regressions relating wages to the *Parity* indicator. We also add *Shock* and *Shock x Parity* to the regression; hence, the coefficient on Parity measures the wage difference between parity-codetermined and non-parity firms during non-shock periods.

We use the median wage at each establishment because the IAB only provides the first quartile, the median, and the third quartile wages. We use two sets of control variables: (1) the same control variables used in the employment regressions, including squared terms to control for size; and (2) these

[21] Some specifications now reject that the sum of *Shock* and *Shock × Parity* equals zero. But it is in favor of a positive net effect, as if employees of parity firms are more than fully protected from the shocks.

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

4.2.1. Wages for all employees

The first two columns in Table 7 report estimation results for all employees. The coefficients on *Parity* are negative and highly significant. The point estimates indicate employees of parity-codetermined firms receive on average about 3.5% lower wages during non-shock periods. The estimate results also provide inferences on the wage difference over the cycle of shock and non-shock periods; $\beta_{Parity} + \beta_{Shock \times Parity}\overline{Shock}$ reported at the bottom of the table, where $\overline{Shock} = 0.16$ represents the average frequency of shocks across all industry-years, an estimate based on the regressions generating Figure 1.[22] We shall refer to these estimates as through-the-cycle estimates. The estimates suggest employees of parity firms earn 3.2% to 3.3% through the cycle of shock and non-shock periods.

The coefficient on *Shock* is negative but mostly insignificant, except for one specification for low-qualified employees. This mostly insignificant shock effect on wages reflects the downward rigidity in German wages. The prevalence of industry-wide collective bargaining agreements makes wages sticky in response to adverse industry shocks.[23] The *Shock × Parity* term shows positive coefficients in all specifications, but is mostly insignificant. With an insignificant negative shock effect on wages of non-parity firms due to sticky wages, it is not surprising that the marginal shock effects associated with parity-codetermination is also insignificant. However, the cumulative effects of *Shock* on parity firms (the

[22] This method does not yield exactly the same result as running the regression without Shock and its interaction with Parity. However, the values are numerically very close.

[23] Harris and Holmstrom (1982) and Thomas and Worrall (1988) rationalize wage stickiness and show that asymmetric insurance, which protects workers against downward shocks, but permits wage increases after upward productivity shocks, may be part of a self-enforcing agreement.

sum of coefficients on *Shock* and *Shock x Parity*) is never negative, suggesting employees of parity firms are more or less fully protected against wage cuts.

Estimated coefficients on controls are mostly consistent with intuition. Unsurprisingly, 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 Brown and Medoff (1989) finding that an increase in establishment size as measured by the number of employees is associated with an increase in wages. Perhaps the difference is due to differences in sample and specification. Our sample is German establishments, heavily skewed towards large firms, and our regression contains a number of other firm size variables, whereas Brown and Medoff (1989) rely on US survey data and samples that include small businesses.

4.2.2. Wages by qualification

The remaining columns in Table 7 report separate estimates for each type of employees in terms of educational and vocational qualifications: low-qualified employees, qualified employees, and highly-qualified employees. Table 1 shows most low-qualified workers tend to be unskilled blue collar workers; most qualified workers, either white collar or skilled blue collar workers; and most highly-qualified workers, white collar workers. But most white-collar workers are classified as qualified rather than highly-qualified, and more than a third of unskilled blue-collar workers are not classified as low-qualified. As such, one needs to exercise caution in relating these separate wage regression estimates to occupational status. For example, we repeat the employment regressions using the breakdown by educational and vocational qualifications and report the estimation results in Table A-4 in the Appendix. The results are qualitatively similar to those based on occupational status, but statistical significance of the coefficient on *Shock x Parity* is weaker for highly-qualified and qualified workers.

The sub-group wage regressions in Table 7 show coefficients on *Parity* ranging from 3.2% to 3.5% for all three qualification levels. The coefficients are highly significant for the qualified and highly-qualified groups, implying 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 large standard errors, probably because roughly one third belong to skilled blue-collar or white-collar workers.

In sum, the wage results, together with the employment results, suggest that skilled blue-collar and white-collar employees receive protection against layoffs during industry downturns and pay an insurance premium in the form of approximately 3.2% to 3.5% lower wages. The employment results also imply unskilled blue-collar workers do not receive protection against layoffs during an industry downturn. However, the wage results are ambiguous as to whether unskilled blue-collar workers also pay an insurance premium. The weaker statistical significance and the inclusion of blue collar and white collar workers in the low-qualified employee group suggest they do not pay the premium. However, our results do not rule out the possibility that all employees of parity firms pay an insurance premium of about 3.5%, but unskilled blue-collar employees do not benefit from the insurance because their interests are not properly represented by the labor representatives on the board.

4.2.3. Employment insurance in high-unemployment and low-unemployment counties

We expect that the exposure to shocks and the demand for insurance is stronger in regions where unemployment is high and where a single employer dominates the local labor market. If unemployment is high, workers take longer to find a new job and may even have to move to find new employment; hence, layoffs are more costly for them. Based on the insurance hypothesis, we therefore expect that employees have a higher willingness to pay for employment protection in these environments. There is no corresponding hypothesis for the employment regressions, because we have no hypothesis that establishments in high-unemployment counties lay off workers more frequently. (They might have laid off more workers in the past.) The argument is based on the likelihood of finding new employment in the same region and therefore on the impact of becoming unemployed for the individual worker, not the probability of becoming unemployed.

Germany has 402 counties and we obtain unemployment rates for each year at the county-level. We define a dummy variable *HighUnemployment*, which equals one in those county-years with above-median unemployment (high-unemployment counties), and zero in all others (low-unemployment counties). In Table 8, we rerun regressions (1) and (2) from Table 7 and enter *HighUnemployment* as well as its interaction with *Parity*. The prediction based on the insurance hypothesis is that workers in parity codetermined firms are willing to pay more for employment insurance compared to workers of parity firms in low-unemployment counties. Hence, the coefficient on the interaction should be negative, which is exactly what we find: the coefficient indicates that for parity firms, wages in high-unemployment counties are 0.40% lower compared to low-unemployment counties, and the difference is statistically highly significant. This finding supports the insurance hypothesis.

4.3. Identification: Regression discontinuity analysis

In the previous analysis we control for size by controlling for the number of employees and firm sales, sometimes including higher-order terms or interactions of these control variables. In this section we perform regression discontinuity analysis to buttress our argument and to detect whether our conclusions hold not only globally in parametric regressions, but also locally around the 2,000-employee cut-off, which determines whether a firm is parity codetermined or not.

Based on the histogram and density plot of the number of employees (Figures 2 and 3), we conclude above that there is no evidence for firms to stay strategically above or below the 2,000 threshold and whether a firm just above or just below this threshold can therefore be regarded as a randomized experiment. We still need to control for size and therefore apply a sharp regression discontinuity design to the residuals from a regression of employment (wages) on the control variables in Table 5 (Table 7). The reason for the sample restriction is that the predictions of the insurance hypothesis apply only during shock periods. We use the optimized bandwidth based on Imbens and Kalyanaraman (2012) as a benchmark. We also show the results from doubling or halving the bandwidth.

Table 9 shows the results of the tests and Figure 4 plots local polynomial regressions around the 2,000-employee cutoffs for employment, where the sample is restricted to shock periods only (Shock = 1). The results show a significant upwards jump of employment residuals above the 2,000 cutoff, confirming the inference from the difference-in-differences analysis, consistent with the notion that the impact of the shock is felt much more strongly on the non-parity firms compared to the parity firms. The local Wald-test shows that the jump at 2,000 employees is statistically significant.

For wages we compare again the through-the-cycle impact of parity codetermination and therefore use the entire sample and not only the shock years. Table 10 and Figure 5 show the results, which corroborate the findings from the panel regressions in Section 4.2. Estimates for the wage premium range from 2.71% (half of the optimal bandwidth) to 3.34% (optimal bandwidth) and are statistically significant at the 5%-level (optimal bandwidth; 10%-level if we halve the optimal bandwidth). Regression discontinuity design therefore corroborates the findings from Sections 4.1 and 4.2 above.

5 Firm-level differences in performance, risk, and asset sales

In this final section, we test the prediction that the insurance provided by parity firms leads to higher operating leverage, exposing them to larger reductions in profitability and valuation from an industry shock relative to non-parity firms. We also test the worker-management entrenchment hypothesis against the hypothesis that mandated codetermination is efficient. The former predicts parity firms are less profitable and valued lower relative to non-parity firms, whereas the latter predicts the opposite.

These predictions are made at the firm level. We therefore run regressions at the firm level and 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.

The dependent variables in the firm-level analysis are the firms' return on assets, Tobin's Q, the CAPM beta, and an indicator for major asset sales. We provide each regression with two sets of control variables and with two specifications: The first specification, reported in Panel A of Table 11, includes *Shock* and the interaction $Shock \times Parity$; this specification measures the exposure of firms to shocks. The second specification, reported in Panel B of Table 11, omits *Shock* and the interaction, and measures the through-the-cycle effect of *Parity* to evaluate whether parity firms outperform non-parity firms.

5.1. Operating leverage

To estimate the effect of insurance on operating leverage, we use ROA, the logarithm of Tobin's Q, and the CAPM beta as dependent variables. Our main interest in the difference-in-differences analysis is again the coefficient of *FirmShock × Parity*, which we expect to be negative. Panel A of Table 11 reports the results; columns (1) and (2) for ROA, columns (3) and (4) for Tobin's Q, and columns (5) and (6) for the CAPM beta. Beta is estimated using the market model and daily stock returns for each calendar year.

All ROA and Tobin's Q regressions show significant and negative coefficients on *FirmShock × Parity*. Economic significance is also large. The estimates for ROA show that profitability of parity-codetermined firms falls by about 3 percentage points more if all employees of a firm are affected by a shock. This number is substantial, when considering that the mean (median) ROA of all firms in the sample is 7.5% (6.9%) (see Panel B of Table 3) and that the effect of FirmShock itself is only 2.6%. The incremental decline in Tobin's Q for parity firms ranges from 9.2% to 12.9% if all employees are affected by a shock. Again, this effect is larger than the effect of the shock on non-parity firms. These estimates for ROA and Tobin's Q suggest that parity codetermination more than doubles the impact of shocks on parity firms compared to non-parity firms.

The estimation results for CAPM beta are also consistent with higher operating leverage. The coefficient on *FirmShock × Parity* is positive and significant and also large, implying that the parity-codetermined firm's beta increases markedly by about 0.21 to 0.25 during adverse industry shocks.

5.2. Asset sales

One way to finance the employment protection during negative shock periods is to sell assets (Atanassov and Kim, 2009). Major asset sales and restructurings belong to the key strategic decisions decided by the supervisory board. Thus, we expect parity-codetermined firms to undertake more major asset sales to protect their core employees during adverse industry shock periods. To test this prediction, we define major asset sales by a dummy variable, *Net PPE dummy*, which equals one if net PPE declines by more than 15%, and zero otherwise. We estimate the PPE regressions as linear probability models even though the dependent variable is a dummy variable, because Probit estimates may not be reliable if many explanatory variables are dummies. To check robustness, we re-estimate the regressions using Probit and find qualitatively similar results.

The results are reported in columns (7) and (8) of Table 11, Panel A. The coefficient on *FirmShock × Parity* is positive and significant, indicating that parity-codetermined firms undertake more major asset sales during shock periods than non-parity firms.

5.3. Firm performance

The evidence of higher operating leverage does not indicate whether parity firms on average perform better or worse than non-parity firm, i.e., through the cycle of non-shock and shock periods. To shed light on the overall performance issue, we re-estimate the regressions in Panel A while omitting *FirmShock* and *FirmShock X Parity,* and report the results in Panel B. The coefficient on *Parity* in Panel B measures the impact of parity-codetermination on profitability and firm value through the cycle. As such, the entrenchment hypothesis predicts a negative coefficient on *Parity*. By contrast, the pro-regulation arguments of Levine and Tyson (1990) and others suggest the opposite prediction.

The coefficient on *Parity* in the ROA regression is negative and highly significant, implying that parity-codetermined firms are in general less profitable than non-parity firms by 1.6 to 1.8 percentage points. This evidence provides little support for the optimistic claims of Levine and Tyson (1990). Rather, the evidence seems to be more consistent with the skeptical view of Jensen and Meckling (1979) on the benefits of worker participation in governance.

However, the *Parity* coefficient in the Tobin's Q regression is numerically positive, but insignificant, suggesting no significant valuation impact of parity codetermination.[24] The discrepancy between ROA and Tobin's Q may be explained by the fact that Tobin's Q reflects not only current profitability but also investor expectations of the firm's future actions such as divestitures of less profitable assets. The *Parity* coefficient in the asset sales regression (Columns (7) and (8)) indeed shows parity firms undertake significantly more major asset sales than non-parity firms. Alternatively, the measurement of firm valuation with Tobin's Q may simply be too imprecise for our purpose. Petry (2009) hand collects the exact transition dates when firms announce changes in their parity status and performs an event study around these dates. He finds a negative impact of changing from non-parity to parity status, corroborating our findings for ROA.[25]

Do the asset sales by parity firms enhance profitability? To answer this question, we relate ROAs one, two, and three years after the asset sale to *PPE dummy*, *Parity*, the interaction of the *PPE dummy* and *Parity*, and the usual controls. The estimation results are reported in Table 12. The ROA in years 1, 2, and 3 after the divestiture is 1.4%, 2.7%, and 1.6% higher for parity firms that undertook major asset sales. This profit enhancing effect is most significant two years after the asset sale, perhaps because it takes time for the effect to materialize but the effect become less noticeable beyond two years, probably because of other confounding effects.

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24 The inconclusive findings for Tobin's Q echo the survey of Renaud (2007), who summarizes four studies that use either Tobin's Q or the market-to-book ratio, with two studies finding negative effects and the other two finding no effect of worker representation.

25 His sample starts in 1998. We cannot repeat his exercise for our larger sample, which also covers the 1990s, during which such announcements would not be reflected in electronic documents.

6 Conclusions and implications

We find parity-codetermined firms provide employees greater protection against layoffs during adverse industry shocks. Employment protection leads parity firms to suffer bigger declines in firm profitability and valuation and exhibit higher betas during shock periods than non-parity firms. Through-the-cycle, parity firms are less profitable and have a lower return on assets compared to non-parity firms. Parity firms also engage in more major asset sales during shock periods, and these asset sales appear to be efficient, because they are associated with a strong recovery of ROA after the shock.

We contrast two theoretical explanations. According to the *insurance hypothesis*, parity-codetermination serves as an ex-post enforcement mechanism to ensure firms honor implicit insurance contracts, whereby workers receive protection against adverse shocks in return for accepting lower wages. The *entrenchment hypothesis*, by contrast, suggests the worker control rights bestowed by parity-codetermination leads to worker-management alliances that may harm shareholders. Both hypotheses predict workers employed by parity firms receive protection when others in the same industry lay off their workers in response to adverse industry shocks.

What distinguishes the two hypotheses is the wage differential between parity and non-parity firms as well as differences in profitability and valuation. If employment protection represents the payoff from insurance, we expect employees of parity firms to accept wage concessions relative to those working for non-parity firms such that parity firms are at least as well off as non-parity firms. While we do observe wage concessions, they appear to be insufficient to compensate parity firms for the increase in operating leverage and for the costs of providing workers with more insurance, leading to a significantly lower profitability of parity firms. Hence, shareholders should not voluntarily adopt parity codetermination, and historically they never did.

Overall, many of our empirical results are consistent with the hypothesis that labor representation on supervisory boards implements implicit insurance contracts. However, labor representatives use their power on supervisory boards also to limit wage concessions. This interpretation is also supported by the finding that only skilled blue-collar and white-collar workers benefit from employment insurance, whereas unskilled blue-collar workers do not. Unskilled blue-collar workers have no single representative on any supervisory board in the sample for which we had data, a finding which suggests that labor representatives use parity codetermination to press for employment insurance, but only for their clientele.

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7 Figures

Figure 1: Distribution of shocks

This figure presents results for OLS regressions with two different industry shock dummies (2-year and 4-year interval) as dependent variable. The independent variables are year dummies and a constant. The plots show the regression coefficients of the year dummies. Year 1991 is omitted.

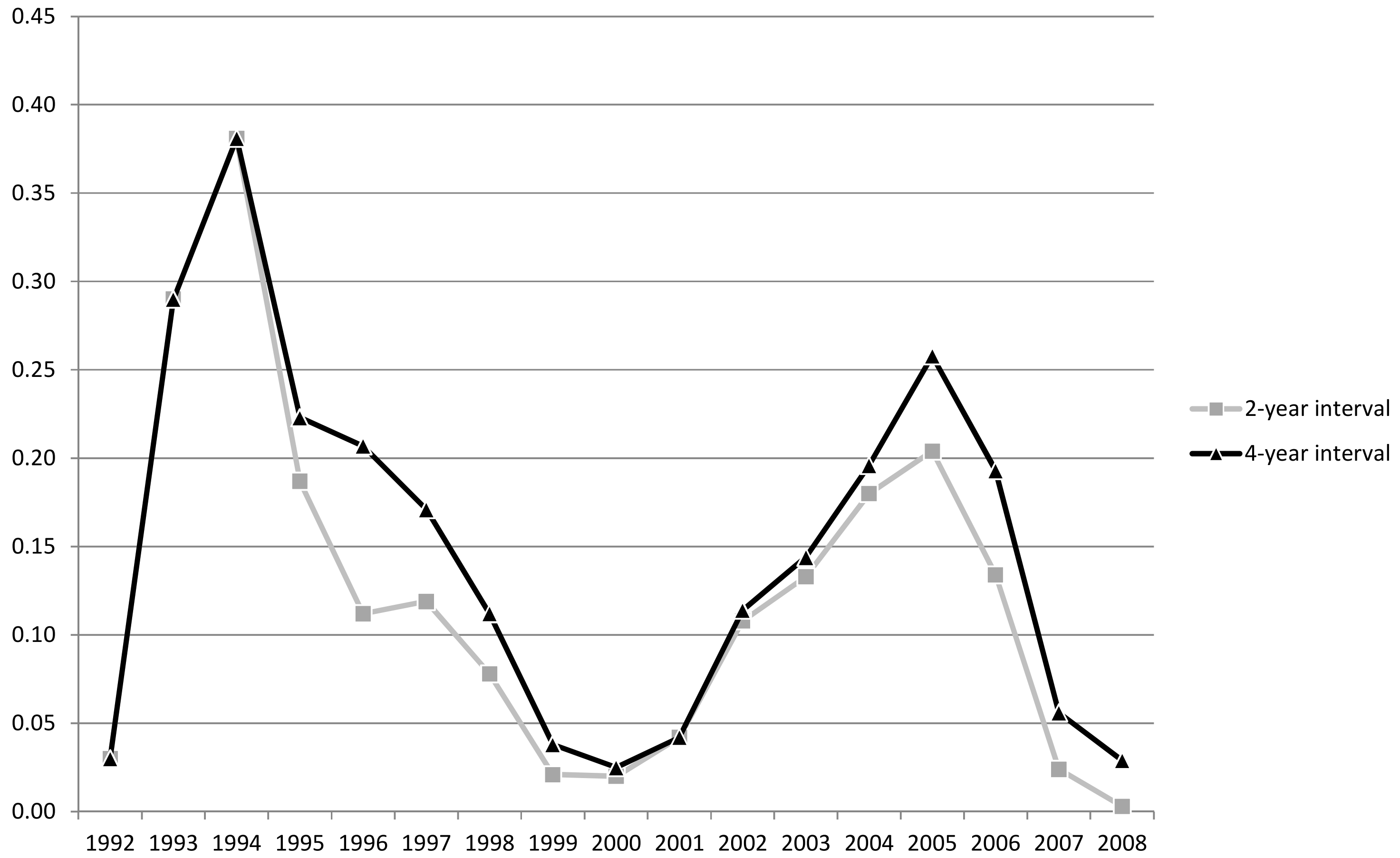


Figure 2: Distribution of firms by number of employees (density plot)

The figure shows a kernel density plot of the frequency distribution of all firm-year observations for which the number of employees in Germany is between 500 and 3,500. An Epanechnikov kernel with bandwidth 192 is used.

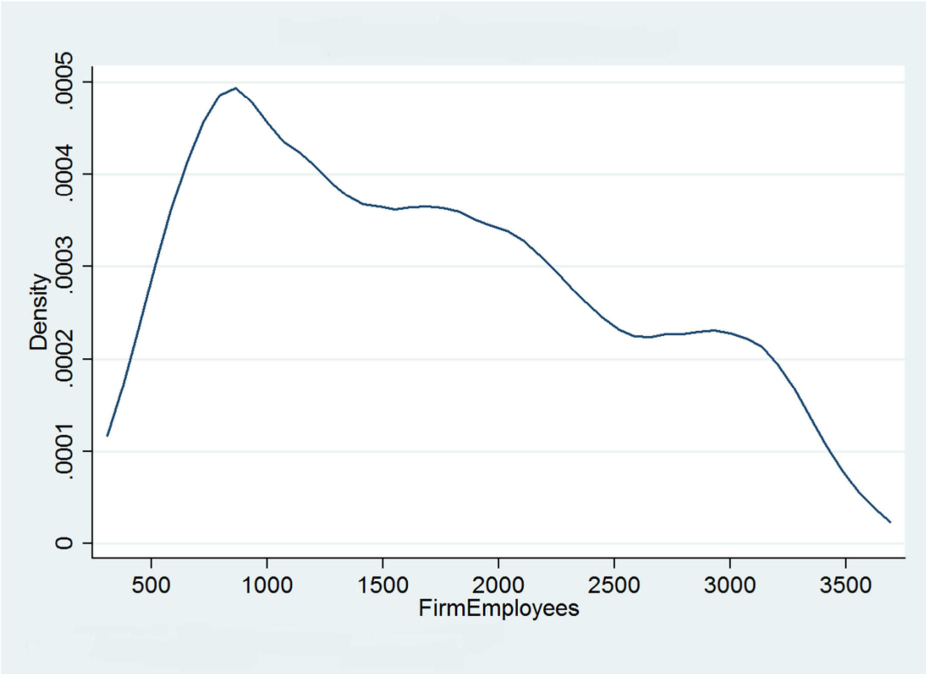


Figure 3: Distribution of firms by number of employees (histogram)

The figure shows a histogram that displays 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.

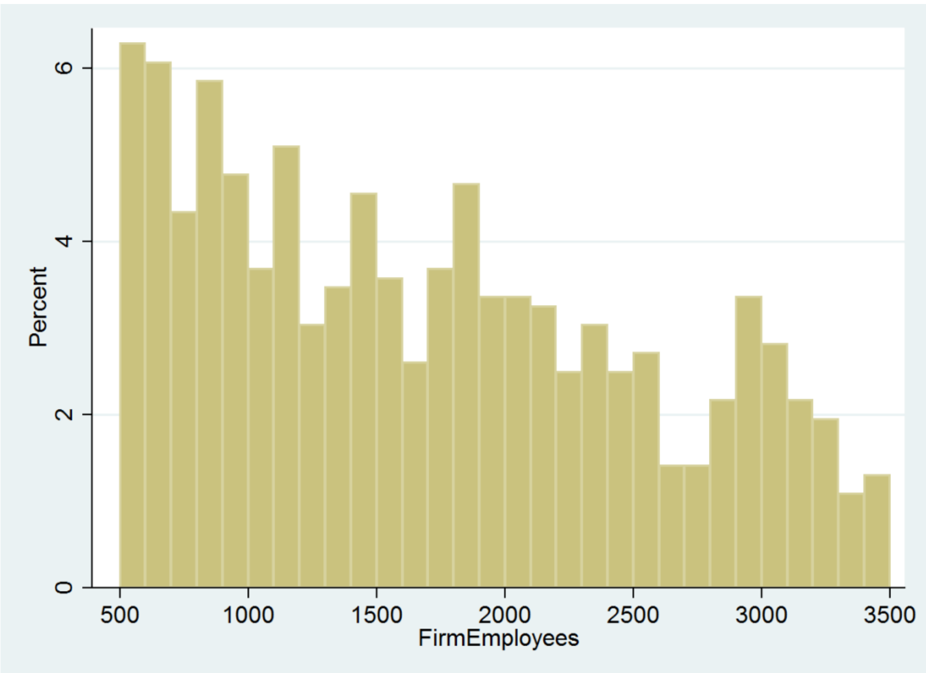


Figure 4: Employment - discontinuity at the 2000-employee cutoff

This figure shows the result of two local polynomial regressions for residual employment during shock periods around the parity cutoff of 2000 employees. Residual employment is estimated using column (4) in Table 5.

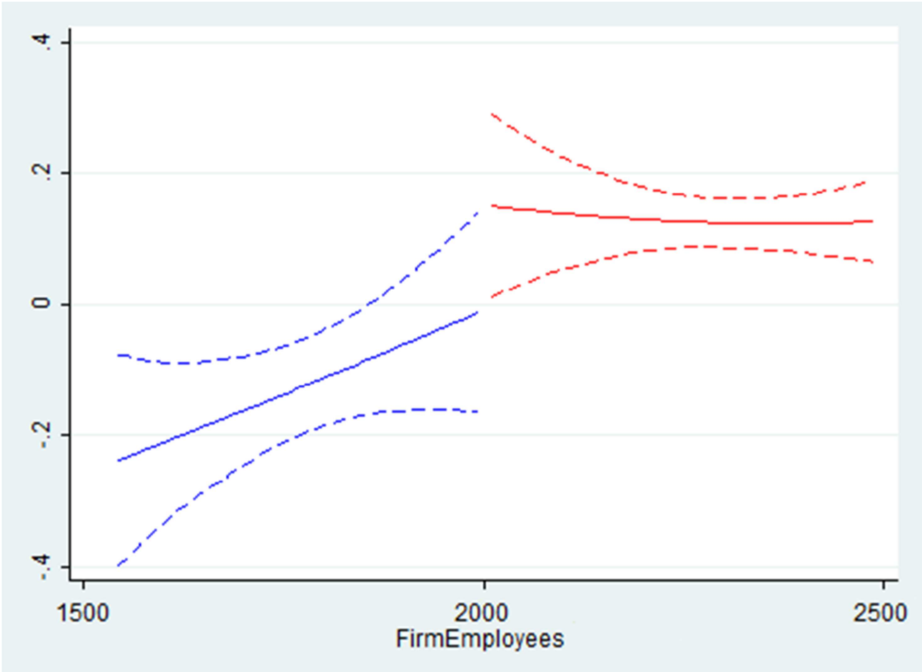
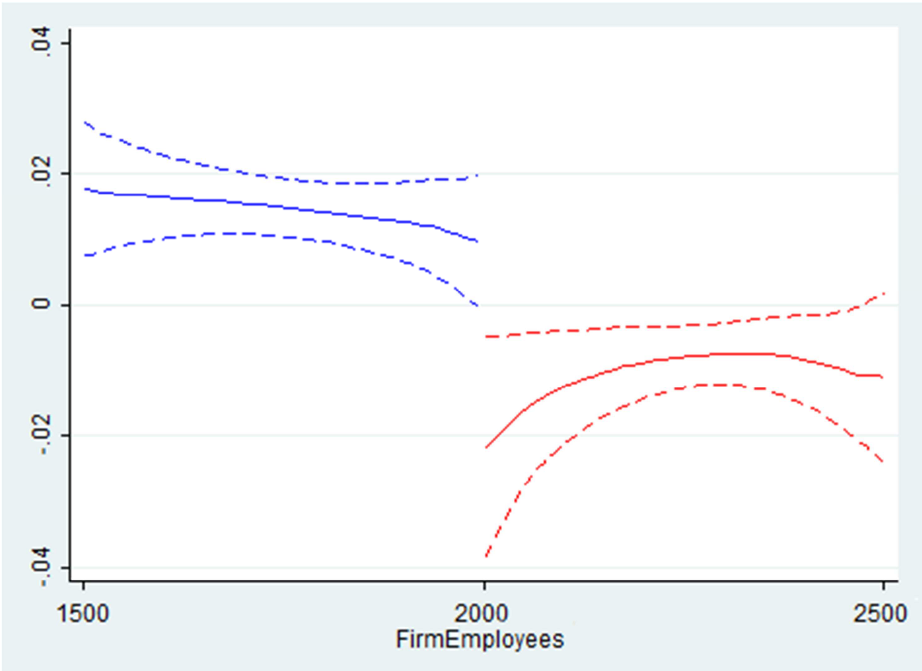


Figure 5: Wages- discontinuity at the 2000-employee cutoff

This figure shows the result of two local polynomial regressions for residual median daily wage around the parity cutoff of 2000 employees. Residual employment is estimated using column (1) in Table 7.



8 Tables

Table 1: Qualification and occupational status

This table presents how the classification based on educational and vocational qualification corresponds to the breakdown by occupational status. 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").

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

Table 2: Qualification and occupational status of employee representatives

Panel A reports the occupational status and (2) the educational and vocational qualification of labor representatives on supervisory boards. We hand collected this information for all sample firms still existing in 2008. The tabulation is based on 229 labor representatives of 48 of 113 sample firms, as the rest of sample firms do not 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 in most cases their occupational status is similar to white collar employees. 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 A

Occupational status	%
Unskilled blue collar	0.0%
Skilled blue collar	22.3%
White collar	56.3%
Union representative	21.4%
Sum	100.0%

Panel B

Qualification	%
Low-qualified	0.0%
Qualified	59.4%
Highly qualified	40.6%
Sum	100.0%

Table 3: Descriptive statistics

This table presents descriptive statistics for all 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. 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	27.982	1.02	61.20	97.99	214.42	53,956
DailyWageP50	94.23	88.38	32.6	7.66	69.56	113.53	228.92	53,956
DailyWageP75	108.76	104.68	34.865	7.66	81.01	132.69	228.92	53,956
DailyWageP50LQ	82.50	77.52	29.1	1.87	61.99	99.04	781.59	44,783
DailyWageP50Q	93.11	88.53	30.2	7.66	70.37	110.98	199.33	53,811
DailyWageP50HQ	124.56	126.03	34.838	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

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 €)	35.2	2.4	117.0	0.029	0.8	14.6	2,020.0	1,991
NetPPE (bn €)	2.6	0.3	7.6	0	0.1	1.5	77.2	2,057
Parity	0.674	1	0.469	0	0	1	1	2,168
ROA	0.075	0.069	0.096	-1.152	0.031	0.110	0.671	1,926
ROE	0.093	0.110	0.227	-2.285	0.058	0.170	2.294	2,023
Sales (bn €)	9.2	1.9	18.5	0.006	0.7	8.3	162.0	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

Table 4: Definition of Shock

This table illustrates our definitions of *Shock* using four different sequences of employment growth.

	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 5: Employment – all employees

This table presents OLS estimation results with log number of employees as dependent variable. Only establishments with more than 50 employees are included. T-statistics are reported in parentheses. Standard errors are clustered at the firm level. The table also reports the p-value for the F-test that *Shock + Shock × Parity = 0*.

Dependent variable	**log number of employees**					
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Shock × Parity	0.2000	0.1690	0.1460	0.1360	0.1190	0.1870
	(3.00)	*(3.05)*	*(2.33)*	*(2.16)*	*(1.66)*	*(2.50)*
Shock × LogFirmEmployees					0.0090	-0.2740
					(0.57)	*(-1.74)*
Shock × LogFirmEmployees²						0.0140
						(1.82)
Shock	-0.1860	-0.1380	-0.1360	-0.1260	-0.2060	1.1010
	(-3.16)	*(-2.82)*	*(-2.51)*	*(-2.48)*	*(-1.61)*	*(1.51)*
Parity	-0.1780	-0.0390	-0.1070	-0.1040	-0.1060	-0.1050
	(-1.48)	*(-0.55)*	*(-1.08)*	*(-1.12)*	*(-1.06)*	*(-1.13)*
LogEstAge		0.1100	0.0930	0.0930	0.0930	0.0930
		(4.03)	*(3.74)*	*(3.82)*	*(3.73)*	*(3.86)*
LogSales		0.1050	0.0120	0.1100	0.0110	0.0990
		(2.30)	*(0.30)*	*(0.34)*	*(0.29)*	*(0.31)*
Leverage		-0.1720	-0.0680	-0.0640	-0.0680	-0.0610
		(-2.30)	*(-1.02)*	*(-0.74)*	*(-1.02)*	*(-0.70)*
LogFirmEmployees			0.4120	0.6430	0.4110	0.6620
			(3.93)	*(1.47)*	*(3.93)*	*(1.51)*
LogSales²				-0.0020		-0.0020
				(-0.29)		*(-0.26)*
LogFirmEmployees²				-0.0130		-0.0140
				(-0.47)		*(-0.50)*
adj. R^2	0.908	0.915	0.919	0.919	0.919	0.919
Observations	52,756	51,271	51,271	51,271	51,271	51,271
F-Test: Shock × Parity+Shock=0	0.675	0.259	0.744	0.730	0.573	0.096
Year F.E.	No	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 6: Employment – white-collar, skilled blue-collar, and unskilled blue-collar employees

This table presents OLS estimation results with log number of (1) white-collar, (2) skilled blue-collar, and (3) unskilled blue-collar employees as 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 F-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)**	**(7)**	**(8)**	**(9)**
Shock × Parity	0.1780	0.1590	0.1710	0.1870	0.1680	0.1490	-0.0220	-0.0120	-0.0190
	(2.23)	*(2.18)*	*(2.29)*	*(3.85)*	*(3.12)*	*(2.91)*	*(-0.43)*	*(-0.23)*	*(-0.35)*
Shock	-0.1130	-0.1120	-0.1230	-0.1260	-0.1240	-0.1050	-0.0750	-0.0950	-0.0880
	(-1.51)	*(-1.66)*	*(-1.83)*	*(-2.97)*	*(-2.63)*	*(-2.46)*	*(-1.80)*	*(-2.03)*	*(-1.86)*
Parity	-0.1440	-0.2000	-0.1990	-0.0680	-0.1250	-0.1200	0.0440	-0.0220	-0.0240
	(-1.58)	*(-1.91)*	*(-1.92)*	*(-0.87)*	*(-1.20)*	*(-1.26)*	*(0.90)*	*(-0.43)*	*(-0.41)*
LogEstAge	0.2530	0.2380	0.2380	0.2780	0.2630	0.2640	0.3190	0.3010	0.3020
	(5.81)	*(6.07)*	*(6.00)*	*(4.37)*	*(4.42)*	*(4.50)*	*(7.65)*	*(8.50)*	*(8.61)*
LogSales	0.1250	0.0490	-0.2640	0.0680	-0.0110	0.2470	0.1080	0.0180	0.3710
	(2.17)	*(0.88)*	*(-0.68)*	*(1.63)*	*(-0.28)*	*(0.79)*	*(1.66)*	*(0.26)*	*(0.71)*
Leverage	-0.0570	0.0280	0.0050	-0.2020	-0.1150	-0.1000	-0.0280	0.0690	0.0940
	(-0.62)	*(0.33)*	*(0.05)*	*(-2.15)*	*(-1.31)*	*(-1.09)*	*(-0.30)*	*(0.76)*	*(0.77)*
LogFirmEmployees		0.3370	0.4090		0.3470	0.6660		0.3970	0.2860
		(3.22)	*(1.05)*		*(4.34)*	*(1.74)*		*(2.35)*	*(0.44)*
$LogSales^2$			0.0070			-0.0060			-0.0080
			(0.74)			*(-0.79)*			*(-0.61)*
$LogFirmEmployees^2$			-0.0040			-0.0170			0.0060
			(-0.15)			*(-0.79)*			*(0.13)*
adj. R^2	0.936	0.9370	0.9370	0.898	0.8990	0.9000	0.898	0.8990	0.8990
Observations	51,271	51,271	51,271	51,271	51,271	51,271	51,266	51,266	51,266
F-Test: Shock × Parity+Shock=0	0.003	0.037	0.035	0.007	0.067	0.067	0.040	0.013	0.012

Table 7: Wages – all, low-qualified, qualified, and highly-qualified employees

This table presents OLS estimation results with median wages of all, low-qualified, qualified, and highly qualified employees as dependent variable. The wage variables are defined as the log of median gross average daily wage for (1) all full-time employees, (2) without educational/vocational qualifications, (3) with educational/vocational qualifications, (4) with higher educational 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: Median wage of…	All Employees		Employees w/o educational/ vocational qualifications		Employees with educational/ vocational qualifications		Employees with higher educational qualifications	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Shock × Parity	0.0130	0.0180	0.0230	0.0280	0.0110	0.0170	0.0060	0.0080
	(0.97)	*(1.43)*	*(1.49)*	*(1.86)*	*(0.84)*	*(1.37)*	*(0.27)*	*(0.34)*
Shock	-0.0090	-0.0140	-0.0220	-0.0260	-0.0070	-0.0130	0.0000	-0.0020
	(-0.73)	*(-1.24)*	*(-1.58)*	*(-1.93)*	*(-0.63)*	*(-1.15)*	*(-0.01)*	*(-0.07)*
Parity	-0.0340	-0.0360	-0.0340	-0.0350	-0.0320	-0.0340	-0.0330	-0.0330
	(-3.50)	*(-4.22)*	*(-1.56)*	*(-1.63)*	*(-3.30)*	*(-4.08)*	*(-2.56)*	*(-2.62)*
LogEstAge	0.0500	0.0490	0.0310	0.0310	0.0510	0.0510	0.0600	0.0610
	(3.60)	*(3.44)*	*(1.88)*	*(1.77)*	*(3.63)*	*(3.51)*	*(6.24)*	*(6.09)*
LogSales	-0.2150	-0.1960	-0.0690	-0.0570	-0.2470	-0.2320	-0.0220	-0.0190
	(-2.28)	*(-2.36)*	*(-0.72)*	*(-0.65)*	*(-2.67)*	*(-2.80)*	*(-0.36)*	*(-0.31)*
Leverage	-0.0210	-0.0200	-0.0740	-0.0720	-0.0140	-0.0130	0.0060	0.0070
	(-0.86)	*(-0.84)*	*(-2.85)*	*(-2.83)*	*(-0.57)*	*(-0.57)*	*(0.31)*	*(0.37)*
LogFirmEmployees	0.0280	0.0660	0.0140	0.0220	0.0380	0.0650	-0.0590	-0.0500
	(0.30)	*(0.75)*	*(0.13)*	*(0.21)*	*(0.43)*	*(0.77)*	*(-0.95)*	*(-0.80)*
$LogSales^2$	0.0060	0.0050	0.0020	0.0020	0.0060	0.0060	0.0010	0.0010
	(2.50)	*(2.59)*	*(1.02)*	*(0.96)*	*(2.81)*	*(2.95)*	*(0.69)*	*(0.65)*
$LogFirmEmployees^2$	-0.0030	-0.0040	-0.0020	-0.0020	-0.0030	-0.0030	0.0030	0.0030
	(-0.44)	*(-0.69)*	*(-0.24)*	*(-0.25)*	*(-0.46)*	*(-0.63)*	*(0.87)*	*(0.81)*
Log#Employees		-0.0330		-0.0150		-0.0320		-0.0090
		(-4.03)		*(-1.77)*		*(-3.82)*		*(-1.57)*
LogMedianEmplAge		0.1830		0.2090		0.1890		0.0730
		(3.83)		*(4.96)*		*(5.43)*		*(2.49)*
RatioWhiteCollar		0.1510		0.0470		0.0710		0.0210
		(2.81)		*(0.75)*		*(1.54)*		*(0.94)*
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
Parity + 0.167 x Shock x Parity	-3.18%	-3.30%	-3.02%	-3.03%	-3.02%	-3.12%	-3.20%	-3.17%

Table 8: Wages and unemployment

This table presents results for OLS regressions with median wages of all employees as dependent variable. The wage variable is defined as the log of median gross average daily wage for all full-time employees. HighUnemployment is one for establishments in counties with above median unemployment rate. Only establishments with more than 50 employees are included in the regression sample. All regressions contain year and establishment fixed effects. The t-statistics for the coefficient estimates are reported in parentheses below the estimates. Standard errors allow for clustering at the firm level.

Dependent variable: Median wage of…	**All Employees**	
	(1)	**(2)**
Shock × Parity	0.0060	0.0190
	(0.29)	*(1.17)*
Shock	0.0070	-0.0070
	(0.37)	*(-0.47)*
$\text{HighUnemployment}_{t-2}$	0.0050	0.0050
	(3.22)	*(2.85)*
Parity x $\text{HighUnemployment}_{t-2}$	-0.0040	-0.0030
	(-2.03)	*(-1.90)*
Parity	-0.0280	-0.0300
	(-2.49)	*(-2.82)*
LogEstAge	0.0180	0.0170
	(1.65)	*(1.54)*
LogSales	0.0280	-0.1500
	(2.21)	*(-1.96)*
Leverage	0.0110	-0.0020
	(0.76)	*(-0.09)*
LogFirmEmployees	-0.0050	0.0520
	(-0.18)	*(0.70)*
LogSales^2		0.0040
		(2.18)
$\text{LogFirmEmployees}^2$		-0.0020
		(-0.49)
Log#Employees		-0.0270
		(-3.17)
LogMedianEmplAge		0.2020
		(4.13)
RatioWhiteCollar		0.1240
		(1.99)
adj. R^2	0.958	0.96
Observations	44,354	44,354
Parity + 0.167 x Shock x Parity	-2.70%	-2.68%
Year F.E.	Yes	Yes
Establishment F.E.	Yes	Yes

Table 9: Employment – 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 residual employment, which is estimated using column (4) in Table 5. We modify the optimized bandwidth by factors of 0.5 and 2 to ensure robustness of our results. A sharp regression discontinuity design is assumed, where the treatment variable (Parity) changes from one to zero at 2000 employees (firm level). The z-statistics for the coefficient estimates are reported in parentheses below the estimates.

Dependent variable	**residual employment**
Parity (bw = 1)	0.1600
	(1.74)
Parity (bw = 0.5)	0.2738
	(1.46)
Parity (bw = 2)	0.4350
	(1.81)
Observations	305

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 residual median daily wage, which is estimated using column (4) in Table 5. We modify the optimized bandwidth by factors of 0.5 and 2 to ensure robustness of our results. A sharp regression discontinuity design is assumed, where the treatment variable (Parity) changes from one to zero at 2000 employees (firm level). The z-statistics for the coefficient estimates are reported in parentheses below the estimates.

Dependent variable	**residual median daily wage**
Parity (bw = 1)	-0.0334
	(-2.45)
Parity (bw = 0.5)	-0.0271
	(-1.72)
Parity (bw = 2)	-0.0331
	(-2.97)
Observations	4,629

Table 11: Firm-level regressions

This table presents OLS estimation results with (1) ROA, (2) log Tobin's q, (3) CAPM beta, and (4) net PPE decrease (< -15%) dummy as the dependent variable. Panel A includes *FirmShock* and *FirmShock* × *Parity.* Panel B does not include these two variables. 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	**ROA**		**Log TobinsQ**		**CAPM beta**		**Net PPE dummy**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
FirmShock × Parity	-0.0310	-0.0320	-0.1290	-0.0920	0.2120	0.2530	0.3990	0.3990
	(-2.27)	*(-2.41)*	*(-2.47)*	*(-1.80)*	*(1.86)*	*(2.21)*	*(2.65)*	*(2.64)*
FirmShock	-0.0260	-0.0260	-0.1010	-0.0750	-0.1270	-0.1540	-0.2390	-0.2390
	(-2.13)	*(-2.14)*	*(-2.24)*	*(-1.70)*	*(-1.27)*	*(-1.54)*	*(-1.81)*	*(-1.81)*
Parity	-0.0140	-0.0110	0.0340	0.0310	0.0470	0.0330	0.1430	0.1410
	(-1.75)	*(-1.42)*	*(1.70)*	*(1.58)*	*(1.11)*	*(0.78)*	*(2.36)*	*(2.33)*
LogFirmAge	-0.0210	-0.0160	-0.0530	-0.0370	-0.0730	-0.0650	0.0840	0.0830
	(-3.02)	*(-2.20)*	*(-3.23)*	*(-2.26)*	*(-2.17)*	*(-1.92)*	*(1.75)*	*(1.71)*
LogSales	0.0320	-0.1740	-0.0100	-0.7470	0.1650	-0.5090	-0.0660	-0.0290
	(8.21)	*(-4.77)*	*(-0.98)*	*(-8.39)*	*(7.48)*	*(-2.42)*	*(-2.28)*	*(-0.10)*
Leverage	-0.1020	-0.1170	-0.2090	-0.2490	0.0530	0.0190	0.0370	0.0390
	(-10.21)	*(-11.48)*	*(-8.13)*	*(-9.72)*	*(0.96)*	*(0.34)*	*(0.49)*	*(0.52)*
LogFirmEmployees	-0.0110	-0.0260	0.0220	0.2770	0.0540	0.3680	0.0040	0.0440
	(-2.89)	*(-1.74)*	*(2.16)*	*(6.32)*	*(2.51)*	*(3.66)*	*(0.14)*	*(0.38)*
LogSales^2		0.0050		0.0180		0.0160		-0.0010
		(5.64)		*(8.38)*		*(3.23)*		*(-0.13)*
$\text{LogFirmEmployees}^2$		0.0010		-0.0190		-0.0230		-0.0030
		(1.08)		*(-5.95)*		*(-3.18)*		*(-0.36)*
adj. R^2	0.501	0.512	0.666	0.682	0.580	0.584	0.115	0.114
Observations	1,815	1,815	1,885	1,885	1,675	1,675	1,809	1,809

Panel B

Dependent variable	ROA		Log TobinsQ		CAPM beta		Net PPE dummy	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Parity	-0.0180	-0.0160	0.0250	0.0250	0.0620	0.0520	0.1750	0.1730
	(-2.35)	*(-2.05)*	*(1.27)*	*(1.27)*	*(1.48)*	*(1.23)*	*(2.94)*	*(2.91)*
LogFirmAge	-0.0220	-0.0160	-0.0550	-0.0380	-0.0690	-0.0600	0.0930	0.0910
	(-3.06)	*(-2.25)*	*(-3.35)*	*(-2.32)*	*(-2.04)*	*(-1.78)*	*(1.94)*	*(1.89)*
LogSales	0.0320	-0.1730	-0.0080	-0.7570	0.1630	-0.4700	-0.0680	0.0240
	(8.28)	*(-4.74)*	*(-0.86)*	*(-8.53)*	*(7.43)*	*(-2.24)*	*(-2.35)*	*(0.08)*
Leverage	-0.1020	-0.1170	-0.2080	-0.2490	0.0510	0.0190	0.0260	0.0320
	(-10.27)	*(-11.53)*	*(-8.08)*	*(-9.72)*	*(0.92)*	*(0.34)*	*(0.36)*	*(0.42)*
LogFirmEmployees	-0.0100	-0.0250	0.0210	0.2800	0.0550	0.3550	0.0070	0.0320
	(-2.83)	*(-1.67)*	*(2.12)*	*(6.39)*	*(2.57)*	*(3.54)*	*(0.25)*	*(0.27)*
$LogSales^2$		0.0050		0.0180		0.0150		-0.0020
		(5.62)		*(8.53)*		*(3.04)*		*(-0.32)*
$LogFirmEmployees^2$		0.0010		-0.0190		-0.0220		-0.0020
		(1.03)		*(-6.03)*		*(-3.04)*		*(-0.22)*
adj. R^2	0.5	0.511	0.665	0.681	0.580	0.583	0.112	0.111
Observations	1,815	1,815	1,885	1,885	1,675	1,675	1,809	1,809

Table 12: Firm-level regressions – long-run performance after asset sales

This table presents OLS estimation results with (1) ROA_{t+1}, (2) ROA_{t+2}, and (3) ROA_{t+3} as the dependent variable. All regressions control for year and firm fixed effects. The t-statistics are reported in parentheses. Standard errors are clustered at the firm level.

Dependent variable	**ROA_{t+1}**		**ROA_{t+2}**		**ROA_{t+3}**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Net PPE dummy × Parity	0.0140	0.0140	0.0270	0.0270	0.0160	0.0160
	(1.98)	*(1.98)*	*(3.32)*	*(3.35)*	*(1.82)*	*(1.80)*
Net PPE dummy	-0.0070	-0.0070	-0.0070	-0.0070	-0.0070	-0.0070
	(-0.77)	*(-0.75)*	*(-0.59)*	*(-0.60)*	*(-0.59)*	*(-0.58)*
Parity	-0.0100	-0.0100	-0.0170	-0.0180	-0.0090	-0.0090
	(-1.74)	*(-1.75)*	*(-2.47)*	*(-2.50)*	*(-1.26)*	*(-1.24)*
ROA_t	0.4300	0.4290	0.2220	0.2230	0.0880	0.0850
	(17.47)	*(17.36)*	*(7.28)*	*(7.26)*	*(2.71)*	*(2.59)*
LogFirmAge	-0.0100	-0.0090	-0.0130	-0.0130	-0.0200	-0.0180
	(-1.28)	*(-1.20)*	*(-1.47)*	*(-1.44)*	*(-2.03)*	*(-1.88)*
LogSales	0.0050	-0.0130	-0.0060	0.0040	-0.0070	-0.0620
	(1.21)	*(-0.31)*	*(-1.06)*	*(0.08)*	*(-1.21)*	*(-1.13)*
Leverage	-0.0030	-0.0040	0.0310	0.0320	0.0420	0.0390
	(-0.26)	*(-0.36)*	*(2.49)*	*(2.45)*	*(3.12)*	*(2.76)*
LogFirmEmployees	-0.0050	-0.0180	-0.0050	-0.0260	-0.0040	-0.0080
	(-1.17)	*(-0.64)*	*(-1.01)*	*(-0.77)*	*(-0.65)*	*(-0.22)*
$LogSales^2$		0.0000		0.0000		0.0010
		(0.43)		*(-0.20)*		*(1.00)*
$LogFirmEmployees^2$		0.0010		0.0010		0.0000
		(0.46)		*(0.62)*		*(0.10)*
adj. R^2	0.596	0.596	0.491	0.49	0.466	0.465
Observations	1,548	1,548	1,425	1,425	1,303	1,303

9 Appendix

Table A-1: 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 is 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 years	Worldscope
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
RatioWhiteCollar	= #WhiteCollar / #Employees	IAB
ROA	= $EBIT_t$ [18191] / {(total $assets_t$ [02999] + total $assets_{t-1}$)/2}	Worldscope
ROE	= Net income [01651] / {(common $equity_t$ [03501] + common $equity_{t-1}$)/2}	Worldscope
Sales	= Net sales or revenues [01001] in 2005 Euros	Worldscope
Shock	= 1 if employment in the same industry (3-digit NACE-code) of the establishment decreases >5% and if the following year also shows a non-positive change in employment, a detailed definition is provided in Section 0.	IAB
DailyWageP25	1st 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	3rd 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 A-2: Occupational status and nationality

This table presents the proportion of the five most common nationalities across our three occupational statuses. 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").

	Unskilled blue collar	Skilled blue collar	White collar
German	79.5%	92.5%	96.4%
Turkish	7.1%	1.9%	0.5%
Italian	2.5%	0.9%	0.3%
Yugoslavian	2.8%	1.5%	0.2%
Greek	1.3%	0.3%	0.1%
Other	6.8%	2.8%	2.6%
Sum	100.0%	100.0%	100.0%

Table A-3: Distribution of shocks

This table presents results for OLS regressions with two different industry shock dummies as dependent variable. The independent variables are year dummies and a constant. Year 1991 is omitted.

Dependent variable	**Industry shock dummy**	
Shock definition	**2 years**	**4 years**
year_1992	0.0300	0.0300
	(0.84)	*(0.78)*
year_1993	0.2900	0.2900
	(8.01)	*(7.44)*
year_1994	0.3810	0.3810
	(10.52)	*(9.77)*
year_1995	0.1870	0.2230
	(5.17)	*(5.73)*
year_1996	0.1120	0.2070
	(3.11)	*(5.34)*
year_1997	0.1190	0.1710
	(3.33)	*(4.43)*
year_1998	0.0780	0.1120
	(2.18)	*(2.91)*
year_1999	0.0210	0.0380
	(0.58)	*(0.98)*
year_2000	0.0200	0.0250
	(0.56)	*(0.67)*
year_2001	0.0420	0.0420
	(1.18)	*(1.10)*
year_2002	0.1080	0.1140
	(3.05)	*(2.98)*
year_2003	0.1330	0.1440
	(3.78)	*(3.79)*
year_2004	0.1800	0.1960
	(5.11)	*(5.17)*
year_2005	0.2040	0.2580
	(5.82)	*(6.81)*
year_2006	0.1340	0.1930
	(3.83)	*(5.10)*
year_2007	0.0240	0.0560
	(0.68)	*(1.48)*
year_2008	0.0030	0.0290
	(0.07)	*(0.78)*
adj. R^2	0.082	0.076
Observations	3,171	3,171

Table A-4: Employment – highly qualified, qualified, and low-qualified employees

This table presents results for OLS regressions with log number of employees with higher educational qualifications ("Highly qualified", regressions (1), (2)), with educational/vocational qualifications ("Qualified", regressions (3), (4)), and (3) without educational/vocational qualifications as dependent variable. Only establishments with more than 50 employees are included in the regression sample. The t-statistics for the coefficient estimates are reported in parentheses below the estimates. Standard errors allow for clustering at the firm level. The table also reports the p-value for the F-test that Shock + Shock × Parity=0.

Dependent variable: log number of employees	**Highly-qualified**		**Qualified**		**Low-qualified**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Shock × Parity	0.1310	0.1410	0.1800	0.1610	-0.0310	-0.0440
	(1.51)	*(1.65)*	*(2.06)*	*(1.93)*	*(-0.53)*	*(-0.69)*
Shock	-0.0910	-0.1000	-0.1570	-0.1380	-0.0970	-0.0860
	(-1.08)	*(-1.24)*	*(-2.14)*	*(-2.13)*	*(-1.98)*	*(-1.77)*
Parity	0.0630	0.0640	-0.1290	-0.1220	-0.0460	-0.0470
	(1.24)	*(1.24)*	*(-0.85)*	*(-0.87)*	*(-0.39)*	*(-0.41)*
LogEstAge	0.1890	0.1890	0.1640	0.1650	0.2080	0.2090
	(3.75)	*(3.70)*	*(2.31)*	*(2.38)*	*(5.20)*	*(5.24)*
LogSales	0.0350	-0.2210	-0.0030	0.2170	0.0270	0.5840
	(0.76)	*(-0.63)*	*(-0.09)*	*(0.65)*	*(0.38)*	*(1.00)*
Leverage	-0.0090	-0.0280	-0.2540	-0.2430	-0.1630	-0.1230
	(-0.11)	*(-0.32)*	*(-1.48)*	*(-1.38)*	*(-1.06)*	*(-0.73)*
LogFirmEmployees	0.2290	0.2990	0.3590	0.7860	0.4650	0.3490
	(3.16)	*(0.97)*	*(3.91)*	*(1.54)*	*(3.42)*	*(0.69)*
LogSales^2		0.0060		-0.0050		-0.0130
		(0.70)		*(-0.64)*		*(-0.88)*
$\text{LogFirmEmployees}^2$		-0.0040		-0.0230		0.0060
		(-0.20)		*(-0.78)*		*(0.18)*
adj. R^2	0.942	0.943	0.912	0.912	0.932	0.932
Observations	51,271	51,271	51,271	51,271	51,266	51,266
F-Test: Shock × Parity+Shock=0	0.115	0.110	0.603	0.584	0.023	0.021
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table A-5: Wages – robustness

This table is identical to Table 10 except that it also includes the interaction terms of *Shock × LogFirmEmployees* and *Shock × LogFirmEmployees²*. For further details please see Table 9 and 10.

Dependent variable: Median wage of…	**Employees w/o educational/ vocational qualifications**		**Employees with educational/ vocational qualifications**		**Employees with higher educational qualifications**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Shock × Parity	0.0040	-0.0090	-0.0030	-0.0100	0.0010	0.0030
	(0.21)	*(-0.46)*	*(-0.22)*	*(-0.58)*	*(0.03)*	*(0.10)*
Shock × LogFirmEmployees	0.0080	0.0540	0.0070	0.0310	0.0020	-0.0040
	(2.19)	*(1.33)*	*(2.04)*	*(0.68)*	*(0.93)*	*(-0.12)*
Shock × LogFirmEmployees²		-0.0020		-0.0010		0.0000
		(-1.10)		*(-0.52)*		*(0.18)*
Shock	-0.0900	-0.2980	-0.0660	-0.1760	-0.0190	0.0110
	(-2.92)	*(-1.62)*	*(-2.37)*	*(-0.83)*	*(-0.81)*	*(0.07)*
Parity	-0.0340	-0.0330	-0.0330	-0.0330	-0.0330	-0.0330
	(-1.59)	*(-1.57)*	*(-3.79)*	*(-3.74)*	*(-2.54)*	*(-2.54)*
LogEstAge	0.0310	0.0310	0.0510	0.0510	0.0600	0.0600
	(1.79)	*(1.79)*	*(3.55)*	*(3.55)*	*(6.12)*	*(6.12)*
LogSales	-0.0640	-0.0620	-0.2370	-0.2360	-0.0200	-0.0200
	(-0.72)	*(-0.71)*	*(-2.87)*	*(-2.87)*	*(-0.34)*	*(-0.34)*
Leverage	-0.0730	-0.0730	-0.0130	-0.0140	0.0070	0.0070
	(-2.86)	*(-2.87)*	*(-0.58)*	*(-0.59)*	*(0.36)*	*(0.36)*
LogFirmEmployees	0.0270	0.0250	0.0690	0.0680	-0.0480	-0.0480
	(0.26)	*(0.24)*	*(0.81)*	*(0.80)*	*(-0.77)*	*(-0.76)*
LogSales²	0.0020	0.0020	0.0060	0.0060	0.0010	0.0010
	(1.02)	*(1.02)*	*(3.01)*	*(3.01)*	*(0.68)*	*(0.68)*
LogFirmEmployees²	-0.0020	-0.0020	-0.0040	-0.0040	0.0030	0.0030
	(-0.30)	*(-0.28)*	*(-0.67)*	*(-0.66)*	*(0.78)*	*(0.78)*
Log#Employees	-0.0150	-0.0150	-0.0320	-0.0320	-0.0090	-0.0090
	(-1.79)	*(-1.78)*	*(-3.82)*	*(-3.81)*	*(-1.57)*	*(-1.57)*
LogMedianEmplAge	0.2080	0.2090	0.1890	0.1890	0.0730	0.0730
	(4.96)	*(4.98)*	*(5.42)*	*(5.42)*	*(2.48)*	*(2.48)*
RatioWhiteCollar	0.0470	0.0480	0.0710	0.0710	0.0210	0.0210
	(0.76)	*(0.76)*	*(1.54)*	*(1.54)*	*(0.94)*	*(0.94)*
adj. R^2	0.837	0.837	0.945	0.945	0.870	0.87
Observations	42,336	42,336	51,060	51,060	38,670	38,670
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table A-6: Firm-level regressions – robustness

This table is identical to Table 12 except that it also includes the interaction terms of *Shock × LogFirmEmployees* and *Shock × LogFirmEmployees²*. For further details please see Table 12.

Dependent variable	**ROA**		**Log TobinsQ**		**CAPM beta**		**Net PPE dummy**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
FirmShock × Parity	-0.0370	-0.0480	-0.0590	-0.0210	0.2680	0.3160	0.4370	0.4150
	(-2.36)	*(-3.01)*	*(-0.96)*	*(-0.34)*	*(2.16)*	*(2.54)*	*(2.64)*	*(2.46)*
Shock × LogFirmEmployees	0.0030	0.0790	-0.0370	-0.3640	-0.0350	-0.6510	-0.0240	0.2440
	(0.78)	*(2.12)*	*(-2.45)*	*(-2.63)*	*(-1.14)*	*(-2.18)*	*(-0.55)*	*(0.61)*
Shock × LogFirmEmployees²		-0.0040		0.0190		0.0350		-0.0150
		(-2.03)		*(2.48)*		*(2.12)*		*(-0.68)*
FirmShock	-0.3320	-0.3220	-0.3680	1.6830	0.1260	2.7100	-0.0680	-1.2100
	(-2.03)	*(-1.99)*	*(-3.16)*	*(2.83)*	*(0.52)*	*(2.08)*	*(-0.20)*	*(-0.70)*
Parity	-0.0130	-0.0100	0.0280	0.0290	0.0440	0.0270	0.1400	0.1400
	(-1.64)	*(-1.19)*	*(1.40)*	*(1.48)*	*(1.02)*	*(0.64)*	*(2.31)*	*(2.30)*
LogFirmAge	-0.0210	-0.0150	-0.0550	-0.0390	-0.0710	-0.0640	0.0860	0.0840
	(-3.00)	*(-2.17)*	*(-3.37)*	*(-2.38)*	*(-2.11)*	*(-1.89)*	*(1.78)*	*(1.73)*
LogSales	0.0320	-0.1740	-0.0120	-0.7490	0.1640	-0.5310	-0.0670	0.0070
	(8.22)	*(-4.78)*	*(-1.22)*	*(-8.34)*	*(7.44)*	*(-2.48)*	*(-2.29)*	*(0.03)*
Leverage	-0.1010	-0.1160	-0.1820	-0.2270	0.0520	0.0170	0.0360	0.0430
	(-10.18)	*(-11.44)*	*(-7.06)*	*(-8.80)*	*(0.94)*	*(0.30)*	*(0.48)*	*(0.56)*
LogFirmEmployees	-0.0110	-0.0260	0.0040	0.3460	0.0560	0.3860	0.0050	0.0380
	(-2.93)	*(-1.72)*	*(0.38)*	*(5.19)*	*(2.62)*	*(3.83)*	*(0.19)*	*(0.33)*
LogSales²		0.0050		0.0180		0.0170		-0.0020
		(5.66)		*(8.30)*		*(3.28)*		*(-0.25)*
LogFirmEmployees²		0.0010		-0.0240		-0.0230		-0.0030
		(1.00)		*(-5.15)*		*(-3.26)*		*(-0.33)*
adj. R^2	0.501	0.512	0.677	0.692	0.581	0.585	0.115	0.114
Observations	1,815	1,815	1,842	1,842	1,675	1,675	1,809	1,809
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes