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"Labor Representation in Governance as an Insurance Mechanism"

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Labor Representation in Governance as an Insurance Mechanism[*]

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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). Firms and employees are likely to commit to such implicit insurance contracts when employees have a means to monitor and enforce its implementation, an aspect that is 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.

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.

Using a difference-in-differences approach, we find white-collar and skilled blue-collar workers of parity-codetermined firms are protected against layoffs when other, *non-sample* firms in the same industry substantially reduce employment. In contrast, unskilled blue-collar workers 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 and may favor skilled blue-collar and white-collar workers. We hypothesize that employees with low qualifications may not have true representation on the boards championing their cause. Indeed, our examination of occupational status and qualifications of labor representatives of parity firms providing the necessary personnel data in 2008 reveals no representation of either unskilled blue collar workers or those with low educational qualifications.

Next, we ask if employees of parity-codetermined firms are also protected from wage cuts during shocks. Our evidence shows that skilled blue-collar and white-collar workers are always fully protected from wage cuts, independently of whether firms are parity codetermined or not. We attribute this finding to the downward rigidity in German worker wages, probably stemming from the prevalence of industry-wide collective bargaining agreements. Thus, the incremental insurance provided against wage cuts through parity-codetermination seems rather modest in comparison to the protection against layoffs.

The protection for white-collar and skilled blue-collar workers does not necessarily imply the implementation of implicit insurance contracts. It may also be due to greater worker influence arising from their representation on boards. If it is this influence, rather than insurance, that prevents layoffs during adverse 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.

We investigate the impact of parity codetermination on operating leverage and firms' sensitivity to industry shocks. 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. Indeed, we find these firms are more vulnerable to industry shocks; their profitability and firm valuation suffers 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; 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 also 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. By contrast, Jensen and Meckling (1979) argue that firms rarely invite worker representatives on the board voluntarily; hence, mandatory codetermination must be inefficient because workers' decision rights may guide the firm towards value-decreasing policies. The argument is even more salient in the German context; for example, 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. 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. We conclude that the insurance hypothesis is consistent with many, but not with all our findings. Mandated codetermination implements employment insurance, and workers do make wage concessions in return, but these concessions are insufficient to enhance firms' profitability. We conclude that parity codetermination provides a commitment device and implements an insurance mechanism, but it also gives workers a bargaining tool to limit the wage concessions they have to offer in return for insurance.

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. We also explicitly consider the commitment problem inherent in the insurance hypothesis by comparing firms that have the mechanism to enforce the contract via worker participation in governance with those that do not. In so far as German firms are concerned, insurance is not automatic. The insurance effects are most prevalent when workers have a sufficient representation on the board. Even with such representation, not all workers are covered by this insurance mechanism. Only workers with board representation of their kind seem to be covered.

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. Ellul, Pagano, and Schivardi (2013) make a similar claim for a cross-section of countries in contemporaneous work.[1] Sraer and Thesmar (2007) also find that family firms outperform widely-held firms, which is in contrast to our finding that parity-codetermined firms, which implement implicit insurance contracts, tend to be less profitable.

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 builds on the approach of Botero et.al. (2004), who construct indices of legal institutions protecting employment and worker

[1] 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.

rights, which help understand the political economy of labor market regulations. These firm-level or country-level studies do not consider that workers can also be protected by implicit insurance contracts.[2] We fill this void by conducting a microeconomic study at the workplace level that focuses on how the allocation of control rights through board representation can help implement an effective transfer of 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. Our analysis adds to this discussion by analyzing establishment-level data and by addressing a specific economic rationale for codetermination.[3]

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, entrepreneurs) insure risk-averse workers against firm-level shocks by promising them a constant wage instead of making wage payments vary with workers' productivity from period to period. In most models, insurance affects wages as well as

[2] Lafontaine and Sivadasan (2009) is the only study we are aware of that uses establishment-level data in a cross-country setting by studying all establishments of one multi-national firm. They focus on how quickly their firm adjusts employment in different countries.

[3] A more recent study by Petry (2009) finds a negative effect of codetermination using event-study methodology.

[4] Berk and Walden (2013) argue that frictions in capital markets can be negligible. If firms insure workers, they can offload workers' human-capital risk to capital markets. This indirect insurance of workers 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.

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 are vulnerable to breaches of implicit contracts by the firm, because they make wage concessions, 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. The question remains how workers ensure that firms will refrain from layoffs and cutting wages when they suffer adverse shocks. 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

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).

employment insurance implicitly rule out frictionless bargaining between firms and workers. This 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 provide advantages to shareholders and workers. 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 provide workers with decision-making rights voluntarily and conclude that labor representation on the board is inefficient and mandating it is likely to be harmful.[7]

Levine and Tyson (1990) argue firms do not voluntarily invite worker representatives on the board because competition for talented workers creates externalities, suggesting mandatory worker representation as a means to remove this externality. They argue that firms are caught in a prisoners' dilemma. All firms would collectively benefit if they introduced labor representation, which 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 most efficient workers to firms without labor representation; hence, the equilibrium with labor representation will unravel and only an inferior

[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 large empirical evidence for 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).

equilibrium without labor representation will prevail.[10] The empirical relevance of this argument for Germany is unclear, however, because not all German firms, let alone foreign firms that compete on the German market, are subject to codetermination.

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

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

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 governance and the wage bargaining process in Germany

Germany has a two-tier board system, where the management board (*Vorstand*) manages day-to-day operations and the supervisory board (*Aufsichtsrat*) supervises the management board and appoints its members, including the CEO. The structure of the board is regulated by the German stock corporation act (*Aktiengesetz*) and the codetermination act (*Mitbestimmungsgesetz*) as well as other laws. 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 size and composition of the supervisory board is mandated by law and 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 from middle management (*leitende Angestellte*). 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.

Wages in most German firms are set through collective bargaining agreements between trade unions and employers' associations.[11] 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.

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.[12] 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*.

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

[12] 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.

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 the United Nations.[13] 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.

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

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 sale. This will blur the distinction between parity and non-parity status of the establishment and potentially lead to attenuation bias.

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.

Our employment regressions rely on the occupational status of unskilled blue-collar workers, skilled blue-collar workers, and white-collar employees. However, our wage analyses have to rely on the breakdown by educational and vocational qualifications because IAB does not report wage distributions according to occupational status. 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 Panel A of 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 93% of skilled blue-collar workers and 96% of white-collar workers are Germans, only 80% of unskilled blue-collar workers are German.

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.

A striking finding from these tabulations is that we cannot find any 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 for unskilled blue-collar or low-qualified workers.

3.2.3. Descriptive statistics

Table 3 provides summary statistics, in which 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 main 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.[14] 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.[15] 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 67.4% of our sample firms 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. Definition of shocks

A key in any difference-in-differences approach is the identification of an exogenous intervention. We identify exogenous shocks using employment shocks to firms that are *not* in our sample. With these external shocks, we analyze how parity and non-parity firms in our sample respond differently to shocks. We define shocks 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 *not* belonging to our sample firms but belonging to the same 3-digit NACE-code industry as a whole suffer a decrease of at least 5% in employment. These establishments may belong to either German or foreign firms. 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

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

15 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)).

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.

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, which shows that the shocks have no impact on the employment of our sample firms. The last observation suggests Germany follows a different business cycle from other EU countries. For example, during 2011-2012, the German economy was booming while most other European countries were in, or on the verge of, a recession.

A potential concern with using German non-sample firms to define shocks may be that they 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.

Four-year interval (baseline): 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%. 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.

Two-year interval (robustness): As a robustness check, we define shocks over a two-year interval. As before, a decrease of 5% or more in employment may trigger a shock period, if the following year also shows a non-positive change in employment. After that, the shock ends. Hence, Table 4shows *Shock* = 1 for years 1 and 2 in case A; there are no shock years in B as before; *Shock* = 1 for years 1 and 2 in case C, but not for year 4 because the decline of 2% is not large enough to define a new shock; and *Shock* = 1 for years 1, 2, 4, and 5 in case D because employment growth in period 4 is -5%, which initiates a new shock.[16]

[16] It would make no difference even if year 3 had a negative growth, say -1%, because the shock period is over after 2 years.

To get a feel for how the two different definitions identify employment shocks during our sample period, we estimate OLS regressions for both definitions of the shock dummy as the dependent variable. The independent variables are year dummies with 1991 as the base year. The year dummy coefficients in both regressions (four- and two-year interval) are plotted in Figure 1. Both shock definitions seem to be highly correlated. The four-year definition is 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 German industry 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. The R^2s of these regressions, reported in Table A-3 in the Appendix, are 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 not tabulated to conserve space.

3.3.2. Specification

Our baseline regression model is as follows:

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

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, 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 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.

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 control variables are calculated at the firm level.

3.3.3. Identification issues

One identification concern is the potential endogeneity of *Parity*. Employers and employees may attempt to impact the firm's parity status through non-market influence on 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 a discontinuity in the distribution 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. On the other hand, our sample does not offer much scope for a regression discontinuity design, which would require a sufficient amount of empirical information around the 2000-threshold, which we do not have.

Another important concern is that *Parity* may proxy for firm size. This is why we control for the number of workers employed in Germany and sales revenue. The *Parity* indicator is a non-linear function of the number of employees in Germany, which jumps at 2,000. Thus, we add square terms of the number of employees and of sales to control for the possible non-linear impact of firm size. In addition, we also control for an interaction between Shock and *LogFirmEmployees*, because the effect of interest is the one on $Parity \times Shock$, and if *Parity* would proxy for size, then the coefficient on $LogFirmEmployees \times Shock$ should become significant and the coefficient on $Parity \times Shock$ should become insignificant.

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. Accordingly, 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), which shows a coefficient of 0.146; hence, parity-codetermined firms have 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.[17] 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

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

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. 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. Interestingly, the coefficients on the interaction terms with employment size in column (6) suggest employment size may have a non-linear negative effect on employment during shock periods, although the coefficients on the interactions are now only significant at the 10%-level and the coefficient of *Shock* itself becomes insignificant, probably because it is absorbed into the new interactions; firms with more employees tend to lay off more workers. Thus, the employment protection associated with parity-codetermination cannot be due to parity firms having large number of employees.

4.1.2. Factors that influence demand for employment insurance

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. We therefore expect that employees demand more employment protection in these environments. Germany has 402 counties and we obtain unemployment rates for each year at the county-level. In Table 6 we rerun regressions (2) to (4) from Table 5 separately for those German county-years with above-median unemployment (high-unemployment counties) and those county-years with below-median unemployment (low-unemployment counties). Regressions (2) and (5) repeat our baseline specification for high and low unemployment counties, respectively. The coefficients on the interaction $Shock \times Parity$ and on *Shock* alone are each about twice as large in high-unemployment counties compared to low unemployment counties and to the pooled results in Table 5. This observation holds for all specifications. We therefore observe that workers in establishments of parity-codetermined firms that are located in high-unemployment counties are more exposed to adverse industry shocks, and they do receive correspondingly more employment insurance.

We are also interested in the prevalence of employment insurance in counties in which one employer has a dominant position in the labor market. If one employer is large relative to all others, then this employer enjoys a quasi-monopsony for labor and a stronger position vis-à-vis the workers in that county. We therefore expect that workers in these counties also demand more employment insurance. We compute a Herfindahl index based on employment of all establishments in each county and split the sample again, this time into county-years with above (below) median values for the employer-Herfindahl index. Table 7 reports the results and uses the same specifications as Table 6. Table 7 shows that the provision of insurance is statistically, but not economically, more significant in counties with above-median employer concentration. Hence, in high-employer-concentration counties the insurance against shocks is more consistently related to the shocks workers are exposed to. Together with the results on high vs. low unemployment counties, these results support the employment-insurance hypothesis.

4.1.3. Employment insurance and workers' skills

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 closely the interests of employee representatives are aligned with those of the employees. 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 8 re-estimates the employment regressions separately for each skill level. We include the same set of control variables as in Tables 6 and 7, repeating the specifications (2) to (4) from Table 5. The coefficient on *Shock* is negative and significant for all three skill levels and statistically significant in the baseline specification at the 10%-level or higher. The coefficient on *Shock x Parity* is positive, economically large, and statistically significant for all specifications for white-collar workers and skilled blue-collar workers.[18]

The same cannot be said about unskilled blue-collar workers. For unskilled blue-collar workers, none of the specifications yields a significant coefficient on *Shock × Parity*, and signs of the coefficient are mostly negative. Unlike white-collar and skilled blue-collar workers, there is no evidence these workers are protected against an industry-wide decline in employment. This finding is surprising, because we would expect that unskilled blue-collar workers are, if anything, less protected against adverse industry-level employment shocks. Unskilled workers typically have lower employment possibilities, higher unemployment rates, and spend longer time searching for a new job when they become unemployed, hence their demand for employment insurance should be larger. We attribute the absence of insurance for unskilled workers to their lack of effective representation of supervisory boards, which we mentioned in Section 3.2.2 above.

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

[18] 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.

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 all other firms during non-shock periods. In Table 9, we also calculate the wage difference across shock and non-shock periods by computing $\beta_{Parity} + \beta_{Shock \times Parity} \overline{Shock}$ at the bottom of the table, where $\overline{Shock} = 0.16$ represents the average frequency of shocks across all industry-years, based on the regressions that generate Figure 1.[19] We shall refer to these estimates as through-the-cycle estimates, because they estimate the impact of *Parity* across shock and 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 control variables used in the employment regressions and (2) these variables plus the number of employees in the establishment, the median employee age, and the percentage of white collar employees. Prior research suggests the additional control variables help explain average employee 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 9 report estimation results for all employees. The variable of interest is *Parity,* which shows negative and highly significant coefficients. Employees of parity-codetermined firms receive on average about 3.5% lower wages. Hence, during non-shock periods, employees of parity-

19 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.

codetermined firms receive about 3.5% lower wages; the through-the-cycle estimates are 3.2% to 3.3% and therefore only marginally smaller.

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.[20] 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 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 and reflect the empirical distribution of firms' size.

4.2.2. Wages by qualification

The remaining columns in Table 9 report separate estimates for each type of employees in terms of educational and vocational qualifications: low-qualified employees, qualified employees, and highly-

[20] The arguments of Harris and Holmstrom (1982) and Thomas and Worrall (1988) imply that asymmetric insurance, which protects workers against downward shocks but not upward shocks, may be part of a self-enforcing agreement.

qualified employees. As mentioned above in the discussion of Table 1, 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 9 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 insurance and pay approximately 3.2% to 3.5% of their wages as a premium. 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. Wages and factors that influence demand for employment insurance

We argue above (Section 4.1.2) that the local unemployment rate and the local employer concentration influence the demand for employment insurance. Based on the same reasoning it is therefore natural to ask if employees accept larger wage concessions in counties with higher unemployment, respectively, a higher employer concentration. Table 10 reports the results. The comparisons in the table use the same sample splits as Tables 6 and 7 and the specifications are the same as those in Table 9 (column (2)).

We obtain similar results for both analyses. In both cases, we observe a numerically larger wage premium in those counties where workers face higher costs of unemployment and for which we conjecture higher demand for employment insurance. In high-unemployment counties, the wage premium is 4.3%, compared to 3.0% in low-unemployment counties. Similarly, in high employer-concentration counties, the wage premium is a highly significant 3.5%, whereas it is only 1.7% and insignificant in low-employer-concentration counties. However, the differences between the subsamples is statistically insignificant in both cases.

4.3. 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, a dummy for large asset sales, and the CAPM beta. 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.

4.3.1. 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 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 2) 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. The coefficient on *FirmShock × Parity* in the regressions for the CAPM beta 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. The evidence supports our hypothesis that adverse industry shocks affect parity-codetermined firms' performance much more negatively than non-parity firms. The results for ROA and Tobin's Q show that parity codetermination more than doubles the impact of shocks on parity firms compared to non-parity firms.

4.3.2. Asset sales

One way to finance the employment protection during negative shock periods is to sell assets (Atanassov and Kim, 2009). 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. In 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.

4.3.3. Firm performance

The analysis in Panel B of Table 11 helps examine whether parity firms perform better or worse than non-parity firm on average, i.e., through the cycle. The entrenchment hypothesis predicts a negative coefficient on *Parity* in both ROA and Q regressions. This coefficient measures the impact of parity-codetermination on profitability and firm value after controlling for the shock and for the interaction effect of the shock with *Parity*. By contrast, the pro-regulation arguments of Levine and Tyson (1990) and others lead to the opposite prediction. The results provide more support for the skeptical argument of Jensen and Meckling (1979) and little support for the optimistic claims of Levine and Tyson (1990). In Panel B of Table 11, the coefficient on *Parity* in the regression for ROA is negative and highly significant, implying that parity-codetermined firms underperform non-parity firms by 1.6 to 1.8 percentage points; the coefficient on *Parity* in the regression for Tobin's Q is numerically positive, but insignificant. 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.

The coefficient on *Parity* is also positive, revealing the tendency of parity-codetermined firms to undertake more asset sales than non-parity firms on average. A comparison with the coefficient on *Parity* in Panel A reveals that parity firms sell more assets than non-parity firms even outside shock periods. However, the coefficient on *FirmShock × Parity* in Panel A is much larger and indicates that some of the insurance provided to workers is paid for by additional asset sales.

We analyze asset sales further in Table 12 and ask if asset sales enhance or destroy profitability. If firms sacrifice profitable investments in order to pay off the wage claims of core workers, then profitability in the years after the asset sales should decline. By contrast, if asset sales enhance profitability than we should see higher ROAs in the years after the divestitures. We therefore regress ROAs one, two, and three years after the asset sale on *PPE dummy*, *Parity*, the interaction of the *PPE dummy* with *Parity*, and the usual controls.

The results show unequivocally that the asset sales of parity firms enhance profitability in subsequent years. 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 asset sales, and the effect is significant at the 5%-level in the first two years. Hence, there is no evidence that parity codetermined firms sell assets to pay off workers, or that workers in parity codetermined firms prevent efficient restructurings.

5 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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6 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.

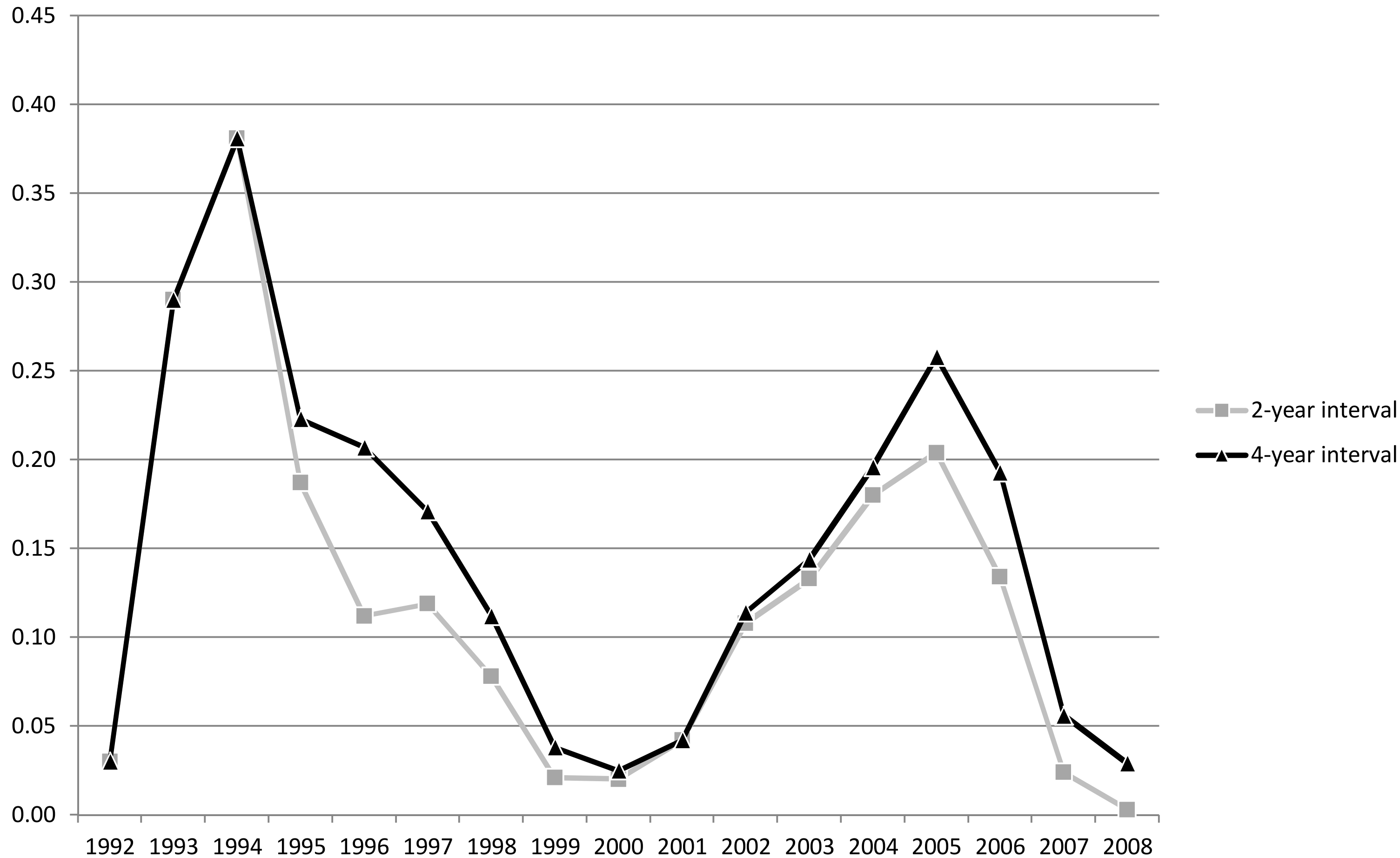


Figure 2: Distribution of firms by number of employees (density plot)

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.

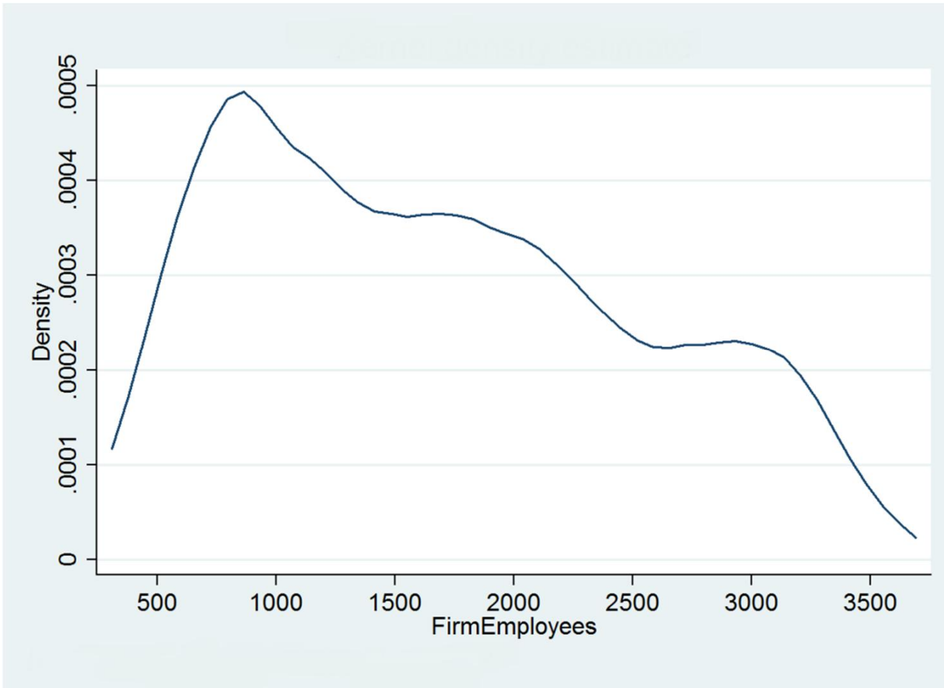
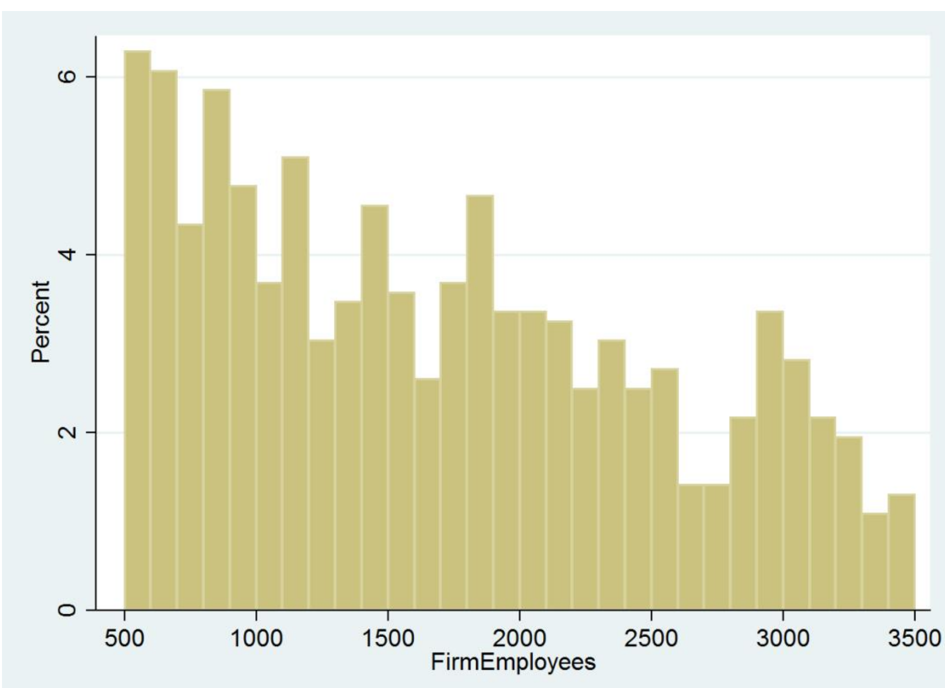


Figure 3: Distribution of firms by number of employees (histogram)

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.



7 Tables

Table 1: Qualification, occupational status and nationality

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

This table presents (1) in Panel A 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. This personal information is not always stated in annual reports. Therefore we could only obtain it for 48 of 113 sample firms with 229 labor representatives. To follow the structure of the IAB data, we categorized labor representatives in Panel A in (1) unskilled blue collar, (2) skilled blue collar, and (3) white collar. Additionally we created the category union representatives because for those the occupational status is usually not reported. However, in most cases their occupational status is similar to white collar employees. In Panel B we categorize labor representatives in (1) low-qualified, (2) qualified, and (3) highly qualified. We exclude all union representatives from this analysis 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 on the establishment level. N reports 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 reports 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	0.48	0.45	0.297	0.00	0.23	0.75	1.00	54,042

Panel B

Variable	Mean	Median	Std	Min	P25	P75	Max	N
Beta	0.678	0.620	0.467	-3.198	0.324	0.997	3.002	1,832
FirmAge	84.5	86	53.3	0	36	124	259	1,989
Leverage	0.392	0.358	0.273	0.000	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	0.000	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

Table 4: Definition of Shock

This table presents the definition 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 results for OLS regressions with log number of employees 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**					
	(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 – high vs. low unemployment

This table presents results for OLS regressions with log number of employees as dependent variable for two subsamples (1) counties with above median and (2) counties with below median unemployment rate. 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**					
	High unemployment			**Low unemployment**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Shock × Parity	0.3040	0.2250	0.2080	0.0950	0.1010	0.0890
	(2.90)	*(2.94)*	*(2.52)*	*(1.25)*	*(1.13)*	*(1.07)*
Shock	-0.2690	-0.2160	-0.1970	-0.0570	-0.0800	-0.0690
	(-2.68)	*(-3.00)*	*(-2.70)*	*(-0.84)*	*(-0.99)*	*(-0.95)*
Parity	-0.0240	-0.0720	-0.0610	-0.0560	-0.1410	-0.1360
	(-0.38)	*(-0.73)*	*(-0.64)*	*(-0.62)*	*(-1.19)*	*(-1.30)*
LogEstAge	0.1320	0.1150	0.1170	0.1610	0.1410	0.1410
	(3.18)	*(3.30)*	*(3.51)*	*(5.36)*	*(4.88)*	*(4.91)*
LogSales	0.0680	-0.0130	0.2630	0.1090	0.0250	0.0490
	(1.37)	*(-0.31)*	*(0.68)*	*(2.74)*	*(0.71)*	*(0.18)*
Leverage	-0.1320	-0.0260	-0.0110	-0.2060	-0.1110	-0.1120
	(-1.55)	*(-0.34)*	*(-0.11)*	*(-2.86)*	*(-1.67)*	*(-1.47)*
LogFirmEmployees		0.4130	0.7440		0.3870	0.7600
		(2.72)	*(1.04)*		*(4.48)*	*(2.12)*
$LogSales^2$			-0.0060			-0.0010
			(-0.67)			*(-0.09)*
$LogFirmEmployees^2$			-0.0170			-0.0200
			(-0.42)			*(-0.94)*
adj. R^2	0.912	0.915	0.915	0.927	0.929	0.929
Observations	23,864	23,864	23,864	23,702	23,702	23,702
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 7: Employment – high vs. low employer concentration

This table presents results for OLS regressions with log number of employees as dependent variable for two subsamples (1) counties with above median and (2) counties with below median employer Herfindahl index. The employer Herfindahl index is estimated based on employment of all establishments in the county. 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**					
	High employer concentration			**Low employer concentration**		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Shock × Parity	0.1520	0.1310	0.1270	0.1270	0.1180	0.1000
	(2.29)	*(2.11)*	*(2.14)*	*(2.04)*	*(1.92)*	*(1.58)*
Shock	-0.1350	-0.1320	-0.1270	-0.0890	-0.0970	-0.0790
	(-2.22)	*(-2.43)*	*(-2.53)*	*(-1.60)*	*(-1.88)*	*(-1.51)*
Parity	-0.0950	-0.1790	-0.1660	0.0240	-0.0200	-0.0290
	(-0.98)	*(-1.25)*	*(-1.29)*	*(0.44)*	*(-0.33)*	*(-0.52)*
LogEstAge	0.0850	0.0630	0.0630	0.1340	0.1220	0.1240
	(2.90)	*(2.10)*	*(2.13)*	*(3.32)*	*(3.33)*	*(3.47)*
LogSales	0.1180	0.0230	-0.0520	0.0850	0.0140	0.3930
	(2.21)	*(0.52)*	*(-0.13)*	*(2.29)*	*(0.45)*	*(1.57)*
Leverage	-0.1480	-0.0480	-0.0590	-0.1940	-0.1100	-0.0940
	(-1.97)	*(-0.72)*	*(-0.62)*	*(-3.46)*	*(-2.04)*	*(-1.47)*
LogFirmEmployees		0.4420	0.8630		0.3540	0.6340
		(3.78)	*(1.78)*		*(3.16)*	*(1.26)*
$LogSales^2$			0.0020			-0.0090
			(0.17)			*(-1.47)*
$LogFirmEmployees^2$			-0.0230			-0.0150
			(-0.74)			*(-0.54)*
adj. R^2	0.939	0.942	0.942	0.918	0.920	0.921
Observations	24,737	24,737	24,737	26,451	26,451	26,451
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes

Table 8: Employment – white-collar, skilled blue-collar, and unskilled blue-collar employees

This table presents results for OLS regressions 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 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	**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
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

This table presents results for OLS regressions 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 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		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%
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 10: Wages – high vs. low unemployment and high vs. low employer concentration

This table presents results for OLS regressions with median wages of all employees as dependent variable for four subsamples (1) counties with above median and (2) counties with below median unemployment rate as well as (3) counties with above median and (4) counties with below median employer Herfindahl index. The employer Herfindahl index is estimated based on employment of all establishments in the county. The wage variable is defined as the log of median gross average daily wage for all full-time employees. 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**		**All Employees**	
	High un-employment	**Low un-employment**	**High employer concentration**	**Low employer concentration**
	(1)	**(2)**	**(3)**	**(4)**
Parity	-0.0430	-0.0300	-0.0350	-0.0170
	(-3.90)	*(-3.07)*	*(-3.96)*	*(-1.35)*
LogEstAge	0.0480	0.0020	0.0550	0.0130
	(2.85)	*(0.22)*	*(4.05)*	*(0.91)*
LogSales	-0.1830	-0.1920	-0.1910	-0.2030
	(-1.85)	*(-2.50)*	*(-2.50)*	*(-2.38)*
Leverage	0.0060	-0.0290	-0.0290	-0.0160
	(0.21)	*(-1.66)*	*(-1.28)*	*(-0.72)*
LogFirmEmployees	0.0320	0.0750	0.1220	0.0230
	(0.29)	*(0.92)*	*(1.20)*	*(0.28)*
$LogSales^2$	0.0050	0.0050	0.0050	0.0050
	(2.07)	*(2.69)*	*(2.75)*	*(2.64)*
$LogFirmEmployees^2$	-0.0020	-0.0040	-0.0080	-0.0010
	(-0.31)	*(-0.71)*	*(-1.25)*	*(-0.21)*
Log#Employees	-0.0310	-0.0240	-0.0430	-0.0220
	(-3.70)	*(-2.78)*	*(-4.51)*	*(-2.73)*
LogMedianEmplAge	0.1960	0.1920	0.1680	0.2090
	(3.41)	*(4.55)*	*(3.93)*	*(3.87)*
RatioWhiteCollar	0.0750	0.1740	0.1560	0.1220
	(1.33)	*(2.51)*	*(2.64)*	*(1.73)*
adj. R^2	0.953	0.966	0.962	0.962
Observations	23,843	23,695	25,929	26,420
Year F.E.	Yes	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table 11: Firm-level regressions

This table presents results for OLS regressions with (1) ROA, (2) log Tobin's q, (3) CAPM beta, and (4) net PPE decrease (<-15%) dummy as 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. The t-statistics for the coefficient estimates are reported in parentheses below the estimates. Standard errors allow for clustering 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
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

This table presents results for OLS regressions with (1) ROA_{t+1}, (2) ROA_{t+2}, and (3) ROA_{t+3} as dependent variable. 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	**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
Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Higher-order terms	No	Yes	No	Yes	No	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes

8 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	1^{st} quartile of gross average daily wage for all full-time employees in 2005 Euros	IAB
DailyWageP50	Median of gross average daily wage for all full-time employees in 2005 Euros	IAB
DailyWageP75	3^{rd} quartile of gross average daily wage for all full-time employees in 2005 Euros	IAB
TobinsQ	= (market capitalization [08001] + total assets [02999] – common equity [03501]) / total assets	Worldscope

Table 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)*
$LogFirmEmployees^2$		-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 × LogFirmEmployees*^{2}. 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