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Abstract

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 establishments 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.

Keywords: works councils, worker sorting, worker quality, between-firm wage inequality, productivity, profits

JEL classification: J5, J24, J31

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1 Introduction

Mandated worker participation in firm decision-making is present in many European countries for decades. Whether employee participation boosts productivity and drives up wages has been discussed intensively and is nowadays increasingly relevant against the background of the productivity slowdown and falling labor shares in national income. The German model of plant-level participation via works councils has attracted particular interest because of the strong legal rights councils enjoy there. Standard economic theory perceived 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 firm productivity directly, e.g. via generating collective voice, reducing information asymmetries between workers and management, and fostering trust and longer-term relations between them. Mounting empirical research indeed demonstrates that council firms have less employee fluctuation (Addison *et al.* 2001, Hirsch *et al.* 2010, Adam 2019), are able to 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 (Oberfichtner 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 establishments has been documented in a number of studies.[2] As works council establishments usually are high-productivity high-wage establishments, a core question is whether councils directly increase these outcomes or whether council establishments employ workers of higher quality who will increase productivity (see Bender

[1] An important question is why 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. Mueller and Stegmaier (2018) provide an explanation for employer resistance against works councils even in the light of positive profitability effects of councils.

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

et al. 2018) and earn higher wages by definition. The worker codetermination literature usually argued along the lines of the first scenario (e.g. Mueller 2012, Jirjahn and Smith 2018) and less so regarding a potential self-selection of high-quality workers into works council firms. However, for most workers, going to a high-paying firm offering stable employment perspectives is attractive and, hence, assortative matching of high-quality workers into high-paying works council plants seems plausible.

Should assortative matching be the driver of positive outcomes of works council firms, high-wage high-performance firms with works councils would coexist with low-wage low-performance firms 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-firm wage inequality (Card *et al.* 2013, Hirsch and Mueller 2020) and productivity dispersion across firms (Syverson 2011).

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 in Abowd *et al.* (1999, henceforth AKM) and implemented by Card *et al.* (2013) for Germany. In this model, higher worker-effects are rewarded higher across all employers, which justifies labeling individuals with high AKM worker effects as high-quality workers. Importantly, AKM person effects capture all human capital components that are invariant in the time span under consideration and therefore include observable human capital variables like education or initial age but also unobservable concepts like 'ability' or 'motivation'.[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 only roughly control for worker quality, e.g. including the share of skilled workers (Mueller 2012, Jirjahn and Mueller 2014). To the extent that these controls do not fully capture unobserved worker quality differences, previous studies may suffer from an omitted

[3] A detailed discussion of AKM person effects will be provided in section 3.

variable bias of unknown magnitude and our ability to fix this 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 establishment performance and wages is driven by sorting, we also look at profit effects to see whether the net effect of councils on productivity and wages benefits employers. In doing so, we examine to what extent the surplus generated by councils is shared with workers and we therefore present evidence on how worker involvement 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 in the spirit of Mueller and Stegmaier (2017) and analyze works council introductions in a 'within-firm' approach. We extend Mueller and Stegmaier (2017) in several ways and in particular provide first results for wages and profits. The dynamics before and after council introduction provide additional insights regarding a causal interpretation of our results.

We will find that council firms indeed employ workers of higher quality even if other plant characteristics are 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 firms' labor productivity but that the OLS estimate of the works council effect declines just by one fifth if AKM person effects are controlled for. In fixed effects event study regressions, the council effect is unchanged when AKM person 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 only moderately upwards. We also find that council establishments pay higher wages, though some increase in wages is already present before the council introduction. We show that the surplus originating from council firms' higher labor productivity is shared by firms and workers and find positive profitability effects both in our OLS and fixed effects frameworks. High-quality workers' productivity premium is higher in council firms, which confirms the existence of complementarity between worker participation and worker quality. In combination, our fixed effects event study results show that *future* council plants as compared to non-council plants experience a turbulent time with worker churning, stronger wage growth and a productivity decline that sharply reduce pre-introduction profits. After council introduction, wage growth flattens and productivity growth sets in, which allows council firms to sustain long-run profitability within a high-wage high-productivity strategy.

Our paper is similar in spirit to Bender *et al.* (2018) who are also interested in worker sorting and firm productivity but focus on management practices instead of formal employee participation. The main difference to Bender *et al.* (2018) is that we show how collective worker action leads firms to employ better workers, which 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 adoption. The main take away will therefore be that an adequately designed scheme for worker participation in decision making can shift firms into an equilibrium with high wages and high productivity.

2 Institutional setting, theory and some literature

2.1 Regulatory framework and worker sorting

The German system of industrial relations rests on two pillars, i.e. plant-level codetermination via works councils and sectoral collective wage bargaining between unions and employer associations.[4] The works constitution act (Betriebsverfassungsgesetz) requires works councils to act in the interest of workers and the firm 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 - that will be the focus of our analysis - worked in

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

the 9 percent of plants that have a works council (Ellguth and Kohaut 2016).[5]

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 firm'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 firm, information rights may, for instance, prevent inefficient firm 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, this need for consultation may reduce employers incentives to adapt new technologies, but on the other hand, if managed appropriately, the consultation process addresses potential fears of workers and results in a well informed workforce committed to the desired change.[6]

Works councils have their strongest rights in social matters. For instance, if a council formally disagrees with an individual dismissal this dismissal turns void until a labor court finally decides the matter. Firing costs thus increase for employers and this may well have implications on productivity and sorting. Increased firing costs may, at 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 incentives individual workers to care about the economic viability of their firm. Firms may react to increased firing costs by investing in screening activities

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[5] Why only a small and declining share of eligible firms has a council (Oberfichtner and Schnabel 2019) is not fully understood. Employers are prohibited to interfere with works council elections and even have to bear the costs for running the election. Once elected, councilors enjoy very strong employment protection. Because of this, and because time spent on work as a works councilor counts as regular working time, the nonexistence of councils in many eligible plants points to additional costs potential councilors face. This cost may, for instance, include the costs of positioning oneself as a works councilor while many employers have reservations against codetermination (Mueller and Stegmaier 2018) as well as the costs of actively organizing a joint position of workers, representing their interests, and being responsible for the negotiation outcomes.

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

when hiring new workers, which in turn should improve their ability to identify high-productivity workers.

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 fluctuation in codetermined plants. Using plant-level data, Frick (1996) finds that works council existence is related to fewer quits and, among others, Addison *et al.* (2001), Frick and Moeller (2003), Pfeifer (2011), and Grund *et al.* (2016) confirm that fluctuation is reduced. Whether these are indeed direct collective voice effects or whether they are rather monopoly 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 to estimate a difference-in-differences setting and finds strong voice effects being the source for reduced fluctuation. To sum up, the literature almost uniformly finds reduced employee fluctuation and some role for collective voice in explaining it.

On top of enjoying a stable job as well as stronger legal rights in the workplace, one of the main arguments for workers to go to works council firms 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 firm wage premia even conditional on firms' quasi rents.

Collective wage bargaining between unions and employer associations forms the second cornerstone 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 from each other, works councils and unions have close ties, with unions, e.g. providing works councilors with resources and councils recruiting new union members at the shop floor (Behrens 2009). Freeman and Lazear (1995) argue that the existence of sector level wage bargaining should increase the productivity effect of councils because councils are then less engaged in distributional conflicts and care more about increasing the overall pie to be shared between workers and the firm.

2.2 Works councils and firm and worker outcomes

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

One major issue that has long been unresolved is works council endogeneity due to unobserved firm 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 firms over time, which makes it hard to detect statistically significant evidence in any fixed effect or first difference estimation strategy. Early attempts to use fixed effects estimators indeed yielded insignificant productivity effects (Addison *et al.* 2004).[7] 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 firms after an introduction period of about five years, leading to a substantial productivity premium of council firms in the long run. 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 establishments facing adverse conditions, a finding that has repeatedly been used to argue that conventional estimates of productivity effects of works councils are, if at all, biased downwards. Although Mueller and Stegmaier (2017) do not aim on tackling employee sorting and were 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 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, literature on the productivity effects of works councils finds univocally non-negative and in most cases substantial positive effects. With few exceptions, this

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

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.

Literature on works councils and wages documents mainly a positive relationship. Using collective voice, works councils can be assumed to strengthen the bargaining power of workers. Through their extensive rights to intervene in management decisions, works councils have additional possibilities to increase workers' bargaining power. Using a sample of manufacturing firms from Lower Saxony, Addison *et al.* (2001) find 15% higher wages in works council establishments. Later studies using linked employer-employee data support the positive relationship between works councils and wages (Gürtzgen 2009). At the individual level, Addison *et al.* (2010) show that workers in establishments with a works council benefit from works council wage premiums, a result that has been reinforced by Hirsch and Mueller (2020).

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 literature uses subjective management assessments of profits and finds a negative relationship between works councils and profits supporting the view that wage increases do more than fully eat up productivity gains (Addison and Wagner 1997, Addison *et al.* 2001). Using an objective measure of profits, Mueller (2011) finds a positive relationship between profits and works councils. In line with Freeman and Lazear (1995), the profit effect in Mueller (2011) is higher when a collective wage agreements is present. Again, these studies do not fully control for (un)observed worker quality. Whether previous studies over or underestimate the profit effects 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.

3 Data and empirical strategy

3.1 Data

We use the Linked-Employer-Employee-Data (LIAB) of the Institute for Employment Research (IAB), which links survey information from the IAB Establishment Panel to social security records of all workers who are subject to social security contributions and are employed at a survey establishment (Heining *et al.* 2014, Schmidtlein *et al.* 2019). The IAB Establishment Panel covers yearly information from 1993 (1996 for East Germany) onwards. It is a representative survey of German establishments with at least one employee subject to social security contributions (Ellguth *et al.* 2014). Since 2001, it covers between 15,000 and 16,000 establishments per year and contains information on works council existence, revenue, employment, capital stock,[8] intermediate inputs and other establishment characteristics. The works council age is surveyed in the years 2012, 2014, and 2016. We use this information alongside with the panel structure of our data to determine the year of council introduction. The worker data provide demographic information and details about wages, education, and occupation. To generate our worker quality measure at the plant level, we use the AKM person fixed effects, Bellmann *et al.* (2020) estimated for apprentices and full time workers liable to social security aged 20 to 60 for the years 1985 to 2017.

We discard the survey years 1993-1997 as information on works councils and other covariates are missing or incomplete for those years. We lose the year 2017 as survey information on revenue and intermediate inputs asked in year t always refers to year $t-1$. Thus, we cover the years 1998 to 2016 in our sample. We drop East German plants to exclude the influence of the dramatic structural changes after the German reunification in the 1990s and of different conventions of industrial relations before 1990 (Behrens 2009) that might be persistent. Plants that are publicly owned or belong to financial services, insurance, or the real estate industry are omitted as for those industries measures of sales

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

(financial services and insurances) or capital stock (real estate) are ill defined. We exclude plants with less than five permanent employees because they are legally not entitled to introduce a works council. 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 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 person characteristics at the plant level. Worker fixed effects come from the following wage model:

$$log(wage_{it}) = \alpha_i + \Psi_{J(i,t)} + x'_{it}\beta_k + \epsilon_{it} \tag{1}$$

where the logarithm of the wage of worker i is the sum of a time invariant worker-effect (α_i), a time invariant establishment-effect ($\Psi_{J(i,t)}$) for the establishment worker i is employed at time t, plus time varying worker characteristics ($x'_{it}\beta_k$)[9] affecting workers' wages equally at all firms, and a residual pay component ϵ_{it}, which is by assumption independent of the right-hand-side variables.[10]

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

The *levels* of AKM effects as originally estimated by Bellmann *et al.* (2020) can

9 The time varying person characteristics ($x'_{it}\beta_k$) 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).

10 Card *et al.* (2013) discuss exogeneity assumptions in detail and provide suggestive evidence for them being fulfilled.

only be interpreted *within* the time intervals they used for estimation.[11] 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 establishment level.[12] The average quality of workers within a certain establishment and a certain time interval is fixed unless worker composition changes.

3.2 Empirical strategy

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

$$y_{jt} = \beta_0 + \beta_1 woco_{jt} + \beta_2 \overline{a}_{jt} + \beta_3 kn_{jt} + \beta_4 l_{jt} + controls_{jt} + u_{jt}, \qquad (2)$$

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 minus 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.[13] In equation (2), $woco_{jt}$ is a dummy indicating the presence of a works council, $\overline{a}_{jt}$ is the standardized mean of worker quality at the plant level as described in the previous section, l_{jt} is a set of seven dummies[14] 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, plus dummies for export, single-plant status, and the technical

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

[12] Part-time employees, minijob workers, employees younger than 20 and older than 60 years are excluded from the aggregated measure of individual person characteristics at the establishment level (Card *et al.* 2013). We also exclude workers who have an employment status other than "employees liable to social security without special characteristics" or "trainees without special characteristics" (Bellmann *et al.* 2020).

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

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

sophistication of the equipment. We further include dummies for 2-digit industries, regions and years. For the worker sorting regressions we use $\overline{a}_{jt}$ as the dependent variable and omit this variable on the right hand side.

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

In a standard difference-in-differences setting, the considered event happens at the same point in time for all treated units. In our case, however, works council introductions are observed for almost all time periods analyzed. We therefore apply a setting in relative time that reorganizes the data such that all events happen at the same point in relative time (see e.g. Hijzen *et al.* 2010). We define yearly event cohorts where the treatment group of a particular cohort consists of plants that have no works council in the previous observed years but have one in the event year. The control group of a particular cohort consists of all plants, which 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. The relative time indicator is set to zero in the year 2005 for this event cohort.

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 to 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 sample, and each observation is indexed by relative time (t), a plant identifier (j), and a cohort identifier (c). The model to be estimated is

$$y_{jct} = \sum_{k=-L}^{K} \beta_k (D_{jc} \times time_{ct}^k) + \sum_{k=-L}^{K} \gamma_{1,k} time_{ct}^k + \gamma_3 control_{jct} + \mu_{jc} + \varepsilon_{jct}, \quad (3)$$

where y_{jct} are the same outcomes as defined above for the OLS estimations. The plant-cohort fixed effect μ_{jc} ensures that we only compare the within plant variation of the control and treatment groups within one, but not across cohorts. Each dummy variable $time_{jct}^k$ equals one in the observed relative time period and captures time effects. The plant-cohort specific dummy D_{jc} equals one if a plant introduces a works council in a specific cohort and equals zero otherwise. The interaction of dummy D_{jc} and the relative time dummies captures the evolution of works council plants over time relative to non-council plants and β_k thereby identifies the council effect along the entire set of time dummies. We omit the relative time dummy in the year of the council introduction to define the 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 introduction period purged from cohort-specific time trends identified via control group plants. The vector $control_{jct}$ captures the same control variables as in model (2).

4 Results

4.1 Descriptive findings

The descriptive statistics in Table 1 reinforce standard results in terms of showing that council plants have higher labor productivity, pay higher wages, earn higher quasi rents, employ more workers, have a higher capital intensity, and are more likely to be covered by a collective wage agreement. A new result is that works council plants employ workers whose AKM person effects are almost one half of a standard deviation higher compared to non-council firms. Interestingly, the share of skilled workers is very similar across both groups of plants indicating that AKM worker effects indeed capture a different information set and discriminate 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 and high-productivity plants.

Columns (3) and (4) summarize the outcomes of plants before the introduction of a works council. Compared to non-council plants, the 67 plants introducing a council have higher labor productivity, wages and worker quality even before the introduction.[15] Their outcomes are, however, worse than that of council plants, which indicates that works council introduction may further improve outcomes. Hence, our results show descriptively that plants with high performance, somewhat higher worker churning, and high worker quality seem to be more likely to introduce a council and that performance and wages increase after council introduction whereas churning decreases. To scrutinize these results, we later show the dynamics before and after the council introduction in a multivariate fixed effects event study setting.

4.2 Worker Sorting

The OLS regression results of model (2) explaining AKM worker effects at the plant level are displayed in Table 2. Works council existence enters positively and significantly in all specifications. Omitting the fraction of skilled jobs (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 jobs (column 2) reduces the coefficient to 0.157. Remember that the AKM worker effect captures also observable human capital components embodied in age and formal education (see section 3.1). Including in the regression both average worker age and the share of workers having an university degree reduces the council coefficient to 0.115 (column 4). Hence, including both observable AKM components does only account for a small fraction of the worker quality effect.

The event study results for the average AKM person effect at the plant level are depicted in Figure 1. It shows the coefficients of the relative time dummies[16] with their 90% confidence intervals, where the post introduction years 1 to 3 serve as base category. In our baseline specification (Figure 1A) worker quality rises by 0.164 standard deviations from introduction to a works council age of more than 9 years. The insignificant pre-

[15] Less than three years before council introduction there are 106 plant-year observations, in the $[-2,0]$-relative-time-interval 186, in the $[1,3]$–interval 183, in the $[4,6]$–interval 136, in the $[7,9]$–interval 126, and in years after ten years of council introduction we have 155 plant-year observations.

[16] The six relative time intervals are $time_{ct}^{k}$ ($k \in \{\leq -3; [-2,0]; [1,3]; [4,6]; [7,9]; \geq 10\}$)

event trends support the conclusion that council introduction increases worker quality as opposed to a narrative where councils are introduced in plants that upgrade worker quality anyway. Our results remain unchanged once we omit the control variables (Figure 1A) and we conclude that our results are not affected by any issue that might arise from controlling for post-treatment realizations of the control variables (sometimes called 'bad control' problem).

Upgrading along time-invariant observable worker characteristics is one possible explanation for worker quality improvements after council introduction. We add average worker age to the event study, but the post-event coefficients stay unchanged (Figure 1B). The council coefficients do not change significantly either, when we control for the share of university graduates alone (Figure 1C) or jointly with 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. We further check whether higher quality of newly hired workers or lower quality among separators improve worker quality but, presumably due to the limited number of council introductions in our sample, find no clear evidence for either explanation.

Summing up, we find that worker quality is higher in works council plants, that this difference is partly already present before council introductions, and that it increases further after the council is introduced. Our event studies further show that improvements of unobserved worker quality and not changes of workers' formal education or age drive quality improvements.

4.3 Productivity

Table 3 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 council firms 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 person effects (column 3) yields a strong positive impact of it on productivity and a further reduction of the council effect from 0.145 to 0.128. 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 person effects are strongly related to productivity even if the percentage of skilled jobs is controlled for.

Coefficients for covariates not in the center of our analysis show no surprises, i.e. establishments 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 do ceteris paribus have higher labor productivity. In column (4) we confirm the strongly positive interaction effect between councils and collective agreements. Column (5) shows that interacting worker quality and council status yields a significant and positive coefficient, which means that the effect of worker quality on productivity is by one third larger in council plants. This leads to the conclusion that while council plants do employ better workers as documented in Table 1 and Table 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 council introducing plants experience a downturn in productivity before the introduction and increasing productivity as the council grows older. This fits to 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. 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 is unrelated to council introduction (see Table 1) and, thus, captured by the fixed effect. We thus support Mueller and Stegmaier (2017) in their conclusion that the productivity increase is a genuine council effect.

4.4 Wages

Table 4 shows our OLS wage estimates. The table follows the same structure as the productivity table and results are based on the same sample. The coefficients of the control variables mostly have the same sign as in the productivity regressions, underlining the close link between productivity and wages. Without controlling for skill requirements and worker quality, works councils are ceteris paribus associated with 12 percent higher wages (column 1), which drops to 10 percent when the share of skilled workers is added (column 2). Our estimates are smaller than e.g. those in Addison *et al.* (2001) who reported about 15 percent higher wages. Adding AKM person effects reduces the council wage premium further to about 8 percent (column 3). The relatively mild reduction of the council coefficient shows that the council premium is not fully explained by the council plants' better workers. It rather supports the notion that factors like the workers' bargaining power drive the council premium (Hirsch and Mueller 2020). Our results show that one standard deviation increase in AKM person effects is associated with a wage increase of 11 percent (column 3), conditional on the share of skilled jobs.

The interaction of council existence and collective agreements is positive and significant (column 4) and adding AKM person effects does not shape the interaction effect (column 6). The interaction between council existence and AKM person 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 firms.

The results of the event studies for wages are shown in Figure 3. Though we find insignificant wage increases up to 5.4 percent after the works council introduction, the pre-event dummies are negative, statistically significant, and increase over time. 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 may 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 are barely changed.

4.5 Profits

Table 5 presents our OLS estimates for the quasi rent, where we interpret regression coefficients as profit effects because we always condition on capital intensity. Again, the table follows the same structure as the tables for productivity and wages and uses the same sample. 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 to 0.168 (in the specifications without interaction terms). Hence, councils are *ceteris paribus* associated with about 15 to 18 percent higher profits. This is in the same order of magnitude as reported in Mueller (2011) who estimated a council coefficient of 7,200 Euro and an average quasi rent of 33,300 Euro.

We also find some (statistically insignificant) confirmation of the positive interaction between works councils and collective wage agreements as theoretically suggested by Freeman and Lazear (1995) and empirically confirmed in Mueller (2011). Interestingly, AKM person effects are themselves positively related to profitability. This suggests that employers capture parts of the additional productivity high-wage workers bring to the company, which provides a rational 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 to 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 works councils and wages is outpaced by the positive effect on productivity, which ultimately increases profits.

In combination our results show that *future* council plants experience turbulent times 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 firms to sustain long-run profitability within a high-wage high-productivity strategy.

5 Conclusions

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

Finally, we show a positive link between council existence and establishment 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 *future* council plants experience turbulent times 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 firms to sustain long-run profitability within a high-wage high-productivity strategy.

We conclude that councils contribute to productivity, wage, and profit dispersion across establishments, first, by attracting and sustaining high-wage high-productivity workers and, second, by a genuine council effect on all three outcomes. We show strong positive productivity contributions of high-wage workers that are even stronger when works councils are present. This is lending support to the notion that worker quality and worker participation as a form of high performance management practices are complements in productivity. We conclude that sorting of high-quality workers to works council firms can improve allocative efficiency and aggregate productivity.

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Tables

Table 1: Summary statistics

Variable	Works Council	no Works Council	years before council intro. 2 − 0	years before council intro. < 3
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Log(Labor productivity)	11.280 (0.606)	10.905 (0.663)	11.105 (0.685)	11.351 (0.721)
Log(Wage bill per worker)	10.386 (0.397)	10.006 (0.527)	10.244 (0.473)	10.218 (0.528)
Log(Profit per worker)	10.281 (1.232)	9.894 (1.275)	10.009 (1.346)	10.608 (1.164)
Log(Employment)	5.368 (1.201)	3.141 (1.040)	4.225 (1.088)	3.981 (0.984)
Log(Capital intensity)	11.042 (1.234)	10.509 (1.246)	10.488 (1.875)	10.620 (1.202)
Collective bargaining	0.783 (0.412)	0.341 (0.474)	0.414 (0.494)	0.340 (0.476)
Worker quality	0.292 (0.700)	-0.144 (1.090)	0.284 (0.884)	0.262 (0.923)
Skilled employees as share of all employees	0.674 (0.250)	0.643 (0.252)	0.689 (0.279)	0.670 (0.273)
Part-time employees as share of all employees	0.110 (0.162)	0.209 (0.205)	0.146 (0.206)	0.146 (0.218)
Apprentices as share of all employees	0.041 (0.039)	0.051 (0.073)	0.043 (0.050)	0.044 (0.051)
Female employees share of all employees	0.275 (0.215)	0.374 (0.262)	0.317 (0.259)	0.332 (0.243)
Churning-rate	0.041 (0.066)	0.059 (0.267)	0.068 (0.134)	0.064 (0.118)
Exporter	0.693 (0.461)	0.363 (0.481)	0.468 (0.500)	0.651 (0.479)
Single plant	0.509 (0.500)	0.835 (0.371)	0.516 (0.501)	0.651 (0.479)
Technical state of machinery				
excellent	0.176 (0.381)	0.220 (0.414)	0.253 (0.436)	0.236 (0.427)
good	0.514 (0.499)	0.503 (0.500)	0.414 (0.494)	0.575 (0.497)
fair	0.275 (0.447)	0.256 (0.436)	0.306 (0.462)	-†
poor	0.034 (0.181)	0.022 (0.145)	-†	-†
Average worker age	41.961 (3.489)	41.132 (5.501)	39.865 (3.992)	39.308 (3.978)
University degree share of all employees	0.082 (0.122)	0.063 (0.135)	0.095 (0.177)	0.103 (0.210)
N	7,467	15,109	186	106

Notes: LIAB cross-sectional model, 1998–2016, West Germany. Summary of 22,576 plant-year observations. Worker quality is the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one.
-† The values are not shown due to reasons of data protection.

Table 2: Worker quality, OLS regressions

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

Notes: LIAB cross-sectional model, 1998–2016, West Germany. The dependent variable is the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from an OLS with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are 7 plant size dummies, 15 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

Table 3: Labor Productivity, OLS regressions

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

Notes: LIAB cross-sectional model, 1998–2016, West Germany. The dependent variable is the logarithm of the value added divided by the number of employees. Worker quality is the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from an OLS with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are 7 plant size dummies, 15 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

Table 4: Wages, OLS regressions

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

Notes: LIAB cross-sectional model, 1998–2016, West Germany. The dependent variable is the logarithm of the wage bill divided by the number of employees. Worker quality is the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from an OLS with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are 7 plant size dummies, 15 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

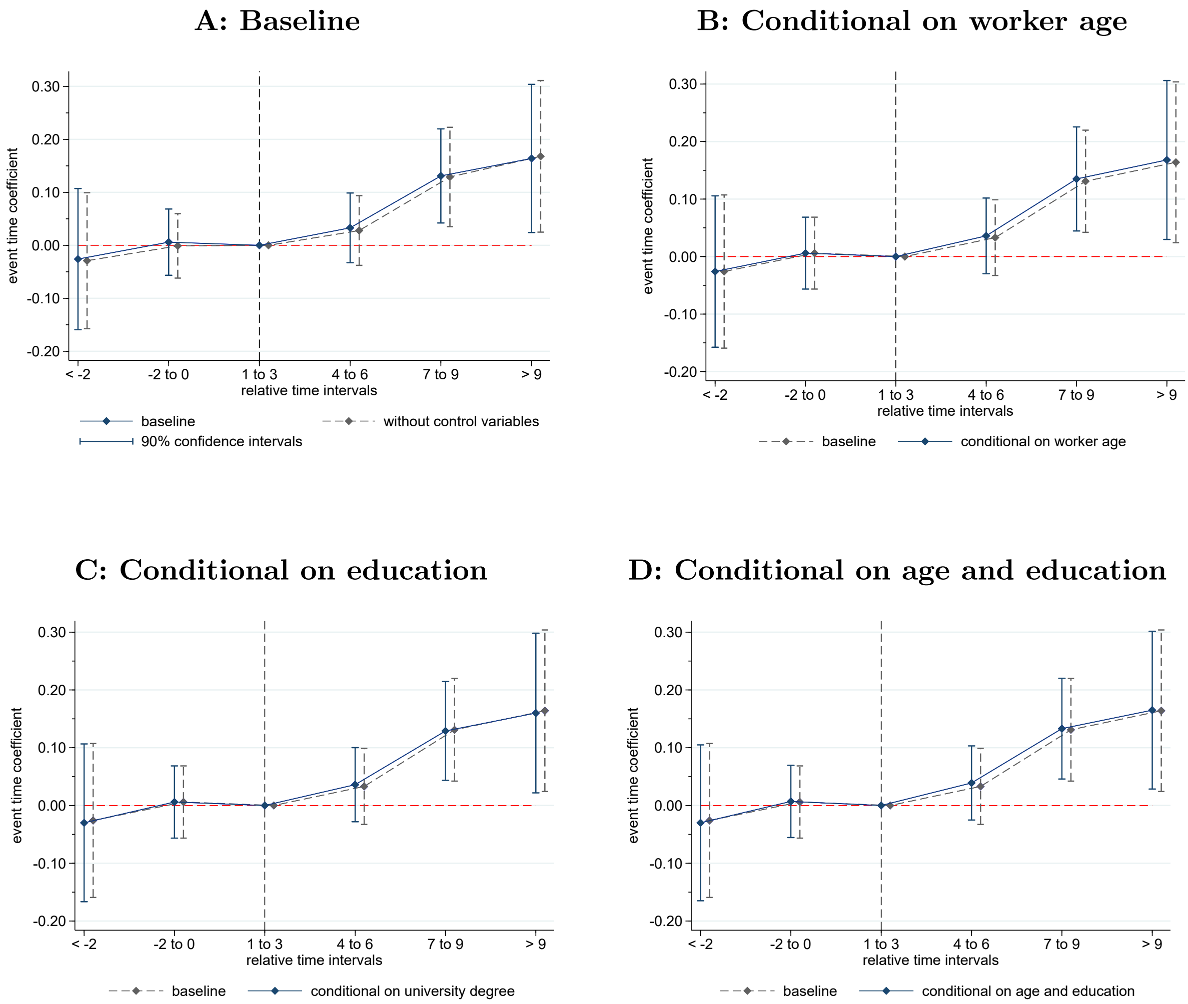
Table 5: Profits, OLS regressions

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

Notes: LIAB cross-sectional model, 1998–2016, West Germany. 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 person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. Reported numbers are coefficients from an OLS with standard errors clustered at the plant level in parentheses. ***/**/* denotes statistical significance at the 1%/5%/10% level. Further covariates included in all specifications are 7 plant size dummies, 15 federal state dummies, 37 two-digit sector dummies and 18 time dummies.

Figures

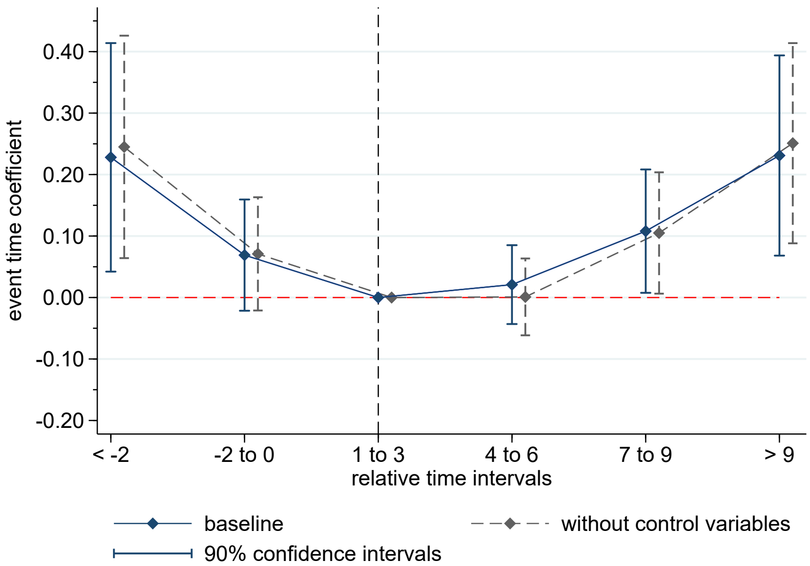
Figure 1: Worker Quality, Event Study



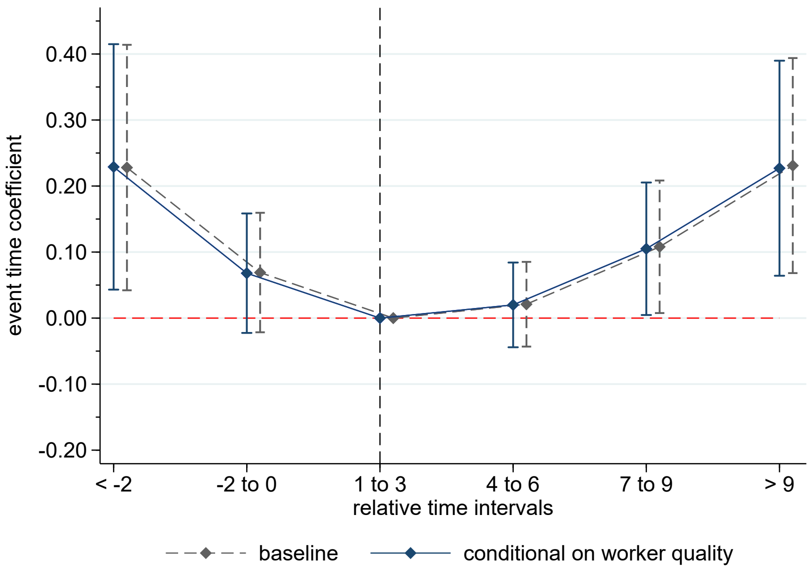
Notes. LIAB cross-sectional model, 1998–2016, West Germany, 141,098 plant-year-cohort observations. Works council introductions between 1998 and 2016. This figure shows the mean outcome of worker quality relative to the introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The event (year = 1 to 3) 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 education. The 90% confidence intervals are shown using standard errors clustered at the plant level.

Figure 2: Labor Productivity, Event Study

A: Baseline



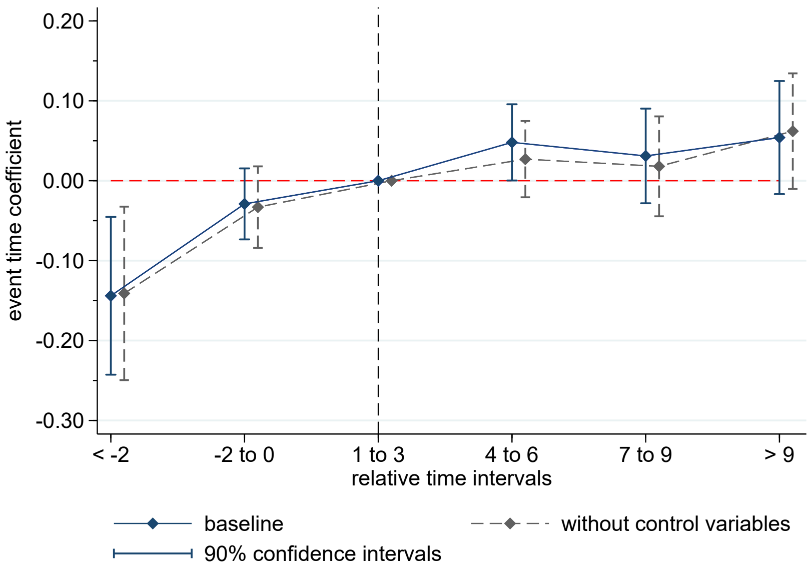
B: Conditional on worker quality



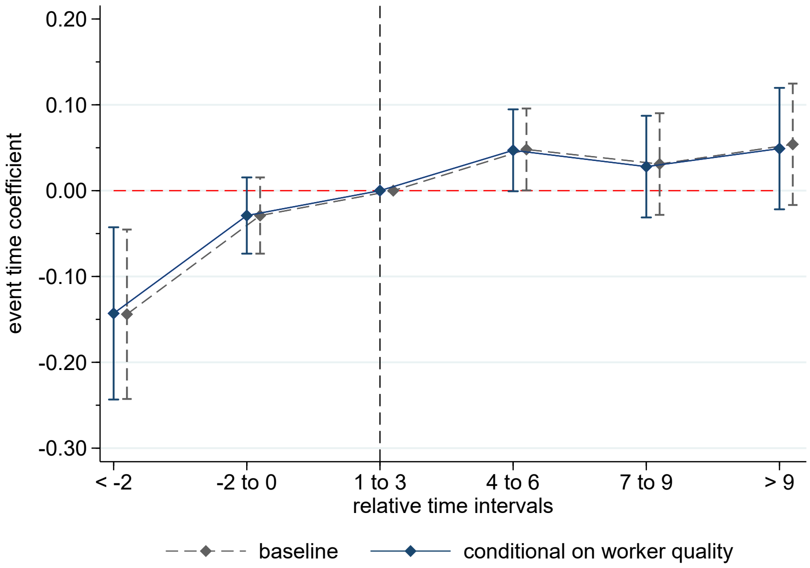
Notes. LIAB cross-sectional model, 1998–2016, West Germany, 141,098 plant-year-cohort observations. Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of value added divided by the number of employees relative to the introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The event (year = 1 to 3) 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.

Figure 3: Wages, Event Study

A: Baseline



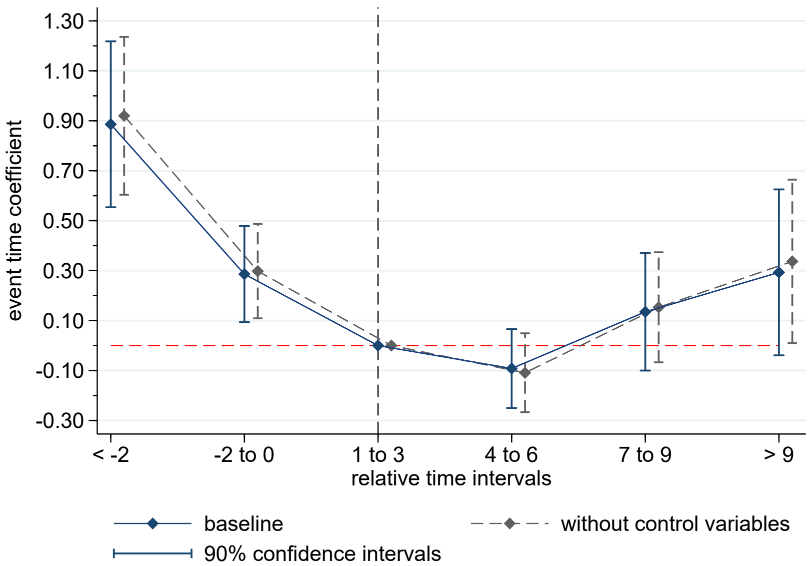
B: Conditional on worker quality



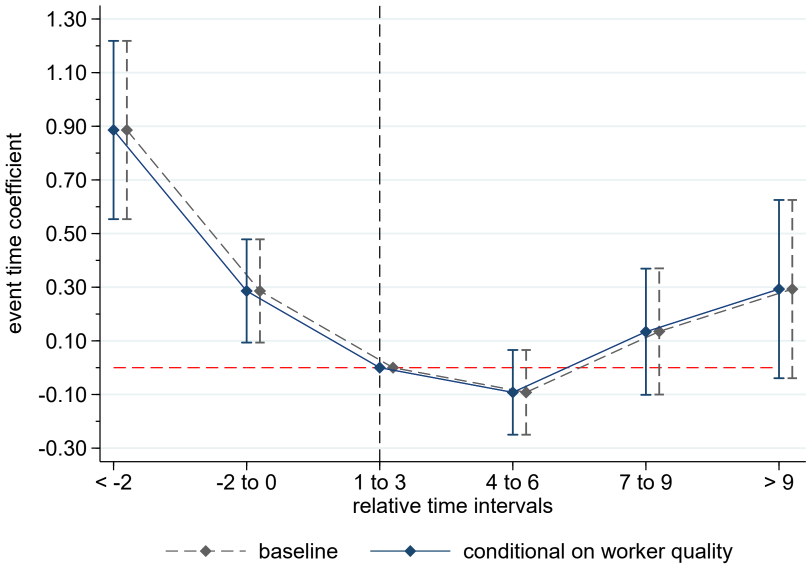
Notes. LIAB cross-sectional model, 1998–2016, West Germany, 141,098 plant-year-cohort observations. Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of the wage bill divided by the number of employees relative to the introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The event (year = 1 to 3) 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.

Figure 4: Profits, Event Study

A: Baseline



B: Conditional on worker quality



Notes. LIAB cross-sectional model, 1998–2016, West Germany, 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 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 person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The event (year = 1 to 3) 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.

Appendix A Definitions of Variables

Table A.1: Definitions of Variables

Variable	**Definition**
Log(Labor Productivity)	Logarithm of the value added per worker
Log(Wage bill per worker)	Logarithm of the wage bill per worker
Log(Profit per worker)	Logarithm of the profit 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 person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one
Skilled employees as share of all employees	Share of workers who have a vocational qualification, relevant professional experience or an university degree
Part-time employees as share of all employees	Share of part-time workers
Apprentices as share of all employees	Share of workers who are doing their vocational training under the vocational training law or the Handicrafts Regulation Act and other training stipulations of all workers
Female employees share of all employees	Share of women
Churning-rate	Measure of employment stability. Worker flow rate minus the absolute value of the net rate of employment change.
Exporter	= 1 if plant makes revenue abroad, = 0 if plant does not make revenue abroad
Single Plant	= 1 if the plant is an independent company or an independent organization without any other places of business, = 0 if plant does have other/belongs to other branches
Technical state of machinery	Assessment of the overall state of the technical state of the plant and machinery compared to other plants in the same industry. Scale from 1 to 5.
excellent	= 1
good	= 2
fair	= 3
poor	= 4 and 5
Average worker age	Average age of all employees
University graduates as share of all employees	Share of university graduates

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

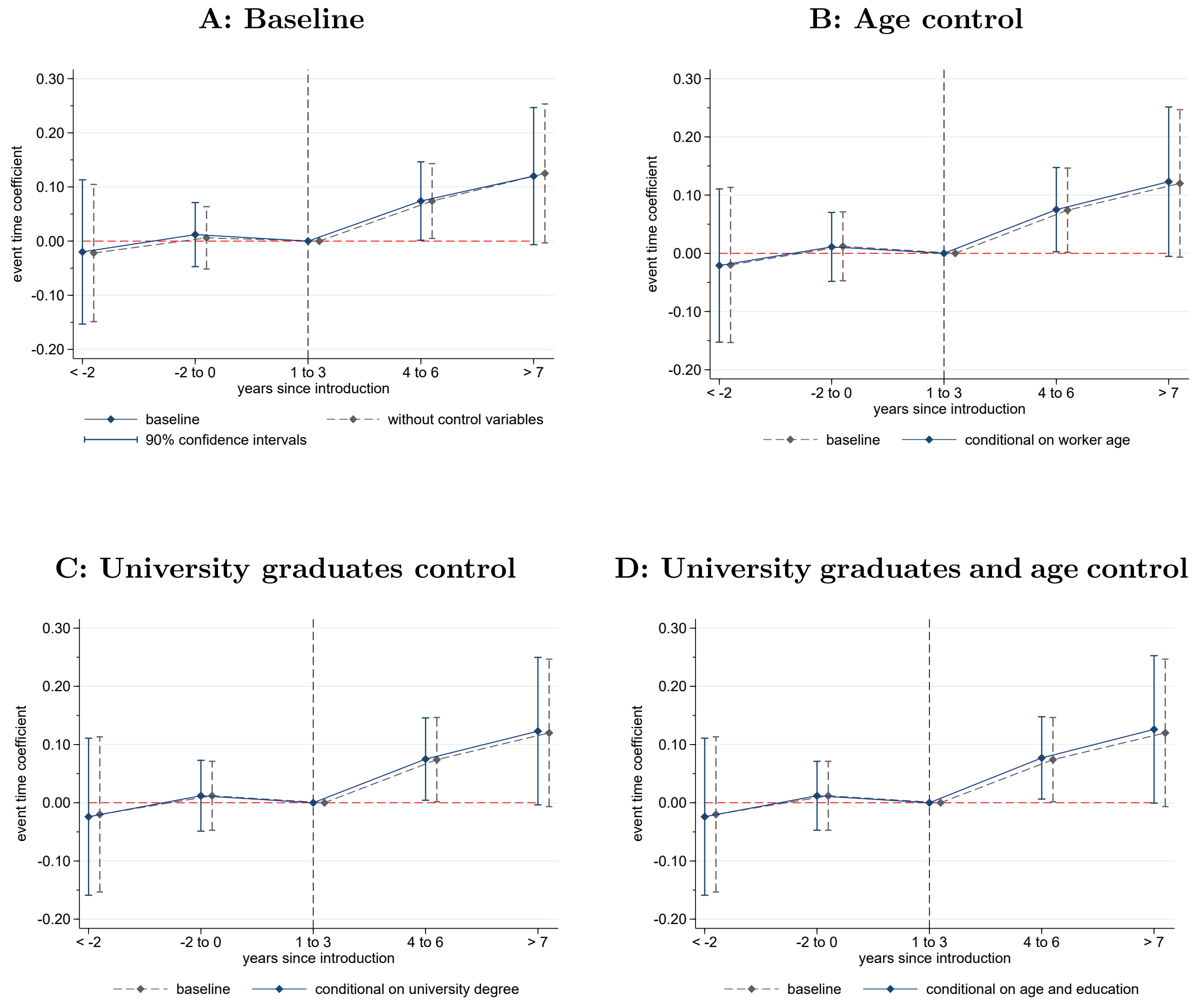
Appendix B Robustness Checks

B. 1 Selective Panel attrition

In this section we present robustness checks of the results in section (3.2) 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 firm or the council itself and regardless whether the firm survives until the years where council age is surveyed (i.e. 2012, 2014, 2016). The results are depicted in Figure B.1, B.2, B.3 and B.4 and show for each outcome the same patters as in our baseline results presented in section 3.2 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.

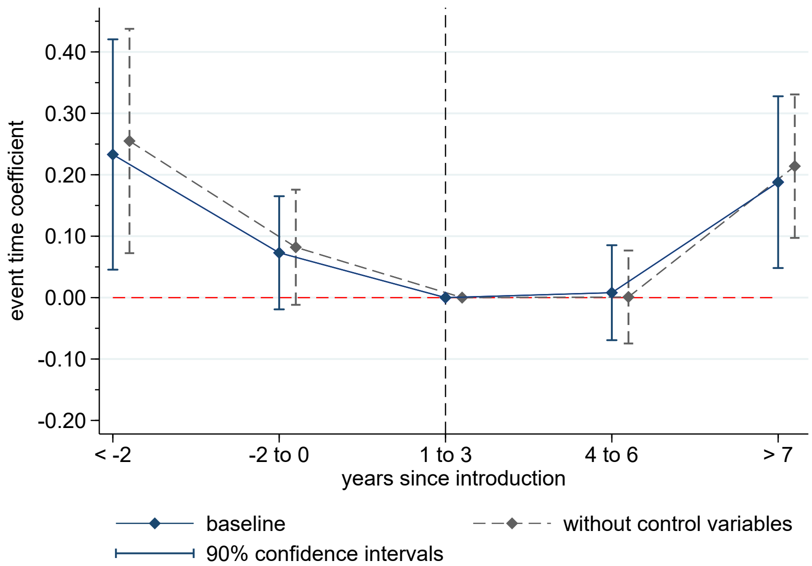
Figure B.1: Worker quality, Event Study (young works councils)



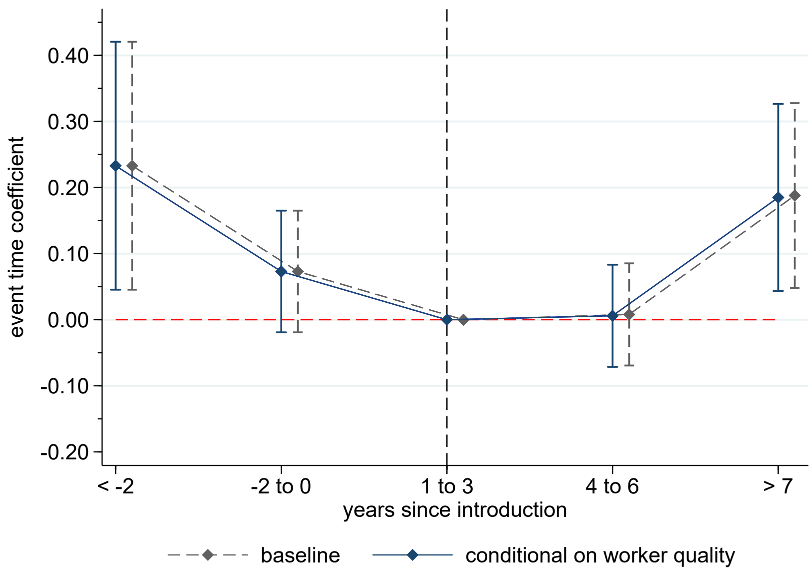
Notes. LIAB cross-sectional model, 1998–2016, West Germany, 140,782 plant-year-cohort observations. Works council introductions between 1998 and 2016. This figure shows the mean outcome of worker quality relative to the introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 1 to 3) 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 education. The 90% confidence intervals are shown using standard errors clustered at the plant level.

Figure B.2: Labor Productivity, Event Study (young works councils)

A: Baseline



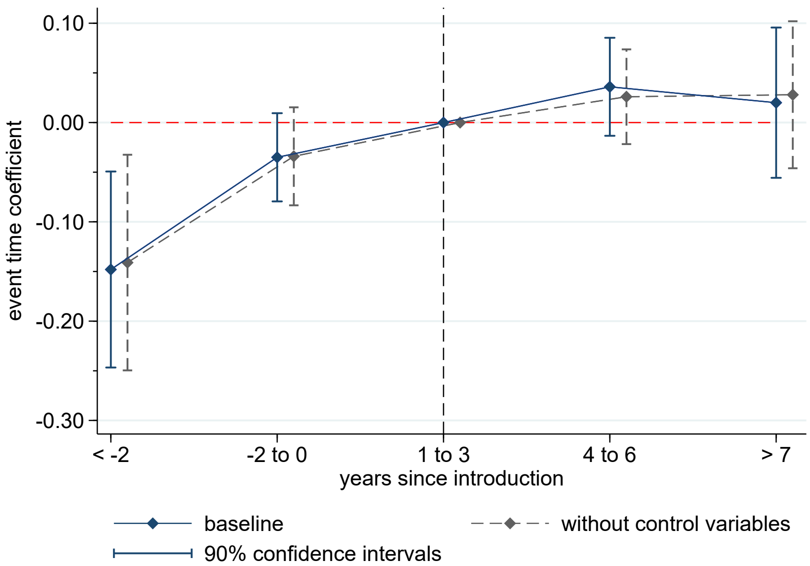
B: Control for worker quality



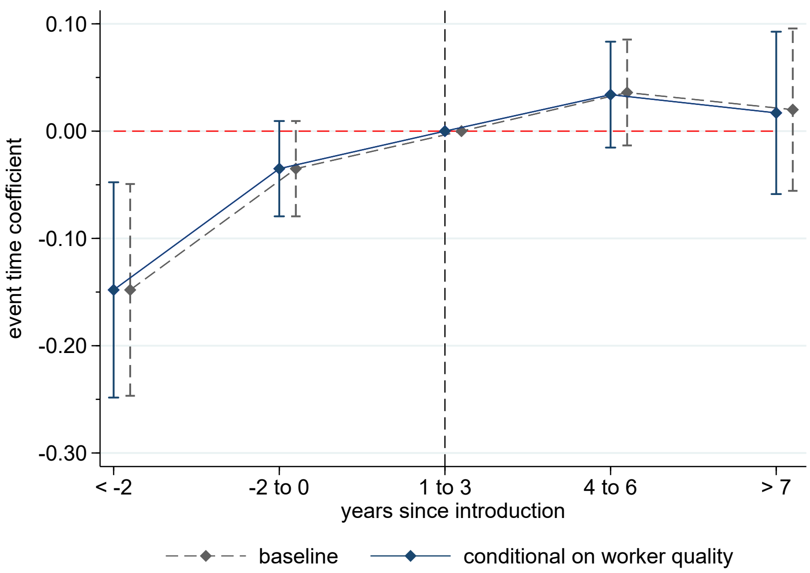
Notes. LIAB cross-sectional model, 1998–2016, West Germany, 140,782 plant-year-cohort observations. Works council introductions between 1998 and 2016. This figure shows the mean outcome of value added divided by the number of employees relative to the introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 1 to 3) 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.

Figure B.3: Wages, Event Study (young works councils)

A: Baseline



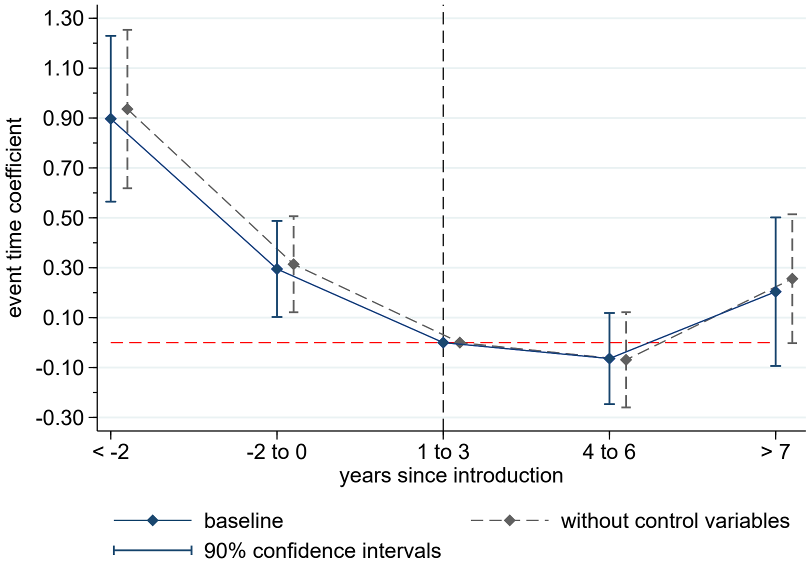
B: Control for worker quality



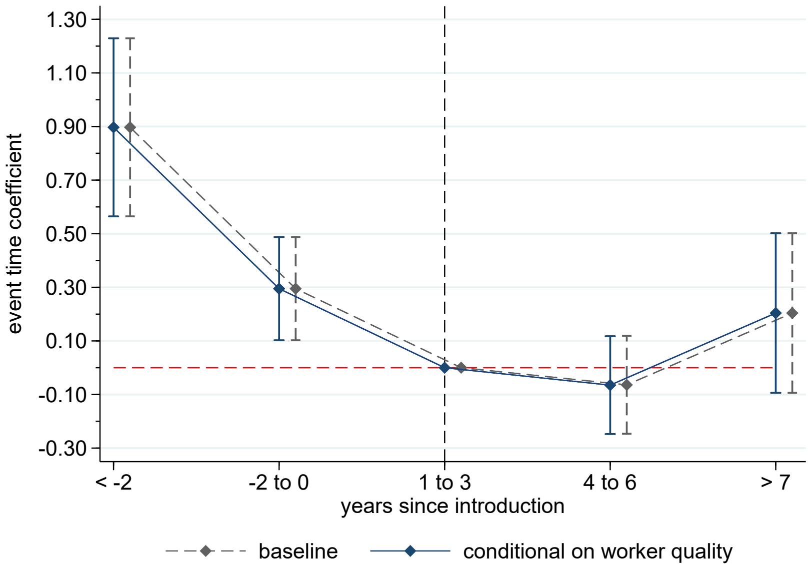
Notes. LIAB cross-sectional model, 1998–2016, West Germany, 140,782 plant-year-cohort observations. Works council introductions between 1998 and 2016. This figure shows the mean outcome of logarithm of the wage bill divided by the number of employees relative to the introduction period of the works council and net of the evolution in the control group. Worker quality is measured as the mean of the AKM person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 1 to 3) 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.

Figure B.4: Profits, Event Study (young works councils)

A: Baseline



B: Control for worker quality



Notes. LIAB cross-sectional model, 1998–2016, West Germany, 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 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 person effects (α_i) at the plant level (as described in section 3) standardized with a mean of zero and a standard deviation of one. The treatment group of this sample includes only works council introductions, that are determined using the panel structure of the data only. The event (year = 1 to 3) 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.

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