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Worker Participation in Decision-making, Worker Sorting, and Firm Performance

STEFFEN MUELLER and GEORG NEUSCHAEFFER 

Worker participation in decision-making is often associated with high-wage and high-productivity firm strategies. Using linked employer–employee data for Germany and worker fixed effects from a two-way fixed-effects model of wages capturing observed and unobserved worker quality, we find that plants with formal worker participation via works councils indeed employ higher quality workers. We show that worker quality is already higher in plants before council introduction and further increases after the introduction. Importantly, we corroborate previous studies by showing positive productivity and profitability effects even after taking into account worker sorting.

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, for example, via generating collective voice, reducing information asymmetries between workers and management, and fostering

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trust and longer-term relations between them. Existing empirical research indeed demonstrates that council plants have less employee turnover (Adam 2019; Addison, Schnabel and Wagner 2001; Hirsch, Schank and Schnabel 2010), pay higher wages (Addison et al. 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 (Oberfichter and Schnabel 2019)¹ 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.² 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., Jirjahn and Smith 2018; Mueller 2012) 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, Heining and Kline 2013; Hirsch and Mueller 2020) and to productivity dispersion across plants (Syverson 2011).

¹An 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.

²This includes Andrews et al. (2012) for Germany, Bonhomme et al. (2019) using Swedish data, and Lopes de Melo (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, for example Abowd et al. (1999) for France and the United States. 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).

To analyze whether sorting explains the productivity and wage effects of works 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, Kramarz, and Margolis (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 not only observable human capital variables such as education or initial age but also unobservable components such as ability.³ 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, for example, by including the share of skilled workers (Jirjahn and Mueller 2014; Mueller 2012). 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 and profit effects of councils.⁴ We argue that the dynamics before and after council introduction provide additional insights regarding a causal interpretation of our results.

³A detailed discussion of AKM worker effects will be provided in Section “Data and Empirical Strategy”.

⁴Mueller and Stegmaier (2017) report productivity effects in a similar setting.

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 in our both 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 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.

Institutional Setting, Theory and Some Literature

Regulatory framework and worker sorting. The German system of industrial relations rests on two pillars, that is, plant-level codetermination via works

councils and sectoral collective wage bargaining between unions and employer associations.⁵ 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).⁶

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 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.⁷

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

⁵For excellent theoretical discussions on non-union worker representation and the German experience, we refer to Addison (2009) and Jirjahn and Smith (2018).

⁶Why only a small and declining share of eligible plants has a council (Oberfichter 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) and the costs of actively organizing a joint position of workers, representing their interests, and being responsible for the negotiation outcomes.

⁷The 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 and Kraft (2011) find a positive link with incremental product innovations but not with drastic innovations.

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 incentivizes 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 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 Möller (2003), Pfeifer (2011) and Grund, Martin and Schmitt (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 and 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, Marginson and Backes-Gellner (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, for example, 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.

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 work-horse 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, Schank, Schnabel and Wagner (2006) and even 30 percent in Frick and Möller (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 such as 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, Fortin and Lemieux 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., Brändle 2017; Hübler and Jirjahn 2003; Jirjahn and Mueller 2014).

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, Schnabel and Wagner 2004).⁸ 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, leading to a substantial productivity premium of council plants in the long run.⁹ 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

⁸Hübler and Jirjahn (2003) and Mueller (2012) aim on tackling council endogeneity by using endogenous 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.

⁹Jirjahn et al. (2011) report a hump-shaped link between council age and productivity in their OLS setting.

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, Teixeira and Zwick (2010) show that workers in plants with a works council benefit from works council wage premia, a result that has been reinforced by Hirsch and Mueller (2020). Recently, Dobbelaere, Hirsch, Mueller, and Neuschaeffer (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 et al. 2001; Addison and Wagner 1997). 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.

Data and Empirical Strategy

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 June 30 (Heining, Klosterhuber and Seth 2014; Schmidlein, Seth and Umkehrer 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 and Kohaut 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,¹⁰ intermediate inputs, and other plant characteristics. The administrative worker-level data provide demographic information and details about wages, education, and occupation and allow 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, Lochner, Seth and Wolter 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:

$$\log(\text{wage}_{it}) = \alpha_i + \Psi_{J(i,t)} + \mathbf{x}'_{it}\boldsymbol{\beta} + \varepsilon_{it} \quad (1)$$

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 ($\mathbf{x}'_{it}\boldsymbol{\beta}$)¹¹

¹⁰The 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).

¹¹The 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).

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.¹²

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.¹³ To obtain a time-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.¹⁴ 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 and 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 with 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 is 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

¹²Card 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 (1) including a worker–employer match effect increases the statistical fit of the German data only slightly.

¹³The time intervals Bellmann et al. (2020) use to estimate model (1) are 1985–1992, 1993–1999, 1998–2004, 2003–2010, and 2010–2017.

¹⁴AKM effects are only available for 20- to 60-year-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).

works council is dissolved, which includes plants with multiple switches in works council existence. Thus, we focus on persistent works council introductions.¹⁵ 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 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 cannot observe works council foundation in our panel, we use direct information on works council age surveyed in the years 2012, 2014, and 2016.

Finally, we construct two unbalanced panels,¹⁶ that is, 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 4.1.

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

$$y_{jt} = \beta_0 + \beta_1 woco_{jt} + \beta_2 \bar{\alpha}_{jt} + \beta_3 kn_{jt} + \beta_4 l_{jt} + controls_{jt} + u_{jt}, \quad (2)$$

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 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.¹⁷ In equation (2), $woco_{jt}$ is a dummy indicating the presence of a works council, $\bar{\alpha}_{jt}$ is the

¹⁵We observe in our final sample 63 works council dissolutions. Including them does not change our results.

¹⁶We decide for unbalanced panels to use as many observations as possible.

¹⁷Strictly 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.

standardized mean of worker quality at the plant level as described in the previous section, l_{jt} is a set of seven dummies¹⁸ 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), but use $\bar{\alpha}_{jt}$ as the dependent variable and omit this variable on the right-hand side.¹⁹

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, Upward and Wright 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 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

¹⁸We 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, and ≥ 1000 .

¹⁹Table A1 in Appendix A gives a detailed explanation the variables.

$$y_{j\tau} = \sum_{k=-L}^K \beta_k (D_{jc} \times \text{time}_{c\tau}^k) + \sum_{k=-L}^K \gamma_{1,k} \text{time}_{c\tau}^k + \gamma_2 \text{control}_{j\tau} + \mu_{jc} + \varepsilon_{j\tau}, \quad (3)$$

where $y_{j\tau}$ 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 $\text{time}_{c\tau}^k$ 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 between 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 $\text{control}_{j\tau}$ captures the same control variables as in model (2).

While being a step toward 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 groups 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 do not have a randomized experiment, we cannot 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.

Results

Descriptive findings. The descriptive statistics in Table 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 with non-council

TABLE 1
SUMMARY STATISTICS

Variable	Works council Mean (SD)	No works council Mean (SD)	Years before council introduction	
			less than 3 Mean (SD)	at least 3 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)
<i>N</i>	7467	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 “Data and Empirical Strategy”) standardized with a mean of zero and a standard deviation of one.

-†The values are not shown due to reasons of data protection.

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 with non-council plants, the 67 plants

introducing a council have higher labor productivity, wages, and worker quality even before the introduction.²⁰ Their outcomes are, however, worse than those 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.

Worker sorting. When analyzing worker sorting, the standardized plant-level average of the AKM worker effect ($\bar{\alpha}_{jt}$) becomes the dependent variable in model (2). The corresponding OLS regression results are displayed in Table 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 Data). 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 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 1. It shows the coefficients of the relative time dummies²¹ with their 90% confidence intervals, where the three years before introduction (-3 to -1) serve as base category. In our baseline specification (Figure 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 supports the conclusion that council introduction 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 1A) so that we conclude that our results are not affected by any issue that might arise from

²⁰More 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; and in the $[6, 8]$ -interval 126, and nine years after council introduction, we have 155 plant-year observations.

²¹The six relative time intervals are time_{ct}^k ($k \in \{[-\infty, -4]; [-3, -1]; [0, 2]; [3, 5]; [6, 8]; [9, \infty]\}$)

TABLE 2
 WORKER QUALITY, OLS REGRESSIONS

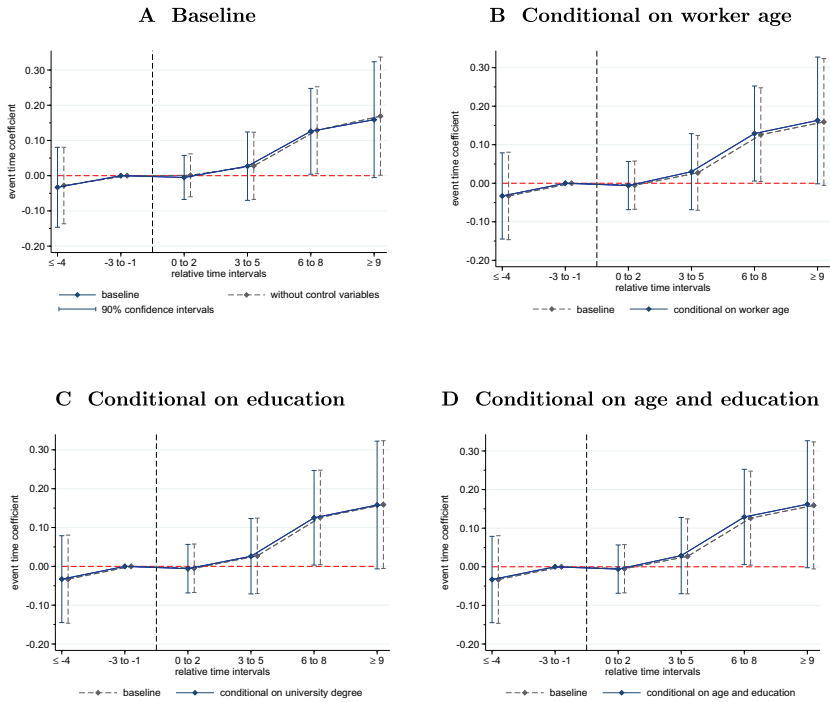
	(1)	(2)	(3)	(4)
Works council	0.194*** (0.028)	0.157*** (0.026)	0.142*** (0.032)	0.115*** (0.025)
Skilled employees		0.857*** (0.043)	0.858*** (0.043)	0.700*** (0.042)
Collective bargaining	0.039* (0.023)	0.011 (0.022)	0.004 (0.027)	0.007 (0.021)
Works council × collective bargaining			0.026 (0.039)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

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.

FIGURE 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 “Data and Empirical Strategy”) 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 (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 (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 [Color figure can be viewed at wileyonlinelibrary.com]

We add average worker age to the event study, but the post-event coefficients stay unchanged (Figure 1B). The council coefficients do not change a lot either, when we control for the share of university graduates alone (Figure 1C) or jointly with average worker age (Figure 1D). We conclude that the increase in worker quality is driven by an increase in unobserved components of the AKM worker effect.²²

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 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.²³ 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 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 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.²⁴

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 in unobserved worker quality rather than changes in workers' formal education or age drive quality improvements.

²²In Appendix B, 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 main results are unchanged.

²³We 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.

²⁴Presumably 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.

TABLE 3

WORKER QUALITY OF JOINING, LEAVING, AND STAYING WORKERS, OLS REGRESSIONS

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

Productivity. Table 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 controlling for worker quality and shows that

TABLE 4
LABOR PRODUCTIVITY, OLS REGRESSIONS

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

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 0.128.²⁵ 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.²⁶

Coefficients for covariates not at the center stage of our analysis show no surprises; that is, 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.²⁷ 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 Tables 1 and 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 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 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

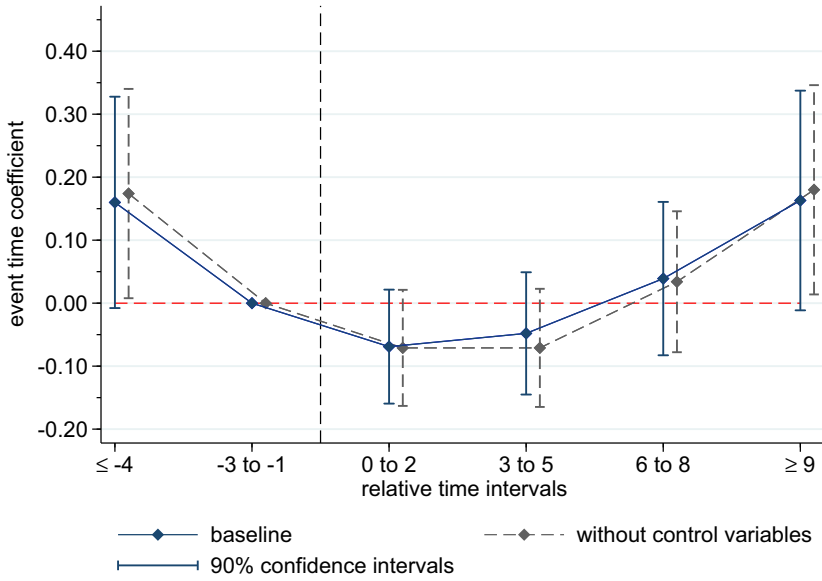
²⁵Albeit being very precisely estimated, the difference in council coefficients is not statistically significant.

²⁶Note 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.

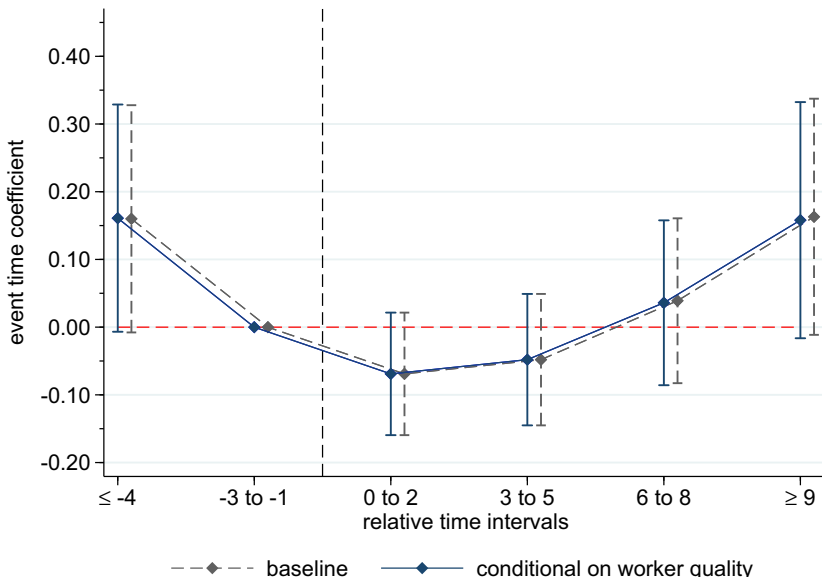
²⁷We do not control for the ownership structure in our regressions as this would reduce our sample size without changing any results.

FIGURE 2
LABOR PRODUCTIVITY, 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 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 “Data and Empirical Strategy”) 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 (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 (3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level [Color figure can be viewed at wileyonlinelibrary.com]

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 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.

Wages. Table 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, for example, 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 such as 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.

TABLE 5
WAGES, OLS REGRESSIONS

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

The interaction between 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 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 3B barely change.

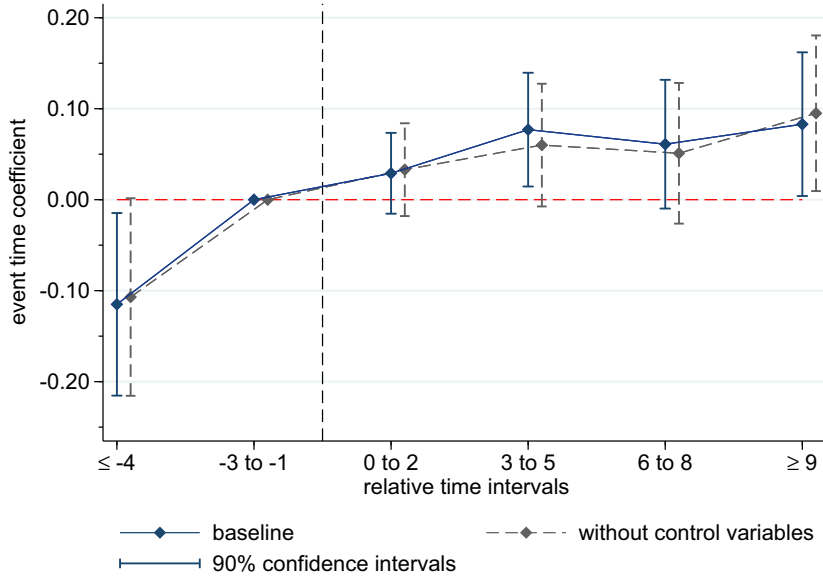
Profits. Table 6 presents our OLS estimates for the quasi-rent, where we interpret regression coefficients as profit effects because we always condition on capital intensity. The coefficients of the control variables show no surprises. Confirming Mueller (2011), we report a positive link between council existence and profits across all specifications. Controlling 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 estimated a council coefficient of 7200 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 4 are similar to the productivity estimates but its U-shape is more pronounced. Controlling for worker quality does not 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.

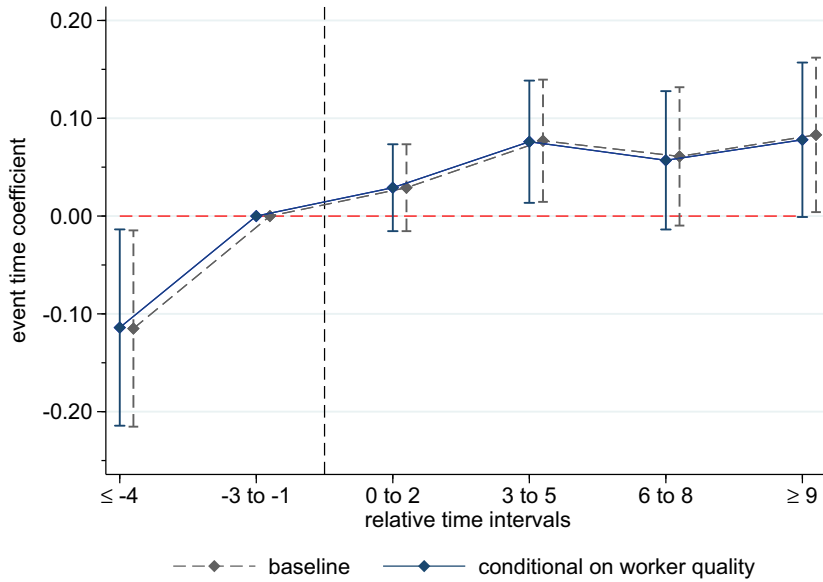
After council introduction, profits rise since productivity increases and wage growth flattens. While the post-event coefficients are insignificant, they are in line with the OLS results and imply that the positive association between

FIGURE 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 “Data and Empirical Strategy”) 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 (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 (3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level [Color figure can be viewed at wileyonlinelibrary.com]

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.

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

TABLE 6
PROFITS, OLS REGRESSIONS

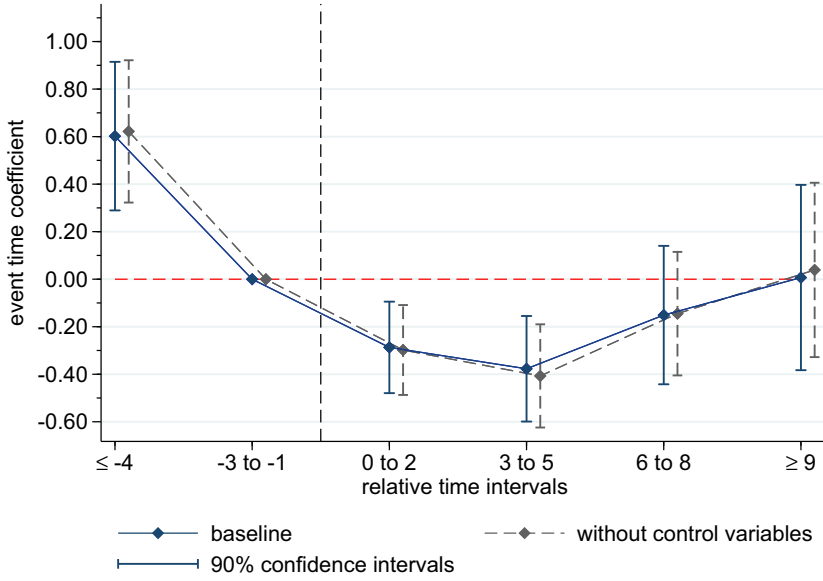
	(1)	(2)	(3)	(4)	(5)	(6)
Works council	0.168*** (0.044)	0.155*** (0.044)	0.140*** (0.044)	0.108** (0.053)	0.127*** (0.045)	0.095* (0.053)
Skilled employees		0.303*** (0.054)	0.222*** (0.054)	0.306*** (0.054)	0.206*** (0.055)	0.225*** (0.054)
Worker quality			0.094*** (0.016)		0.086*** (0.016)	0.094***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

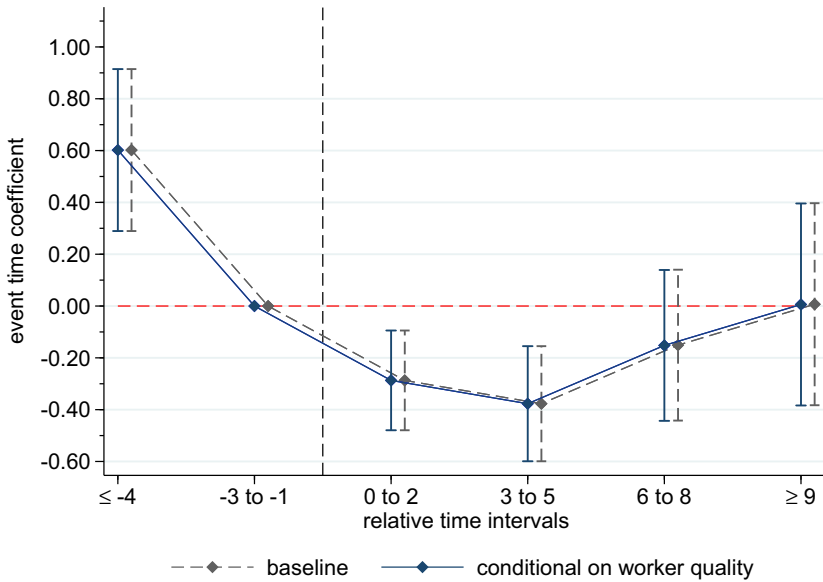
labor productivity, wages, and profits, respectively, in OLS regressions. In the fixed-effects event study setting, we found productivity increases within plants conditional on worker quality. We conclude that worker sorting is not

FIGURE 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 “Data and Empirical Strategy”) 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 (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 (3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level [Color figure can be viewed at wileyonlinelibrary.com]

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 A

Definitions of Variables

TABLE A1
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 "Data and Empirical Strategy") 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 as 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 with 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
University graduates as share of all employees	Share of university graduates

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

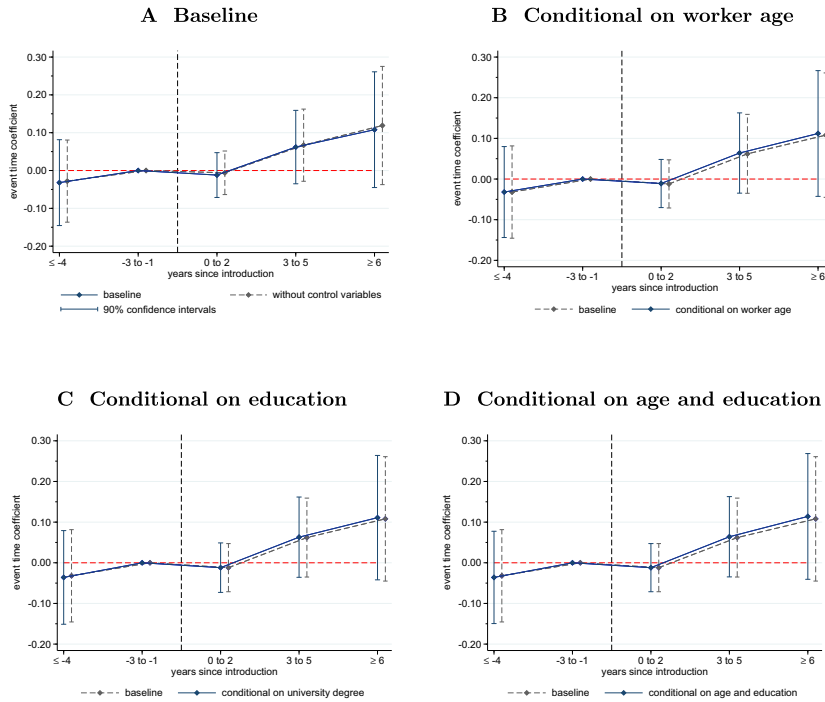
Robustness Checks

Selective panel attrition. In this section, we present robustness checks of the results in Section 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 Figures B1-B4 and show for each outcome the same patterns as in our baseline results presented in Section 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 B1

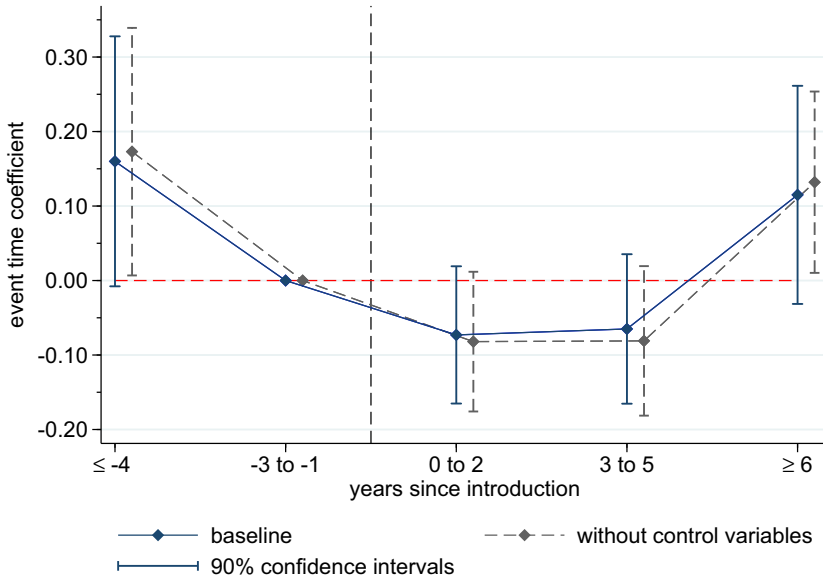
WORKER QUALITY, EVENT STUDY (YOUNG WORKS COUNCILS).



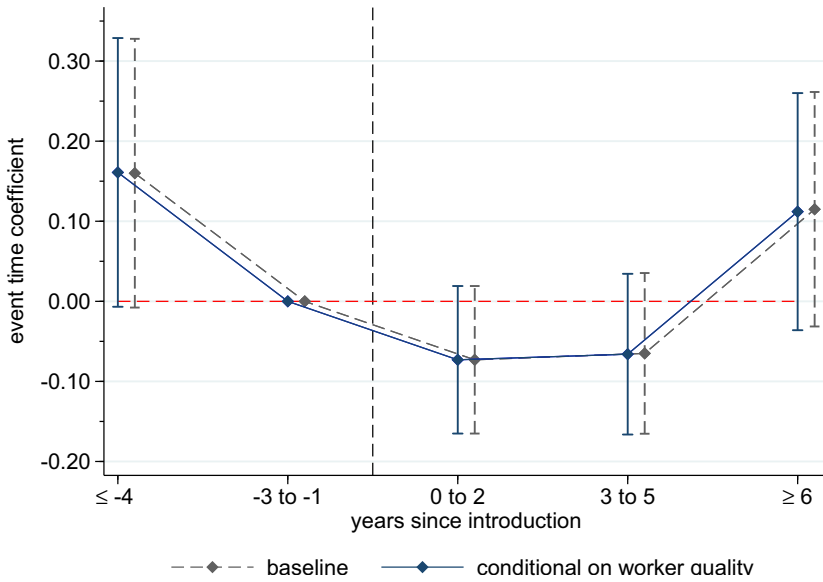
Notes: LIAB cross-sectional model, 1998–2016, West Germany, event study sample (140,782 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of worker quality relative to the pre-introduction period of the works council and net of the 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 “Data and Empirical Strategy”) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, which 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 (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 (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 [Color figure can be viewed at wileyonlinelibrary.com]

FIGURE B2
LABOR PRODUCTIVITY, EVENT STUDY (YOUNG WORKS COUNCILS).

A Baseline



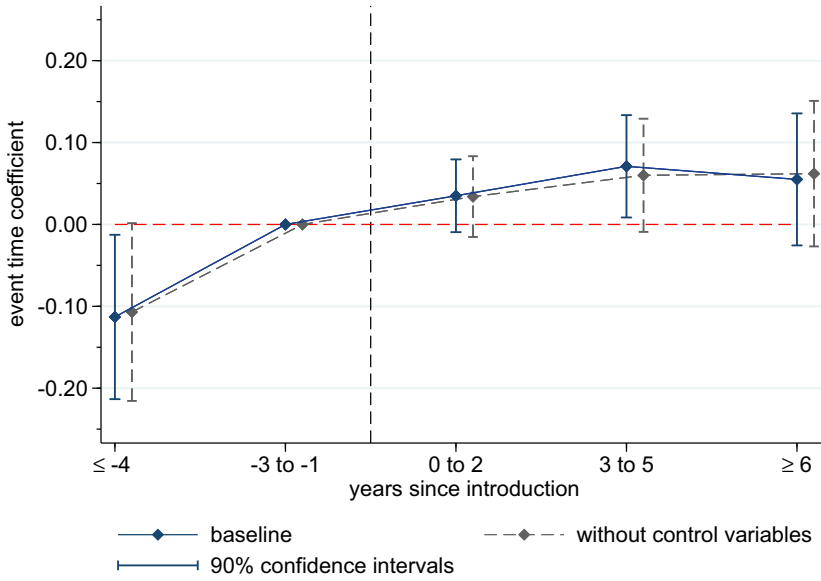
B Conditional on worker quality



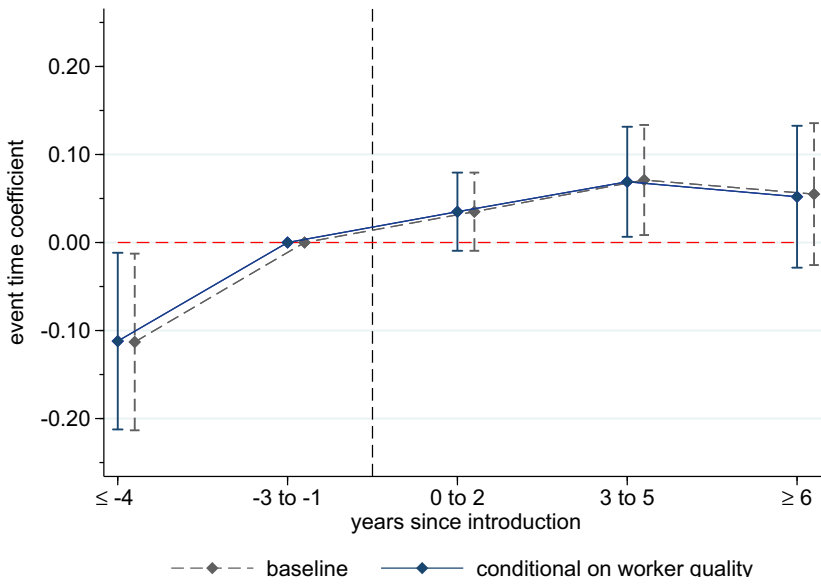
Notes. LIAB cross-sectional model, 1998–2016, West Germany, event study sample (140,782 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of value added divided by the number of employees relative to the pre-introduction period of the 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 “Data and Empirical Strategy”) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, which 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 (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 (3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level [Color figure can be viewed at wileyonlinelibrary.com]

FIGURE B3
WAGES, EVENT STUDY (YOUNG WORKS COUNCILS).

A Baseline



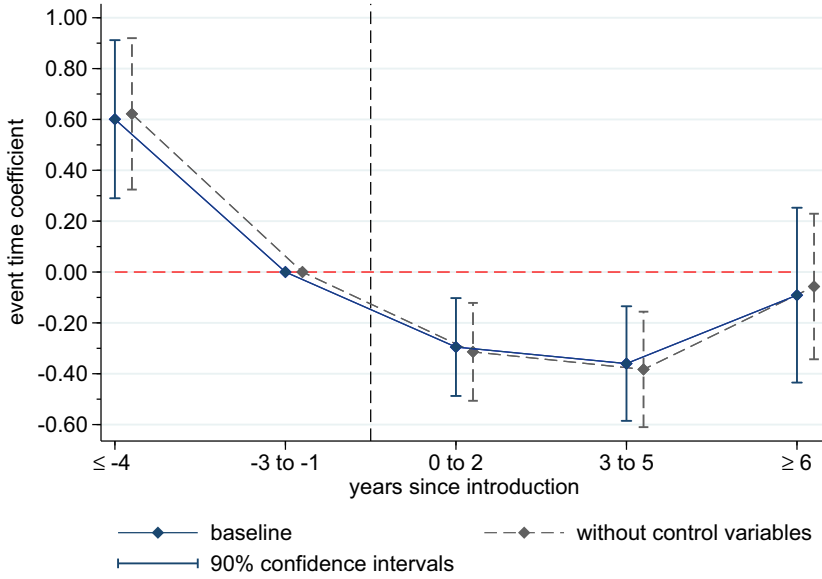
B Conditional on worker quality



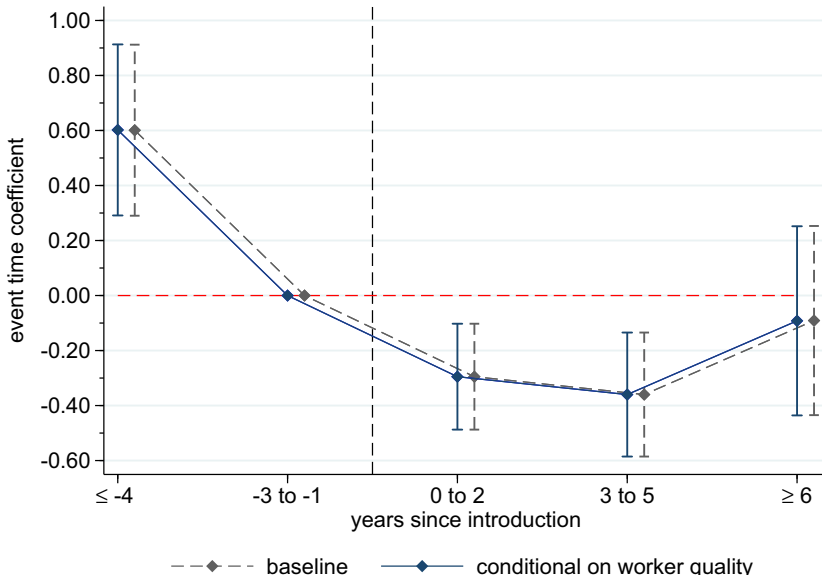
Notes. LIAB cross-sectional model, 1998–2016, West Germany, event study sample (140,782 plant-year-cohort observations). Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of the wage bill divided by the number of employees relative to the pre-introduction 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 “Data and Empirical Strategy”) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, which 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 (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 (3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level [Color figure can be viewed at wileyonlinelibrary.com]

FIGURE B4
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 “Data and Empirical Strategy”) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, which 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 (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 (3) are used. For panel B: holding constant worker quality. The 90% confidence intervals are shown using standard errors clustered at the plant level [Color figure can be viewed at wileyonlinelibrary.com]