# Firm wage premia, industrial relations, and rent sharing in Germany[*]

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***PRELIMINARY, PLEASE DO NOT CITE***

***Abstract*:** This paper investigates the sources of firm wage premia in Germany. OLS regressions for the firm effects from a two-way fixed effects decomposition of workers' wages by Card, Heining, and Kline (2013) document that average premia are larger in firms bound by collective agreements and in firms with a works council, whereas firm performance is unimportant. RIF regressions show that premia are less dispersed among covered firms but more dispersed among firms with a works council. Hence, deunionization is the only among the suspects investigated, that contributes to explaining the rise in the dispersion of firm wage premia over time.

***Keywords*:** firm wage premium, industrial relations, rent sharing, wage inequality, Germany

***JEL classification*:** J31, J52, J53

[*] We thank Alex Bryson, Stefano Collonello, Bernd Fitzenberger, Nicole Gürtzgen, and Heiko Stüber for very useful suggestions. We further appreciate comments by participants of the SOLE 2017 and VfS 2017 conferences. Last, but not least, we thank Alexander Giebler and Georg Märker for excellent research assistance.

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# 1 Introduction

A substantial body of research documents that firms pay different wages to similar workers. Based on the two-way fixed effects methodology by Abowd, Kramarz, and Margolis (1999, AKM hereafter), several studies find that roughly 15–20 percent of the total variance in wages can be attributed to firm wage premia, that is to wage differences that are left after differences in workers' human capital and their unobservable skills have been rewarded.[1] Clearly, such persistent firm wage premia are at odds with a competitive labor market where firms take wages as given and act as wage takers. They rather point at substantial employment rents that are split between workers and firms.

Obviously, differences in firm wage premia may stem, on the one hand, from firm differences in the surpluses to be shared between workers and firms and, on the other hand, from firm differences in bargaining power in the wage-formation process. However, evidence on the sources of these firm wage premia is still sparse. In particular, there exists little evidence on how firm wage premia differ between firms bound by collective agreements and uncovered firms. We further lack evidence on how plant-level codetermination affect firm wage premia. To be sure, there exist both a large body of evidence (surveyed by Card et al., 2017) on the relationship between firm performance (measured as either productivity, profits, or quasi rents) and wages as well as considerable research into the wage effects of unions (Bryson, 2014) and works councils (Addison, Teixeira, and Zwick, 2010). However, as these two strands of the literature investigate wages rather than firm wage premia, they cannot shed light on how unions and worker codetermination as well as firm performance affect these premia. Furthermore, wage differences across firms may stem from workers with different unobserved abilities sorting into firms that differ in performance and their industrial relations regimes,

[1] Among these studies are Abowd , Lengermann, and McKinney (2003) for the US, Card, Heining, and Kline (2013) for West Germany, Card, Cardoso, and Kline (2016) for Portugal, as well as Macis and Schivardi (2016) for Italy.

thereby contaminating estimates.

What is more, applying the AKM approach to West German data Card, Heining, and Kline (2013, CHK hereafter) document an increasing wage inequality in West Germany that is to a large extent driven by a rise in the dispersion of firm wage premia or, as they put it, rising workplace wage inequality. They further provide some suggestive evidence that the dispersion of firm wage premia is larger among those firms which are unbound by collective agreements. The rising dispersion of firm wage premia may thus reflect a falling prevalence of collective agreements or works councils provided these institutions compress the premia distribution, which seems plausible. As a case in point, in firms paying low wage premia workers' better bargaining position rooted in collective bargaining coverage may raise the wage premium to a larger extent than in firms at the top of the premia distribution where workers arguably already capture considerable rents. Yet, we lack evidence on whether bargaining coverage or worker codetermination exert a differential impact on firm wage premia along the premia distribution.

Against this backdrop, this paper investigates the sources of firm wage premia and which of these sources are likely candidates for explaining the rising dispersion of firm wage premia in West Germany over time. To that end, we will proceed in three steps. First, we will analyze how the firm wage premium varies depending on firm performance and its industrial relations regime. Specifically, we will regress the CHK plant wage effect obtained from an AKM-type two-way fixed effects approach on dummies indicating coverage by a collective agreement and works council existence as well as the plant-level quasi rent, i.e. the surplus left after the factors of production have been paid their outside options (Abowd and Lemieux, 1993), as a performance measure. Our core finding will be that on average the wage premium is larger in plants with a works council and, to a lesser degree, in plants bound by a collective agreement, and further that quasi rents have a small positive influence on wage premia.

Second, we will investigate whether the impact of the industrial relations regime on wage premia changes over time by redoing our analysis from the first step separately for the beginning and the end of our observational window. Our core finding will be that the impact of both collective bargaining coverage and works council existence on the average wage premium rises over time as does the modest influence of the quasi rent.

Third, we will examine whether the impact of the industrial relations regime on the wage premium differs along the wage premium distribution using the recentered influence function (RIF) approach (Firpo, Fortin, and Lemieux, 2009). Running RIF regressions for the first and ninth decile as well as the variance of the premium distribution, we will find that collective bargaining is associated with less dispersed wage premia, whereas works council existence comes along with an increase in their dispersion. Hence, the decline in collective bargaining coverage turns out to be the only source of wage premia investigated in this study that contributes to the rising inequality in firm wage premia in West Germany.

The remainder of this paper is organized as follows. Section 2 provides an overview of the institutional setting in Germany and provides hypotheses for the impact of firms' industrial relations regime on firm wage premia. Section 3 describes our data and introduces our empirical approach. Section 4 presents and discusses our results, and Section 5 concludes.

## 2 Institutional setting and some hypotheses

In Germany, the constitutionally protected principle of bargaining autonomy gives trade unions and employers the right to regulate wages and working conditions absent state interference. Collective agreements are legally binding, are predominantly concluded as multi-employer agreements between a single union and an employers association at sectoral level, and almost always apply to all of the covered employers' workers irrespectively of workers' union status.[2] Although sectoral negotiations mostly take place in regional bargaining units, officials of the two bargaining parties closely coordinate the regional negotiations within one sector, and consequently variations between them are small. Furthermore, there even exists some cross-sectoral coordination by both parties, giving rise to some uniformity in collective bargaining policy across sectors (for further details, see also Hirsch and Schnabel, 2014).

Collective bargaining in Germany predominantly concerns wages, but also determines job classifications, working time, and working conditions. Norms stipulated in the collective agreement are generally minimum terms, and employers bound by the agreements thus cannot undercut, but only improve upon these terms and conditions. Exceptions to this general rule are in some cases laid down in so-called opening clauses that allow renegotiating collective bargaining issues, mostly wages and working time, at the plant level, typically under conditions of economic hardship.

Whereas a minority of employers do in fact pay wages higher than stipulated in the collective agreements (for details on this wage cushion, see Jung and Schnabel, 2011) and opening clauses have grown in importance in the last decades, for most workers the wages set in the collective agreements are crucial for the level and development of their actual wages. In 2015, 59 (31) percent of workers (plants) were covered by collective agreements in West Germany (Ellguth and Kohaut, 2016). Moreover, 51 percent of West German workers employed by uncovered plants were covered indirectly by collective agreements because 43 percent of plants report to act upon a sector-level agreement (Ellguth and Kohaut, 2016).

On average, plants covered by a collective agreement have a higher value added per worker (Mueller, 2011) and also pay higher wages (Gürtzgen, 2009). Furthermore, studies by Dustmann, Ludsteck, and Schönberg (2009), Baumgarten, Felbermayr, and Lehwald (2016),

2 More recently, some employers associations have started to offer memberships that do not force members to adopt collective agreements (Behrens, 2013).

as well as Biewen and Seckler (2017) find that declining unionization can account for a substantial part of the rise in German wage inequality. Yet, evidence on these links between collective bargaining coverage and wages has not to carry over to firm wage premia. Further, these findings may suffer from unobserved differences in worker quality between bound and unbound firms. As a case in point, workers of better quality, who arguably possess a better bargaining position than less qualified workers, may select into uncovered firms that are more flexible when it comes to decide on firm wage premia. Moreover, since collective agreements are predominantly concluded at sectoral level, they not only offer highly productive firms to save on transaction costs (and thus to save on total labor costs despite higher wage bills), but also offer high-productivity firms the opportunity of hiding behind less productive firms when choosing to get unionized. On account of this “hide effect”, highly productive firms may even pay lower wage premia when covered by a collective agreement, thereby hiding behind the collective wage.[3] We thus expect the impact of collective bargaining coverage to be less pronounced (or even negative) in the upper part of the wage premium distribution than in the lower part, thereby compressing the premium distribution. Investigating firm wage premia rather than wages permits us to test for these possibilities.

On top of collective bargaining typically conducted at sectoral level, the second backbone of the Germany’s dual system of industrial relations is given by worker codetermination at plant level through works councils, the German counterpart of the workplace union in other countries. Works councils are mandatory but not automatic in all plants with at least five permanent workers, for setting up a works council requires three workers or a union representative to initiate an election procedure in the plant. In 2015, 42 percent of workers in West Germany worked in the 9 percent of plants with a works council (Ellguth and Kohaut, 2016).

[3] This possibility of hiding behind a sector-level collective agreement oriented toward less productive and usually smaller firms in an industry has been recognized before in the industrial relations literature (e.g., Kohaut and Schnabel 2003). In a case study of a large firm in the German metalworking industry, Arrowsmith, Marginson, and Sisson (2003) report that the firm is committed to sector-level bargaining because it feels that the powerful metalworkers’ union would achieve more otherwise.

Whereas works councils are formally independent of unions, in practice most works councilors are union members (Behrens, 2009). The size of the works council is a function of the plant's employment level, and the entire cost of the works council apparatus is borne by the employer with works councilors being full time once certain threshold levels are reached.[4] Works councils have extensive information, consultation, and codetermination rights (for details, see Addison 2009). In particular, and in contrast to continental European counterparts of workplace representation, German works councils have codetermination rights on what are termed "social matters", which comprise remuneration arrangements, the commencement and termination of working hours, the regulation of overtime and reduced working hours, as well as health and safety measures. Works councils can also negotiate social plans, which establish compensation for the dislocation caused by (partial) plant closings and by major changes in plant organization. Other than unions, though, works councils may not call a strike and they are excluded from reaching agreement with the employer on wages and working conditions that are settled or normally settled by collection agreements between unions and employer associations at sectoral level. One exception to this general rule is that collective agreements contain opening clauses (mentioned before) that explicitly authorize works councils to do so.

However, even opening clauses are absent, works councils' extensive information, consultation, and codetermination rights on many other issues mean that works councils existence is likely to improve workers' bargaining position and thus to spur rent-seeking activities. In line with this conjecture, Addison, Schnabel, and Wagner (2001) show that plants with works councils pay higher effective wages (see also Addison, Teixeira, and Zwick, 2010). They further find that these plants are less satisfied with their profit situation,

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[4] Yet, works councils are absent in many eligible plants. Since actions by employers to block the introduction of a works council are legally prohibited, since works councilors enjoy additional employment protection, and since the time spent on work as works councilor counts as regular working time, this absence implies that introducing a works council imposes also costs on workers. Those costs comprise, for instance, the costs of becoming exposed as a works councilor, as many employers have reservations against works councils, as well as the costs of actively representing one's colleagues interests and being personally responsible for the negotiation outcomes. For a recent discussion of the causes of employer resistance against the introduction of works council, see Mueller and Stegmaier (2018).

whereas Mueller (2011) shows that they are in fact more profitable, which is readily explained by a positive productivity effect originating in works councils' collective voice (Addison, Schnabel, and Wagner, 2001, Mueller 2012, as well as Mueller and Stegmaier 2017) that dominates their adverse rent-seeking activities. As in the case of union wage effects, however, this evidence on the works council effect of wages does not necessarily carry over to firm wage premia, and it may be contaminated by worker sorting on unobservables. Furthermore, Germany saw an increased decentralization in wage formation recently, which is seen as one crucial ingredient in Germany's recovery from the "sick man of Europe" to an "economic superstar" (Dustmann et al., 2014). We thus not only expect a positive association between works council existence and wage premia, but also a rise in this positive association over time. Moreover, decentralization tendencies lending an increasing role to works councils in (local) wage formation may have led to a more pronounced impact of works councils on wage premia in high-performance plants in the upper part of premium distribution, and may thus even have led to a widening of the premium distribution in recent years.

# 3 Data and empirical approach

## 3.1 The LIAB cross-sectional model

Our data come from the LIAB cross-sectional model, a linked employer–employee data set provided by the Institute for Employment Research (IAB) and described in Alda, Bender, and Gartner (2005). The LIAB merges worker-level social security information with an establishment panel survey, the IAB Establishment Panel described in Ellguth, Kohaut, and Möller (2014). Starting in 1993the IAB Establishment Panel surveys establishments from all industries that employ at least one worker covered by the social security system on June 30 of the survey year, and is representative of the population of these establishments. Crucial for our purpose, the IAB Establishment Panel contains information on plants' value added, employment, wages, capital stock, and industrial relations regime (i.e. collective bargaining coverage and works council existence).[5] To fruitfully make use of this information in our application, we exclude plants in the agricultural and mining sectors, as well as the financial and public sectors, publicly-owned plants, and plants with less than five employees.

Since the worker-level data of the LIAB stem from the same source as used by CHK in their AKM-type wage decomposition, the LIAB data allow us to merge the plant fixed effects from CHK which will be our measure of plant wage premia (details will be given below). We further make use of the administrative data to construct a consistent sector classification as put forward by Eberle et al. (2011) and use the information on plant age and detailed plant location contained in these data. In order to minimize the impact of possible outliers in the survey data, we further decided to truncate the bottom and top one percent of the distributions of as well value added per worker as capital costs per worker within any two-digit industry–year cell. For the same reason, we truncated the top percent of the distribution of the wage bill per worker and, similar to CHK, dropped plants that pay less than €10 per worker as daily wage in the administrative data.

The IAB Establishment Panel oversamples large plants in all waves of the survey. Since the number of sampled plants rises over time and this rise is due to increased numbers of medium-sized and small plants, the extent of oversampling of large plants drops over time. This change not only leads to a drop in average plant size, but also affects other plant characteristics as, for example, smaller plants are on average less productive and pay lower wages than larger plants. For this reason, we will only present results from weighted samples. Besides, information on plants' wage cushions and on the distinction between sector-level and

[5] Although the IAB Establishment Panel has no direct information on plants' capital stock, it can readily be computed from the included investment data as put forward by Mueller (2008). Note further that data on wages and employment are also contained in the social security data. In our analysis, we decided to use the wage and employment information from the survey which is arguably more consistent to the survey information on plants' value added and capital stock that we will use to construct quasi rents than the worker-level information aggregated at plant level. What is more, wages in the social security data are top-coded at the contribution ceiling to the unemployment insurance, whereas the survey data contain plants' uncensored monthly wage bill.

firm-level bargaining, which would be of potential interest for our investigation, is not available for some early waves of the survey data. Lest to lose too many observations, we decided against using this information.[6]

## 3.2 Measuring wage premia

Most of the literature on how rents are shared between workers and employers (recently summarized by Card et al., 2017) relate workers' wages measured at industry, firm, or individual level to some measure of firm performance, such as productivity, profits, or quasi rents. Using wages, however, ignores that workers with high unobserved abilities may sort into jobs at high-performance employers (as, for example, shown by CHK), thereby overestimating the extent of rent sharing. Indeed, Card et al. (2017) demonstrate that the rent-sharing coefficient obtained from regressing wages on some measure of firm performance mixes up returns to observed worker shills, returns to unobserved worker skills, and true rent sharing. Using firm wage premia that come from an AKM-type decomposition approach rather than wages avoids the bias from worker sorting as premia are net of unobserved worker heterogeneity as explained below.

Our measure of the plant wage premium therefore builds on the decomposition approach of AKM, which splits up individual wages into worker-specific and plant-specific components. In the AKM framework, the log wage of worker $i$ in period $t$ is decomposed as

$$\log w_{it} = \alpha_i + \varphi_{j(i,t)} + x_{it}'\beta + u_{it} \tag{1}$$

where $\alpha_i$ is a fixed log wage component specific to worker $i$, $\varphi_{j(i,t)}$ is a fixed log wage component specific to plant $j$ employing worker $i$ at time $t$, $x_{it}'\beta$ is a time-varying log wage component stemming from time-varying worker characteristics $x_{it}$ that are rewarded equally

6 Note that information on collective bargaining coverage and work council existence is available for the first wave of the IAB Establishment Panel, but is missing in some of the following waves. Since both variables are highly persistent over time, we imputed missing values with the value of adjacent observations for the same plant provided these are unchanged.

across plants, and $u_{it}$ is an idiosyncratic log wage component. In the AKM framework, $\alpha_i$ reflects the worker's time-invariant human capital, such as education and ability, and $x'_{it}\beta$ mirrors the worker's time-varying human capital, such as experience, that affects his or her productivity regardless of where the job is held, while $\varphi_{j(i,t)}$ gives the percentage wage premium enjoyed by every worker employed at plant $j$.

The crucial assumption for this interpretation of the AKM decomposition to hold is that the idiosyncratic log wage component $u_{it}$ is unrelated to the sequence of worker $i$'s employers $\{j(i,t)\}_t$. In their estimation of the AKM model for Germany, CHK not only demonstrate that this decomposition accounts for about 90 percent of the variation in the wages of full-time workers, but also present evidence that the assumptions of AKM hold and thus, in particular, the interpretation of $\varphi_{j(i,t)}$ as plant $j$'s wage premium is valid. The estimated worker and plant effects from CHK are available to the scientific community (Card, Heining, and Kline, 2015), and we gratefully make use of their plant effects as our measure of plant wage premia.

## 3.3 Measuring quasi rents

Rents to be shared between workers and employers consist of the surplus that is generated by the plant. A proper measure of this surplus is the quasi rent, i.e. value added net of the costs for the competitively priced capital and labor inputs (e.g., Abowd and Lemieux, 1993). To arrive at the quasi rent, it is crucial to properly account for workers' outside options that, in turn, depend on their unobserved abilities. Typically, studies that use quasi rents as measure of firm performance proxy outside options by average wages in the same industry (e.g., Abowd and Lemieux, 1993, Van Reenen, 1996, and Gürtzgen, 2009). As with analyzing wages rather than wage premia, however, using the uncorrected quasi rent as performance measure ignores that high-ability workers sort into jobs with high-rent employers and thus gives rise to bias.

To our knowledge, Abowd and Allain (1996) are the only to account for workers' unobserved abilities in basing their measure of quasi rents on the worker fixed effects from an AKM-type decomposition approach.[7] In order to arrive at a proper measure of rents that accounts for worker sorting and the resulting heterogeneity in workers' outside options, we follow their approach when constructing the quasi rent. The quasi rent at plant level $QR$[8] is defined as

$$QR = VAD - xN - rK \tag{2}$$

where $VAD$ denotes the plant's value added, i.e. sales net of the value of intermediates, $x$ denotes workers' outside wage, $N$ denotes the plant's number of workers, $r$ denotes the competitive rental price of capital, which we compute from the plant's capital stock distinguishing between prices for debt and prices for equity at sector level, and $K$ denotes the plant's capital stock.[9]

When constructing workers' alternative wage $x$ we again follow Abowd and Allain (1996) and calculate workers' outside option as

$$\log x = \log\left(\frac{wage}{N}\right)_s + \left(\alpha_j - \bar{\alpha}_s\right) - (\bar{\varphi}_s - \varphi_{s10}), \tag{3}$$

separately for each of the three time intervals 1990–1996, 1996–2002, and 2002–2009 for which CHK provide worker and plant effects that can be merged to the establishment survey data. In equation (3), $\left(\frac{wage}{N}\right)_s$ is the average plant-level wage per worker in the respective

7 Note that even the most recent study by Card et al. (2017), which uses firm wage premia from an AKM-type decomposition as dependent variable, sticks to value added as profitability measure and thus does not correct for worker sorting and the resulting heterogeneity in workers' outside options.

8 In contrast to the quasi rent definition (2), workers and employer may split rents before deducting the capital costs. Following the approach outlined in Card, Devicienti, and Maida (2014) we find no evidence for bargaining over the competitive returns to capital, which is in line with their findings for Italy.

9 Note that the IAB data do not contain information on debt and equity financing at plant level. We therefore use the sector-level information provided by Aswath Damodaran at http://pages.stern.nyu.edu/~adamodar/. Specifically, we assessed data on the "costs of capital by industry sector" for Europe issued on January 5, 2016. Using additional data on the long-run treasury bond rate for Germany gives an average rental rate of capital of 10.8 percent during 1994–1996, 9.2 percent during 1997–2002, and 8.1 percent during 2003–2009.

one-digit sector,[10] $\alpha_j$ is the average CHK worker effect (log wage component) in plant $j$, $\bar{\alpha}_s$ is the average CHK worker effect in the one-digit sector, $\varphi_{s10}$ is the 10th percentile of the distribution of the CHK plant effects in the one-digit sector, and $\bar{\varphi}_s$ is the average CHK plant effect in the one-digit sector.

In equation (3), the term $\alpha_j - \bar{\alpha}_s$ captures the deviation (in logs) in worker quality between plant $j$ and the sector average and thus accounts for ability differences in the workforces across employers. Yet, note that CHK estimate worker and plant effects separately for male and female workers and thus the level of both worker and plant effects cannot be meaningfully compared across the genders. As a consequence, computing average plant-level worker (plant) effects by simply averaging over the worker (plant) effects at plant level would not account for differences in the gender composition of the workforce, and thus such an aggregation would be uninformative on the average worker (plant) effect that mirrors the average workforce ability (the plant wage premium). We therefore purge within each year–gender cell the year–gender mean of the worker (plant) effect from the individual worker (plant) effect and use these demeaned CHK effects when computing the average worker (plant) effect at plant level. Besides, CHK calculate worker effects only for full-time workers. Hence, $\alpha_j - \bar{\alpha}_s$ is unknown for part-time workers who make up 14 percent of the workers in our sample. Under the assumption that full-time workers and part-time workers who hold jobs at the same plant did not differ in $\alpha_j - \bar{\alpha}_s$, that is $\left(\alpha_j - \bar{\alpha}_s\right)_{FT} = \left(\alpha_j - \bar{\alpha}_s\right)_{PT}$, our measure of workers' outside options would not depend on the share of part-time workers in the plant's workforce. We will impose this assumption in the following, and we will provide further explanations and checks for robustness in Appendix A.

The outside option of plants' workers further depends on the wage premia paid by

[10] This measure of average industry wages closely mimics what the before mentioned studies employed as their measure for workers outside option.

possible future employers. In equation (3), subtracting the term $\bar{\varphi}_s - \varphi_{s10}$ from $\log\left(\frac{wage}{N}\right)_s + \left(\alpha_j - \bar{\alpha}_s\right)$ means subtracting the spread between the average CHK plant effect in the respective one-digit sector and the 10th percentile of the plant effects there. In that we assume that workers are risk averse and independently of the wage premium obtained from their current employer expect to receive just a modest pay premium at the 10th percentile of the premium distribution when changing employers. That workers, in their expectations, consider random draws from the premium distribution is plausible within the AKM framework, in which the same plant pay premium is enjoyed by all workers of a certain plant and is thus, in particular, independent of individual workers' characteristics. We will impose this assumption in the following, and we will provide additional explanations and checks for robustness in Appendix B.[11] Also, the choice of the 10th percentile of the sector-level plant premium distribution as risk-averse workers' reference point is somewhat arbitrary. We therefore decided to experiment with different choices, such as the 25th percentile of this distribution, and obtained very similar results (see Appendix B).

CHK estimate plant wage and worker wage effects for four time intervals, where we use those for the latter three time intervals 1990–1996, 1996–2002, and 2002–2009 and merge these to the plant-level survey information for the years 1994–2009. Note that within these time intervals, CHK normalize their plant wage effects by omitting the last plant dummy in their wage equation (CHK, 988). In other words, plant wage effects have to be interpreted "relative to the last plant in the sample" and for this reason the level of these effects jumps across time intervals and has no clear-cut interpretation.[12] Notwithstanding, within the tree

[11] One objection to our approach is that some bargaining models consider receiving unemployment benefits rather than holding alternative jobs as the relevant outside option available to workers. Note, however, that the wage cut induced by subtracting $\bar{\varphi}_s - \varphi_{s10}$ roughly mimics the gap between continued wage payments and recipience of unemployment benefit. At the end of our observational window the replacement rate of the German unemployment insurance was 60–67 percent of the net wage in the last employment, which corresponds well with the average $\bar{\varphi}_s - \varphi_{s10} = -0.37$ in our sample.

[12] Because of this feature of the CHK plant wage effects (and because of transitory fluctuation in average plant-level wages) it is not possible to infer workers' outside option from simply subtracting the CHK plant effect

time intervals differences in the CHK wage effects across plants can be readily interpreted as can be their variances and other distributional parameters, such as the interdecile range, across the time intervals. Against this background, a further useful feature of subtracting the spread $\bar{\varphi}_s - \varphi_{s10}$ in equation (3) is the invariance of workers' outside options to the jumps of the level of the CHK plant wage effects across time intervals, so that we can interpret the evolution of quasi rents over time.

## 3.4 Wage premium regressions

To investigate the sources of plant wage premia, we regress the CHK plant wage effect $\varphi_j$ on plant $j$'s quasi rent $QR_j$ calculated as in equation (2), dummies for collective bargaining coverage $CWB_j$ and works council existence $WOCO_j$, and a rich set of plant-level controls.[13] Our baseline specification is thus given by

$$\varphi_j = \beta_0 + \beta_1 QR_j + \beta_2 CWB_j + \beta_3 WOCO_j + Controls_j' \gamma + \varepsilon_j \tag{4}$$

where the controls comprise 32 district dummies (*Regierungsbezirke*), 62 two-digit sector dummies, four plant size dummies, four plant age dummies, a dummy indicating plants not belonging to a multi-branch company, and the percentages of women as well as part-time workers in the plant's workforce.[14] For selective descriptive statistics on our final regression samples in the three time intervals 1994–1996, 1996–2002, and 2002–2009, for which CHK provide separate plant wage effects, see Table 1. Table 2 compares the dispersion of the CHK

from the average plant-level log wage.

[13] As an alternative measure of the plant wage premium, we also experimented with using the difference between the average plant-level wage bill per worker net of the plant-level outside option of workers. Note that this alternative measure is highly correlated with the CHK plant wage effect and using it as alternative regressand did not change any of our conclusions. For the sake of comparability with the existing literature, such as CHK, though, we decided against using this alternative measure in our main specifications.

[14] Apart from these controls, some further plant characteristics contained in the IAB Establishment Panel might be of potential interest, such as information on plants' export activity, on foreign ownership, on the wage cushion, or on whether there exist formal profit-sharing agreements. Yet, including these variables would lead to a massive drop in observations and is thus not viable in our application because some of them have a considerable extent of missing values while others were completely absent in some of the waves of the survey.

plant wage effects at worker level, i.e. as in the original CHK paper, and in our final sample (using the sample weights of the IAB Establishment Panel). Notably, both at the worker level and at the plant level the dispersion of the CHK plant wage effects is very similar. Further, at both levels of aggregation their standard deviation is rising by about 0.06 log points or roughly a third during our observational window, which reflects the rise in workplace wage heterogeneity at the heart of CHK's contribution.

We will estimate equation (4) by OLS pooling the observations from all three time intervals as well as separately for each of the time intervals, where the latter allows us to assess whether the drivers of average plant wage premia change over time.[15] To examine how quasi rents and industrial relations shape the dispersion of wage premia across plants, we will further run RIF regressions (Firpo, Fortin, and Lemieux, 2009) that regress the variance of the premia as well as their first and ninth deciles on the same set of regressors as in model (4).

# 4 Results

## 4.1 OLS wage premium regressions

Table 3 presents the core results of a group of OLS regressions in which we regress the CHK plant wage effect on the plant's quasi rent, dummies for collective bargaining coverage and works council existence, and the controls detailed above. When pooling observations for the entire observation period 1994–2009, the relation between the wage premium and the quasi rent is statistically significant at the 1 percent level. An increase in the quasi rent per worker by €100,000 is associated with a rise in the wage premium by 6.8 log points, which means that a rise in the quasi rent by one standard deviation (i.e. roughly €40,000 per worker) comes

15 Note that before pooling data over the time intervals we purge the sample means in each interval from the CHK plant wage effects. Doing so takes account of the jumps in the levels of the plant effects due to CHK's normalization, which we discussed in detail in the previous subsection.

along with an increase in the premium by just an eighth of a standard deviation (i.e. about 2.7 log points). Or, to make our result more comparable to existing studies on rent sharing, one may note that the average quasi rent per worker is about €18,000 so that a 1 percent increase in the quasi rent by €180 is associated with an increase in the plant wage premium by 0.012 percent. This rent-sharing elasticity is at the lower end of the range of estimates of earlier studies surveyed by Card et al. (2017). Yet, one should keep in mind that those studies analyze wages rather than wage premia and may thus suffer from worker sorting, thereby getting upward-biased rent-sharing elasticities (Card et al., 2017). Albeit statistically significant, we conclude that the association between plants' quasi rent and the plant wage premium is insignificant from an economic point of view.

Turning to plants' industrial relations regime, works council existence comes along with a sizeable increase in the premium that is again statistically significant at the 1 percent level. In plants with a works council, the wage premium is 5 log points larger, *ceteris paribus*, which corresponds to the change in the premium associated with a rise in the quasi rent per worker by about two standard deviations. On the other hand, the association between collective bargaining coverage and the wage premium is less pronounced and only statistically significant at the 5 percent level. Plants bound by a collective agreement pay an additional premium of 1.9 log points, *ceteris paribus*, which is a bit more than a third of the works council effect. Overall, whereas plant performance seems to exert just a minor influence on the level of plant wage premia, plants' industrial relations regime plays a much larger role, in particular plant-level worker codetermination through works councils.

When running separate regressions for the three time intervals 1994–1996, 1996–2002, and 2003–2009, for which CHK provide separate plant wage effects, we find that the impact of the plant's quasi rent, of works council existence, and of collective bargaining coverage on the plant wage premium is largest in the latest 2003–2009 interval. Specifically, the additional wage premium when working for a plant bound by a collective agreement is near zero in the earlier intervals and accounts to 3.5 log points in this latter period, which turns out to be statistically significant at the 1 percent level. Similarly, the additional wage premium associated with a works council rises somewhat over time, as does the relation between the plant's quasi rent and the wage premium, which nevertheless remains very small in magnitude. Besides, this latter finding is balanced to some extent by a drop in the dispersion of quasi rents across plants, which is lowest (roughly 20 percent less compared to the 1996–2002 interval) at the end of our observational window.

In summary, we find that plants with a works council and plants bound by collective agreements pay larger wage premia as do plants with higher quasi rents per worker, though the latter relationship is very modest in size. Our results further show that the influence of all these variables on the average plant wage premium rises over time and are most pronounced at the end of our observational window. In terms of the level of the plant wage premium, where one is working thus gains in importance over time.

As plant wage premia also become more dispersed over time, our findings, in turn, lead to the question of whether recent trends in industrial relations, such as the rise of the so-called codetermination-free zone with neither collective bargaining nor works councils (for details, see Oberfichtner and Schnabel, 2017), may have contributed to the steady rise in the dispersion of wage premia across plants observed by CHK and also present in our data (see Table 2). But before we will turn to this question, we have to check how quasi rents and industrial relations relate to the wage premia dispersion, which we will do next.

## 4.2 RIF wage premium regressions

In order to investigate the influence of plant performance and industrial relations on the dispersion of the wage premium across plants, we run RIF regressions for the variance and the first and ninth decile of the wage premium distribution including the same set of regressors as in the OLS regressions. Table 4 presents the core of a group of RIF regressions when pooling observations for 1994–2009 as well as when running separate regressions for the three time intervals 1994–1996, 1996–2002, and 2003–2009, for which CHK provide separate plant wage premia.

In the pooled sample, a larger quasi rent is associated with a lower premia variance, which is statistically significant at the 5 percent level, and a lower interdecile range of wage premia across plants. The latter finding is visible from the somewhat larger influence of the quasi rent at the first decile compared to the ninth decile of the premium distribution. An increase in the quasi rent per worker by €100,000 comes along with a rise in the first (ninth) decile of the premium distribution by 7.8 (5.5) log points, which is statistically significant at the 1 percent level. As stressed earlier, though, these numbers point at a very modest influence of quasi rents on wage premia and the difference in these thus at a negligible impact on quasi rents on the dispersion of premia across plants. Running separate RIF regressions for the three time intervals, we further see no clear changes in the influence of quasi rents on wage premia dispersion. Whereas the influence on the variance is somewhat increasing, the differential impact on the first and ninth decile is getting somewhat less pronounced.

In short, quasi rents have virtually no influence on the dispersion of wage premia across plants. Due to their minor impact on premia, changes in (the distribution of) plants' quasi rents offer no plausible point of departure for explaining the rise in this dispersion that we observe. So, for instance, the explanation that a possible widening of the distribution of quasi rents across plants due to intensified competition in increasingly globalized product markets have led to progressively diverse plant wage premia, though plausible *prima facie*, is not supported by our findings.

Turning to industrial relations, collective bargaining coverage and works council existence differ in their influence on the dispersion of plant wage premia across plants. In the pooled sample comprising the years 1994–2009, the variance of wage premia is larger in plants with a works council, which is statistically significant at the 1 percent level, as is the interdecile range. Works council existence is associated with an increase in the wage premium by 7.6 log points at the ninth decile, which is statistically significant at the 1 percent level, whereas there shows up no such association at the first decile. As a result, the wage premium distribution is wider in plants with a works council, *ceteris paribus*, with premia being lifted at the upper part of the distribution. Running separate RIF regressions for the three time intervals 1994–1996, 1996–2002, and 2003–2009, we further find that this pattern of a differential influence of works council existence at the lower and the upper part of the premium distribution is least pronounced in the early 1994–1996 period.

Together, these finding are in line with the notion that works councils use their local bargaining power to capture additional rents in high-premium plants and are increasingly able so more recently due to rising decentralization in the wage-formation process. That said, the decline in plant-level worker codetermination in recent years (see Ellguth and Kohaut, 2016, as well as Oberfichtner and Schnabel, 2017) is ruled out as a plausible explanation for more dispersed wage premia across plants because falling prevalence of works councils is expected to narrow the premium distribution rather than to widen it.

These findings for worker codetermination through works councils contrast with those for collective bargaining coverage. In the pooled sample, the variance of wage premia is lower in covered than in uncovered plants, which is statistically significant at the 1 percent level, as is the interdecile range. Whereas bargaining coverage is associated with a 5.1 log points larger wage premium at the first decile of the premium distribution, which is statistically significant at the 5 percent level, there is no such association at the ninth decile. As a result, the wage premium distribution in covered plants is compressed from below. This finding is clearly in line with collective agreements that settle minimum terms that are more binding for low-premium plants and less so for high-premium plants, which may even exploit collective agreements to "hide" behind the collective wage. Running separate RIF regressions for the three time intervals 1994–1996, 1996–2002, and 2003–2009, we further see that this pattern of a differential influence of bargaining coverage on the first and ninth deciles of the plant wage premium is most pronounced in the most recent 2003–2009 period.

Together, these findings make clear that falling collective bargaining coverage in recent years (see Ellguth and Kohaut, 2016, as well as Oberfichtner and Schnabel, 2017) offers one plausible explanation for the rise in the dispersion of wage premia across plants, as was also suggested by CHK. This result additionally squares up with existing evidence for wages (rather than wage premia) by Dustmann, Ludsteck, and Schönberg (2009), Baumgarten, Felbermayr, and Lehwald (2016), as well as Biewen and Seckler (2017) that declining unionization can account for a substantial part of the rise in German wage inequality. Yet, we should also stress that the rise in the variance of plant wage premia from 0.041 in the 1994–1996 period to 0.069 in the 2003–2009 period is accompanied by a non-negligible increase in the variance of the OLS regression residuals. The variance of the regression residuals increases from 0.026 in the 1994–1996 period to 0.050 in the 2003–2009 period. In other words, a substantial part of the rise in the dispersion of plant wage premia across plants is not accounted for by the variables in the model, which suggests that deunionization is only part of the story behind the widening of the plant wage premium distribution over time.

# 5 Conclusions

Linking employer survey data to administrative data for West Germany for the years 1994–2009, this paper has investigated how the level and the dispersion of wage premia across plants depends on plants' performance and their industrial relations regime. To measure wage premia, we used the plant wage effects of CHK that stem from an AKM-type two-way fixed effects decomposition of individual workers' log wages. Hence, our wage premium measure gives the wage premium enjoyed by every worker employed by a certain plant and accounts for worker sorting into plants that may have contaminated prior studies based on workers' individual wages rather than plant wage premia. In our econometric analysis, we regressed the plant wage premium on the plant's quasi rent per worker as a performance measure, dummies for the existence of collective agreements and a works council, which mirror the plant's industrial relations regime, and a rich set of control variables. Similarly to our wage premium measure, we measured plant performance such that worker sorting into plants is accounted for in that we based the quasi rent per worker on the outside options available to a plant's workforce making use both of the plant and the worker wage effects of CHK.

In OLS regressions pooling observations for our entire observational window, we found that level of the plant wage premium is only marginally influenced by plant performance, with a rent-sharing elasticity of just 0.012, i.e. a 0.012 percent increase in the premium for a 1 percent rise in the quasi rent per worker. This small impact of plant performance contrasts with the influence of a plant's industrial relations regime. Plant-level codetermination through a works council comes along with an additional plant wage premium of 5.0 log points while collective bargaining coverage is associated with an extra premium of 1.9 log points. Running separate OLS regressions for the three time intervals within our observational window, for which CHK provide separate plant wage premia, we further found that all these drivers of the level of the plant wage premium gained in importance over time. These results imply that union wage differentials and, even more so, wage effects of worker codetermination are not (fully) explained by unobserved differences in worker quality and that worker representations are able to negotiate a genuine wage premium. We even show that over time unions and works councils became increasingly able to generate premia. Panel attrition in our data, however, does not allow us to test convincingly whether this observed increase in rent extraction was driven by changes in the type of firms covered or by changes in bargaining power within existing firms over time.

In RIF regressions for the variance as well as the first and the ninth decile of the wage premium, we further saw that the quasi rent has a negligible impact on the dispersion of wage premia across plants whereas collective bargaining coverage is associated with less dispersed premia and the opposite holds for works council existence. While we found that collective bargaining compresses the premium distribution from below, works council existence is associated with larger additional wage premia in high-premium than in low-premium plants, thereby adding to the premium dispersion across plants. Hence, the widening of the plant wage premium distribution over time observed in our data cannot be plausibly related to more dispersed performance across plants, nor to the drop in works council prevalence over time. Among the suspects in this study, only the fall in collective bargaining coverage contributes to explaining the rise in the wage premium dispersion across plants. These findings imply, for instance, that the explanation that a possible widening of the distribution of quasi rents across plants due to intensified competition in increasingly globalized product markets have led to progressively diverse plant wage premia, though plausible *prima facie*, is not supported. Instead, any effects of trade integration and new technologies on the firm component of wages in Germany must work mainly via affecting bargaining institutions and not via affecting firm performance directly. Yet, most of this rise in premia variance remains unaccounted for despite the large set of explanatory variables in our regressions.

# Appendices

## A Outside options of full-time workers and part-time workers

In our main specifications (see the OLS and RIF regressions in Tables 3 and 4), we calculated the plant's quasi rent based on its workers' outside wage according to equation (3). Since we lack CHK worker (and plant) wage effects for part-time workers, we assumed that full-time workers and part-time workers holding jobs at the same plant $j$ do not differ in the spread between their average fixed effect at the plant level and their average fixed effect at the one-digit sector level, that is we assumed $\left(\alpha_j - \bar{\alpha}_s\right)_{FT} = \left(\alpha_j - \bar{\alpha}_s\right)_{PT}$. Under this assumption, our measure of workers' outside options does not depend on the share of part-time workers in the plant's workforce and the lack of information on part-time workers is innocuous.

An alternative assumption with some plausibility, though, would be that the observable $(\alpha_j - \bar{\alpha}_s)_{FT}$ and the unobservable $(\alpha_j - \bar{\alpha}_s)_{PT}$ are of the same sign with the former exceeding the latter, meaning that the quality of part-time workers is still positively correlated with the quality of full-time workers but less dispersed across plants. If this latter case were true, the dispersion of workers' outside options across plants would be smaller than the one we get by imposing $(\alpha_j - \bar{\alpha}_s)_{FT} = (\alpha_j - \bar{\alpha}_s)_{PT}$. In order to limit possible bias stemming from this latter possibility, we control for the share of part-time workers in the plant's workforce in all our specifications. And to further scrutinize the sensitivity of our findings, in a robustness check we imposed the extreme case $(\alpha_j - \bar{\alpha}_s)_{PT} = 0$ instead, that is we switched off any (positive) correlation between the quality of a plant's full-time workforce and its part-time workforce. Reassuringly, even the imposition of this extreme case did not affect our results by much (see the first column of the Appendix Table for an OLS regression for the entire sample analogous to the one in the first column of Table 3).

## B Alternative assumptions regarding workers' outside options

When calculating the plant's quasi rent based on its workers' outside wage according to equation (3), we subtracted the term $\bar{\varphi}_s - \varphi_{s10}$, i.e. the spread between the average CHK plant effect in the respective one-digit sector and the 10th percentile of the effects there, from $\log\left(\frac{wage}{N}\right)_s + (\alpha_j - \bar{\alpha}_s)$. In that we assumed that workers are risk averse and independently of their current plant wage premium expect to receive just a modest premium at the 10th percentile of the premium distribution when changing employers. As already stressed, the assumption that the future plant wage premium is unrelated to the current premium as well as to worker characteristics is valid in the AKM framework.

That said, one could leave the AKM framework and entertain the assumption of assortative matching where high-ability workers (in terms of their $\alpha_i$) expect to gain jobs at high-premium plants (in terms of plants' $\varphi_j$). To gauge the possible impact of such an alternative assumption regarding our measure of workers' outside wage, we calculated the slope coefficient of an univariate OLS regression of the CHK plant wage effects on the CHK worker wage effects from the sample standard deviations and correlations reported in CHK's Table 3. For example, for the 1990–1996 interval the covariance of the plant and wage effects amounts to $0.097 \cdot 0.304 \cdot 0.172 = 0.0051$ and thus the slope coefficient is $\frac{0.0051}{0.304^2} = 0.055$, meaning that a rise in the worker wage effect by 10 log points is associated with an increase in the plant wage effect by just 0.55 log points. Put differently, a one standard deviation increase in the worker wage effect (i.e. by 30 log points) is expected to raise the plant wage effect by just 1.65 log points, which is slightly less than a tenth of a standard deviation of the plant wage effects. Since it is unclear whether (and if to what extent) workers anticipate the correlation of worker and plant wage effects, and since the relationship between the two is small in magnitude even under full anticipation, we do not think that staying within the AKM framework and ignoring this type of assortative matching poses much of a problem.

The second question concerns how risk averse workers really are. One may readily argue that subtracting $\bar{\varphi}_s - \varphi_{s10}$ from $\log\left(\frac{\overline{wage}}{N}\right)_s + \left(\alpha_j - \bar{\alpha}_s\right)$, meaning that workers expect to end up as low as at the first decile of the plant wage premium distribution in their current sector, imposes quite a pronounced level of risk aversion. As robustness check, we therefore repeated our analysis subtracting $\bar{\varphi}_s - \varphi_{s25}$ instead, suggesting a considerably lower degree of risk aversion among workers. Reassuringly, doing so left our results unaltered (see the second column of the Appendix Table for an OLS regression for the entire sample analogous to the one in the first column of Table 3). We further experimented with using average worker and average plant wage effects for the entire sample instead of averages at the one-digit sector level, i.e. subtracting $\bar{\varphi} - \varphi_{10}$ from $\log\left(\frac{wage}{N}\right) + \left(\alpha_j - \bar{\alpha}\right)$ when calculating workers' outside wage. Doing so did not change our insights.

## C Further checks of robustness

In two further robustness checks, we repeated our analysis omitting the capital stock information when calculating the quasi rent according to equation (2) and restricting to the more homogenous group of manufacturing plants (see the third and fourth columns of the Appendix Table for an OLS regression for the entire sample analogous to the one in the first column of Table 3). Omitting the capital stock does not change our results and considering the subsample of manufacturing plants leaves our main insights unchanged, too, though we find a somewhat higher effect of the quasi rent there.

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# Tables

## Table 1: Descriptive statistics at plant level (using sample weights)

| | 1994–1996 | | 1997–2002 | | 2003–2009 | |
|---|---|---|---|---|---|---|
| | Mean | St.Dev. | Mean | St.Dev. | Mean | St.Dev. |
| *In €1000 per year and worker* | | | | | | |
| Wage | 23.4 | 10.8 | 25.4 | 12.2 | 25.5 | 12.8 |
| Value added | 51.4 | 44.9 | 48.7 | 48.1 | 48.5 | 39.9 |
| Workers' outside wage | 23.2 | 6.0 | 23.5 | 6.2 | 23.1 | 7.3 |
| Costs of capital | 10.2 | 15.4 | 7.8 | 12.0 | 7.0 | 10.6 |
| Quasi rent | 18.1 | 44.2 | 17.3 | 46.2 | 18.4 | 38.5 |
| *Other plant characteristics* | | | | | | |
| Percentage part-time | 18.3 | 20.1 | 18.3 | 20.9 | 21.1 | 21.2 |
| Percentage female | 33.9 | 27.6 | 34.3 | 27.4 | 36.3 | 27.5 |
| Collective bargaining | 0.76 | 0.43 | 0.61 | 0.49 | 0.48 | 0.50 |
| Works council | 0.13 | 0.34 | 0.11 | 0.31 | 0.09 | 0.29 |
| Single plant firm | 0.88 | 0.33 | 0.88 | 0.33 | 0.86 | 0.35 |
| Plant size: | | | | | | |
| ≤ 10 workers | 0.50 | 0.50 | 0.47 | 0.50 | 0.45 | 0.50 |
| 11-50 workers | 0.40 | 0.49 | 0.44 | 0.50 | 0.45 | 0.50 |
| 51-250 workers | 0.09 | 0.28 | 0.09 | 0.28 | 0.09 | 0.28 |
| > 250 workers | 0.01 | 0.12 | 0.01 | 0.11 | 0.01 | 0.12 |
| Observations | 495,880 | | 1,508,950 | | 1,824,611 | |
| Plants (unweighted) | 1,213 | | 4,977 | | 6,079 | |

*Notes*: IAB Establishment Panel, 1994–2009, and CHK plant wage effects provided by Card, Heining, and Kline (2015).

## Table 2: Standard deviations of the plant wage effects

| | (1) | (2) | (3) | (3) – (1) |
|---|---|---|---|---|
| | 1990–1996 | 1997–2002 | 2003–2009 | |
| CHK (worker level, male workers only) | 0.172 | 0.194 | 0.230 | 0.058 |
| LIAB (plant level, final sample, weighted) | 0.200 | 0.221 | 0.262 | 0.062 |

*Notes*: The first row refers to Table III in CHK. The second row refers to our final sample that aggregates male and female plant wage effects from CHK at plant level in the LIAB data and weights the data using the sample weights of the IAB Establishment Panel.

## Table 3: Wage premium regressions (OLS using sample weights)

| | (1) 1994–2009 | (2) 1994–1996 | (3) 1997–2002 | (4) 2003–2009 |
|---|---|---|---|---|
| Quasi rent per worker (in €100,000) | 0.068*** | 0.062*** | 0.055*** | 0.091*** |
| | (0.008) | (0.016) | (0.009) | (0.013) |
| Collective wage agreement | 0.019** | –0.012 | 0.012 | 0.035*** |
| | (0.008) | (0.025) | (0.011) | (0.012) |
| Works council | 0.050*** | 0.044*** | 0.029* | 0.067*** |
| | (0.010) | (0.017) | (0.015) | (0.012) |
| Observations | 3,829,441 | 495,880 | 1,508,950 | 1,824,611 |
| Plants (unweighted) | 9,054 | 1,273 | 4,977 | 6,079 |
| $R^2$ | 0.25 | 0.35 | 0.37 | 0.27 |

*Notes*: IAB Establishment Panel, 1994–2009, and CHK plant wage effects provided by Card, Heining, and Kline (2015). The regressand is the CHK plant wage effect. The control variables consist of 32 district dummies (*Regierungsbezirke*), 62 two-digit sector dummies, four plant size dummies, four plant age dummies, a dummy for a single plant (as opposed to a plant belonging to a multi-branch company), and the percentages of women as well as part-time workers in the plant's workforce. Standard errors (in parentheses) are clustered at the plant level. ***/**/* indicates statistical significance at the 1/5/10 percent level.

## Table 4: Wage premium RIF regressions (using sample weights)

| | (1) 1994–2009 | (2) 1994–1996 | (3) 1997–2002 | (4) 2003–2009 |
|---|---|---|---|---|
| *Variance* | | | | |
| Quasi rent per worker (in €100,000) | –0.012** | –0.008 | –0.010** | –0.017 |
| Collective wage agreement | –0.013*** | –0.015 | 0.001 | –0.014* |
| Works council | 0.018*** | 0.003 | 0.020** | 0.016* |
| *First decile* | | | | |
| Quasi rent per worker (in €100,000) | 0.078*** | 0.063 | 0.076 | 0.093** |
| Collective wage agreement | 0.051** | 0.004 | 0.010 | 0.071** |
| Works council | 0.006 | 0.021 | –0.018 | 0.050* |
| *Ninth decile* | | | | |
| Quasi rent per worker (in €100,000) | 0.055*** | 0.016 | 0.023** | 0.073*** |
| Collective wage agreement | –0.004 | –0.036* | 0.012 | 0.012 |
| Works council | 0.076*** | 0.029 | 0.056*** | 0.070*** |
| Observations | 3,829,441 | 495,880 | 1,508,950 | 1,824,611 |
| Plants (unweighted) | 9,054 | 1,273 | 4,977 | 6,079 |

*Notes*: IAB Establishment Panel, 1994–2009, and CHK plant wage effects provided by Card, Heining, and Kline (2015). The regressand is the respective parameter of the distribution of the CHK plant wage effects. The control variables consist of 32 district dummies (*Regierungsbezirke*), 62 two-digit sector dummies, four plant size dummies, four plant age dummies, a dummy for a single plant (as opposed to a plant belonging to a multi-branch company), and the percentages of women as well as part-time workers in the plant's workforce. Standard errors (in parentheses) come from a block bootstrap at plant level with 500 replications. ***/**/* indicates statistical significance at the 1/5/10 percent level.

## Appendix Table: Wage premium regressions (OLS using sample weights)

| | (1) Part-time workers | (2) Risk aversion (25th percentile) | (3) Capital stock omitted | (4) Manufacturing only |
|---|---|---|---|---|
| Quasi rent per worker (in €100,000) | 0.062*** | 0.070*** | 0.074*** | 0.120*** |
| | (0.007) | (0.008) | (0.008) | (0.015) |
| Collective wage agreement | 0.019** | 0.019** | 0.017** | 0.014 |
| | (0.008) | (0.008) | (0.008) | (0.010) |
| Works council existence | 0.051*** | 0.050*** | 0.051*** | 0.047*** |
| | (0.010) | (0.010) | (0.010) | (0.011) |
| Observations | 3,829,441 | 3,829,441 | 3,829,441 | 825,416 |
| Plants (unweighted) | 9,054 | 9,054 | 9,054 | |
| $R^2$ | 0.24 | 0.25 | 0.25 | 0.35 |

*Notes*: IAB Establishment Panel, 1994–2009, and CHK plant wage effects provided by Card, Heining, and Kline (2015). The regressand is the CHK plant wage effect. The control variables consist of 32 district dummies (*Regierungsbezirke*), 62 two-digit sector dummies, four plant size dummies, four plant age dummies, a dummy for a single plant (as opposed to a plant belonging to a multi-branch company), and the percentages of women as well as part-time workers in the plant's workforce. Standard errors (in parentheses) are clustered at the plant level. ***/**/* indicates statistical significance at the 1/5/10 percent level.