Employee Representation and the Manager-to-Worker Pay Ratio[☆]

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Abstract

German law mandates that firms with more than 2,000 domestic employees have half of the supervisory board seats held by employee representatives. We investigate whether the increased employee participation reduces pay inequality. Our empirical strategies combine regression discontinuity with a law change that granted employees additional influence over compensation. Surprisingly, strengthened employee voice leads to *higher* pay ratio between managers and workers, and this is driven by higher managerial compensation. Employees are better off due to increased job security. Our results are consistent with the existence of a manager-worker alliance and suggest that employee representation may not reduce pay inequality.

Keywords: Employee representation, manager-to-worker pay ratio, managerial compensation, job security, manager-worker alliance

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> If hourly workers at Walmart were well represented on its board, I doubt you would see the CEO of Walmart making over a thousand times more than its average worker.
>
> Bernie Sanders, May 2019 (Washington Post: link)

1. Introduction

The rise of pay inequality within firms and the policy proposals to reduce it have stimulated a debate in the media and the academia. Policy makers increasingly suggest that workers be given more rights to intervene in corporate governance to restrain the pay gap between the top manager and the average worker.[1] However, relatively little is known about the effects of direct employee participation in firms' governance on the within-firm pay structure.

One approach to give employees a direct voice within the firm is to grant them seats on the corporate board, and this can affect the ratio between managers' compensation and the average worker's pay (the "pay ratio") in two ways. Naturally, we may expect employees on the board to decrease the pay ratio by putting a cap on managerial compensation and/or by increasing employee wages due to fairness considerations (Akerlof and Yellen, 1990; Fehr and Schmidt, 1999; Breza, Kaur, and Shamdasani, 2018). However, an important alternative is that employees may form an alliance with managers that benefits both parties (Pagano and Volpin, 2005). This alternative predicts that employees set generous executive compensation in exchange for benefits, such as higher wages or better job security. The effect on the pay ratio will then depend on the relative magnitudes of the changes at the top and at the bottom. Given that workers' wages are often influenced by unions at the industry, rather than firm, level, and workers may value job insurance over a higher pay (Kim et al. (2018)), the change at the top can dominate, implying that the pay

[1] For instance, the U.S. Accountable Capitalism Act, a bill introduced by senator Elizabeth Warren, proposed to grant employees 40 percent of the seats on corporate boards (link). Senator Bernie Sanders proposed a similar plan in May 2019 (link), and the Labour Party in the U.K. presented a proposal in 2018 to grant workers one-third of the board seats in large firms (link).

ratio can rise with increased employee representation. This channel is important, because it suggests employee representation may not necessarily achieve the policy goal of reducing pay inequality.

To shed light on the empirical relationship between the pay ratio and employee representation, we examine the unique setting of Germany, where employee representation on corporate boards is substantial and legally mandated. German firms operate under a two-tier board system, with a management board that is responsible for managing the firm[2] and a supervisory board that is responsible for appointing members to the management board, monitoring, and setting the compensation for the management board members, i.e., the supervisory board is similar to the board of directors in the U.S.. The German co-determination law passed in 1976 requires that in companies with more than 2,000 domestic employees (DEs) half of the seats on the supervisory board have to be held by employee representatives, while the other half of the supervisory board members represent the shareholders, an arrangement also known as "parity employee representation" (PER). In addition to the substantial participation of employees in corporate governance, the German context is also useful for our purpose because we can directly measure the average employee wage from the firms' profit and loss statements, and this enables us to calculate pay ratios going back to the 1990s.

Our first empirical strategy takes advantage of the discontinuous increase in the voting rights of employees around the threshold of 2,000 DEs. A graphical analysis examining the firms with between 1,500 DEs and 2,500 DEs provides the first evidence for a discontinuous increase in the pay ratio at the threshold: Firms slightly above the threshold have a pay ratio that is one-third higher compared to firms slightly below the threshold. We confirm this result in a formal regression discontinuity (RD) approach that controls for firm characteristics and potentially different effects of the number of DEs on either side of the threshold. We then evaluate the potential problems of applying RD to the setting of co-determination

[2]Throughout this paper, we refer to members of the management board as managers.

and find that the main results are robust using sub-samples matching PER firms and non-PER firms on asset size and profitability, which reduces concerns over different firm characteristics on the two sides of the threshold; and that the ratio of DEs to total assets or to total employees is flat and smooth around the threshold, which mitigates concerns over manipulation, in particular, over-staffing.

The lack of evidence for manipulation might seem surprising as managers could profit from PER, but there exist a few frictions that can hinder upward manipulation. The work council ("Betriebsrat") that consists of employees and represents their interests at the establishment-level has to agree when new employees are hired. Because over-staffing can lead to layoffs in the existing workforce, the work council can block hiring. In addition, over-staffing may lower profits and reduce the variable compensation to managers (part of their compensation is variable and can be performance-linked), or lead to terminations of managers' contracts if shareholders suspect manipulation (shareholders have an incentive to stop manipulation because that would reduce their vote share).

Our second and third identification strategies rely on a difference-in-differences (DiD) estimation around a compensation law change in Germany. Before 2009, managerial compensation could be determined by a subset of the supervisory board called the "compensation committee." Even in PER firms, employees were frequently under-represented on the compensation committee. In June 2009, however, the German parliament passed the "VorstAG" which mandated that managerial compensation must be decided by the whole supervisory board. This law eliminated the under-representation problem and increased the influence of employees in PER firms over managerial compensation. We find that the VorstAG lead to an increase in the pay ratio by between 10% and 18% in firms with PER, compared with firms below the PER threshold. We further combine the RD and DiD methodologies for a "discontinuity in differences" estimation, and show that firms with just above 2,000 DEs before the law's passage increased the pay ratio by more, compared with firms just below the threshold.

We break down the pay ratio into its components to analyze the effect of PER on managerial compensation and the average worker wage. For managers, we find that PER leads to a roughly one-third increase in compensation once a firm crosses the 2,000 DEs threshold, and that the VorstAG of 2009 leads to an increase in the managerial compensation level by slightly more than ten percent. Turning to workers' wages, we find no effect of PER. Though the lack of an effect on wages is counter-intuitive, it is consistent with results on new unionization using U.S. data (DiNardo and Lee, 2004; Frandsen, 2021).

These results are consistent with a manager-worker alliance hypothesis since managers are better off as a result of employee board representation, and this is consistent with news media speculation.*Süddeutsche Zeitung*, a leading newspaper in Germany, writes: "Due to rapid increases in management salary [...] there is a suspicion that union members cooperate with the management [...] How should employee representatives be critical of the management if it can pay off to be friendly?"[3]

Despite no effect on wages, workers could still be better off as a result of PER, because they may be willing to sacrifice higher pay for better job insurance. Anecdotal evidence for this view comes from a *Business Week* article, which states, that "CEOs and top managers depend on votes from the labor reps to be reappointed. Instead of making tough decisions on restructuring or job cuts, German managers are inclined to delay or avoid change and instead curry favor with union bosses sitting on their boards, often to the detriment of their companies."[4] Kim, Maug, and Schneider (2018) provide empirical evidence for higher employment protection in PER, which we confirm in our dataset.

This paper contributes to the literature on wage inequality within firms. This literature has so far focused on the determinants for the pay ratio, for example, the relative bargaining power between the CEO and lower-level employees (Faleye et al. (2013)), and firm size, growth, performance (Mueller et al. (2017a) and Mueller et al.

[3]"Die Mittäter," *Süddeutsche Zeitung*, April 16, 2012).
[4]"The Real Scandal at Volkswagen," *Business Week*, July 18, 2005.

(2017b)). In contrast, our paper examines the effect of employee board representation on the pay ratio, thereby directly shedding light on the potential effects of policy proposals that push for more labor participation in corporate governance. As far as we know, despite its policy relevance, this question has not been answered by the previous literature. Our results suggest that because of the potential alliance between managers and workers and rigidity in worker wages, employee board representation may affect pay ratios in the opposite direction than originally intended.

Our paper is also related to other empirical evidence on the manager-worker alliance. The theoretical foundation is provided by Pagano and Volpin (2005), who demonstrate that managers and employees may have a natural tendency to collude, to jointly benefit at the shareholders' cost. Empirical studies that test the existence of such an alliance have mostly focused on labor unions and employee stock ownership plans (ESOPs).[5] We deviate from the literature by studying a setting where employees have direct and large influence over corporate governance. With high voting rights, employees may be more likely to contradict management, compared with the cases where they have only indirect or small influence. Therefore, it is a new result to the literature that the manager-worker alliance can prevail even under a high degree of worker participation in corporate governance.

Last but not least, we want to point out that there are many important works on the co-determination in Germany that proceed our research. Our main contributions to this literature are to examine the pay ratio as a new outcome variable, and to introduce a new source of exogenous variation in the time series from before to after a compensation law reform in 2009.

On the effects of co-determination, Gorton and Schmid (2004), Fauver and Fuerst (2006), and Petry (2018) study the effect on performance and valuation, with mixed results. Fauver and Fuerst and Gorton and Schmid have touched upon the relationship between PER and managerial compensation, but compensation is not the focus

[5] Chaplinsky and Niehaus (1994) and Rauh (2006) show that ESOPs reduce the probability of a takeover, Kim and Ouimet (2014) analyze how ESOPs affect wages, and Masulis, Wang, and Xie (2019) report that ESOPs increase managerial entrenchment.

of these papers - nor do these papers examine the pay ratio as an outcome or the manager-worker alliance as a potential channel.[6]

On the methodology, previous studies have used the co-determination threshold of 2,000 domestic employees. In particular, Gorton and Schmid (2004) implement an early version of RD, and Lin et al. (2018), and Kim et al. (2018) perform full-fledged RD estimates, looking at the leverage ratio and job security as outcome variables. As we mentioned earlier, though manipulation is not detectable in the data, using the RD approach itself is not free of concerns in the setting of co-determination. Part of our contribution therefore is to supplement the RD with difference-in-differences, therefore further establishing the causal impact of employee representation on the pay structure.

2. Background

2.1. Co-determination in Germany

German firms typically operate under a two-tier board: a "management board" ("Vorstand"), which consists of the managers, and a "supervisory board" ("Aufsichtsrat"), which is similar to the board of directors in U.S. firms. The supervisory board is responsible for appointing and monitoring members of the management board, as well as deciding the compensation package for the management board.

The law of co-determination ("Mitbestimmungsgesetz," MitBestG) regulates employee representation on the boards of German firms. The roots of this law go back to the period after World War II when in 1951, employee representation was mandated for firms in the iron and steel industry ("Montanindustrie"). In 1976, employee representation on boards became mandatory for other industries as well. This general "parity co-determination" law requires that companies with more than

[6]Based on data for the year 2003, Fauver and Fuerst infer in untabulated results that managerial compensation is lower in PER firms. Gorton and Schmid report that managerial compensation is negatively related to firm performance in PER firms. In unreported results, we also find that managerial compensation responds less to firm performance in codetermined firms. This finding could be interpreted as another benefit for employees as it is consistent with employees' lower risk-taking preferences.

2,000 DEs have half of their supervisory board consist of employee representatives (PER), while the other half of the board members are elected by shareholders. For firms with fewer than 2,000 DEs but more than 500 DEs, another law ("Drittelbeteiligungsgesetz," DrittelbG) requires one-third of the supervisory board to be employee representatives. There are ways for firms to circumvent the one-third co-determination requirement, for example, through holding structures. However, it is not possible to avoid PER if a firm passes the 2,000 DEs threshold.

Employee representatives have half of the voting rights in firms with PER, and when the representatives cast homogeneous votes, they may dominate in votes on corporate policies.[7] As a result, employees are highly influential in corporate policies for firms above the 2,000 DEs threshold. Firms below the threshold, in contrast, are controlled by the shareholders, because workers have at most one-third of the voting rights. All employee representatives are elected by the workforce. Company employees and union members can serve as employee representatives.[8] The tenure for employee representatives is at most four years (the same as owner representatives). However, employee representatives can be dismissed during their tenure with elections.

Virtually all firms in the sample comply with the law. After a company has grown above or shrunk below the threshold, the composition of the supervisory board has to be adjusted.[9] Specifically, the law mandates that the management board of the company has to start the transition process ("Überleitungsverfahren"). The details of this process are regulated in §97 and following of the German Stock Corporation Act ("Aktiengesetz," AktG). Because this process and the election of employee representatives is slow, there is a time lag between the crossing of the

[7]The dominance of employee representatives in voting is not guaranteed. In the case of a tie between employee and shareholder representatives, the head of the supervisory board—who usually is a shareholder representative—has a double vote. Although large block holdings are common in Germany, shareholders have more diverse interests relative to workers, and thus, are less likely to cast homogeneous votes. We investigate the impact of ownership concentration on our results in Appendix A.

[8]For details of the election process, see Weiss and Schmidt (2008), p.254.

[9]The law does not forbid firms below the threshold from establishing PER. However, this is very rare in practice because this would require that owners' representatives voluntarily give up control.

threshold and the actual adjustment of the supervisory board. In the data, we find that more than 90 percent of our observations are in line with the law's regulations if we allow for an adjustment period of up to two years.

2.2. The determination of managerial compensation and the 2009 law change

Before 2009, managerial compensation was usually determined by the compensation committee.[10] The compensation committee, as any other committee, consists of a subset of supervisory board members. The law mandates that the supervisory board selects the committee members, but there is no legal requirement for parity employee representation. In fact, employees were frequently under-represented on the compensation committee. This under-representation reduces the influence of employees on managerial compensation. During an official evaluation of the co-determination law by the German government ("Biedenkopf Commission"), a key proposal was that committees *should* consist of an equal number of owners' and employees' representatives, but this proposal was not realized.

In mid-2009, Germany passed a new law ("Gesetz zur Angemessenheit der Vorstandsverguetung," VorstAG). The first draft of the VorstAG was presented and discussed by the German parliament in March 2009. Shortly thereafter, the law was passed on July 31, 2009, and became effective on August 5, 2009. This law mainly affects three areas. First, it mandates a deductible in the liability insurance contracts of members of the management board. Second, the law introduces several new regulations for variable compensation, such as a longer mandatory vesting period. Third, the law tackles the way in which managerial compensation is determined within the company. In particular, the VorstAG mandates that managerial compensation must be decided by the whole supervisory board (§1, Number 4).[11]

[10] Some firms performed this task in other committees, such as the staff committee ("Personalausschuss") or the executive committee ("Praesidialausschuss"). For simplicity, we refer to the committee that determines managerial compensation as the compensation committee.

[11] This article led to a change in §107 of the German Stock Corporation Act. The question of whether the law also applies to private firms arose. Among others, a legal opinion by Hartmut Oetker for the Hans Boeckler Stiftung concludes that the new regulations also apply to private corporations. Thus, we also consider private firms for the difference-in-differences tests. However,

This last aspect of the VorstAG leads to an increase in employee influence on managerial compensation, but only in PER firms. After the law is implemented, it is no longer possible to transfer the decision on managerial compensation from the entire supervisory board to the compensation committee. In non-PER firms, employees have limited influence on compensation before and after the law change. The other aspects of the VorstAG likely have the same impact on managerial compensation in PER and non-PER firms.

3. Data

3.1. Data sources

Our dataset consists public and private German firms. The sample period is from 1998 to 2016. Accounting data comes from Hoppenstedt GmbH, a commercial provider of business information for German firms. Coverage before 1998 is generally poor in this database. We exclude financial firms, non-profit firms, subsidiaries of a domestic or foreign business group, and firms that are exempt from the co-determination law (e.g., news publishers or Societates Europaeae without PER).

For the empirical design, we need data on the number of *domestic* employees. In the Hoppenstedt database, this item is missing in many cases. For those firms, we manually collected the number of DEs from annual reports retrieved from firms' websites, the Hoppenstedt database, and the Thomson Reuters filings database. We also manually collect whether a firm has PER or not from the same annual reports.

Data on managerial compensation is also retrieved from the Hoppenstedt database or when missing, manually collected from firms' annual reports. During the sample period, the disclosure of person-level data on managerial compensation and data on individual components of managerial compensation (e.g., stock options) is not mandatory for all firms in Germany. Thus, we focus on the average per-person compensation for the management board, calculated as the total compensation for the

the results are similar if we exclude private firms for this test.

management board divided by the number of management board members. Total compensation includes, but is not restricted to, the fixed salary, bonus payments, or stock-based compensation (see §285 HGB). Data on the board composition, which we use to calculate the size of the management board, is obtained from Hoppenstedt directly and if missing, manually collected from the annual reports.

For the cross-sectional RD analysis, we are interested in firms around the threshold of 2,000 DEs. This main RD sample includes 551 firm-year observations from 122 different firms with between 1,500 DEs and 2,500 DEs. For the DiD analysis, we use the entire sample instead of imposing any size restrictions. The time period of the DiD analysis is 2005 to 2013. We code 2005 to 2008 as the pre-period and 2010 to 2013 as the post-period; the introduction year 2009 is excluded. The DiD sample consists of 1,915 firm-year observations from 302 unique firms.

3.2. Summary statistics

The summary statistics of the sample are presented in Table 1, and the definitions of all variables are shown in Table A.1. In the RD sample with firm-years between 1,500 DEs and 2,500 DEs, firms above the threshold represent 34 percent of our observations, and the average firm has 1,898 DEs. The average pay ratio in the sample is 15.8 times. Breaking this ratio down, we observe that the mean total compensation per capita for the management board is 691,000 Euros, and the average worker wage is 47,400 Euros per annum.[12] For the DiD sample, the fraction of firms above the threshold in 2008 (the "treated" firms) is 40 percent. As we do not impose a size threshold for the DiD sample, the size distribution is much wider for this sample compared to the RD sample, although the average size is similar in the samples. For the discontinuity-in-differences approach, we present results for both the RD sample and the DiD sample.

[12]Note that the managerial pay level in the sample is lower than that in Fernandes et al. (2013) because we focus on mid-sized firms.

4. Employee representation and the manager-to-worker pay ratio

In this section, we present the main set of analyses of the impact of direct employee participation in the board room on the pay ratio. We start with the RD analysis around the PER threshold, followed by the DiD strategy exploiting the 2009 compensation law change, and finally the combination of the two.

4.1. Regression discontinuity around the PER threshold

4.1.1. RD specification

The RD design takes advantage of the discontinuity in the power of employees on the corporate board due to the co-determination law. The estimation follows a parametric strategy with an intent-to-treat approach. We use a sample of firms with around 2,000 DEs to compare the pay ratio on both sides of the threshold. In the main estimations, we use a range of 1,500 DEs to 2,500 DEs, and in the robustness checks, we further narrow it down to 1,750 DEs to 2,250 DEs and 1,850 DEs to 2,150 DEs. The main independent variable is a dummy that indicates whether a firm has more than 2,000 DEs. This dummy variable is lagged by two periods because the supervisory board is not adjusted instantaneously if a firm crosses the threshold, and because the new supervisory board requires time to change the compensation contracts.[13] In line with the literature, the control variables (except for current profitability) are lagged by one period. The full model specification we use is as follows:

$$Ln(\text{PayRatio}_{ilt}) = \alpha + \beta \text{DE\_2000}_{i,t-2} + \gamma(DE_{i,t-2} - 2000) + \delta(DE_{i,t-2} - 2000) \times DE\_2000_{i,t-2} + \nu' X'_{i,t-1} + \tau_t + \iota_l + \tau_t \times \iota_l + \epsilon_{ilt}$$

where $\text{PayRatio}_{i,t}$ is calculated as the ratio between the average compensation per member of the management board in firm i of industry l and year t, divided by the average salary per worker in the same firm and year. DE_2000 is a dummy that

[13] The time structure in the empirical model assumes that the number of DEs in year $t-2$ determines the co-determination status in year $t-1$, which affects the pay ratio in year t.

equals one for firms with more than 2,000 DEs and zero otherwise. The term (DE-2,000) controls for a potential direct effect of the number of DEs on the outcome variable, and we allow this effect to be different on both sides of the threshold by interacting (DE-2,000) with the dummy variable DE_2000 (or a detailed discussion of RD specifications, see Lee and Lemieux, 2010). X is a vector of controls including the lagged logarithm of firm asset size, book leverage, the ratio of tangible assets out of total assets, an indicator for exchange-listed firms, and current profitability measured as EBIT to total assets. Year times Fama-French five industry fixed effects are represented by $\tau \times \iota$.[14] The coefficient of interest is β, which represents the causal effect of being above the threshold on the dependent variable. In all tests, we estimate Huber-White robust standard errors clustered at the firm level.

4.1.2. RD results

We start with a graphical analysis to investigate whether there is any discontinuity in the pay ratio around the threshold of 2,000 DEs. With the assumption that other firm characteristics on either side of the threshold are smoothly distributed, any difference in the pay ratios between the two sides is likely caused by employee representation (we will assess the assumption in the following subsection). In Figure 1(a) and (b), we plot the raw pay ratio and the natural logarithm of the pay ratio by bins of 100 DEs. The plot includes the linear fits as well as the corresponding 90 percent confidence intervals. We find clear evidence for higher pay ratios in firms on the right side of the threshold: The average pay ratio is around 14 times on the left side of the threshold, and about 20 times immediately on the right of the threshold.[15]

[14] We use the five-industries classification for the baseline specification because our sample size does not allow us to include year times industry fixed effects when using a more precise classification. However, we show that the result are robust when we include year plus industry fixed effects based on the Fama/French 17 or 38 industries classifications (see Panel F in Table A.3).

[15] Interestingly, we do not observe positive slopes in the fitted lines on either side of the threshold. On the surface, this may seem at odds with the results in Mueller, Ouimet, and Simintzi (2017b) who argue that in general, the pay ratio should be higher in larger firms due to higher managerial talent. However, the number of DEs is not a direct proxy for firm size, and we show in Figure A.1 that firms' assets are positively correlated with the pay ratio, managerial compensation, and worker wage, which is in line with the previous literature.

We then move on to regression estimates of the RD specification and present the results in Table 2. The sample includes all firm-year observations where the number of DEs in year $t-2$ is between 1,500 and 2,500, which leads to 551 observations from 122 unique firms. The dependent variable is the natural logarithm of the pay ratio. In Column (1), we do not control for co-variates, year, or industry fixed effects. The baseline estimation suggests that firms above the threshold have a 34 percent higher pay ratio compared to firms below. In Column (2), we add in year and industry fixed effects, which produces a similar but slightly bigger result. Columns (3) and (4) include firm-level characteristics as co-variates, and Column (4) further controls for industry-year specific shocks. Across these specifications, we observe that employee representation is associated with an increase in the pay ratio by approximately one-third. The coefficients on the co-variates suggest that the pay ratio is higher in larger firms, firms with higher financial leverage, more profitable firms, firms with less tangible assets, and publicly listed firms.

4.1.3. RD robustness: balancing of co-variates

The first potential concern with the RD approach is that other firm-level determinants of the pay ratio could change around the threshold and drive the result. Firm size and profitability likely have strong impacts on the pay for managers and average workers, for example, because of profit sharing and higher skills in larger firms. At the same time, PER firms are larger and potentially also more profitable that non-PER firms. We rely on three econometric tests to confirm that such differences in firm characteristics do not drive our results.

We examine how total assets and profitability, measured as EBIT to total assets, are distributed around the threshold. The RD design permits firm characteristics to be different on the two sides of the threshold as long as they are continuously distributed around the threshold, but a positive jump in firm size or profitability around the threshold would cast doubt on the interpretation of our previous findings.

However, Figure 2(a) and (b) fail to identify any discontinuities of profitability[16] and asset size around the threshold.

Then, we match the PER and non-PER firms by total assets and profitability and repeat the RD estimation for the matched samples. The size-matching exploits that the number of DEs and total assets are not perfectly correlated (their correlation coefficient is about 0.75). Technically, we use one-to-one nearest-neighbor matching based on the lagged logarithm of total assets. Table 3 presents the RD results for the size-matched sample. The estimated effect of PER in Panel A remains similar to the main results, implying that size differences do not cause higher pay ratios in PER firms. Panel B shows that the total assets are very similar among the PER and non-PER firms in the size-matched sample. If we repeat this matching for lagged EBIT scaled by total assets in Panels C and D, we again find that differences in profitability cannot explain the higher pay ratios in PER firms.

Last, we introduce an RD specification with firm fixed effects using a subset of firms that switched PER statuses during the sample period. The firm fixed effects control for any unobserved and time-constant differences between the firms on the two sides of the threshold. We use all firm-year observations from the switching firms, independently of their number of DEs, and present the results in Table 4. The magnitude of the estimates is smaller compared to those in Table 2, but the RD estimate with firm fixed effects remains economically and statistically significant. The results suggest that as the same firm moves from the left side to the right side of the PER threshold, the pay ratio increases by about one-quarter.

4.1.4. RD robustness: manipulation concerns

The second concern is related to the fact that the number of DEs is clearly not randomly assigned. Instead, it is controlled by the firm's management and could be manipulated. Given the result that co-determination benefits managers, the main concern in this setting is upward manipulation. In Figure 3, we plot the density

[16] Using the alternative profitability measures ROA (net income to assets), ROE (net income to equity), and ROS (net income to sales) leads to similar results, which are not reported.

of firm distribution around the 2,000 DEs threshold with the help of the McCrary (2008) density test. If managers manipulate the number of DEs, we expect to see firms bunching above or below the 2,000 DEs threshold. However, the actual distribution of firms is smooth around the threshold, with no signs of bunching on either side. Although this test does not rule out the possibility that some firms manipulate upward, whereas others manipulate downward, it shows that large-scale manipulation is unlikely.[17]

Next, we examine the ratios of DEs to assets and DEs to the number of total employees. If managers manipulated the number of DEs to force PER, we would expect to find an unusually high ratio of DEs to total employees or assets in firms just above the threshold. However, we find no evidence for such "over-staffing" in Figure 2(c) and (d). The corresponding RD estimates can be found in Appendix A.2. Again, these findings indicate that large-scale manipulation is unlikely.

The lack of empirical support for upward manipulation might seem surprising, given that managers can benefit from co-determination according to our findings.[18] However, there are several frictions that make it difficult for managers to engage in "over-staffing" of DEs. First, the firms' management cannot decide about the appointment of workers alone. In firms with more than 20 employees, which is the case for all the sample firms, the work council ("Betriebsrat") has to agree when new workers are hired.[19] As "over-staffing" increases the firm's risk, which could lead to layoffs in the future, the work council is likely to reject appointments of new employees if they think that these appointments are not due to the growth of the firm.

Second, "over-staffing" by hiring non-necessary workers is expensive for the firm

[17] Anecdotally, a review of the law that was conducted on behalf of the German government (the Biedenkopf Commission) concluded that "only very few cases of companies avoiding board level representation are known" (Executive Summary by the Hans-Böckler-Foundation, p. 3).

[18] Kim, Maug, and Schneider (2018) and Lin, Schmid, and Xuan (2018), who use the same threshold, also fail to find empirical support for actual manipulation.

[19] In particular, the work council has the right to block the appointment of a new employee for several reasons. Among others, the work council can block the appointment if they think that the hiring of a new employee could cause disadvantages for the existing workforce (§99 BetrVG).

and if their compensation is linked to the firm's performance, for the managers. Consider a firm that has 1,900 DEs and no need for any additional workers. If the management wants to manipulate the co-determination status, they would need to hire 100 additional domestic workers. The average worker in the sample costs the firm about 50,000 Euros per year. Given an average (median) EBIT of about 60 (35) million Euro in the sample, this "over-staffing" would lead to a relative decline in profitability of 8 percent (14 percent) for the average (median) firm. This reduction in firm profitability would decrease the variable part of managerial compensation.[20]

Third, shareholders have strong incentives to avoid upward manipulation. One reason is purely financial as "over-staffing" is expensive for the firm and ultimately, for its owners. Furthermore, once the firm is above the threshold, the power of the owners on the supervisory board declines substantially because they lose half of the board seats. Although owners have no direct influence on the daily business decisions (such as human resources), they can terminate the contracts of the managers if they distrust them and suspect manipulation. Thus, owners have the incentives and the means to prevent managers from hiring non-necessary DEs.[21]

4.1.5. RD robustness: miscellaneous

In Appendix A, we discuss further robustness checks on the RD results. Overall, the empirical evidence suggests that the results are unlikely to be driven by manipulation or different firm characteristics around the threshold. Although these tests alleviate concerns about alternative interpretations of the results, we acknowledge the impossibility of completely ruling out manipulation in the RD setting. Therefore, we additionally exploit variation in employee influence over managerial compensation due to a law change in 2009 in a DiD setting.

[20] The cost of upward manipulation are obviously much lower for firms that are very close to the threshold, but these cases are rare in the sample. There are 45 firm-year observations with between 1,900 and 2,000 DEs, 23 firm-year observations with between 1,950 and 2,000 DEs, and 2 firm-year observations with between 1,990 and 2,000 DEs in the sample.

[21] Owners also lose some control when a firm grows naturally above the threshold. However, in contrast to manipulation, such natural growth is also beneficial for the owners in the long term.

4.2. Difference-in-differences around the 2009 law change

Effective in August 2009, the VorstAG mandates managerial compensation has to be decided by the entire supervisory board, not the compensation committee. In PER firms, employees were often under-represented in the committee that determines managerial compensation before the law, but their influence over managerial pay increased after the passage of the law (treated group). In non-PER firms, employees had less than half of the votes on managerial compensation before and after the law, and we use these firms as the control group. Using the number of DEs in 2008, when the details of the law were not yet known, to define treated and control firms mitigates the previously discussed manipulation concerns.

As the DiD setup does not require a narrow sample around the threshold, we use the full sample for this test. Later, we present a specification using the narrow window around the threshold and combine the DiD and RD methodologies to analyze the "discontinuity-in-differences". We code 2005 to 2008 as the pre-period, 2009 as the transition period, and 2010 to 2013 as the post-period. The full model specification is as follows:

$$Ln(\text{PayRatio}_{i,t}) = \alpha_i + \beta \text{Post}_t \times \text{DE\_2000}_{i,2008} + X'_{i,t}\nu + \tau_t \times \iota + \epsilon_{i,t},$$

where α_i are the firm fixed effects. Post_t equals zero in the three years 2005 to 2008 and one in the three years 2010 to 2013. $\text{DE\_2000}_{i,2008}$ equals one for firms that have more than 2,000 DEs in 2008, and zero for the control firms, $X_{i,t}$ represents control the variables that are the same as in the RD specification, $\tau_t \times \iota$ are industry times year fixed effects, and $\epsilon_{i,t}$ is the error term. T-statistics based on Huber-White robust standard errors clustered at the firm level are reported in all tests.

Table 5 presents the DiD regression results with the natural logarithm of the pay ratio as dependent variable. Column (1) shows a specification with firm-fixed effects, Column (2) adds year-fixed effects, Columns (3) adds year times industry fixed effects, and Column (4) shows the full model with additional firm-level controls. The result suggests that the law change led to an increase in the pay ratio in treated

firms by 11 percent to 16 percent relative to control firms. We explore the time dynamics of this treatment effect by interacting the treated dummy with separate year indicators. The estimated coefficients on the year indicators are plotted in Figure 4. The estimates imply that the law started to have an effect on the pay ratio in 2011. The coefficients on the interaction terms with years before the law became effective are economically small and statistically insignificant, suggesting that the treated and control groups followed parallel trends.

We present robustness tests for the DiD analysis in Table A.4. First, we analyze whether profitability differences around the law change can explain the differences between treated and control firms. However, Column (1) shows that there is no evidence for such profitability differences. In Column (2), we use an alternative treatment indicator. Instead of the dummy for 2,000 DEs in 2008, this indicator explicitly considers the under-representation of employees on the compensation committee before the law change. It equals one if a firm had more than 2,000 DEs in 2008 and employees were under-represented in the compensation committee; it is zero for firms with less than 2,000 DEs in 2008. Although this specification is more affected by endogeneity concerns because under-representation is not exogenous, it is reassuring to see that the results are similar as in the main specification. In Column (3) of Table A.4, we restrict the sample to firms with between 1,500 and 2,500 DEs and control for the number of DEs interacted with the post dummy. This approach, which combines elements of the RD and DiD design, leads to similar results as the baseline specification. In Column (4), we additionally match treated and control firms by their total assets and find similar results.

4.3. Discontinuity-in-differences around the threshold and law change

Now we adopt an alternative approach to combine the RD and DiD methodologies. We first calculate, for each firm, the pay ratio difference from before to after the law change. After that, we estimating an RD model to identify the discontinuity in these differences around the PER threshold. Concerns about manipulation are less pronounced for this "discontinuity-in-differences" approach, which follows Krishnan,

Nandy, and Puri (2015) and Grembi, Nannicini, and Troiano (2016), because we use the number of DEs in 2008, when the law was not yet announced, to define treated and control firms.

We present the results in Table 6. The regression specification follows the RD framework, and the dependent variable is the change in the median pay ratio from the pre-period (2005 to 2008) to the post-period (2010 to 2013). Therefore, we obtain a collapsed panel, where each firm is represented by one observation. In Panel A, we use all 213 firms in the full sample, without imposing any restrictions on the number of DE. We find that the pay ratio in firms with more than 2,000 DEs increases by 22 percent to 30 percent more from the law change compared with the firms below the threshold, depending on whether we control for industry fixed effects or firm-level changes in the co-variates. To estimate the discontinuous effect on the threshold more precisely, in Panel B we focus on the 45 firms that have between 1,500 DEs and 2,500 DEs in 2008.[22] Despite the small sample size, the coefficient estimates indicate that firms above the threshold increase their pay ratios by about one-third more after the law change, compared to firms below the threshold.

5. Mechanism

The result thus far are consistent with a manager-worker alliance hypothesis in which managers and workers are better off as a result of PER. To further investigate the alliance hypothesis, we now separately evaluate managerial compensation and employee wages as the two components of the pay ratio. For this purpose, we repeat the RD, DiD, and discontinuity-in-differences estimations, but replace the dependent variable with measures of managerial compensation and employee wages.

5.1. Executive compensation

We first investigate how PER affects executive compensation and present the results of our analysis in Table 7. The dependent variable is the natural logarithm of

[22]This sample size is considerably smaller compared to the plain RD analysis because we need data on managerial compensation in the pre- and post-period.

the per-manager compensation calculated as an average for the management board. The RD estimates suggest that managerial compensation is about one-third higher in PER firms compared with the non-PER firms that are just below the 2,000 DEs threshold. Figure 1(c) presents the graphical illustrations of the RD results using managerial compensation as the outcome variable. In line with the regression result, we find a jump in managerial compensation around the threshold. Moreover, firms that switch to PER experience a one-quarter increase in managerial compensation. The DiD result suggests that the 2009 law change leads to increase in managerial compensation of slightly more than 10 percent, and the combination of DiD and RD yields further evidence for a positive impact of PER on managerial compensation. These results suggest that employee representatives are friendly to the managers and are in line with the alliance hypothesis.

5.2. Employee wages

Higher wages may be an obvious benefit for employees if managers and employees form an alliance. However, we know from U.S. data that unionization does not necessarily lead to higher wages (DiNardo and Lee (2004); Frandsen (2021)). We investigate the effect of PER on worker wages in Table 8. The dependent variable is the natural logarithm of the per-employee wage calculated as the total expenditures for employees divided by the number of employees. Throughout various regression specifications (RD, DiD, and a combination of the two), however, we observe no evidence that PER lead to higher worker wages. Figure 1(d) also fails to provide graphical illustrations for any discontinuity of workers' wages around the threshold. This result adds to the mixed findings in the previous literature about the effects of PER on wages. Among others, Gorton and Schmid (2004) find no effect, FitzRoy and Kraft (1993) show a positive effect, and Kim, Maug, and Schneider (2018) find a negative effect. Together, the results for the pay ratio components indicate that the positive effect of equal employee board representation on the pay ratio is driven by an increase in managerial compensation.

5.3. Employee job security

We then investigate another potential benefit for employees which is higher job security. In this perspective, PER firms would offer their employees protection against layoffs. [23] Indeed, Kim et al. (2018) show that employees in PER firms are better protected against layoffs during adverse industry shocks than their peers in non-PER firms. Using a slightly different specification and approach than their paper, we estimate the overall employment-profitability sensitivity in PER versus non-PER firms. Panel A of Table 9 presents the results. The dependent variable is the natural logarithm of the number of DEs. The estimate again follows an RD setup where we regress employment on an indicator for more than 2,000 DEs, profitability (EBIT scaled by total assets), and the interaction between the two. The results in Columns (1) and (2) show that the employment-performance sensitivity is less pronounced in firms above the threshold, suggesting that PER leads to employment smoothing and better job security for workers. In Columns (3) and (4), we split the sample into observations with above and below average profitability by year and industry. We find that the lower sensitivity is driven by less profitable firms, which is consistent with fewer layoffs during times of lower performance in co-determined firms.

In Panel B of Table 9, we focus directly on layoffs. The dependent variable is the percentage reduction in the number of DEs from year $t-1$ to year t and is set to zero if the change in the number of DEs is positive in a given year. The estimation follows a regular RD setup, and the result complements that in Panel A by showing that layoffs are less likely in PER firms. The difference between our specification and that in Kim, Maug, and Schneider (2018) is that we examine the effects of general fluctuations in profitability (panel A) and the unconditional probability of layoff (panel B), while their paper focuses on negative shocks to industries. Overall, the results presented in Table 9 reveal that workers in PER firms enjoy better job

[23] In a related context, Ellul et al. (2018) show that unemployment insurance offered by firms are governments are substitutes.

security. This result, together with the higher benefit to management, lends support to the manager-worker alliance hypothesis as an explanation for the higher pay ratio in firms with employee participation in corporate decision making.

6. Conclusion

Several recent policy proposals call for more direct worker participation on corporate boards to reduce within-firm wage inequality. To the best of our knowledge, we are the first to provide empirical evidence for how the allocation of board seats to employees affects the pay ratio. For identification, we first apply an RD approach based on the German co-determination law, which mandates that half of the seats on firms' supervisory boards belong to employees in firms with more than 2,000 DEs. Second, we conduct a DiD analysis around a compensation law change in 2009 that strengthened the power of employee representatives. Third, we combine the two approaches in a "discontinuity in differences" design.

The results show that direct employee influence on firms' governance leads to an increase in the pay ratio by about one-third, which is driven by higher managerial compensation. Although none of the three empirical approaches we used represents by itself a perfect setting, it is reassuring that we obtain qualitatively and quantitatively similar results from the RD approach, the DiD estimation, and the combination of the two. We also find that workers are better off in codetermined firms, as they enjoy more job security. Overall, these findings indicate that workers and managers form an alliance that benefits both parties (Pagano and Volpin, 2005).

These findings cannot be interpreted as evidence against the participation of employees in firms' governance. There could be other consequences of co-determination, most notably increased productivity or stronger commitment, which we do not investigate. As a result, there may be "bright" and "dark" sides of co-determination, which can counterbalance each other. The lack of a significant effect of co-determination on various profitability measures in the sample points in this direction.

Behind the broader background of recent calls for more direct involvement of

employees in corporate governance, these findings have important consequences, as they can help policy makers to improve the regulatory framework for employee participation. An optimal design, which enforces the bright side but limits the dark side, may well benefit all stakeholders.

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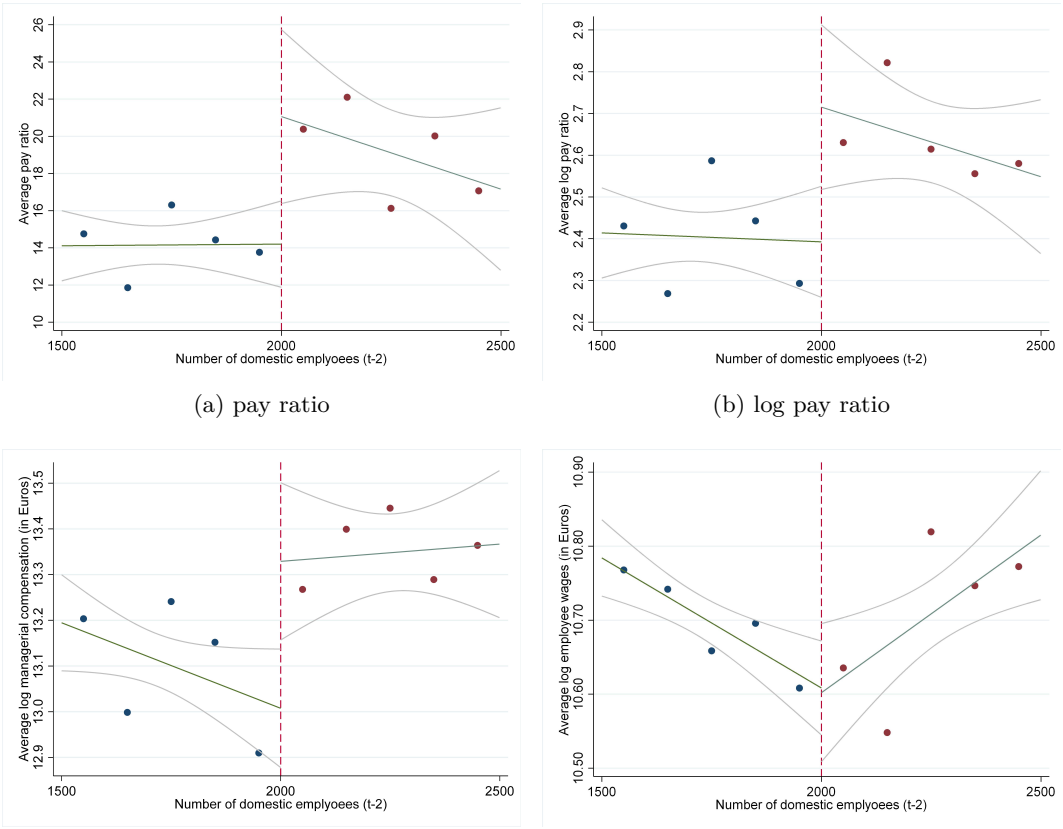


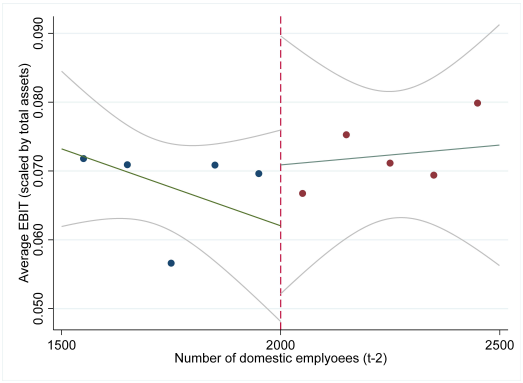
(a) pay ratio

(b) log pay ratio

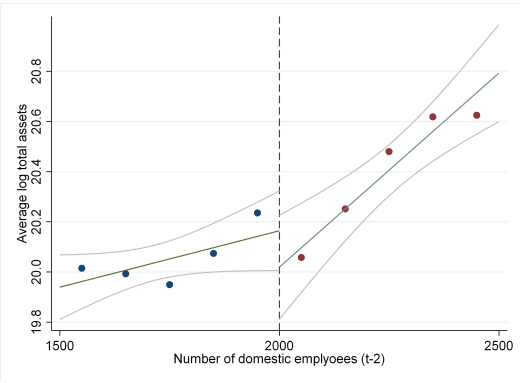
(c) log managerial compensation

(d) log wage

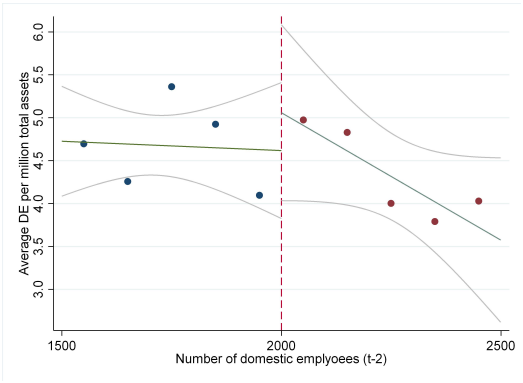
Figure 1: This figure shows the pay ratio, log pay ratio, log managerial compensation, and log wage around the threshold of 2,000 domestic employees. The horizontal axis shows the number of domestic employees in year t-2 and the vertical axis displays the mean of the outcome variable for each bin in year t. We include a linear fit on both sides of the threshold and the corresponding 90% confidence intervals.



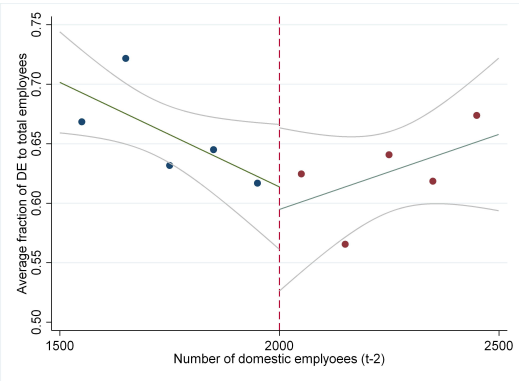
(a) profitability (EBIT to total assets)



(b) Log total assets



(c) DEs to assets



(d) DEs to total employees

Figure 2: This figure shows the EBIT to total assets, log total assets, domestic employees per million total assets, and domestic employees to total employees around the threshold of 2,000 domestic employees. The horizontal axis shows the number of domestic employees in year t-2, and the vertical axis displays the mean of the outcome variable for each bin in year t. We include a linear fit on both sides of the threshold and the corresponding 90% confidence intervals.

Figure 3: This figure shows a McCrary (2008) density plot for the distribution of domestic employees around the threshold of 2,000 domestic employees.

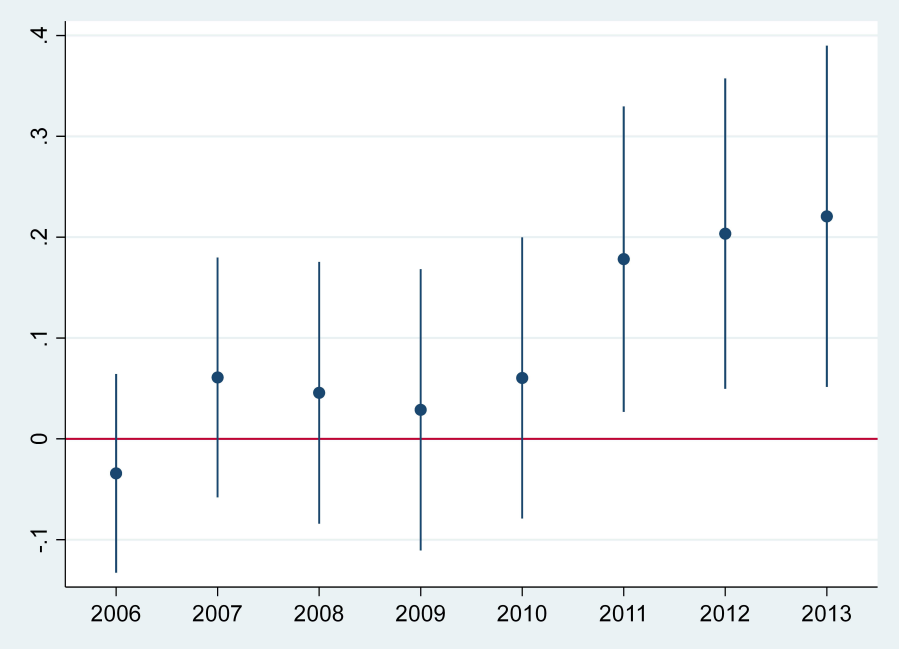


Figure 4: This figure shows the coefficient estimates for the years around the law change in 2009. The base year 2005 is omitted. The coefficient estimates are based on the regression model in Column 3 of Table 5.

Table 1: Descriptive statistics

Panel A: RD sample								
	N	mean	SD	p10	p25	p50	p75	p90
Pay ratio	548	15.78	14.75	4.49	6.95	11.58	19.80	30.75
Managerial comp. (tsd EUR)	548	691	605	230	323	502	863	1305
Wage (tsd EUR)	548	47.36	15.44	29.54	37.61	47.30	54.84	63.25
DE_2000$_{t-2}$	548	0.34	0.47	0.00	0.00	0.00	1.00	1.00
DE$_{t-2}$	548	1,898	295	1,558	1,641	1,841	2,137	2,364
Size (ln)	548	20.16	0.85	19.09	19.66	20.17	20.72	21.31
Leverage	548	0.52	0.20	0.24	0.39	0.54	0.66	0.78
Profitability	548	0.07	0.07	-0.01	0.03	0.07	0.11	0.15
Tangibility	544	0.37	0.22	0.13	0.22	0.32	0.48	0.75
Listing	548	0.45	0.50	0.00	0.00	0.00	1.00	1.00
Panel B: DiD sample								
	N	mean	SD	p10	p25	p50	p75	p90
Pay ratio	1,915	19.96	21.82	4.28	6.83	12.49	23.84	44.61
Managerial comp. (tsd EUR)	1,915	917	921	211	337	586	1,102	2,051
Wage (tsd EUR)	1,915	50.98	17.33	31.22	39.71	50.50	61.03	70.67
DE_2000$_{2008}$	1,915	0.40	0.49	0.00	0.00	0.00	1.00	1.00
Size (ln)	1,914	20.03	1.84	17.65	18.74	19.97	21.15	22.71
Leverage	1,914	0.50	0.20	0.23	0.36	0.51	0.65	0.74
Profitability	1,914	0.07	0.10	-0.01	0.03	0.07	0.11	0.16
Tangibility	1,913	0.26	0.18	0.04	0.13	0.23	0.35	0.48
Listing	1,907	0.67	0.47	0.00	0.00	1.00	1.00	1.00

This table shows the number of observations (N), mean, standard deviation (SD), 10% percentile, 25% percentile, median, 75% percentile, and 90% percentile for the variables used in the RD analysis (Panel A) and DiD analysis (Panel B) of the pay ratio. The RD sample includes firm-years for which the number of DEs is between 1,500 and 2,500. The DiD sample includes the four years before the law change (2005 to 2008) and the four years thereafter (2010 to 2013). A detailed description of all variables can be found in Table A.1.

Table 2: Pay ratio around the threshold

Sample: 1,500 to 2,500 domestic employees				
Column	1	2	3	4
DE_2000$_{t-2}$	**0.34****	**0.42*****	**0.29****	**0.36*****
	(2.16)	**(2.99)**	**(2.35)**	**(2.75)**
Size$_{t-1}$			0.20***	0.20***
			(3.50)	(3.39)
Leverage$_{t-1}$			0.42**	0.46**
			(2.13)	(2.16)
Profitability$_t$			2.68***	2.59***
			(3.74)	(3.43)
Tangibility$_{t-1}$			-0.63***	-0.63***
			(-2.85)	(-2.72)
Listing$_{t-1}$			0.32***	0.34***
			(2.82)	(2.83)
Observations	548	547	539	525
Firms	122	122	122	119
adj. R^2	0.018	0.15	0.39	0.37
Year FE	no	yes	yes	yes
Industry FE	no	yes	yes	yes
Year x Ind FE	no	no	no	yes
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes

The dependent variable is the natural logarithm of the pay ratio in year t. The pay ratio is calculated as the average compensation per manager divided by the average employee salary. Only firm-years for which the number of domestic employees in year t-2 is between 1,500 and 2,500 are considered. DE_2000$_{t-2}$ equals one if a firm has more than 2,000 DE in year t-2. Polynomial degree indicates how we control for the centered number of domestic employees. Either side means that we interact the polynomial with the DE_2000$_{t-2}$ dummy. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table 3: Pay ratio around the threshold: matching

Panel A: Size-matched sample				
Column	1	2	3	4
DE_2000$_{t-2}$	**0.36***	**0.47*****	**0.37****	**0.40****
	(1.91)	**(2.67)**	**(2.61)**	**(2.60)**
Observations	368	367	361	349
Firms	108	108	108	105
adj. R^2	0.0077	0.13	0.39	0.37
Year FE	no	yes	yes	yes
Industry FE	no	yes	yes	yes
Year x Ind FE	no	no	no	yes
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes

Panel B: Balancing of log(total assets)$_{t-1}$		
	Obs	Size$_{t-1}$
DE_2000$_{t-2}$ = 0	184	20.32
DE_2000$_{t-2}$ = 1	184	20.35

Panel C: Profitability-matched sample				
Column	1	2	3	4
DE_2000$_{t-2}$	**0.38****	**0.56*****	**0.47*****	**0.55*****
	(2.49)	**(3.98)**	**(3.61)**	**(3.71)**
Observations	368	367	361	345
Firms	112	112	112	107
adj. R^2	0.011	0.15	0.39	0.37
Year FE	no	yes	yes	yes
Industry FE	no	yes	yes	yes
Year x Ind FE	no	no	no	yes
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes

Panel D: Balancing of profitability$t-1$		
	Obs	Profitability$_{t-1}$
DE_2000$_{t-2}$ = 0	184	0.076
DE_2000$_{t-2}$ = 1	184	0.079

continued on next page

Table 3 continued

This present es RD regressions for matched samples. In Panel A, we use one-to-one nearest-neighbour matching based on the lagged logarithm of total assets to obtain a size matched sample. The balancing of the matching variable is investigated in Panel B. In Panel C, we use one-to-one nearest-neighbour matching based on the lagged EBIT, scaled by total assets, to obtain a profitability matched sample. The balancing of the matching variable is investigated in Panel D. The dependent variable is the natural logarithm of the pay ratio in year t. The pay ratio is calculated as the average compensation per manager divided by the average employee salary. Only firm-years for which the number of domestic employees in year t-2 is between 1,500 and 2,500 are considered. DE_2000_{t-2} equals one if a firm has more than 2,000 DE in year t-2. Polynomial degree indicates how we control for the centered number of domestic employees. Either side means that we interact the polynomial with the threshold dummy. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table 4: Pay ratio around the threshold: switching firms

Column	1	2	3	4
DE_2000$_{t-2}$	**0.26****	**0.22****	**0.20***	**0.23****
	(2.45)	**(2.13)**	**(1.99)**	**(2.14)**
Size$_{t-1}$			0.50**	0.51**
			(2.42)	(2.32)
Leverage$_{t-1}$			0.096	0.30
			(0.34)	(0.81)
Profitability$_t$			1.51**	1.01*
			(2.34)	(1.71)
Tangibility$_{t-1}$			-0.28	-0.33
			(-0.31)	(-0.37)
Listing$_{t-1}$			0.023	-0.018
			(0.11)	(-0.067)
Observations	474	474	459	454
Firms	51	51	50	50
adj. R^2	0.52	0.59	0.64	0.63
Firm FE	**yes**	**yes**	**yes**	**yes**
Year/industry FE	no	yes	yes	yes
Year x Ind FE	no	no	no	yes
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes

The dependent variable is the natural logarithm of the pay ratio in year t. The pay ratio is calculated as the average compensation per manager divided by the average employee salary. Only firms that switch from below to above the threshold or vice versa during the sample period are included in the sample. For those firms, we include all their firm-years, independently of the number of domestic employees. DE_2000$_{t-2}$ equals one if a firm has more than 2,000 DE in year t-2. Polynomial degree indicates how we control for the centered number of domestic employees. Either side means that we interact the polynomial with the threshold dummy. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table 5: Pay ratio around the 2009 law change

Column	1	2	3	4
DE_2000_{2008} x $Post_t$	**0.16****	**0.16****	**0.14****	**0.11****
	(2.56)	**(2.49)**	**(2.27)**	**(1.99)**
$Size_{t-1}$				0.29***
				(3.49)
$Leverage_{t-1}$				0.22
				(1.27)
$Profitability_t$				0.40
				(1.44)
$Tangibility_{t-1}$				0.12
				(0.38)
$Listing_{t-1}$				0.055
				(0.57)
Observations	1,915	1,915	1,915	1,856
Firms	302	302	302	293
adj. R^2	0.81	0.82	0.81	0.82
Year FE	no	yes	yes	yes
Firm FE	yes	yes	yes	yes
Ind x Year FE	no	no	yes	yes

The dependent variable is the natural logarithm of the pay ratio in year t. The pay ratio is calculated as the average compensation per manager divided by the average employee salary. The sample includes fours years before and four years after the law change in 2009. The year of the law change is excluded from the sample. Post equals zero from 2005 to 2008 and one from 2010 to 2013. The dummy variable DE_2000, which indicates whether a firm has more than 2,000 DE or not, is measured in 2008. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table 6: RD analysis of changes around the law change

Panel A: full sample				
Column	1	2	3	4
DE_2000$_{2008}$	**0.30*** **	**0.30*** **	**0.22** **	**0.22** **
	(2.63)	**(2.66)**	**(2.05)**	**(2.13)**
Δ Profitability			0.031	0.026
			(0.90)	(0.86)
Δ Size			6.44**	6.37**
			(2.60)	(2.56)
Δ Tangibility			0.18	0.21*
			(1.60)	(1.66)
Δ Leverage			0.065	0.069
			(0.54)	(0.57)
Observations	213	213	212	212
adj. R^2	0.016	0.021	0.073	0.075
Industry FE	no	yes	no	yes
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes
Panel B: 1,500 to 2,500 DEs				
Column	1	2	3	4
DE_2000$_{2008}$	**0.45** **	**0.53** **	**0.45***	**0.50** **
	(2.03)	**(2.33)**	**(1.95)**	**(2.15)**
Δ Pay ratio$_{ind}$		3.20*		2.74
		(1.69)		(0.98)
Δ Profitability			-0.11	-0.067
			(-0.75)	(-0.52)
Δ Size			3.38	3.77
			(0.84)	(0.95)
Δ Tangibility			-0.16	-0.042
			(-0.44)	(-0.10)
Δ Leverage			0.27	0.21
			(1.30)	(0.90)
Observations	45	45	44	44
adj. R^2	0.020	0.037	-0.044	-0.046
Industry FE	no	no	no	no
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes

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Table 6 continued

The dependent variable is the relative change of the median pay ratio between the pre-period 2005 to 2008 and the post period 2010 to 2013. No size restrictions is imposed in Panel A. In Panel B, we only include firm-years for which the number of DEs is between 1,500 and 2,500 in 2008. Δ indicates the relative change of the median value of a given covariate between the pre- and post-period. Polynomial degree indicates how we control for the centered number of DEs. Either side means that we interact the polynomial with the threshold dummy. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table 7: Employee representation and managerial compensation

Column	1	2	3	4	5	6	7	8	9	10
Estimation	RD		RD		DiD		RD plus DiD			
Sample	1,500 to 2,500 DEs		switchers only		4-year pre/post period		full sample		1,500 to 2,500 DEs	
DE_2000$_{t-2}$	**0.35****	**0.39*****	**0.22****	**0.25****						
	(2.49)	**(3.18)**	**(2.27)**	**(2.60)**						
DE_2000$_{2008}$ x Post$_t$					**0.16*****	**0.11***				
					(2.82)	**(1.87)**				
DE_2000$_{2008}$							**0.17***	**0.12**	**0.47****	**0.43***
							(1.71)	**(1.25)**	**(2.15)**	**(1.76)**
Observations	551	529	480	460	1,936	1,890	214	213	45	44
Firms	122	119	51	50	303	308	214	213	45	44
adj. R^2	0.027	0.44	0.55	0.65	0.82	0.54	-0.0022	0.025	0.075	0.13
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
Firm FE	no	no	yes	yes	yes	yes	n/a	n/a	n/a	n/a
Year x Ind FE	no	yes	no	yes	no	yes	no	yes	no	no
Polynomial both sides	one	one	one	one	n/a	n/a	one	one	one	one

The dependent variable is the natural logarithm of the average compensation per manager in year t. The estimation methodology is either based on RD around the threshold of 2,000 DE (Columns 1 and 2), an RD approach for firms that switch above or below this threshold (Columns 3 and 4), a difference-in-differences (DiD) approach around the law change in 2009 (Columns 5 and 6), or a combination of the RD and DiD approach (Columns 7 to 10). The model specifications follow those for the pay ratio in Tables 2, 4, 5, and 6. The sample in all models is restricted to firm-years for which the pay ratio is available. Polynomial degree indicates how we control for the centered number of domestic employees. Either side means that we interact the polynomial with the threshold dummy. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table 8: Employee representation and employee wages

Column	1	2	3	4	5	6	7	8	9	10
Estimation	RD		RD		DiD		RD plus DiD			
Sample	1,500 to 2,500 DEs		switchers only		4-year pre/post period		full sample		1,500 to 2,500 DEs	
DE_2000$_{t-2}$	**0.0016**	**0.066**	**-0.00050**	**0.016**						
	(0.020)	**(0.89)**	**(-0.016)**	**(0.59)**						
DE_2000$_{2008}$ x Post$_t$					**0.0066**	**0.016**				
					(0.29)	**(0.59)**				
DE_2000$_{2008}$							**-0.020**	**-0.011**	**0.047**	**-0.0098**
							(-0.39)	**(-0.20)**	**(0.86)**	**(-0.12)**
Observations	548	535	474	469	1,915	1,923	213	212	45	44
Firms	122	121	51	51	302	310	213	212	45	44
adj. R^2	0.019	0.21	0.81	0.84	0.87	0.12	-0.013	-0.012	-0.043	0.14
Controls	no	yes	no	yes	no	yes	no	yes	no	yes
Firm FE	no	no	yes	yes	yes	yes	n/a	n/a	n/a	n/a
Year x Ind FE	no	yes	no	yes	no	yes	no	yes	no	no
Polynomial both sides	one	one	one	one	n/a	n/a	one	one	one	one

The dependent variable is the natural logarithm of the average wage per employee in year t. The estimation methodology is either based on RD around the threshold of 2,000 DE (Columns 1 and 2), an RD approach for firms that switch above or below this threshold (Columns 3 and 4), a difference-in-differences (DiD) approach around the law change in 2009 (Columns 5 and 6), or a combination of the RD and DiD approach (Columns 7 to 10). The model specifications follow those for the pay ratio in Tables 2, 4, 5, and 6. The sample in all models is restricted to firm-years for which the pay ratio is available. Polynomial degree indicates how we control for the centered number of domestic employees. Either side means that we interact the polynomial with the threshold dummy. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table 9: Employee job security around the threshold

Panel A: Employment-performance sensitivity around the threshold				
Column	1	2	3	4
Sample		full	low profit	high profit
DE_2000_{t-2}	0.070	0.093**	0.075*	0.056
	(1.59)	(2.41)	(1.76)	(0.64)
$Profitability_t$	0.27	0.66**	0.64*	0.70*
	(0.97)	(2.42)	(1.84)	(1.94)
DE_2000_{t-2} x $profitability_t$	**-0.56***	**-0.79****	**-2.53****	**-0.68**
	(-1.77)	**(-2.61)**	**(-2.50)**	**(-1.28)**
$Size_{t-1}$		0.36***	0.53***	0.14***
		(5.75)	(8.20)	(3.04)
$Leverage_{t-1}$		-0.24***	-0.22	-0.14
		(-2.62)	(-1.25)	(-1.44)
$Tangibility_{t-1}$		0.22	-0.019	0.30
		(0.85)	(-0.052)	(0.90)
$Listing_{t-1}$		-0.053	0.021	-0.0073
		(-1.01)	(0.43)	(-0.16)
Observations	915	808	359	410
Firms	168	144	87	84
adj. R^2	0.51	0.61	0.65	0.79
Year FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes
/x profitability	yes	yes	yes	yes

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Table 9 continued

Panel B: Employee layoffs around the threshold				
Column	1	2	3	4
DE_2000$_{t-2}$	**-0.014**	**-0.017***	**-0.014**	**-0.017***
	(-1.53)	**(-1.91)**	**(-1.33)**	**(-1.67)**
Size$_{t-1}$			-0.0077**	-0.0067*
			(-2.29)	(-1.97)
Leverage$_{t-1}$			0.0075	0.0028
			(0.53)	(0.22)
Profitability$_t$			-0.13**	-0.12***
			(-2.43)	(-2.71)
Tangibility$_{t-1}$			-0.019	-0.015
			(-1.13)	(-1.10)
Listing$_{t-1}$			0.0077	0.0075
			(0.99)	(0.98)
Observations	941	941	830	820
Firms	199	199	169	168
adj. R^2	0.0057	0.043	0.074	0.15
Year FE	no	yes	yes	yes
Industry FE	no	yes	yes	yes
Year x Ind FE	no	no	no	yes
Polynomial degree	one	one	one	one
/either side	yes	yes	yes	yes
/x profitability	yes	yes	yes	yes

The dependent variable in Panel A is the natural logarithm of domestic employees in year t. In Panel B, the dependent variable is reduction of domestic employees between years t-1 and t, scaled by the number of domestic employees in year t-1; this variable is set to zero if the number of domestic employees increased between years t-1 and t. Only firm-years for which the number of domestic employees in year t-2 is between 1,500 and 2,500 are considered in all models. In Panel A, Column 3 (4), we only include firm-years with a profitability that is higher (lower) than the average profitability in the same industry (Fama-French 5-industries classification) and year. DE_2000$_{t-2}$ equals one if a firm has more than 2,000 DE in year t-2. In all models, we include the (centered) number of DE, interacted with the threshold dummy and, in Panel A, with profitability. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Appendix A: Additional RD robustness tests

We conduct additional robustness checks of the main RD result on the effect of parity employee representation on the pay ratio in Table A.3. In Panel A, we narrow down the estimation window to 1,750 to 2,250 DEs (± 12.5 percent around the threshold) and 1,850 to 2,150 DEs (± 7.5 percent), respectively. Consistent with Figure 1, which shows a jump in the pay ratio from the left to the right of the threshold, the results remain robust. Most importantly, the coefficient estimate for being above the threshold does not decline when we narrow the estimation window. We do not use these narrower windows as the main specifications for two reasons. First, the number of firms is substantially lower (72 or 54 vs. 122 firms). Second, manipulation concerns, which we discuss in detail in Section 4.1.4, are more severe for firms that are very close to the threshold.

In Panel B, we estimate a local polynomial RD with optimal bandwidth following Hahn, Todd, and Van der Klaauw (2001). We use the rdrobust implementation in STATA (Calonico et al., 2017). All parameters are set at their default values (i.e., triangular kernel, mserd bandwidth selector, local linear regression for point estimator, and local quadratic regression to construct bias correction). To control for industry times year fixed effects, we estimate a first-stage regression with the pay ratio as the dependent variable and use the residual for the local RD estimation. The optimal bandwidths are ± 697 DEs without controls, and ± 700 DEs with controls. The results yield further evidence that firms above the threshold have higher pay ratios.

In Panel C, we explore alternative polynomials of degrees two (the same for both sides of the threshold) and three (on either side of the threshold). The results are very similar as those for the main specification which uses a polynomial degree of one on either side of the threshold.

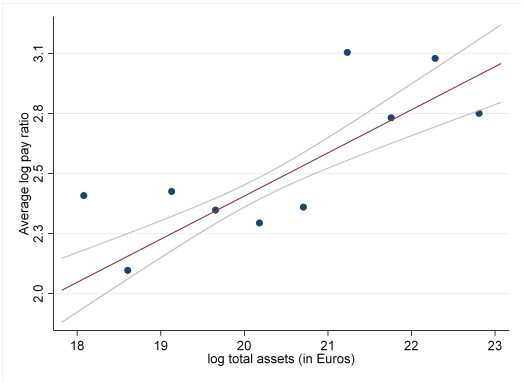
In Panel D, we present placebo tests using artificial thresholds to rule out the possibility that the results are driven by any mechanical structure of the data. For the placebo tests, we use arbitrary thresholds of 1,500 DEs or 2,500 DEs. We do not observe an increase in compensation around these arbitrary thresholds.

We investigate the effect of ownership concentration on the results in Panel E.

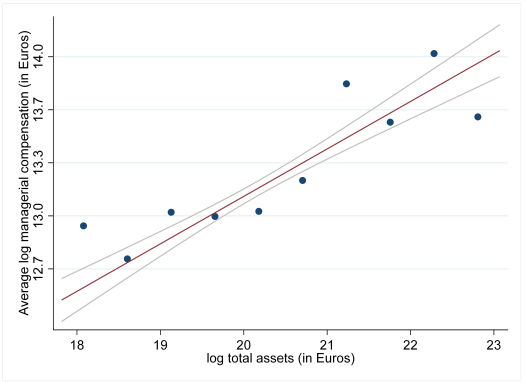
Previously, we argued that employees have substantial power on the supervisory board because shareholders have diverse interests relative to workers, and thus, are less likely to cast homogeneous votes. However, this is not the case if the ownership concentration is very high: In the extreme case, employees can easily be outvoted in firms with a single 100 percent owner. We use ownership data that was directly obtained from Hoppenstedt for the years 2005 to 2013. Because this data is not available for all years, the resulting sample is substantially smaller than the base sample.[24] As expected, we find that the results are concentrated in firms with multiple owners.

Last, in Panel F, we show the robustness of the RD result using more fine-grained definitions of industries. In contrast to the Fama-French five industries classification, we cannot include industry times year fixed effects when we use Fama-French 17 or 38 industry classification due to the size of the sample. Nevertheless, the results remain robust if we control for those alternative industry fixed effects.

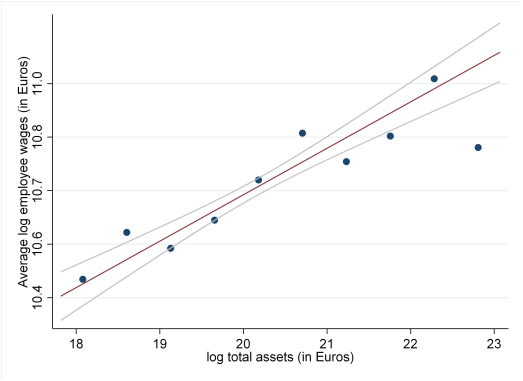
[24] We have 548 firm-year observations in the base sample without control variables, 372 firm-year observations are within the 2005 to 2013 time period, and we have ownership data for 353 of those observations.



(a) log pay ratio



(b) log managerial compensation



(c) log wage

Figure A.1: This figure shows the correlation between firm size, measured as log total assets, and the log pay ratio, log managerial compensation, and log wage per employee. The horizontal axis shows ten bins of log total assets in year t, and the vertical axis displays the average of the outcome variable for each bin in year t. We include a linear fit and the corresponding 90% confidence interval.

Table A.1: Definition of variables

Variable	**Description**
DE	Number of domestic employees. Source: Hoppenstedt (HS) and manually collected.
DE_2000	Dummy that equals one if the number of domestic employees exceeds 2,000.
Post	Dummy variable that equals zero from 2005 to 2008 (per-period) and one for 2010 to 2013 (post-period).
Pay ratio	Average compensation per manager divided by the average wage per employee. Source: HS and manually collected.
Managerial compensation	Average compensation per member of the management board. It is calculated as the total compensation for the whole management board, based on firms' annual reports, divided by the size of the management board. Total compensation includes all forms of compensation (e.g., fixed salary, bonuses, or stock-based compensation). Board size is based on firms' annual reports and adjusted for changes during a firm's fiscal year on a monthly basis. For example, board size would be 3.5 if the board has three members in the first half of the fiscal year and four members in the second half. Source: HS and manually collected.
Wage	Average wage per employee. Calculated as total employee expenditures ("Personalaufwand") divided by the total number of employees. Source: HS.
Size	Natural logarithm of total assets. Source: HS.
Leverage	Total debt divided by total debt plus book value of equity. Source: HS.
Profitability	Earnings before interest and taxes, scaled by total assets. Source: HS.
Tangibility	Long-term tangible assets scaled by total assets. Source: HS.
Listing	Dummy that equals one if shares of the firm are listed on any EU- or exchange-regulated market in Germany. Source: manually collected.

Table A.2: Profitability, firm size, and the fraction of DE around the threshold

Panel A: Discontinuity in profitability or firm size				
Column	1	2	3	4
Dependent variable	ebit/assets		ln(total assets)	
DE_2000$_{t-2}$	**0.0069**	**0.011**	**-0.16**	**-0.061**
	(0.40)	**(0.52)**	**(-0.91)**	**(-0.37)**
Observations	551	529	551	529
Controls	no	yes	no	yes
Year/industry FE	no	yes	no	yes
Polynomial both sides	one	one	one	one
Panel B: Discontinuity in DEs to assets or fraction of DEs				
Column	1	2	3	4
Dependent variable	DEs/assets		DEs/total employees	
DE_2000$_{t-2}$	**0.56**	**-0.26**	**-0.014**	**-0.0014**
	(0.62)	**(-0.49)**	**(-0.20)**	**(-0.033)**
Observations	549	527	548	526
Controls	no	yes	no	yes
Year/industry FE	no	yes	no	yes
Polynomial both sides	one	one	one	one

The dependent variables in Panel A are EBIT scaled by total assets and the logarithm of total assets. In Panel B, we use the number of DEs scaled by total assets and the number of DEs scaled by total employees as dependent variables. Only firm-years for which the number of domestic employees in year t-2 is between 1,500 and 2,500 are considered. Polynomial degree indicates how we control for the centered number of domestic employees. Either side means that we interact the polynomial with the threshold dummy. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

Table A.3: Pay ratio around the threshold: robustness

Panel A: Narrower bandwidth				
Column	1	2	3	4
Bandwidth	1,750 - 2,250 DEs		1,850 - 2,150 DEs	
DE_2000$_{t-2}$	**0.33***	**0.59*****	**0.41***	**0.66***
	(1.73)	**(3.17)**	**(1.93)**	**(1.89)**
Observations	227	213	136	115
Firms	72	70	54	49
Controls	no	yes	no	yes
Year x industry FE	no	yes	no	yes
Polynomial both sides	one	one	one	one
Panel B: Local polynomial RD with optimal bandwidth				
Column	1	2	3	4
	Conventional SE		Robust SE	
DE_2000$_{t-2}$	**0.30****	**0.19***	**0.30***	**0.19***
	(2.00)	**(1.67)**	**(1.72)**	**(1.67)**
Observations	805	793	805	793
Controls	no	yes	no	yes
Year x industry FE	no	yes	no	yes
Panel C: Alternative polynomials				
Column	1	2	3	4
DE_2000$_{t-2}$	**0.35****	**0.37*****	**0.55****	**0.69*****
	(2.18)	**(2.75)**	**(2.24)**	**(3.26)**
Observations	548	525	548	525
Controls	no	yes	no	yes
Year x industry FE	no	yes	no	yes
Polynomial	two	two	three	three
/either side	no	no	yes	yes
Panel D: Placebo tests				
Column	1	2	3	4
DE_1500$_{t-2}$	**0.19**	**-0.052**		
	(0.50)	**(-0.15)**		
DE_2500$_{t-2}$			**-0.46**	**0.22**
			(-0.88)	**(0.50)**
Observations	711	688	326	299
Controls	no	yes	no	yes
Year x industry FE	no	yes	no	yes
Polynomial both sides	one	one	one	one

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Table A.3 continued

Panel E: Ownership concentration				
Column	1	2	3	4
Sample	1 owner		> 1 owner	
DE_2000$_{t-2}$	**-0.27**	**-0.035**	**0.32***	**0.36****
	(-0.72)	**(-0.088)**	**(1.78)**	**(2.07)**
Observations	99	92	254	246
Firms	29	27	76	75
Controls	no	yes	no	yes
Year x industry FE	no	yes	no	yes
Polynomial both sides	one	one	one	one
Panel F: Industry fixed effects				
Column	1	2	3	4
Industry classification	Fama-French 17		Fama-French 38	
DE_2000$_{t-2}$	**0.28****	**0.24****	**0.33*****	**0.30*****
	(2.61)	**(2.19)**	**(2.91)**	**(2.68)**
Observations	547	539	546	538
Controls	no	yes	no	yes
Year/industry FE	yes	yes	yes	yes
Polynomial both sides	one	one	one	one

The dependent variable is the natural logarithm of the pay ratio in year t. In the base specification, firm-years for which the number of DEs in year t-2 is between 1,500 and 2,500 are considered. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.

In *Panel A*, we apply a window of ± 250 DEs and ± 150 DEs around the threshold of 2,000 DEs. In *Panel B*, we apply a local polynomial RD estimation with optimal bandwidth estimation. . The first two columns display conventional standard errors, and the last two columns show robust bias-corrected standard errors. In *Panel C*, we use alternative polynomials to control for the assignment variable. In *Panel D*, we show a placebo test around the thresholds of 1,500 and 2,500 DEs. The estimation window for the placebo tests is set to ±500 DEs. *Panel E* investigates the effect of ownership concentration. Ownership data is only available for the years 2005 to 2013, which leads to a smaller sample size. The first two columns show the results for firms that have only one 100% owner. The last two columns include firms with multiple owners. *Panel F* shows the results for alternative industry definitions.

Table A.4: Pay ratio around the 2009 law change: robustness

Column	1	2	3	4
Sample	full		1,500 to 2,500 DEs	
Dependent	EBIT		ln(pay ratio)	
Size matching	no	no	no	yes
DE_2000$_{2008}$ x Post$_t$	**0.0030**		**0.40****	**0.51****
	(0.51)		**(2.00)**	**(2.05)**
Treated x Post$_t$		**0.19****		
		(2.44)		
Observations	3,033	1,381	263	202
Firms	435	218	69	52
adj. R^2	0.59	0.79	0.77	0.79
Controls	no	no	no	no
Year FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Ind x Year FE	yes	yes	yes	yes

The dependent variable is profitability in year t, measured as EBIT, in Column 1 and the natural logarithm of the average compensation per manager in year t in all other columns. The full sample is considered in the first two columns; only firm-years with more than 1,500 and fewer than 2,500 DE are included in the latter two columns. We additionally include a control for the number of DE, interacted with post, in Columns (3) and (4). In Column 4, we perform a one-to-one nearest neighbor matching based on firm size, measured as total assets.

Post equals zero before 2009, one thereafter, and is set to missing for 2009 when the law became effective. The sample includes fours years before and after the law change. The dummy variable DE_2000 which indicates whether a firm has more than 2,000 DE or not is measured in 2008. Treated equals one if a firm had more than 2,000 DE in 2008 and employees were under-represented in the compensation committee; it is zero for firms with less than 2,000 DE in 2008. T-statistics based on Huber-White robust standard errors clustered by firms are presented in parentheses. ***, ** and * indicate significance on the 1%-, 5%- and 10%-levels, respectively. A detailed description of all variables can be found in Table A.1.