Essays on firm wage differentials and industrial relations

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Summary

This dissertation is about questions on how German institutions of industrial relations shape plant-level outcomes, and how this influences employer wage differentials. Employer wage differentials point toward imperfect labor markets in which both, employers and employees, benefit from employment rents. It puts the employer at the center of explaining wage differences and how employer characteristics influence these, over which individual employees have only limited control. Arguably, how employers and employees split these rents depend on industrial relations. The German dual model of industrial relations consists of collective bargaining at the industry level and worker co-determination through works councils at the plant level. This dissertation illuminates different aspects of industrial relations and how rent-sharing mechanisms can explain wage inequality in Germany. It does not only focus on how industrial relations shape labor market power and whether labor market power translates into the level and dispersion of employer wage premia. It also contributes to questions that explain differences in plant-level outcomes relating to industrial relations. These include the role of worker co-determination on assortative matching. It is further investigated how works councils affect plant-level reactions during economy-wide shocks. In addition, it offers new causal evidence of rent-sharing mechanisms in Germany. The insights of this dissertation are relevant for policy and economic research alike. It contributes to a better understanding of the role of organized labor in imperfect labor markets and its determinants of employer wage differentials. It approaches the role of worker co-determination from different angles that are important at times of erosion of formal organized labor but gaining interest in worker representation.

The first chapter that is joint work with Boris Hirsch, Sabien Dobbelaere and Steffen Müller examines how collective bargaining through unions and workplace co-determination through works councils shape labor market imperfections and how labor market imperfections matter for employer wage premia. Labor market imperfections are determined by applying a semi-structural, production-based approach to the data, which jointly identifies labor and product market imperfections. In this approach, labor and product market imperfections drive a wedge between the output elasticities of labor and intermediate inputs and their revenue shares that is informative on these imperfections. This wedge, allows to determine whether wages are below or above the marginal revenue product of labor and also to infer the intensity of imperfections in labor and product markets. To obtain a measure of employer wage premia that does not suffer from worker sorting, plant wage effects from a two-way fixed-effects decomposition of log wages are used. In this decomposition, the plant wage effect measures the wage premium enjoyed by all workers in a plant's workforce adjusted for observed and unobserved worker quality.

To apply the semi-structural model the IAB Establishment Panel between 1999 and 2016 is used. This data have all the relevant information to estimate the structural parameters and to map the structural estimates on information on collective bargaining and works councils. It allows further to match the data on employer wage premia that are provided to external researchers. Revenue production functions are estimated using a control function approach. To investigate how organized labor shape market imperfections, probit models are estimated for the extensive margin and Tobit type 2 for the intensive margin. Recentered influence functions (RIF) and OLS regressions estimated to understand the relationship between employer wage premia and the intensive and extensive margins of labor market imperfections.

The results document that labor market imperfections are the norm rather than the exception. Wage mark-downs, that is wages below the marginal revenue product of labor rooted in employers' monopsony power, are the most prevalent outcome. In addition, both types of organized labor are accompanied by a smaller prevalence and intensity of wage mark-downs whereas the opposite holds for wage mark-ups that is wages above the marginal revenue product of labor rooted in workers' monopoly power. Finally, a close link between the production-based labor market imperfection measures and employer wage premia is documented. The prevalence and intensity of wage mark-downs are associated with a smaller level and larger dispersion of premia whereas wage mark-ups are only accompanied by a higher premium level.

The second chapter that is joint work with Steffen Müller focuses on works councils and the roots of the positive association between worker participation in decision-making and high-wage and high-productivity firm strategies. The idea behind is the assumption that high-paying works council plants employ high-quality workers, who have more tenure and higher formal education. If works councils are a driver of positive assortative matching, then high-wage, high-performance plants with works councils would coexist with low-wage, low-performance plants without councils. This would imply estimating spurious productivity and wage gains from co-determination and suggest that the legal mandate for councils contributes to between-plant wage inequality and productivity dispersion across plants. To analyze whether sorting explains the productivity and wage effects of works councils, a summary measure of observable and unobservable general human capital components of workers is used. Specifically, fixed effects from the same wage decomposition as described for chapter 1 are used. In this model, higher worker effects are rewarded higher across all employers. This justifies labeling individuals with high worker effects as high-quality workers. Importantly, those worker effects capture all invariant human capital components and include observable human capital variables like education or initial age but also unobservable components like ability.

The Linked-Employer-Employee Data (LIAB) of the Institute for Employment Research (IAB) is used which links plant-level survey information from the IAB Establishment Panel to administrative worker-level data. The data consist of relevant employee information like the worker fixed effects or information on education and further includes all the relevant plant-level information such as value added per worker. To capture worker quality at the plant level average worker quality is calculated for the respective year. To investigate whether assortative matching imply spurious productivity and wage effects of works council OLS regressions are estimated. To test whether works councils aggravate assortative matching, event studies for the works council introduction are estimated.

The results support the evidence that worker quality is already higher in plants before council introduction and further increases after the introduction. The improvement of worker quality cannot explain any changes in wages once a council is introduced. This result support the notion that time consistent factors (i.e. employer wage premia) explain wage differences between council and non-council plants. Importantly, previous studies are corroborated by showing positive productivity and profitability effects of works councils even after taking into account worker sorting.

The third chapter investigates the role of works councils during economy-wide shocks. Evidence about how works councils affect plant-level outcomes during adverse times is sparse. While cross-sectional studies find that works councils improve efficiency and enhance workers' voice, they may have potential adverse effects during economic adverse times. The improved workers' voice and consent rights potentially lead to beneficial outcomes for workers, but harmful ones for plants. While differences in reactions may be present during the adverse time it may also influence development during economic recovery.

The chapter uses the IAB Establishment Panel between 2006 and 2013 and focus on plants between 20 and 500 employees. Differences in plant-level employment, value added per worker, the wage bill per worker and profits per worker between works council and non-council plants during and after the Great Recession in 2008 and 2009 are investigated. To understand whether works council plants reacted differently a triple difference-in-differences estimation is applied, resting on the assumption that different changes in the outcomes of interest are driven by the presence of a works council. To identify whether a plant was affected by the Great Recession, survey information of the IAB Establishment Panel is used.

The results show, that affected plants with more than 100 employees and a works council experience a smaller reduction in employment and a larger reduction in labor productivity during the crisis years. These results support, what have been suggested in previous studies, that works councils may improve labor productivity, but reduce labor productivity when plants face adverse times. Large affected works council plants do not adjust their labor costs through reduced wages. This supports the notion that these plants shield off their workers from economy wide shocks. Importantly, these plants experience no different growth rates in labor productivity than affected non-council plants during the economic recovery. This suggests that those negative effects on labor productivity are accompanied with long term growth in labor productivity instead of strong recovery phases immediately after the crisis. While the differences in large plants are very pronounced neither differences in smaller plants nor differences in all plants are found.

The fourth chapter that is joint work with Matthias Mertens and Steffen Müller presents causal evidence on the rent-sharing elasticity of German manufacturing firms. The identification of rent-sharing elasticities builds on standard rent-sharing models in which employers and employees bargain over a joint surplus. The general model rests on Nash-Bargaining models that emphasize how changes to the quasi-rent can affect workers' wages if firms share rents. OLS estimates of rent-sharing elasticities are most likely biased, which may for example stem from reversed causality (e.g. effciency wages), simultaneity (e.g. management practices) or omitted variable bias. To deal with endogeneity a firm-level Bartik instrument for the quasi-rent is constructed, that combines the firm-level consumption shares of five different energy carriers with national price shifts of these energy carriers. Different exercises show that the instrument is relevant and strictly exogenous conditional on covariates.

The data for this chapter combine different modules of the administrative AFiD-Panel (Amtliche Firmendaten für Deutschland) on German manufacturing plants. It allows to calculate firm-level average wages, value added and other variables used in the regression analysis. Importantly, it allows constructing firm-level consumption shares of electricity, natural gas, light oil, heavy oil and hard coal. The Bartik instrument is the weighted sum of time shifts of the logarithm of national prices of energy carriers where the weights are the firm-level shares of each of the five energy sources in total energy consumption. Intuitively, the identification of the instrumental variable for the quasi-rent rests on the

assumption that national price shifts of a specific energy carrier affect firms with different exposures of this energy carrier differently. Through this channel wages are affected, as the quasi-rent relates to differences in exposure to energy carriers and rent sharing is identified.

The results show that rising energy prices depress output, but leave the amount and composition of labor inputs unaffected. The instrumental variable estimator yields a rent-sharing elasticity of about 0.20, implying that a 10 percent increase in firms' value added per worker increases wages by about 2 percent. The rent-sharing elasticity is substantially bigger in small firms (0.31) compared to large firms (0.14).

Chapter 1

Organized labor, labor market imperfections, and employer wage premia[1]

1.1 Introduction

It has not been long since most labor economists abandoned the textbook model of perfect competition and embraced the idea that workers and employers possess some market power in the wage formation process. In the broadest sense, imperfect competition in the labor market can be seen as a situation where substantial employment rents accrue to workers and employers (Manning 2011). This vision immediately raises the question of how these rents are split among workers and employers or, in other words, what wage emerges under a bilateral monopoly in which both parties possess some market power.

As Addison *et al.* (2014) put it, the essential question is whether the labor market outcome will be on the labor demand curve with employers paying wages equal to the marginal revenue product of labor as under perfect competition or off this curve due to labor market imperfections. In a recent survey, Booth (2014) approaches this question

[1] This chapter is joint work with Sabien Dobbelaere, Boris Hirsch and Steffen Müller and is published as IZA Discussion Paper (No. 13909, November 2020) under the title ‘Organised Labour, Labour Market Imperfections, and Employer Wage Premia’.

by considering two polar cases of wage formation under imperfect competition: employer wage setting, where employers possess monopsony power, and union wage setting, where workers exercise monopoly power when negotiating wages. Put differently, labor market imperfections may either result in a wage mark-down with employers' monopsony power allowing them to set wages below the marginal revenue product of labor, or in a wage mark-up with workers' monopoly power permitting them to push through wages above the marginal revenue product.

Against this backdrop, our contribution is to investigate for Germany the extent of labor market imperfections, how industrial relations shape employers' and workers' market power in the labor market and how labor market imperfections relate to employer wage premia. Building on a production-based approach that enables us to jointly identify labor and product market imperfections and a large representative sample of about 9,000 plants for the years 1999–2016, this paper is the first to document the prevalence and intensity of both wage mark-downs and wage mark-ups in the labor market as well as mark-up pricing in the product market for Germany. We will see that labor and product market imperfections are the norm rather than the exception in Germany. They should thus figure much more prominently in both science and politics, not the least since the (lack of) competition in the labor market promises important insights into recent labor market trends like the falling labor share in income and rising wage inequality.

Our core result will be that collective bargaining and works councils matter for both the prevalence and the intensity of labor market imperfections. Specifically, we will find that the presence of any of these labor market institutions is associated with a lower probability of a wage mark-down and a higher probability of a wage mark-up. On top of these findings at the extensive margin, we will also see that both forms of organized labor are accompanied with lower monopsony power of employers and higher bargaining power of workers at the intensive margin, that is given an outcome involving either a wage mark-down or a wage mark-up. These results suggest that organized labor benefits workers in shifting market power from employers to workers.

Moreover, we will see that the presence of collective bargaining and works councils is negatively related to the probability of switching from marginal-product wages to a wage mark-down as well as to the switching probability from a wage mark-up to marginal-product wages. These findings lend further credence to the hypothesis that industrial relations shape labor market imperfections and they also suggest that the erosion of organized labor during our period of observation has contributed to shifting market power from workers to employers.

Finally, we will document that employer wage premia, that is wage differences that are left after differences in workers' human capital and unobservable skills have been rewarded, are closely related to labor market imperfections. Holding constant the rents to be split between workers and employers, the mean employer wage premium is lower when a wage mark-down exists and larger when there is a wage mark-up compared to marginal-product wages, and it is also related in the same way to employers' and workers' market power at the intensive margin. Moreover, wage premia are more dispersed when there is a wage mark-down, so that employers' monopsony power not only depresses workers' wage outcomes, but also aggravates inequality. In short, our evidence strongly suggests that organized labor matters for labor market imperfections that, in turn, matter for employer wage premia.

Whereas wage mark-ups and their theoretical foundation in union wage-setting models form the starting point of the broad empirical rent-sharing literature (surveyed by Card *et al.*, 2018, and Dobbelaere and Mairesse, 2018), possible wage mark-downs are at the heart of a recent literature on the prevalence and causes of monopsony in the labor market (for overviews, see Manning, 2011, 2021). Until recently, though, both strands of the literature evolved separately. What is more, they have also largely neglected possible links between labor and product market imperfections that may contaminate findings. The only exception we are aware of is the study by Dobbelaere and Mairesse (2013) that introduces an estimation approach encompassing both types of labor market imperfections while also allowing for product market imperfections.

This approach's origins lie in Hall's (1988) framework that allows estimating price-cost mark-ups under the assumptions of constant returns to scale in production and marginal-product wages and its extensions to non-constant returns by Klette (1999) and imperfect labor markets involving wage mark-ups by Crépon *et al.* (2005). Generalising Crépon *et al.*'s framework to allow for labor market imperfections that yield either a wage mark-down or a wage mark-up, Dobbelaere and Mairesse's (2013) approach uses production function estimates to measure how imperfect labor and product markets are. Intuitively spoken, Dobbelaere and Mairesse show that labor and product market imperfections drive a wedge between the output elasticities of labor and intermediate inputs and their revenue shares that is informative on these imperfections. This wedge, which they term the joint market imperfections parameter, allows not only to determine whether wages are below or above the marginal revenue product of labor, but also to infer the intensity of imperfections in labor and product markets. It thus accounts for a possible interdependency between both types of market imperfections that contaminates estimates of wage mark-downs, wage mark-ups, and price-cost mark-ups (for a discussion in the case of price-cost mark-ups, see De Loecker *et al.* 2016).

In their empirical analysis, Dobbelaere and Mairesse (2013) document substantial labor and product market imperfections for France as do other studies using their approach for Japan and the Netherlands (Dobbelaere *et al.* 2015), for Chile (Dobbelaere *et al.* 2016), and for Portugal (Félix and Portugal 2016). Dobbelaere and Kiyota (2018) further show for Japan that exporters are more likely to operate in imperfect product markets and to share rents with their workers by paying a wage mark-up, but are less likely to set a wage mark-down, whereas the opposite patterns emerge for multinationals.

What is lacking, though, is empirical evidence on how industrial relations, such as collective bargaining through unions and workplace co-determination through works councils, shape market imperfections. In theory, such labor market institutions matter for how employment rents are split between workers and employers. As a case in point, Falch and Strøm (2007) show that wage bargaining between a union and an employer with wage-setting power not only limits the employer's monopsony power, but may also lead to an efficiency gain compared to the solution without the union, that is compared to the wage mark-down under pure monopsonistic wage setting. And, notably, in a recent

Issue Brief, the Council of Economic Advisors (2016) argues that declining unionisation in the US has raised employers' monopsony power, which has led to lower wage growth and increased wage inequality.

By examining how industrial relations shape labor market imperfections, this paper thus not only contributes to the literature that investigates the determinants of workers' and employers' market power in rent splitting, but also adds to the literatures on the falling labor share in income (e.g. Karabarbounis and Neiman 2014) and rising wage inequality (for a survey, see Acemoglu and Autor, 2011, and for the German case, see Dustmann *et al.*, 2009). For example, Card *et al.* (2013) document that increasing dispersion in employer wage premia during the 1990s and 2000s contributed to the rise in wage inequality in West Germany, and Hirsch and Mueller (2020), in turn, observe that the fall in collective bargaining coverage during that period contributed to the rise in the dispersion of employer wage premia. If organized labor matters for the prevalence and the intensity of labor market imperfections in that it shifts market power from employers to workers, then the erosion of industrial relations documented for Germany (e.g. Oberfichtner and Schnabel 2019) as for other countries may be one common source of the trends of a decreasing labor share in income and increasing wage inequality. Therefore, our analysis not only promises insights into the relevance of industrial relations for labor market imperfections, but it is also likely to inform important recent debates among scientists and policy-makers alike.

The remainder of this paper is organized as follows. Section 1.2 gives an overview of the institutional setting in Germany and provides hypotheses for the relationship between industrial relations on labor market imperfections. Section 1.3 introduces the theoretical foundations of our estimation approach, and Section 1.4 gives the details on its econometric implementation. Section 1.5 describes our data. Sections 1.6 and 1.7 present and discuss our results for the link between industrial relations and labor market imperfections and between labor market imperfections and employer wage premia, respectively, and Section 1.8 concludes.

1.2 Institutional backdrop and hypotheses

In Germany, the principle of bargaining autonomy gives 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 the sectoral level, and almost always apply to all of the covered employers' workers irrespectively of workers' union status. Although sectoral negotiations mostly take place in regional bargaining units, officials of the two bargaining parties closely coordinate the regional negotiations within one sector, so that variations between them are small. There even exists some cross-sectoral coordination by both parties, giving rise to some uniformity in collective bargaining policy across sectors (for more details, see 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, so that employers bound by the agreement 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 re-negotiating collective bargaining issues, mostly wages and working time, at the plant level, typically under conditions of economic hardship.

Whereas many employers do in fact pay higher wages 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. At the end of our observational window in 2016, 58% (47%) of workers in West (East) Germany held jobs in the 32% (21%) of plants covered by a collective agreement (Ellguth and Kohaut 2017). Compared to the start of our observation period, we see a marked fall in collective bargaining coverage. In 2000, 70% (55%) of workers in West (East) Germany were employed by the 48% (28%) of covered plants (Kohaut and Schnabel 2003).

On average, plants covered by a collective agreement pay higher wages than uncovered plants (Gürtzgen 2009; Fitzenberger *et al.* 2013). In a recent study, Hirsch and Mueller (2020) further show that higher average wages in covered plants reflect higher employer wage premia, that is higher wages paid to equally productive workers, holding constant the level of rents to be split between workers and employers. They interpret their finding as evidence that collective bargaining increases workers' bargaining power. This interpretation is in line with evidence from the empirical rent-sharing literature and with a host of theoretical contributions arguing that collective bargaining enables workers to push through wage mark-ups (e.g. McDonald and Solow 1981). Hence, we expect a higher prevalence and intensity of wage mark-ups in covered than in uncovered plants. We further suspect the opposite to hold for wage mark-downs, although we lack direct empirical evidence on this received wisdom analyzed by Falch and Strøm (2007), who show theoretically that collective bargaining limits employers' wage-setting power. In this paper, we will put these hypotheses to a rigorous test.

On top of collective bargaining typically conducted at the sectoral level, the second backbone of Germany's dual system of industrial relations is given by workplace co-determination 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 (for details, see Addison 2009). At the end of our observation period in 2016, 43% (34%) of workers in West (East) Germany were employed by the 9% (9%) of plants with a works council (Ellguth and Kohaut 2017). As collective bargaining coverage, plant-level co-determination dropped compared to the start of our observational window. In 2000, 50% (41%) of workers in West (East) Germany held jobs in the 12% (12%) of plants with a works council (Ellguth and Kohaut 2018). Together, shrinking collective bargaining coverage and works council prevalence point at an erosion of the traditional model of industrial relations in Germany.

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 an increasing 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 exempted from work once certain plant size thresholds are reached. Works councils have extensive information, consultation, and co-determination rights (for details, see Addison 2009). In particular and in contrast to continental European counterparts of workplace representation, German works councils have co-determination 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 organisation. Unlike 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 collective agreements between unions and employers’ associations at the sectoral level. One exception to this general rule is that collective agreements contain opening clauses (mentioned before) that explicitly authorise works councils to do so.

However, even if opening clauses are absent, works councils’ extensive information, consultation, and co-determination rights on many other issues mean that works council existence is likely to improve workers’ bargaining power and thus to spur rent-seeking activities (Freeman and Lazear 1995). In line with this conjecture, extant studies have documented that works council presence is accompanied by higher average wages (Addison *et al.* 2001, 2010). Furthermore, Hirsch and Mueller (2020) show that the higher average wages in plants with a works council mirror higher employer wage premia holding constant the level of rents and interpret their finding as evidence that workplace co-determination increases workers’ bargaining power. Although we lack direct empirical evidence on how works council presence shapes labor market imperfections, we follow the received wisdom that it shifts market power from employers to workers and thus expect a lower prevalence and intensity of wage mark-downs when works councils are present and the opposite for wage mark-ups. As with collective bargaining, we will put these hypotheses to a rigorous test.

1.3 Theoretical framework

To determine a plant's market power in its labor and product markets, we follow the approach introduced by Dobbelaere and Mairesse (2013) that allows to infer both types of market imperfections from production function estimates.[2] Consider plant i at time t that produces a good Q_{it} from its labor input N_{it}, its intermediate inputs M_{it}, and its capital input K_{it}, subject to the twice differentiable, strictly increasing (in all its arguments) and concave production function:

$$Q_{it} = Q(N_{it}, M_{it}, K_{it}) \tag{1.1}$$

In terms of the plant's input choices, we assume (i) that labor and intermediate inputs are free of adjustments costs and are thus choice variables in the short run, (ii) that capital is predetermined and thus no choice variable in the short run, and (iii) that the plant takes the price of its intermediate inputs as given.[3] We further assume that all plants in the market maximise short-run profits. Plant i's short-run profits at time t are given by

$$\Pi_{it} = R_{it} - W_{it}N_{it} - J_{it}M_{it}, \tag{1.2}$$

where $R_{it} = P_{it}Q_{it}$ denotes the plant's revenues, P_{it} the price of the good, and W_{it} and J_{it} the input prices of labor and intermediate inputs, respectively. Then, the plant's optimisation problem involves maximising short-run profits (1.2) with respect to output Q_{it}, labor N_{it}, and intermediate inputs M_{it}.

Turning to the plant's product market first, the first-order condition with respect to

[2] In our data, we observe plants rather than firms and will thus refer to plants throughout the paper.
[3] Given recent evidence on imperfections in intermediate inputs markets by Kikkawa *et al.* (2019) and Morlacco (2019), this latter assumption of price taking for intermediate inputs might be perceived as being restrictive. This evidence notwithstanding, we stick to the assumption for two reasons. The first is a data reason. Like Morlacco (2019), we could easily model imperfections in intermediate inputs markets as an additional unit cost that drives a wedge between the marginal cost of production and the marginal products of plants' inputs. Data constraints, however, prevent us from putting this approach to work. The second reason is that we want to focus our empirical analysis on the relationship between industrial relations and labor and product market imperfections faced by plants, abstaining from non-competitive buyer behaviour in the market for intermediate inputs.

Q_{it} yields the plant's price-cost mark-up:

$$\mu_{it} = \frac{P_{it}}{(C_Q)_{it}} = \left(1 + \frac{s_{it}\kappa_{it}}{\eta_t}\right)^{-1} \tag{1.3}$$

In equation 1.3, $(C_Q)_{it} = \partial C_{it}/\partial Q_{it}$ denotes the marginal cost of production, C_{it} the plant's cost function, $s_{it} = Q_{it}/Q_t$ the market share of plant i in industry demand Q_t, $\eta_t = (\partial Q_t/\partial P_t)(P_t/Q_t)$ the own-price elasticity of industry demand, and $\kappa_{it} = \partial Q_t/\partial Q_{it}$ a conjectural variations parameter that captures competitors' quantity response to plant i's output choice.[4]

Turning to plant i's choice of intermediate inputs next, the first-order condition with respect to M_{it} yields $(Q_M)_{it} = \mu_{it}J_{it}/P_{it}$, where $(Q_M)_{it} = \partial Q_{it}/\partial M_{it}$ denotes the marginal product of intermediate inputs. Multiplying this expression by M_{it}/Q_{it} yields

$$(\varepsilon_M^Q)_{it} = \mu_{it}\alpha_{Mit} \tag{1.4}$$

with the output elasticity of intermediate inputs $(\varepsilon_M^Q)_{it} = (\partial Q_{it}/\partial M_{it})(M_{it}/Q_{it})$ and their revenue share $\alpha_{Mit} = J_{it}M_{it}/R_{it}$. Hence, in the optimum the output elasticity of intermediate inputs equals the share of their expenditures in output evaluated at the marginal cost of production. In what follows, we will use equation (1.4) to identify the price-cost mark-up as

$$\mu_{it} = \frac{(\varepsilon_M^Q)_{it}}{\alpha_{Mit}} \tag{1.5}$$

from the plant's production technology that provides us with the output elasticity $(\varepsilon_M^Q)_{it}$ and its intermediate input choice that provides us with the revenue share α_{Mit}.

Unlike the price of intermediate inputs that the plant takes as given, wage formation

[4] Specifically, under Cournot competition with plants producing a homogenous good and competing in quantities, $\kappa_{it} = \partial Q_t/\partial Q_{it} = 1$ with a single industry-wide output price in equilibrium $P_{it} = P_t$. Hence, in this case the price-cost mark-up is $\mu_{it} = P_t/(C_Q)_{it} = (1 + s_{it}/\eta_t)^{-1}$. Under Betrand competition with plants producing a horizontally differentiated good and competing in prices instead of quantities, $\partial P_t/\partial P_{it} = 1$ and thus $\kappa_{it} = \partial Q_t/\partial Q_{it} = \eta_t/(s_{it}\eta_{it})$ with $\eta_{it} = (\partial Q_{it}/\partial P_{it})(P_{it}/Q_{it})$ denoting plant i's own-price elasticity of residual demand. Hence, in this case the price-cost mark-up is $\mu_{it} = P_{it}/(C_Q)_{it} = (1 + s_{it}/\eta_{it})^{-1}$.

depends on possible labor market imperfections as does the plant's optimal labor demand. If the plant takes the wage as given too, the first-order condition with respect to N_{it} is analogous to intermediate inputs $(Q_N)_{it} = \mu_{it} W_{it}/P_{it}$, where $(Q_N)_{it} = \partial Q_{it}/\partial N_{it}$ denotes the marginal product of labor. In other words, we arrive at a solution on the labor demand curve, which nests both perfect competition and right-to-manage bargaining (Nickell and Andrews 1983), in which the plant and a union bargain over wages and the plant is then free to choose the employment level at this bargained wage. Multiplying $(Q_N)_{it} = \mu_{it} W_{it}/P_{it}$ by N_{it}/Q_{it} yields

$$(\varepsilon_N^Q)_{it} = \mu_{it} \alpha_{Nit} \tag{1.6}$$

with the output elasticity of labor $(\varepsilon_N^Q)_{it} = (\partial Q_{it}/\partial N_{it})(N_{it}/Q_{it})$ and its revenue share $\alpha_{Nit} = W_{it} N_{it}/R_{it}$. As with intermediate inputs, this condition means that in the optimum the output elasticity of labor equals the share of the plant's payroll in its output evaluated at the marginal cost of production.

Absent labor market imperfections, comparing equations (1.4) and (1.6) shows that there exists no wedge

$$\psi_{it} = \frac{(\varepsilon_M^Q)_{it}}{\alpha_{Mit}} - \frac{(\varepsilon_N^Q)_{it}}{\alpha_{Nit}} = 0 \tag{1.7}$$

between the output elasticities of intermediate inputs and labor and their respective revenue shares. Hence, $\psi_{it} = 0$ indicates the absence of labor market imperfections, that is a labor market setting in which workers obtain the marginal product of labor and thus an outcome on the labor demand curve. We will denote ψ_{it} as the plant's joint market imperfections parameter in the following, the reason of which will become clear shortly.

Things look different when labor market imperfections are present as these give rise to an outcome off the labor demand curve.[5] Imperfections may either stem from plants'

[5] Strictly speaking, labor market imperfections give rise to a solution off the marginal revenue product curve that coincides with the plant's labor demand curve under marginal-product wages (but, for instance, not under labor market monopsony where the wage-employment outcome will not lie on the marginal revenue product curve). Yet, for the sake of intuition and readability, we will refer to

monopsony power that enables them to set a wage mark-down or from workers' monopoly power that allows them to impose a wage mark-up on plants.

We first consider a solution below the labor demand curve. In this case, plants' wage-setting power may originate from concentration or collusion, but may also be pervasive in labor markets with many competing employers due to search frictions, mobility costs, or job differentiation (Manning 2011, 2021). All these possible channels impede workers' responsiveness to wages, so that the labor supply curve faced by a single employer is upward-sloping rather than horizontal as it would be under perfect competition. Let the labor supply faced by the plant paying wage W_{it} be $N_{it}(W_{it})$ and its inverse $W_{it}(N_{it})$. Plugging the latter into the plant's profits (1.2) and maximising these with respect to N_{it} yields the first-order condition

$$(R_N)_{it} = (W_N)_{it}N_{it} + W_{it}(N_{it}), \tag{1.8}$$

where $(R_N)_{it} = \partial R_{it}/\partial N_{it}$ denotes the marginal revenue product of labor and $(W_N)_{it} = \partial W_{it}/\partial N_{it}$ the slope of the labor supply curve to the plant.

Rewriting equation (1.8) gives

$$W_{it} = \beta_{it}(R_N)_{it}, \tag{1.9}$$

where $\beta_{it} = W_{it}/(R_N)_{it} = (\varepsilon_W^N)_{it}/[(\varepsilon_W^N)_{it} + 1] \leqslant 1$ denotes the wage mark-down and $(\varepsilon_W^N)_{it} = (\partial N_{it}/\partial W_{it})(W_{it}/N_{it})$ the wage elasticity of plant-level labor supply. The latter informs us on how wage-driven workers are and thus on the plant's monopsony power. Under perfect competition, the plant-level labor supply curve is horizontal with $(\varepsilon_W^N)_{it} = \infty$ and workers obtain the marginal revenue product of labor, i.e. $\beta_{it} = 1$. Under monopsony, workers respond imperfectly to wages, which provides the plant with wage-setting power that is inversely related to the elasticity of labor supply $(\varepsilon_W^N)_{it}$. Rewriting

the marginal revenue product curve as the labor demand curve throughout.

equation (1.9) using $(R_N)_{it} = P_{it}(Q_N)_{it}/\mu_{it}$, we arrive at:

$$(\varepsilon^Q_N)_{it} = \mu_{it}\alpha_{Nit}\left[1 + \frac{1}{(\varepsilon^N_W)_{it}}\right] \tag{1.10}$$

Combining equations (1.10) and (1.5) yields the joint market imperfections parameter

$$\psi_{it} = \frac{(\varepsilon^Q_M)_{it}}{\alpha_{Mit}} - \frac{(\varepsilon^Q_N)_{it}}{\alpha_{Nit}} = -\frac{\mu_{it}}{(\varepsilon^N_W)_{it}} < 0, \tag{1.11}$$

which now has a negative sign. In words, the plant's wage-setting power allows it to set a mark-down on wages that, in turn, drives a negative wedge between the output elasticities of intermediate inputs and labor and their respective revenue shares. A negative ψ_{it} thus signifies a labor market setting favouring plants that impose a wage mark-down on workers. Based on ψ_{it}, we can further recover the plant-level labor supply elasticity $(\varepsilon^N_W)_{it}$ and the wage mark-down β_{it} as structural parameters that inform us on the intensity of labor market imperfections. Finally, the more negative ψ_{it} gets, the more pronounced are the combined labor and product market imperfections, which is the reason why we refer to ψ_{it} as the joint market imperfections parameter.

That said, labor market imperfections may also originate from workers' monopoly power enabling them to impose a wage mark-up on plants, thereby yielding an outcome above the labor demand curve. As an underlying structural model, we will consider efficient bargaining (McDonald and Solow 1981) between a risk-neutral plant and its risk-neutral workforce, though other theoretical structural models are possible as well. For instance, Stole and Zwiebel (1996) show that a wage mark-up may also arise from wage bargaining between individual workers and their employer when incomplete labor contracts provide incumbent workers with some hold-up power. What is crucial, though, is that the plant is no longer able to unilaterally set employment once the wage has been determined and thus cannot achieve a solution on the labor demand curve.

Under efficient bargaining, the negotiated wage-employment pair maximises both parties' joint surplus and follows from maximising the generalised Nash product

$$\Omega = [N_{it}(W_{it} - \overline{W}_{it})]^{\phi_{it}}[R_{it} - W_{it}N_{it} - J_{it}M_{it}]^{1-\phi_{it}} \tag{1.12}$$

with respect to the wage and employment, where $\overline{W}_{it}$ denotes workers' alternative wage and $0 < \phi_{it} < 1$ the part of the surplus accruing to workers. In other words, ϕ_{it} measures workers' bargaining power. In the generalised Nash product (1.12), workers' surplus is the amount by which their payroll exceeds their outside option while the plant's surplus is its short-run profits.[6]

In the (interior) optimum, the first-order condition with respect to W_{it} yields

$$W_{it} = \overline{W}_{it} + \gamma_{it}\left[\frac{R_{it} - W_{it}N_{it} - J_{it}M_{it}}{N_{it}}\right] \tag{1.13}$$

with workers' relative bargaining power $\gamma_{it} = \phi_{it}/(1 - \phi_{it}) > 0$. The first-order condition with respect to N_{it} gives:

$$W_{it} = (R_N)_{it} + \phi_{it}\left[\frac{R_{it} - (R_N)_{it}N_{it} - J_{it}M_{it}}{N_{it}}\right] \tag{1.14}$$

Combining the first-order conditions (1.13) and (1.14) yields the so-called contract curve

$$(R_N)_{it} = \overline{W}_{it} \tag{1.15}$$

that characterises efficient wage-employment pairs. In equilibrium, the price-cost mark-up satisfies $\mu_{it} = P_{it}/(C_Q)_{it} = P_{it}/(R_Q)_{it}$ with the marginal revenue $(R_Q)_{it} = \partial R_{it}/\partial Q_{it}$. Plugging equation (1.15) into equation (1.13), we thus arrive at:

$$(\varepsilon_N^Q)_{it} = \mu_{it}\alpha_{Nit} - \mu_{it}\gamma_{it}(1 - \alpha_{Nit} - \alpha_{Mit}) \tag{1.16}$$

[6] This formulation of efficient bargaining assumes that all employed union members immediately return to the external labor market when negotiations fail. Yet, results do not change when considering a sequence of bargaining sessions between the plant and a union of declining size whose members gradually lose jobs when disagreement continues (Dobbelaere and Luttens 2016).

Combining equations (1.16) and (1.5) gives the joint market imperfections parameter

$$\psi_{it} = \frac{(\varepsilon_M^Q)_{it}}{\alpha_{Mit}} - \frac{(\varepsilon_N^Q)_{it}}{\alpha_{Nit}} = \mu_{it}\gamma_{it}\left[\frac{1-\alpha_{Nit}-\alpha_{Mit}}{\alpha_{Nit}}\right] > 0, \tag{1.17}$$

which now has a positive sign. In words, workers' monopoly power allows them to capture part of the surplus by imposing a wage mark-up on the plant that, in turn, drives a positive wedge between the output elasticities of intermediate inputs and labor and their respective revenue shares. A positive ψ_{it} thus indicates a labor market setting favouring workers who achieve a wage mark-up. Based on ψ_{it}, we can further recover workers' absolute (relative) bargaining power ϕ_{it} (γ_{it}) as a structural parameter that informs us on the intensity of labor market imperfections and on the magnitude of the resulting wage mark-up. Lastly, the more positive ψ_{it} gets, the more pronounced are the combined labor and product market imperfections.

In summary, the outlined theoretical framework allows us to determine the plant's labor market and product market setting from its production technology providing us with the output elasticities of intermediate inputs $(\varepsilon_M^Q)_{it}$ and labor $(\varepsilon_N^Q)_{it}$ and its input choices providing us with the revenue shares of intermediate inputs α_{Mit} and labor α_{Nit}. Equation (1.5) permits us to determine the price-cost mark-up and thus the product market setting as either involving marginal-cost pricing (PMC) or mark-up pricing (PMU):

$$\mu_{it} = \frac{(\varepsilon_M^Q)_{it}}{\alpha_{Mit}} \begin{cases} = 1 & \text{if } PMS_{it} = PMC \\ > 1 & \text{if } PMS_{it} = PMU \end{cases} \tag{1.18}$$

On top of this extensive margin, the size of the price-cost mark-up allows us to directly infer the magnitude of product market imperfections at the intensive margin.

The sign of the wedge between the output elasticities of intermediate inputs and labor and their respective revenue shares allows us to determine the labor market setting as either one without imperfections involving marginal-product wages (WMP), or as one with imperfections that result either in a wage mark-down (WMD) or in a wage mark-up

(WMU):

$$\psi_{it} = \frac{(\varepsilon_M^Q)_{it}}{\alpha_{Mit}} - \frac{(\varepsilon_N^Q)_{it}}{\alpha_{Nit}} \begin{cases} = 0 & \text{if } LMS_{it} = WMP \\ = -\dfrac{\mu_{it}}{(\varepsilon_W^N)_{it}} < 0 & \text{if } LMS_{it} = WMD \\ = \mu_{it}\gamma_{it}\left[\dfrac{1-\alpha_{Nit}-\alpha_{Mit}}{\alpha_{Nit}}\right] > 0 & \text{if } LMS_{it} = WMU \end{cases} \tag{1.19}$$

On top of this extensive margin, equation (1.19) permits us to recover the magnitude of labor market imperfections at the intensive margin, that is the structural parameters of the labor market for a given labor market setting $LMS_{it} \in \{WMP, WMD, WMU\}$. For $LMS_{it} = WMD$ or $\psi_{it} < 0$ we can recover the plant-level labor supply elasticity $(\varepsilon_W^N)_{it}$ and the wage mark-down β_{it} and for $LMS_{it} = WMU$ or $\psi_{it} > 0$ workers' (relative) bargaining power ϕ_{it} (γ_{it}), which informs us on the size of the wage mark-up.

1.4 Econometric implementation

To determine labor and product market imperfections based on the price-cost mark-up (1.18) and the joint market imperfections parameter (1.19), we have to estimate the output elasticities of intermediate inputs $(\varepsilon_M^Q)_{it}$ and labor $(\varepsilon_N^Q)_{it}$ as well as their revenue shares α_{Mit} and α_{Nit}. Our econometric implementation is based on a production function

$$q_{it} = f(n_{it}, m_{it}, k_{it}; \boldsymbol{\beta}) + \omega_{it} \tag{1.20}$$

with lower-case letters denoting logs of variables, e.g. $q_{it} = \ln Q_{it}$, a vector of common (within two-digit sectors) technology parameters $\boldsymbol{\beta}$, and a Hicks-neutral productivity shock ω_{it} observed by the plant, but unobserved by us. Identifying $\boldsymbol{\beta}$ crucially depends on controlling for the productivity shocks ω_{it} because these will be correlated with the plant's input choices and ignoring them could thus induce omitted variable bias. To control for them, we follow the estimation approach by Ackerberg *et al.* (2015) that builds on the insight that plants' optimal input choices hold information about unobserved productivity.[7] We provide the details in Appendix 1.A .

In our empirical specification, we approximate the unknown regression function $f(\cdot)$ by means of a second-order Taylor polynomial and estimate the coefficients of a translog production function at the two-digit sector level (including a full set of region dummies and a linear time trend, which we will omit in the following for notational ease). Specifically, we estimate

$$\begin{aligned} y_{it} = \beta_0 &+ \beta_n n_{it} + \beta_m m_{it} + \beta_k k_{it} + \beta_{nn} n_{it}^2 + \beta_{mm} m_{it}^2 + \beta_{kk} k_{it}^2 \\ &+ \beta_{nm} n_{it} m_{it} + \beta_{nk} n_{it} k_{it} + \beta_{mk} m_{it} k_{it} + \omega_{it} + \epsilon_{it}, \end{aligned} \tag{1.21}$$

where the regression constant β_0 measures the mean efficiency level across plants and ϵ_{it} is an idiosyncratic error term that comprises unpredictable output shocks and potential measurement error in output and inputs and is assumed to be mean independent of current and past input choices.

We arrive at estimates of the output elasticities $(\varepsilon_M^Q)_{it}$ and $(\varepsilon_N^Q)_{it}$ by combining the estimated $\widehat{\boldsymbol{\beta}}$ with data on plants' input choices:

$$(\widehat{\varepsilon}_N^Q)_{it} = \widehat{\beta}_n + 2\widehat{\beta}_{nn} n_{it} + \widehat{\beta}_{nm} m_{it} + \widehat{\beta}_{nk} k_{it} \tag{1.22}$$

$$(\widehat{\varepsilon}_M^Q)_{it} = \widehat{\beta}_m + 2\widehat{\beta}_{mm} m_{it} + \widehat{\beta}_{mn} n_{it} + \widehat{\beta}_{mk} k_{it} \tag{1.23}$$

Hence, both output elasticities vary across plants and over time.[8] Since the observed output $Y_{it} = Q_{it} \exp \epsilon_{it}$ includes idiosyncratic factors that are orthogonal to input use and productivity, we cannot take revenue shares from our data without correcting for these factors. We do so by recovering an estimate of ϵ_{it} from the production function estimation

[7] Note that some recent papers have shown that factor adjustment costs and non-neutral productivity shocks could also drive a wedge between the output elasticities of labor and intermediate inputs and their respective revenue shares (e.g. Doraszelski and Jaumandreu 2018; Bond *et al.* 2021; Raval 2020). However, these papers ignore labor market imperfections and assume competitive labor markets instead. To the best of our knowledge, there exists no comprehensive approach that would allow us to incorporate their insights into our investigation of labor market imperfections.

[8] Note that with a Cobb-Douglas production technology, output elasticities would simplify to $(\widehat{\varepsilon}_N^Q)_{it} = \widehat{\beta}_n$ and $(\widehat{\varepsilon}_M^Q)_{it} = \widehat{\beta}_m$ and would thus neither vary across plants (within two-digit industries) nor over time.

and calculate adjusted revenue shares as:

$$\widehat{\alpha}_{Nit} = \frac{W_{it}N_{it}}{P_{it}Y_{it}/\exp\widehat{\epsilon}_{it}} \tag{1.24}$$

$$\widehat{\alpha}_{Mit} = \frac{J_{it}M_{it}}{P_{it}Y_{it}/\exp\widehat{\epsilon}_{it}} \tag{1.25}$$

Combining the estimated output elasticities (1.22) and (1.23) and the adjusted revenue shares (1.24) and (1.25), we arrive at estimates of the price-cost mark-up and the joint market imperfections parameter:

$$\widehat{\mu}_{it} = \frac{(\widehat{\varepsilon}^Q_M)_{it}}{\widehat{\alpha}_{Mit}} \tag{1.26}$$

$$\widehat{\psi}_{it} = \frac{(\widehat{\varepsilon}^Q_M)_{it}}{\widehat{\alpha}_{Mit}} - \frac{(\widehat{\varepsilon}^Q_N)_{it}}{\widehat{\alpha}_{Nit}} \tag{1.27}$$

Based on $\widehat{\mu}_{it}$, we then use equation (1.18) to classify plant i's product market setting at time t as either marginal-cost pricing ($\mu_{it} = 1$) or mark-up pricing ($\mu_{it} > 1$), that is $PMS_{it} \in \{PMC, PMU\}$. Based on $\widehat{\psi}_{it}$, we further use equation (1.19) to characterise its labor market setting as either involving marginal-product wages ($\psi_{it} = 0$) or a wage mark-down ($\psi_{it} < 0$) or a wage mark-up ($\psi_{it} > 0$), that is $LMS_{it} \in \{WMP, WMD, WMU\}$.

We account for the estimation uncertainty in $\widehat{\mu}_{it}$ and $\widehat{\psi}_{it}$ by basing our classification on the two-sided 95% confidence intervals (CI) for μ_{it} and $gap_{Nit} = \mu_{it} - \psi_{it} = (\varepsilon^Q_N)_{it}/\alpha_{Nit}$

$$[A_{\widehat{\mu}_{it}}, B_{\widehat{\mu}_{it}}] = [\widehat{\mu}_{it} - 1.96 \times \widehat{\sigma}_{\widehat{\mu}_{it}}, \widehat{\mu}_{it} + 1.96 \times \widehat{\sigma}_{\widehat{\mu}_{it}}] \tag{1.28}$$

$$[A_{\widehat{gap}_{Nit}}, B_{\widehat{gap}_{Nit}}] = [\widehat{gap}_{Nit} - 1.96 \times \widehat{\sigma}_{\widehat{gap}_{Nit}}, \widehat{gap}_{Nit} + 1.96 \times \widehat{\sigma}_{\widehat{gap}_{Nit}}] \tag{1.29}$$

with $\widehat{\sigma}_{\widehat{\mu}_{it}}$ and $\widehat{\sigma}_{\widehat{gap}_{Nit}}$ denoting the respective standard errors computed using the Delta method (e.g. Wooldridge 2010, p. 47). Specifically, to pin down plant i's product market setting in t, we consider the lower bound $A_{\widehat{\mu}_{it}}$ of the CI for μ_{it} and classify the plant's product market setting as marginal-cost pricing ($PMS_{it} = PMC$) if $A_{\widehat{\mu}_{it}} \leqslant 1$ and as mark-up pricing ($PMS_{it} = PMU$) if $A_{\widehat{\mu}_{it}} > 1$. To classify the plant's labor market setting, we check for an overlap of the CIs for μ_{it} and gap_{Nit} that informs us on whether the difference of these two, i.e. ψ_{it}, is statistically significant. If the CIs overlap, we conclude that $\psi_{it} = 0$ and classify the labor market setting as involving marginal-product wages ($LMS_{it} = WMP$). If they do not overlap and $A_{\widehat{gap}_{Nit}} > B_{\widehat{\mu}_{it}}$, we conclude that $\psi_{it} < 0$ with a wage mark-down ($LMS_{it} = WMD$), whereas if they do not overlap and $A_{\widehat{\mu}_{it}} > B_{\widehat{gap}_{Nit}}$, we conclude that $\psi_{it} > 0$ with a wage mark-up ($LMS_{it} = WMU$).

The classification of plants' labor and product market settings allows us to make statements about market imperfections at the extensive margin. Yet, statements about the intensity of imperfections are of no less interest. In the case of product market imperfections, $\widehat{\mu}_{it}$ at once allows assessing their intensity. Turning to labor market imperfections, we can assess the size of wage mark-downs and wage mark-ups using the structural parameters of the respective labor market setting. For $LMS_{it} = WMD$, we can recover the plant-level labor supply elasticity $(\varepsilon_W^N)_{it}$ and the wage mark-down β_{it} using equation (1.19) together with the estimates (1.22)–(1.27) as:

$$(\widehat{\varepsilon}_W^N)_{it} = -\frac{\widehat{\mu}_{it}}{\widehat{\psi}_{it}} \tag{1.30}$$

$$\widehat{\beta}_{it} = \frac{(\widehat{\varepsilon}_W^N)_{it}}{(\widehat{\varepsilon}_W^N)_{it} + 1} \tag{1.31}$$

For $LMS_{it} = WMU$, we can recover workers' absolute (relative) bargaining power ϕ_{it} (γ_{it}) using equation (1.19) together with the estimates (1.22)–(1.27) as:

$$\widehat{\gamma}_{it} = \frac{\widehat{\psi}_{it}}{\widehat{\mu}_{it}} \left[\frac{\widehat{\alpha}_{Nit}}{1 - \widehat{\alpha}_{Nit} - \widehat{\alpha}_{Mit}}\right] \tag{1.32}$$

$$\widehat{\phi}_{it} = \frac{\widehat{\gamma}_{it}}{1 + \widehat{\gamma}_{it}} \tag{1.33}$$

These, in turn, inform us on the size of the wage mark-up.

1.5 Data

Our data come from the IAB Establishment Panel described by Ellguth *et al.* (2014). Starting in 1993 (1996), the IAB Establishment Panel has surveyed West (East) German plants (not firms) that employ at least one worker covered by the social security system on 30th June of the survey year, and is representative of the population of these plants. Crucial for our purpose, it contains information on plants' revenues and intermediate inputs, employment, wage bill, export status, and industrial relations (i.e. collective bargaining coverage and works council existence). To arrive at plants' total labor costs, we use information from the Federal Statistical Office on the non-wage labor costs at the two-digit sector level and add it to the wage bill. We further deflate all nominal values using two-digit price deflators and apply the procedure by Eberle *et al.* (2011) to construct a time-consistent sector classification. Although the IAB Establishment Panel has no direct information on plants' capital stock, it can readily be computed from the included investment data using a modified perpetual inventory approach put forward by Mueller (2008). Since our estimation approach uses lagged information on plants and since the survey information for plants' revenues and intermediate inputs is for the previous year, plants only enter the sample if we observe them in at least three consecutive years. Using information from the survey waves for 1998–2017, we are thus able to build a panel for the years 1999–2016.[9]

In our analysis, we focus on the manufacturing and service sectors and discard the financial and insurance sectors, for which output measures are not comparable to the other sectors in our sample. We further exclude plants producing tobacco products (i.e. 89 plant-year observations belonging to this highly regulated industry) and disregard plants with less than five workers, which are not at risk of having a works council. Before estimating production functions for each two-digit sector, we drop observations with revenue shares of labor and intermediate inputs outside the unit interval and, to remove outliers, only keep observations within the sector-specific 1% trimmed range of value added per worker and capital intensity. Our final regression sample comprises 40,856 observations of 9,061 plants belonging to 38 two-digit sectors (for descriptive statistics, see Table 1.1; the included sectors are visible from Table 1.2).[10]

[9] We cannot use earlier waves because of a change in the questionnaire regarding plants' industrial relations and because we do not want to constrain our analysis to West Germany.

[10] Note that we drop the small number of observations with a negative estimate of the price-cost mark-

Table 1.1: Descriptive statistics

	Mean	SD	p25	p50	p75
Real plant output growth rate (Δq_{it})	0.001	0.228	–0.087	0.000	0.092
Labor growth rate (Δn_{it})	0.013	0.154	–0.029	0.000	0.072
Intermediate inputs growth rate (Δm_{it})	0.002	0.424	–0.172	0.000	0.171
Capital growth rate (Δk_{it})	0.006	0.128	–0.054	–0.028	0.027
Revenue share of intermediate inputs (α_{Mit})	0.471	0.197	0.322	0.474	0.620
Revenue share of labor (α_{Nit})	0.281	0.180	0.142	0.249	0.380
$1 - \alpha_{Nit} - \alpha_{Mit}$	0.206	0.214	0.064	0.188	0.347
ln(wagebill$_{it}$)	5.716	1.224	4.864	5.557	6.410
ln(employment$_{it}$)	2.618	0.905	1.946	2.398	3.045
ln(capital$_{it}$)	13.093	1.533	12.113	12.999	13.968
ln(material$_{it}$)	13.264	1.604	12.158	13.122	14.272
ln(output$_{it}$)	14.093	1.330	13.122	13.868	14.896
Capital intensity ($\ln(\frac{K}{N})_{it}$)	10.457	1.133	9.756	10.515	11.201
Value added per worker ($\ln(\frac{Q-M}{N})_{it}$)	10.609	0.819	10.156	10.617	11.077
Solow residual (SR_{it})	–0.026	0.202	–0.094	–0.005	0.067
Works council (dummy)	0.093	0.290	0.000	0.000	0.000
Collective bargaining (dummy)	0.364	0.481	0.000	0.000	1.000
Single-plant company (dummy)	0.852	0.355	1.000	1.000	1.000
Plant age $\leqslant$ 4 years (dummy)	0.051	0.221	0.000	0.000	0.000
Plant age 5–9 years (dummy)	0.121	0.327	0.000	0.000	0.000
Plant age 10–14 years (dummy)	0.102	0.302	0.000	0.000	0.000
Plant age 15–19 years (dummy)	0.075	0.264	0.000	0.000	0.000
Plant age $\geqslant$ 20 years (dummy)	0.650	0.477	0.000	1.000	1.000
Share of skilled workers	0.647	0.249	0.500	0.714	0.833
Share of apprentices	0.048	0.077	0.000	0.000	0.083
Share of part-time workers	0.265	0.249	0.067	0.188	0.400
Share of female workers	0.423	0.288	0.167	0.357	0.667
Exporting activity (dummy)	0.239	0.426	0.000	0.000	0.000
West Germany (dummy)	0.791	0.407	1.000	1.000	1.000
Observations			40,856		
Plants			9,061		

Notes: IAB Establishment Panel, 1999–2016, weighted using sample weights. The Solow residual is defined as $SR_{it} = \Delta q_{it} - \alpha_{Nit}\Delta n_{it} - \alpha_{Mit}\Delta m_{it} - (1 - \alpha_{Nit} - \alpha_{Mit})\Delta k_{it}$.

up (236 plant-year observations) and an estimated parameter of workers' absolute bargaining power outside the unit interval (1,145 plant-year observations). Note also that including these observations would not change any of our conclusions.

1.6 Do industrial relations matter for labor market imperfections?

1.6.1 Descriptive analysis

Using our panel of German plants for 1999–2016, we now apply the estimation approach described in detail in Section 1.4. In a first step, we estimate translog production functions for each two-digit sector based on the control function approach by Ackerberg *et al.* (2015) that allows us to control for unobserved productivity shocks. In a second step, we use the estimated coefficients together with information on plants' input use to infer their labor and product market settings and to quantify the intensity of market imperfections in both markets.

Table 1.2 presents means (overall and by two-digit sectors) of the estimated output elasticities of labor, intermediate inputs, and capital as well as the resulting returns to scale, i.e. the sum of the three output elasticities. For our whole sample, average output elasticities are 0.44 for labor, 0.55 for intermediate inputs, and 0.10 for capital, with returns to scale amounting to 1.10 and thus slightly above constant returns. We also see marked differences in production technologies across sectors.

We now use plants' estimated output elasticities and revenue shares to infer their joint market imperfections parameter and price-cost mark-up that allow us to pin down plants' time-varying labor and product market settings. Throughout, our descriptive evidence will come from population weighted samples, thereby allowing us to draw conclusions on the population of manufacturing and service plants in Germany.[11]

As is clear from Table 1.3, which summarizes our classification, the majority of (plant-year) observations involve an imperfect labor market. Just 36% of observations are classified as free from labor market imperfections involving marginal-product wages, whereas for 49% of observations we find a wage mark-down at the detriment of workers and for another 15% a wage mark-up at the detriment of plants. Market imperfections are

[11] We also repeated our descriptive analysis weighting plants with their number of workers, which did not change any of our insights.

Table 1.2: Estimated output elasticities and returns to scale by two-digit sector (means)

Sector (NACE Rev.2)	$(\widehat{\varepsilon}_N^Q)_{it}$	$(\widehat{\varepsilon}_M^Q)_{it}$	$(\widehat{\varepsilon}_K^Q)_{it}$	RTS	Obs.	Plants
Food products (10)	0.461	0.498	0.118	1.077	1,863	444
Beverages (11)	0.386	0.601	0.192	1.180	266	45
Textiles (13)	0.060	0.585	0.268	0.913	521	112
Wearing apparel, leather (14–15)	0.297	0.827	0.084	1.207	203	48
Wood and wood products (16)	0.287	0.713	0.076	1.076	888	181
Paper and paper products (17)	0.385	0.571	0.013	0.969	372	75
Printing and recorded media (18)	0.490	0.265	0.274	1.028	664	131
Chemicals and petroleum products (19–20)	0.241	0.689	0.086	1.016	1,190	236
Basic pharmaceutical products (21)	0.399	0.666	0.059	1.124	151	35
Rubber and plastic products (22)	0.267	0.708	0.049	1.024	1,363	271
Non-metallic mineral products (23)	0.391	0.579	0.106	1.076	1,402	279
Basic metals (24)	0.525	0.469	0.060	1.054	1,419	270
Fabricated metal products (25)	0.529	0.482	0.085	1.096	3,479	669
Computer and electronic products (26)	0.561	0.645	0.169	1.375	1,048	250
Electrical equipment (27)	0.317	0.573	0.106	0.996	1,057	219
Machinery and equipment (28)	0.350	0.553	0.043	0.946	3,108	636
Motor vehicles and trailers (29)	0.412	0.625	0.037	1.073	1,196	259
Other transport equipment (30)	0.266	0.681	0.069	1.016	281	78
Furniture (31)	0.519	0.504	0.025	1.048	658	130
Other manufacturing (32)	0.580	0.471	0.062	1.113	1,049	211
Repair, installation of machinery (33)	0.414	0.564	0.091	1.069	617	149
Wholesale trade (w/ vehicles) (45)	0.231	0.636	0.129	0.996	1,996	433
Wholesale trade (w/o vehicles) (46)	0.343	0.757	0.031	1.131	3,097	672
Retail trade (w/o vehicles) (47)	0.382	0.672	0.026	1.079	4,064	923
Transport and warehousing (49–53)	0.377	0.620	0.194	1.191	2,329	586
Publishing activities (58–63)	0.402	0.413	0.201	1.016	1,037	291
Legal and accounting activities (69)	0.833	0.260	0.099	1.191	1,283	284
Consultancy activities (70)	0.492	0.569	0.203	1.264	311	89
Engineering activities (71)	0.570	0.293	0.345	1.208	1,191	285
Scientific research (72)	0.505	0.442	0.104	1.051	401	101
Advertising, market research (73)	0.424	0.533	–0.049	0.908	219	58
Other professional activities (74–75)	0.622	0.381	0.155	1.158	188	43
Rental and leasing activities (77)	0.271	0.653	0.021	0.945	102	28
Employment activities (78)	0.750	0.184	0.239	1.173	450	164
Travel agencies (79)	0.370	0.599	0.112	1.081	140	39
Security activities (80)	1.021	0.372	–0.156	1.237	105	32
Services to buildings and landscape (81)	0.570	0.443	0.147	1.160	850	230
Office administration and support (82)	0.087	0.678	0.023	0.787	298	75
All	0.441	0.553	0.104	1.097	40,856	9,061

Notes: IAB Establishment Panel, 1999–2016, weighted using sample weights.

Table 1.3: Plants' labor and product market settings

Labor market setting	Product market setting		$\sum$
	Marginal-cost	Mark-up pricing	
Wage mark-down	21.6	27.0	48.6
Marginal-product wages	17.1	19.4	36.4
Wage mark-up	0.7	14.3	15.0
$\sum$	39.4	60.6	

Notes: IAB Establishment Panel, 1999–2016, percentages of 40,856 plant-year observations, weighted using sample weights. Based on the estimates of the price-cost mark-up (equation 1.26) and the joint market imperfections parameter (equation 1.27), we classify observations to labor market settings using equation (1.19) and to product market settings using equation (1.18).

no less frequent in the product market where 61% of observations show mark-up pricing while only 39% involve marginal-cost pricing.

Simultaneously considering labor and product market imperfections, we find that only 17% of observations are free from market imperfections in that they combine marginal-product wages with marginal-cost pricing. The largest group are the 27% of observations that involve a wage mark-down and mark-up pricing followed by the 22% of observations with a wage mark-down together with marginal-cost pricing. Third come the 19% of observations combining marginal-product wages with mark-up pricing. Another 14% of observations involve a wage mark-up together with mark-up pricing, whereas we rarely observe the combination of a wage mark-up and marginal-cost pricing, which is unsurprising given that rents to be split between employers and workers are arguably small under marginal-cost pricing.

Turning to plants' industrial relations, we observe clear differences in the prevalence of market imperfections across plants covered and uncovered by collective bargaining and across plants with and without a works council (see Tables 1.4 and 1.5). In terms of labor market imperfections, a wage mark-down is less frequent and a wage mark-up is more frequent where collective bargaining or works councils are present. These correlations suggest that both forms of organized labor benefit workers in that they limit employers' ability to set a wage mark-down and more often give rise to a wage mark-up.

Table 1.4: Labor and product market settings of plants covered (uncovered) by collective agreements

Labor market setting	Product market setting		$\sum$
	Marginal-cost	Mark-up pricing	
Wage mark-down	14.2 (25.8)	33.2 (23.5)	47.4 (49.3)
Marginal-product wages	15.6 (17.9)	20.7 (18.6)	36.3 (36.5)
Wage mark-up	0.5 (0.8)	15.8 (13.4)	16.3 (14.2)
$\sum$	30.3 (44.6)	69.7 (55.4)	

Notes: IAB Establishment Panel, 1999–2016, percentages of 40,856 plant-year observations, weighted using sample weights. Based on the estimates of the price-cost mark-up (equation 1.26) and the joint market imperfections parameter (equation 1.27), we classify observations to labor market settings using equation (1.19) and to product market settings using equation (1.18).

Table 1.5: Labor and product market settings of plants with (without) a works council

Labor market setting	Product market setting		$\sum$
	Marginal-cost	Mark-up pricing	
Wage mark-down	14.7 (22.3)	22.3 (27.5)	37.0 (49.8)
Marginal-product wages	16.5 (17.1)	24.9 (18.8)	41.3 (35.9)
Wage mark-up	1.1 (0.7)	20.6 (13.6)	21.7 (14.3)
$\sum$	32.2 (40.1)	67.8 (59.9)	

Notes: IAB Establishment Panel, 1999–2016, percentages of 40,856 plant-year observations, weighted using sample weights. Based on the estimates of the price-cost mark-up (equation 1.26) and the joint market imperfections parameter (equation 1.27), we classify observations to labor market settings using equation (1.19) and to product market settings using equation (1.18).

Yet, they are also consistent with a causal link in the opposite direction with workers unionising or setting up a works council to foster rent extraction when confronted with a rather weak employer who pays a wage mark-up from the outset. Since our interest lies in how industrial relations shape labor market imperfections, we will later regress market imperfections on industrial relations and further control variables to substantiate a possible causal link running from industrial relations to imperfections.

Product market imperfections are more frequent when collective bargaining or works councils are present. Again, causality may be in both directions. On the one hand, these positive correlations are consistent with the view that organized labor hampers product market competition, for example through successfully lobbying for anti-competition policies. On the other hand, these correlations are also in line with workers unionising or electing works councils where product market competition is limited and rents to be distributed are therefore high, that is with a causal link from product market imperfections to the existence of any of these forms of organized labor. Later regressions may shed more light on the plausibility of a causal link running from industrial relations to imperfections.

To assess the intensity of labor market imperfections, we use our estimates of the joint market imperfections parameter and the price-cost mark-up to recover the structural parameters of the respective labor market setting off the labor demand curve. In other words, we look at our outcomes through the lens of monopsony or efficient bargaining as two structural models that are compatible with either a wage mark-down or a wage mark-up and that make sense in the German institutional setting. Specifically, we arrive at estimates of the plant-level labor supply elasticity $(\varepsilon_W^N)_{it}$ and the implied wage mark-down β_{it} when the outcome is below the labor demand curve and at estimates of workers' (relative) bargaining power ϕ_{it} (γ_{it}) when the outcome is above the curve. In the latter case, larger bargaining power points at an outcome farther away from the labor demand curve and thus at a wider wage mark-up. While using these specific models allows us to give a structural interpretation to our labor market imperfection parameters, the reported means in Table 1.6 could more generally be interpreted as sizes of wage mark-downs and wage mark-ups.

For the 49% of observations involving a wage mark-down, we find that the average plant-level labor supply elasticity amounts to 1.13, which points at marked monopsony power for plants. This number is not too different from the median of 1,320 elasticity estimates of 1.68 reported in Sokolova and Sorensen (2021) and is almost identical to the average elasticity estimate for US firms of 1.08 in Webber (2015), which is one of the rare studies that provide elasticity estimates at the individual firm level as we do. Note, however, that our average elasticity estimate is also in line with previous studies obtaining larger estimates because the average elasticity for all plants estimated by earlier studies is a weighted average of the elasticity in plants with a significant wage mark-down and the elasticity in those with no mark-down at all. The latter are plants paying

Table 1.6: The intensity of labor and product market imperfections (means)

Market imperfection intensity	All	Collective bargaining		Works council	
		Yes	No	Yes	No
Joint market imperfections parameter (ψ_{it})	–0.71	–0.76	–0.68	–0.49	–0.74
... when wage mark-down ($\psi_{it} < 0$)	–1.89	–1.93	–1.87	–1.93	–1.90
... when wage mark-up ($\psi_{it} > 0$)	1.67	1.33	1.91	1.19	1.75
Given wage mark-down ($\psi_{it} < 0$) ...					
Plant-level labor supply elasticity ($(\varepsilon_W^N)_{it}$)	1.13	1.17	1.11	1.29	1.12
Wage mark-down (β_{it})	0.45	0.46	0.45	0.47	0.45
Given wage mark-up ($\psi_{it} > 0$) ...					
Workers' absolute bargaining power (ϕ_{it})	0.48	0.44	0.50	0.45	0.48
Workers' relative bargaining power (γ_{it})	3.33	2.58	3.83	2.13	3.52
Price-cost mark-up (μ_{it})	1.23	1.24	1.23	1.32	1.22
... when mark-up pricing ($\mu_{it} > 1$)	1.39	1.35	1.41	1.46	1.38

Notes: IAB Establishment Panel, 1999–2016, weighted using sample weights. Based on the estimates of the price-cost mark-up (equation 1.26) and the joint market imperfections parameter (equation 1.27), we classify observations to labor market settings using equation (1.19) and to product market settings using equation (1.18). For a given labor market setting, structural parameters are recovered using equations (1.30)–(1.33).

wages on or above the labor demand curve, and thus plants facing very large elasticities. Plants' marked monopsony power translates into an average wage mark-down of 0.45, so on average workers obtain just 45% of the marginal product of labor when the outcome is below the labor demand curve.

For the 15% of observations involving a wage mark-up, we observe on average an absolute bargaining power of workers of 0.48, meaning that workers' bargaining power is roughly at par with employers' bargaining power. Note, however, that the average relative bargaining power of workers is much larger than one, which reflects that it is unbounded above and thus sensitive to outliers (i.e. observations with workers' absolute bargaining power near one).

Turning to the intensity of product market imperfections, we obtain an average price-cost mark-up of 1.23. Hence, on average prices are 23% above marginal costs, rendering the average mark-up across plants economically significant, but rather modest in size compared to existing estimates in the literature. Yet, one has to bear in mind that previous studies typically ignore labor market imperfections in that they assume marginal-product wages and thus, given that wage mark-downs are much more prevalent than wage mark-ups in our data, are prone to overstating the wedge between prices and marginal costs (as discussed in detail by De Loecker *et al.* 2016). And, reassuringly, our numbers are similar in size to recent estimates that allow for labor market imperfections (e.g. Dobbelaere *et al.* 2015; Soares 2020).

At the extensive margin, we find that the presence of collective bargaining or works councils is associated with a labor market setting that is more favourable to workers, that is a lower prevalence of wage mark-downs and a higher prevalence of wage mark-ups, and also with more mark-up pricing on the product market. Now that we look at the intensity of market imperfections, the picture emerging is less clear (see Table 1.6). Both types of organized labor are associated with a somewhat larger plant-level labor supply elasticity and thus a narrower wage mark-down. On the other hand, when a wage mark-up is present, workers' bargaining power is even somewhat lower if collective bargaining or works councils exist. These inconsistent correlation patterns, however, may simply be the result of confounding factors, such as plant size. Therefore, we now turn to partial correlations coming from regressions.

1.6.2 Regression analysis

So far, our statements about plants' labor and product market settings, the intensity of market imperfections, and their link to industrial relations have been entirely descriptive in nature. Although such a comprehensive description of market imperfections for Germany is novel and thus interesting on its own, we ultimately seek to make statements about how industrial relations shape market imperfections, both at the extensive and intensive margins. Obviously, the descriptive correlations between industrial relations and market imperfections cannot establish a causal link running from industrial relations to imperfections. To come a bit closer to causal statements, we now run several regressions for the prevalence and the intensity of market imperfections.

In terms of the extensive margin, we investigate which factors including industrial relations captured by dummies for collective bargaining coverage and the existence of works councils influence plants' labor and product market settings. Specifically, we run multinomial probit regressions for the labor market setting being one either involving a wage mark-down or a wage mark-up (as opposed to the baseline involving marginal-product wages) and probit regressions for mark-up pricing on the product market (as opposed to the baseline involving marginal-cost pricing).

Starting with labor market imperfections, Tables 1.7 and 1.8 report average marginal effects for the probability of a wage mark-down and a wage mark-up, respectively, from successively richer multinomial probit regressions. All models include as controls a full set of region, year, and two-digit sector dummies as well as a dummy for a single-plant company. We then successively include plant size, i.e. log employment, and dummies for plant age (model 2); information on workforce composition, i.e. the share of skilled workers, apprentices, part-time workers, and female workers (model 3); and a dummy for exporting activity (model 4).

Once we add plant size and plant age to the multinomial probit regression (models 2–4 of Table 1.7), we find that both the presence of collective bargaining and works councils is associated with a marked reduction in the conditional probability of a wage mark-down, which is in all models statistically significant at least at the 5% level. In our richest specification (model 4), collective bargaining is accompanied by an average drop in the probability of 3.1pp and works council existence even by a drop of 5.3pp, both of which are statistically significant at the 1% level. Furthermore, both forms of organized labor are positively related to the probability of a wage mark-up, though in the richest specification (model 4 of Table 1.8) the marginal effect of collective bargaining is only statistically significant at the 5% level. Collective bargaining is accompanied by an average rise in the probability of a wage mark-up of 1.6pp and works council existence even by a rise of 5.1pp. These findings support the view that organized labor matters for the labor market setting

Table 1.7: Average marginal effects for the probability of a wage mark-down from multinomial probit regressions

	(1)	(2)	(3)	(4)
Collective bargaining	–0.015*	–0.023**	–0.027***	–0.031***
	(0.009)	(0.009)	(0.009)	(0.009)
Works council	–0.016	–0.072***	–0.055***	–0.053***
	(0.011)	(0.012)	(0.012)	(0.012)
Log employment		0.036***	0.039***	0.043***
		(0.004)	(0.004)	(0.005)
Plant age 5–9 years		0.010	0.008	0.008
		(0.014)	(0.014)	(0.014)
Plant age 10–14 years		0.007	0.008	0.007
		(0.015)	(0.015)	(0.015)
Plant age 15–19 years		0.012	0.015	0.015
		(0.017)	(0.016)	(0.016)
Plant age $\geqslant$ 20 years		0.024*	0.025*	0.024*
		(0.014)	(0.014)	(0.014)
Share of skilled workers			–0.092***	–0.092***
			(0.018)	(0.018)
Share of apprentices			0.693***	0.677***
			(0.061)	(0.061)
Share of part-time workers			0.296***	0.283***
			(0.027)	(0.027)
Share of female workers			0.052**	0.057**
			(0.024)	(0.024)
Exporting activity				–0.045***
				(0.009)
Log likelihood	–32,941.3	–32,765.6	–32,102.1	–32,038.7
Number of observations	40,856			

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is a categorical variable for the classification of the labor market setting as involving either marginal-product wages or a wage mark-down or a wage mark-up. Reported numbers are average marginal effects on the probability of a wage mark-down with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

in that it seems to reduce the likelihood that employers can impose a wage mark-down on workers and to raise the likelihood that workers can push through a wage mark-up. And in line with our descriptive evidence, works council existence appears to matter more

Table 1.8: Average marginal effects for the probability of a wage mark-up from multinomial probit regressions

	(1)	(2)	(3)	(4)
Collective bargaining	0.004	0.009	0.013*	0.016**
	(0.007)	(0.007)	(0.007)	(0.007)
Works council	0.031***	0.064***	0.052***	0.051***
	(0.008)	(0.010)	(0.010)	(0.009)
Log employment		–0.020***	–0.019***	–0.023***
		(0.003)	(0.003)	(0.003)
Plant age 5–9 years		–0.015	–0.013	–0.013
		(0.011)	(0.011)	(0.011)
Plant age 10–14 years		–0.006	–0.004	–0.004
		(0.012)	(0.012)	(0.012)
Plant age 15–19 years		–0.005	–0.005	–0.004
		(0.013)	(0.013)	(0.013)
Plant age $\geqslant$ 20 years		–0.015	–0.014	–0.013
		(0.011)	(0.011)	(0.011)
Share of skilled workers			0.030**	0.031**
			(0.013)	(0.013)
Share of apprentices			–0.465***	–0.454***
			(0.052)	(0.052)
Share of part-time workers			–0.134***	–0.123***
			(0.021)	(0.021)
Share of female workers			–0.068***	–0.071***
			(0.019)	(0.018)
Exporting activity				0.038***
				(0.007)
Log likelihood	–32,941.3	–32,765.6	–32,102.1	–32,038.7
Number of observations		40,856		

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is a categorical variable for the classification of the labor market setting as involving either marginal-product wages or a wage mark-down or a wage mark-up. Reported numbers are average marginal effects on the probability of a wage mark-up with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

than collective bargaining coverage.

We further observe some interesting patterns for the control variables. Plant size shows a positive association with the probability of a wage mark-down and a negative one with the probability of a wage mark-up, whereas we find the opposite pattern for exporting plants (in line with previous evidence by Dobbelaere and Kiyota, 2018, for Japan). Hence, larger and non-exporting plants seem to be more powerful in the labor market. Finally, the composition of the workforce seems to matter. The probability of a wage mark-down is lower the more skilled workers are employed, whereas it is larger the more apprentices, part-timers, and females are among the workers, suggesting a more pronounced power imbalance for the latter groups. This latter suggestion is further substantiated by mirror-inverted patterns for the probability of a wage mark-up.

In analogy to the multinomial probit regressions for the labor market setting, Table 1.9 reports average marginal effects for the probability of mark-up pricing on the plant's product market. In all models, collective bargaining coverage turns out to be statistically insignificant. Once we add plant size and age to the probit regression (models 2–4), we find that works council presence is associated with a statistically significantly larger probability of an imperfect product market by 2.4–3.0pp. However, we are cautious not to over-interpret these partial correlations because workers arguably have a greater incentive to set up a works council where product market imperfections give rise to high rents and because later findings at the intensive margin will show that price-cost mark-ups are unrelated to works council existence. Hence, industrial relations seem to be of less importance for plants' product market setting. For the control variables, we find that the probability of an imperfect product market shows a statistically significant association with plant size (negative), workforce composition (positive for the share of skilled workers and negative for the shares of apprentices, part-timers, and female workers), and exporting activity (positive).

Turning to the intensive margin, we examine how industrial relations and the other plant characteristics included in our preferred specification of the (multinomial) probit regression influence the magnitude of labor and product market imperfections. Yet, meaningful measures of these imperfections are only available if plants' labor and product markets were classified as imperfect, that is if we have either $\psi_{it} < 0$ and thus a wage mark-down or $\psi_{it} > 0$ and thus a wage mark-up in the labor market or $\mu_{it} > 1$ and

Table 1.9: Average marginal effects from probit regressions for mark-up pricing

	(1)	(2)	(3)	(4)
Collective bargaining	0.004	0.009	0.010	0.012
	(0.007)	(0.007)	(0.007)	(0.007)
Works council	–0.022**	0.030***	0.025**	0.024**
	(0.009)	(0.010)	(0.010)	(0.010)
Log employment		–0.034***	–0.034***	–0.037***
		(0.004)	(0.004)	(0.004)
Plant age 5–9 years		0.001	0.001	0.001
		(0.012)	(0.012)	(0.012)
Plant age 10–14 years		0.006	0.007	0.007
		(0.013)	(0.013)	(0.013)
Plant age 15–19 years		0.011	0.010	0.011
		(0.014)	(0.014)	(0.014)
Plant age ⩾ 20 years		0.010	0.009	0.010
		(0.012)	(0.012)	(0.012)
Share of skilled workers			0.047***	0.048***
			(0.015)	(0.015)
Share of apprentices			–0.164***	–0.153***
			(0.046)	(0.046)
Share of part-time workers			–0.040**	–0.034*
			(0.020)	(0.020)
Share of female workers			–0.037**	–0.038**
			(0.019)	(0.019)
Exporting activity				0.024***
				(0.008)
Log likelihood	–19,020.3	–18,871.1	–18,819.9	–18,807.7
Number of observations	40,856			

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is an indicator for the product market setting involving mark-up pricing. Reported numbers are average marginal effects with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

thus mark-up pricing in the product market. Rather than running OLS regressions, we correct for censoring by fitting type II Tobit models, in which the first-stage probit participation equation for $\psi_{it} < 0$, $\psi_{it} > 0$, or $\mu_{it} > 1$, respectively, and the second-stage outcome equation for the respective imperfection parameter include the same regressors, but these are allowed to have different coefficients in the two equations (e.g. Cameron and Trivedi 2005). Table 1.10 presents the results for the second-stage outcome equations and underscores that what we found at the extensive margin with few exceptions also shows up at the intensive margin. Since all dependent variables are in logs, estimated coefficients are interpretable as (approximate) percentage changes and thus directly inform us on the economic significance of the respective variables.

Given a wage mark-down, we find that the presence of collective bargaining and works councils reduces plants' wage-setting power, which is in line with some suggestive earlier evidence presented by Bachmann and Frings (2017). The labor supply elasticity is on average 5.7% larger and the gap between the marginal revenue product and workers' wages is on average 4.5% narrower in covered than in uncovered plants, both statistically significant at least at the 5% level. Furthermore, works council existence is associated with a 7.2% higher elasticity and a 5.2% narrower gap. We also find the same patterns for the control variables that we obtained at the extensive margin. Plants' monopsony power shows a positive association with plant size and a negative with exporting activity (as is found by Dobbelaere and Kiyota, 2018, for Japan). Moreover, plants' monopsony power is significantly related to workforce composition. It is smaller the more skilled workers are employed and larger the more apprentices, part-timers, and females are in the workforce. Particularly the latter finding for females is in line with existing evidence that employers possess more monopsony power over female as opposed to male workers (see the recent survey by Hirsch, 2016, and Hirsch *et al.*, 2010, for Germany).

Given a wage formation process involving a wage mark-up, we find that the presence of collective bargaining is associated with a rise in workers' relative bargaining power by 8.9% and the presence of a works council even with a rise by 12.7%, though only the latter association is statistically significant at the 10% level, which may reflect the rather small number of observations involving a wage mark-up. As with the extensive margin, works councils seem to be more important for workers' monopoly power in wage formation than collective bargaining. These findings do make sense as collective bargaining is typically conducted at the sectoral level and thus is unlikely to loosen employers' control over

Table 1.10: Estimates of the second-stage output equation of type II Tobit regressions for the intensity of market imperfections

	Log of...			
	plant-level labor supply elasticity $((\varepsilon_W^N)_{it})$	wage mark-down (β_{it})	workers' relative bargaining power (γ_{it})	price-cost mark-up (μ_{it})
Collective bargaining	0.057**	0.045***	0.089	0.006
	(0.025)	(0.015)	(0.055)	(0.004)
Works council	0.072**	0.052**	0.127*	0.008
	(0.034)	(0.021)	(0.066)	(0.006)
Log employment	–0.148***	–0.095***	–0.074**	–0.015***
	(0.013)	(0.008)	(0.030)	(0.003)
Plant age 5–9 years	0.029	0.024	–0.213**	–0.010
	(0.038)	(0.024)	(0.095)	(0.008)
Plant age 10–14 years	0.082*	0.041	–0.149	–0.019**
	(0.042)	(0.026)	(0.100)	(0.009)
Plant age 15–19 years	0.081*	0.036	–0.161	–0.021**
	(0.044)	(0.027)	(0.109)	(0.009)
Plant age $\geqslant$ 20 years	0.102***	0.048**	–0.113	–0.017**
	(0.038)	(0.024)	(0.092)	(0.008)
Share of skilled workers	0.524***	0.277***	0.320***	–0.006
	(0.046)	(0.027)	(0.095)	(0.010)
Share of apprentices	–0.660***	–0.472***	–2.183***	–0.121***
	(0.150)	(0.094)	(0.423)	(0.032)
Share of part-time workers	–0.963***	–0.546***	–0.132	–0.000
	(0.062)	(0.038)	(0.171)	(0.015)
Share of female workers	–0.209***	–0.087**	–0.299*	–0.001
	(0.064)	(0.037)	(0.154)	(0.013)
Exporting activity	0.093***	0.052***	0.097*	–0.005
	(0.026)	(0.016)	(0.054)	(0.005)
Log likelihood	–31,369.9	–20,131.8	–17,290.1	–2,022.6
Number of observations	26,930	26,930	13,642	31,695

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is the logarithm of the respective market imperfection intensity measure. Reported numbers are coefficients from the outcome equation of type II Tobit regressions with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

employment, whereas worker co-determination at the workplace may more plausibly force employers off the labor demand curve. They further square up with the result of Hirsch and Mueller (2020) that works council existence has a stronger association with the mean employer wage premium than collective bargaining coverage. For the control variables, we obtain the same patterns as at the extensive margin, which are again mirror-inverted *vis-à-vis* the patterns for plants' monopsony power.

Finally, we find that the intensity of product market imperfections measured by the size of above-one price-cost mark-ups is unrelated to industrial relations, both in terms of effect size and statistical significance. In terms of the control variables, we see some differences to the extensive margin, particularly for plant age, but typically effect sizes are modest.

1.6.3 Analysis of switches in plants' labor market setting

Exploiting the time-varying nature of our estimates of the joint market imperfections parameter and the price-cost mark-up and the resulting classification of plants' labor market setting, we next investigate how switches in plants' labor market setting are related to the presence of collective bargaining and works councils. In doing so, we hope to further back up the claim that industrial relations shape labor market imperfections (rather than the other way round). Besides, such an analysis promises suggestive evidence on whether the deterioration of organized labor during our period of observation shifted market power from workers to employers and is thus plausibly contributing to the long-term trends of a falling labor share in income and rising wage inequality.

Table 1.11 provides a transition matrix for the three labor market settings. What emerges is that wage mark-downs are by far the most persistent among the three settings. For 87% of plants with a wage mark-down, we also find a wage mark-down in the subsequent observation. On the other hand, 13% of plants with a wage mark-down change their labor market setting, with almost all of them changing to marginal-product wages. In terms of persistence, marginal-product wages come next. 73% of plants with marginal-

Table 1.11: Transition matrix for plants' labor market setting

Labor market setting in t	Labor market setting in $t+1$		
	Wage mark-down	Marginal-product wages	Wage mark-up
Wage mark-down	86.8	11.9	1.2
Marginal-product wages	16.1	73.1	10.8
Wage mark-up	6.3	26.0	67.7

Notes: IAB Establishment Panel, 1999–2016, percentages of 31,795 plant-year observations, weighted using sample weights in t. Based on the estimates of the joint market imperfections parameter (equation 1.27), we classify observations to labor market settings using equation (1.19).

product wages stay in this setting in the subsequent observation, whereas 16% of plants change to a wage mark-down and 11% of plants switch to a wage mark-up. Finally, 68% of plants with a wage mark-up keep having one in the subsequent observation, whereas 26% switch to marginal-product wages and 6% to a wage mark-down.[12]

Table 1.12: Transition matrix for the labor market setting of plants covered (uncovered) by collective agreements

Labor market setting in t	Labor market setting in $t+1$		
	Wage mark-down	Marginal-product wages	Wage mark-up
Wage mark-down	87.0 (86.7)	11.8 (12.0)	1.2 (1.3)
Marginal-product wages	15.9 (16.2)	72.4 (73.5)	11.8 (10.3)
Wage mark-up	4.8 (7.3)	24.6 (27.0)	70.7 (65.7)

Notes: IAB Establishment Panel, 1999–2016, percentages of 31,795 plant-year observations, weighted using sample weights in t. Based on the estimates of the joint market imperfections parameter (equation 1.27), we classify observations to labor market settings using equation (1.19).

Separate transition matrices for plants covered by a collective agreement and uncovered plants (see Table 1.12) reveal that covered plants are more likely to move from marginal-product wages to a wage mark-up than uncovered plants and are considerably more

12 We also checked whether plants entering or exiting our sample differ in terms of their labor market settings from those plants staying in our sample, which contribute to the reported transition matrix. Notably, exit probabilities are very similar across the three labor market settings, and also the prevalence of the respective labor market settings for plants entering our sample does not differ much from the prevalence of settings for incumbent plants. Hence, the picture would not change when accounting for compositional changes following plant entry and plant exit.

Table 1.13: Transition matrix for the labor market setting of plants with (without) a works council

Labor market setting in t	Labor market setting in $t+1$		
	Wage mark-down	Marginal-product wages	Wage mark-up
Wage mark-down	84.6 (87.0)	12.5 (11.9)	2.9 (1.1)
Marginal-product wages	11.2 (16.7)	78.4 (72.4)	10.5 (10.9)
Wage mark-up	5.8 (6.3)	20.1 (26.9)	74.1 (66.7)

Notes: IAB Establishment Panel, 1999–2016, percentages of 31,795 plant-year observations, weighted using sample weights in t. Based on the estimates of the joint market imperfections parameter (equation 1.27), we classify observations to labor market settings using equation (1.19).

likely to keep having a wage mark-up. Separate transition matrices for plants with and without a works council (see Table 1.13) show more persistent wage mark-ups in co-determined plants compared to plants without a works council, whereas the opposite holds for the persistence of wage mark-downs. Furthermore, plants with a works council are considerably less likely to switch from marginal-product wages to a wage mark-down than plants without.

Most of these patterns also show up when running probit regressions for the four most observed types of switches in plants' labor market setting, for which the numbers of observations are sufficiently large for estimation purposes (see Table 1.14). Collective bargaining coverage is on average associated with a 2.0pp larger probability of changing from marginal-product wages to a wage mark-up, which is statistically significant at the 1% level. At the same time, it reduces the probability of switching from marginal-product wages to a wage mark-down by 1.6pp, which is statistically significant at the 10% level, as well as the probability of switching from a wage mark-up to marginal-product wages by 1.7pp, which is statistically insignificant at conventional levels probably due to the small number of transitions from a wage mark-up to marginal-product wages. These latter two switching probabilities show an even more pronounced negative correlation with works council existence that is on average associated with a drop in the switching probability from marginal-product wages to a wage mark-down by 4.7pp and a drop in the switching

Table 1.14: Average marginal effects from probit regressions for a switch in the plant's labor market setting

	(1)	(2)	(3)	(4)
	Marginal-product wages to wage mark-down	Wage mark-down to marginal-product wages	Marginal-product wages to wage mark-up	Wage mark-up to marginal-product wages
Collective bargaining	–0.016*	0.007	0.020***	–0.017
	(0.009)	(0.008)	(0.007)	(0.016)
Works council	–0.047***	0.009	0.006	–0.068***
	(0.011)	(0.011)	(0.010)	(0.019)
Log employment	–0.003	–0.032***	–0.015***	–0.000
	(0.004)	(0.005)	(0.003)	(0.008)
Plant age 5–9 years	0.049***	0.001	–0.005	–0.026
	(0.017)	(0.018)	(0.015)	(0.032)
Plant age 10–14 years	0.038**	–0.003	0.002	–0.035
	(0.018)	(0.018)	(0.016)	(0.032)
Plant age 15–19 years	0.038**	0.001	–0.013	–0.027
	(0.018)	(0.018)	(0.016)	(0.034)
Plant age $\geqslant$ 20 years	0.031**	–0.007	–0.008	–0.033
	(0.016)	(0.016)	(0.014)	(0.029)
Share of skilled workers	–0.027	0.061***	0.032**	–0.002
	(0.018)	(0.017)	(0.015)	(0.029)
Share of apprentices	0.335***	–0.131***	–0.127**	0.347***
	(0.061)	(0.049)	(0.055)	(0.111)
Share of part-time workers	0.01	–0.137***	–0.032	0.114**
	(0.031)	(0.022)	(0.023)	(0.056)
Share of female workers	0.053**	–0.082***	–0.017	0.063
	(0.023)	(0.021)	(0.019)	(0.045)
Exporting activity	–0.001	0.020**	0.006	–0.047***
	(0.009)	(0.009)	(0.007)	(0.016)
Log likelihood	–4826.3	–4907.8	–3473.4	–2696.1
Number of observations	12,222	13,630	12,201	5,305

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is a dummy variable that indicates a switch in the labor market setting in the respective direction for two consecutive observations of the same plant. Reported numbers are average marginal effects with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

probability from a wage mark-up to marginal-product wages by 6.8pp, both of which are statistically significant at the 1% level. Given the marked persistence of plants' labor market setting, all these numbers represent sizeable changes.

In summary, we find that both types of organized labor favour workers in that they reduce the probability of unfavourable switches in plants' labor market setting, that is moving away from a wage mark-up or into a wage mark-down. We thus have further evidence suggesting that industrial relations shape labor market imperfections. And, reassuringly, these findings are unlikely to suffer from reversed causality running from labor market imperfections to industrial relations and therefore strengthen our results from regressions of labor market imperfections on industrial relations where issues of reversed causality are more of a concern.

Briefly turning to product market imperfections, the presence of any form of organized labor is associated with a higher persistence of the current product market setting (see the further tables reported in Appendix 1.B). We see this both at the descriptive level by means of transition matrices and in a pair of probit regressions for the two types of switches from marginal-cost to mark-up pricing and the other way round. These findings are in line with the notion that organized labor limits management's flexibility in decision-taking, also when it comes to pricing decisions in the product market.

1.7 Do labor market imperfections matter for employer wage premia?

Our findings so far strongly suggest that industrial relations matter for labor market imperfections. But do labor market imperfections, in turn, matter for the wage premium paid by employers to their workers? In other words, what is the impact of labor market imperfections on the level and the dispersion of wages after accounting for sorting of workers with different abilities into plants that differ in labor market imperfections and in the size of rents to be split between employers and workers? Answering this question is not only crucial for our research question and against the background that rising dispersion in employer wage premia is an important driver of increasing wage inequality in Germany (Card *et al.* 2013), but also provides a most welcome opportunity of cross-validating our measures of labor market imperfections, that is examining their predictive power for actual employer wage premia.

Up to now, there is scant evidence on this issue, though some recent contributions find that labor market imperfections are associated with wages (not employer wage premia). Hirsch *et al.* (2021) show that employers' smaller monopsony power in denser local labor markets accounts for about half of the urban wage premium in Germany. For the US, Azar *et al.* (2021) observe lower posted wages in more concentrated local labor markets and Benmelech *et al.* (2021) find a negative association between labor market concentration and wages that is rising over time and more pronounced where unionisation rates are low. Furthermore, Brooks *et al.* (2021) show that wage mark-downs substantially depress the labor share in China and India whereas Berger *et al.* (2018) find that labor market concentration, while substantial, has not contributed to the falling US labor share. Finally, Rinz (2021) documents for the US that higher labor market concentration is accompanied by higher wage inequality while Webber (2015) finds that a larger labor supply elasticity to the employer reduces the dispersion of wages because its wage-lifting effect is most pronounced at the lower end of the wage distribution.

All this evidence, however, is about individual wages and not about employer wage premia, that is wage differences that are left after differences in workers' human capital and unobservable skills have been rewarded, and thus worker sorting may contaminate findings. To obtain a measure of employer wage premia that does not suffer from worker sorting, we follow Card *et al.* (2018) and Hirsch and Mueller (2020) and rely on the plant wage effect from a two-way fixed-effects decomposition of log wages *à la* Abowd *et al.* (1999, AKM hereafter) estimated for our data by Bellmann *et al.* (2020). In the AKM framework, which provides a suitable approximation of the German wage structure (Card *et al.* 2013), the plant wage effect measures the wage premium enjoyed by all workers in a plant's workforce adjusted for observed and unobserved worker quality. Since we are interested in how labor market imperfections shape wage outcomes for a given plant surplus, we further follow Hirsch and Mueller (2020) in controlling for the quasi rent per worker as the proper measure of this surplus. We provide details on our measures of plant wage premia and plant surplus in Appendix 1.C.

For the subsample of 36,633 plant-year observations for which AKM plant wage effects are available, we investigate the link between employer wage premia and labor market imperfections by regressing the standardised plant wage effect on measures of labor market imperfections, the quasi rent per worker to control for the plant surplus, and all the control variables included in the regressions before.[13] As measures of labor market imperfections at the extensive margin, we include dummy variables for the existence of a wage mark-down and a wage mark-up. And given a specific labor market setting involving either a wage mark-down or a wage mark-up, we include the plant-level labor supply elasticity, the size of the wage mark-down, or workers' relative bargaining power as the respective measure of the intensity of labor market imperfections. For all our labor market imperfection measures, that is for the extensive-margin indicators and the three intensive-margin variables, we estimate four regression models: an OLS regression for the mean employer wage premium, which provides the impact of labor market imperfections on the level of wage premia, and re-centred influence function (RIF) regressions (Firpo *et al.* 2009) for the variance, the first decile, and the ninth decile of the unconditional wage premium distribution, which inform us on their influence on the dispersion of wage premia.

Table 1.15 presents our results at the extensive margin of labor market imperfections. Holding constant plant surplus and the other control variables, a labor market setting involving a wage mark-down is accompanied by a 0.15 standard deviations lower mean wage premium (where a standard deviation in employer wage premia amounts to about 28 log points in our data). Whereas the level of wage premia is thus lower when there is a wage mark-down, the opposite holds for the dispersion of wage premia. A wage mark-down is associated with a 14% larger variance (of standardised wage premia), which reflects that

[13] Note that our earlier results for the link between industrial relations and labor market imperfections also show up in this reduced sample, though estimation precision is a bit lower than in the full sample.

Table 1.15: Level and dispersion of plant wage premia and the plant's labor market setting (wage premium OLS and RIF regressions)

	(1)	(2)	(3)	(4)
	Mean	Variance	First decile	Ninth decile
Wage mark-down	–0.148***	0.139***	–0.196***	–0.042***
	(0.016)	(0.032)	(0.022)	(0.014)
Wage mark-up	0.076***	–0.014	0.059***	0.063***
	(0.017)	(0.037)	(0.022)	(0.016)
Quasi rent per worker	0.002***	–0.001***	0.001***	0.001***
(in 100,000 €)	(0.000)	(0.000)	(0.000)	(0.000)
Log employment	0.183***	–0.089***	0.166***	0.137***
	(0.007)	(0.011)	(0.007)	(0.005)
Plant age 5–9 years	–0.049*	0.096	–0.075	0.001
	(0.030)	(0.077)	(0.052)	(0.032)
Plant age 10–14 years	–0.076**	0.090	–0.104**	–0.022
	(0.033)	(0.076)	(0.053)	(0.030)
Plant age 15–19 years	–0.017	0.028	–0.214***	–0.013
	(0.035)	(0.078)	(0.056)	(0.031)
Plant age ⩾ 20 years	–0.008	–0.042	–0.080*	–0.044
	(0.030)	(0.069)	(0.045)	(0.029)
Share of skilled workers	0.297***	–0.242***	0.479***	0.059**
	(0.032)	(0.059)	(0.043)	(0.025)
Share of apprentices	–0.350***	–0.795***	0.333**	–0.458***
	(0.113)	(0.207)	(0.169)	(0.069)
Share of part-time workers	0.064	1.270***	–0.289***	0.459***
	(0.061)	(0.090)	(0.078)	(0.037)
Share of female workers	–0.315***	0.017	–0.539***	–0.197***
	(0.048)	(0.075)	(0.059)	(0.029)
Exporting activity	0.075***	–0.074**	0.094***	–0.016
	(0.016)	(0.030)	(0.018)	(0.013)
R^2	0.539	0.040	0.197	0.183
Number of observations	36,633			

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is the standardised plant wage effect. Reported numbers are coefficients from OLS and RIF regressions with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

a wage mark-down is associated with a 0.2 standard deviations lower first decile and a 0.04 standard deviations lower ninth decile of wage premia and thus widens the wage premium distribution. All these partial correlations are statistically significant at the 1% level. Our findings suggest not only that wage mark-downs harm workers in reducing the level of employer wage premia for a given surplus, but also that workers with low-premium employers suffer most and that wage mark-downs thus aggravate inequality.

In contrast to wage mark-downs, wage mark-ups are only related to the level but not to the dispersion of wage premia. The existence of a wage mark-up is accompanied by a statistically significant rise in the mean wage premium by 0.08 standard deviations and little change in premia variance because its influence is the same, i.e. roughly 0.06 standard deviations, at the first and the ninth decile of the wage premium distribution. Hence, wage mark-ups seem to benefit workers uniformly leaving inequality unaltered. In passing, we note that the R^2 of 0.54 in the OLS regression means that the included regressors can account for the majority of the variation in wage premia, and we further note that the included control variables show little surprises so that we leave them uncommented.

Turning to the intensive margin of labor market imperfections, Tables 1.16, 1.17, and 1.18 present analogous regressions that include (in logs) the plant-level labor supply elasticity, the size of the wage mark-down, and workers' relative bargaining power as respective measures of the intensity of labor market imperfections provided there exists either a wage mark-down or a wage mark-up. Remarkably and reassuringly, all our findings at the extensive margin also show up at the intensive margin.

Starting with the plant-level labor supply elasticity that measures the intensity of employers' monopsony power, Table 1.16 shows that employers' monopsony power is significantly related to both the level and the dispersion of wage premia. When a wage mark-down is present, which is the case for 15,503 observations in the subsample of plants with AKM plant wage effects, a one standard deviation larger log elasticity, which amounts to 0.88 in our sample, is associated with a 0.09 ($= 0.88 \times 0.107$) standard deviations larger mean plant wage premium, which is statistically significant at the 1% level. Further, such an increase is accompanied by a statistically significant drop in the variance of premia by 19.4%, which reflects the associated rise of the first decile of the premium distribution by

Table 1.16: Level and dispersion of plant wage premia and the plant-level labor supply elasticity (wage premium OLS and RIF regressions)

	(1)	(2)	(3)	(4)
	Mean	Variance	First decile	Ninth decile
Log of plant-level labor	0.107***	–0.220***	0.196***	–0.019
supply elasticity ($(\varepsilon_W^N)_{it}$)	(0.013)	(0.024)	(0.016)	(0.012)
Quasi rent per worker	0.002***	–0.001***	0.001***	0.002***
(in 100,000 €)	(0.000)	(0.000)	(0.000)	(0.000)
Log employment	0.195***	–0.091***	0.163***	0.146***
	(0.011)	(0.017)	(0.011)	(0.010)
Plant age 5–9 years	–0.046	–0.068	0.005	–0.062
	(0.052)	(0.109)	(0.078)	(0.053)
Plant age 10–14 years	–0.067	0.008	–0.079	0.002
	(0.056)	(0.108)	(0.080)	(0.050)
Plant age 15–19 years	–0.026	–0.072	–0.181**	0.012
	(0.057)	(0.110)	(0.083)	(0.050)
Plant age $\geqslant$ 20 years	–0.031	–0.188*	–0.047	–0.057
	(0.052)	(0.096)	(0.068)	(0.047)
Share of skilled workers	0.220***	–0.231***	0.392***	0.022
	(0.051)	(0.084)	(0.060)	(0.043)
Share of apprentices	–0.381***	–0.622**	0.177	–0.531***
	(0.146)	(0.274)	(0.227)	(0.104)
Share of part-time workers	0.304***	1.163***	0.208**	0.519***
	(0.084)	(0.118)	(0.098)	(0.057)
Share of female workers	–0.149**	–0.162	–0.300***	–0.244***
	(0.067)	(0.103)	(0.081)	(0.047)
Exporting activity	0.026	0.027	0.019	–0.019
	(0.023)	(0.045)	(0.025)	(0.020)
R^2	0.560	0.062	0.213	0.219
Number of observations		15,503		

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is the standardised plant wage effect. Reported numbers are coefficients from OLS and RIF regressions with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

0.17 standard deviations and the almost unaltered ninth decile.

Considering the size of the wage mark-down instead, Table 1.17 shows that a one standard deviation narrower log wage mark-down, which amounts to 0.52 in our sample,

Table 1.17: Level and dispersion of plant wage premia and the size of the wage mark-down (wage premium OLS and RIF regressions)

	(1)	(2)	(3)	(4)
	Mean	Variance	First decile	Ninth decile
Log of wage mark-down (β_{it})	0.169***	–0.373***	0.341***	–0.035
	(0.024)	(0.040)	(0.028)	(0.023)
Quasi rent per worker	0.002***	–0.001***	0.001***	0.002***
(in 100,000 €)	(0.000)	(0.000)	(0.000)	(0.000)
Log employment	0.193***	–0.089***	0.162***	0.146***
	(0.011)	(0.017)	(0.011)	(0.010)
Plant age 5–9 years	–0.045	–0.068	0.005	–0.061
	(0.052)	(0.109)	(0.078)	(0.053)
Plant age 10–14 years	–0.066	0.009	–0.080	0.002
	(0.056)	(0.108)	(0.079)	(0.050)
Plant age 15–19 years	–0.025	–0.071	–0.183**	0.012
	(0.057)	(0.110)	(0.083)	(0.050)
Plant age $\geqslant$ 20 years	–0.031	–0.187*	–0.049	–0.057
	(0.052)	(0.096)	(0.068)	(0.048)
Share of skilled workers	0.227***	–0.241***	0.398***	0.022
	(0.052)	(0.084)	(0.060)	(0.043)
Share of apprentices	–0.405***	–0.578**	0.140	–0.528***
	(0.146)	(0.274)	(0.226)	(0.103)
Share of part-time workers	0.293***	1.176***	0.200**	0.519***
	(0.084)	(0.118)	(0.098)	(0.057)
Share of female workers	–0.155**	–0.154	–0.306***	–0.244***
	(0.067)	(0.103)	(0.080)	(0.047)
Exporting activity	0.028	0.024	0.021	-0.019
	(0.023)	(0.045)	(0.025)	(0.020)
R^2	0.559	0.061	0.213	0.219
Number of observations	15,503			

Notes: IAB Establishment Panel, 1999–2016. The dependent variable the is standardised plant wage effect. Reported numbers are coefficients from OLS and RIF regressions with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

is accompanied by a rise in the mean premium by 0.09 standard deviations and a drop in the variance of wage premia by 19.4%, both statistically significant at the 1% level. Observe that these estimates for the wage mark-down are of the very same size as the estimates for the elasticity. This finding is reassuring and hardly surprising given that the elasticity and the wage mark-down are two sides of the same coin under monopsony. The drop in the variance of wage premia reflects the associated rise in the first decile of the premium distribution from the narrowing of the wage mark-down by 0.18 standard deviations and the nearly constant ninth decile.

In short, our findings show that more monopsony power harms workers and particularly those working for low-premium plants thereby aggravating inequality in employer wage premia. This squares with our findings at the extensive margin and documents that both the existence of wage mark-downs and the intensity of employers' monopsony power seem to matter for employer wage premia.

Turning to the intensity of wage mark-ups, which are present for 6,830 observations, Table 1.18 shows that a one standard deviation larger log relative bargaining power of workers, which amounts to 1.15 in our sample, is accompanied by a 0.05 standard deviations larger mean plant wage premium and a 0.06 (0.03) standard deviations larger first (ninth) decile of the premium distribution and thus little change in the variance of plant wage premia. All these partial correlations are statistically significant at least at the 5% level, though effect sizes are more modest than in the case of a wage mark-down. Hence, given a wage mark-up, more bargaining power of workers benefits workers uniformly across the wage premium distribution, though to a modest extent.

In summary, our findings suggest that labor market imperfections matter for employer wage premia, and in the way predicted by theory thereby cross-validating our measures of imperfections in the labor market. The existence of a wage mark-down harms workers, with workers working for low-premium employers suffering most. It thus not only depresses employer wage premia, but also aggravates inequality. In contrast, the presence of a wage mark-up benefits workers uniformly leaving inequality unaltered, though effect sizes are smaller for a wage mark-up than for wage mark-down. These findings at the extensive margin also show up at the intensive margin when considering the plant-level labor supply elasticity, the size of the wage mark-down, and workers' relative bargaining power. In consequence, both the existence and the intensity of labor market imperfections seem to

Table 1.18: Level and dispersion of plant wage premia and workers' relative bargaining power (wage premium OLS and RIF regressions)

	(1)	(2)	(3)	(4)
	Mean	Variance	First decile	Ninth decile
Log of workers' relative	0.040***	0.016	0.050**	0.027**
bargaining power (γ_{it})	(0.013)	(0.028)	(0.021)	(0.012)
Quasi rent per worker	0.001**	–0.001*	0.001***	0.001***
(in 100,000 €)	(0.000)	(0.000)	(0.000)	(0.000)
Log employment	0.187***	–0.097***	0.181***	0.143***
	(0.016)	(0.029)	(0.020)	(0.014)
Plant age 5–9 years	–0.082	0.361**	–0.523***	0.009
	(0.056)	(0.182)	(0.115)	(0.071)
Plant age 10–14 years	–0.124**	0.409**	–0.695***	–0.020
	(0.058)	(0.179)	(0.119)	(0.067)
Plant age 15–19 years	–0.035	0.167	–0.503***	–0.030
	(0.065)	(0.186)	(0.127)	(0.071)
Plant age $\geqslant$ 20 years	–0.011	0.269*	–0.405***	0.015
	(0.051)	(0.159)	(0.085)	(0.063)
Share of skilled workers	0.270***	–0.210	0.423***	0.047
	(0.061)	(0.139)	(0.105)	(0.054)
Share of apprentices	–0.009	–3.141***	1.883***	–0.668***
	(0.251)	(0.521)	(0.483)	(0.149)
Share of part-time workers	–0.087	1.554***	–0.575**	0.437***
	(0.136)	(0.244)	(0.241)	(0.098)
Share of female workers	–0.429***	–0.186	–0.810***	–0.199***
	(0.098)	(0.203)	(0.173)	(0.077)
Exporting activity	0.102***	–0.111	0.141***	0.018
	(0.036)	(0.072)	(0.047)	(0.032)
R^2	0.528	0.076	0.258	0.169
Number of observations	6,830			

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is the standardised plant wage effect. Reported numbers are coefficients from OLS and RIF regressions with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region, year, and two-digit sector dummies as well as a dummy for a single-plant company.

influence the level and the dispersion of employer wage premia while they themselves seem to be shaped by industrial relations.

1.8 Conclusions

This paper has investigated the interplay between industrial relations, labor market imperfections, and employer wage premia in Germany and posed two questions. Do industrial relations matter for labor market imperfections? And do labor market imperfections, in turn, matter for employer wage premia? Using representative plant-level data from the IAB Establishment Panel encompassing the years 1999–2016, we answered both questions in the affirmative.

We approached these two questions using the production function approach of Dobbelaere and Mairesse (2013) that allows to determine labor and product market imperfections from production function estimates. In the labor market, the approach allows for competitive outcomes on the labor demand curve involving marginal-product wages, or outcomes off the curve with employers' monopsony power enabling them to impose a wage mark-down on workers or workers' monopoly power permitting them to push through a wage mark-up. Moreover, the approach enables us to make statements about the intensity of labor market imperfections for outcomes off the labor demand curve. Specifically, it allows to recover the labor supply elasticity to the single employer and thus employers' monopsony power when wage formation involves a wage mark-down and workers' relative bargaining power when there exists a wage mark-up. In the product market, the approach encompasses competitive solutions involving marginal-cost prices as well as mark-up pricing, for which it allows to recover the price-cost mark-up.

At a descriptive level, we found that wage mark-downs are the most prevalent outcome in the labor market (49% of plant-year observations), followed by outcomes on the labor demand curve involving marginal-product wages (36%), whereas wage mark-ups are much less frequent (15%). Notably, wage mark-ups are almost always accompanied by mark-up pricing suggesting that they are only sustainable when product market imperfections shield employers from competition. We further observed that wage mark-downs are less frequent when collective bargaining or plant-level co-determination through works councils are present and that the opposite holds for wage mark-ups. These findings at the extensive margin, that is with respect to the prevalence of outcomes off the labor demand curve, are complemented by results at the intensive margin, i.e. within labor market settings, where we observe that employers' monopsony power (workers' bargaining power) is less (more) pronounced when collective bargaining or works councils exist, other things being equal.

All these descriptive correlations between labor market imperfections and industrial relations also showed up in multinomial probit regressions for the labor market setting and type II Tobit regressions for the intensity of labor market imperfections that control for a broad range of plant characteristics. Collective bargaining and, even more so, works council existence have a marked association with labor market imperfections at the extensive and intensive margins. Turning to switches in plants' labor market setting over time, we further observed that wage mark-downs are most persistent, followed by marginal-product wages, whereas wage mark-ups are least persistent. We also saw in probit regressions that the presence of collective bargaining and works councils is associated with a lower probability of switching from a wage mark-up to marginal-product wages and a lower switching probability from marginal-product wages to a wage mark-down, lending further credence to a causal link running from industrial relations to labor market imperfections.

Finally, we found that employer wage premia are smaller and more dispersed when a wage mark-down is present as workers with low-premium employers suffer most. In contrast, we saw that the existence of a wage mark-up is accompanied by larger pay premia but leaves their dispersion unaltered as wage mark-ups benefit workers uniformly across the premium distribution. On top of these results at the extensive margin, the same patterns showed up for the intensity of labor market imperfections within a given labor market setting.

In short, our results document that labor market imperfections are the norm rather than the exception in Germany and typically give rise to a power imbalance favouring employers who are able to impose a wage mark-down on workers. Wage mark-downs, in turn, harm workers as they are associated with lower employer wage premia. And they also aggravate inequality in that they are accompanied by more dispersed wage premia because workers with low-premium jobs suffer most. What is more, our findings strongly suggest that labor market imperfections are shaped by industrial relations, with collective bargaining and worker co-determination shifting market power from employers to workers. Hence, they point at organized labor's erosion as one possible contributor to the falling labor share and rising wage inequality. While our regression results, in particular those for switches in labor market settings, go some way in substantiating causal links running from industrial relations to labor market imperfections and from labor market imperfections to employer wage premia, establishing causality in a rigorous way using exogenous variation in industrial relations remains a promising avenue for future research.

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Appendix

1.A Estimating plants' production function

Our estimation approach to plants' production function (1.20) follows Ackerberg *et al.* (2015) and rests on the following timing assumptions. We assume that plants decide on their capital input k_{it} one period ahead at time $t-1$, which reflects planning and installation lags and causes capital to be predetermined. Among the variable factors of production, we assume that labor n_{it} is less variable than intermediate inputs m_{it} in that it is determined by plants at time $t-b$ with $0<b<1$. Hence, plants choose labor after capital but prior to intermediate inputs, where the latter is in line with plants requiring time to train new workers, with significant firing or hiring costs, or with long-lasting labor contracts in internal labor markets or unionised plants.

With respect to unobservable productivity, we assume that ω_{it} evolves according to an endogenous first-order Markov process. In particular, we assume that the plant's decision to engage in exporting activity might endogenously affect future productivity, which is at the heart of the Melitz (2003) model and amply supported by existing evidence (e.g. Helpman 2006; Bernard *et al.* 2007, 2012). Consequently, we can decompose ω_{it} into its expectation conditional on the information I_{it-1} available to the plant in $t-1$ and a random innovation to productivity denoted by ξ_{it}:

$$\begin{aligned}\omega_{it} &= \mathrm{E}[\omega_{it}|I_{it-1}]+\xi_{it}\\ &= \mathrm{E}[\omega_{it}|\omega_{it-1}, EXP_{it-1}]+\xi_{it}\\ &= g(\omega_{it-1}, EXP_{it-1})+\xi_{it}\end{aligned} \tag{1.A.1}$$

In (1.A.1), EXP_{it-1} denotes plant i's export status in $t-1$, $g(\cdot)$ denotes some function, and ξ_{it} is assumed to be mean independent of the plant's information set I_{it-1} in $t-1$.

Given these timing assumptions, plant i's demand for intermediate inputs in t directly depends on n_{it} as well as on the other state variables k_{it}, EXP_{it}, and ω_{it}:

$$m_{it} = m_t(n_{it}, k_{it}, EXP_{it}, \omega_{it}) \tag{1.A.2}$$

Crucially, productivity ω_{it} is the only unobservable entering the demand function.[14] Provided strict monotonicity of the demand function $m_t(\cdot)$ with respect to ω_{it}, we can invert the demand function m_t to infer ω_{it} from observables as:[15]

$$\omega_{it} = m_t^{-1}(m_{it}, n_{it}, k_{it}, EXP_{it}) \tag{1.A.3}$$

Enriching our empirical model by an idiosyncratic error term ϵ_{it} that comprises unpredictable output shocks as well as potential measurement error in output and inputs gives

$$y_{it} = f(n_{it}, m_{it}, k_{it}; \boldsymbol{\beta}) + \omega_{it} + \epsilon_{it} \tag{1.A.4}$$

with $y_{it} = q_{it} + \epsilon_{it} = f_{it} + \omega_{it} + \epsilon_{it}$, where we assume ϵ_{it} to be mean independent of current and past input choices.[16] In our empirical specification, we approximate the unknown regression function $f(\cdot)$ by means of a second-order Taylor polynomial and estimate the coefficients of a translog production function (including a full set of region dummies and

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14 Adding the plant's export status EXP_{it} as an observed shifter to the plant's demand for intermediate inputs m_{it} while excluding it from the production function addresses a fundamental identification problem for the output elasticity of intermediate inputs and thus permits us to use Ackerberg *et al.*'s control function approach in the estimation of a gross output production function. To provide intuition for this problem, note that absent such a shifter the plant's demand for intermediate inputs would be $m_{it} = m_t(n_{it}, k_{it}, \omega_{it})$. In that case, unobserved productivity ω_{it} would be the only demand shifter except for the other inputs in the production function n_{it} and k_{it}. Since the output elasticity of intermediate inputs is identified from the co-movement of output and intermediate inputs holding constant the other inputs n_{it} and k_{it}, the only source of variation in the demand for intermediate inputs left would be unobserved productivity ω_{it}. Unobserved productivity ω_{it}, though, shifts both output and the demand of intermediate inputs, rendering the output elasticity of intermediate inputs unidentified in this case.

15 Levinsohn and Melitz (2006) show that strict monotonicity of $m_t(\cdot)$ with respect to ω_{it} holds as long as more productive plants do not set excessively higher price-cost mark-ups.

16 Note that the output elasticities of labor and intermediate inputs are given by $(\varepsilon_N^Q)_{it} = \partial f(\cdot)/\partial n_{it}$ and $(\varepsilon_M^Q)_{it} = \partial f(\cdot)/\partial m_{it}$, respectively, and are thus independent of productivity shocks by definition.

a linear time trend, which we will omit in the following for notational ease)

$$\begin{aligned} y_{it} = \beta_0 + \beta_n n_{it} + \beta_m m_{it} + \beta_k k_{it} + \beta_{nn} n_{it}^2 + \beta_{mm} m_{it}^2 + \beta_{kk} k_{it}^2 \\ + \beta_{nm} n_{it} m_{it} + \beta_{nk} n_{it} k_{it} + \beta_{mk} m_{it} k_{it} + \omega_{it} + \epsilon_{it}, \end{aligned} \tag{1.A.5}$$

where the regression constant β_0 measures the mean efficiency level across plants.

Plugging equation (1.A.3) into (1.A.4) results in a first-stage regression equation

$$\begin{aligned} y_{it} &= f(n_{it}, m_{it}, k_{it}; \boldsymbol{\beta}) + m_t^{-1}(m_{it}, n_{it}, k_{it}, EXP_{it}) + \epsilon_{it} \\ &= \varphi_t(n_{it}, m_{it}, k_{it}, EXP_{it}) + \epsilon_{it} \end{aligned} \tag{1.A.6}$$

that we exploit to separate the productivity shock ω_{it} from the idiosyncratic ϵ_{it}, that is to eliminate the part of output y_{it} that is driven by unanticipated shocks, measurement error, or any other random noise. This first stage uses the regression equation (1.A.6) together with the moment condition $\mathrm{E}[\epsilon_{it}|I_{it}] = 0$ to obtain an estimate $\widehat{\varphi}_{it}$ of the composite term $\varphi_t(n_{it}, m_{it}, k_{it}, EXP_{it}) = f_{it} + \omega_{it}$ or, in other words, an estimate of the plant's output net of idiosyncratic factors $q_{it} = y_{it} - \epsilon_{it}$. For a given coefficient vector $\boldsymbol{\beta}$, we can then estimate ω_{it} (up to a constant) as:

$$\begin{aligned} \widehat{\omega}_{it}(\boldsymbol{\beta}) &= \widehat{m}_t^{-1}(m_{it}, n_{it}, k_{it}, EXP_{it}) \\ &= \widehat{\varphi}_{it} - \beta_n n_{it} - \beta_m m_{it} - \beta_k k_{it} - \beta_{nn} n_{it}^2 - \beta_{mm} m_{it}^2 - \beta_{kk} k_{it}^2 \\ &\quad - \beta_{nm} n_{it} m_{it} - \beta_{nk} n_{it} k_{it} - \beta_{mk} m_{it} k_{it} \end{aligned} \tag{1.A.7}$$

For the identification of the production function coefficients $\boldsymbol{\beta}$, the second stage then uses the timing assumptions of our framework to set up the moment conditions:

$$\mathrm{E}[\xi_{it}(\boldsymbol{\beta})(n_{it-1}, m_{it-1}, k_{it}, n_{it-1}^2, m_{it-1}^2, k_{it}^2, n_{it-1}m_{it-1}, n_{it-1}k_{it}, m_{it-1}k_{it})'] = \mathbf{0} \tag{1.A.8}$$

In order to exploit these moment conditions, we have to recover the innovations to plant productivity ξ_{it}. Based on equation (1.A.7), we arrive at a consistent non-parametric estimate of the conditional expectation $\mathrm{E}[\omega_{it}|\omega_{it-1}, EXP_{it-1}]$ by taking the predicted values of a non-parametric regression of $\widehat{\omega}_{it}(\boldsymbol{\beta})$ on $\widehat{\omega}_{it-1}(\boldsymbol{\beta})$ and EXP_{it-1}. The residuals from this regression, in turn, provide us with consistent estimates of ξ_{it}. Based on these and the moment conditions (1.A.8), we then estimate $\boldsymbol{\beta}$ by standard GMM and rely on the Delta method for the standard errors (e.g. Wooldridge 2010).

1.B Results for product market setting switches

Table 1.B.1: Transition matrix for plants' product market setting

Product market setting in t	Product market setting in $t+1$	
	Marginal cost	Price mark-up
Marginal cost	78.7	21.3
Price mark-up	16.8	83.2

Notes: IAB Establishment Panel, 1999–2016, percentages of 31,795 plant-year observations, weighted using sample weights in t. Based on the estimates of the price-cost mark-up (equation 1.26), we classify observations to product market settings using equation (1.18).

Table 1.B.2: Transition matrix for the product market setting of plants covered (uncovered) by collective agreements

Product market setting in t	Product market setting in $t+1$	
	Marginal cost	Price mark-up
Marginal cost	79.5 (78.4)	20.5 (21.6)
Price mark-up	11.8 (20.6)	88.2 (79.4)

Notes: IAB Establishment Panel, 1999–2016, percentages of 31,795 plant-year observations, weighted using sample weights in t. Based on the estimates of the price-cost mark-up (equation 1.26), we classify observations to product market settings using equation (1.18).

Table 1.B.3: Transition matrix for the product market setting of plants with (without) a works council

Product market setting in t	Product market setting in $t+1$	
	Marginal cost	Price mark-up
Marginal cost	83.1 (78.4)	16.9 (21.6)
Price mark-up	9.8 (17.6)	90.2 (82.4)

Notes: IAB Establishment Panel, 1999–2016, percentages of 31,795 plant-year observations, weighted using sample weights in t. Based on the estimates of the price-cost mark-up (equation 1.26), we classify observations to product market settings using equation (1.18).

Table 1.B.4: Average marginal effects from probit regressions for a switch in the plant's product market setting

	(1)	(2)
	Mark-up to marginal-cost pricing	Marginal-cost to mark-up pricing
Collective bargaining	–0.023***	–0.002
	(0.007)	(0.009)
Works council	–0.020**	–0.021*
	(0.009)	(0.011)
Log employment	0.007**	–0.027***
	(0.004)	(0.004)
Plant age 5–9 years	0.000	–0.003
	(0.014)	(0.018)
Plant age 10–14 years	0.004	–0.003
	(0.014)	(0.019)
Plant age 15–19 years	–0.011	–0.010
	(0.014)	(0.019)
Plant age $\geqslant$ 20 years	0.001	–0.014
	(0.013)	(0.017)
Share of skilled workers	–0.046***	0.009
	(0.014)	(0.018)
Share of apprentices	0.191***	0.068
	(0.043)	(0.055)
Share of part-time workers	0.039**	–0.020
	(0.019)	(0.023)
Share of female workers	0.022	–0.018
	(0.018)	(0.022)
Exporting activity	–0.015**	0.001
	(0.007)	(0.009)
Log Likelihood	–6734.7	–4731.3
Number of observations	18,826	12,498

Notes: IAB Establishment Panel, 1999–2016. The dependent variable is a dummy variable that indicates a switch in the product market setting in the respective direction for two consecutive observations of the same plant. Reported numbers are average marginal effects with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are region dummies, two-digit sector dummies, and a dummy for a single-plant company.

1.C Measuring employer wage premia and surplus

To measure employer wage premia and plant surplus, we follow Card *et al.* (2018) and Hirsch and Mueller (2020). Our measure of wage premia builds on the two-way fixed-effects decomposition by AKM, which splits up a worker's individual wage into a worker-specific and a plant-specific component. Specifically, the log wage of worker m in period t is decomposed as:

$$\ln W_{mt} = \eta_m + \theta_{i(m,t)} + \mathbf{X}'_{mt}\boldsymbol{\beta} + \upsilon_{mt} \tag{1.C.1}$$

In (1.C.1), η_m is a permanent log wage component specific to worker m, $\theta_{i(m,t)}$ is a permanent log wage component specific to plant i employing worker m at time t, $\mathbf{X}'_{mt}\boldsymbol{\beta}$ is a time-varying log wage component stemming from time-varying worker characteristics $\mathbf{X}_{mt}$ that are rewarded equally across plants, and υ_{mt} is an idiosyncratic log wage component.

In the AKM framework, η_m reflects the worker's permanent human capital, such as education and ability, $\mathbf{X}'_{mt}\boldsymbol{\beta}$ mirrors the worker's time-varying human capital, such as experience, that affects the worker's productivity no matter where the job is held, and $\theta_{i(m,t)}$ is the percentage wage premium paid to every worker of plant i. The crucial assumption for this interpretation of the AKM decomposition to hold is that the idiosyncratic log wage component υ_{mt} is unrelated to the sequence of employers $\{i(m,t)\}_t$, for which Card *et al.* (2013) provide supporting evidence in their AKM-type wage decomposition for Germany. For a critical assessment of the validity of the AKM framework in the U.S. context, we refer to Lamadon *et al.* (2019).

To measure the plant surplus to be split between employers and workers, we follow Abowd and Lemieux (1993) and use the quasi rent per worker, with the plant's quasi rent Υ_{it} being defined as:

$$\Upsilon_{it} = P_{it}Q_{it} - J_{it}M_{it} - \overline{R}_{it}K_{it} - \overline{W}_{it}N_{it} \tag{1.C.2}$$

That is, the quasi rent Υ_{it} is revenues $P_{it}Q_{it}$ net of the value of intermediate inputs $J_{it}M_{it}$

and capital inputs $\overline{R}_{it}K_{it}$, where $\overline{R}_{it}$ denotes the competitive rental rate of capital, and net of labor inputs priced at workers' alternative wage $\overline{W}_{it}N_{it}$.[17]

When constructing workers' alternative wage $\overline{W}_{it}$ we follow Abowd and Allain (1996) and calculate workers' outside option as:

$$\ln \overline{W}_{it} = \overline{\ln W}_{st} + (\eta_{it} - \bar{\eta}_{st}) - (\bar{\theta}_{st} - \theta_{st}^{p10}) \tag{1.C.3}$$

In (1.C.3), $\overline{\ln W}_{st}$ is the average log wage (i.e. plant-level payroll per worker) in the respective first-digit sector s, η_{it} is the average AKM worker wage effect in plant i, $\bar{\eta}_{st}$ is the average AKM worker wage effect, $\bar{\theta}_{st}$ is the average AKM plant wage effect, and θ_{st}^{p10} its 10th percentile in the one-digit sector. The term $\eta_{it} - \bar{\eta}_{st}$ captures the deviation in worker quality between plant i and the sector average and thus accounts for unobserved quality differences between plants' workforces. Moreover, subtracting the spread between the average AKM plant effect and its 10th percentile $\bar{\theta}_{st} - \theta_{st}^{p10}$ in the respective one-digit sector accounts for the influence of wage premia paid by future employers on workers' current alternative wage. Specifically, we assume that risk averse workers expect to receive just a modest pay premium at the 10th percentile when switching employers. As detailed in Hirsch and Mueller (2020), this way of constructing workers' alternative wage involves quite some decisions, and out of these some may seem somewhat arbitrary. Yet, as also discussed there, in general different choices, such as using the 25th percentile of wage premia rather than the 10th percentile, make only little difference.

[17] Note that we compute the competitive rental rate of capital $\overline{R}_{it}$ from the plant's capital stock and in so doing distinguish between prices for debt and equity at the two-digit sector level because the IAB data do not contain such information at the plant level. Specifically, we use the information on the "cost of equity and capital" for Europe issued by Aswath Damodaran on 5th January 2019 at `http://pages.stern.nyu.edu/~adamodaran` and the 10-year long-term treasury bond rate for Germany to calculate the average rental rate of capital at the two-digit sector. Our average rental rate of capital is 9.9% for the years 1998–2004, 9.0% for 2005–2010, and 6.9% for 2011–2016.

Chapter 2

Worker participation in decision-making, worker sorting, and firm performance[1]

2.1 Introduction

Mandated worker participation in firm decision-making is present in many European countries for decades. Whether employee participation boosts productivity and drives up wages has been discussed intensively and is nowadays increasingly relevant against the background of the productivity slowdown and falling labor shares in national income. The German model of plant-level participation via works councils has attracted particular interest because of the strong legal rights councils enjoy there. Standard economic theory perceives works councils to be a labor market friction generating adverse economic effects (Jensen and Meckling 1979). However, several of German works councils' legal rights (discussed later in more detail) have the potential to increase plant productivity directly, e.g. via generating collective voice, reducing information asymmetries between workers and management, and fostering trust and longer-term relations between them. Existing empirical research indeed demonstrates that council plants have less employee turnover (Addison *et al.* 2001, Hirsch *et al.* 2010, Adam 2019), pay higher wages (Addison *et al.*

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2001, Hirsch and Mueller 2020), and enjoy a productivity premium (Mueller 2012, Mueller and Stegmaier 2017). Against the background of these economically desirable effects, the continued decline in works council coverage (Oberfichtner and Schnabel 2019)[2] raises concerns about productivity growth perspectives and workers share in firm surplus.

Hitherto unrelated to the worker participation literature, assortativeness of high-wage workers to high-wage employers has been documented in a number of studies.[3] As works council plants usually are high-productivity, high-wage employers, a core question is whether councils directly increase these outcomes or whether council plants employ workers of higher quality who will increase productivity (see Bender *et al.* 2018) and earn higher wages irrespectively of works council presence. The worker codetermination literature usually argues along the lines of the first scenario (e.g. Mueller 2012, Jirjahn and Smith 2018) and places little emphasis on potential self-selection of high-quality workers into works council plants. However, for most workers, going to a high-paying employer offering stable employment perspectives is attractive and, hence, assortative matching of high-quality workers into high-paying works council plants is likely.

If works councils are a driver of positive assortative matching, then high-wage, high-performance plants with works councils would coexist with low-wage, low-performance plants without councils. This would not only imply estimating spurious productivity and wage gains from codetermination. It would also suggest that the legal mandate for councils contributes both to between-plant wage inequality (Card *et al.* 2013, Hirsch and Mueller 2020) and productivity dispersion across plants (Syverson 2011).

To analyze whether sorting explains the productivity and wage effects of works

[2] An important question is why works council incidence declines despite these positive effects. Freeman and Lazear (1995) argue that employers fight against productivity increasing councils as long as the latter deteriorate profits. What is more, as soon as employer utility also depends on managerial prerogatives, employers may oppose even profit-increasing councils. Mueller and Stegmaier (2020) reason that employer associations might oppose productivity improving works councils as the latter have non-positive effects for many small firms forming the majority in employer associations.

[3] This includes e.g. Andrews *et al.* (2012) for Germany, Bonhomme *et al.* (2019) using Swedish data, and Lopes (2018) for Brazil. Studies applying two-way fixed effects models of wages as pioneered in Abowd *et al.* (1999) often show very small or even negative assortative matching, e.g. Abowd *et al.* (1999) for France and the US. However, the procedure of Abowd *et al.* (1999) may underestimate positive assortative matching due to 'limited mobility bias' (see Andrews *et al.* 2008). Card *et al.* (2013) document positive assortative matching for Germany even when using the method of Abowd *et al.* (1999).

councils, we attempt to improve on prior research by utilizing a summary measure of observable and unobservable general human capital components of workers. Specifically, we use worker fixed effects from a wage decomposition as pioneered by Abowd *et al.* (1999, henceforth AKM) and implemented by Card *et al.* (2013) for Germany. In this model, higher worker effects are rewarded higher across all employers, which justifies labeling individuals with high AKM worker effects as high-quality workers. Importantly, AKM worker effects capture all human capital components that are invariant in the time span under consideration and therefore include observable human capital variables like education or initial age but also unobservable components like 'ability'.[4] Our first contribution will be to present evidence on the magnitude and the dynamics of sorting by works council existence.

Previous studies on productivity and wage effects of works councils typically try to control for worker quality by means of (plant) observables, e.g. by including the share of skilled workers (Mueller 2012, Jirjahn and Mueller 2014). To the extent that these controls do not fully capture unobserved worker quality differences, previous studies may suffer from an omitted variable bias of unknown magnitude and our ability to control for unobservables is a potentially important contribution to this literature. We will also test whether there is complementarity in labor productivity between worker participation and workforce quality, which is informative about whether such sorting may improve allocative efficiency.

Besides testing whether positive effects of works councils on plant performance and wages are driven by sorting, we also consider profit effects to see whether the net effect of codetermination on productivity and wages benefits employers. In doing so, we examine to what extent the surplus generated by works councils is shared with workers and we therefore present evidence on how worker participation in decision-making shapes the labor share at the plant level. To overcome any biases that may stem from unobserved plant heterogeneity, we apply an event study framework and analyze works council introductions in a 'within-plant' approach and provide first event study results for wage

[4] A detailed discussion of AKM worker effects will be provided in section 2.3.

and profit effects of councils.[5] We argue that the dynamics before and after council introduction provide additional insights regarding a causal interpretation of our results.

We will find that council plants indeed employ workers of higher quality even if a rich set of observable plant characteristics is taken into account. Though some quality differences exist already before the introduction of a works council, they widen as the council matures. We further find that the share of high-quality workers strongly increases plants' labor productivity but that the OLS estimate of the works council effect declines only moderately by one-fifth if AKM worker effects are controlled for. In fixed-effects event study regressions, the council effect is unchanged when AKM worker effects are controlled for. This is good news for the validity of previous studies as it implies that ignoring labor sorting, if at all, biased previous estimates of labor productivity effects of councils moderately upwards. We also find that works council plants pay higher wages, though some increase in wages is already present before the council's introduction. We show that the surplus originating from the higher labor productivity of plants with a works council is shared by employers and workers and find positive profitability effects both in our OLS and fixed effects frameworks. What is more, the productivity premium of high-quality workers is greater when a works council is present, which suggests a complementarity between worker participation and worker quality. In combination, our fixed effects event study results show that plants introducing a works council as compared to non-council plants experience a turbulent time before introduction with worker churning, stronger wage growth and a productivity decline that sharply reduce profits prior to council introduction. After council introduction, wage growth flattens and productivity growth sets in, which allows council plants to sustain long-run profitability within a high-wage, high-productivity strategy.

Our paper is similar in spirit to Bender *et al.* (2018) who focus on the role of management practices and worker sorting on firm productivity, rather than formal employee participation. The main difference to Bender *et al.* (2018) is that we show how codetermination induces plants to employ better workers, that is, according to Bender

[5] Mueller and Stegmaier (2017) report productivity effects in a similar setting.

et al. (2018), associated with the adoption of superior management practices. In contrast to Bender *et al.* (2018) we utilize the panel structure of our data and show that quality upgrading indeed follows council introduction. The main take away will therefore be that an adequately designed scheme for worker participation in decision making can shift plants into an equilibrium with high wages and high productivity.

2.2 Institutional setting, theory and some literature

2.2.1 Regulatory framework and worker sorting

The German system of industrial relations rests on two pillars, i.e. plant-level codetermination via works councils and sectoral collective wage bargaining between unions and employer associations.[6] The Works Constitution Act (Betriebsverfassungsgesetz) requires works councils to act in the interest of workers and the plant and in a spirit of mutual trust. The law further codifies the rules for council elections and the rights elected councils have. Workers of plants with at least five permanent employees have the right to establish a council but there is no automatism to do so. In fact, as of 2015 only 42 percent of workers in West Germany, which will be the focus of our analysis, worked in the 9 percent of eligible plants that have a works council (Ellguth and Kohaut 2016).[7]

The Works Constitution Act grants councils several information and consultation rights and additionally defines topics where councils are able to block decisions (veto rights) or have the right to codetermine 'social matters'. Information rights, for instance, include the right to get access to information on the plant's economic and financial situation. These rights put councils in the position to verify management provided

[6] For excellent theoretical discussions on non-union worker representation and the German experience we refer to Addison (2009) and Jirjahn and Smith (2018).

[7] Why only a small and declining share of eligible plants has a council (Oberfichtner and Schnabel 2019) is not fully understood. Employers are prohibited to interfere with works council elections and even have to bear the costs for running the election. Once elected, councilors enjoy very strong employment protection. Because of this, and because time spent on work as a works councilor counts as regular working time, the nonexistence of councils in many eligible plants points to additional costs potential councilors face. This cost may, for instance, include the costs of positioning oneself as a works councilor while many employers have reservations against codetermination (Mueller and Stegmaier 2020) as well as the costs of actively organizing a joint position of workers, representing their interests, and being responsible for the negotiation outcomes.

information and, thus, potentially lead to a more credible top-down communication. By reducing information asymmetries between workers and the employer, information rights may, for instance, prevent inefficient plant closure (Freeman and Lazear 1995). Works councils have to be informed and consulted if the employer plans major changes in the work environment or the production process. On the one hand, consultation induced decision delay might be costly, but on the other hand, if managed appropriately, the consultation process addresses potential fears of workers and results in a well-informed workforce being more committed to desired changes.[8]

Works councils' codetermination rights are strongest in 'social matters'. For instance, if a council formally disagrees with an individual dismissal this dismissal turns void until a labor court finally decides the matter. Firing costs thus increase for employers and this may well have implications on productivity and sorting. Increased firing costs may, on the one hand, deteriorate productivity by reducing incentives to work hard (Addison *et al.* 2001, p. 671) but, on the other hand, let both sides take a longer-term view on the employment relationship, which incentivises individual workers to care about the economic viability of their plant. Employers may react to increased firing costs by investing in screening activities when hiring new workers or introduce high-performance work practices such as performance pay (Lazear 2000), which in turn should improve their ability to identify and attract high-productivity workers. When laying off workers gets expensive, employers in codetermined plants may provide additional training measures (Stegmaier 2012) to upgrade the skills of their incumbent workers to allow the latter to compete with well trained labor market entrants (Janssen and Mohrenweiser 2018).

What is more, the standard 'collective voice' argument can be made also for workplace representation via works councils. Collective voice (Freeman 1976) as opposed to 'exit-voice' (Hirschman 1970) emphasizes that worker representation at the workplace gives dissatisfied workers a chance to anonymously express their dissatisfaction without having

[8] The link between council existence and innovative activity has been analyzed in Schnabel and Wagner (1994), Addison and Wagner (1997), and Addison *et al.* (2001). Neither of these studies found any statistically significant relationship. Interestingly, Jirjahn (2011) find a positive link with incremental product innovations but not with drastic innovations.

to fear sanctions by the employer. This may prevent these workers from quitting their jobs (or from reducing effort without quitting formally) and it provides employers with more information about worker preferences than 'exit-voice' would do.

Both the firing cost argument and the collective voice argument imply reduced worker turnover in codetermined plants. Using plant-level data, Frick (1996) finds that works council existence is related to fewer quits and, among others, Addison *et al.* (2001), Frick and Moeller (2003), Pfeifer (2011), and Grund *et al.* (2016) confirm that turnover is reduced. Whether these are indeed direct collective-voice effects or whether they are rather rent-seeking effects is analyzed by Hirsch *et al.* (2010) and Adam (2019). Utilizing employer-employee data, Hirsch *et al.* (2010) find voice effects only for a subgroup of low tenure workers. Adam (2019) resorts on plant-level data and exploits a change in the legal framework within a difference-in-differences setting and finds strong voice effects as the source for reduced turnover. To sum up, the literature almost uniformly finds reduced employee turnover and some role for collective voice in explaining it.

On top of enjoying a stable job as well as stronger legal rights in the workplace, one of the main arguments for workers to move to works council plants is that the latter pay wage premia to their workers. This is documented in Hirsch and Mueller (2020) who show that councils are associated with higher employer wage premia even conditional on plants' quasi rents and accounting for worker sorting.

Having discussed why high-quality workers match with councils firms, another channel fostering assortative matching might come from higher quality workers' incentives to establish a council to protect their quasi rents. Jirjahn (2009) argues that workers who invested into their human capital will have a strong incentive to protect their quasi rents by founding a council in firms with deteriorating economic performance. Besides documenting a higher likelihood for council adoption in poorly performing firms, Jirjahn (2009) also shows that plants with a higher fraction of skilled blue collar workers are more likely to found a council. Whereas Jirjahn and Mohrenweiser (2016) and Oberfichtner (2019) also report a higher likelihood of council introductions for plants with high skilled workers, Addison *et al* (2013) and Mohrenweiser *et al.* (2012) do not find support for this notion.

We are not aware of any study on council introductions that incorporates measures of worker quality that go beyond observable skill levels.

Collective wage bargaining between unions and employer associations forms the second pillar of industrial relations in Germany. In 2015, 59 (31) percent of workers (plants) were covered by collective agreements in West Germany (Ellguth and Kohaut 2016). The Works Constitution Act clarifies the relationship between works councils and unions by stipulating that councils are not allowed to interfere with union wage setting and are not allowed to call strikes. Although formally independent of each other, works councils and unions have close ties, e.g. providing works councilors with resources and councils recruiting new union members at the shop floor (Behrens 2009). Freeman and Lazear (1995) argue that the existence of sector-level wage bargaining should increase the productivity effect of councils because councils are then less engaged in distributional conflicts and care more about increasing the overall pie to be shared between workers and the employer.

2.2.2 Works councils and plant and worker outcomes

As the literature on the economic consequences of works councils has not systematically examined (unobserved) worker quality sorting by council status, the subsequent literature review focuses on the literature on economic consequences of German works councils in general.

Productivity

The empirical economic literature on the productivity effect of works councils started in the 1980s. While early studies had to rely on very small samples and estimated negative council effects (FitzRoy and Kraft 1987), later studies were able to utilize large scale plant-level data. As a workhorse model, these studies employed production function estimations in which a council dummy indicates the ceteris paribus productivity advantage/disadvantage of works council existence. Council coefficients from OLS estimations range from 15 percent in Wolf and Zwick (2002) and 18 percent in Mueller (2015) to 25 percent in Addison (2006) and even 30 percent in Frick and Moeller (2003). Though these studies usually control for the fraction of skilled craftsman in the workforce (and sometimes also for the share of university graduates), they were not able to control for additional human capital components like worker experience or unobserved ability. Mueller (2012, 2015) analyzes the council's productivity effect and control for the fractions of skilled workers, apprentices, and part-time workers in the workforce and for the capital stock. Mueller (2012) combines a GMM-SYS production function estimation with an endogenous switching regression and finds a productivity effect of about 7 percent in the manufacturing sector, and Mueller (2015) employs recentered influence function techniques (Firpo *et al.* 2009) and reports that the council effect is higher in less productive plants. Furthermore, Freeman and Lazear's (1995) hypothesis for a moderating effect of sector-level wage bargaining on the productivity effect of councils has received strong support in empirical work (e.g. Hübler and Jirjahn 2003, Jirjahn and Mueller 2014, Brändle 2017).

One major issue that has long been unresolved is works council endogeneity due to unobserved plant heterogeneity as a source of bias in works council productivity estimates. The main difficulty with unobserved heterogeneity is that works council status does rarely change within plants over time, which makes it hard to detect statistically significant evidence in any fixed-effects or first-difference estimation strategy. Early attempts to use fixed-effects estimators indeed yielded insignificant productivity effects (Addison *et al.* 2004).[9] However, with much more observations at hand, Mueller and Stegmaier (2017) recently showed within a fixed-effects event study approach that works councils are associated with declining productivity prior to council introduction and that productivity growth outpaced that of non-council plants after an introduction period of about five years,

[9] Hübler and Jirjahn (2003) and Mueller (2012) aim on tackling council endogeneity by using endogeneous switching regression models. Both find positive effects but, as these models either identify effects exclusively via assumptions on the joint distribution of error terms (Hübler and Jirjahn 2003) or, additionally, by an exclusion restriction that may or may not hold (Mueller 2012), the matter of self-selection can be considered as being still unresolved.

leading to a substantial productivity premium of council plants in the long run.[10] The pre-introduction decline in productivity is in line with the findings in Kraft and Lang (2008), Jirjahn (2009), and Mohrenweiser *et al.* (2012) who find that councils are introduced in plants facing adverse conditions, a finding that has repeatedly been used to argue that conventional estimates of productivity effects of works councils are, if at all, biased downwards. Although Mueller and Stegmaier (2017) do not aim on tackling employee sorting and are only able to control for the fraction of skilled workers, their fixed-effects strategy should address differences in unobserved worker quality to the extent that these differences are permanent over time. However, Mueller and Stegmaier (2017) are unable to directly examine employee sorting and its importance for the works council's productivity effect. By looking at unobserved worker quality difference, we aim on addressing this potentially important source of unobserved heterogeneity directly.

To sum up, the literature on the productivity effects of works councils finds unequivocally non-negative and, in most cases, substantial positive effects. With few exceptions though, this literature is not dealing econometrically with endogeneity issues. In particular, no study has been able to control directly for employee sorting based on unobserved worker quality differences.

Wages

The literature on works councils' impact on wages documents mainly a positive relationship. Though not entitled to negotiate wages directly, works councils can be assumed to use their extensive veto and codetermination rights to strengthen the workers' wage bargaining power. Using a sample of manufacturing firms from Lower Saxony, Addison *et al.* (2001) find 15% higher wages in works council plants. Later studies using linked employer-employee data support the positive relationship between works councils and wages (Gürtzgen 2009). At the individual level, Addison *et al.* (2010) show that workers in plants with a works council benefit from works council wage premia, a result

10 Jirjahn *et al.* (2011) report a hump-shaped link between council age and productivity in their OLS setting.

that has been reinforced by Hirsch and Mueller (2020). Recently, Dobbelaere *et al.* (2020) document that works councils are indeed positively related to worker bargaining power.

Profits

The effect on profits depends on the relative size of the positive council effects on productivity and on wages, respectively, where the former increases profits and the latter reduces it. The model of Freeman and Lazear (1995) refers directly to firm surplus and suggests an inverted U-shaped relation between profits and the degree of worker rights. Empirical literature on the effects of works councils on profits is sparse. Early studies use subjective management assessments of profits and find a negative relationship between works councils and profits supporting the view that wage increases outweigh productivity gains (Addison and Wagner 1997, Addison *et al.* 2001). Using an objective measure of profits, Mueller (2011) finds a positive relationship between profits and works councils. In line with Freeman and Lazear (1995), the profit effect in Mueller (2011) is higher when a collective wage agreements is present. Again, these studies do not fully control for (un)observed worker quality. Whether previous studies overestimate or underestimate the profit effect depends on whether any bias due to omitted worker quality is stronger in the productivity or the wage estimates, respectively. An additional contribution to this literature is our ability to analyze profitability effects within a fixed-effects event study framework.

2.3 Data and empirical strategy

2.3.1 Data

We use the Linked Employer-Employee Data (LIAB cross-sectional model) of the Institute for Employment Research (IAB), which links plant-level survey information from the IAB Establishment Panel to administrative worker-level data of all workers who are subject to social security contributions and employed at a survey plant at the 30th of June

(Heining *et al.* 2014, Schmidtlein *et al.* 2019). The IAB Establishment Panel covers yearly information from 1993 (1996 for East Germany) onwards. It is a representative survey of German plants with at least one employee subject to social security contributions (Ellguth *et al.* 2014). Since 2001, it covers between 15,000 and 16,000 plants per year and contains information on works council existence, revenue, employment, capital stock[11], intermediate inputs and other plant characteristics. The administrative worker-level data provide demographic information and details about wages, education, and occupation and allows us to merge our measure of worker quality, the AKM worker effects, to our data. AKM effects are made available by the IAB to external researchers (Bellmann *et al.* 2020).

To capture unobserved worker quality, we rely on the AKM model estimated by Card *et al.* (2013) and updated by Bellmann *et al.* (2020) and generate an aggregated measure of individual worker characteristics at the plant level. Worker fixed effects come from the following wage model:

$$\log(wage_{it}) = \alpha_i + \Psi_{J(i,t)} + \mathbf{x'_{it}}\boldsymbol{\beta} + \epsilon_{it} \tag{2.1}$$

where the logarithm of the wage of worker i is the sum of a time-invariant worker effect (α_i), a time-invariant plant effect ($\Psi_{J(i,t)}$) for the plant worker i is employed at time t, plus time-varying worker characteristics ($\mathbf{x'_{it}}\boldsymbol{\beta}$)[12] affecting workers' wages equally at all plants, and a residual pay component ϵ_{it}, which is by assumption independent of the right-hand-side variables.[13]

The *levels* of AKM effects as originally estimated by Bellmann *et al.* (2020) can only be interpreted *within* the time intervals they used for estimation.[14] To obtain a time-

[11] The capital stock is not directly observed in the plant panel and is computed using information on investments with the use of the modified perpetual inventory approach by Mueller (2008, 2010, 2017).

[12] The time varying person characteristics ($\mathbf{x'_{it}}\boldsymbol{\beta}$) include an unrestricted set of year dummies as well as quadratic and cubic terms in age fully interacted with educational attainment (Bellmann *et al.* 2020, p. 7).

[13] Card *et al.* (2013) discuss exogeneity assumptions in detail and provide suggestive evidence for them being fulfilled. They further show that a richer version of model (2.1) including a worker-employer match effect increases the statistical fit of the German data only slightly.

[14] The time intervals Bellmann *et al.* (2020) use to estimate model (2.1) are 1985–1992, 1993–1999, 1998–2004, 2003–2010, 2010–2017.

consistent measure of plant-level worker effects, we first demean the worker effect for each year within those time intervals. In a second step, we generate year-specific means of the demeaned worker effects at the plant level.[15] The average worker effect within a certain plant and a certain time interval is fixed unless worker composition changes.

For our analysis, the worker effect is key as it captures time invariant worker characteristics that are rewarded equally among employers. These are, for example, observable characteristics as education and initial age as well as inherently unobservable wage and productivity components as problem-solving skills or ability. Including worker and plant fixed effects at the same time ensures that what is deemed to be a worker-specific effect is not obscured by employer-wide pay policies. This is the main advantage of using an AKM setting compared to just using worker fixed-effects from a simple one-way fixed effects model of wages where estimates of worker effects mix up both worker and employer pay components.

We discard the survey years 1993–1997 as information on works councils and other covariates are missing or incomplete for those years. We lose the year 2017 as survey information on revenue and intermediate inputs asked in year t always refers to year $t-1$. Thus, we cover the years 1998 to 2016 in our sample. We drop East German plants to exclude the influence of the dramatic structural changes after the German reunification in the 1990s and of different conventions of industrial relations before 1990 (Behrens 2009) that might be persistent. We only include plants from the service and manufacturing sector. We omit plants that are non-profit organizations or belong to public administration, financial services, insurance, or the real estate industry as for those industries measures of sales (financial services and insurances) or capital stock (real estate) are ill-defined. We exclude plant-year observations with less than five permanent employees because in those plants workers are legally not entitled to introduce a works council. We further exclude plants in which the works council is dissolved, which includes plants with

[15] AKM effects are only available for 20-60 years old full time workers liable to social security (Card *et al.* 2013). Workers who have an employment status other than "employees liable to social security without special characteristics" or "trainees without special characteristics" are also excluded (Bellmann *et al.* 2020).

multiple switches in works council existence. Thus, we focus on persistent works council introductions.[16] Within 1-digit sectors and four year periods, we truncate the top and bottom 1% of the value added per worker and capital stock per worker distributions.

To determine the year of a works council introduction and the leads and lags for the difference-in-differences event study setting (see section 2.3.2), we use the panel structure of our data and determine the year of council introduction as the year of the first occurrence of a works council. When we can't observe works council foundation in our panel, we use direct information on works council age surveyed in the years 2012, 2014, 2016.

Finally, we construct two unbalanced panels[17], i.e. an OLS sample and an event study sample. The event study sample includes all non-council plants and those council plants for which the year of works council introduction falls into our observation window (141,098 plant-year-cohort observations constructed from 15,700 plant-year observations). The construction of the event study sample is explained along with the event study estimation in the next section. As the OLS analysis does not rely on information on council age, it makes use of all works council and all non-works council plants that meet the sample selection criteria explained above (22,576 plant-year observations). The descriptive results are presented in section 2.4.1.

2.3.2 Empirical strategy

In a first step, we perform OLS estimations of the model

$$y_{jt} = \beta_0 + \beta_1 woco_{jt} + \beta_2 \overline{\alpha}_{jt} + \beta_3 kn_{jt} + \beta_4 l_{jt} + controls_{jt} + u_{jt}, \tag{2.2}$$

where y_{jt} is either the log of value added per worker, the log of the wage bill per worker, or the quasi rent per worker (our profit measure) of plant j at time t. More specifically, in defining the profit measure we follow Mueller (2011) and use a per worker measure of value

[16] We observe in our final sample 63 works council dissolutions. Including them does not change our results.

[17] We decide for unbalanced panels to use as many observations as possible.

added net of wage costs, the latter including employers' social security contributions. As we control for (the log of) capital per worker kn_{jt}, ceteris paribus differences in the quasi rent per worker reflect differences in the rent going to employers.[18] In equation (2.2), $woco_{jt}$ is a dummy indicating the presence of a works council, $\overline{a}_{jt}$ is the standardized mean of worker quality at the plant level as described in the previous section, l_{jt} is a set of seven dummies[19] flexibly capturing plant size, and $controls_{jt}$ include a collective wage bargaining dummy, the share of qualified workers, part-time workers, apprentices and women among all employees at the plant, the churning rate, plus dummies for export, single-plant status, and the technical sophistication of the equipment. We further include dummies for 2-digit industries, federal states, and years. When we analyze worker sorting instead of firm performance, we use equation (2.2), but use $\overline{\alpha}_{jt}$ as the dependent variable and omit this variable on the right-hand side.[20]

The coefficient of interest β_1 is the outcome difference between works council and non-council plants, holding all other factors fixed. To deal with unobserved plant heterogeneity such as management quality, we apply a second estimation strategy and include plant fixed effects. Since we are not only interested in the pooled works council effect, but seek to gain insights in the dynamics before and after the council introduction, we follow Mueller and Stegmaier (2017) and estimate the fixed-effects strategy within a difference-in-differences event study setting.

In a standard difference-in-differences setting, the considered event happens at the same point in time for all treated units. In our case, however, works council introductions are observed for almost all time periods analyzed. We therefore apply a setting in relative time that reorganizes the data such that all events happen at the same point in relative time (see e.g. Hijzen *et al.* 2010). We define yearly event cohorts where the treatment group of a particular cohort consists of plants that have no works council in the previous

18 Strictly speaking we additionally need to assume that, conditional on covariates, employers pay similar interest rates for capital. Assuming well-functioning financial markets we believe this to be a sensible assumption.

19 We construct a dummy for each of the following plant size ranges: 5–19, 20–39, 40–79, 80–149, 150–299, 300–499, 500–999, ≥ 1000.

20 Table 3.A.1 in 3.A gives a detailed explanation the variables.

observed years but have one in the event year. The control group of a particular cohort consists of all plants that neither introduce nor have a works council in that or previous years. For example, the year 2006 event cohort compares plants introducing a council in that year to plants that neither had a council in one of the previous years nor introduce one in the current year. For this event cohort, the relative time indicator is set to zero in the year 2006.

Relative time indicators include leads and lags so that we can trace the evolution of the treatment effect over (relative) time. We construct 19 introduction cohorts for the years 1998–2016 and exclude the cohort of 1999, because we do not identify a works council introduction in that year. The cohorts are merged to one event study sample, and each observation is indexed by relative time (τ), a plant identifier (j), and a cohort identifier (c). The model to be estimated is

$$y_{jc\tau} = \sum_{k=-L}^{K} \beta_k (D_{jc} \times time_{c\tau}^{k}) + \sum_{k=-L}^{K} \gamma_{1,k} time_{c\tau}^{k} + \gamma_2 control_{jc\tau} + \mu_{jc} + \varepsilon_{jc\tau}, \quad (2.3)$$

where $y_{jc\tau}$ are the same outcomes as defined above for the OLS estimations. The plant-cohort fixed effect μ_{jc} ensures that we only compare the within-plant variation of the control and treatment groups within one, but not across cohorts. Each dummy variable $time_{c\tau}^{k}$ equals one in the observed relative time period and captures time effects. The plant-cohort specific dummy D_{jc} equals one if a plant introduces a works council in a specific cohort and equals zero otherwise. The interaction of dummy D_{jc} and the relative time dummies captures the evolution of works council plants over time relative to non-council plants and β_k thereby identify the council effect along the entire set of time dummies. We omit the relative time dummy capturing the years shortly before council introduction to define the pre-introduction period as the base category. Hence, the coefficients of interest β_k measure the evolution in the outcome of the treated plants relative to the pre-introduction period purged from cohort-specific time trends identified via control group plants. The vector $control_{jc\tau}$ captures the same control variables as in model (2.2).

While being a step towards a causal interpretation of the works council coefficient, we would like to point out that our event study setting is not a strictly causal setting. First of all, we will not make attempts to balance pre-treatment trends between control and treatment group but rather take differing trends into consideration when interpreting council effects. Second, anything that changes with council introduction but is unrelated to it, might yield biased council coefficients. As we don't have a randomized experiment, we can't rule out the existence of such parallel events. That being said, we argue that any employer reaction to council adoption should not be distinguished from the council effect as it is part of the answer to the question of what happens if workers adopt a council.

2.4 Results

2.4.1 Descriptive findings

The descriptive statistics in Table 2.1 reinforce standard results in terms of showing that plants with a works council have higher labor productivity, pay higher wages, earn higher quasi rents, employ more workers, have lower churning rates, have a higher capital intensity, and are more likely to be covered by a collective wage agreement than plants without. A new result is that works council plants employ workers whose AKM worker effects are almost one half of a standard deviation higher compared to non-council plants. Interestingly, the share of skilled workers, which is a definition capturing a relatively broad skill set (see Appendix A), is very similar across both groups of plants indicating that AKM worker effects indeed convey different information and distinguishes better between workers of different quality. Together with the results on productivity, profits and wages, the descriptive analysis therefore points to strong assortative matching of high-wage workers to high-wage, high-productivity plants.

Columns (3) and (4) summarize the outcomes of plants before the introduction of a works council. Compared to non-council plants, the 67 plants introducing a council have higher labor productivity, wages and worker quality even before the introduction.[21] Their

[21] More than three years before council introduction there are 106 plant-year observations, in the $[-3,-1]$–relative-time-interval 186, in the $[0,2]$–interval 183, in the $[3,5]$–interval 136, in the $[6,8]$–

outcomes are, however, worse than that of council plants, which indicates that works council introduction may further improve outcomes. Hence, our results show descriptively that plants with high performance, somewhat higher worker churning, and high worker quality seem to be more likely to introduce a council and that performance and wages increase after council introduction whereas churning decreases. To scrutinize these results, we later show the dynamics before and after the council introduction in a multivariate fixed-effects event study setting.

2.4.2 Worker sorting

When analyzing worker sorting, the standardized plant-level average of the AKM worker effect ($\overline{\alpha_{jt}}$) becomes the dependent variable in model (2.2). The corresponding OLS regression results are displayed in Table 2.2. Works council existence enters positively and significantly in all specifications. Omitting the share of skilled workers (column 1) yields a works council coefficient of 0.194, implying that worker quality is higher by nearly one-fifth of a standard deviation. Controlling for the fraction of skilled workers reduces the coefficient to 0.157 (column 2). Remember that the AKM worker effect captures also observable human capital components embodied in age and formal education (see section 2.3.1). Including in the regression both average worker age and the share of workers having

an university degree reduces the council coefficient to 0.115 (column 4). Hence, including both observable human capital components components does only account for a modest fraction of the worker quality effect.

The event study results for the average AKM worker effect at the plant level are depicted in Figure 2.1. It shows the coefficients of the relative time dummies[22] with their 90% confidence intervals, where the three years before introduction (-3 to -1) serve as base category. In our baseline specification (Figure 2.1A) worker quality rises by 0.159 standard deviations from the pre-introduction period to a works council age of more than eight years. The insignificant pre-event trend support the conclusion that council introduction

interval 126, and nine years after council introduction we have 155 plant-year observations.

[22] The six relative time intervals are $time^k_{c\tau}$ ($k \in \{[-\infty,-4];[-3,-1];[0,2];[3,5];[6,8];[9,\infty]\}$)

Table 2.1: Summary statistics

	Works Council	No works council	Years b. council intro. less than 3	at least 3
Variable	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Log(labor productivity)	11.280 (0.606)	10.905 (0.663)	11.105 (0.685)	11.351 (0.721)
Log(wage bill per worker)	10.386 (0.397)	10.006 (0.527)	10.244 (0.473)	10.218 (0.528)
Log(profit per worker)	10.281 (1.232)	9.894 (1.275)	10.009 (1.346)	10.608 (1.164)
Log(employment)	5.368 (1.201)	3.141 (1.040)	4.225 (1.088)	3.981 (0.984)
Log(capital intensity)	11.042 (1.234)	10.509 (1.246)	10.488 (1.875)	10.620 (1.202)
Collective bargaining	0.783 (0.412)	0.341 (0.474)	0.414 (0.494)	0.340 (0.476)
Worker quality	0.292 (0.700)	-0.144 (1.090)	0.284 (0.884)	0.262 (0.923)
Skilled employees as share of all employees	0.674 (0.250)	0.643 (0.252)	0.689 (0.279)	0.670 (0.273)
Part-time employees as share of all employees	0.110 (0.162)	0.209 (0.205)	0.146 (0.206)	0.146 (0.218)
Apprentices as share of all employees	0.041 (0.039)	0.051 (0.073)	0.043 (0.050)	0.044 (0.051)
Female employees share of all employees	0.275 (0.215)	0.374 (0.262)	0.317 (0.259)	0.332 (0.243)
Churning rate	0.041 (0.066)	0.059 (0.267)	0.068 (0.134)	0.064 (0.118)
Exporter	0.693 (0.461)	0.363 (0.481)	0.468 (0.500)	0.651 (0.479)
Single plant	0.509 (0.500)	0.835 (0.371)	0.516 (0.501)	0.651 (0.479)
Technical state of machinery				
excellent	0.176 (0.381)	0.220 (0.414)	0.253 (0.436)	0.236 (0.427)
good	0.514 (0.499)	0.503 (0.500)	0.414 (0.494)	0.575 (0.497)
fair	0.275 (0.447)	0.256 (0.436)	0.306 (0.462)	-†
poor	0.034 (0.181)	0.022 (0.145)	-†	-†
Average worker age	41.961 (3.489)	41.132 (5.501)	39.865 (3.992)	39.308 (3.978)
University degree share of all employees	0.082 (0.122)	0.063 (0.135)	0.095 (0.177)	0.103 (0.210)
N	7,467	15,109	186	106

Notes: LIAB cross-sectional model, 1998–2016, West Germany. Summary of 22,576 plant-year observations. Worker quality is the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one.
-† The values are not shown due to reasons of data protection.

increases worker quality as opposed to a narrative where councils are introduced in plants that would have upgraded worker quality anyway. Our results remain unchanged when we omit the control variables (Figure 2.1A) so that we conclude that our results are not affected by any issue that might arise from controlling for post-treatment realizations of the control variables (sometimes called ‘bad control’ problem).

Upgrading along time-invariant observable worker characteristics is one possible explanation for worker quality improvements after council introduction. We add

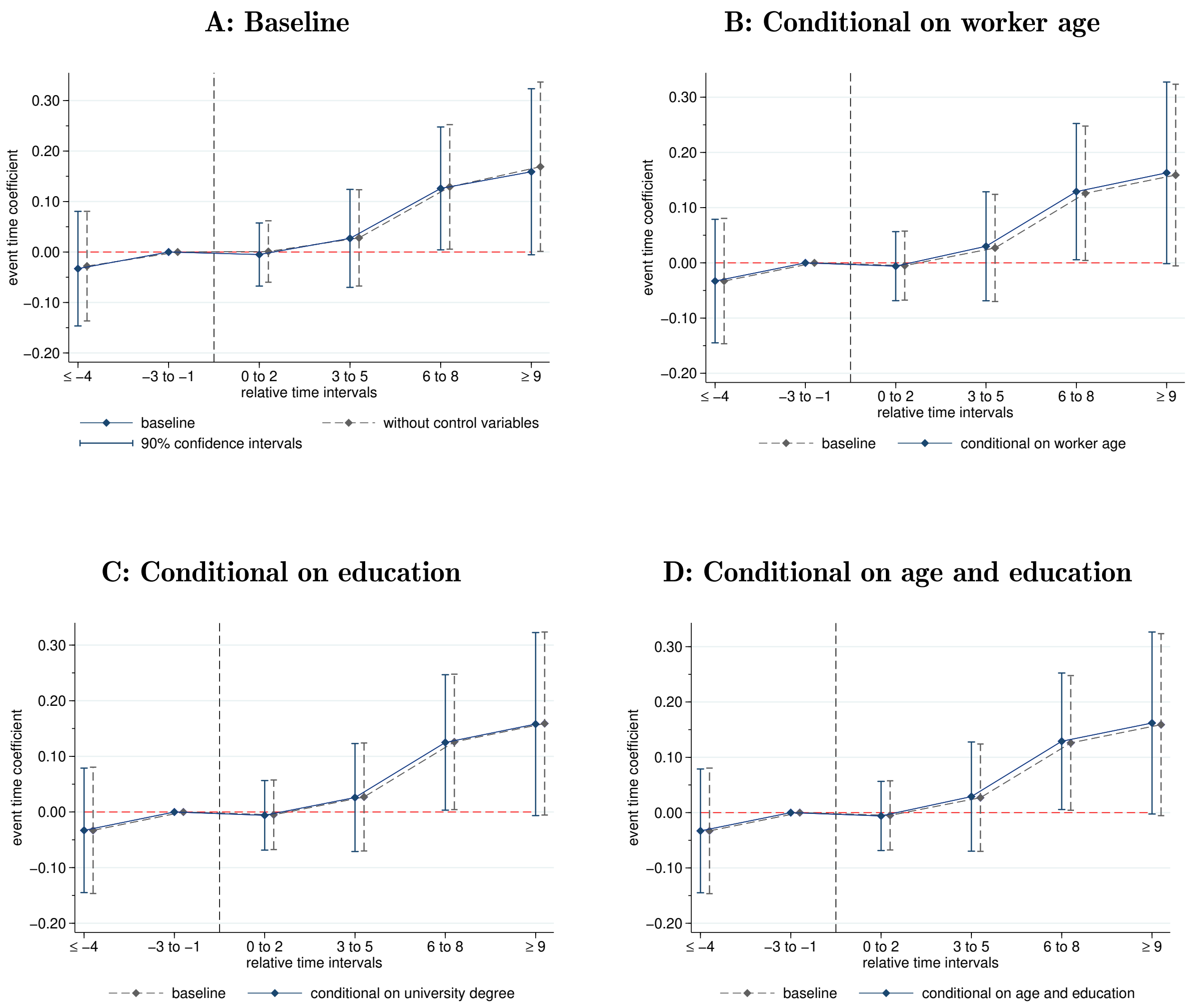
Table 2.2: Worker quality, OLS regressions

	(1)	(2)	(3)	(4)
Works council	0.194***	0.157***	0.142***	0.115***
	(0.028)	(0.026)	(0.032)	(0.025)
Skilled employees		0.857***	0.858***	0.700***
		(0.043)	(0.043)	(0.042)
Collective bargaining	0.039*	0.011	0.004	0.007
	(0.023)	(0.022)	(0.027)	(0.021)
Works council ×			0.026	
collective bargaining			(0.039)	
Log(capital intensity)	0.038***	0.028***	0.028***	0.028***
	(0.010)	(0.009)	(0.009)	(0.009)
Exporter	0.130***	0.135***	0.134***	0.090***
	(0.027)	(0.025)	(0.025)	(0.024)
Single plant	-0.166***	-0.133***	-0.133***	-0.109***
	(0.023)	(0.022)	(0.022)	(0.021)
Technical state = good	-0.043*	-0.026	-0.026	-0.036*
	(0.022)	(0.022)	(0.022)	(0.021)
Technical state = fair	-0.124***	-0.079***	-0.079***	-0.094***
	(0.027)	(0.026)	(0.026)	(0.025)
Technical state = poor	-0.155***	-0.095**	-0.096**	-0.122***
	(0.047)	(0.045)	(0.045)	(0.045)
Part-time employees	-0.224***	-0.016	-0.016	-0.040
	(0.086)	(0.083)	(0.083)	(0.082)
Apprentices	-0.448**	-0.200	-0.195	0.448**
	(0.190)	(0.182)	(0.181)	(0.192)
Female employees	-0.682***	-0.559***	-0.558***	-0.558***
	(0.071)	(0.066)	(0.066)	(0.063)
Churning rate	-0.087	-0.056*	-0.056*	-0.041*
	(0.053)	(0.032)	(0.032)	(0.024)
Average worker age				0.015***
				(0.003)
University degree				1.834***
				(0.105)
Constant	0.038	-0.655	-0.651	-1.034**
	(0.635)	(0.615)	(0.615)	(0.524)
R^2	0.304	0.339	0.339	0.377
N	22,576	22,576	22,576	22,576

Notes: LIAB cross-sectional model, 1998–2016, West Germany, OLS sample. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from OLS regressions with standard errors clustered at the plant level in parentheses. Further covariates included in all specifications are 7 plant size dummies, 8 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

average worker age to the event study, but the post-event coefficients stay unchanged (Figure 2.1B). The council coefficients do not change a lot either, when we control for

Figure 2.1: Worker quality, event study



Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (141,098 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of worker quality relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker age at the plant level. For Panel C: holding constant the share of university graduates. For Panel D: holding constant age and the share of university graduates. The 90% confidence intervals are shown using standard errors clustered at the plant level.

the share of university graduates alone (Figure 2.1C) or jointly with average worker age (Figure 2.1D). We conclude that the increase in worker quality is driven by an increase

Table 2.3: Worker quality of joining, leaving, staying workers, OLS regressions

	Worker quality			Difference of worker quality	
	joining workers	leaving workers	staying workers	joining vs. leaving	staying vs. staying of $t-1$
	(1)	(2)	(3)	(4)	(5)
Works council	0.129***	0.073***	0.136***	0.058**	-0.032
	(0.025)	(0.026)	(0.038)	(0.024)	(0.027)
Skilled employees	0.498***	0.469***	0.888***	0.037	0.050
	(0.053)	(0.052)	(0.054)	(0.048)	(0.048)
Collective bargaining	0.056**	0.032	0.002	0.025	-0.032
	(0.024)	(0.022)	(0.027)	(0.023)	(0.024)
Log(capital intensity)	0.010	0.021*	0.034***	-0.012	0.001
	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)
Exporter	0.028	0.032	0.083***	-0.004	-0.018
	(0.027)	(0.024)	(0.030)	(0.025)	(0.024)
Single plant	-0.071***	-0.041*	-0.127***	-0.032	0.008
	(0.021)	(0.021)	(0.026)	(0.020)	(0.021)
Technical state = good	-0.025	-0.046*	-0.048*	0.021	-0.026
	(0.025)	(0.025)	(0.026)	(0.027)	(0.028)
Technical state = fair	-0.054*	-0.031	-0.085***	-0.024	-0.020
	(0.029)	(0.029)	(0.031)	(0.030)	(0.032)
Technical state = poor	-0.057	-0.030	-0.084	-0.029	-0.003
	(0.069)	(0.062)	(0.062)	(0.076)	(0.054)
Part-time employees	-0.505***	-0.304***	0.179	-0.212*	0.066
	(0.130)	(0.117)	(0.111)	(0.119)	(0.096)
Apprentices	0.632**	0.137	0.940***	0.510	-0.284
	(0.281)	(0.298)	(0.269)	(0.322)	(0.261)
Female employees	-0.363***	-0.484***	-0.703***	0.116	-0.042
	(0.083)	(0.083)	(0.082)	(0.081)	(0.071)
Churning rate	-0.680*	-0.708***	0.068	0.017	0.126
	(0.352)	(0.229)	(0.064)	(0.170)	(0.226)
Average worker age	0.024***	0.021***	0.008**	0.003	-0.013***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
University degree	1.301***	1.151***	2.712***	0.173	0.288**
	(0.149)	(0.145)	(0.169)	(0.129)	(0.128)
Constant	-2.205***	-1.870***	-2.041***	-0.374	0.735***
	(0.330)	(0.288)	(0.284)	(0.289)	(0.261)
R^2	0.308	0.306	0.511	0.015	0.083
N	10,317	10,317	10,317	10,317	10,317

Notes: LIAB cross-sectional model, 1998–2016, West Germany, OLS sample. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variables of column 1 to 3 are the means of the AKM worker effects (α_i) at the plant level (as described in section 2.3) of joining, leaving or staying workers, respectively, weighted with their shares in plant employment. The dependent variables of column 4 and 5 are the difference of the weighted means of joining and leaving workers and the first difference of the weighted means of staying workers, respectively. The dependent variables are standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from OLS regressions with standard errors clustered at the plant level in parentheses. Further covariates included in all specifications are 7 plant size dummies, 8 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

in unobserved components of the AKM worker effect.[23]

[23] In 3.C we present further robustness checks for selective panel attrition including only observations for which we observe the council introduction from the panel structure of our data. We find that our

It is interesting to understand whether the higher average worker quality results from worker churning or skill upgrades of incumbent workers. Using the OLS sample, we estimate model 2.2 explaining the average AKM worker effects of either joining, leaving, and staying workers, respectively. We also analyzed directly the difference in the AKM effects of joiners versus (lagged) leavers and the first difference in the AKM effects of stayers.[24] As joiners (leavers) cannot be identified for a plant's first (last) observation, sample size decreases substantially. We find that the AKM worker effects of joining, staying and leaving workers are higher in works council plants. Importantly, we also find that the difference in the AKM effects between joiners and leavers is more positive and statistically significant in works council plants (Table 2.3, column 4), whereas we find no statistically significant difference between council and non-council plants regarding the change in the AKM effects of stayers (Table 2.3, column 5). These results favor the notion that worker quality improvements in council plants are rather driven by worker churning than by quality improvements of incumbent workers.[25]

Summing up, we find that worker quality is higher in plants with a works council than in plants without, that this difference is partly already present before council introduction (see Table 2), that it increases further after the council is introduced, and that this increase is best explained by worker churning. Our event studies further show that improvements of unobserved worker quality rather than changes of workers' formal education or age drive quality improvements.

2.4.3 Productivity

Table 2.4 presents our labor productivity OLS regressions. The focus is on the effect of council existence and how worker quality shapes the effect. The first column is not

main results are unchanged.

24 We weight the averages of joining, leaving, and staying workers with their share in plant employment to account for their relative importance for the plant's average AKM effect. This is crucial as churning may not only affect the average quality within these groups of workers but also the weight with which either of the three groups enter the plant average.

25 Presumably due to the sharply reduced sample size, we found no clear evidence for either the churning channel nor for skill upgrades of incumbent workers when we apply fixed effects event study regressions.

controlling for worker quality and shows that council plants are ceteris paribus 16 percent more productive. Adding the share of skilled workers in column (2) yields a positive impact of skill on productivity and a reduction of the council coefficient from 0.160 to 0.145. With this result, we are in the same range of magnitude as other recent studies (compare e.g. Jirjahn and Mueller 2014, Mueller 2015). Including AKM worker effects (column 3) yields a strong positive impact of them on productivity and a further reduction of the council effect from 0.145 to 0.128.[26] This leads to two conclusions: first, properly controlling for worker quality reduces the council effect by about 12 percent but there is still a substantial productivity effect left and, second, AKM worker effects are strongly

related to productivity even if the percentage of skilled jobs is controlled for. [27]

Coefficients for covariates not at the center stage of our analysis show no surprises, i.e. plants that export, belong to multi-branch firms, use more capital per worker and more up-to-date equipment, and employ less apprentices and part-time workers have ceteris paribus higher labor productivity.[28] In column (4) we confirm the strongly positive interaction effect between councils and collective agreements. Column (5) shows that interacting worker quality and council status yields a significant and positive coefficient, which means that the effect of worker quality on productivity is by one third larger in council plants. This leads to the conclusion that while council plants do employ better workers as documented in Table 2.1 and Table 2.2, they are also making better use of them. Column (6) finally documents that the interaction term between works council presence and collective agreements is not shaped by controlling for AKM effects.

Figure 2.2 displays the event study estimates for labor productivity. Confirming Mueller and Stegmaier (2017), Panel A shows that plants introducing a works council experience a downturn in productivity before the introduction and increasing productivity

26 Albeit being very precisely estimated, the difference in council coefficients is not statistically significant.

27 Note that the few previous studies on the economic effects of works councils that employ plant fixed effects (e.g. Addison *et al.* 2004) implicitly also control for unobserved worker heterogeneity. This, however, comes at the cost of only being able to use within-firm variation in works council status (and in all other variables, too) and of not being able to actually pin down the effect of worker sorting and its interaction with other variables.

28 We do not control for the ownership structure in our regressions as this would reduce our sample size without changing any results.

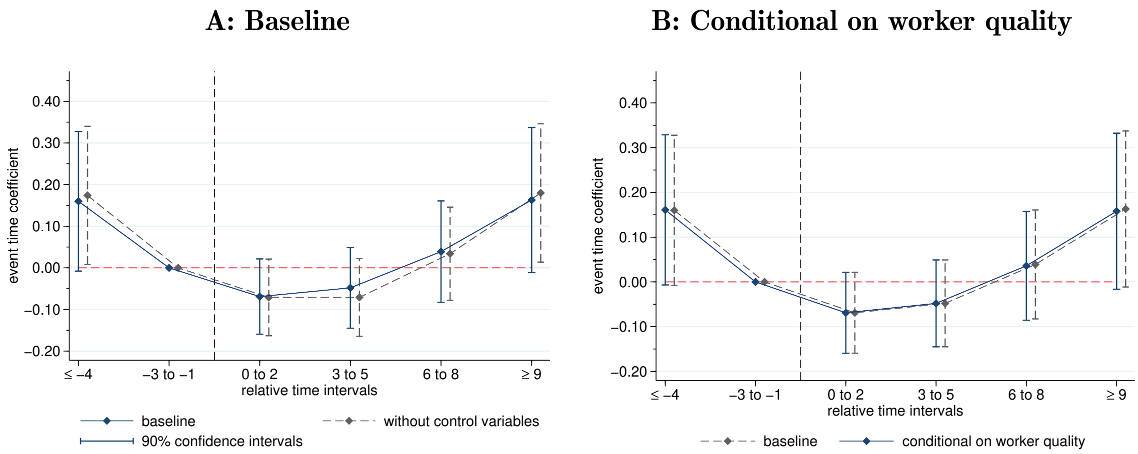
Table 2.4: Labor productivity, OLS regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Works council	0.160***	0.145***	0.128***	0.090***	0.121***	0.075***
	(0.023)	(0.023)	(0.022)	(0.027)	(0.022)	(0.027)
Skilled employees		0.367***	0.278***	0.370***	0.269***	0.281***
		(0.029)	(0.028)	(0.029)	(0.029)	(0.028)
Worker quality			0.104***		0.100***	0.104***
			(0.009)		(0.009)	(0.009)
Collective bargaining	-0.002	-0.014	-0.015	-0.039**	-0.015	-0.039**
	(0.015)	(0.014)	(0.014)	(0.017)	(0.014)	(0.017)
Works council ×				0.090***		0.088***
collective bargaining				(0.031)		(0.030)
Works council ×					0.037**	
worker quality					(0.017)	
Log(capital intensity)	0.099***	0.095***	0.092***	0.094***	0.091***	0.091***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Exporter	0.120***	0.122***	0.108***	0.121***	0.109***	0.107***
	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.015)
Single plant	-0.137***	-0.123***	-0.109***	-0.121***	-0.109***	-0.107***
	(0.016)	(0.016)	(0.015)	(0.016)	(0.015)	(0.015)
Technical state	-0.056***	-0.049***	-0.046***	-0.049***	-0.046***	-0.046***
= good	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Technical state	-0.108***	-0.089***	-0.081***	-0.090***	-0.081***	-0.082***
= fair	(0.017)	(0.017)	(0.016)	(0.017)	(0.016)	(0.016)
Technical state	-0.152***	-0.126***	-0.116***	-0.127***	-0.117***	-0.117***
= poor	(0.030)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
Part-time employees	-0.983***	-0.894***	-0.892***	-0.893***	-0.896***	-0.892***
	(0.052)	(0.049)	(0.049)	(0.049)	(0.050)	(0.049)
Apprentices	-0.912***	-0.806***	-0.785***	-0.789***	-0.794***	-0.769***
	(0.106)	(0.105)	(0.104)	(0.106)	(0.104)	(0.104)
Female employees	-0.113**	-0.060	-0.002	-0.058	0.002	0.000
	(0.045)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)
Churning rate	-0.035	-0.022	-0.016	-0.022	-0.016	-0.016
	(0.050)	(0.040)	(0.037)	(0.040)	(0.038)	(0.037)
Constant	10.036***	9.739***	9.807***	9.755***	9.814***	9.823***
	(0.370)	(0.352)	(0.299)	(0.347)	(0.301)	(0.293)
R^2	0.385	0.400	0.416	0.400	0.416	0.416
N	22,576	22,576	22,576	22,576	22,576	22,576

Notes: LIAB cross-sectional model, 1998–2016, West Germany, OLS sample. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the value added divided by the number of employees. Worker quality is the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from OLS regressions with standard errors clustered at the plant level in parentheses. Further covariates included in all specifications are 7 plant size dummies, 8 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

as the council grows older. This is in line with the findings in Jirjahn (2009), Kraft and Lang (2008) and Mohrenweiser *et al.* (2012) who show that works council introductions are more likely when the plant is under economic distress. The negative pre-trend of council adopters implies that the positive effects we measure after council introduction might even understate the causal productivity effect of works councils. When adding worker quality to the event study (Panel B), we find that the growth in productivity is not driven by the upgrade in worker quality. This is not surprising because a large portion of the worker quality advantage of council plants already existed prior to council introduction (see Table 2.1) and, thus, is captured by the fixed effect. We thus support Mueller and Stegmaier (2017) in their conclusion that the productivity increase is likely to be a genuine council effect.

Figure 2.2: Labor productivity, event study



Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (141,098 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of value added divided by the number of employees relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level.

2.4.4 Wages

Table 2.5 shows our OLS wage estimates. The coefficients of the control variables mostly have the same sign as in the productivity regressions, underlining the close link between productivity and wages. Without controlling for skill requirements and worker quality, works councils are ceteris paribus associated with 12 percent higher wages (column 1), which drops to 10 percent when the share of skilled workers is added (column 2). Our estimates are smaller than e.g. those in Addison *et al.* (2001) who reported about 15 percent higher wages. Adding AKM worker effects reduces the council wage premium further to about 8 percent (column 3). The relatively mild reduction of the council coefficient shows that the council premium is not fully explained by the council plants' better workers. It rather supports the notion that factors like the workers' bargaining power drive the council premium (Hirsch and Mueller 2020). Our results show that one standard deviation increase in AKM worker effects is associated with a wage increase of 11 percent (column 3), conditional on the share of skilled jobs.

The interaction of council existence and collective agreements is positive and significant (column 4) and adding AKM worker effects does not shape the interaction effect (column 6). The interaction between council existence and AKM worker effects is insignificant (column 5), and we therefore find no evidence for the notion that high-wage workers earn a higher wage premium relative to low-wage workers in council plants.

The results of the event studies for wages are shown in Figure 2.3. Though we find statistically insignificant wage increases after the works council introduction, the pre-event dummies point at a positive pre-trend in wages. This wage increase before the event casts doubt on the hypothesis that works councils causally trigger wage increases, at least in the short run, and suggests that introducing plants remain on their above-average wage-growth path. If we control for worker quality in the event study, the works council coefficients in Figure 2.3B barely change.

2.4.5 Profits

Table 2.6 presents our OLS estimates for the quasi rent, where we interpret regression coefficients as profit effects because we always condition on capital intensity. The coefficients of the control variables show no surprises. Confirming Mueller (2011), we report a positive link between council existence and profits across all specifications. Controlling

Table 2.5: Wages, OLS regressions

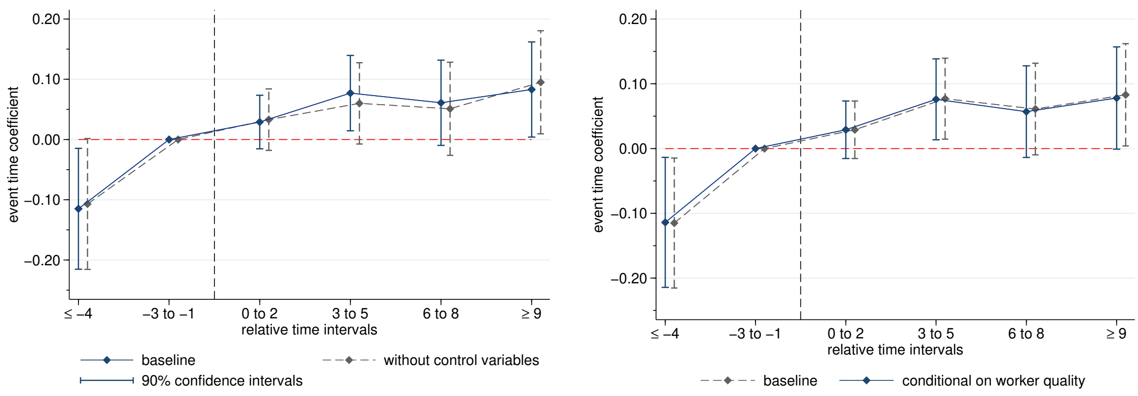
	(1)	(2)	(3)	(4)	(5)	(6)
Works council	0.119***	0.101***	0.084***	0.078***	0.085***	0.063***
	(0.012)	(0.012)	(0.011)	(0.015)	(0.011)	(0.015)
Skilled employees		0.436***	0.346***	0.437***	0.346***	0.347***
		(0.020)	(0.019)	(0.020)	(0.020)	(0.019)
Worker quality			0.105***		0.105***	0.105***
			(0.007)		(0.007)	(0.007)
Collective bargaining	0.014	0.000	-0.001	-0.011	-0.001	-0.011
	(0.009)	(0.009)	(0.009)	(0.011)	(0.009)	(0.010)
Works council ×				0.039**		0.036**
collective bargaining				(0.018)		(0.017)
Works council ×					-0.001	
worker quality					(0.010)	
Log(capital intensity)	0.059***	0.054***	0.051***	0.054***	0.051***	0.051***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Exporter	0.099***	0.101***	0.087***	0.101***	0.087***	0.087***
	(0.011)	(0.010)	(0.009)	(0.010)	(0.009)	(0.009)
Single plant	-0.082***	-0.065***	-0.051***	-0.064***	-0.051***	-0.050***
	(0.010)	(0.009)	(0.008)	(0.009)	(0.008)	(0.008)
Technical state	-0.031***	-0.022***	-0.019**	-0.022***	-0.019**	-0.019**
= good	(0.009)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Technical state	-0.044***	-0.020*	-0.012	-0.021**	-0.012	-0.013
= fair	(0.011)	(0.011)	(0.010)	(0.011)	(0.010)	(0.010)
Technical state	-0.076***	-0.045**	-0.035*	-0.046**	-0.035*	-0.036*
= poor	(0.021)	(0.020)	(0.019)	(0.020)	(0.019)	(0.019)
Part-time employees	-0.970***	-0.864***	-0.863***	-0.864***	-0.862***	-0.862***
	(0.039)	(0.035)	(0.034)	(0.034)	(0.035)	(0.034)
Apprentices	-0.946***	-0.820***	-0.799***	-0.813***	-0.799***	-0.793***
	(0.076)	(0.071)	(0.067)	(0.071)	(0.067)	(0.067)
Female employees	-0.170***	-0.107***	-0.048*	-0.106***	-0.048*	-0.047*
	(0.032)	(0.029)	(0.028)	(0.029)	(0.028)	(0.028)
Churning rate	-0.037	-0.021	-0.015	-0.021	-0.015	-0.015
	(0.042)	(0.031)	(0.028)	(0.031)	(0.028)	(0.028)
Constant	9.526***	9.173***	9.242***	9.180***	9.242***	9.249***
	(0.336)	(0.312)	(0.260)	(0.309)	(0.260)	(0.256)
R^2	0.544	0.578	0.605	0.578	0.605	0.605
N	22,576	22,576	22,576	22,576	22,576	22,576

Notes: LIAB cross-sectional model, 1998–2016, West Germany, OLS sample. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the wage bill divided by the number of employees. Worker quality is the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from OLS regressions with standard errors clustered at the plant level in parentheses. Further covariates included in all specifications are 7 plant size dummies, 8 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

for skill generally reduces the works council coefficient but the reduction is modest so that the council coefficient is still in the range of 0.140–0.168 (in the specifications without interaction terms). Hence, councils are *ceteris paribus* associated with about 15–18 percent higher profits. This is in the same order of magnitude as reported in Mueller (2011) who

Figure 2.3: Wages, event study

A: Baseline **B: Conditional on worker quality**



Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (141,098 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of the wage bill divided by the number of employees relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level.

estimated a council coefficient of 7,200 Euro and an average quasi rent of 33,300 Euro.

We also find some (statistically insignificant) confirmation of the positive interaction between works councils and collective wage agreements as theoretically suggested by Freeman and Lazear (1995) and empirically confirmed in Mueller (2011). Interestingly, AKM worker effects are themselves positively related to profitability. This suggests that employers capture parts of the additional productivity high-wage workers contribute to the company, which provides a rationale for employers to hire such workers although they earn higher wages. As in the productivity regressions, we find a positive interaction effect between councils and worker quality. Hence, employing high worker quality pays off even more when a works council is present.

The event study dynamics for profits in Figure 2.4 are similar to the productivity estimates but its U-shape is more pronounced. Controlling for worker quality does not

Table 2.6: Profits, OLS regressions

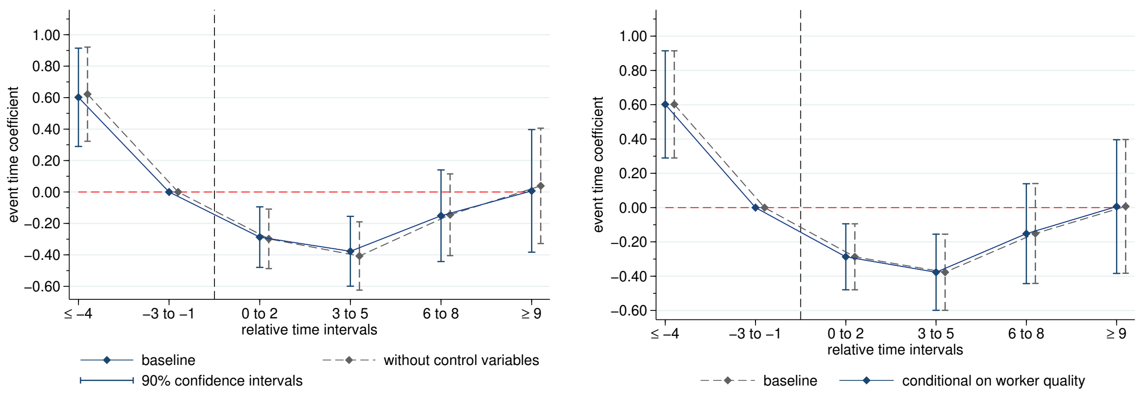
	(1)	(2)	(3)	(4)	(5)	(6)
Works council	0.168***	0.155***	0.140***	0.108**	0.127***	0.095*
	(0.044)	(0.044)	(0.044)	(0.053)	(0.045)	(0.053)
Skilled employees		0.303***	0.222***	0.306***	0.206***	0.225***
		(0.054)	(0.054)	(0.054)	(0.055)	(0.054)
Worker quality			0.094***		0.086***	0.094***
			(0.016)		(0.016)	(0.016)
Collective bargaining	-0.014	-0.024	-0.025	-0.046	-0.026	-0.046
	(0.028)	(0.028)	(0.028)	(0.033)	(0.028)	(0.033)
Works council ×				0.078		0.075
collective bargaining				(0.059)		(0.059)
Works council ×					0.068*	
worker quality					(0.036)	
Log(capital intensity)	0.161***	0.158***	0.155***	0.158***	0.154***	0.155***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Exporter	0.160***	0.162***	0.149***	0.161***	0.151***	0.148***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Single plant	-0.181***	-0.170***	-0.157***	-0.168***	-0.157***	-0.155***
	(0.032)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Technical state	-0.091***	-0.085***	-0.083***	-0.085***	-0.084***	-0.083***
= good	(0.027)	(0.026)	(0.027)	(0.026)	(0.027)	(0.027)
Technical state	-0.197***	-0.181***	-0.173***	-0.181***	-0.174***	-0.174***
= fair	(0.034)	(0.034)	(0.034)	(0.034)	(0.033)	(0.034)
Technical state	-0.242***	-0.221***	-0.212***	-0.222***	-0.213***	-0.213***
= poor	(0.064)	(0.063)	(0.063)	(0.063)	(0.063)	(0.063)
Part-time employees	-1.075***	-1.002***	-1.000***	-1.001***	-1.006***	-1.000***
	(0.088)	(0.087)	(0.087)	(0.087)	(0.088)	(0.087)
Apprentices	-0.835***	-0.747***	-0.728***	-0.732***	-0.745***	-0.714***
	(0.197)	(0.198)	(0.198)	(0.198)	(0.198)	(0.198)
Female employees	-0.046	-0.002	0.051	-0.001	0.057	0.052
	(0.078)	(0.079)	(0.079)	(0.079)	(0.079)	(0.079)
Churning rate	-0.021	-0.010	-0.005	-0.010	-0.005	-0.005
	(0.064)	(0.057)	(0.054)	(0.057)	(0.055)	(0.054)
Constant	8.644***	8.398***	8.460***	8.412***	8.473***	8.474***
	(0.422)	(0.412)	(0.365)	(0.409)	(0.368)	(0.361)
R^2	0.193	0.195	0.199	0.195	0.199	0.199
N	22,576	22,576	22,576	22,576	22,576	22,576

Notes: LIAB cross-sectional model, 1998–2016, West Germany, OLS sample. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the value added minus labor costs divided by the number of employees. Worker quality is the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from OLS regressions with standard errors clustered at the plant level in parentheses. Further covariates included in all specifications are 7 plant size dummies, 8 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

change the results. Introducing plants experience a drop in profits before and an increase after council introduction. The severe drop in profits before the introduction can be explained both by the increase in wages and by the decrease in productivity we reported earlier.

Figure 2.4: Profits, event study

A: Baseline **B: Conditional on worker quality**



Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (141,098 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of the value added minus labor costs divided by the number of employees relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level.

After council introduction, profits rise since productivity increases and wage growth flattens. While the post-event coefficients are insignificant they are in line with the OLS results and imply that the positive association between works councils and wages is outpaced by the positive effect on productivity, which ultimately increases profits. The negative pre-trend and the profit increase after introduction imply that the profit effects could be underestimated, in particular if one takes into account that wages might partially just follow their above average pre-trend.

In combination our results show that plants experience turbulent times before council introduction with strong wage growth and a substantial productivity decline that sharply reduces pre-introduction profits. After council introduction, wage growth flattens and productivity growth sets in, which allows council plants to sustain long-run profitability within a high-wage high-productivity strategy.

2.5 Conclusions

In this study, we take stock of the mounting literature on the economic effects of works councils and this literature's overall positive assessment of worker participation. We asked whether high-quality workers sort into council plants, whether the positive assessment remains when such sorting is taken into account, and whether there is a complementarity between worker participation and worker quality visible as excess productivity premia. We documented substantial sorting in the sense that high-quality workers sort into works council plants. Advantages in worker quality exist before the introduction of a works council and increase further after its introduction, which is however only modestly muting the positive link between works councils and labor productivity, wages, and profits, respectively in OLS regressions. In the fixed-effects event study setting we found productivity increases within plants conditional on worker quality. We conclude that worker sorting is not invalidating the general result of positive council effects as documented in the mounting literature on works councils.

Finally, we documented a positive link between council existence and plant profitability even after controlling for worker quality. Councils seem to make sure that the productivity gains associated with them are split between labor and capital. In combination, our fixed-effects event study results show that plants experience turbulent times before council introduction with strong wage growth and a substantial productivity decline that sharply reduces pre-introduction profits. After council introduction, wage growth flattens and productivity growth sets in, which allows council plants to sustain long-run profitability within a high-wage high-productivity strategy.

We conclude that councils contribute to productivity, wage, and profit inequality across plants, first, by attracting and sustaining high-wage high-productivity workers and, second, by a genuine council effect on firm performance. We show strong positive productivity contributions of high-wage workers that are even stronger when works councils are present. This lends support to the notion that worker quality and worker participation, as a form of high performance management practices, are complements.

We conclude that sorting of high-quality workers to works council plants can improve allocative efficiency and aggregate productivity.

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Appendix

2.A Definitions of variables

Table 2.A.1: Definitions of variables

Variable	**Definition**
Log(labor productivity)	Logarithm of the value added per worker
Log(wage bill per worker)	Logarithm of the wage bill per worker
Log(profit per worker)	Logarithm of the value added net of wage costs (including employers' social security contributions) per worker
Log(employment)	Logarithm of the number of workers
Log(capital intensity)	Logarithm of the capital stock per worker
Works council	= 1 if a works council is present, = 0 if no works council is present
Collective bargaining	= 1 if collective bargaining is present, = 0 if no collective bargaining is present
Worker quality	Mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of 0 and a standard deviation of 1
Skilled employees as share of all employees	Share of workers who have a vocational qualification, relevant professional experience or an university degree
Part-time employees as share of all employees	Share of part-time workers
Apprentices as share of all employees	Share of workers who are doing their vocational training under the vocational training law or the Handicrafts Regulation Act and other training stipulations of all workers
Female employees share of all employees	Share of women
Churning rate	Measure of employment stability. Worker flow rate minus the absolute value of the net rate of employment change.
Exporter	= 1 if plant makes revenue abroad, = 0 if plant does not make revenue abroad
Single plant	= 1 if the plant is an independent company or an independent organization without any other places of business, = 0 if plant does have other/belongs to other branches
Technical state of machinery	Assessment of the overall state of the technical state of the plant and machinery compared to other plants in the same industry. Scale from 1 to 5.
excellent	= 1
good	= 2
fair	= 3
poor	= 4 and 5
Average worker age	Average age of all employees
Universi ty graduates as share of all employees	Share of university graduates

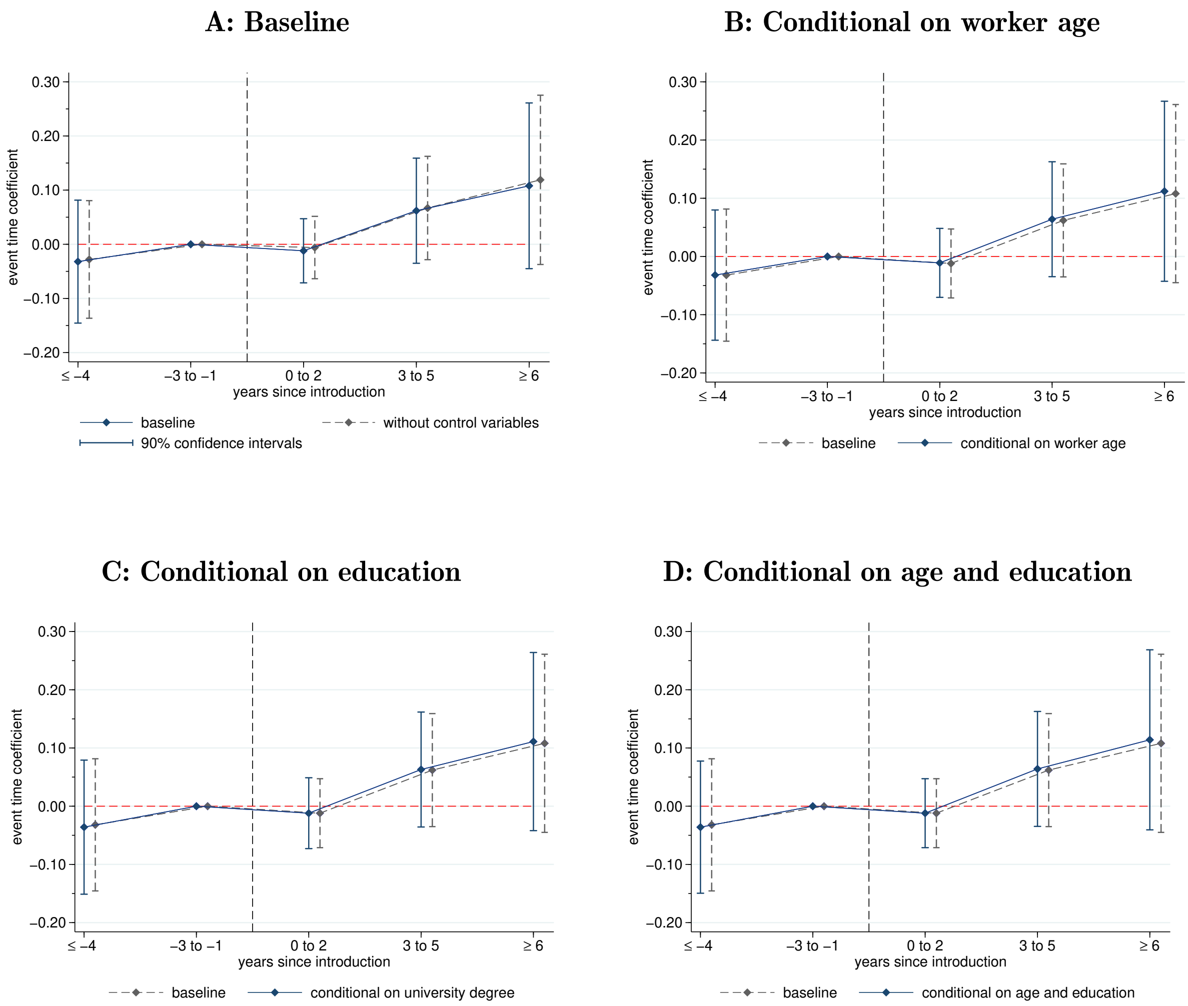
Notes: Linked Employer-Employee Data of the IAB (LIAB), cross-sectional model.

2.B Robustness checks: Selective panel attrition

In this section we present robustness checks of the results in section 2.4 regarding selective panel attrition. Panel attrition is a feature of most panel data sets and may also be an issue in ours. So far we use information on the observed survey years of 2012, 2014 and 2016. If being observed in one of the three years is more likely for successful council plants, we oversample successful council plants because unsuccessful council plants dropped out (survivorship bias) earlier.

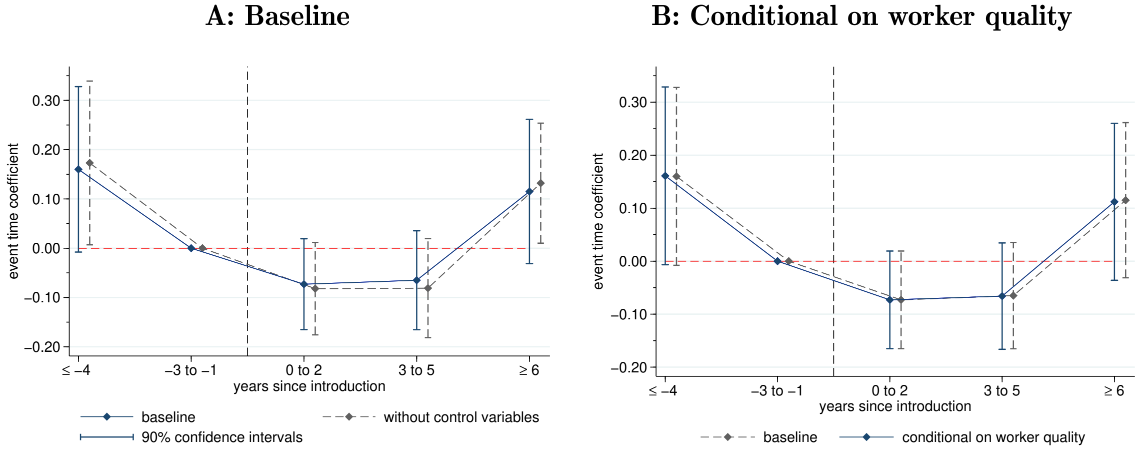
To address selective panel attrition, we conduct an event study in which we only include plants, for which we directly observe council introduction in our data. This means that this sample includes all young works councils, regardless of the quality of the council plant or the council itself and regardless whether the plant survives until the years where council age is surveyed (i.e. 2012, 2014, 2016). The results are depicted in Figure 2.B.1, 2.B.2, 2.B.3 and 2.B.4 and show for each outcome the same patters as in our baseline results presented in section 2.4 that relied on the council age survey question. We include two post-event time dummies instead of three, because higher works council age categories are poorly filled.

Figure 2.B.1: Worker quality, event study (young works councils)



Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (140,782 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of worker quality relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker age at the plant level. For Panel C: holding constant the combined share of university graduates. For Panel D: holding constant age and the share of university graduates. The 90% confidence intervals are shown using standard errors clustered at the plant level.

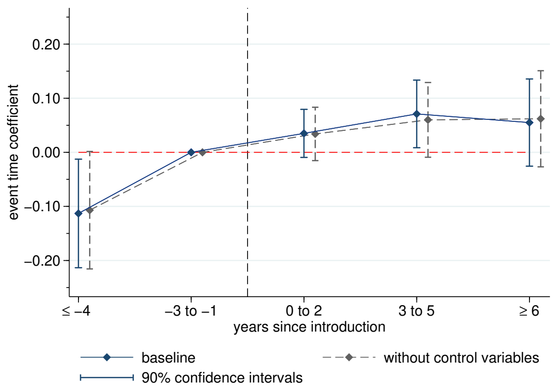
Figure 2.B.2: Labor productivity, event study (young works councils)



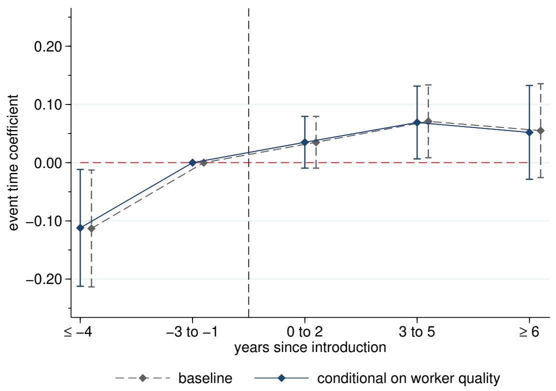
Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (140,782 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of value added divided by the number of employees relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level.

Figure 2.B.3: Wages, event study (young works councils)

A: Baseline



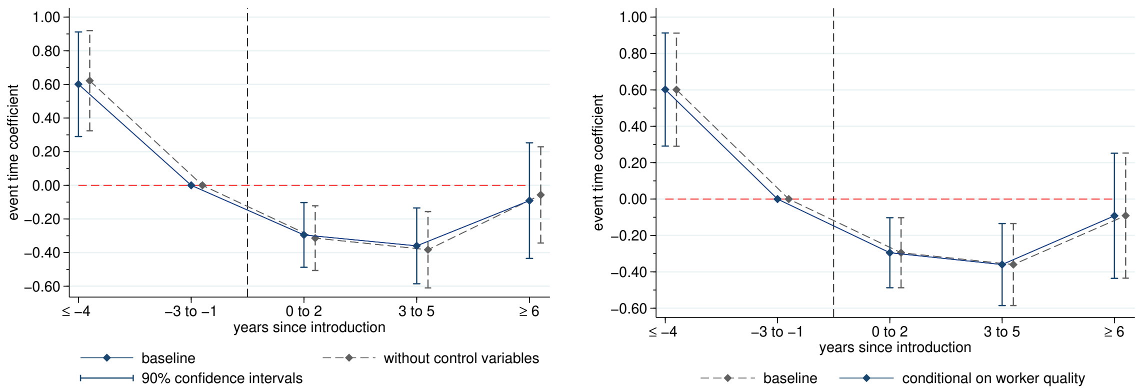
B: Conditional on worker quality



Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (140,782 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of the wage bill divided by the number of employees relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level.

Figure 2.B.4: Profits, event study (young works councils)

A: Baseline **B: Conditional on worker quality**



Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (140,782 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of the value added minus labor costs divided by the number of employees relative to the pre-introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM worker effects (α_i) at the plant level (as described in section 2.3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 0 to 2) is the introduction period of the works council and relative time (in years) is depicted at the horizontal axis. As specified in equation (2.3) the regression includes controls for collective wage agreement presence, capital intensity, export status, single plant status, the state of technical machinery, the share of skilled employees, part-time workers, apprentices and women of all employees, 7 plant size dummies and plant-cohort fixed effects. For panel A: no controls other than specified in equation (2.3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level.

Chapter 3

Co-determination and plant performance: the role of works councils in turbulent times[1]

3.1 Introduction

Economic crises have dramatic consequences for employers and employees alike. How management interacts with the employees during a crisis can determine the success or failure of firms. In this regard co-determination of German works councils may have a special role to play. On the one hand, better-informed workers are potentially more willing to adjust their effort to adverse economic conditions and worker co-determination may cushion rising uncertainty and anxiety about the workplace, which improves motivation as workers have some control over their workplace. On the other hand, firms make concessions and adapt their strategies to worker co-determination. Surprisingly, there is little evidence regarding the performance of works council plants in comparison to non-council plants during economy wide shocks. In this paper I look for how plant-level outcome adjustments during the Great Recession of 2008 and 2009 depend on works council presence. As crisis reactions shape how plants evolve, I also investigate whether these lead to differences during economic recovery.

A pertinent feature of management-employee-interactions is organized labor at the plant- or firm-level that recently gained attention (e.g. Jäger *et al.* 2021, Jäger *et al.*

[1] JEL-Classification: D22, J5, J21,Keywords: works councils, labor hoarding, labor productivity, financial crisis, great recession.

2021a). The German model of industrial relations offers such features as it contains legally determined works councils that organize plant-level worker co-determination. The increased interest in worker co-determination is also rooted in recent calls for stakeholder capitalism[2] and policy notes on the future of work[3] that emphasize strengthening the workers' role in decision making. These calls are made at a time when collective bargaining coverage declines in several countries[4] but interest in organized labor is upward trending[5].

Theory suggests that works councils improve workers' voice, reduce information asymmetries and enhance trust between workers and management (Freeman and Lazear 1995). The majority of ongoing empirical research commend works councils as they improve productivity (Mueller 2012) and may contribute to allocative efficiency through improved worker sorting (Mueller and Neuschaeffer 2021). For the individual worker, works councils can be beneficial in that they relate to higher wages (Addison *et al.* 2010), employer wage premia (Hirsch and Mueller 2020) and less employer wage-setting power (Dobbelaere *et al.* 2020). Besides these, works councils influence the plant's personnel policy and reduce worker turnover, separations and recruitment of employees (e.g. Addison *et al.* 2001, Gralla and Kraft 2018, Hirsch *et al.* 2010).

Despite these results, Jäger *et al.* (2021) argue that worker co-determination influence firm outcomes during both good times and bad times in a way that on average they cancel each other out. This may explain the small causal effects of works councils on economic outcomes. For example, labor hoarding during adverse economic times can lead to reduced plant productivity and downward biased estimated productivity effects of works councils (Mueller 2012). By contrast, worker retention can result in better start-off positions and higher labor productivity growth during economic recovery. In a similar fashion, reduced information asymmetries may improve management decisions, but worker

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2 For example, in 2019 nearly 200 US American CEOs signed a document to commit to stakeholder capitalism in the future (`https://www.businessroundtable.org`).

3 In a policy note Autor *et al.* (2019) emphasize that co-decision making of workers should be put more into focus in the future.

4 The Issue Brief No.1 on trends in collective bargaining coverage by the International Labour Office shows that collective bargaining coverage declined over the last decades in several countries due to country specific reasons.

5 `https://news.gallup.com/poll/354455/approval-labor-unions-highest-point-1965.aspx`

co-determination may hinder and delay decisions that are necessary to be made fast in a quickly changing business environment (Jensen and Meckling 1979).

Additional adjustment costs through works councils may hinder job reallocation and impede potential aggregate welfare and productivity growth (Decker *et al.* 2020, Haltiwanger *et al.* 2014, Hopenhayn and Rogerson 1993).

Evidence on how and whether works councils influence plant-level reaction during and after adverse economic times is scarce (Jäger *et al.* 2021). Hence, while several previous studies have investigated how works council influence employment turnover (Addison *et al.* 2001, Hirsch *et al.* 2010, Grund *et al.* 2016), labor productivity (Mueller 2012, 2015, Mueller and Stegmaier 2017), wages (Addison *et al.* 2010) and profits (Mueller 2011, Mueller and Neuschaeffer 2021), the role of worker co-determination in plants' capability to weather economic ups and downs is relatively unexplored. Only recently it has been shown that works councils are associated with more employment stability during the COVID-19 Pandemic in 2020 (Fackler*et al.* 2021).

In this paper I provide new evidence on questions of the role of shop-floor worker co-determination during economy wide shocks. Therefore, I ask whether works councils affect plants' employment, value added, wage bill and profits per worker in the Great Recession of 2008 and 2009. Next to the reaction during the crisis, I elaborate whether works councils induced differences lead to different outcomes during the economic recovery.

To address these questions I use the IAB Establishment Panel between 2006 and 2013. I use a triple difference-in-differences (triple DID) identification strategy to estimate changes in the plant-level outcome between council and non-council plants that have been affected by the crisis. Triple DIDs enhance DID settings by analyzing subgroups, i.e. council and non-council plants, where differences in changes between those two groups are assumed to be driven by its presence only (Gruber 1994). To define whether a plant was affected I use survey information about the Great Recession of 2008 and 2009.

When analyzing all plants I find no significant difference in performance between works council and non-council plants during and after the Great Recession. While affected plants reduce employment, value added and profits per worker differences between works council and non-council plants are absent. This suggests that on average works council plants neither hoarded more labor than non-council plants nor experienced different changes in labor productivity and profitability. The average wage bill per worker does not change differently for affected plants and there is also no difference between council and non-council plants. Scrutinizing plant sizes uncovers important differences between large and small plants.

I observe that affected works council plants with more than 100 employees retain more employees during and after the Great Recession but I find no employment differences between small affected council and non-council plants. These results coincide with larger reductions in value added per worker in large, but not in small works council plants. Large works council plants seem to hoard more labor than affected non-council plants, which is accompanied with a larger reduction of value added per worker. This reduction is not accompanied by higher productivity growth rates during the economic recovery from 2010 onward. In addition, the wage bill per worker stays constant throughout the crisis, which supports the idea that large works council plants foster insurance mechanisms for employment and wages.

The results support previous considerations that works councils unfold their full potential in large plants. For example, can plants with more than 100 employees exert more information from the management through economic committees or have stronger veto rights on dismissals (Addison *et al.* 2001).

The results also indicate that works councils are not on average harmful or beneficial during and after economic shocks. The results, however, point at heterogeneous effects of works councils during adverse times. Workers in large plants use their collective voice to stay employed saving their employment rents, which is potentially harmful for plant productivity and profits. This aspect may be relevant to understand why management may oppose the presence of works councils (Mueller and Stegmaier 2020), as there is no immediate benefit to the plant's performance after the crisis.

The next section describes the institutional background and discusses respective hypotheses. Section 3.3 describes the data and the sample and section 3.4 introduces the empirical strategy. Section 3.5 presents the main results, section 3.6 discusses some robustness checks and section 3.7 concludes.

3.2 Institutional background and hypotheses

German industrial relations consist of two relevant cornerstones (Oberfichtner and Schnabel 2019). At the center of this analysis is worker representation through works councils at the plant level which is determined by the Works Constitution Act (WCA)[6]. It defines rights, obligations and election modalities of works councils. Furthermore, the WCA states that the management is prohibited to hinder the formation and functions of works councils and must provide relevant resources.

Works councils have information, co-determination and veto rights on different matters. For example, they have information rights in personnel planning and can veto management decisions to prevent measures that employers would like to take. The co-determination rights concern 'social matters' in which the management may not decide on overtime regulation, safety measures or remuneration arrangements without the consent of the works council. Works councils must act in 'mutual trust' and co-determination must be in the plant's and workers' interest.

Specifically, works councils have extensive rights in the plant's personnel planning concerning current and future employment levels or overtime regulations. Works councils approve management decisions on recruitment and can use their co-determination rights to disagree on dismissals (§102 WCA). In case of a failed consent, the dismissal is declared void until the labor court decides on it. Works councils have also the right to interfere with the restructuring of plants to prevent uncoordinated mass layoffs (§112 WCA).

Works councils are allowed to be elected in plants with at least five permanent employees. Usually, employees in small plants do not take advantage of the right to establish a works council, so works councils are rarely observed in small plants. Ellguth and Kohaut (2019) report that in 2018 around 26 percent of all plants with more than

[6] Betriebsverfassungsgesetz.

20 employees had a works council, whereas only 5 percent of the plants with 5 to 50 employees had one.

Works council's rights and its actual size increase with plant size. The number of works council members is an increasing function of plant size and with 200 or more employees at least one plant's council member is exempt from her job to work only for the duties of the works council (§38 WCA). The WCA also concedes more rights on decisions of works councils on the personnel planning and job security with increasing plant size. For example, works councils are allowed to make proposals to secure and support plant-level employment. While in small plants the management can neglect such proposals verbally, in plants with more than 100 employees a written justification is required and employees are allowed to get feedback from the employment agency (§92 WCA). Once the plant has more than 20 permanent employees, the management has to inform the works council about potential changes in plant-level operations that may have negative effects on employees, and once the employment surpasses 300 employees the works council is allowed to reach out for external consultants. In addition, an economic committee (Wirtschaftsausschuss) has to be formed in plants with more than 100 employees. It consists of works council members and qualified employees who inform and consult the works council about the economic and financial condition of the plant (§106, WCA).

The second cornerstone, not at the center of this analysis, are collective wage agreements mainly at the industry[7] level. Collective wage agreements are set between trade unions and employer associations and both negotiate over minimum terms for wages and working hours without state interference. There are no legal obligations between trade unions and works councils, though traditional links between works councilors and trade unions exist.

What follows is a discussion of the theoretical and empirical context of the economic effects of works councils. In particular, I will focus on the potential effects of works councils on employment, labor productivity, wages and profits during times of economic hardship.

[7] Only a small share of 3 percent have a firm-level collective agreement in 2013. (own calculations from the IAB Establishment Panel).

3.2.1 Employment

Works councils improve the collective voice of workers. Following the exit-voice theory by Hirschman (1970), Freeman (1976) describes that institutions of organized labor improve the collective voice of workers. Instead of exiting the plant in case of disagreements, workers are able to formulate jointly their discontent. This potentially lowers voluntary quits as workers are directly improving their workplace, fosters internal management of the workforce and improves the likelihood that management proposes alternatives to laying off workers if the plant is affected by an exogenous shock.

The majority of studies investigating collective-voice mechanisms and works councils' consent rights in personnel planning ignore economic adverse times and focus on the average effects instead. However, in a very recent study it is shown that works councils are associated with lower churning rates and dismissal rates and more employment stability during the COVID-19 Pandemic in 2020 (Fackler*et al.* 2021).

In cross-sectional studies, it is argued that smaller employment turnover in works council plants relates to improved collective voice making voluntary resignations a less favorable option (Addison *et al.* 2001, Pfeifer 2011). Furthermore, Adam (2019) shows in a difference-in-differences setting, that improved rights of works councils increase worker voice and reduce voluntary quits.

Grund *et al.* (2016) provide evidence that the consent rights of councils in personnel planning reduce involuntary quits. Hirsch *et al.* (2010) further show that involuntary quits of high tenured workers into unemployment are less likely with a works council. Boockmann and Steffes (2010) argue that larger separation costs in works council plants reduce job mobility and find larger job duration in works council plants. Evidence of smaller hiring rates in works council plants (e.g. Addison *et al.* 2001, Gralla and Kraft 2018) complement these studies.

Based on the previous literature the first hypothesis for employment is: Affected works council plants retain more workers during the Great Recession. This hypothesis is based on the idea that works councils increase labor adjustment costs and improve the collective voice of workers. Both reduce (in)voluntary quits and foster mechanisms of internal labor markets.

Plants exert labor hoarding if they adjust labor input disproportionately to a negative shock (Hamermesh 1993). In this case labor input is underutilized and plants employ temporarily more workers than needed for actual production. Labor hoarding may be a coherent strategy if labor adjustment costs are high. As a direct consequence of labor hoarding the second hypothesis is: Affected works council plants have smaller employment growth rates during the economic recovery.

The extent of changes in employment may vary by plant size, since works council rights improve and the benefits of collective voice mechanisms are larger in larger plants. For example, (Addison *et al.* 2001) find smaller differences in hiring and departure rates between council and non-council plants in plants between 21 and 100 employees than including all plants. They argue that collective voice effects of works councils are potentially smaller in plants with fewer employees, because potential information asymmetries are small and informal mechanisms in reducing information asymmetries work better. The third hypothesis for employment is: The retention of employees are higher in large affected works council plants than in small affected works council plants. Collective voice effect and rights of works councils improve with plant size.

3.2.2 Productivity

The works council's ability to verify the management's statements improves top-down communication and reduces information asymmetries, which enhances efficiency and makes potential calls on the plant's economic condition more credible (Freeman and Lazear 1995). Works councils also improve bottom-up communication. Workers can share their discontent anonymously and provide information that could improve management decisions. One-sided hierarchical decisions may, therefore, be less efficient than decisions that benefit from additional shop-floor information (Freeman and Lazear 1995).

Strong co-determination rights complement the improved information symmetry.

Workers who participate in decision-making are more concerned with their workplace and potentially bear more unpopular decisions, offer effective alternatives, and are more flexible in their efforts (Addison *et al.* 2001, Freeman 1976). It is important to stress that works councils are important during economic downturns because they verify the information provided by management which makes their declarations more reliable. It is, however, two sides of the same coin that the communication of the management during good times happens in a similar fashion to be trustworthy. Workers who believe in these statements are more willing to adapt their effort levels to the economic situation (Freeman and Lazear 1995), irrespective of the plant's economic situation. Workers may also improve productivity by using their collective voice instead of exiting the plant and withholding efficiency-enhancing information (Freeman and Lazear 1995).

The majority of the empirical literature argues along Freeman and Lazear (1995) that reduced information asymmetries and positive effects of collective voice explain the genuine productivity effect of works councils. Mueller (2012) finds a labor productivity effect of works council around 7 percent for the manufacturing sector between 2001 and 2005, which may be downward biased as works councils are potentially hoarding more labor than non-council plants during economic downturns.

Works councils are also associated with larger labor productivity along the productivity distribution, which supports the idea that works councils enhance efficiency even in well-managed, highly productive, plants (Mueller 2015). Productivity effect of works councils also increases over time (e.g. Mueller and Stegmaier 2017, Jirjahn *et al.* 2011) because works councils and management improve their interaction over time. In recent study Mueller and Neuschaeffer (2021) confirm the positive relationship between works councils and labor productivity when controlling for worker quality.

Though works council presence and labor productivity are positively related, the first hypothesis for labor productivity argues along Mueller (2012): Affected works council plants experience larger drops in labor productivity during economic adverse times. Since I expect to find more labor hoarding in council plants a simultaneous drop in value added per worker is most likely.

Holding workers may lead to a better start-off position during the economic recovery, especially when works council plants employ workers of higher quality (Mueller and Neuschaeffer 2021) and if workers use their collective voice to improve plant-specific human capital (Frick and Moeller 2003). Taking a long-term view on employment may increase plant's investments in plant-specific human capital (Freeman and Lazear 1995). Works council plants have indeed more workers with higher average tenure (e.g. Addison *et al.* 2010) and Stegmaier (2012) finds more employer-provided training in works council plants. This may especially play a role at adverse times, when plants invest in the human capital of underutilized labor. Trained workers may be better prepared to start-off during an economic recovery, especially when they are more willing to adjust their effort during the economic recovery (Addison *et al.* 2001). In addition, search and training costs are avoided and works council plants can better utilize the retained human capital. Thus, the second hypothesis for labor productivity is: Affected works council plants experience larger productivity growth rates during the economic recovery relative to non-council plants.

Effects on labor productivity differ most likely by plant size because the works council power increases with plant size. Works councils in large plants potentially have a greater impact on plant productivity (Addison *et al.* 2001, Addison 2006) but the negative effect of increased labor hoarding on productivity is also expected to increase with plant size, potentially counteracting any positive impacts. Based on expected larger employment retention in large plants the third hypothesis is: Large affected works council plants experience larger productivity reductions during the crisis than smaller works council plants.

3.2.3 Wages

Freeman and Lazear (1995) argue that works councils are able to improve workers' bargaining power through their extensive co-determination rights, which is supported by the most recent empirical literature. Though works councils are forbidden to negotiate wages directly, most cross-sectional studies using plant-level data find larger wages in works council plants (Addison *et al.* 2010). Addison *et al.* (2001) show that wages are up to 18.5 percent higher in works council plants. Addison *et al.* (2010) demonstrate at the worker-level that workers in works council plants receive 9.5 percent higher wages, even when co-worker characteristics are taken into account. This finding is reinforced by Mueller and Neuschaeffer (2021) who show that controlling for worker quality reduces the wage net gap of 10.1 percent between council and non-council plants by only 1.7 percentage points[8].

In addition to higher wages, Hirsch and Mueller (2020) find larger employer wage premia in works council plants. Their results suggest that works councils increase the workers' bargaining power because they account for differences in the quasi rents and worker sorting. This hypothesis is supported by Mueller and Neuschaeffer (2021), who do not find wage increases after works council introductions despite average worker quality improvements.

While these studies rest implicitly on rent-sharing models (Card *et al.* 2018), an alternative channel offer risk-sharing models (Guiso *et al.* 2005). Both models assume that plant performance affects wages, but the channels differ (Cardoso and Portela 2009). In both models works councils differ in their mechanism of influencing the underlying effect: on the one hand, works councils theoretically improve the bargaining power (Freeman and Lazear 1995) and on the other hand, works councils enlarge provided information and improve insurance mechanisms that shield off workers' wages from exogenous shocks (Guertzgen 2014).

Insurance mechanisms work during exogenous shocks when risks are shared between risk-averse workers and risk-neutral firms. In this case, firms act as insurance providers to workers and firms have to guarantee to stick to their arrangement. This guarantee can arguably be influenced by works councils. Works councils are able to lower the risk that plants deviate from communicated agreements as workers are better informed on changes in the plant's financial condition and can co-determine on crisis-related changes

8 The respective works council coefficient reduces from 10.1 to 8.4 percent (Mueller and Neuschaeffer 2021, Table 5, pg. 460).

(Guertzgen 2014). For example, workers may gather over-time in the years before the Great Recession (Burda and Hunt 2011), and works councils ensure through working time accounts (Gerner 2012) that this is considered during economic downturns. While works councils seem likely to improve insurance mechanisms, workers do not seem to pay a relevant insurance premium as workers in works council plants receive higher wages (Addison *et al.* 2010). It opposes the rationale and findings of Kim *et al.* (2019) for employee board-level representation, where workers pay an insurance premium through lower wages to be insured against economic shocks.

In both potential mechanisms, works councils can use their co-determination and information rights to prevent potential wage reductions. The first hypothesis for wages is: Affected works council plants do not experience other wage adjustments than affected non-council during the economic downturn. If works council plants protect their workers from exogenous negative shocks by keeping them employed at constant wages average wages should stay constant during this time. In contrast, non-council plants may adjust the wage cost at the extensive margin leaving average wages also unchanged. Thus, the second hypothesis for wages is: Wage growth during the economic recovery is similar between affected non-council and works council plants.

Alternatively, if I find larger wage reductions in affected works council plants during the crisis and more labor hoarding, the insurance mechanism may only work for employment, but not wages. In this case, workers accept lower wages during a crisis in order to be insured against potential unemployment.

Another alternative is that only a specific clientele of workers benefitAnother alternative is that only a specific clientele of workers benefit from works councils during adverse times. The evidence by Hirsch *et al.* (2010) suggest that workers with high tenure have more power within the works council, making this group less vulnerable to potential redundancies. As those workers usually receive larger wages, a potential increase in average wages may be plausible during adverse times if works councils only shield of only this specific group of workers.

Evidence is mixed whether works councils affect wages differently in small and large plants. Using worker-level data Addison *et al.* (2010) find that workers receive smaller works council wage premia in plants between 21 to 100 employees, whereas Addison *et al.* (2001) do not find wage differences in smaller works council plants compared to all plants. However, as works councils have more rights in larger plants, the third hypothesis for wages is: Rent sharing or risk sharing mechanisms work better in larger plants, where no different wage growth rates are present between affected council and non-council plants. Large works council plants are better at insuring employment and wages of workers against exogenous shocks, whereas works councils in small plants have less power in doing so.

3.2.4 Profits

If affected works council plants and non-council plants differ in wage and labor productivity growth, then profits may also be affected during a crisis and afterward. In the framework by Freeman and Lazear (1995) plant profits have an inverted U-shaped function with respect to works council power. If this power exceeds a certain level the relationship between works councils and profits is negative. Most recent studies show that works councils are positively associated with profits per worker (Mueller and Neuschaeffer 2021), whereas older studies using subjective measures found negative relationships (for a review see Mueller 2011). When the power exceeds a certain level, works councils exert a negative impact on profits. This situation may be present during economic hardship. I expect that profit changes in works council plants changes are similar to labor productivity changes, but more pronounced, because average wages are more likely to stay constant during the crisis.

3.3 Data

I use the IAB Establishment Panel of the Institute of Employment Research (IAB), a representative survey of German plants with at least one worker eligible for social security. It is surveyed since 1993 (1996 for East Germany) on 30th June of each year and covers since 2001 yearly between 15,000 and 16,000 plants (Ellguth *et al.* 2014).

The data contain information on the employment composition, revenues, intermediate inputs, wages and industrial relations. Further, it includes information to calculate the capital stock using a modified perpetual inventory method of Mueller (2008, 2017). I further transfer the information on revenues and intermediate inputs to the previous year, because in the questionnaire it refers to the previous year.[9] I use time consistent 2-digit industry classifications, which are provided by the IAB (Eberle *et al.* 2011) and use price deflators to receive real values for nominal information.

Identification of affected plants During the Great Recession of 2008 and 2009 German GDP by dropped 5 percent (see e.g. Möller 2010). It was set off by the Financial Crisis in 2007, which led to a reduction of global demand for capital products and affected mainly highly productive, export-oriented, large plants from the automotive or chemistry sector (e.g. Möller 2010). Between September 2008 and September 2009, German export value fell by around 17 percent[10]. Though this drop in GDP was one of the largest since World War II it was not accompanied by a strong increase in unemployment rates, which was termed by different observers as the 'German employment miracle' (e.g. Burda and Hunt 2011).

To identify whether a plant was affected by the Great Recession, I use information from the wave 2010 of the IAB Establishment Panel. In the business policy and performance section of the questionnaire, plants were asked if they were affected by the Great Recession.[11] I use this information to construct the control and treatment group for the empirical strategy. The treatment group consists of plants that report in 2010 to be negatively affected by the Great Recession, whereas the control group reported being either positively affected or unaffected by the crisis. The definition of the control and treatment group is based on 1,510 plants. 916 plants define the treatment group observations and 594 define the control group.

9 For example, the revenue reported in 2014 refers to plants' performance in the year 2013 and thus plants are only included if they are observed in both years.
10 German Statistical Offices, own calculations.
11 The question is: "Did the economic and financial crisis of the last two years affect your establishment/office?" Plants could assess on a Likert scale between 1 and 5 the severity of being affected.

Using the survey information allows me to identify affected plants based on subjective information. This survey measure has the drawback that it may not capture real affection of the crisis and rather may be independent of factors related to the Great Recession. To show that my treatment indicator captures affection I show in section 3.5.1 that the share of treated plants of 2-digit industries cells is negatively related with sectors that experienced the largest drop in GDP p.c. and global import demand. I further show that reductions in revenue are high in those plants that reported being affected relative to those which reported otherwise.

Restriction of the sample I restrict the sample to the years 2006 to 2013 and include only manufacturing and service private-sector plants that are observed at least in 2010. Plants belonging to financial services, insurances and real estate are omitted, because sales of financial services and insurances have an indistinct revenue definition and the capital stock in real estate is ambiguously defined.

I exclude plant-year observations with less than 20 employees.[12] Excluding the very small plants ensures that affected and unaffected plants are more comparable. Including small plants would inflate the number of non-council plants and render the comparison group less comparable, especially because only a small share of plants with less than 20 employees have a works council (Oberfichtner and Schnabel (2019) report 5.1 percent in 2015). I also exclude plant-year observations with more than 500 employees to reduce the likelihood that the results are affected by board-level employee representation, which is mandatory in plants with more than 500 employees, coincides with the presence of a works council and also influences firm-level reactions to economy wide shocks (Gregorič and Rapp 2019). Moreover, the likelihood of works council presence increases with plant size and around 90 percent of plants with more than 500 employees have a works council (Oberfichtner and Schnabel 2019) making this group infeasible to construct triple difference-in-differences. Within 2-digit industries I truncate the first and last percentile of the value added per worker and the capital stock per worker distributions.

12 Redoing the exercises with plant-year observations with no less than 15 employees and no more than 550 employees the overall results do not change.

I get an unbalanced panel of 10,398 plant-year observations. Average descriptive statistics are shown in Table 3.1 and are discussed in section 3.5.

3.4 Empirical strategy

To test the different hypotheses of section 3.2, I use a triple difference-in-differences strategy (Gruber 1994), in which a difference-in-difference estimator is interacted with a works council dummy. Standard difference-in-differences estimators capture the change of an outcome between two points in time of a treatment group relative to the change of the outcome of a control group. The triple DID captures differences in the difference-of-differences estimator of council and non-council plants and rests on the assumption that only the existence of works councils drive this captured difference (Verhoogen 2008). It therefore informs about whether treated works council plants experienced different changes than treated non-council plants. I estimate the model in an event study style using a plant-level fixed effects estimator:

$$\begin{aligned} y_{jt} = &\sum_{t=-m}^{M} \beta_{1,t} T_t + \sum_{t=-m}^{M} \beta_{2,t}(T_t \times D_j) + \sum_{t=-m}^{M} \beta_{3,t}(T_t \times woco_j) \\ &+ \sum_{t=-m}^{M} \beta_{4,t}(T_t \times woco_j \times D_j) + \mu_j + \gamma_X X_{jt} + \varepsilon_{jt} \end{aligned} \tag{3.1}$$

I use four different dependent variables of interest (y_{jt}). The logarithm of the number of employees of plant j in year t captures the degree of changes in employment over time and therefore measures labor hoarding. To capture changes in labor productivity I use the logarithm of value added per worker. The logarithm of the average wage bill per worker captures changes in average wages and the logarithm of the value added minus the wage costs (employers' social security contributions included) capture profits per worker.

The dummy variable T_t equals one for a respective year pair[13], where the years before the crisis $(2006, 2007)$ is the base category. The time-invariant treatment dummy D_j equals one if the plant reports in 2010 to be negatively affected by the Great Recession and zero

[13] 2006–2007, 2008–2009, 2010–2011, 2012–2013.

otherwise. The time-invariant dummy variable $woco_j$ equals one if a plant has a works council throughout the observed period and zero otherwise.[14] To construct the triple difference-in-differences estimator I interact $woco_j$ with the treatment-dummy D_j and the time dummies T_t. The interaction of D_j and T_t is the standard difference-in-differences estimator ($\beta_{2,t}$) and capture the outcome of affected plants relative to unaffected ones. The coefficient $\beta_{4,t}$ captures the difference in outcome between affected works council plants (relative) to affected non-council plants in each of the year pairs. The plant fixed effect μ_j captures time consistent plant heterogeneity and controls for unobserved plant characteristics.[15] The vector of other control variables (X_{jt}) includes the presence of collective wage agreements, exporting status, single establishment status, churning rate, share of skilled workers, part-time workers, apprentices and women, and capital intensity. Some control variables included in X_{jt} may potentially change as a result of the affection of the Great Recession. To ensure that I do not run into the 'bad control' problem I run each regression without vector X_{jt}. Importantly, since I control for plant-fixed effects level differences are not concerned in these regressions, but changes in the outcome variables that potentially differ by affection and works council status.

3.5 Results

3.5.1 Descriptive findings

Table 3.1 shows the cross-sectional summary statistics of all council, non-council, affected and unaffected plant-year observations for the years 2006 to 2013. Works council plants are more than twice as large and have smaller churning, leaving and joining rates. In addition, the logarithm of the value added per worker, the wage bill and profits per worker are higher in works council plants, which supports previous findings. Correlates of works councils such as plant size or industry affiliation may explain the differences. However, most recently Mueller and Neuschaeffer (2021) show that differences in these

14 Works council status changes are discussed in section 3.6.

15 This includes the time invariant works council status $woco_j$ and the affection dummy D_J.

outcomes are persistent even when correlates such as worker characteristics, plant size or industry affiliation are controlled for in multivariate regressions.

Table 3.1: Descriptive statistics

	Works council	no Works council	affected	unaffected
Variable	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Log(employment)	4.869 (0.787)	3.857 (0.681)	4.328 (0.865)	4.147 (0.886)
Log(value added per worker)	11.189 (0.576)	10.829 (0.602)	10.987 (0.588)	10.948 (0.660)
Log(wage bill per worker)	10.313 (0.359)	10.007 (0.433)	10.170 (0.414)	10.060 (0.451)
Log(profits per worker)	10.173 (1.221)	9.745 (1.257)	9.900 (1.242)	9.939 (1.289)
Log(revenue per worker)	12.063 (0.762)	11.628 (0.805)	11.787 (0.773)	11.823 (0.882)
Log(revenue)	16.932 (1.179)	15.485 (1.054)	16.114 (1.280)	15.970 (1.359)
Works council (dummy)	1.000 (0.000)	0.000 (0.000)	0.438 (0.496)	0.331 (0.471)
Affected by the crisis (dummy)	0.682 (0.466)	0.577 (0.494)	1.000 (0.000)	0.000 (0.000)
Log(capital stock per worker)	11.046 (1.128)	10.528 (1.233)	10.794 (1.213)	10.635 (1.223)
Collective bargaining (dummy)	0.691 (0.462)	0.251 (0.434)	0.427 (0.495)	0.423 (0.494)
Exporting status (dummy)	0.678 (0.467)	0.500 (0.500)	0.654 (0.475)	0.436 (0.496)
Single establishment (dummy)	0.568 (0.495)	0.815 (0.388)	0.730 (0.444)	0.696 (0.460)
Technical state of machinery				
= excellent (dummy)	0.159 (0.366)	0.206 (0.404)	0.172 (0.378)	0.212 (0.409)
= good (dummy)	0.494 (0.500)	0.533 (0.499)	0.502 (0.500)	0.543 (0.498)
= fair (dummy)	0.310 (0.462)	0.241 (0.428)	0.296 (0.456)	0.224 (0.417)
= poor (dummy)	0.037 (0.189)	0.019 (0.137)	0.030 (0.170)	0.021 (0.142)
Joining rate	0.032 (0.051)	0.055 (0.091)	0.042 (0.074)	0.051 (0.085)
Leaving rate	0.029 (0.049)	0.040 (0.074)	0.036 (0.068)	0.034 (0.061)
Churning rate	0.030 (0.049)	0.051 (0.116)	0.041 (0.096)	0.045 (0.096)
Share of skilled workers	0.745 (0.233)	0.733 (0.240)	0.745 (0.232)	0.726 (0.245)
Share of part-time workers	0.100 (0.159)	0.139 (0.181)	0.096 (0.137)	0.169 (0.213)
Share of apprentices	0.044 (0.040)	0.053 (0.059)	0.048 (0.050)	0.050 (0.056)
Share of women	0.263 (0.202)	0.321 (0.246)	0.263 (0.206)	0.355 (0.257)
N	4,126	6,272	6,433	3,965

Notes: IAB Establishment Panel. 2006–2013. Plants with 20–500 employees. Affected by the crisis (dummy) are all plant-year observations of plants that reported in 2010 being negatively affected by the financial and economic crisis.

Table 3.1 also shows that in total affected plants are larger, have higher value added per worker and pay higher average wages. This supports previous descriptions that large, highly productive plants have been affected most by the Great Recession (Möller 2010). Profits and revenue per worker are smaller in affected plants which potentially already capture the exogenous shock to revenue most of these plants faced.

As discussed in section 3.3, I test whether the definition of affection captures the exogenous drop of global import demand and GPD found in other data sources. The two scatter plots in figure 3.C.1A compare the shares of affected plants within 2-digit-industries with global import demand (panel A) from the UN Comtrade Database and growth rates between 2007 and 2009 of revenue per worker (panel B) from the German Federal Statistical Office. The share of affected plants is high in 2-digit industry cells that experienced larger drops in revenue per worker and global demand between 2007 and 2009.[16]

I also plot the revenue (Figure 3.C.2) for affected and unaffected plants, to check whether plants defined as the treatment group experienced larger drops in revenue. The figure shows that treated plants experienced a large drop in revenue, whereas plants defined as unaffected experienced a smaller one.

Overall, the descriptive results support previous studies with regard to differences of works council presence. I further show proof that the plant-level survey information matches with the reductions in global demand and GDP per capita revenue from the IAB Establishment Panel.

3.5.2 Employment

The triple difference-in-differences estimations are shown in the following. Next to exercises for the whole sample, I investigate differences in plants with more and less than 100 employees. I select this cutoff point, because works council rights are almost invariant in plants up to 100 employees. This sample-split is further motivated by previous studies (e.g. Addison *et al.* 2001 or Addison *et al.* 2010) who use similar thresholds, that orient at the WCA.

Table 3.2 shows the results with the logarithm of the number of employees as the dependent variable. In column 1 no control variables are included except the dummy structure needed for the triple difference-in-differences estimation. Affected plants had a 2.8 percentage points lower employment growth relative to the control group during the

[16] Revenue per worker and global demand can only be shown for the manufacturing sector.

years 2008 and 2009. This difference increases from 9.0 percentage points in 2010 and 2011 to 11.5 percentage points in 2011–2013.

Table 3.2: Employment, triple difference-in-differences

	(1)	(2)
$T_{2008,2009}$	0.056***	0.041***
	(0.010)	(0.007)
$T_{2010,2011}$	0.102***	0.079***
	(0.013)	(0.010)
$T_{2012,2013}$	0.159***	0.122***
	(0.016)	(0.013)
$T_{2008,2009}$ × works council	-0.026*	-0.022*
	(0.015)	(0.012)
$T_{2010,2011}$ × works council	-0.050**	-0.044***
	(0.021)	(0.016)
$T_{2012,2013}$ × works council	-0.061**	-0.047**
	(0.029)	(0.023)
$T_{2008,2009} \times D_j$	-0.028**	-0.006
	(0.012)	(0.009)
$T_{2010,2011} \times D_j$	-0.090***	-0.047***
	(0.017)	(0.014)
$T_{2012,2013} \times D_j$	-0.115***	-0.057***
	(0.021)	(0.017)
$T_{2008,2009}$ ×works council × D_j	0.014	0.017
	(0.018)	(0.015)
$T_{2010,2011}$ ×works council × D_j	0.018	0.022
	(0.026)	(0.021)
$T_{2012,2013}$ ×works council × D_j	0.019	0.011
	(0.035)	(0.029)
Other control variables	no	yes
R^2	0.057	0.360
N	10,398	10,398

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery.

The triple difference-in-difference coefficients (T_{year} ×works council × D_j), informing about the difference in employment changes between affected council and non-council plants, range between 0.014 and 0.019 (column 1) and are statistically insignificant. The results do not change when including further control variables (column 2). The results show that none of the three proposed hypotheses for employment are supported for the whole sample.

Table 3.3 shows the results for the two size groups.[17] Affected plants with less than 100 employees have no smaller employment growth in the year 2008 and 2009 (column 1), whereas plants with more than 100 employees experienced 11.5 percentage point smaller growth rates in 2008 and 2009 (column 3). The difference between affected and unaffected plants increase in the years from 2010 onward.[18]

The triple difference-in-differences coefficients are statistically not different from zero for plants with less than 100 employees. In contrast, plants with more than 100 employees, affected works council plants experienced a drop that was 10.5 percentage points smaller during the crisis years (2008–2009) relative to large affected non-council plants (column 3). Large works council plants experienced almost no smaller growth in employment during the crisis than unaffected plants. The triple DID coefficients reduce in the years 2010 to 2013, supporting the idea that employment growth is slightly smaller during economic recovery. Including further control variables reduce these coefficients and the 2012–2013 coefficient looses statistical power.

Overall, the results do not support the three proposed hypothesis for all and small plants, but only for larger plants. The difference between small and large plants supports the idea that works councils are able to retain more workers once a certain plant size threshold is reached.

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[17] The descriptive statistics are found in appendix Table 3.B.1.

[18] It is important to emphasize that average growth rates for large plants are rather small. Relative to the base period affected plants decreased their employment by 0.8 percent (0.107 - 0.115) in 2008 and 2009, by 2.5 percent in 2010 and 2011 and grow by 2.3 percent in 2012 and 2013.

Table 3.3: Employment by plant size, triple difference-in-differences

	< 100 employees		≥ 100 employees	
	(1)	(2)	(3)	(4)
$T_{2008,2009}$	0.042***	0.034***	0.107***	0.072***
	(0.009)	(0.008)	(0.026)	(0.019)
$T_{2010,2011}$	0.086***	0.069***	0.133***	0.119***
	(0.013)	(0.011)	(0.028)	(0.023)
$T_{2012,2013}$	0.139***	0.109***	0.186***	0.169***
	(0.016)	(0.014)	(0.034)	(0.029)
$T_{2008,2009}$ × works council	-0.036**	-0.045***	-0.070**	-0.038*
	(0.018)	(0.015)	(0.030)	(0.023)
$T_{2010,2011}$ × works council	-0.048	-0.050**	-0.081**	-0.072***
	(0.030)	(0.024)	(0.033)	(0.027)
$T_{2012,2013}$ × works council	-0.096***	-0.091***	-0.083**	-0.072**
	(0.034)	(0.029)	(0.041)	(0.034)
$T_{2008,2009} \times D_j$	-0.004	0.006	-0.115***	-0.065**
	(0.012)	(0.010)	(0.031)	(0.025)
$T_{2010,2011} \times D_j$	-0.059***	-0.030**	-0.158***	-0.116***
	(0.017)	(0.014)	(0.039)	(0.033)
$T_{2012,2013} \times D_j$	-0.085***	-0.043**	-0.163***	-0.116***
	(0.020)	(0.017)	(0.047)	(0.040)
$T_{2008,2009}$ ×works council × D_j	-0.011	0.028	0.105***	0.064**
	(0.025)	(0.022)	(0.035)	(0.029)
$T_{2010,2011}$ ×works council × D_j	-0.019	0.015	0.111**	0.089**
	(0.036)	(0.031)	(0.044)	(0.037)
$T_{2012,2013}$ ×works council × D_j	0.008	0.032	0.090*	0.056
	(0.043)	(0.040)	(0.054)	(0.047)
Other control variables	no	yes	no	yes
R^2	0.062	0.330	0.075	0.308
N	6,807	6,807	3,591	3,591

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and the technical state of machinery.

3.5.3 Labor productivity

Table 3.4 follows the structure of table 3.2 and reports the triple difference-in-differences results of the logarithm of value added per worker. Affected plants have a lower labor productivity of around 5.8 percent in 2008 and 2009. When including other control variables (column 2) the coefficient reduces to 0.070. Excluding the control variables the effect is temporary, whereas with control variables the reduction of labor productivity is nearly constant over time.

The negative triple difference-in-differences coefficients support the notion of smaller labor productivity in affected works council plants, but the coefficients are statistically insignificant. This result is in accordance with the results for employment (see section 3.5.2), where I do not find more labor hoarding in all works council plants during the crisis, which potentially reduce labor productivity.

The results for small plants (Table 3.5, columns 1 and 2) do not support the hypotheses of smaller labor productivity either. In plants with more than 100 employees, affected plants do not have a statistically significant reduction in labor productivity during 2008 and 2009 (columns 3 and 4). An absent drop is in line with the labor adjustment in large affected plants found in the employment regressions. I find a significant drop in value added per worker in affected works council plants that is by 15.4 percentage points (column 3) larger. It supports the hypothesis that more labor hoarding in large council plants leads to a reduction of labor productivity. This is further supported when I include the number of employees as additional control variable in the exercise (column 4). In this exercise the triple difference-in-differences coefficient reduces to -0.104 and the significance vanishes.

The results of the triple difference-in-differences with revenue as dependent variable (Table 3.C.1) rule out the explanation, that the drop in labor productivity in large works council plants may result from larger drops in revenue that works council plants experienced during the crisis, which further reinforce the idea that labor productivity decreases as works council plants hoard more labor.

The results do not support the hypothesis that all works council plants differ in labor

Table 3.4: Labor productivity, triple difference-in-differences

	(1)	(2)
$T_{2008,2009}$	-0.015	-0.000
	(0.020)	(0.019)
$T_{2010,2011}$	-0.022	0.001
	(0.022)	(0.021)
$T_{2012,2013}$	-0.054**	-0.016
	(0.026)	(0.026)
$T_{2008,2009}$ × works council	0.038	0.033
	(0.031)	(0.030)
$T_{2010,2011}$ × works council	0.064*	0.056
	(0.038)	(0.037)
$T_{2012,2013}$ × works council	0.075*	0.062
	(0.043)	(0.043)
$T_{2008,2009} \times D_j$	-0.058**	-0.070***
	(0.024)	(0.024)
$T_{2010,2011} \times D_j$	-0.041	-0.073***
	(0.028)	(0.027)
$T_{2012,2013} \times D_j$	-0.020	-0.064**
	(0.032)	(0.031)
$T_{2008,2009}$ ×works council × D_j	-0.040	-0.040
	(0.038)	(0.037)
$T_{2010,2011}$ ×works council × D_j	-0.039	-0.036
	(0.045)	(0.044)
$T_{2012,2013}$ ×works council × D_j	-0.058	-0.050
	(0.052)	(0.051)
Other control variables	no	yes
R^2	0.009	0.040
N	10,398	10,398

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the value added divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery.

productivity changes during and after the crisis. They rather point at a nuanced picture that works councils can have a temporary negative effect, when labor is not adjusted

Table 3.5: Labor productivity by plant size, triple difference-in-differences

	< 100 employees		**≥ 100 employees**	
	(1)	(2)	(3)	(4)
$T_{2008,2009}$	0.004	0.015	-0.105**	-0.060
	(0.021)	(0.021)	(0.044)	(0.044)
$T_{2010,2011}$	0.002	0.027	-0.094*	-0.050
	(0.023)	(0.023)	(0.053)	(0.051)
$T_{2012,2013}$	-0.031	0.012	-0.100*	-0.044
	(0.028)	(0.028)	(0.058)	(0.057)
$T_{2008,2009}$ × works council	0.016	0.014	0.130**	0.093*
	(0.039)	(0.039)	(0.055)	(0.054)
$T_{2010,2011}$ × works council	0.043	0.038	0.134**	0.106*
	(0.056)	(0.054)	(0.065)	(0.063)
$T_{2012,2013}$ × works council	0.071	0.050	0.116	0.088
	(0.068)	(0.069)	(0.071)	(0.068)
$T_{2008,2009} \times D_j$	-0.074***	-0.076***	0.007	-0.046
	(0.026)	(0.026)	(0.058)	(0.058)
$T_{2010,2011} \times D_j$	-0.068**	-0.093***	0.035	-0.030
	(0.030)	(0.029)	(0.071)	(0.071)
$T_{2012,2013} \times D_j$	-0.047	-0.086**	0.043	-0.030
	(0.034)	(0.034)	(0.084)	(0.083)
$T_{2008,2009}$ ×works council × D_j	0.082	0.060	-0.154**	-0.104
	(0.052)	(0.052)	(0.070)	(0.068)
$T_{2010,2011}$ ×works council × D_j	0.050	0.028	-0.143*	-0.099
	(0.068)	(0.068)	(0.084)	(0.083)
$T_{2012,2013}$ ×works council × D_j	0.015	0.008	-0.157	-0.110
	(0.080)	(0.081)	(0.096)	(0.094)
Other control variables	no	yes	no	yes
R^2	0.007	0.044	0.026	0.057
N	6,807	6,807	3,591	3,591

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of of the value added divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery.

accordingly.

3.5.4 Wages

Table 3.6 presents the results of the logarithm of the wage bill per worker. The difference-in-difference coefficient is negative and statistically insignificant (column 1), which implies that affected plants do not reduce the wage bill per worker relative to unaffected ones. Interestingly the average wage bill is larger in the years 2012 and 2013. I further find no different adjustments of the wage bill per worker in affected works council plants relative to non-council plants. This does support the hypothesis that works council plants adjust wages not differently than non-council plants.

The triple interaction term of the years 2008–2009 in the wage regressions of small plant is positive and statistically significant for affected works council plants (Table 3.7, column 1) and loses statistical significance once other control variables are included (column 2). While this result is counter-intuitive with the above described mechanism, it would supports the idea that workers with higher wages (and tenure) remain occupied in works council plants, which increases the average wage bill. This may relate to the idea that low-tenured workers are more likely to leave works council plants, because this group is less represented by the works council (e.g. Hirsch *et al.* 2010).

In plants with more than 100 employees (columns 3 and 4) the difference-in-differences coefficients are statistically insignificant. This result supports the idea that large plants without a council adjust labor cost mainly at the extensive margin (as seen in the employment regressions) but not the intensive one. This result further supports the idea that larger wages in works council plants are explained by larger employer wage premia (Hirsch and Mueller 2020). Workers in works council plants do not bear the costs of economic shocks instead workers keep their employment at a constant wage rate.

Table 3.6: Wages, triple difference-in-differences

	(1)	(2)
$T_{2008,2009}$	-0.017	-0.011
	(0.012)	(0.012)
$T_{2010,2011}$	-0.019	-0.007
	(0.014)	(0.013)
$T_{2012,2013}$	-0.043***	-0.021
	(0.016)	(0.015)
$T_{2008,2009}$ × works council	0.020	0.021
	(0.018)	(0.017)
$T_{2010,2011}$ × works council	0.033	0.030
	(0.021)	(0.021)
$T_{2012,2013}$ × works council	0.060**	0.051**
	(0.025)	(0.025)
$T_{2008,2009} \times D_j$	-0.015	-0.022
	(0.015)	(0.015)
$T_{2010,2011} \times D_j$	-0.002	-0.019
	(0.017)	(0.017)
$T_{2012,2013} \times D_j$	0.046**	0.020
	(0.020)	(0.020)
$T_{2008,2009}$ ×works council × D_j	0.006	0.003
	(0.023)	(0.022)
$T_{2010,2011}$ ×works council × D_j	0.006	0.004
	(0.026)	(0.026)
$T_{2012,2013}$ ×works council × D_j	-0.028	-0.024
	(0.031)	(0.030)
Other control variables	no	yes
R^2	0.007	0.046
N	10,398	10,398

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the wage bill divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery.

Table 3.7: Wages by plant size, triple difference-in-differences

	< 100 employees		**≥ 100 employees**	
	(1)	(2)	(3)	(4)
$T_{2008,2009}$	-0.007	-0.001	-0.055	-0.036
	(0.013)	(0.012)	(0.038)	(0.034)
$T_{2010,2011}$	-0.007	0.008	-0.058*	-0.046
	(0.015)	(0.014)	(0.034)	(0.032)
$T_{2012,2013}$	-0.032*	-0.006	-0.075**	-0.053
	(0.017)	(0.016)	(0.036)	(0.035)
$T_{2008,2009}$ × works council	0.007	0.012	0.063	0.047
	(0.023)	(0.022)	(0.042)	(0.038)
$T_{2010,2011}$ × works council	0.014	0.012	0.079*	0.074*
	(0.030)	(0.029)	(0.041)	(0.038)
$T_{2012,2013}$ × works council	0.033	0.023	0.102**	0.091**
	(0.035)	(0.034)	(0.044)	(0.042)
$T_{2008,2009} \times D_j$	-0.024	-0.027*	0.018	-0.008
	(0.017)	(0.016)	(0.045)	(0.042)
$T_{2010,2011} \times D_j$	-0.016	-0.032*	0.046	0.020
	(0.019)	(0.019)	(0.044)	(0.042)
$T_{2012,2013} \times D_j$	0.036	0.010	0.068	0.035
	(0.023)	(0.022)	(0.045)	(0.044)
$T_{2008,2009}$ ×works council × D_j	0.058*	0.037	-0.050	-0.026
	(0.032)	(0.031)	(0.050)	(0.047)
$T_{2010,2011}$ ×works council × D_j	0.047	0.031	-0.060	-0.047
	(0.039)	(0.038)	(0.050)	(0.048)
$T_{2012,2013}$ ×works council × D_j	-0.010	-0.020	-0.062	-0.040
	(0.046)	(0.045)	(0.054)	(0.052)
Other control variables	no	yes	no	yes
R^2	0.006	0.050	0.015	0.050
N	6,807	6,807	3,591	3,591

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the wage bill divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery.

3.5.5 Profits

The results for profits per worker are shown in table 3.8. Plants affected by the crisis experience a reduction of profits per worker by 14.7 percentage points in the years 2008 and 2009. This result is robust when control variables are added (columns 1 and 2). I find no significant difference for council and non council plants, which speaks against the hypothesis that works council plants outperform non-council plants during the economic recovery.

The results for profits per worker are also in line with the results of the other economic outcomes for small and large plants. In small plants, the profits of affected plants drop during the crisis, but there is no statistically significant difference between affected council and non-council plants (Table 3.9, columns 1 and 2).

For large plants the results support the labor productivity findings. Large (non-council) plants being affected did not reduce profits per worker, because they (most likely) adjusted employment accordingly. The profits, however, change for those affected works council plants. Keeping workers and paying them the same wages and at the same time reducing labor productivity lead to a higher reduction of profits.

3.6 Selection, plant survivability, works council switchers

The identification rests on the assumption that there is no selection of works council plants into the treatment group. If works councils are, ceteris paribus, more often affected, potential results may only relate to the fact that works council plants reacted differently, because they were affected. In the most extreme case, all affected plants have a works council. Estimating triple difference-in-differences would be not feasible, because works council status uniquely identifies the affection of plants and differences of the respective groups could not be constructed. To make sure that I do not run into this problem I show in table 3.10 that works council presence is not associated with a higher likelihood of reporting of being affected, for all plants and the two different size groups.

Workers are more prone to introduce works councils, when the plant faces economic

Table 3.8: Profits, triple difference-in-differences

	(1)	(2)
$T_{2008,2009}$	-0.016	0.014
	(0.057)	(0.057)
$T_{2010,2011}$	0.011	0.057
	(0.064)	(0.063)
$T_{2012,2013}$	-0.043	0.025
	(0.071)	(0.071)
$T_{2008,2009}$ × works council	0.097	0.084
	(0.079)	(0.079)
$T_{2010,2011}$ × works council	0.043	0.025
	(0.099)	(0.099)
$T_{2012,2013}$ × works council	0.091	0.073
	(0.105)	(0.105)
$T_{2008,2009} \times D_j$	-0.147**	-0.167**
	(0.073)	(0.073)
$T_{2010,2011} \times D_j$	-0.154*	-0.208***
	(0.080)	(0.080)
$T_{2012,2013} \times D_j$	-0.176**	-0.242***
	(0.089)	(0.088)
$T_{2008,2009}$ ×works council × D_j	-0.100	-0.093
	(0.104)	(0.104)
$T_{2010,2011}$ ×works council × D_j	-0.032	-0.022
	(0.122)	(0.122)
$T_{2012,2013}$ ×works council × D_j	-0.093	-0.086
	(0.134)	(0.134)
Other control variables	no	yes
R^2	0.006	0.015
N	10,398	10,398

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the value added minus labor costs divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery.

adverse times (Oberfichtner 2019). If works council introductions depend on the affection of the crisis, then those plants may potentially bias outcomes. For example, if an affected

Table 3.9: Profits by plant size, triple difference-in-differences

	< 100 employees		**≥ 100 employees**	
	(1)	(2)	(3)	(4)
$T_{2008,2009}$	0.026	0.051	-0.241*	-0.178
	(0.062)	(0.063)	(0.125)	(0.131)
$T_{2010,2011}$	0.062	0.112	-0.139	-0.078
	(0.070)	(0.070)	(0.132)	(0.132)
$T_{2012,2013}$	-0.001	0.084	-0.109	-0.044
	(0.081)	(0.080)	(0.129)	(0.129)
$T_{2008,2009}$ × works council	0.020	0.007	0.342**	0.290*
	(0.105)	(0.108)	(0.145)	(0.150)
$T_{2010,2011}$ × works council	-0.074	-0.086	0.228	0.184
	(0.146)	(0.148)	(0.161)	(0.160)
$T_{2012,2013}$ × works council	0.011	-0.032	0.186	0.152
	(0.164)	(0.168)	(0.155)	(0.153)
$T_{2008,2009} \times D_j$	-0.192**	-0.194**	0.063	-0.004
	(0.080)	(0.080)	(0.171)	(0.173)
$T_{2010,2011} \times D_j$	-0.220**	-0.260***	0.008	-0.081
	(0.089)	(0.088)	(0.177)	(0.176)
$T_{2012,2013} \times D_j$	-0.236**	-0.293***	-0.051	-0.151
	(0.101)	(0.100)	(0.189)	(0.186)
$T_{2008,2009}$ ×works council × D_j	0.149	0.126	-0.399**	-0.335*
	(0.142)	(0.145)	(0.197)	(0.196)
$T_{2010,2011}$ ×works council × D_j	0.242	0.209	-0.281	-0.217
	(0.178)	(0.181)	(0.209)	(0.209)
$T_{2012,2013}$ ×works council × D_j	0.198	0.184	-0.347	-0.281
	(0.209)	(0.212)	(0.222)	(0.218)
Other control variables	no	yes	no	yes
R^2	0.005	0.017	0.017	0.026
N	6,807	6,807	3,591	3,591

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the value added minus labor costs divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery.

plant introduces a works council after the majority of workers has been laid off (as a reaction to the crisis) the triple DID coefficient of the works council may underestimate the works council effect on employment. Alternatively, works council introduction may be a reaction to the Great Recession leading to self-selection of works council plants into the treatment group. To deal with this I exclude plants that experience a works council status change between 2006 and 2013 in my main regressions. In total there are 655 plant-year observations that belong to plants which change their works council status at least once. In appendix 3.D I show the results of exercises that include plants with a change in council status (Appendix Tables 3.D.1, 3.D.2, 3.D.3, 3.D.4). The coefficients change only slightly, ruling out that the findings are driven by some plants that change their works council status.

If works council plants affected by the crisis are more likely to exit than non-council plants (or vice versa), the results may be biased by selective panel attrition. For example, Jensen and Meckling (1979) suggest that delayed decisions may make it more likely that works council plants exit more often when affected by the crisis. Up to this point evidence on whether works councils increase the likelihood of plant closings is mixed. Results depend on plant heterogeneity such as collective bargaining or ownership structure (e.g. Addison *et al.* 2004, Jirjahn 2011) and most recently Addison *et al.* (2019) have shown that plants with dissonant works councils are more likely to close. However, generalizations to all works councils cannot be inferred because non-dissonant works councils are not associated with a higher likelihood of plant closings (Addison *et al.* 2019). With the chosen definition of crisis affection, I cannot examine whether affected works council plants are more likely to leave in 2008 or 2009 (I use information from 2010). However, I find no difference in the exit probability of affected works council and non-council plants after they reported it in 2010. While this measure is not perfect, it tends to refute the assumption that delayed decisions of affected works council causes them to exit the market.

Table 3.10: Average marginal effects from probit regressions for the probability of being affected by the Great Recession, based on the years 2006 and 2007

	All	< 100 employees	≥ 100 employees
Works council	-0.005	-0.019	0.050
	(0.031)	(0.040)	(0.049)
Collective wage agreement	0.026	0.046	-0.041
	(0.026)	(0.033)	(0.039)
Log(capital intensity)	-0.019	-0.010	-0.026
	(0.012)	(0.016)	(0.019)
Log(employment)	0.031*	0.065*	-0.048
	(0.017)	(0.035)	(0.043)
Export status	0.093***	0.085***	0.109**
	(0.026)	(0.032)	(0.046)
Share of skilled workers	-0.060	-0.025	-0.123
	(0.054)	(0.070)	(0.080)
Share of part-time workers	-0.152*	-0.198*	-0.040
	(0.085)	(0.111)	(0.128)
Share of apprentices	-0.167	-0.229	0.001
	(0.235)	(0.274)	(0.504)
Share of women	-0.122	-0.159*	-0.046
	(0.076)	(0.097)	(0.121)
Single establishment	-0.000	0.033	-0.024
	(0.027)	(0.038)	(0.036)
Log Likelihood	-1423.155	-909.6513	-439.7849
N	2,488	1,559†	893†

Notes: IAB Establishment Panel. 2006 and 2007. ***/**/* denotes statistical significance at the 1%/5%/10% level. Marginal effecets of a probit regression. The dependent variable is the dummy indicating whether the plant has been affected by the Great Recession as described in section (3.3). Further covariates included are 15 federal state dummies, 37 two-digit sector dummies and three dummies capturing the technical state of machinery.

† In total 36 observations are omitted because they perfectly predict the outcome.

3.7 Conclusion

This paper explores the role of works councils on plant-level responses during an economy-wide shock and how this translates into differences in employment, labor productivity, wage bill and profit growth. I use plant-level data between 2006 and 2013 and exploit subjective survey information to identify whether a plant has been affected by the Great Recession of 2008 and 2009.

I find that in plants with more than 100 employees, the presence of works councils is associated with smaller reductions in employment during the Great Recession. This smaller decrease is connected to larger reductions in labor productivity, but no larger productivity growth rates in the first years of the economic recovery. It supports the suggestion previously made (e.g. Mueller 2012) that genuine productivity effects of works councils are potentially underestimated during times of economic crises. While large affected works council plants hoard more labor, the average wage bill per worker stays unchanged. It seems that those plants bear the costs of labor hoarding. Since I control for plant fixed effects in the wage regressions, my results support the idea that higher wages in council plants are explained by employer wage premia (Hirsch and Mueller 2020), which are rather time-consistent. Given that the presence of a works council aggravates the reduction in labor productivity, while employment decreases only slightly and average wages remain constant, these plants also suffer larger reductions in profits per worker.

It is important to stress that the different hypotheses are neither supported when considering all plants nor when analyzing those with less than 100 employees. It backs, on the one hand, legitimate reasons that works councils may develop their full potential once a certain employment threshold is reached. On the other hand, it does not rule out that the chosen sample and method lacks statistical power, because the estimated coefficients are plausible but statistically insignificant.

For this reason, it is important to examine the role of plant size in terms of effects, because descriptive statistics and previous cross-sectional studies show that works councils are also relevant in small plants. Furthermore, this paper shows a multifaceted picture of works councils and that average results and dynamic results shown can be in conflict with each other. In particular, it should be investigated to what extent works councils function with other forms of co-determination, i.e. at the board level level, or with management practices. These aspects should be considered carefully, especially at times of the COVID-19 Pandemic in which co-determination is a hot topic in research and politics alike.

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Appendix

3.A Definitions of variables

Table 3.A.1: Definitions of variables

Variable	**Definition**
Log(employment)	Logarithm of the number of workers
Log(value added per worker)	Logarithm of the value added per worker
Log(wage bill per worker)	Logarithm of the wage bill per worker
Log(profit per worker)	Logarithm of the value added net of wage costs (including employers' social security contributions) per worker
Log(revenue per worker)	Logarithm of the revenue per worker
Log(revenue)	Logarithm of the revenue
Works council	= 1 if a works council is present, = 0 if no works council is present
Affected by the crisis	= 1 if plant reported being affected by the Great Recession = 0 if plant did not being affected by the Great Recession
Log(capital intensity)	Logarithm of the capital stock per worker
Collective bargaining	= 1 if collective bargaining is present, = 0 if no collective bargaining is present
Exporting status	= 1 if plant makes revenue abroad, = 0 if plant does not make revenue abroad
Single plant	= 1 if the plant is an independent company or an independent organization without any other places of business, = 0 if plant does have other/belongs to other branches
Technical state of machinery	Assessment of the overall state of the technical state of the plant and machinery compared to other plants in the same industry. Scale from 1 to 5. (excellent = 1, good = 2, fair = 3, poor = 4 and 5)
Joining rate	share of workers that join the plant
Leaving rate	share of workers that leave the plant
Churning rate	Measure of employment stability. Worker flow rate minus the absolute value of the net rate of employment change.
Skilled employees as share of all employees	Share of workers who have a vocational qualification, relevant professional experience or an university degree
Part-time employees as share of all employees	Share of part-time workers
Share of apprenticestices as share of all employees	Share of workers who are doing their vocational training under the vocational training law or the Handicrafts Regulation Act and other training stipulations of all workers
Female employees as share of all employees	Share of women

Notes: IAB Establishment Panel.

3.B Descriptive statistics by plant size

Table 3.B.1: Descriptive statistics by plant size

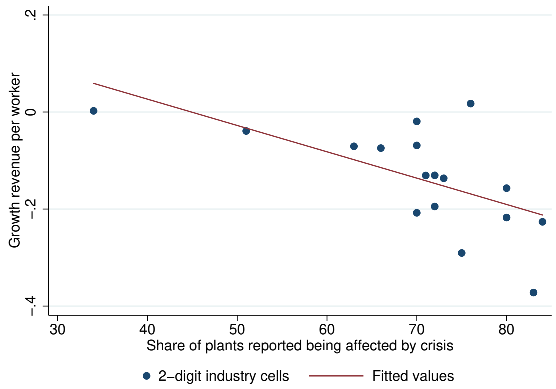
	< 100 employees		≥ 100 employees	
	Works council	no Works council	Works council	no Works council
Variable	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Log(employment)	3.997 (0.417)	3.635 (0.431)	5.352 (0.458)	5.122 (0.410)
Log(value added per worker)	11.107 (0.594)	10.834 (0.586)	11.234 (0.560)	10.801 (0.684)
Log(wage bill per worker)	10.248 (0.360)	10.011 (0.431)	10.349 (0.353)	9.982 (0.444)
Log(profits per worker)	10.056 (1.221)	9.752 (1.244)	10.238 (1.216)	9.706 (1.327)
Log(revenue per worker)	11.918 (0.802)	11.632 (0.779)	12.143 (0.727)	11.604 (0.941)
Log(revenue)	15.915 (0.915)	15.267 (0.901)	17.495 (0.897)	16.726 (1.006)
Affected by the crisis (dummy)	0.641 (0.480)	0.579 (0.494)	0.705 (0.456)	0.567 (0.496)
Log(capital stock per worker)	10.841 (1.227)	10.529 (1.172)	11.160 (1.052)	10.520 (1.534)
Collective bargaining (dummy)	0.612 (0.487)	0.242 (0.428)	0.734 (0.442)	0.304 (0.460)
Exporting status (dummy)	0.513 (0.500)	0.481 (0.499)	0.770 (0.421)	0.610 (0.488)
Single establishment (dummy)	0.602 (0.490)	0.836 (0.371)	0.549 (0.498)	0.699 (0.459)
Technical state of machinery				
= excellent (dummy)	0.154 (0.361)	0.189 (0.392)	0.162 (0.368)	0.302 (0.459)
= good (dummy)	0.448 (0.497)	0.531 (0.499)	0.520 (0.500)	0.545 (0.498)
= fair (dummy)	0.342 (0.474)	0.258 (0.437)	0.292 (0.455)	0.148 (0.356)
= poor (dummy)	0.056 (0.231)	0.022 (0.146)	0.026 (0.160)	-†
Churning rate	0.029 (0.057)	0.045 (0.106)	0.030 (0.043)	0.088 (0.158)
Share of skilled workers	0.770 (0.234)	0.740 (0.233)	0.731 (0.231)	0.695 (0.274)
Share of part-time workers	0.111 (0.170)	0.140 (0.179)	0.094 (0.152)	0.132 (0.191)
Share of apprentices	0.038 (0.049)	0.054 (0.062)	0.047 (0.035)	0.045 (0.042)
Share of women	0.265 (0.211)	0.316 (0.246)	0.261 (0.196)	0.353 (0.241)
N	1,472	5,335	2,654	937

Notes. IAB Establishment Panel. 2006–2013. Plants with 20–500 employees. Affected by the crisis (dummy) are all plant-year observations of plants that reported in 2010 being negatively affected by the financial and economic crisis.
-† The value is not shown due to reasons of data protection.

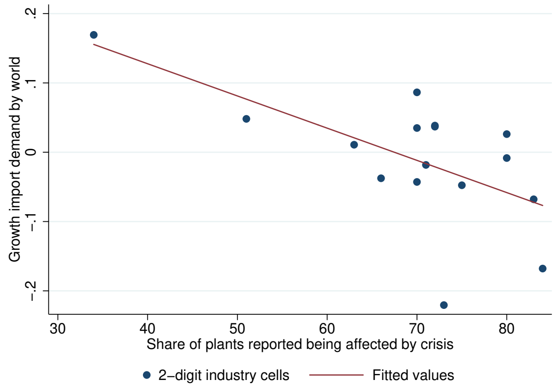
3.C Checks of being affected

Figure 3.C.1: Growth rates between 2007 and 2009 and share of plants that reported being affected

A: Growth rate of revenue per worker and being affected



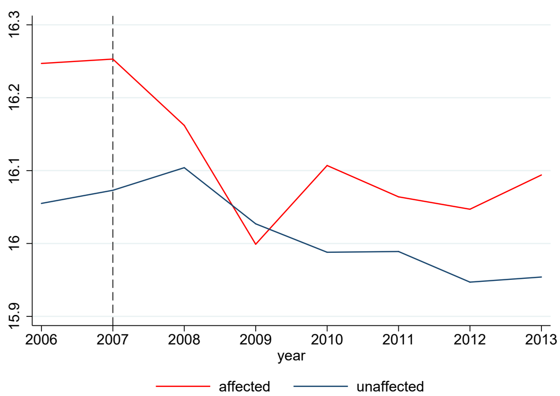
B: Growth rate of world demand



Notes: IAB Establishment Panel. German Statistical Offices. UN ComTrade Database. The graphs show on the X-axis the share of plants that reported in 2010 being affected by the Great Recession by two-digit manufacturing industries. Panel A: the y-axis depicts the growth rate of global imports, excluding imports from Germany, between 2007 and 2009 to measure the reduction of global demand by the 2-digit industries (Aghion *et al.* 2021). Panel B: the y-axis shows the average growth rate of revenue per worker between 2007 and 2009.

Figure 3.C.2: Log(revenue) over time by affected and unaffected

A: All



Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. 10,398 plant-year observations. The logarithm of revenue by plants that reported in 2010 to be affected by the Great Recession.

Table 3.C.1: Revenue by plant size, triple difference-in-differences

	< 100 employees		**≥ 100 employees**	
	(1)	(2)	(3)	(4)
$T_{2008,2009}$	0.022*	-0.007	0.069***	0.004
	(0.013)	(0.011)	(0.023)	(0.022)
$T_{2010,2011}$	0.067***	0.008	0.137***	0.044*
	(0.017)	(0.014)	(0.027)	(0.025)
$T_{2012,2013}$	0.078***	-0.014	0.142***	0.013
	(0.022)	(0.017)	(0.039)	(0.031)
$T_{2008,2009}$ × works council	-0.002	0.028	-0.009	0.029
	(0.026)	(0.023)	(0.029)	(0.028)
$T_{2010,2011}$ × works council	-0.002	0.036	-0.031	0.027
	(0.033)	(0.027)	(0.034)	(0.032)
$T_{2012,2013}$ × works council	-0.051	0.017	-0.007	0.048
	(0.045)	(0.034)	(0.047)	(0.039)
$T_{2008,2009} \times D_j$	-0.078***	-0.077***	-0.166***	-0.098***
	(0.017)	(0.015)	(0.035)	(0.031)
$T_{2010,2011} \times D_j$	-0.114***	-0.078***	-0.194***	-0.094**
	(0.022)	(0.019)	(0.042)	(0.037)
$T_{2012,2013} \times D_j$	-0.108***	-0.061***	-0.167***	-0.075
	(0.028)	(0.023)	(0.057)	(0.049)
$T_{2008,2009}$ ×works council × D_j	-0.016	-0.021	0.024	-0.036
	(0.036)	(0.032)	(0.042)	(0.037)
$T_{2010,2011}$ ×works council × D_j	-0.049	-0.048	0.050	-0.022
	(0.047)	(0.040)	(0.049)	(0.043)
$T_{2012,2013}$ ×works council × D_j	-0.047	-0.058	0.013	-0.031
	(0.060)	(0.047)	(0.066)	(0.057)
Other control variables	no	yes	no	yes
R^2	0.024	0.210	0.054	0.244
N	6,807	6,807	3,591	3,591

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of revenue. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, employment, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and the technical state of machinery.

3.D Works council switchers

As discussed in sections 3.3 and 3.6, works councils can change their status. The results in tables 3.D.1,3.D.2, 3.D.3 and 3.D.4 include 655 plant-year observations of plants that change their status throughout the observed period. Works council status change include dissolution and introductions of works councils.

Table 3.D.1: Employment, triple difference-in-differences, with works council switchers

	all plants		**< 100 employees**		**≥ 100 employees**	
	(1)	(2)	(3)	(4)	(5)	(6)
$T_{2008,2009} \times D_j$	-0.027**	-0.005	-0.009	0.002	-0.097***	-0.051**
	(0.012)	(0.009)	(0.012)	(0.010)	(0.030)	(0.024)
$T_{2010,2011} \times D_j$	-0.087***	-0.047***	-0.062***	-0.034**	-0.143***	-0.103***
	(0.016)	(0.013)	(0.017)	(0.014)	(0.037)	(0.031)
$T_{2012,2013} \times D_j$	-0.114***	-0.057***	-0.089***	-0.047***	-0.149***	-0.103***
	(0.020)	(0.017)	(0.020)	(0.017)	(0.044)	(0.038)
$T_{2008,2009}$ ×works council × D_j	0.020	0.019	0.003	0.031	0.092***	0.054*
	(0.018)	(0.015)	(0.023)	(0.020)	(0.034)	(0.028)
$T_{2010,2011}$ ×works council × D_j	0.024	0.026	-0.003	0.025	0.098**	0.079**
	(0.024)	(0.020)	(0.034)	(0.029)	(0.042)	(0.034)
$T_{2012,2013}$ ×works council × D_j	0.018	0.010	-0.004	0.018	0.078	0.048
	(0.033)	(0.027)	(0.041)	(0.037)	(0.051)	(0.044)
Other control variables	no	yes	no	yes	yes	no
R^2	0.054	0.360	0.060	0.323	0.071	0.318
N	11,053	11,053	7,244	7,244	3,809	3,809

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery. Plants included that change their works council status.

Table 3.D.2: Labor productivity, triple difference-in-differences, with works council switchers

	all plants		< 100 employees		≥ 100 employees	
	(1)	(2)	(3)	(4)	(5)	(6)
$T_{2008,2009} \times D_j$	-0.059**	-0.070***	-0.072***	-0.076***	0.001	-0.045
	(0.023)	(0.023)	(0.026)	(0.025)	(0.054)	(0.053)
$T_{2010,2011} \times D_j$	-0.039	-0.067**	-0.068**	-0.093***	0.056	-0.000
	(0.027)	(0.026)	(0.029)	(0.028)	(0.067)	(0.066)
$T_{2012,2013} \times D_j$	-0.017	-0.058*	-0.041	-0.080**	0.038	-0.024
	(0.031)	(0.030)	(0.033)	(0.033)	(0.079)	(0.077)
$T_{2008,2009}$ ×works council × D_j	-0.040	-0.037	0.078	0.065	-0.154**	-0.109*
	(0.036)	(0.036)	(0.049)	(0.050)	(0.066)	(0.064)
$T_{2010,2011}$ ×works council × D_j	-0.048	-0.045	0.047	0.033	-0.175**	-0.136*
	(0.043)	(0.042)	(0.064)	(0.063)	(0.079)	(0.079)
$T_{2012,2013}$ ×works council × D_j	-0.060	-0.053	0.022	0.010	-0.159*	-0.119
	(0.049)	(0.049)	(0.075)	(0.076)	(0.091)	(0.089)
Other control variables	no	yes	no	yes	yes	no
R^2	0.009	0.039	0.006	0.042	0.029	0.060
N	11,053	11,053	7,244	7,244	3,809	3,809

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the value added divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery. Plants included that change their works council status.

Table 3.D.3: Wages, triple difference-in-differences, with works council switchers

	all plants		**< 100 employees**		**≥ 100 employees**	
	(1)	(2)	(3)	(4)	(5)	(6)
$T_{2008,2009} \times D_j$	-0.014	-0.020	-0.019	-0.023	0.012	-0.007
	(0.015)	(0.015)	(0.017)	(0.016)	(0.042)	(0.039)
$T_{2010,2011} \times D_j$	-0.003	-0.019	-0.016	-0.032*	0.045	0.026
	(0.017)	(0.016)	(0.019)	(0.018)	(0.040)	(0.039)
$T_{2012,2013} \times D_j$	0.047**	0.021	0.037*	0.012	0.073*	0.046
	(0.020)	(0.019)	(0.022)	(0.021)	(0.043)	(0.041)
$T_{2008,2009}$ ×works council × D_j	0.006	0.004	0.053*	0.038	-0.043	-0.024
	(0.022)	(0.021)	(0.031)	(0.030)	(0.047)	(0.044)
$T_{2010,2011}$ ×works council × D_j	0.010	0.008	0.056	0.045	-0.059	-0.050
	(0.025)	(0.025)	(0.037)	(0.036)	(0.047)	(0.045)
$T_{2012,2013}$ ×works council × D_j	-0.035	-0.031	-0.014	-0.023	-0.072	-0.055
	(0.030)	(0.029)	(0.043)	(0.043)	(0.051)	(0.050)
Other control variables	no	yes	no	yes	yes	no
R^2	0.009	0.047	0.009	0.051	0.016	0.051
N	11,053	11,053	7,244	7,244	3,809	3,809

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the wage bill divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery. Plants included that change their works council status.

Table 3.D.4: Profits, triple difference-in-differences, with works council switchers

	all plants		**< 100 employees**		**≥ 100 employees**	
	(1)	(2)	(3)	(4)	(5)	(6)
$T_{2008,2009} \times D_j$	-0.152**	-0.171**	-0.190**	-0.195**	0.002	-0.053
	(0.070)	(0.070)	(0.078)	(0.078)	(0.159)	(0.161)
$T_{2010,2011} \times D_j$	-0.146*	-0.195**	-0.221**	-0.260***	0.068	-0.005
	(0.077)	(0.077)	(0.086)	(0.085)	(0.165)	(0.164)
$T_{2012,2013} \times D_j$	-0.149*	-0.211**	-0.210**	-0.267***	-0.015	-0.101
	(0.086)	(0.085)	(0.098)	(0.097)	(0.180)	(0.177)
$T_{2008,2009}$ ×works council × D_j	-0.086	-0.077	0.174	0.160	-0.352*	-0.294
	(0.101)	(0.101)	(0.139)	(0.142)	(0.186)	(0.185)
$T_{2010,2011}$ ×works council × D_j	-0.048	-0.036	0.234	0.211	-0.355*	-0.301
	(0.117)	(0.117)	(0.171)	(0.173)	(0.198)	(0.198)
$T_{2012,2013}$ ×works council × D_j	-0.091	-0.085	0.207	0.185	-0.365*	-0.307
	(0.130)	(0.130)	(0.199)	(0.203)	(0.213)	(0.211)
Other control variables	no	yes	no	yes	yes	no
R^2	0.006	0.014	0.004	0.016	0.018	0.028
N	11,053	11,053	7,244	7,244	3,809	3,809

Notes: IAB Establishment Panel. 2006–2013. 20–500 employees. ***/**/* denotes statistical significance at the 1%/5%/10% level. The dependent variable is the logarithm of the value added minus labor costs divided by the number of employees. The base category of time dummy T is $T_{2006,2007}$. D_j indicates if a plant is affected by the Great Recession (section 3.3). Clustered standard errors at the plant level are in parentheses. Regressions include plant fixed effects. Other control variables are the logarithm of capital intensity, logarithm of the number of employees, the share of skilled employees, part-time, apprentices and women of all employees, churning rate, collective bargaining, export, single establishment status and three dummies capturing the technical state of machinery. Plants included that change their works council status.

Chapter 4

Identifying rent-sharing using firms' energy input mix[1]

4.1 Introduction

Studies documenting that equally productive workers are paid different wages by different employers are legion (Abowd *et al.* 1999, Card *et al.* 2013, Card *et al.* 2016, Song *et al.* 2019, and the survey in Card *et al.* 2018). Persistent firm wage differentials point at imperfect labor markets where employers and workers possess market power in the wage formation process and employment rents accrue to workers and employers. Despite a number of early studies point at employer wage setting power (Robinson 1933, Slichter 1950), for long, most economists relied on the workhorse model of competitive labor markets to explain wage inequality. However, the last decade witnessed a strong revival of studies incorporating the notion of imperfect labor markets into analyzes of the role of firms and worker-firm bargaining in wage inequality (e.g. Card *et al.* 2018, Goldschmidt and Schmieder 2017, Sorkin 2018). Analyzes of worker-firm rent-sharing processes offer a potentially important explanation for between-firm wage differences of workers with similar skills and within comparable occupations. Understanding how workers and firms share employment rents is thus key to this literature.

Many early studies estimating sector-level relationships between performance and

[1] This chapter is joint work with Matthias Mertens and Steffen Müller. JEL-Classification: J30, P18, C26. Keywords: Rent-sharing, Wage inequality, Bartik instrument, Shift-share analysis, Energy markets.

wages (e.g. Christofides and Oswald 1992, Blanchflower *et al.* 1996) may suffer from general-equilibrium effects. In particular, variation in sector-level performance directly impacts sector-level market wages and, thus, workers' outside options.[2] Outside options are part of the right-hand-side of the standard bargaining solution and are typically unobserved in empirical studies. Hence, unobserved variation in the outside option directly biases estimates of the rent-sharing elasticity. Variation in the performance of individual firms, however, leaves workers' outside options unaffected. In a recent survey of the rent-sharing literature, Card *et al.* (2018) conclude that there is a particularly strong need for causal estimates of rent-sharing elasticities based on *firm-level* productivity shocks. Our article provides exactly such evidence. Of course, endogeneity concerns are crucial at the firm level, too. They include, for instance, unobserved differences in worker quality, affecting wages and firm performance, or reversed causality when efficiency wages drive firm performance.

Finding exogenous idiosyncratic firm-level productivity shocks is notoriously difficult and thus credible studies are scarce. What is more, in a standard production function setting with a constant output elasticity of labor and without frictions at labor and product markets, even firm-level demand shocks will cause adjustments in employment that leave rents per worker and wages unaffected (Abowd and Lemieux 1993). In such a setting and in settings close to it, instruments for revenue function shifters will be weak. Thus, the challenge is to find an instrument that acts in an environment with strong imperfections in product and labor markets. Van Reenen (1996) and Kline *et al.* (2019) use major innovations and patents as instruments for firm performance. These innovation based settings are prototypical for a setting with product market imperfections and workers possessing firm-specific human capital. In fact, Kline *et al.* (2019) directly argue that their patent allowance setting captures monopoly rents. However, by focusing on innovations, both papers zoom into a relatively small and selected sub-sample of the economy. Correspondingly, Kline *et al.* (2019) argue that their focus on innovative firms

[2] Outside options are often modeled in terms of unemployment benefits instead of outside wages. Importantly, Jäger *et al.* (2020) show that unemployment benefits are unlikely to define workers outside options.

may be directly responsible for their large estimates of the rent-sharing elasticity.

Instead of relying on innovation outcomes, we use exogenous changes in input costs at the firm level and employ a large and representative sample of German manufacturing firms. A general advantage of using cost shocks as an instrument instead of revenue shifters or measures that shift firms' revenue productivity at constant costs (like certain innovation outcomes) is that cost shocks will be largely unobservable for workers and are thus less likely to have a direct effect on worker effort. The German economy is a particular interesting case as a series of studies have shown that Germany is characterized by strong labor market imperfections (e.g. Dobbelaere *et al.* 2020, Hirsch and Mueller 2020). Our setting leverages on the strong rigidities in the German labor market, making employment adjustments in response to input cost shocks costly and thus incomplete. Recent findings of a decline in firm responsiveness to productivity shocks observed in US data (Decker *et al.* 2020) imply that such a setting might be increasingly relevant in the US, too. In fact, Kline *et al.* (2019) conclude that US workers in innovating firms who are hardest to replace capture the largest fraction of rents.

More concretely, we develop a new Bartik instrument for firm value added per full time equivalent that uses firms' exact energy consumption by energy carrier (in kilowatt-hours). The instrument combines the (predetermined) energy carrier mix at the firm level with economy-wide changes in energy prices by energy carrier. A price change of a certain energy carrier affects firms more the more intensively they use this carrier.[3] We assume that firms act as price takers on energy markets and that economy-wide changes in the cost for individual energy carriers do not affect workers' wages directly. We condition on industry and region $\times$ year fixed effects and thus use identifying variation across firms operating in the same labor market. This firm-level perspective addresses the important issue of correlated shocks influencing market-level wages (i.e. outside options) that plagues analyzes using productivity shocks at the sector or regional level. To address any biases from unobserved heterogeneity in firm or worker characteristics, the model is estimated

[3] Other studies use changes in observed energy costs as instrument for firm performance (e.g. Blanchflower *et al.* 1996, Arai and Heyman 2009) In contrast to them, we do not rely on total energy costs as these are at least partly under the control of the firm and thus endogenous.

in first differences and we conduct additional analyzes using firm-worker-level data. The new instrument passes the recently developed plausibility checks for Bartik instruments (Goldsmith-Pinkham *et al.* 2020, henceforth GPSS).

First stage F-values of around 30 indicate that our instrument is a strong predictor of changes in firms' labor productivity. Our instrumental variables estimator yields a rent-sharing elasticity of about 0.20, implying that a 10 percent increase in firms' labor productivity increases wages by about 2 percent. This estimate is close to the average estimate in the firm-level studies reviewed by Card *et al.* (2018), but substantially below the innovation-based estimates provided by Van Reenen (1996) and Kline *et al.* (2019) that mark the upper end of the distribution of estimates in the rent-sharing literature. Interestingly, we do not find evidence for the notion that innovative firms and non-innovative firms differ in their rent-sharing elasticities. Confirming recent studies (Mertens 2021, Wong 2020), we find that the rent-sharing elasticity is substantially bigger in small firms (0.31) compared to large firms (0.14). In addition, we document a small decline in the rent-sharing elasticity over time. In terms of transmission channels, we find that energy cost shocks reduce labor productivity by reducing total sales and intermediate input use but leave total employment unaffected. Leveraging on additional linked employer-employee-data and a difference-in-differences setting, we find no effects of the instrument on workforce composition, hours worked, and worker tenure, the latter lending support to the notion that our rent-sharing estimates are primarily based on repeated observations of the same worker-firm matches.

The remainder of this study is structured as follows: Section 4.2 discusses related literature. Section 4.3 present our firm-level data on productivity, wages, and energy use. Section 4.4 discusses our empirical strategy and the novel Bartik instrument. Section 4.5 shows our results, scrutinizes the plausibility of our new instrument, and discusses effect heterogeneity. Section 4.6 concludes.

4.2 Related literature

A robust finding in the literature is that wages vary with firm performance. Yet, depending on specifications and datasets, quantitative estimates of rent-sharing elasticities vary widely. A concern that can explain the wide range of estimates is that, despite the considerable progress in the rent-sharing literature, many studies still lack plausibly exogenous variation in firm performance to estimate rent-sharing elasticities. In the following, we thus restrict ourselves to review studies that identify the rent-sharing elasticity by using exogenous productivity shocks varying at the level of individual firms as opposed to studies using aggregated variation. Hence, we do not consider studies employing variation in firm performance shared by many or even all firms in a certain labor market (e.g. same sector or region) as such variation directly affects workers' outside options.[4] We also do not discuss structural approaches as such studies typically do not employ exogenous variation in firm performance.[5]

The two seminal studies closest to our article are Van Reenen (1996) and Kline *et al.* (2019) who both use instrumental variables carrying arguably exogenous firm-level variation in firm performance that originates from uncertainties in the innovation process. In a sample of 600 firms, Van Reenen (1996) uses information on firms' innovations as an instrument for firms' quasi rents. The key assumption is that, apart from raising performance, innovations have no impact on wages on their own. Possible violations of this assumption include efficiency wages spurring innovations or expected changes in firms' product demand both affecting wages and the timing of innovations. Van Reenen (1996, Table III) reports OLS estimates of 0.11 indicating a wage increase of 1.1 percent if quasi rents increase by 10 percent. The (static) IV estimates point at a substantially higher elasticity of 0.29.

Kline *et al.* (2019) use initial patent decisions of the US patent office as an instrument

[4] Card *et al.* (2018) review sector-level studies. Berger *et al.* (2018) and Fuest *et al.* (2018) are recent examples for studies using regional variation in business taxes.

[5] For instance, Lamadon *et al.* (2019) estimate the relationship between productivity and wages within a difference-in-difference setting but without resorting to exogenous variation in productivity. Similarly, Friedrich *et al.* (2019) define residuals from firm-level productivity regressions as productivity shocks without employing a causal identification strategy.

for firm surplus. They provide suggestive evidence that patent decisions are unrelated to counterfactual changes in firm surplus and that patent decisions are hard to anticipate. Kline *et al.* (2019, Table VIII) report OLS estimates for the value-added based rent-sharing elasticity of 0.15. The corresponding IV estimate of 0.47 is large compared to the studies surveyed in Card *et al.* (2018) but similar to Van Reenen (1996) when taking into account that value-added based estimates yield on average twice as high rent-sharing elasticities compared to quasi-rent-based approaches (Card *et al.* (2018).[6] Kline *et al.* (2019) also report that elasticities are smaller for non-inventors within inventing firms (Table VIII, columns 5 and 6), which suggests that rent sharing is driven by inventors and might thus be lower in non-innovating firms. Still, it is an open question whether the exceptionally high rent-sharing estimates in Van Reenen (1996) and Kline *et al.* (2019) are caused by the superior research design or by resorting to innovation-based variation (or both). Our study can help to answer this question it resorts on exogenous firm-level variation, too, but is not relying on innovation outcomes.

There are further studies using instrumental variable approaches. For instance, Carlsson *et al.* (2016) use physical total factor productivity (TFPQ) as an instrument for labor productivity but acknowledge that investments in firm TFPQ may be the consequence of rising wages (see for instance Nguyen 2019 on productivity effects of the minimum wage). Arai and Heyman (2009) use multiple instruments that are, however, all choice variables to the firm. For instance, energy costs (as opposed to the shift-share instrument on energy usage we apply) reflect firms' input decisions and are therefore endogenous. Other instruments as e.g. foreign sales or pricing are also at least partly controlled by the firm. Some papers rely on (dynamic) panel estimators in which lagged differences and levels of firm performance, respectively, provide valid technical instruments if the panel model is dynamically complete. Yet, either these studies have to rely on sector-level wage information (Hildreth and Oswald 1997), demanding assumptions to distinguish between permanent and transitory shocks plus the notoriously critical timing assumptions

[6] Notably, first stage F-statistics are relatively small (8.99 for the value-added specification) and small deviations from the exogeneity assumptions could therefore considerably influence IV estimates.

for using productivity lags as instruments (Guiso *et al.* 2005), or specification tests directly reject the panel model's dynamic completeness (Gürtzgen 2009).[7]

Finally, Saez *et al.* (2019) analyze a nation-wide payroll tax cut for young workers in Sweden employing a difference-in-differences setting. Without reporting a rent-sharing elasticity, they find that young workers' wages did not rise disproportionally and that the resulting decline in firms' total wage costs is shared with all workers in the firm. By comparing firms that are differently affected by the tax cut because of employing many versus few young workers, the variation used in their setting is similar to ours in the sense that input costs change differently across firms because of different initial conditions.

4.3 Data

We use administrative yearly panel data on German manufacturing plants from 2003 to 2017. The data are supplied by the statistical offices of Germany and consist of two complementary data sets. One is a firm-level data set called "cost structure survey" which contains information on firms' outputs and inputs, including, among others, information on sales, employment in full-time equivalents (FTE), investment, labor costs, and intermediate input expenditures for a representative and periodically rotating 40% sample of all German manufacturing sector firms with more than 19 employees (firm data, henceforth).[8] We use this data set to calculate firm-level average wages, value added and other variables used in our regression analysis.

The other data set is a census of all manufacturing *plants* containing detailed information on plants' total energy consumption in terms of quantities (energy data, henceforth). The data reports plant's energy consumption by multiple energy source categories. For our analysis we focus on the five main categories, electricity, heavy fuel oil,

7 In unpublished work, Garin and Silverio (2018) analyze rent-sharing elasticities for incumbent full-time workers being employed at some 3,000 Portuguese exporters before the 2009 recession. They do not restrict the sample to workers still employed at the shocked firm after the export shock. Garin and Silverio (2018) instrument employer-level value added (instead of value added per worker) with idiosyncratic export demand shocks and report IV pass-through elasticities of 0.15.

8 We follow Bräuer *et al.* (2019) in calculating capital stocks based on available information on firms depreciation and investment using a perpetual inventory method where the first capital stock is derived from observed capital depreciation and assumptions on the depreciation rate.

light fuel oil, natural gas, and hard coal, as only for them official price data is available. These five main carriers together account for more than 95% of the average firm's energy consumption. We will keep firms in the sample that use other than the five main carriers. Our results hold when reducing the sample to firms that exclusively use the five main energy carriers. Our energy data reports quantity information by energy source category in kilowatt-hours (kWh), allowing us to readily aggregate across the different source categories and to calculate the shares of each energy carrier in total energy consumption.

We merge national energy price data from the Federal Ministry of Economic Affairs and Energy (BMWI)[9] to our data. From that, we calculate energy prices per kWh using conversion tables from the BMWI.[10] Figure 4.1 shows the price development (normalized to one in 2003) for our five energy carriers. Overall, energy prices increased substantially over observation period.

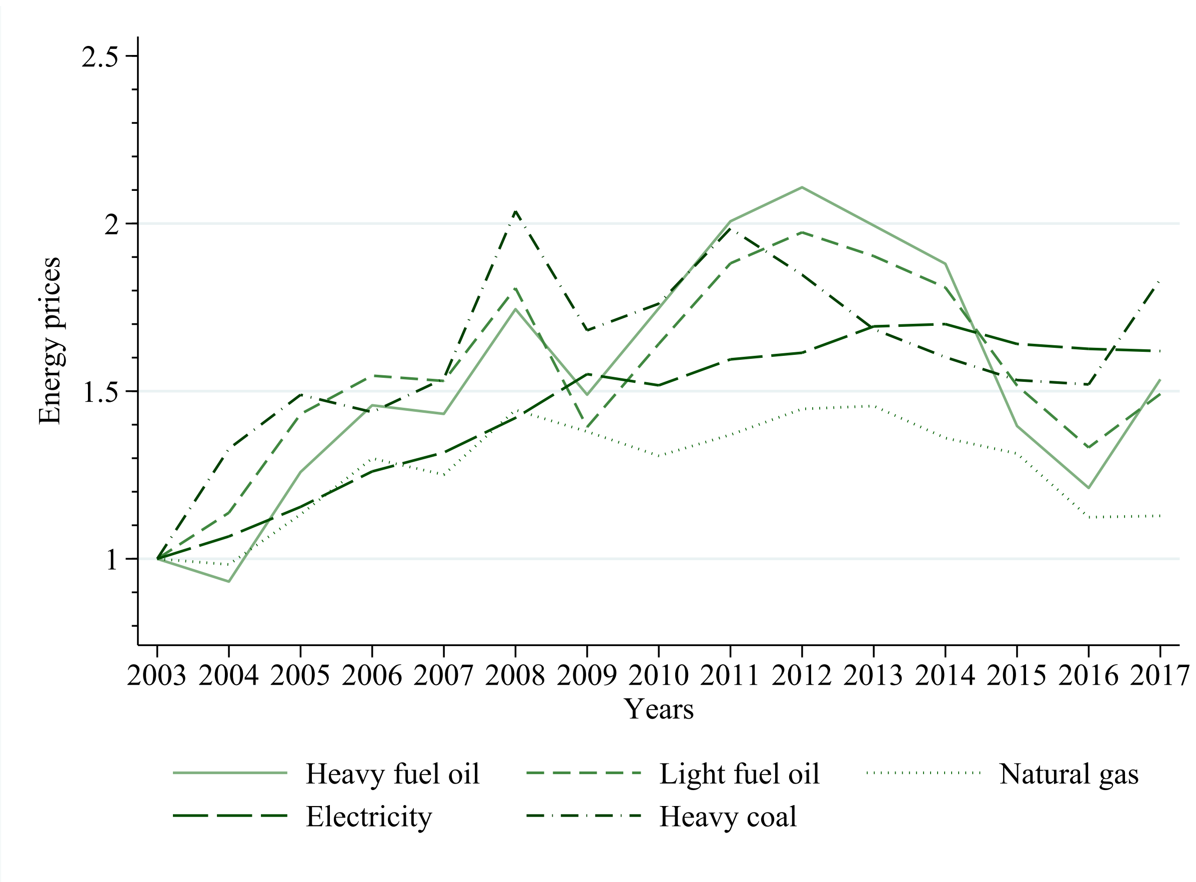
Note that our energy data refers to the plant level, whereas our firm data contains firm-level information. We combine both data sets using a link between the unique plant- and firm-level identifiers provided by the statistical offices of Germany. Given this data structure, we focus on single-plant firms which account for 90% of all manufacturing firms in our data.

Before we run our regressions, we clean the data by excluding the top and bottom one percent in revenue over production inputs and wages, value added over revenue and total consumed kWh over capital for each year and two-digit industry. We further exclude recycling industry firms from our analysis, because these firms generate additional energy from sources other than reported in our energy data (i.e. from recycling). Similarly, we exclude manufactures of coke and refined petroleum products because energy price changes directly impact on their *output* prices, too. We present and discuss further summary statistics on the data in our empirical results section.

9 The price data can be accessed via `http://www.bmwi.de/Navigation/DE/Themen/energiedaten.html`. We use the update of 05.03.2021.

10 Prices for electricity and gas are provided in Euro per kWh. For heavy fuel oil, light fuel oil, and coal, prices are given per tons, hectolitre, and coal units respectively.

Figure 4.1: Development of energy prices relative to 2003



Notes: Federal Ministry for Economic Affairs and Energy, Zahlen und Fakten Energiedaten. Log price changes. Year 2003 is normalized to unity. All prices were converted to kWh per Euro using of standard conversion tables from the Federal Ministry for Economic Affairs and Energy.

4.4 Empirical strategy

4.4.1 The rent-sharing model

Our identification of rent-sharing elasticities builds on a standard rent-sharing model (for a review see Card *et al.* 2018), in which firms and workers bargain over a joint surplus. The general model we sketch below is based on the right-to-manage bargaining framework (Nickell and Andrews 1983), emphasizing that changes in firms' quasi rents can affect workers' wages if firms share rents. To see this, we first define quasi rents in the following way:

$$QR_{it} = VA_{it} - w_{ot}N_{it} - rK_{it}, \tag{4.1}$$

where the quasi rent QR_{it} is the value added VA_{it} (gross output minus intermediate inputs) net of the *alternative* wage bill of the employed workers $w_{ot}N_{it}$ and the cost of capital rK_{it}. The *actual* wage a worker receives at a firm i will be denoted by w_{it} below, and the alternative wage bill can be viewed as workers' outside option. Value added relates to labor and capital through a production function that we do not need to specify for our purposes. Workers' utility is a function of income and depends on the wage, $u(w)$. Hence, there are two scenarios for a worker employed at firm i: at firm i, his/her utility equals $u(w_{it}) = w_{it}$, whereas outside of firm i, utility is given by $u(w_{ot}) = w_{ot}$. For wage bargaining to exist, it must hold that $w_{it} > w_{ot}$. If this is the case, risk neutral workers and firms bargain over the quasi rent. Formally:

$$max_{N,w_{it}}[\phi log((u(w_{it}) - u(w_{ot}))N_{it}) + (1-\phi)log(QR_{it})], \tag{4.2}$$

with $\phi \leq 1$ being the Nash-bargaining parameter.

From the first order condition of this maximization problem, we find:

$$w_i \cong \frac{\phi}{1-\phi}\frac{QR_{it}}{N_{it}} + w_{ot}, \tag{4.3}$$

where we used the first order Taylor approximation: $u(w_{ot}) \cong u(w_{it}) + (w_{it} - w_{ot})u'(w_{it})$. Equation (4.3) shows that workers' wages are a sum of their outside option and the quasi rents they can capture. This equation motivates our baseline structural equation that we bring to the data:

$$log(w_{it}) = \beta_0 + \gamma log(\frac{VA_{it}}{N_{it}}) + x_{kit}\beta_k + \eta_{it}, \tag{4.4}$$

where $\gamma = \frac{\phi}{1-\phi}$ defines workers' relative bargaining power, β_0 captures workers' outside options, the vector x_{kit} includes firm-level controls, and η_{it} is an error term. In our application, we augment equation (4.4) by various fixed effects and control variables to account for time (in)variant confounders (see section 4.4.2).

Note that in equation (4.4) we replaced quasi rents per worker by value added per worker (we use full time equivalents in our empirical application). We do this because quasi rents are not directly measurable in the data. Yet, our value-added based estimate of γ equals the elasticity of wages with respect to the quasi rent per worker multiplied with the value-added over quasi rents ratio. When comparing our results to quasi-rent-based results, we therefore rescale γ with the value-added over quasi rents ratio (see also Card *et al.* 2018). To do so we resort to the IAB establishment panel[11] and apply the quasi rent estimation as in (Hirsch and Mueller 2020). They define the quasi rent as we do in equation (4.1) and compute the outside option based on worker quality estimates derived from the two-way-fixed effects wage decomposition performed by Card *et al.* (2013) and worker expectations on firm wage premia at subsequent employers. When applying the strategy in (Hirsch and Mueller 2020) to our population of German manufacturing firms with more than 19 employees, we find a value-added over quasi rents ratio of 2.8.[12]

Finally, note that the general bargaining setting above nests labor market outcomes where wages are below and above firms' marginal revenue products of labor (MRPL). Whether wages are above or below the MRPL depends on the realized values of ϕ and the definition of workers' outside option. For instance, workers outside option could either be equal to the MRPL or the monopsonistic wage of firm i, or even to the unemployment benefits.[13] The attractive feature of the general setting we apply is that is does not require specifying these labor market aspects as it does not depend on specific labor market settings.

[11] The IAB establishment panel is a large scale plant survey being representative for all German plants having at least one worker subject to social security contributions. See Ellguth *et al.* (2014) for description of the data.

[12] Card *et al.* (2014) report a value-added over quasi rents ratio of about 2 in Italian data.

[13] See Falch and Strøm (2007) for a model that combines the classical efficient bargaining model with the monopsonistic labor market model by defining that workers' outside option equals the monopsonistic wage. See Dobbelaere *et al.* (2020) for a comprehensive mapping of bargaining power in Germany.

4.4.2 Identifying the rent-sharing parameter

Our point of departure is equation (4.4) in first differences ($\triangle x_{it} = x_{it} - x_{it-1}$) for firm i at time t to capture unobserved heterogeneity:

$$\begin{aligned} \triangle wagen_{it} = {} & \beta_0 + \gamma_1 \triangle vadn_{it} + \beta_1 \triangle kn_{it} + \beta_2 \triangle n_{it} + \beta_3 kwhn_{it-1} \\ & + \beta_4 D_{it}^{ElecProd} + \beta_5 D_{it}^{ElecRec} + \beta_6 D_{it}^{ElecSup} + \beta_7 D_{it}^{Coal} + \beta_8 D_{it}^{10GWh} \\ & + ind_i + region_i \times time_t + \eta_{it} \end{aligned} \tag{4.5}$$

The dependent variable is the logarithm of the wage bill per full-time equivalent ($wagen_{it}$) and the independent variable of interest is the logarithm of value added per full-time equivalent ($vadn_{it}$). We hold constant capital intensity (kn_{it}) and the number of full-time equivalents (n_{it}) (both in logs).

The coefficient of interest γ_1 captures workers' relative bargaining power, i.e. the rent-sharing elasticity. OLS estimates of γ_1 will most likely be biased. Sources of biases covered in the literature stem either from reversed causality (e.g. efficiency wages) (Katz 1986) or simultaneity (e.g. firm amenities or management practices) (Bender *et al.* 2018). To deal with endogeneity, we employ an instrumental variables approach. We define our firm-level Bartik instrument ($\triangle EI_{it}$) as the weighted sum of time shifts of the logarithm of national prices of energy carriers (in Euro/kWh, $\triangle pe_{st}$), where the weights are the firm-level shares (e_{is0}) of each energy source $s \in S = \{electricity, naturalgas, lightoil, heavyoil, hardcoal\}$ in total energy consumption. We fix the shares to their initial value (i.e. when firms are first observed in the data) to guarantee that time variation comes from shifts in prices only.[14] Formally:

$$\triangle EI_{it} = \sum_{s=1}^{S} \triangle pe_{st} e_{is0}. \tag{4.6}$$

To have a valid instrument, two conditions must hold. First, $\triangle EI_{it}$ has to be a relevant instrument for labor productivity. Our productivity measure captures value added, i.e.

[14] We also run a version using previous year weights.

sales minus intermediate inputs, per full time equivalent. Energy costs are part of intermediate inputs and should thus be negatively correlated with labor productivity.[15] As we discuss in the next section, the instrument enters the first stage highly statistically significant and with the expected sign.[16]

The second condition is that the instrument is strictly exogenous conditional on covariates. Before discussing this, note that we further control for whether firms receive electricity from other firms ($D_{it}^{ElecRec}$), supply other firms with electricity ($D_{it}^{ElecSup}$), produce electricity ($D_{it}^{ElecProd}$), or use lignite and hard coal (D_{it}^{Coal}). From the descriptive statistics presented in Table (4.1) below, we already see that only a few firms belong to these groups. We also control for predetermined total energy consumption ($kwhn_{it-1}$) and include a dummy for firms using more than 10 GWh electricity (D_{it}^{10GWh}) per year.[17] We include industry (ind_i) fixed effects to control for industry specific components of wage changes. We observe national prices for electricity and gas and must account for differences in the fee of both the gas and electricity networks, which vary regionally and over time due to differences in legislation, network coverage, investments, age and quality of the regional gas and electricity network (Bundesnetzagentur). Therefore, we include region $\times$ year fixed effects thereby comparing firms operating in the same region in the same year, only.

Despite their popularity, an in-depth analysis of Bartik instruments and their identifying assumptions has been undertaken only recently by Borusyak *et al.* (2021) and GPSS. In the notion of GPSS, our setting is best described by its identification

15 What is more, price increases should additionally depress revenue in standard production function settings. We discuss this in the results section.

16 Though larger German firms tend to use derivatives to hedge against price volatility in commodity markets, the share of firms doing so is small. The Deutsches Aktieninstitut (2012) uses the survey by Bodnar and Gebhardt (1999) and shows that only 7% of firms with less than 100 million Euro revenue use derivatives to hedge against raw materials and commodity price volatility. One third of firms with more than 100 million Euro revenue hedge against this risk, which supports earlier results of Bodnar and Gebhardt (1999). If relevant at all, hedging should work against finding a strong first stage in our IV regression.

17 Price differences in electricity may depend on the amount of electricity used, e.g. because of tax benefits for electricity intensive firms (German Renewable Energy Sources Act, EEG). Note that our instrument would not directly be affected by these price differences as we use changes in log prices instead of price levels.

coming from energy shares as opposed to price changes.[18] Differences in firm-level energy carrier shares create a differential exposure of firms to economy-wide price changes.[19] The identifying assumption is that, conditional on covariates, initial energy carrier shares are exogenous to wage *changes* so that wage *changes* are only affected by the instrument via its impact on productivity *changes*.[20] GPSS propose a series of diagnostics on the validity of the instrument that we also run after presenting our baseline results.

4.5 Empirical results

4.5.1 Main results

The summary statistics for our sample with almost 97,000 firm-year observations are depicted in Table (4.1) showing means and percentiles for all variables used in the main regressions. We see not only substantial variation in wages and productivity but, importantly, also enormous heterogeneity in the amount of energy use and the composition of energy carriers. Firms at the 10th percentile of the energy consumption per worker distribution use just about 5,650 kwh per full-time equivalent whereas this number is more than 20times larger at the 90th percentile. To illustrate firm-level heterogeneity in the energy mix, we consider the example of electricity. Firms at the 10th percentile of the electricity share distribution cover only about 20 percent of their total energy consumption from electricity, whereas firms at the 90th percentile almost exclusively use electricity (89 percent). Hence, changes in electricity prices impact firms very differently.

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[18] GPSS frame their study within the canonical Bartik setting where locations are regional entities, say commuting zones, having different industry shares and are hit by an aggregate shock affecting regions differently because of their differing industry composition. We have establishments instead of regional entities and energy carrier shares instead of industries and the price shock affects establishments differently because of their different energy mix.

[19] Borusyak *et al.* (2021) analyze Bartik designs in which independent shocks hit a huge number of different industries (i.e. energy shares in our setting). In their setting, the Bartik estimator is consistent even if the exogenous shares assumption is violated.

[20] Firms may anticipate price changes. Note however that anticipation effects would pose a threat to identification only if they are systematically related to future wage changes. We argue that technological preconditions rooted in firms' idiosyncratic production processes coupled with uncertainty about future energy prices put narrow limits on the firms' capability to adjust their short-run energy mix to future price changes. By fixing the shares to the year of the first observation of the firm, we further reduce the likelihood of anticipation effects.

Table 4.1: Descriptive Statistics

	Mean	SD	P10	P25	P50	P75	P90
Log(Wage bill per FTE)	10.510	0.320	10.083	10.306	10.536	10.736	10.896
Log(Value added per FTE)	10.854	0.495	10.260	10.542	10.841	11.148	11.468
Log(Full-time equivalent)	4.425	0.944	3.303	3.689	4.290	5.017	5.756
Log(Capital stock per FTE)	11.140	0.886	10.025	10.592	11.168	11.720	12.228
Wage bill per FTE	38508.9	11858.0	23935.4	29918.1	37635.9	45977.1	53974.5
Full-time equivalent (FTE)	142.4	221.7	27.2	40.0	73.0	151.0	316.0
Capital stock per FTE	11.077	0.904	9.936	10.517	11.109	11.673	12.185
kwh per FTE	77231.6	469195.1	5651.1	9981.1	18935.4	43725.3	113792.8
Share of energy source of total used kWh							
Electricity	0.513	0.250	0.203	0.314	0.481	0.702	0.893
Natural gas	0.292	0.288	0.000	0.000	0.229	0.546	0.716
Light fuel oil	0.136	0.238	0.000	0.000	0.000	0.179	0.572
Heavy fuel oil	0.001	0.030	0.000	0.000	0.000	0.000	0.000
Hard coal	0.001	0.021	0.000	0.000	0.000	0.000	0.000
Bartik IV (previous year)	0.032	0.085	-0.059	-0.021	0.029	0.089	0.135
Bartik IV (fixed year)	0.031	0.088	-0.059	-0.022	0.030	0.092	0.136
Electricity							
Producer (dummy)	0.085	0.279	0.000	0.000	0.000	0.000	0.000
Receiver (dummy)	0.035	0.183	0.000	0.000	0.000	0.000	0.000
Supplier (dummy)	0.062	0.242	0.000	0.000	0.000	0.000	0.000
Coal user (dummy)	0.005	0.072	0.000	0.000	0.000	0.000	0.000
$\geq$ 10 GWh of electricity (dummy)	0.086	0.280	0.000	0.000	0.000	0.000	0.000
Export status (dummy)	0.813	0.390	0.000	1.000	1.000	1.000	1.000
R & D	0.326	0.469	0.000	0.000	0.000	1.000	1.000
kWh in total	17.4×10^6	185×10^6	0.25×10^6	0.54×10^6	1.5×10^6	5.5×10^6	18.8×10^6
N			96,397				

Notes: AFiD Panel, 2003–2017, single-plant firms. 96,397 plant-year observations belonging to 20,339 single-plant firms.

Our main OLS and IV regression results are presented in Table (4.2). OLS yields a rent-sharing elasticity of 0.12 indicating that a 10 percent increase in value added per worker is associated with about 1.2 percent higher wages. This is close to results in Jäger *et al.* (2021) who report an OLS based elasticity of 0.084 in German social security data. Our preferred IV estimator relying on the energy mix in the base year gives an estimated coefficient of 0.21. Alternatively, using a time-variant energy carrier composition yields a very similar estimate of 0.20. In both approaches, we obtain a reassuringly high first stage

F-statistic of about 30 (Table 4.2) and the instruments enter the respective first stage estimations with the expected negative sign (Table 4.3).[21] Our estimates are closer to the upper end of the value-added based estimates surveyed in Card *et al.* (2018).

4.5.2 Testing the plausibility of the identifying assumptions

As discussed before, our IV strategy may turn invalid should firms anticipate energy price changes. To sidestep this problem we additionally report results based on fixed energy shares in the base period alongside with a specification that uses previous years' shares. Neither our first stage results (Table 4.3) nor our second stage results (Table 4.2) show any meaningful differences between these two specifications. We conclude that we don't find any evidence on anticipation effects.

Bartik instruments combine individual instruments with a specific weight matrix making the Bartik estimator a black box in the sense that it is not obvious which of the instruments drives the results. Our Bartik instrument is the sum of products of firm-level energy carrier shares with national price shifts for five energy carriers. High weight instruments have a strong impact on the estimation outcome and, thus, GPSS propose that researchers identify and discuss these instruments in particular. Based on Rotemberg (1983) they show how to decompose the Bartik estimator into a weighted combination of just-identified IV estimators. The resulting Rotemberg weights attached to these just-identified estimators are informative about the importance of the specific instrument, i.e. the specific energy carrier, for the overall Bartik estimate.

Following GPSS, we present graphical evidence on the Rotemberg weights (Appendix Figure 4.A.1) in which the x-axis is the first stage F-statistic and the y-axis the second stage estimate associated with each just-identified IV regression. In the scatter plot of

[21] Recently, Lee *et al.* (2021) argued for single instrument IV settings that second stage t-testing needs to be corrected. Lee *et al.* (2021, Table 3a) display correction factors for the 2nd stage standard errors such that the usual critical values for t-tests can be used. The correction factor depends on the first stage F statistic. In our case, this factor is about 1.2 for the 5 percent significance level yielding corrected standard errors of about 0.077 (0.081) for the IV specification with fixed (variable) energy carrier composition as shown in Table 4.2. As, in both cases, corrected t-ratios remain above 1.96, we conclude that our rent-sharing elasticity is statistically significantly different from zero at the 5 percent level.

Table 4.2: Rent sharing, OLS and IV regressions

	OLS	IV	
	(1)	(2)	(3)
Value added per FTE	0.123***	0.205***	0.198***
	(0.003)	(0.064)	(0.068)
Capital stock per FTE	0.042***	0.040***	0.040***
	(0.004)	(0.004)	(0.004)
Full-time equivalent (FTE)	-0.429***	-0.390***	-0.394***
	(0.007)	(0.031)	(0.032)
Electricity producer	0.004***	0.005***	0.005***
	(0.002)	(0.001)	(0.002)
Electricity receiver	-0.000	-0.000	-0.000
	(0.002)	(0.002)	(0.002)
Electricity supplier	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)
Coal user	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)
> 10 GWh of electricity	0.010***	0.009***	0.009***
	(0.001)	(0.002)	(0.002)
kWh per FTE in $t-1$	-0.005***	-0.004***	-0.004***
	(0.000)	(0.000)	(0.000)
Industry fixed effects	yes	yes	yes
Region × year fixed effects	yes	yes	yes
Reference year of instrument	–	fixed	previous
R-squared	0.386	0.358	0.362
1^{st} stage F-Stat of instrument		32.20	28.91
Firm-year observations	96,397	96,397	96,397
Number of Firms	22,513	22,513	22,513

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average wage bill per full-time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The instrument used in the IV regressions is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Figure 4.A.1, circles represent positive weights and triangles represent negative weights. The size of the Rotemberg weight is reflected by the size of the circle and triangles. Finally, the dashed horizontal line depicts the point estimate based on the combined

Table 4.3: First Stage, OLS regressions

	(1)	(2)
Bartik instrument	-0.113***	-0.113***
	(0.020)	(0.021)
Capital stock per FTE	0.022**	0.022**
	(0.009)	(0.009)
Full-time equivalent (FTE)	-0.470***	-0.470***
	(0.013)	(0.013)
Electricity producer	-0.003	-0.003
	(0.005)	(0.005)
Electricity receiver	0.005	0.005
	(0.004)	(0.004)
Electricity supplier	0.002	0.002
	(0.005)	(0.005)
Coal user	-0.003	-0.002
	(0.008)	(0.008)
> 10 GWh of electricity	0.016***	0.016***
	(0.003)	(0.003)
kWh per FTE in $t-1$	-0.005***	-0.005***
	(0.001)	(0.001)
Industry fixed effects	yes	yes
Region × year fixed effects	yes	yes
Reference year of instrument	fixed	previous
R^2	0.166	0.166
Firm-year observations	96,397	96,397
Number of Firms	22,513	22,513

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average value added per full time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The Bartik instrument is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Bartik instrument (our baseline regression). Figure 4.A.1 shows that electricity is by far the dominating instrument and, reassuringly, that the point estimate from the just identified regression based on the electricity share almost exactly resembles the overall Bartik estimate. Whereas light and heavy oil are relatively close to the overall estimate, too, natural gas and hard coal yields counter-intuitive results but only have relatively small Rotemberg weights compared to electricity. In the following, we therefore discuss identifying assumptions with a focus on electricity shares.

Firstly, GPSS propose testing whether initial period shares predict initial period firm characteristics as finding strong correlates with the high-weight carrier helps thinking about potential confounders. In Appendix Table 4.A.1 we regress initial period energy carrier shares on initial period firm characteristics separately for all five energy carriers. Results for electricity show statistically significant but economically very small coefficients for productivity, capital intensity and employment. E.g. the coefficient on employment means that a 1 percent increase in employment is associated with 0.012 percentage point increase in the electricity share in energy consumption (having a firm-level mean of 52 percent).

As stated before, the identifying assumption is that, conditional on covariates, initial energy carrier shares are uncorrelated with wage changes. For settings with a pre-treatment period, GPSS propose checking pre-trends for the instruments with the highest Rotemberg weight. As we do not have a pre-treatment period (i.e. a period with constant energy prices), we cannot directly apply this test. However, we can even go one step further by including firm fixed effects into our first-difference IV specification. Firm fixed effects control for unobserved firm-specific wage growth rates that may be correlated with our instrument. The rent-sharing coefficient in this specification is 0.314 (Appendix Table 4.A.2), which is above our baseline estimate. Note that this estimate is still within the 95 percent confidence interval of the rent-sharing coefficient in our preferred specification and that comparisons between the two estimates are difficult as the sample is reduced by the introduction of fixed effects.

Based on Kolesar *et al.* (2015), GPSS propose comparing results from maximum likelihood estimators with those of two-stage least squares (2SLS) estimators. They argue that obtaining similar results further increases confidence in the identifying assumptions. appendix Table 4.A.3 provides a comparison between results from the proposed Limited information maximum likelihood (LIML) estimator and our 2SLS results.[22] Reassuringly,

[22] GPSS additionally recommend using bias-corrected 2SLS and HFUL estimators. Both estimators

point estimates are almost identical for the rent-sharing coefficient and also for the other covariates. Thus, we find no evidence for potential misspecification. Having multiple instruments also enables us to perform overidentification tests. Test statistics (see Appendix Table 4.A.3) again do not point at misspecification (p-values > 0.2). Finally, in Appendix Table 4.A.4, we report the first stage coefficients of the overidentified 2SLS model. Coefficients of all energy carriers have the expected negative sign and the main carriers (electricity, natural gas, light oil) enter highly significant with t-values between 3.2 and 5.1 and with coefficients of similar size.

Having conducted the battery of plausibility tests for Bartik instruments as proposed by GPSS we are confident that our Bartik instrument works. Summing up, we find

1. no evidence for anticipation effects;
2. that the most commonly used energy carrier, electricity, has the highest Rotemberg weight, enters the first stage with the expected sign and individually yields a rent-sharing elasticity that almost exactly mirrors that of the combined instrument;
3. our results being robust to controlling for firm-specific wage trends;
4. no evidence for misspecification as the LIML estimator perfectly matches our 2SLS results and as overidentification tests do not reject the null hypothesis.

4.5.3 Transmission channels

Next we inquire the transmission channels through which our instrument works. First stage regressions (Table 4.3) confirm that an increase in energy prices reduces value added per worker at the firm level. This effect could be transmitted through changes in all three components of our labor productivity variable, i.e. revenue, intermediate input expenditures, or employment. However, Germany's strict employment protection rules can be expected to turn labor into a quasi-fixed production input. A standard production

have trouble dealing with the high dimensional fixed effects structure we apply. However, according to GPSS, having at least one of the maximum likelihood estimators (here LIML) yielding similar results to 2SLS goes a long way in testing for misspecification.

function with quasi-fixed labor (and capital) but with variable intermediate inputs would predict that higher prices for intermediates transmit into constant labor inputs, lower intermediate input usage and thus lower output and revenue. To test how the instrument transmits into value added per worker, we use our first stage regression setting but now regress the three components of value added per worker separately on the Bartik instrument. Table (4.4) shows exactly the expected pattern: no effect on employment and statistically significant reductions in intermediate inputs and revenue. Hence, the instrument's effect on labor productivity, and thus on wages, works via a reduction in value added at constant labor input levels.

Strict employment protection legislation is, of course, not completely ruling out changes in hours worked or in workforce composition as a reaction to energy cost shocks. As we do not observe changes in precise hours worked (we compute full-time equivalents based on working time *categories*) and in workforce composition in our main data set, we can not directly test whether firms systematically react to energy shocks by adjusting along these margins. To nevertheless test for any such firm reactions in hours worked and workforce composition, we run two checks. First we use the limited information on workforce composition in our plant-level data, i.e. the plant-level shares of female workers and researchers, and study how these shares change in response to our energy cost shocks. Second, we additionally resort to the Structure of Earnings Survey (henceforth, VSE), which is a linked-employer-employee data set generated by the statistical offices containing worker-level information on hours worked and worker characteristics. This survey is conducted every four years starting in 2006 and contains information on about 60.000 randomly drawn plants.[23]

Although our main data and the VSE plants can be easily merged via unique plant identifiers, there are severe limitations to the analysis of this merged data that prevent us from using it as our main data set. Recap that our main data itself is a rotating survey that is drawn anew every four years. Because the dates of drawing the VSE and our

[23] The sample is representative for the population of German employees and contains plants of all size classes from all regions and industries in Germany. The VSE is stratified according to size class, industry, and region. Importantly, is does not follow workers over time.

Table 4.4: Transmission channel, OLS regressions

	Dependent variable		
	Full-time equivalent	Intermediate inputs	Revenue
	(1)	(2)	(3)
Bartik instrument	0.000	-0.081***	-0.089***
	(0.005)	(0.016)	(0.014)
Capital stock per FTE	-0.530***	-0.247***	-0.247***
	(0.010)	(0.007)	(0.007)
Electricity producer	0.002	0.002	0.002
	(0.002)	(0.004)	(0.003)
Electricity receiver	-0.002	-0.008**	-0.003
	(0.002)	(0.004)	(0.003)
Electricity supplier	0.006***	0.005	0.004
	(0.002)	(0.004)	(0.004)
Coal user	-0.010**	-0.004	-0.003
	(0.004)	(0.008)	(0.007)
$>$ 10 GWh of electricity	0.003*	0.014***	0.016***
	(0.001)	(0.002)	(0.002)
kWh per FTE in $t-1$	-0.003***	-0.003***	-0.004***
	(0.0003)	(0.001)	(0.001)
Industry fixed effects	yes	yes	yes
Region $\times$ year fixed effects	yes	yes	yes
R-squared	0.531	0.178	0.242
Firm-year observations	96,397	96,397	96,397
Number of Firms	22,513	22,513	22,513

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variables are in first differences. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The Bartik instrument is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

main data set are not synchronized, the overlap between both data sets is extremely low, particularly when researchers aim at following plants in the VSE over multiple survey years. Nevertheless, we can use the small sample of plants reporting in all three survey years of the VSE as well as the pooled cross sections of plants in the VSE to provide additional tests on adjustments in workforce composition, hours worked, and tenure in response to energy cost shocks over the four year windows of the VSE. Due to focusing on four-year windows, we capture longer-run adjustments and perceive this as a conservative test because if we do not find longer-run adjustments for a quasi-fixed production factor, we should not expect any short-run adjustments either.

We proceed in the following way: First, we use our main data to identify industry-size-class combinations that are strongly and weakly affected by energy shocks.[24] For each of our industry-size cells, we calculate energy shocks as the average of 4-year plant-level energy shocks (i.e. averages of the Bartik instrument). We define a cell as being heavily affected if the cell-level energy shock is above the median of the energy shock distribution across cells.[25]

Having defined the highly/weakly affected cells, we identify plants in the VSE that belong to these cells. Finally, we compare how workforce composition, hours worked, and tenure changed for i) plants in highly affected cells compared to plants in weakly affected cells (we follow plants over the VSE surveys) and ii) highly affected cells compared to weakly affected cells (we calculate average workforce characteristics at the cell-level and compare changes between cells). To account for pre-trends we use a difference in difference setting where we define highly affected cells based on energy shocks in 2010. We then compute the changes in plant/cell characteristics between 2006 and 2010 and 2010 and 2014 and calculate the difference between both changes.[26] Finally, we regress this difference on a dummy indicating that a plant/cell belongs to the highly affected group. The coefficient on this dummy gives us the difference-in-differences effect of being a (plant in a) highly affected cell.

Appendix Tables 4.A.5, 4.A.6 and 4.A.7 shows the associated results. Table 4.A.5 relies on our main data and shows that there are no statistically significant changes in the shares of female workers and researchers in response to energy cost shocks. Table 4.A.6 uses the VSE data and reports results for the plant level. Note that the sample of plants is much

24 We use the 2-dig industries and four size classes: 20-49, 50-100, 101-250, and more than 250 employees.
25 Results are unchanged when weighting plant-level energy shocks with plants' energy consumption.
26 As our sample period comprises the years 2003 to 2017, years 2006, 2010, and 2014 are the only VSE waves in our sample period.

smaller than in our main analysis. This is due to the sampling structure of the VSE and our main data as described above. Nevertheless, also here, we cannot find any evidence for adjustments in tenure, hours, and skill composition within plants in response to energy shocks. Table 4.A.7 takes the analysis from Table 4.A.6 to the cell level. This allows us to use all manufacturing plants in the VSE that are located in our industry-size-class bins (because we do not need to match both data sets at the plant level here). Nevertheless, despite the much larger population of plants underlying Table 4.A.7, we cannot find any evidence for cell-level workforce adjustments in response to cell-level energy shocks.

Overall, we find no evidence for energy-shock induced firm adjustment processes in terms of workforce composition and hours worked. Neither our main data nor the VSE data show any statistically significant effects and all specifications show extremely small coefficients. What is more, as worker churning would have reduced average tenure; our insignificant effects for tenure demonstrate that there is no effect on worker churning either. This implies that our findings of constant employment, stable workforce composition, and unchanged hours worked are not masking significant reshuffling of workers in response to the cost shock. Overall, these results do not come as a surprise as strict German employment protection legislation turns labor into a quasi-fixed input factor that adjusts very slowly to shocks.

Summing up on transmission channels, we find that energy shocks reduce labor productivity by reducing total sales and intermediate input use but leave unaffected total employment, workforce composition, and hours worked. Importantly, our tenure estimates do not imply excess worker turnover and rather lend support to the notion that our rent-sharing estimates are primarily based on repeated observations of the same worker-plant matches.

4.5.4 Effect heterogeneity

R&D-Firms and related studies. Van Reenen (1996) and Kline *et al.* (2019) are the two firm-level rent-sharing studies that are closest to ours. Both studies use innovation output for identification and arrive at the largest estimates for rent-sharing elasticities in the literature. It is therefore informative to check whether innovative firms differ systematically from non-innovative firms in their rent-sharing behavior. To do so we split our sample and run our model on firms with and without research and development (R&D) departments, which is the best proxy for innovativeness in our data (Table 4.5). OLS yields rent-sharing elasticities of 0.13 (0.10) for firms without (with) an R&D department which mimics the full sample estimate of 0.12 very closely. The IV estimate for non R&D firms of 0.26 is also reasonably close to the full sample estimate, whereas the IV estimate for firms with a R&D department is unreliably estimated with a very low first stage F-value. Although being partially inconclusive, this evidence overall does not imply that R&D and non-R&D firms show a fundamentally different rent-sharing behavior. Of course, this evidence is not necessarily conflicting with that in Van Reenen (1996) and Kline *et al.* (2019) as the very same firms may share their rents differently depending on whether rents are driven by innovation output or energy cost shocks.

Rent sharing and firm size. Recently, it has been argued that large firms tend to share rents to a lesser extend with their workers (Mertens 2021, Wong 2020). IV estimates in Table (4.6) strongly support this view: we find IV estimates of 0.31 for small firms and 0.14 for larger firms. Hence, rent-sharing elasticities appear to be twice as big in small firms. Such differences in rent-sharing elasticities between firms are important for understanding the transmission of labor market imperfections to firm wage differences. To the extent that large firms pay higher wages, a significantly lower rent-sharing elasticity of large firms implies that wage premiums of large firms are not explained by workers' bargaining power. Instead, this points to differences in technology and workforce compositions (e.g. sorting) between firms as driver of large firm wage premiums.[27] Furthermore, these differences imply that for given product market rents, large firms share a smaller piece of rents with their workforce, providing an additional explanation for the high profitability of large firms.

[27] This is consistent with findings in Bonhomme *et al.* (2020) showing a particularly importance of sorting effects in explaining between-firm wage differences for the US.

Table 4.5: Rent sharing, OLS and IV regressions, firms with/without research and development department (R&D)

	R&D		non R&D	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Value added per FTE	0.102***	-0.059	0.132***	0.260***
	(0.004)	(0.236)	(0.004)	(0.072)
Capital stock per FTE	0.053***	0.050***	0.036***	0.032***
	(0.008)	(0.010)	(0.005)	(0.005)
Full-time equivalent (FTE)	-0.352***	-0.418***	-0.455***	-0.393***
	(0.012)	(0.097)	(0.008)	(0.036)
Electricity producer	0.002	0.001	0.005**	0.006**
	(0.002)	(0.003)	(0.002)	(0.002)
Electricity receiver	0.001	0.002	0.001	0.000
	(0.003)	(0.003)	(0.002)	(0.002)
Electricity supplier	0.002	0.001	-0.003	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)
Coal user	0.003	0.001	0.000	0.000
	(0.005)	(0.006)	(0.005)	(0.005)
> 10 GWh of electricity	0.005***	0.008*	0.012***	0.010***
	(0.002)	(0.005)	(0.002)	(0.002)
kWh per FTE in $t-1$	-0.005***	-0.006***	-0.004***	-0.004***
	(0.001)	(0.002)	(0.0004)	(0.0005)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Reference year of instrument	–	fixed	–	fixed
R-squared	0.377	0.238	0.427	0.368
1^{st} stage F-Stat of instrument	–	0.636	–	15.05
Firm-year observations	30,692	30,692	64,736	64,736
Number of firms	7,076	7,076	15,762	15,762

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average wage bill per full-time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The instrument used in the IV regressions is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

There are several explanations for these differences in rent-sharing elasticities between large and small firms. Bigger firms account for a large share of the labor market and

Table 4.6: Rent sharing, OLS and 2SLS regressions, small and large firms

	< 100		≥ 100	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Value added per FTE	0.137***	0.313**	0.098***	0.139**
	(0.004)	(0.122)	(0.004)	(0.066)
Capital stock per FTE	0.036***	0.031***	0.048***	0.048***
	(0.005)	(0.006)	(0.006)	(0.006)
Full-time equivalent (FTE)	-0.463***	-0.373***	-0.353***	-0.337***
	(0.008)	(0.063)	(0.010)	(0.028)
Electricity producer	0.008**	0.009**	0.001	0.001
	(0.004)	(0.004)	(0.002)	(0.002)
Electricity receiver	-0.002	-0.004	0.001	0.001
	(0.003)	(0.003)	(0.002)	(0.002)
Electricity supplier	-0.005	-0.008*	-0.001	-0.001
	(0.004)	(0.005)	(0.002)	(0.002)
Coal user	-0.005	-0.003	-0.002	-0.003
	(0.008)	(0.008)	(0.003)	(0.003)
$>$ 10 GWh of electricity	0.017***	0.010	0.005***	0.005***
	(0.004)	(0.007)	(0.001)	(0.002)
kWh per FTE in $t-1$	-0.005***	-0.004***	-0.004***	-0.004***
	(0.001)	(0.001)	(0.0004)	(0.001)
Industry fixed effects	yes	yes	yes	yes
Region $\times$ year fixed effects	yes	yes	yes	yes
Reference year of instrument	–	fixed	–	fixed
R-squared	0.432	0.323	0.382	0.373
1^{st} stage F-Stat of instrument	–	11.25	–	18.62
Firm-year observations	57,117	57,117	38,442	38,442
Number of firms	16,293	16,293	7,907	7,907

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average wage bill per full-time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The instrument used in the IV regressions is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

can exploit their dominance to drive down wages, also reducing the pass-through from profits to wages (Azar *et al.* 2020, Gouin-Bonenfant 2020). Furthermore, as large firms pay higher wages, they face a lower pressure to raise wages if productivity rises because high-wage firms can effectively "hide" behind industry-wide wage standards, i.e. there are no better outside options for workers (Hirsch and Mueller 2020). Additionally, workers in large, high paying firms might favor non-monetary work amenities over higher wages conditional on receiving high wages (Lamadon *et al.* 2019). This reduces the incentives for workers to bargain for higher wages in high-paying (large) firms. Finally, the difference in rent sharing can also partly be explained by the German industrial relations system. Wages in the German economy and in particular in the manufacturing sector are often bound to collective wage agreements between employer associations and unions. These agreements set minimum wages and thereby reduce the firms' scope to adjust wages e.g. in response to cost shocks.[28] What is more, collective agreements are much more common in larger firms, e.g. because transaction cost advantages of such contracts over individualized bargaining are larger the more workers are employed in a firm. Relying on a non-experimental setting, Gürtzgen (2009) confirms weaker rent sharing in firms with an industry-wide collective wage agreement.

Rent sharing over time. Table 4.7 shows how rent-sharing elasticities evolved over time. We split the observation period into two sub-periods lasting from 2003 to 2009 and from 2010 to 2017. Our IV estimates show a decline in the rent-sharing elasticity by 3 percentage points from the first to the second period of analysis. This corroborates evidence highlighting a rise in firm labor market power and decline in worker bargaining power for several countries (see Mertens 2020 for Germany, Stansbury 2020 for the US, and Bell *et al.* 2019 for the UK). Moreover, a declining rent-sharing elasticity is consistent with the decline in union density and share of workers covered by collectively bargained wages documented in several advanced countries, including Germany (Dustmann *et al.* 2014, OECD 2017).

4.6 Discussion and conclusions

This study presents causal evidence on the rent-sharing elasticity of German manufacturing firms based on firm-level variation in productivity. We developed a novel Bartik instrument combining the predetermined firm-level energy mix with nationwide changes in prices of various energy carriers. Our instrumental variables estimator yields

[28] For an in depth discussion of collective wage agreements and their effect on firm wage premia see e.g. Hirsch and Mueller (2020).

Table 4.7: Rent sharing, OLS and 2SLS regressions, over the years

	2003–2009		2010–2017	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Value added per FTE	0.133***	0.207**	0.112***	0.177**
	(0.004)	(0.084)	(0.004)	(0.082)
Capital stock per FTE	0.046***	0.043***	0.038***	0.037***
	(0.006)	(0.007)	(0.005)	(0.005)
Full-time equivalent (FTE)	-0.385***	-0.358***	-0.474***	-0.437***
	(0.010)	(0.033)	(0.009)	(0.046)
Electricity producer	0.005*	0.004	0.004**	0.0049**
	(0.003)	(0.003)	(0.002)	(0.002)
Electricity receiver	0.004	0.003	-0.002	-0.003
	(0.003)	(0.003)	(0.002)	(0.002)
Electricity supplier	0.001	0.004	-0.002	-0.003
	(0.004)	(0.005)	(0.002)	(0.003)
Coal user	-0.006	-0.005	0.002	0.003
	(0.007)	(0.007)	(0.004)	(0.004)
$>$ 10 GWh of electricity	0.011***	0.010***	0.009***	0.008***
	(0.002)	(0.002)	(0.001)	(0.002)
kWh per FTE in $t-1$	-0.005***	-0.004***	-0.004***	-0.004***
	(0.001)	(0.001)	(0.0004)	(0.0004)
Industry fixed effects	yes	yes	yes	yes
Region × year fixed effects	yes	yes	yes	yes
Reference year of instrument	–	fixed	–	fixed
R-squared	0.359	0.406	0.336	0.388
1^{st} stage F-Stat of instrument	–	21.29	–	21.57
Firm-year observations	42,764	42,764	53,633	53,633
Number of firms	16,028	18,118	16,028	18,118

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average wage bill per full-time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The instrument used in the IV regressions is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

a rent-sharing elasticity of about 0.20, implying that a 10 percent increase in firms' value added per worker increases wages by about 2 percent. As one standard deviation in (the log of) value added per worker exceeds 0.5 in our data, the impact of rent sharing on wage inequality can be substantial. The productivity differential between firms at the 10th and the 90th percentile of the distribution of log value added per worker even amounts to 1.2. Evaluating the 0.2 elasticity at this 90-10 productivity gap yields a relatively high range of between firm wage variability of 24 log points. We present extensive evidence on the validity of our novel Bartik instrument.

The innovation-based estimates provided by Van Reenen (1996) and Kline *et al.* (2019) are not only much larger than our estimates, they are also far above most estimates in the literature. As we complement these approaches by presenting an identification strategy that does not rely on innovative activity, we broaden the innovation-centered perspective to the entire manufacturing sector and find considerably smaller rent-sharing estimates. While being unable to directly compare inventing and non-inventing firms, we do not find evidence that firms with and without R&D department differ in their rent-sharing elasticities.

We further document that rent-sharing elasticities monotonically decrease with firm size and that firms having less than 100 employees have an elasticity twice as big as firms with more than 100 employees. This mutes the effect of productivity dispersion on between firm wage inequality as more productive firms are usually larger (see also Mertens 2021). We discussed potential reasons for this finding that generalize beyond the German context including market power and worker preferences for non-wage amenities rising with firm size. However, we also discussed why the centralized German wage setting system might be at least partially responsible for the size gradient. At least for countries with some degree of centralized (union-) wage bargaining, our results indicate that firm size could be a crucial, yet so far neglected, determinant of the rent-sharing elasticity.

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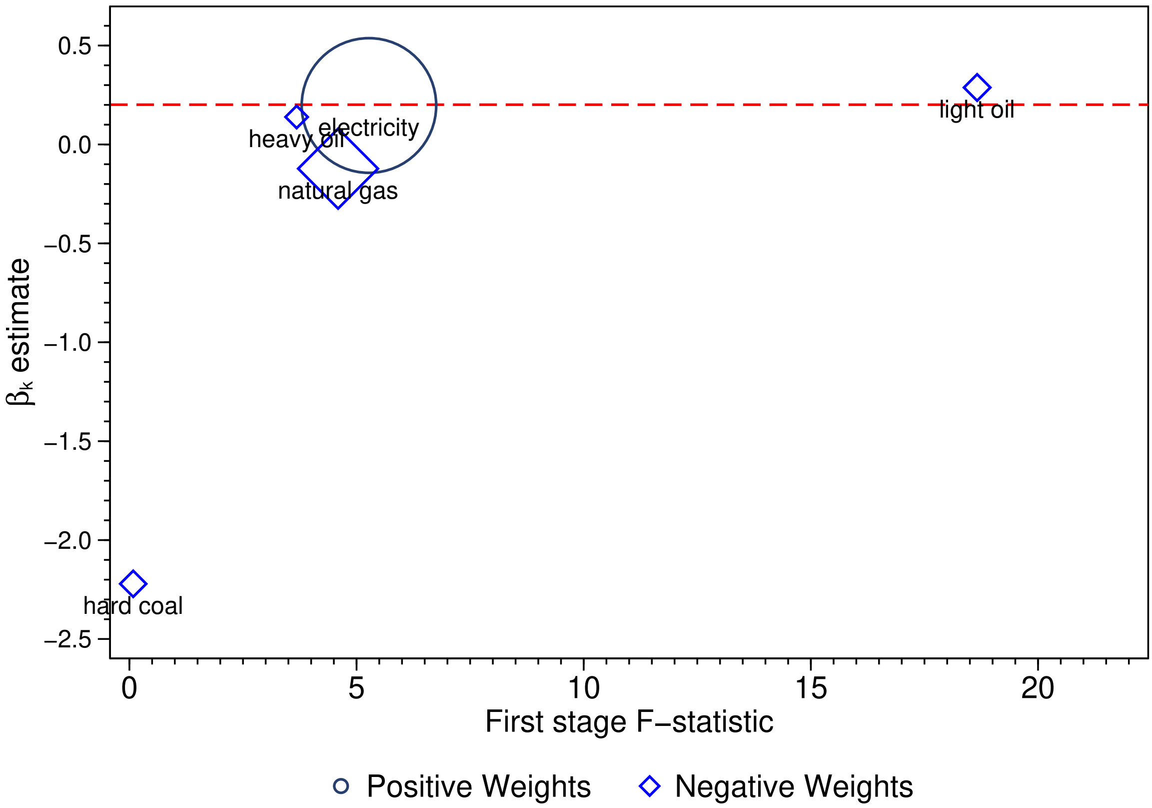
Appendix

4.A Additional material

Figure 4.A.1: Heterogeneity of β_k



Notes: AFiD Panel, 2003–2017, single-plant firms. The y-axis depicts the estimated beta coefficient of the second stage IV regression using GMM, where the instrument is the product of the shifts and shares of a single instrument $s \in S = \{electricity, naturalgas, lightoil, heavyoil, hardcoal\}$. The x-axis depicts the corresponding first stage F-statistic of this regression. The size of the points are scaled by the size of the Rotemberg weight (Goldsmith-Pinkham *et al.* 2020). Circles are have a positive weight and squares a negative Rotemberg weight. The red dashed line is the β_{Bartik} of the baseline IV regression.

Table 4.A.1: Relationship between energy shares and firm characteristics

	Electricity	Light oil	Natural gas	Hard coal	Heavy oil
Log(value added per FTE)	-1.232***	-0.883**	2.208***	0.018	0.017
	(0.421)	(0.419)	(0.467)	(0.031)	(0.051)
Log(capital intensity)	2.907***	-2.111***	-0.765***	-0.012	0.060**
	(0.204)	(0.203)	(0.227)	(0.015)	(0.025)
Log(employment)	1.217***	-2.918***	1.837***	0.024	0.060**
	(0.221)	(0.220)	(0.245)	(0.016)	(0.027)
Electricity Producer	-7.827***	3.153**	3.704***	-0.042	-0.039
	(1.285)	(1.279)	(1.425)	(0.094)	(0.156)
Electricity Receiver	4.211***	-4.889***	-6.278***	-0.053	-0.065
	(0.909)	(0.904)	(1.008)	(0.067)	(0.110)
Electricity supplier	0.460	-4.955***	1.454	0.116	0.225
	(1.479)	(1.471)	(1.639)	(0.108)	(0.180)
Coal User	-26.26***	-2.020	-24.33***	43.56***	-0.392
	(2.721)	(2.707)	(3.016)	(0.199)	(0.330)
> 10 GWh of Electricity	0.501	-5.084***	1.025	-0.0982	0.629***
	(0.860)	(0.855)	(0.953)	(0.063)	(0.104)
Region fixed effects	yes	yes	yes	yes	yes
Industry fixed effects	yes	yes	yes	yes	yes
R-squared	0.131	0.110	0.125	0.702	0.032
Observations	22,513	22,513	22,513	22,513	22,513

Notes: OLS regressions of the energy carrier shares in firm-level energy use on the levels of various economic indicators. Each column represents a separate cross-sectional regression using the first year per firm, only. Clustered standard errors at the firm level are in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Table 4.A.2: Rent Sharing, OLS and 2SLS regressions with firm fixed effects

	OLS	2SLS	
	(1)	(2)	(3)
Labor productivity	0.113***	0.312***	0.314***
	(0.003)	(0.074)	(0.075)
Capital intensity	0.003	0.013*	0.013**
	(0.005)	(0.006)	(0.006)
Labor	-0.537***	-0.416***	-0.414***
	(0.008)	(0.045)	(0.046)
Electricity Producer	-0.000	0.001	0.001
	(0.003)	(0.003)	(0.003)
Electricity Receiver	-0.000	-0.001	-0.001
	(0.004)	(0.004)	(0.004)
Electricity supplier	-0.001	0.001	0.001
	(0.003)	(0.004)	(0.004)
Coal User	0.014	0.016	0.016
	(0.015)	(0.017)	(0.017)
$>$ 10 GWh of Electricity	0.012***	0.006	0.006
	(0.004)	(0.005)	(0.005)
kWh per worker in $t-1$	-0.019***	-0.014***	-0.013***
	(0.002)	(0.003)	(0.003)
Firm fixed effects	yes	yes	yes
Region $\times$ year fixed effects	yes	yes	yes
Reference year of instrument	–	previous	fixed
R-squared	0.489	0.341	0.338
1^{st} stage F-Stat of instrument		27.61	26.86
Firm-year observations	92,289	92,289	92,289
Number of Firms	18,420	18,420	18,420

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average wage bill per full-time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The instrument used in the IV regressions is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Table 4.A.3: Rent-sharing, OLS and IV regressions, overidentified models

	OLS	IV	
	(1)	(2)	(3)
Value added per FTE	0.123***	0.189***	0.197***
	(0.003)	(0.060)	(0.068)
Capital stock per FTE	0.042***	0.040***	0.040***
	(0.004)	(0.004)	(0.004)
Full-time equivalent (FTE)	-0.429***	-0.398***	-0.394***
	(0.007)	(0.029)	(0.032)
Electricity producer	0.004***	0.005***	0.005***
	(0.002)	(0.002)	(0.002)
Electricity receiver	-0.000	-0.000	-0.000
	(0.002)	(0.002)	(0.002)
Electricity supplier	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)
Coal user	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)
> 10 GWh of electricity	0.010***	0.009***	0.009***
	(0.001)	(0.001)	(0.002)
kWh per FTE in $t-1$	-0.005***	-0.004***	-0.004***
	(0.0003)	(0.0004)	(0.0004)
Industry fixed effects	yes	yes	yes
Region × year fixed effects	yes	yes	yes
Reference year of instrument	–	fixed	fixed
R-squared	0.386	0.368	0.363
Estimator	OLS	2SLS	LIML
1^{st} stage F-Stat of instrument	–	7.360	7.360
Over Ident	–	5.993	5.975
Over Ident (p-V)	–	0.200	0.201
Firm-year observations	96,397	96,397	96,397
Number of Firms	22,513	22,513	22,513

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average wage bill per full-time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The five instruments used in the IV regressions are the products of energy shares and price changes of electricity, natural gas, light fuel, heavy fuel, hard coal. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Table 4.A.4: First Stage regressions with energy carriers as separate instruments (overidentified model)

	(1)	(2)
Electricity	-0.172***	-0.204***
	(0.054)	(0.055)
Light fuel	-0.104***	-0.101***
	(0.020)	(0.022)
Natural gas	-0.158***	-0.142***
	(0.037)	(0.037)
Heavy fuel	-0.245**	-0.427**
	(0.108)	(0.180)
Hard coal	-0.051	-0.011
	(0.097)	(0.106)
Other Control variables	yes	yes
Industry fixed effects	yes	yes
Region × year fixed effects	yes	yes
Reference year of instrument	fixed	previous
R^2	0.166	0.166
Estimator	OLS	OLS
Firm-year observations	96,397	96,397
Number of Firms	22,513	22,513

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variable is the first difference of the natural logarithm of the annual average value added per full time equivalent. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The instruments consists of the product of energy shares and price changes for electricity, natural gas, light fuel, heavy fuel, and hard coal. Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Table 4.A.5: Adjustment of workforce composition, baseline sample

	Dependent variable	
	Share of female workers	Share of researchers
	(1)	(2)
Bartik instrument	-0.0009	0.0019
	(0.0030)	(0.0013)
Capital stock per FTE	-0.0001	0.0014**
	(0.0016)	(0.0006)
Full-time equivalent (FTE)	-0.0285***	-0.0064***
	(0.0030)	(0.0012)
Electricity producer	0.0001	-0.0005
	(0.0009)	(0.0004)
Electricity receiver	0.0002	0.0004
	(0.0009)	(0.0004)
Electricity supplier	0.0005	0.0006
	(0.0011)	(0.0004)
Coal user	-0.0005	0.0006
	(0.0012)	(0.0007)
$>$ 10 GWh of electricity	-0.0002	0.0002
	(0.0005)	(0.0002)
kWh per FTE in $t-1$	-0.0001	-0.0001
	(0.0002)	(0.0001)
Industry fixed effects	yes	yes
Region $\times$ year fixed effects	yes	yes
R-squared	0.064	0.062
Firm-year observations	96,397	96,397
Number of Firms	22,513	22,513

Notes: AFiD Panel, 2003–2017, single-plant firms. The dependent variables are in first differences. The independent variables are in first differences of the natural logarithm, except for the dummies (D), and kWh per full-time equivalent is the level of the previous year. The instrument used in the IV regressions is the sum of the products of energy shares and price changes of electricity, natural gas, light fuel oil, hard fuel oil, hard coal as described equation (4.6). Standard errors are clustered at the firm level and reported in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level.

Table 4.A.6: Adjustment of workforce composition, VSE data, plant level

	Dependent variable			
	Log(average tenure)	Log(hours)	Share workers complex tasks	Share workers higher education
	(1)	(2)	(3)	(4)
High energy shock	0.015	-0.006	-0.009	-0.344
(dummy)	(0.046)	(0.007)	(0.022)	(0.966)
Constant	-0.024	0.029***	0.000	0.367
	(0.034)	(0.006)	(0.017)	(0.715)
No. plants	676	678	678	674
R-squared	0.000	0.001	0.000	0.000

Notes: VSE 2006, 2010, 2014. This table shows the results of our difference-in-differences exercise. For each dependent variable, we first take differences from 2006 to 2010 and from 2010 to 2014 for each plant and then calculate the differences between both changes. Subsequently we regress this differences of changes on a dummy indicating a highly affected plant, where highly affected plants are plants located in industry-size-class experiencing a larger energy shock. We use our baseline data to identify 2-digit-industry-size-class cells which are strongly and weakly affected by energy shocks. For each cell, we calculate cell-level energy shocks as the average of plant-level energy shocks within that cell. We define a cell as being heavily affected if the cell-level energy shock is above the median of all cell-level energy shocks. Robust standard errors are reported in parantheses.***/**/* denotes statistical significance at the 1%/5%/10% level.

Table 4.A.7: Adjustment of workforce composition, VSE data, cell level

	Dependent variable			
	Log(average tenure)	Log(hours)	Share workers complex tasks	Share workers higher education
	(1)	(2)	(3)	(4)
High energy shock (dummy)	0.046	-0.016	-0.003	0.364
	(0.048)	(0.016)	(0.016)	(0.929)
Constant	-0.094**	0.027***	-0.029***	-0.320
	(0.037)	(0.008)	(0.010)	(0.525)
No. cells	83	83	83	83
R-squared	0.011	0.012	0.000	0.002

Notes: VSE 2006, 2010, 2014. This table shows the results of our difference-in-differences exercise. For each dependent variable, we first take differences from 2006 to 2010 and from 2010 to 2014 for each cell and then calculate the differences between both changes. Subsequently we regress this differences of changes on a dummy indicating a highly affected cell, where highly affected cells are industry-size-class-cells experiencing a larger energy shock. We use our baseline data to identify 2-digit-industry-size-class cells which are strongly and weakly affected by energy shocks. For each cell, we calculate cell-level energy shocks as the average of plant-level energy shocks within that cell. We define a cell as being heavily affected if the cell-level energy shock is above the median of all cell-level energy shocks. Robust standard errors are reported in parantheses.***/**/* denotes statistical significance at the 1%/5%/10% level.