Labor Representation in Governance as an Insurance Mechanism[1*]

E. Han Kim[a] Ernst Maug[b] Christoph Schneider[c]

PRELIMINARY AND INCOMPLETE

Abstract

We investigate how Germany's mandated 50% labor representation on supervisory boards affects layoffs and wage cuts during industry shocks. We hypothesize that parity codetermination helps the implementation of implicit contracts that insure employees against adverse shocks. We estimate difference-in-differences in employment and wages using panel data at the establishment level. The results show white-collar and skilled blue-collar workers employed by firms with parity codetermination are protected against layoffs and wage cuts during shock periods. Moreover, white-collar workers and skilled blue-collar workers pay an insurance premium of about 3% in the form of lower wages. In contrast, we find no evidence of insurance for unskilled blue-collar workers; they are neither protected against industry shocks nor pay insurance premium in the form of lower wages. These results suggest that parity-codetermined firms extend the insurance to encourage investment in firm-specific human capital by those whose skills and qualifications are more valuable.

JEL classifications: G14, G34, G38

Keywords: Risk-sharing, Insurance, Worker representation on corporate boards, Investment in firm specific human capital.

* We are grateful to Marco Pagano and to seminar participants at the Ackerman Conference on Corporate Governance at Bar Ilan University, the University of Mannheim, and the University of Michigan for helpful comments. We are also grateful to Stefan Bender and his team at the Institut für Arbeitsmarkt- und Berufsforschung for providing access to their data.

[a] Ross School of Business, University of Michigan. E-mail: ehkim@umich.edu. Tel: +1 (734) 764 2282.

[b] University of Mannheim Business School. E-mail: maug@corporate-finance-mannheim.de. Tel: +49 (621) 181 1952.

[c] University of Mannheim Business School. E-mail: schneider@corporate-finance-mannheim.de. Tel: +49 (621) 181 1949.

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 policy regarding employee welfare, which may affect productivity and how the economic pie is shared between shareholders and employees.

This paper focuses on risk-sharing between workers and the firm: Risk-neutral principals of the firm provide implicit insurance to risk-averse employees against layoffs and wage cuts during adverse shocks. Employees, in turn, pay an insurance premium in the form of lower wages (Baily, 1974; Azariadis, 1975; and Guiso, Pistaferri, and Schivardi, 2005). We argue firms and employees are likely to commit to such implicit insurance contracts when employees have a means to monitor and enforce its implementation. We hypothesize labor representation on corporate boards provides an ex-post enforcement mechanism to ensure the contract will be honored when employees need the protection.

To test this hypothesis, we examine the German system, which requires 50% employee representation on supervisory boards – hereafter referred as parity codetermination – when firms have more than 2,000 employees working in Germany. We choose the German case because of the availability of detailed, high quality panel data from the Institute of Employment Research (IAB), which compiles data on employment and wages for all establishments located in Germany over our sample period 1990 – 2008.

Using a difference-in-differences approach, we find white-collar and skilled blue-collar workers of parity-codetermined firms are protected against layoffs and wage cuts when other firms in the same industry substantially reduce employment. In contrast, unskilled blue-collar workers are not protected from layoffs during industry shocks. The protection for white-collar and skilled blue-collar workers does

not necessarily imply the implementation of the implicit insurance contract. It may be due to greater worker influence arising from their representation on boards. If it is the influence, rather than insurance, that prevents layoffs and wage cuts during adverse shocks, there is no reason to expect employees to pay an insurance premium in the form of lower wages. The data supports the insurance hypothesis for workers with vocational and higher educational qualifications, two categories that cover mostly skilled blue-collar and white-collar workers. These groups of workers receive lower wages during normal times. By contrast, workers without educational qualifications do not receive significantly lower wages. The insurance hypothesis does not seem to apply to employees with lower levels of qualifications.

Providing insurance only to more educated and highly skilled employees may be efficient for firms with mandatory parity codetermination because the insurance encourages investment in firm-specific human capital. Such investments enhance productivity more if done by workers with more human capital (education) and higher level of skills. Unskilled workers with lower skill levels and less education have less human capital to invest for firm-specific purposes and are easier to replace.

The lack of protection for unskilled blue-collar workers may also be explained by the composition of labor representatives on the supervisory boards. Labor unions have important influence on selecting labor representatives, as witnessed by the pervasive presence of union leaders among labor representatives. Skilled blue-collar workers and white-collar workers typically have higher degrees of unionization and have greater voice in electing union leaders. Since worker representatives on supervisory boards are likely to look after the interests of those who are influential in electing them to the board, employees with low qualifications may not have true representation on the board championing their cause. Without an effective ex-post mechanism to enforce the contract, unskilled workers will neither pay for the insurance in the form of lower wages nor be protected against industry shocks.

To the extent firms with parity codetermination provide protection to their white-collar and skilled blue-collar workers against adverse shocks, their operating leverage will be higher. We find these firms

are more vulnerable to industry shocks; their profitability and firm valuation take greater hits and their stock price beta increases more during shock periods than firms without parity codetermination. We also find parity codetermined firms do more major asset sales during shock periods, presumably to finance the maintenance of payroll.

Finally, we attempt to address the controversy over whether the mandated parity codetermination is efficient. Jensen and Meckling (1979) argue firms rarely voluntarily invite worker representation on the board; hence, the mandatory codetermination must be inefficient because the worker decision rights may guide the firm towards value decreasing policies. Levine and Tyson (1990) provide a counter-argument: The competition for workers between firms creates externalities and a mandatory worker representation will remove this externality. They argue firms would benefit if they all introduced labor representation, providing workers with stronger incentives to enhance productivity. However, such firms would also have compressed wage structures. In smoothly functioning labor markets, firms with labor participation will lose their star performers to firms without labor participation; thus, the equilibrium with labor participation will unravel and only an inferior equilibrium without labor participation will prevail. These arguments provide a testable prediction; firms with mandatory codetermination will have more compressed wage structure. Furthermore, if investors capture some of the surplus associated with higher productivity, parity codetermined firms will be more profitable and valued higher. We do not find support for either prediction. There is no significant evidence that firms with parity codetermination have more compressed wage structures.

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 shocks. We add to these contributions by relating the insurance hypothesis to worker participation in governance. In so far as German firms are concerned, the insurance is not automatic. The insurance effects are most prevalent when workers have a sufficient representation on

the board. Even with such representation, the insurance does not apply to all workers. Only workers with higher educational qualifications seem to be covered by the insurance to encourage investment in firm-specific human capital.

2 Theoretical considerations and hypothesis development

The insurance hypothesis. Our hypotheses rely on two distinct but related versions of the insurance argument; the efficient risk-sharing and the protection of workers' investments in firm-specific human capital. According to the efficient risk-sharing argument, risk-neutral diversified investors insure risk-averse workers against firm-level shocks. Workers give up a portion of their wages in return for the protection.[2] This shifts human-capital risk from workers to investors. Workers giving up a portion of their wages have to count on firms' honoring their side of bargain in the event of adverse shocks.

When employees can count on a long-term employment with the firm, they are more likely to invest in firm-specific human capital. Relationship-specific investments require ex ante incentives as well as ex post protection through decision-making rights. Hence, according to this argument, workers should have such rights and be entitled to a share in the firm's surplus when they make investments in firm-specific human capital.[3] Such investments can be narrowly defined as acquiring firm-specific skills and knowledge. However, Alesina et. al. (2010) provide a broader perspective on workers' firm-specific investments, arguing that workers suffer utility losses when layoffs force them to give up family relationships. Workers' choice of a domicile close to a firm may thus be regarded as a firm-specific investment as well. The authors then provide evidence on the relation between labor laws and the

[2] Papers that formalize aspects of this argument are Baily (1974), Harris and Holmstrom (1982), Holmstrom (1983), Gamber (1988), and Thomas and Worrall (1988).

[3] The general argument about the correspondence between firm-specific investments and the ex post protection of rents goes back at least to Alchian (1984). Fama and Jensen (1983) refer to the human capital investments of professionals in service firms and deduce that they should accordingly hold residual claims. Pagano and Volpin (2008) refer to this argument, but reference only the general incomplete contracting literature.

strength of family ties in a cross-country study, supporting the view that the legal protection of employment safeguards workers' firm-specific investments.

Both the efficient risk-sharing and the firm-specific investment arguments suggest a role for labor representation to play in facilitating firms to enter long-term commitments with their employees. Both arguments imply that workers are vulnerable to breaches of the implicit contracts by the firm. Workers make wage concessions or investments in firm-specific human capital well before the firm has to honor its side of the bargain, i.e., refrain from layoffs and cutting wages when it suffers adverse shocks. From this perspective, 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 device that ensures workers receive full protection against adverse shocks to employment and wages.*

Providing insurance is costly to firms. Guaranteeing employment against adverse shocks limits firms' ability to reduce payroll in reaction to changes in technology or consumer taste. This increases the fixed components of payroll, increase operating leverage.

Hypothesis 2: *Parity-codetermined firms suffer larger reductions in profitability and valuation from adverse industry shocks than firms without parity-codetermination.*

To cover the costs of the protection, parity-codetermined firms will offer lower wages. Workers have a choice; pay the insurance premium or work elsewhere. More risk averse individuals will accept lower wages, while less risk averse individuals will choose workplaces that do not require the insurance premium.

Hypothesis 3: *Firms with parity codetermination pay on average lower wages than non-parity firms.*

Is the mandated codetermination efficient? If labor representation increases the surplus from production because it enhances worker incentives, then firms should voluntarily invite workers to the

board of directors. However, worker representatives may use their influence not only to protect implicit contracts, but also to prevent restructuring measures necessary for revitalization of the company (Atanassov and Kim, 2009). Moreover, Jensen and Meckling (1979) point out firms almost never provide workers with decision-making rights voluntarily and infer from that labor representation on the board is inefficient and mandating the representation is likely to be harmful.[4]

Levine and Tyson (1990) argue that the competition for workers between firms creates externalities and suggest mandatory worker representation as a means to remove the 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.[5] However, such firms would also have compressed wage structures and would not provide adequate incentives through the threat of dismissals.[6] In smoothly functioning labor markets without mandatory labor representation, firms with labor representation will lose their star performers to firms without labor representation; hence, the equilibrium with labor representation will unravel and only an inferior equilibrium without labor representation will prevail.[7] Accordingly, codetermination has to be mandated to overcome these externalities. Since not all German firms are required to have parity codetermination, the pro-regulation argument predicts:

---

[4] 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.

[5] 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 that provide both cash flow and voting rights enhance productivity, benefiting both workers and shareholders.

[6] 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).

[7] 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.

Hypothesis 4: *Firms with parity codetermination have more compressed wage structure; namely, a narrower gap between top and bottom wage earners.*

Worker protection and managerial entrenchment. One may counter the pro-regulation argument by pointing out inefficiencies stemming from worker participation in governance. 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 when workers have strong legal protection. In such legal environment, management of poorly performing firms shift their allegiance from shareholders to workers, forming worker-management alliance to protect their jobs rather than shareholder value. The German codetermination may help facilitate such worker-management alliance, as labor representatives have influence on top management appointment and retention decisions. Similarly, managers working closely with workers on a daily basis may choose to pursue a "quiet life" by avoiding confrontations with employees (Bertrand and Mullainathan, 2003). Mandatory worker representation may aggravate the quiet life syndrome by further exacerbating management's incentive to please workers.

These worker-management entrenchment hypotheses provide a negative prediction on firm performance. Moreover, if workers enjoy protection against adverse chock through the entrenchment, they are unlikely to offer wage concessions. Thus, under this hypothesis, firms incur the costs of employment protection and suffer the ensuing inefficiencies without any matching benefits.

Hypothesis 5: *Firms with parity codetermination are on average less profitable and valued lower relative to firms without parity codetermination.*

The prediction could be the opposite if the mandated codetermination is efficient. If the codetermination leads to higher productivity and investors capture some of the surpluses from the enhanced productivity, parity codetermined firms will be more profitable and valued higher than firms without parity codetermination.

The previous literature on the valuation and performance effect of the German Codetermination Act is inconclusive. Renaud (2007) surveys 13 studies investigating the impact of codetermination on company performance using different methodological approaches, sample constructions, and performance variables. Some find positive effects, while others find negative or no effects. Five studies investigate various measures of operating performance, with three finding inconsistent or no effects and two as well as Renaud's own study finding positive effects. Four studies use either Tobin's Q or the market-to-book ratio, with two finding negative effects and the other two finding no effect. The two studies finding negative effects compare the difference between one-third codetermination and parity codetermination and show having more labor representatives have negative effects on firm performance. Consistent with these two studies, Petry (2009), a more recent study not included in Renaud's survey, also finds a negative effect that is concentrated among firms experiencing the largest increase in the number of labor representatives (five or more) on the board due to the Codetermination Act. Of the remaining three studies summarized in Renaud, two event studies find no impact of the passage of the codetermination act on stock prices, while Fauver and Fuerst (2006) find positive effects for some subgroups in their sample.

3 Institutional background, data, and empirical design

3.1 Institutional background

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 board 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 the equity capital. The German Stock Corporations 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 one third of the members of the supervisory board have to represent workers. Some worker representatives are elected by the company's workers, and the rest are union representatives. 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.[8] 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.

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

3.2 Data

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.[9] There are 184 such firms, for which we hand collect data on the composition of the supervisory board from annual reports and *Hoppenstedt* company profiles. Stock market data comes from *Datastream*, balance sheet and accounting data from *Worldscope*.

Employment and wage data at the establishment level are obtained from the Institute of Employment Research (IAB). The IAB is the research organization of the German employment agency, the *Bundesagentur für Arbeit* (BA). The BA collects worker and employer contributions to the 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, and so on. The IAB provides detailed establishment level data on industry, location, employment, employee education, age, nationality, and wages.

The IAB reports different industry classifications; unfortunately, none is reported for the entire sample period. We use 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.[10] In our

[9] 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 the 100 DAX firms until 2003 and the 80 DAX and MDAX firms after that.

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

analysis we define an industry based on the first three-digits of the NACE code, which identifies 224 separate economic sub-sectors (groups). The NACE (Rev. 1.1) is available from the IAB database only for 2003 and afterwards. For establishments that exist prior to 2003, we assign the available NACE (Rev. 1.1) classification. This procedure is valid because if an establishment changes its industry classification, it receives a new establishment ID. There are 43,874 establishments changing industry classifications over time.[11] We drop these establishments to avoid the chance of assigning them to incorrect industry codes. We also drop establishment-year observations with missing information on industry classification. These screens yield approximately 33.4 million establishment-year observations on approximately 3.5 million establishments for the sample period 1990 through 2008.

At our request, the IAB matched our sample of listed firms with their establishment-level database (following an automatic procedure) using company name and address information (city, zip code, street, and house number). Additionally, we provide the 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. For example, when establishments are bought or sold in 2007 or 2008, the establishments do not show the new ownership because IAB does not have this information. The matching procedure also does not allow us to analyze restructuring decisions, which involve the divestment or closure of

[11] In a separate robustness check, we also translate industry classifications reported in the earlier sample years into NACE (Rev. 1.1) and obtain very similar results (unreported).

establishments. However, it is not clear why parity firms should take these actions more often than non-parity firms because employees are probably more adversely affected in those cases.

Table 1 contains definitions of all key variables. Table 2 provides summary statistics. Panel A of Table 2 is based on all establishments in our sample, while Panel B shows statistics at the firm level. Panel A also contains statistics for all firm level variables in Panel B, but firms are weighed by their number of establishments. All accounting and market variables are taken from *Worldscope* and *Datastream*, as they are available only at the firm level, not at the establishment level. The IAB does not provide information on any of the firm level variables in Panel B. PPE, Parity, and Sales in Panel A have greater mean values than those in Panel B, because larger firms have more establishments, putting greater weights on the statistics in Panel A. Firm 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 6 months relative to IAB years. Effectively, we assign year-end values from *Worldscope* to June 30 of the same year.

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 do 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.[12] Unfortunately, over our sample period an increasing number of firms stopped reporting these 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. Because of this shortcoming, our employment regressions do not use the breakdown of employees according to educational and vocational qualifications; instead, we use the occupational status of unskilled blue-collar workers, skilled blue-collar workers, and white-collar employees. However, our wage analyses 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.

3.3 Research design

We hypothesize that labor representation in governance is an ex-post enforcement mechanism to ensure the implicit insurance benefits 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 wage cuts of firms with the parity codetermination differently from those with one-third or no labor representation on the supervisory board. This comparison requires a difference-in-differences approach.

[12] The German education system is considerably different from most other developed countries. Historically in Germany only a relatively small fraction of students obtained a highschool (upper secondary school) degree (Abitur), which allows entering a college or university. According to numbers from the German statistical office in 2009 only 25.3% of the German population has such a degree. Even less people (13.6%) obtained a college or university degree. These are categorized as highly qualified employees by IAB. Those who have obtained an upper secondary school degree but no college or university degree are categorized as qualified employees. The usual career path in Germany is to leave school after tenth grade with a secondary general or intermediate school leaving certificate and to enter a vocational training. In 2009 66.3% of the German population had a secondary general or intermediate school leaving certificate and 57.8% had a vocational qualification. All employees who have a vocational training are also classified as qualified employees. In 2009 27.8% of the German population had no vocational qualification attained. All employees who neither have an upper secondary school degree nor a vocational qualification are classified as low-qualified employees.

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 firms with parity codetermination as parity firms and to all others, including those requiring one-third representation, as non-parity firms. Following Gorton and Schmid (2004), we do not distinguish between firms with one-third and no worker representation. This helps preserve the sample size of non-parity firms, which is smaller than that of parity firms. Table 2, Panel B shows 67.4% of our sample firms are parity firms. We focus on whether firm is parity or not, because the fierce debate over the codetermination laws at the time of its passage in 1976 illustrates that parity codetermination was much more controversial and of a major concern to shareholders and managers than one-third representation.[13]

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 to employment in firms that are *not* in our sample. Using these external chocks, we analyze how parity and non-parity firms in our sample respond differently to shocks. We define shocks at the industry level. We count the number of employees in all establishments located in Germany. An industry is in 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 from those of non-

[13] The *Bundestag*, the lower house of the German parliament, passed the codetermination act on March 18, 1976 with only 22 votes against. However, the unions were dissatisfied because they objected to the casting vote of the chairman, which eludes them of the full parity codetermination. Several large corporations and the association of employers were equally dissatisfied because they saw their property rights compromised and challenged the law in the German constitutional court, which decided in favor of the law in 1979. After the ruling the debate subsided.

parity firms. We use the 5% threshold to ensure that shocks are strong enough to have a material effect and frequent enough to permit identification.

These shocks are based on non-sample firms with establishments located in Germany. We do not use non-German European firms with establishments located outside of Germany because Germany seems to follow a different business cycle from other EU countries. For example, at the time of working on this project, 2011-2012, the German economy is booming while most other European countries are in, or at the verge of, a recession. A potential concern with using 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 not that far off from the corresponding numbers for our sample firms in 2006, which are €11.7 bn (€2.0 bn) and 38,700 (9,200), respectively.

We require that a shock is persistent; namely, employment growth in an industry is not positive in the year following the initial shock. Pagano and Pica (2010) distinguish between cash flow shocks and shocks to productivity. Shocks to cash flows may have no implications for future profitability (e.g., a negative cash flow due to a large investment) and, therefore, may not affect current employment levels. By contrast, shocks to productivity may pose persistent shocks to investment opportunities and require adjustments to employment. Our test requires shocks that are likely to lead to a reduction in payroll. Since a shock to productivity is not directly observable, we use persistent shocks to non-sample firms' employment and argue that these shocks may also affect the optimal payroll of our sample firms. We do

not include transitory shocks to employment, because they may reflect temporary fluctuations in demand for products and services, with no direct impact on our sample firms' optimal payroll.[14]

A dummy variable *Shock* is defined, which equals one in any given year when an industry was subject to a persistent shock. We illustrate how *Shock* is defined with Table 3, 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 3 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 3 shows *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

[14] The econometric technique of Guiso, Pistaferri, and Schivardi (2005) uses time series analysis to decompose shocks into a permanent component and a temporary component.

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.[15]

Table 3: Definition of *Shock* using four possible 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

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 estimation results are reported in Table 4. They show years 1993-1998 and 2002-2006 have significant and positive coefficients, whereas all other years are insignificant. This observation is consistent with the long economic downturns in German industry following the post-unification boom in the early 1990s (1990-1992) and with the recession after the burst of the internet bubble in 2000-2001.[16] The shock-periods appear longer because of the lag built into the definition of shocks. The R^2s of these regressions 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 persistency in industry employment downturn

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

[16] Annual real GDP growth rate in Germany during the sample period were: 1988, 3.7%; 1989, 3.9%; 1990, 5.3%; 1991, 5.1%; 1992, 1.9%; 1993, -1.0%; 1994, 2.5%; 1995, 1.7%; 1996, 0.8%; 1997, 1.7%; 1998, 1.9%; 1999, 1.9%; 2000, 3.1%; 2001, 1.5%; 2002, 0.0%; 2003, -0.4%; 2004, 1.2%; 2005, 0.7%; 2006, 3.7%; 2007, 3.3%; 2008, 1.1%.

better, we report results based on the four-year interval. Results based on the two-year interval are robust.

3.3.2 Specification

Our base line regression model is as follows:

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

The dependent variable*, y,* is either the logarithm of the number of employees, the logarithm of the median daily wage, or a measure of profitability or valuation, 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 coefficient of main interest is the slope parameter β on the interaction between *Parity* and *Shock*. It measures the differential impact industry shocks have on employment or wages between parity firms 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 compared to non-parity firms.

Control variables include α_t, year fixed effects; α_i, establishment fixed effects; and X_{ijkt}, a vector of control variables, which includes the logarithms of the number of employees working for a firm*;* the logarithm of sales*;* leverage; and establishment age. It is important to control for size because the parity dummy depends on size: parity codetermination becomes mandatory for firms with 2,000 employees or more. In order to avoid *Parity* picking up higher-order non-linear size effects, we also include the square of the logarithms of the number of employees and sales, both at the firm level. 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 panel regressions with firm performance measures 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 the performance regressions, we include firm fixed effects instead of establishment fixed effects. The control variables are similar, all calculated at the firm level.

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 regression (1) for all employees at the establishment level. Then we separate employees into white-collar, skilled blue-collar, and unskilled blue-collar, and re-estimate the regression for each type. For each of these regressions, we estimate four different specifications including different combinations of controls. All regressions include establishment fixed effects. Reported results are based on *Shock* as defined by the four-year interval. Results based on the two-year interval are qualitatively similar. For employment regressions, we include only establishments with more than 50 employees. Inclusion of establishments with a small number of employees would increase noise; for example, for an establishment with only 10 employees, the loss of one employee accounts for 10% of the work force.

Table 5 reports estimation results for all employees. Specification (1) controls only for establishment fixed effects. As expected, *Shock* has a significantly negative coefficient. More importantly, the variable of main interest, *Shock × Parity,* shows a positive and significant coefficient, consistent with the insurance hypothesis.

The remaining specifications add different combinations of controls; year fixed effects, state fixed effects, establishment age, and firm size. Firm size is an important control because *Parity* is determined by the number of employees a firm has in Germany. Size is measured by sales and the total number of

employees working in establishments located in Germany that belong to the firm. To allow for non-linear size effects, we also include their square terms. The only time varying control at the establishment level is establishment age.

Results reported under specifications (2) through (4) show that, regardless of which combination of controls is used, *Shock × Parity* has a positive coefficient that is economically large and statistically significant, ranging from 0.134 to 0.200. Specification (3), which includes all controls, except for higher-order terms, shows a coefficient of 0.147 on *Shock × Parity*. This suggests that employment in parity-codetermined firms is 14.7% greater in comparison to non-parity firms during shock periods. Note that the non-parity firms often include those with one third of the board seats occupied by worker representatives. Hence, the employment impact implied by the coefficient of *Shock × Parity* may be interpreted as the incremental impact of moving from non-parity codetermination to parity co-determination, not from no employee representation to parity codetermination. In our sample, the number of firm-years with no labor representation is smaller than the number of firm years with one-third co-determination.[17]

The negative coefficient on *Shock* also remains significant regardless of which combination of controls is used, with the magnitude being similar across specifications. 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 perfect 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, regardless of which controls are included. In untabulated robustness checks we obtain similar results if we use the two-year definition of shocks. It appears employees working for parity firms are more or less fully protected against negative industry shocks. An industry-wide decline in

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

employment, on average, leads to a decline of about 15% in employment among non-parity firms, but employees of parity firms are more or less immune to layoffs during shock periods.[18]

This generalization may not apply to all employees, however. If employees are protected from layoffs because the 50% employee representation on the supervisory board helps enforce implicit insurance, the enforcement may vary depending on how closely the employees are aligned with the employee representatives. For example, if worker representatives are mostly drawn from the pool of skilled blue-collar workers and/or white-collar workers, the representatives may focus their efforts on protecting their own kind, namely, fellow skilled blue-collar and/or white-collar workers, rather than unskilled, less educated workers who may have less influence on who gets elected to the board. Moreover, if an important purpose of providing the insurance is to encourage employees to make investments in firm-specific human capital, insurance is more likely to be extended to skilled and better educated employees than to unskilled, less educated workers who can be more easily replaced and whose investment in firm-specific human capital may be worth less. To investigate this potential heterogeneity across different types of employees, we re-estimate regression (1) separately for three types of employees: White-collar employees, skilled blue-collar workers, and unskilled blue-collar workers. This classification is made based on IAB data on workers' occupational status.

Table 6 re-estimates the seven specifications for white-collar employees. The overall results are qualitatively the same as those for all employees in Table 5. The only exception is specification (3), which rejects that *Shock* + *Shock × Parity* = 0 in favor of a positive net effect. Table 7 repeats the same exercise for skilled blue-collar workers. The results are again qualitatively the same as in Table 5. F-statistics do not reject *Shock* + *Shock × Parity*=0, except specification (3), which is significant at the 5% level in favor of

[18] The results get somewhat weaker for the shorter-term definition of shock and if we use higher order controls. Note that in these cases the magnitude and precision of *Shock* goes down as well, suggesting that the definition of *Shock* is noisier and that some of the controls pick up some of the variation in *Shock*.

a positive net effect. White-collar and skilled blue-collar employees seem to be fully protected against industry-wide declines in employment if they have 50% representation on the supervisory board.

This generalization does not apply to workers who are less skilled and less educated. Table 8 re-estimates the seven specifications for unskilled blue-collar workers, yielding no evidence of insurance against negative industry shocks. None of the seven specifications yields a significant coefficient on *Shock × Parity*; furthermore, the coefficient is negative, albeit insignificant, in all specifications. Unlike white-collar and skilled blue-collar workers, there is no evidence these workers are protected against an industry-wide decline in employment. The impact of *Shock* is still mostly significant, although the economic magnitude is somewhat smaller and statistical significance is lower than those for white-collar and skilled blue-collar workers.

Finally, the coefficient on *Parity* codetermination is negative but mostly insignificant, except for the unskilled blue-collar worker subsample, which shows mixed signs. Finally, when we include smaller establishments (those with less than 50 employees) results are very similar, but due to the noise mentioned earlier, the statistical significance decreases, although only marginally.

These subsample analyses yield an interesting new insight. Parity co-determination provides protection against industry-wide declines in employment only for better educated and skilled workers. There is no evidence the same insurance extends to less educated, less skilled workers. There are two potential explanations for the difference. Employee representatives on boards are either union leaders or elected by employees. Union leaders are more likely to come from skilled blue-collar workers and/or white-collar workers. As for elected employee representatives, skilled blue-collar workers and/or white-collar workers may have more influence on who gets elected than unskilled workers. Furthermore, insuring skilled blue-collar workers and white-collar workers against layoffs will encourage investments in firm-specific human capital, enhancing productivity. Unskilled workers are more easily replaceable and probably have less human capital to invest for firm specific purpose.

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 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 influence hypothesis, we examine the relation between wages and parity codetermination. According to the insurance hypothesis, workers receive lower wages in return for job security, i.e., 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 the mandated codetermination.

To distinguish these two hypotheses, we first estimate regressions relating wages to the *Parity* indicator. In these regressions we measure the difference in wages between parity-codetermined firms and all other firms. We then estimate the difference-in-difference by adding *Shock* and *Shock x Parity.*

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) our full set of control variables used in the employment regressions (model 4), (2) the standard set plus the number of employees in the establishment, the median employee age and the percentage of white collar employees. These controls are added for the wage regression because prior research suggests they are important in explaining average employee wages (e.g. Oi and Idson, 1999; Brown and Medoff, 1989). We take logs of all level variables when estimating regressions.

The IAB's wage data breaks down employees according to their educational and vocational qualifications. Thus, we use this information to classify employees to: (1) low-qualified employees, (2) qualified employees and (3) highly qualified employees. An establishment-year observation may not show all categories of employees; for example, if there are no highly qualified employees in an establishment, then no median or quartile wages are reported for that category in the establishment and

therefore fewer observations for those categories in our regressions. We refer to Section 3.2 for the precise definitions of qualification levels and a discussion of the availability of these data.

Table 9 (Panel A) reports estimation results for all employees and each of the three types of fulltime wage earners. The coefficient on *Parity* is negative and around 3%. The results are also highly statistically significant for most specifications. Only for low-qualified employees we do not find a statistically significant *Parity* coefficient, even though the size of the coefficient is similar. The magnitude of the *Parity* coefficient implies that qualified and highly qualified employees in parity firms receive about 3% lower wages in return for insurance.

As mentioned earlier, the IAB uses the occupational status to classify employees into different employee criteria. This classification is different from the criteria used in wage data; hence, we cannot match the qualification levels used for wages with the occupational classification used for employment. Nonetheless, those classified as qualified or highly qualified are most likely skilled blue- and white-collar employees, and are highly unlikely to be classified as unskilled blue-collar workers in the employment data. Thus, the wage results, together with the employment results, suggest that skilled blue- and white-collar employees receive insurance and pay approximately 3% of their wages as a premium. The combined results also imply unskilled blue-collar workers do not receive protection against layoffs during an industry downturn and do not pay an insurance premium. The results for low-qualified employees are somewhat ambiguous because the point estimates for *Parity* is very similar across all qualification levels. However, it is not statistically significant for low-qualified employees. An alternative interpretation is that all employees pay an insurance premium of about 3% in parity firms but only low-qualified employees do not profit from the insurance. The reason for this evident discrimination might be their under representation on the board as discussed above.

Estimated coefficients on controls are consistent with intuition. Unsurprisingly, older employees get paid more. Older and larger firms (measured by sales) also tend to pay higher wages, but larger establishments (measured by number of employees) tend to pay lower wages. The last result is

somewhat surprising given the literature on the employer-size-wage effect (e.g. Brown and Medoff, 1989). However, our sample is considerable skewed towards large firms and we use a number of other firm size controls at the same time. Both facts could explain why we find a negative effect of establishment size on wages.

Earlier regression estimates imply parity firms' employees are protected from layoffs during industry shocks. Are they also protected against cuts in wages? To answer this question, we estimate the difference-in-differences in wages by adding *Shock* and *Shock × Parity* to the regressions in Table 9 (Panel A).

Table 9 (Panel B) reports estimation results consistent with the insurance hypothesis. *Parity* shows significantly negative coefficients, implying that median wages are about 3.2 to 3.6 percent lower for employees of parity firms during normal times (non-shock periods). The *Shock × Parity* term shows positive coefficients in all specifications but is only marginally significant (10%-level) in one specification (4). The F-tests on *Shock* + *Shock × Parity* = 0 can never be rejected (unreported). It appears that employees of parity firms do not get additional insurance against wage cuts. However, the mostly insignificant results on *Shock* imply that firms (parity codetermined and not parity codetermined) in Germany rarely react with significant wage cuts after adverse industry shocks. A potential explanation for this finding might be the industry-wide collective bargaining agreements, which make wages in Germany very downward rigid.

Levine and Tyson's (1990) argument in favor of mandatory worker representation on boards critically hinges on the externality firms face with voluntary worker representation. The externality is caused by the compressed wage structure they conjecture will arise with labor representation; namely, smaller gaps in wages between highly and lowly paid workers. To test this hypothesis, we calculate the difference between 3rd and 1st quartile of the gross average daily wage of all full-time employees at each establishment and scale it by the median. Table 10 relates *Parity* to the scaled interquartile range of wages. The set of control variables is the same as in Table 9. The coefficient of *Parity* is negative and

marginally significant for all employees if we do not control for additional establishment characteristics (Model 1). However, the economic significance is rather small. The interquartile range is only compressed by 1.4%. We repeat the estimation for the three different employee groups separated by their qualification levels and find insignificant negative results for low-qualified and qualified employees and insignificant positive results for highly qualified employees. Although it is possible our estimation is too crude to identify wage compression, the insufficient evidence casts doubt on the empirical validity of the key assumption underlying the pro-regulation argument.

4.3 Performance regressions

Our final analyses are focused on firm profitability and valuation, systematic risk, and major asset sales. The purposes are two-fold. First, we test the prediction that the insurance provided by parity-codetermined firms leads to a higher operating leverage, exposing them to larger reductions in profitability and valuation from an industry shock relative to non-parity firms. Second, we test the worker-management entrenchment hypothesis against the hypothesis that the 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.

All predictions related to firm performance are made at the firm level. We therefore 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. We use ROA and Tobin's Q as our measures of profitability and firm valuation. We estimate difference-in-differences analysis with ROA and the logarithm of Tobin's Q as dependent variables.

To estimate the effect of insurance on operating leverage, our main interest is again in the coefficient of *FirmShock × Parity*. We expect it to be negative. Table 11 reports the results; column (1) and (2) for ROA and column (3) and (4) for Tobin's Q. All four columns show significant negative

coefficients on *FirmShock × Parity*. Economic significance is also large. The estimates for ROA show that profitability falls by about 3 percentage points if all employees of a firm are affected by a shock. This number compares to a mean (median) ROA of 7.5% (6.9%) across all firms in the sample (see Panel B of Table 2). The decline in Tobin's Q ranges from 9.2% to 12.9% if all employees are affected by a shock. The evidence supports our hypothesis that adverse industry shocks affect parity-codetermined firms' performance much more negatively than non-parity firms.

This evidence of higher operating leverage suggests that parity-codetermined firms have a higher systematic risk. Columns (5) and (6) of Table 11 investigate the relation by estimating difference-in-differences for beta. Beta is estimated using the market model and daily stock returns for each calendar year. The coefficient on *Parity* has predicted positive sign but is insignificant. However, the coefficient on *FirmShock × Parity* is positive and significant, implying that the parity-codetermined firm's beta increases markedly during adverse industry shocks.

One way to finance the maintenance of payroll during negative shock periods is to sell assets (Atanassov and Kim, 2009). Thus, we expect parity-codetermined firms to undertake more asset sales to protect their core employees during adverse industry shock periods. To test this prediction, we define 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. However, when we re-estimate the regressions using Probit we find qualitatively similar results.

The results are reported in columns (7) and (8) of Table 11. The coefficient on *FirmShock × Parity* is positive, indicating that parity-codetermined firms undertake more asset sales during shock periods than non-parity firms. The coefficient on *Parity* is also positive, revealing the tendency of parity-codetermined firms to undertake more asset sales even outside shock periods. However, the coefficient on *FirmShock × Parity* is much larger and indicates that some of the insurance provided to workers is paid for by

additional asset sales. The performance estimates in Table 11 also help examine whether parity firms perform better or worse than non-parity firm. If Hypothesis 4 is correct, then the coefficient on *Parity* should be positive in both regressions, because 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*.

The results are somewhat contradictory. The impact of *Parity* on ROA are negative (-1.1% to -1.4%), although it is only marginally significant at the 10%-level if we do not include non-linear size controls. By contrast, the estimates on *Parity* in the regressions of Tobin's Q are positive, but also only marginally significant at the 10%-level if we do not include non-linear size controls. The results suggest that Tobin's Q is higher by 3.1% to 3.4%. Our results for Tobin's Q are therefore consistent with Hypothesis 4, whereas those for ROA tend to contradict it.

5 Conclusions and implications

We find parity-codetermined firms provide employees greater protection against layoffs during adverse industry shocks. The employment protection leads parity firms to suffer bigger declines in firm profitability and valuation and exhibit higher beta during the shock periods than non-parity firms. Parity firms also engage in more major asset sales during shock periods, presumably to maintain the payroll. These phenomena are consistent with both the insurance and the worker-management entrenchment hypothesis. 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 stronger worker influence stemming from parity codetermination leads to worker-management alliance that may harm shareholders. Both hypotheses predict workers employed by parity firms receive protection against adverse shocks.

What distinguishes the two hypotheses is the wage differential between parity and non-parity firms. If the employment protection represents the payoff from the insurance contracts, we expect employees of parity firms to receive lower wages than those working for non-parity firms. This is what we observe. Thus, much of the employment protection seems to be the results of an implicit insurance contract between parity firms and their employees.

However, this generalization does not apply to all employees. A closer examination reveals strong evidence in support of the insurance hypothesis for skilled blue-collar and white-collar employees, but no evidence of insurance for unskilled blue-collar workers.

We find that qualified and highly qualified employees the two groups likely to include most skilled blue-collar and white-collar employees, pay an insurance premium, whereas workers with lower qualifications, a group likely to include most unskilled blue-collar workers, do not. These findings imply the insurance is extended to those whose investment in firm-specific human capital is more valuable, but not to those who are more easily replaceable and have less human capital to invest to help enhance productivity.

We also investigate if parity-codetermined firms are more profitable or more valuable relative to other firms, based on the notion that according to the insurance hypothesis, codetermination should help implement efficiency-enhancing implicit contracts. On this question the evidence is mixed. While parity-codetermined firms are on average less profitable, they are somewhat more valuable.

6 References

Acharya, Viral V., Marco Pagano, and Paolo F. Volpin, 2010, Seeking Alpha: Excess Risk Taking and Competition for Managerial Talent, Working Paper, New York University.

Adams, Renée B., Amir N. Licht, and Lilach Sagiv, 2010, Shareholders and Stakeholders: How Do Directors Decide?, Working Paper, University of Queensland.

Agrawal, Ashwini K., 2008, Corporate Governance Objectives of Labor Union Shareholders: Evidence from Proxy Voting, NYU Stern Working Paper Series.

Alchian, Armen A., 1984, Specificity, Specialization, and Coalitions, Journal of Institutional and Theoretical Economics.

Atanassov, Julian, and E. Han Kim, 2009, Labor and Corporate Governance: International Evidence from Restructuring, Journal of Finance 64, 341-374.

Azariadis, Costas, 1975, Implicit Contracts and Underemployment Equilibria, Journal of Political Economy 83, 1183-1202.

Baily, Martin Neil, 1974, Wages and Employment under Uncertain Demand, The Review of Economic Studies 41, 37-50

Benelli, Giuseppe, Claudio Loderer, and Thomas Lys, 1987, Labor Participation in Corporate Policy-Making Decisions: West Germany's Experience with Codetermination, Journal of Business 60, 553-575.

Benmelech, Efraim, Nittai Bergman, and Ricardo Enriquez, 2010, Negotiating with Labor under Financial Distress, Working Paper, Harvard University.

Berk, Jonathan B., Richard Stanton, and Josef Zechner, 2010, Human Capital, Bankruptcy, and Capital Structure, Journal of Finance 65, 891-926.

Bertrand, Marianne, and Sendhil Mullainathan, 2003, Enjoying the Quiet Life? Corporate Governance and Managerial Preferences, Journal of Political Economy 111, 1043-1075.

Bhagat, Sanjay, Andrei Shleifer, and Robert Vishny, 1990, Hostile Takeovers in the 1980's: The Return to Corporate Specialization, Brookings Papers on Economic Activity; Microeconomics 1-84.

Botero, Juan C., Simeon Djankov, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer, 2004, The regulation of labor, Quarterly Journal of Economics 119, 1339-1382.

Bronars, Stephen G., and Donald R. Deere, 1991, The Threat of Unionization, the Use of Debt, and the Preservation of Shareholder Wealth, The Quarterly Journal of Economics 106, 231-254.

Brown, Charles and James Medoff, 1989, The employer size wage effect, Journal of Political Economy 97, 1027-1059.

Cronqvist, Henrik, Fredrik Heyman, Mattias Nilsson, Helena Svaleryd, and Jonas Vlachos, 2009, Do Entrenched Managers Pay Their Workers More?, Journal of Finance 64, 309-339.

Denis, D., and T. Kruse, 2000, "Managerial Discipline and Corporate Restructuring Following Performance Declines" Journal of Financial Economics 55, March, 391-424.

Faleye, Olubunmi, Vikas Mehrotra, and Randall Morck, 2006, When Labor Has a Voice in Corporate Governance, Journal of Financial and Quantitative Analysis 41, 489-510.

Fama, Eugene F., and Michael C. Jensen, 1983, Agency Problems and Residual Claims, Journal of Law and Economics 26, 327-349.

Fauver, Larry, and Michael E. Fuerst, 2006, Does Good Corporate Governance Include Employee Representation? Evidence from German Corporate Boards, Journal of Financial Economics 82, 673-710.

FitzRoy, Felix R., and Kornelius Kraft, 1993, Economic Effects of Codetermination, The Scandinavian Journal of Economics 95, 365-375.

Franks, Julian, and Colin Mayer, 2001, Ownership and Control of German Corporations, Review of Financial Studies 14, 943-977.

Furubotn, Eirik G., 1988, Codetermination and the Modern Theory of the Firm: A Property-Rights Analysis, The Journal of Business 61, 165-181.

Gamber, Edward N., 1988, Long-Term Risk-Sharing Wage Contracts in an Economy Subject to Permanent and Temporary Shocks, Journal of Labor Economics 6, 83-99.

Gorton, Gary, and Frank A. Schmid, 2004, Capital, Labor, and the Firm: A Study of German Codetermination, Journal of the European Economic Association 2, 863-905.

Guertzgen, Nicole, 2009, Wage Insurance within German Firms: Do Institutions Matter?, ZEW - Centre for European Economic Research Discussion Paper.

Guiso, Luigi, Luigi Pistaferri, and Fabiano Schivardi, 2005, Insurance within the Firm, Journal of Political Economy 113, 1054-1087.

Guertzgen, Nicole, 2009, Wage Insurance within German Firms: Do Institutions Matter?, ZEW - Centre for European Economic Research Discussion Paper.

Harris, Milton, and Bengt Holmstrom, 1982, A Theory of Wage Dynamics, Review of Economic Studies 49, 315-333.

Holmstrom, Bengt, 1983, Equilibrium Long-Term Labor Contracts, The Quarterly Journal of Economics 98, 23-54.

Jensen, Michael C., and William H. Meckling, 1979, Rights and Production Functions: An Application to Labor-Managed Firms and Codetermination, Journal of Business 52, 469-506.

Kang, Jun-Koo, and Anil Shivdasani, 1997, Corporate Restructuring During Performance Declines in Japan, Journal of Financial Economics 46, 29-65.

Kaplan, Steven N., 1994, Top Executives, Turnover, and Firm Performance in Germany, Journal of Law, Economics, and Organization 10, 142 - 159.

Kim, E. Han, 2009, Corporate Governance and Labor Relations, Journal of Applied Corporate Finance 21:1

Kim, E. Han, and Paige Parker Ouimet, 2008, Employee Capitalism or Corporate Socialism? Broad-Based Employee Stock Ownership, Working Paper, University of Michigan.

Klasa, Sandy, William F. Maxwell, and Hernán Ortiz-Molina, 2009, The Strategic Use of Corporate Cash Holdings in Collective Bargaining with Labor Unions, Journal of Financial Economics 92, 421-442.

Lamp, Felix, 2009, The Costs of Different European Corporate Governance Legislation: Evidence from the New Legal Form - Societas Europaea, Working Paper, Erasmus University.

Lee, David, and Alexandre Mas, 2009, Long-Run Impacts of Unions on Firms: New Evidence from Financial Markets, 1961-1999, NBER Working paper.

Levine, David I., and Laura D'Andrea Tyson, 1990, Participation, Productivity and the Firm's Environment, in Alan S. Blinder, ed.: Paying for Productivity: A Look at the Evidence (Brookings Institution, Washington, D.C.).

Mueller, Holger, and Thomas Philippon, 2006, Concentrated Ownership and Labour Relations, CEPR Discussion Paper.

Oi, Walter Y. and Todd L. Idson, 1999, Firm Size and Wages, Handbook of Labor Economics, Vol. 3, edited by O. Ashenfelder and D. Card, 2165-2214.

Pagano, Marco, and Giovanni Pica, 2010, Finance and Employment, Working Paper, University of Naples Frederico II.

Pagano, M., and P. F. Volpin, 2005, Managers, Workers, and Corporate Control, The Journal of Finance 60, 841-868.

Pagano, Marco, and Paolo Volpin, 2006, Shareholder Protection, Stock Market Development, and Politics, Journal of the European Economic Association 4, 315-341.

Pagano, Marco, and Paolo F. Volpin, 2008, Labor and Finance, Working Paper, London Business School.

Perotti, Enrico C., and Kathryn E. Spier, 1993, Capital Structure as a Bargaining Tool: The Role of Leverage in Contract Renegotiation, The American Economic Review 83, 1131-1141.

Perotti, Enrico C., and Ernst-Ludwig von Thadden, 2006, The Political Economy of Corporate Control and Labor Rents, Journal of Political Economy 114.

Petry, Stefan, 2009, The Wealth Effects of Labor Representation on the Board - Evidence from German Codetermination Legislation, Working Paper, University of Cambridge.

Ray, Debraj, 2002, The Time Structure of Self-Enforcing Agreements, Econometrica 70, 547-582.

Renaud, Simon, 2007, Dynamic Efficiency of Supervisory Board Codetermination in Germany, Labour 21, 689-712.

Rosett, Joshua G., 1990, Do Union Wealth Concessions Explain Takeover Premiums?: The Evidence on Contract Wages, Journal of Financial Economics 27, 263-282.

Shleifer, Andrei, and Lawrence H. Summers, 1988, Breach of Trust in Hostile Takeovers, in Alan J. Auerbach, ed.: Corporate Takeovers: Causes and Consequences (University of Chicago Press, Chicago, London).

Shleifer, Andrei, and Robert W. Vishny, 1997, A Survey of Corporate Governance, Journal of Finance 52, 737-783.

Simintzi, Elena, Vikrant Vig, and Paolo F. Volpin, 2010, Labor and Capital: Is Debt a Bargaining Tool?, Working Paper, London Business School.

Sraer, David, and David Thesmar, 2007, Performance and Behavior of Family Firms: Evidence from the French Stock Market, Journal of the European Economic Association 5, 709-751.

Thomas, Jonathan, and Tim Worrall, 1988, Self-Enforcing Wage Contracts, Review of Economic Studies 55, 541-553.

Verwijmeren, Patrick, and Jeroen Derwall, 2010, Employee Well-Being, Firm Leverage, and Bankruptcy Risk, Journal of Banking & Finance 34, 956-964.

7 Tables

Table 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]	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 3.3.1.	IAB
DailyWageP25	1st quartile of gross average daily wage for all full-time employees	IAB
DailyWageP50	Median of gross average daily wage for all full-time employees	IAB
DailyWageP75	3rd quartile of gross average daily wage for all full-time employees	IAB
TobinsQ	= (market capitalization [08001] + total assets [02999] – common equity [03501]) / total assets	Worldscope

Table 2: 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 on 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
#Emplyees	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
#BlueCollar	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: 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.

All industry years	**(1)**	**(2)**
Dependent variable	**Industry shock dummy**	
Shock definition	2 years	up to 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 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.

	(1)	(2)	(3)	(4)
Dependent variable	**log number of employees**			
Shock × Parity	0.2000	0.1700	0.1470	0.1380
	(3.00)	*(3.09)*	*(2.37)*	*(2.20)*
Shock	-0.1860	-0.1390	-0.1370	-0.1270
	(-3.16)	*(-2.85)*	*(-2.54)*	*(-2.51)*
Parity	-0.1780	-0.0400	-0.1070	-0.1030
	(-1.48)	*(-0.56)*	*(-1.08)*	*(-1.12)*
LogEstAge		0.1200	0.1010	0.1020
		(4.17)	*(4.05)*	*(4.13)*
LogSales		0.1040	0.0110	0.1090
		(2.31)	*(0.29)*	*(0.34)*
Leverage		-0.1740	-0.0710	-0.0670
		(-2.36)	*(-1.08)*	*(-0.79)*
LogFirmEmployees			0.4080	0.6490
			(3.93)	*(1.48)*
LogSales²				-0.0020
				(-0.29)
LogFirmEmployees²				-0.0130
				(-0.49)
adj. R²	0.908	0.916	0.919	0.920
Observations	52,756	51,188	51,188	51,188
F-Test: Shock + Shock × Parity=0	0.675	0.244	0.729	0.714
Year F.E.	No	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table 6: Employment – white-collar employees

This table presents results for OLS regressions with log number of white-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.

	(1)	(2)	(3)	(4)
Dependent variable	**log number of white-collar employees**			
Shock × Parity	0.2360	0.1650	0.1450	0.1570
	(3.08)	*(2.05)*	*(1.96)*	*(2.04)*
Shock	-0.1870	-0.1090	-0.1080	-0.1190
	(-2.71)	*(-1.45)*	*(-1.60)*	*(-1.75)*
Parity	-0.2550	-0.1500	-0.2090	-0.2080
	(-2.15)	*(-1.57)*	*(-1.92)*	*(-1.92)*
LogEstAge		0.1580	0.1410	0.1400
		(6.06)	*(5.45)*	*(5.43)*
LogSales		0.1370	0.0550	-0.2520
		(2.28)	*(0.94)*	*(-0.61)*
Leverage		-0.0510	0.0400	0.0170
		(-0.50)	*(0.42)*	*(0.16)*
LogFirmEmployees			0.3600	0.4350
			(3.22)	*(1.09)*
LogSales²				0.0070
				(0.68)
LogFirmEmployees²				-0.0040
				(-0.15)
adj. R²	0.928	0.942	0.943	0.943
Observations	52,756	51,188	51,188	51,188
F-Test: Shock + Shock × Parity=0	0.113	0.017	0.148	0.146
Year F.E.	No	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table 7: Employment – skilled blue-collar employees

This table presents results for OLS regressions with log number of blue-collar employees (skilled workers) 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.

	(1)	(2)	(3)	(4)
Dependent variable	**log number of blue-collar employees**			
Shock × Parity	0.2030	0.1690	0.1480	0.1300
	(3.68)	*(3.43)*	*(2.65)*	*(2.42)*
Shock	-0.1720	-0.1200	-0.1180	-0.0990
	(-3.76)	*(-2.79)*	*(-2.45)*	*(-2.26)*
Parity	-0.2030	-0.0730	-0.1340	-0.1290
	(-1.76)	*(-0.91)*	*(-1.24)*	*(-1.32)*
LogEstAge		0.1670	0.1490	0.1500
		(4.01)	*(3.92)*	*(4.01)*
LogSales		0.0840	0.0000	0.2500
		(1.98)	*(-0.01)*	*(0.79)*
Leverage		-0.1980	-0.1040	-0.0900
		(-2.32)	*(-1.32)*	*(-1.02)*
LogFirmEmployees			0.3730	0.6970
			(4.38)	*(1.80)*
LogSales²				-0.0060
				(-0.75)
LogFirmEmployees²				-0.0180
				(-0.78)
adj. R²	0.886	0.910	0.912	0.912
Observations	52,756	51,188	51,188	51,188
F-Test: Shock + Shock × Parity=0	0.319	0.040	0.260	0.251
Year F.E.	No	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table 8: Employment – unskilled blue-collar employees

This table presents results for OLS regressions with log number of unskilled employees (non-formally qualified 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.

	(1)	(2)	(3)	(4)
Dependent variable	**log number of unskilled blue-collar employees**			
Shock × Parity	-0.0440	-0.0300	-0.0190	-0.0260
	(-0.54)	*(-0.58)*	*(-0.36)*	*(-0.45)*
Shock	-0.0880	-0.0730	-0.0940	-0.0880
	(-1.44)	*(-1.78)*	*(-2.01)*	*(-1.86)*
Parity	-0.1670	0.0440	-0.0250	-0.0270
	(-1.87)	*(0.88)*	*(-0.47)*	*(-0.46)*
LogEstAge		0.2500	0.2310	0.2310
		(7.04)	*(7.02)*	*(7.12)*
LogSales		0.1150	0.0210	0.3350
		(1.72)	*(0.29)*	*(0.62)*
Leverage		-0.0260	0.0750	0.0980
		(-0.27)	*(0.79)*	*(0.77)*
LogFirmEmployees			0.4150	0.3080
			(2.39)	*(0.47)*
LogSales²				-0.0070
				(-0.53)
LogFirmEmployees²				0.0060
				(0.13)
adj. R^2	0.881	0.901	0.902	0.902
Observations	52,751	51,183	51,183	51,183
F-Test: Shock + Shock × Parity=0	0.051	0.034	0.010	0.010
Year F.E.	No	Yes	Yes	Yes
Establishment F.E.	Yes	Yes	Yes	Yes

Table 9 Panel A: 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: Median wage of…	**All Employees**		**Employees w/o educational/ vocational qualifications**		**Employees with educational/ vocational qualifications**		**Employees with higher educational qualifications**	
Parity	-0.0310	-0.0330	-0.0320	-0.0330	-0.0290	-0.0320	-0.0310	-0.0310
	(-3.09)	*(-3.78)*	*(-1.57)*	*(-1.64)*	*(-3.00)*	*(-3.70)*	*(-2.49)*	*(-2.56)*
LogEstAge	0.0490	0.0490	0.0310	0.0300	0.0500	0.0510	0.0590	0.0600
	(3.56)	*(3.42)*	*(1.89)*	*(1.78)*	*(3.61)*	*(3.49)*	*(6.16)*	*(6.04)*
LogSales	-0.2120	-0.1940	-0.0590	-0.0470	-0.2430	-0.2290	-0.0220	-0.0190
	(-2.27)	*(-2.38)*	*(-0.61)*	*(-0.54)*	*(-2.69)*	*(-2.85)*	*(-0.39)*	*(-0.33)*
Leverage	-0.0240	-0.0230	-0.0750	-0.0730	-0.0160	-0.0150	0.0050	0.0060
	(-0.93)	*(-0.91)*	*(-2.90)*	*(-2.88)*	*(-0.65)*	*(-0.64)*	*(0.23)*	*(0.29)*
LogFirmEmployees	0.0330	0.0680	0.0180	0.0260	0.0400	0.0650	-0.0610	-0.0520
	(0.35)	*(0.79)*	*(0.17)*	*(0.24)*	*(0.45)*	*(0.79)*	*(-1.03)*	*(-0.88)*
$LogSales^2$	0.0060	0.0050	0.0020	0.0020	0.0060	0.0060	0.0010	0.0010
	(2.49)	*(2.61)*	*(0.91)*	*(0.86)*	*(2.82)*	*(3.00)*	*(0.74)*	*(0.70)*
$LogFirmEmployees^2$	-0.0030	-0.0040	-0.0020	-0.0020	-0.0030	-0.0030	0.0040	0.0030
	(-0.49)	*(-0.74)*	*(-0.29)*	*(-0.30)*	*(-0.49)*	*(-0.65)*	*(0.93)*	*(0.87)*
Log#Employees		-0.0330		-0.0160		-0.0320		-0.0090
		(-4.05)		*(-1.88)*		*(-3.82)*		*(-1.62)*
LogMedianEmplAge		0.1850		0.2070		0.1950		0.0710
		(3.99)		*(4.89)*		*(5.67)*		*(2.47)*
RatioWhiteCollar		0.1540		0.0480		0.0730		0.0230
		(2.89)		*(0.77)*		*(1.60)*		*(1.04)*
adj. R^2	0.942	0.946	0.802	0.804	0.926	0.93	0.826	0.827
Observations	52,395	52,395	43,472	43,472	52,250	52,250	39,675	39,675

Table 9 Panel B: Wages – all, low-qualified, qualified, and highly qualified employees (with shock and interaction)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: Median wage of…	**All Employees**		**Employees w/o educational/ vocational qualifications**		**Employees with educational/ vocational qualifications**		**Employees with higher educational qualifications**	
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

Table 10: Wage compression

This table presents results for OLS regressions with the scaled interquartile range of wages as dependent variable. It is defined as the difference of the 3rd and 1st quartile scaled by the median of 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: 3rd - 1st quartile wage	**All Employees**		**Employees w/o educational/ vocational qualifications**		**Employees with educational/ vocational qualifications**		**Employees with higher educational qualifications**	
Parity	-0.0140	-0.0100	-0.0010	0.0020	-0.0120	-0.0090	0.0220	0.0210
	(-1.81)	*(-1.59)*	*(-0.09)*	*(0.16)*	*(-1.46)*	*(-1.24)*	*(1.35)*	*(1.32)*
LogEstAge	0.0210	0.0190	0.0090	0.0050	0.0200	0.0190	-0.0160	-0.0190
	(2.81)	*(2.30)*	*(1.31)*	*(0.71)*	*(2.51)*	*(2.18)*	*(-2.79)*	*(-3.19)*
LogSales	0.0910	0.0890	-0.0770	-0.0790	0.0500	0.0490	0.1400	0.1300
	(1.35)	*(1.51)*	*(-1.07)*	*(-1.18)*	*(0.79)*	*(0.87)*	*(2.43)*	*(2.43)*
Leverage	0.0310	0.0320	0.0130	0.0150	0.0170	0.0180	0.0110	0.0110
	(1.45)	*(1.65)*	*(0.83)*	*(1.00)*	*(1.00)*	*(1.13)*	*(0.52)*	*(0.52)*
LogFirmEmployees	-0.1270	-0.1320	-0.0600	-0.0690	-0.0890	-0.0880	-0.0910	-0.1080
	(-2.44)	*(-2.65)*	*(-0.98)*	*(-1.11)*	*(-1.62)*	*(-1.65)*	*(-1.47)*	*(-1.95)*
LogSales²	-0.0020	-0.0020	0.0020	0.0020	-0.0010	-0.0010	-0.0030	-0.0030
	(-1.41)	*(-1.60)*	*(0.99)*	*(1.09)*	*(-0.84)*	*(-0.94)*	*(-2.37)*	*(-2.41)*
LogFrimEmployees²	0.0090	0.0080	0.0030	0.0030	0.0060	0.0060	0.0060	0.0060
	(2.66)	*(2.84)*	*(0.96)*	*(0.78)*	*(1.81)*	*(1.79)*	*(1.66)*	*(2.02)*
Log#Employees		0.0280		0.0390		0.0200		0.0360
		(3.72)		*(5.87)*		*(3.24)*		*(7.02)*
LogMedianEmplAge		-0.1070		-0.0830		-0.0970		-0.1160
		(-4.27)		*(-3.12)*		*(-3.45)*		*(-4.34)*
RatioWhiteCollar		0.1000		0.1460		0.1110		0.0650
		(2.55)		*(4.21)*		*(5.09)*		*(2.65)*
adj. R²	0.745	0.751	0.262	0.266	0.681	0.686	0.487	0.492
Observations	52,395	52,395	43,472	43,472	52,250	52,250	39,675	39,675

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. 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.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	**ROA**		**Log TobinsQ**		**CAPM beta**		**Net PPE dummy**	
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)*
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