
# The Dynamic Effects of Works Councils on Labor Productivity: First Evidence from Panel Data

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**Abstract**

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# The Dynamic Effects of Works Councils on Labor Productivity:

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Keywords: non-union worker representation, works council, labor productivity, dynamic effects

JEL Classification: D24; J53

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## 1. Introduction

How to improve competitiveness is one of the core questions for individual managers and firm owners but also for entire societies. Given the high labor productivity of the German economy compared to other developed countries, it is natural to ask for the reasons for the German success and the role of industrial relations and labor market institutions. Germanys' powerful works councils have been subject of many empirical studies examining plant productivity but unambiguous evidence on the *causal* effect of councils is hard to pin down given possibly endogenous council introductions and the persistence of this institution within plants over time.

This paper provides a methodological framework for the analysis of dynamic effects of worker representation. We employ this framework to estimate dynamic effects of German works councils on labor productivity using panel data and newly available information on council age. The idea is that the evolution of council effects within plants over time can provide strong evidence against or in favor of positive productivity effects of councils. Intuitively, it is hard to argue that productivity improvements related to works council age have nothing to do with what the council is doing.

Works councils are an integral part of the industrial and labor relations system in a number of industrialized countries, including Canada, Germany, and Korea. In Germany, they provide a highly developed mechanism for employer-employee communication and negotiation at the plant level. The potential to improve worker representation and to increase plant productivity has directed considerable attention to German-style works councils in countries where union density has declined and international competition has increased. For instance, in the 1990ies the United States experienced a substantial political and scientific debate about whether German-style works councils could be a means to improve industrial relations and productivity in the US.[1] The earlier US debate faded without legislative changes, which might partly be due to the uncertainty about

[1] See, e.g., Rogers and Streeck (1995) or the final report of the Commission on the Future of Worker-Management Relations ("Dunlop Commission") initiated by the U.S. Department of Labor (1994).

the economic consequences of formalized participation. However, also without being at the top of the political agenda the debate is still active. In February 2014, workers at Volkswagen in Chattanooga (Tennessee) voted against the adoption of German-style works councils. One reason for this result might be the opposition of Republican politicians and conservative lobby groups who organized a high-profile public campaign against worker representation at Volkswagen.

A considerable number of econometric studies have analyzed the economic consequences of German works councils. One part of this literature estimates effects on plant productivity (Addison et al. 2001, Mueller 2012), profitability (Addison and Wagner 1997, Mueller 2011, Beckmann and Kraekel 2012), or wages (Addison et al. 2010). These studies typically find higher productivity and higher wages as a result of works councils but are split as to their effect on profits. Although it is known that employee participation may have dynamic effects on firm performance (Kato 2006), it has long been neglected that dynamics in the council effect can be a valuable source of variation to understand whether the positive association with plant performance is spurious or whether councils really improve productivity. Conditioning on plant fixed effects and a variety of control variables, we find negative productivity effects during the first five years after council introduction but a steady increase in the effect thereafter. We do not find positive pre-treatment trends in productivity and conclude that the long-run within-plant increase in productivity can be attributed to enduring works council existence. Our results therefore suggest that, in the long run, the competitiveness of the German economy is strengthened by the presence of formalized non-union worker representation.

## 2. Institutional and Theoretical Background

The Works Constitution Act (WCA, *Betriebsverfassungsgesetz*) of 1952, to which important amendments were made in 1972 and 2001, constitutes the legal basis for German works councils. The law entitles workers to initiate a works council election if an establishment has at least five

permanent employees. However, the law imposes no automatism to adopt a works council and, as a consequence, there are eligible establishments with and without works councils and works council incidence rises with plant size.[2]

The law affords works councils a number of legal rights, including rights on information, consultation, and codetermination, but the number of rights increases with plant size. Employers and works councils meet frequently in order to discuss a broad range of topics, including working conditions. The Works Constitution Act (WCA §2) explicitly states that works councils should act for the benefit of both the workers and the firm in "a spirit of mutual trust" and the law further clarifies the relationship between works councils and unions, the latter being typically organized at a sectoral level in Germany. It prescribes that negotiating over wages and calling strikes remain exclusive union rights. In practice, unions and works councils cooperate closely (Behrens 2009). Taken together, the Works Constitution Act protects *and* restrains works councils in Germany at the same time. Thus, German works councils come close to Freeman and Lazear's (1995) notion of a beneficial voice institution. The productivity-enhancing capacity of works councils is typically explained by their potential to mitigate information asymmetries and to promote trust and long-term relations between management and employees (Rogers and Streeck 1995). We will first summarize the static arguments regarding positive productivity effects and will then discuss its dynamic aspects.

**Static Perspectives**

First, works councils serve as an economic and protective information channel for bottom-up communication. Workers may withhold their knowledge about productivity-enhancing practices if they cannot be sure that their employer is not using this knowledge to their disadvantage. Works councils can reduce such information asymmetries by using their codetermination and consultation

[2] Addison (2009) provides a good overview of the history and the functioning of German works councils.

rights to assure that worker-provided knowledge is exploited in a way that is in the interest of both the firm and the workers. For instance, sections 90 and 91 of the WCA require that the council must be informed before changes in workplace organization or the production process take place and that the employer must discuss the council's suggestions and concerns with respect to these issues. Moreover, works councils provide efficient grievance procedures for dissatisfied workers without having to reveal their identities to the employer. These mechanisms can be derived from the classical voice-argument by Freeman and Medoff (1980, 1984). In both cases, the employer receives highly valuable information on the workforce or the production process.

Second, works councils can assist management with top-down communication. Freeman and Lazear (1995) argue that councils can use their information rights to verify management-provided information. This reduces incentives for opportunistic employer behavior and fosters employees' trust in such information. Councils can therefore help to generate workforce support for changes in production processes that entail additional burdens for the workers but are necessary to maintain competitiveness.[3]

Of course, communication between employers and employees is not limited to simply providing each other with pieces of information. Councils need to be a credible agent for both employees and management. Thus, building trust and the ability to serve as a mediator between capital and labor becomes a crucial factor for the productivity-enhancing capacity of works councils. This ability is at least partially inherent in works councils because, as mentioned above, the WCA is not only the source of a works council's power but also limits its strength and obliges the council to care not only about the good of the employees but about the benefit of the firm. Altogether, in the static

[3] This argument gains even more relevance in the light of recent decentralization tendencies. Opening clauses were first introduced to provide companies with the option to fall below collectively agreed standards if they ran into economic trouble. But today even establishments that are economically viable can apply opening clauses as the enhancement of the establishment's competitiveness is reason enough to undercut standards (Ellguth et al. 2014).

view, works councils promote intra-firm communication and cooperation not only by transmitting information but also by maintaining trustful employer-employee relations.

**Dynamic Perspectives**

As noted earlier (e.g. Kato 2006), the productivity effects of participatory employment practices may take time. In the case of newly founded works councils, it seems plausible that the full potential to boost a firms' productivity is not an instantaneous quality that is simply there. Therefore, the productivity effects of works councils might not be static over the lifetime of works councils but might also have dynamic aspects: i.e., works councils need to learn how they can influence the production process and management has to learn how to absorb information and how to respond. With regard to the productivity channels discussed above, the learning process comprises learning in the sense that works councils must gather knowledge and expertise on the production process. A newly established works council, arguably, also has less knowledge about the economic situation of the plant and the pressure exerted by competitors. Jirjahn et al. (2011) conclude that learning processes on the side of the works council may lead to diminishing information asymmetries over time. They also stress management attitudes towards an uninformed council, where employers may still have an opening for opportunistic behavior or may be inclined to ignore or deceive the council. As a result, the council may not be able to play its intended role until it has enough knowhow to be recognized as a serious partner. Jirjahn et al. (2011:431) assume that, in such a situation, the council may use its substantial codetermination and consultation rights to fight for the relevant information. Case study evidence by Kotthoff (1994) also suggests that, over the years, an initially ideological confrontation between managers and works councilors turns into more businesslike and professional attitudes on both sides. Hence, besides a pure learning mechanism, positive long-run effects of councils may also originate from improvements in communication and firm culture.

## 3. Identification of Age Effects

We outline in this section what exactly a works council age effect is and how the effect can be identified. The proposed framework requires several cross sections but not necessarily panel data.[4] A basic model to estimate the effect of works council age on plant productivity is

$$\log\left(\frac{VAD}{N}\right)_{it} = \alpha_1 woco_{it} + \alpha_2 w\_age_{it} + \sum_{t=1}^{T} m_{wt} year_{it} + x_{it}\gamma + \varepsilon_{it} \qquad (1),$$

with gross value added per worker of plant *i* regressed on a works council dummy, works council age, a vector of covariates $x_{it}$ including a constant term, and year effects. In equation (1), *t* indicates cross section *t* (*t* = 1, …, T) drawn in calendar year $C_t$, and $year_{it}$ is a period dummy indicating that the observation is drawn from cross section *t*. In this setting, $\alpha_1$ measures the effect of a newly founded works council on labor productivity while $\alpha_2$ measures the difference between newly founded and older councils, i.e., the works council age effect. Consequently, the regressors $woco_{it}$ and $w\_age_{it}$ are zero for plants without a council. As $\alpha_2$ describes the effect of aging it may also pick up plant age effects. This is particularly relevant because plants do not frequently change their works council status. Hence, an augmented model should include plant age or, equivalently, plant's year of birth $p\_birth_i$ as a regressor:

$$\log\left(\frac{VAD}{N}\right)_{it} = \delta p\_birth_i + \alpha_1 woco_{it} + \alpha_2 w\_age_{it} + \sum_{t=1}^{T} m_{wt} year_{it} + x_{it}\gamma + \varepsilon_{it} \qquad (2).$$

It is tempting to interpret a positive estimate for $\alpha_2$ as evidence for positive works council age effects. There is, however, another interpretation of this cross sectional result: a positive $\alpha_2$ is also in line with a general decline in the productivity effect of subsequent cohorts of works councils over time. It is important to recognize that $\alpha_2$ mixes age and cohort effects and that a regression

[4] The following argumentation is very similar to the standard approach for the identification of wage assimilation in the immigration literature outlined in Borjas (1999).

based on equation (2) can therefore artificially produce *every* statistical relationship between age and productivity.

Cohort effects might, for instance, be induced by major changes in the legal framework that initiate the foundation of a new generation of works councils. If, after the trigger event, works councils are first introduced in plants where council effects are highest, these works council cohorts are more productive independently of age. The literature has generated evidence for larger productivity effects in larger plants (Addison et al. 2001, Mueller 2012) and in plants with inferior management practices (FitzRoy and Kraft 1985, Mueller 2013). If workers take into account the economic conditions of the plant and/or if managers support the introduction of employee representation as they expect beneficial effects on plant performance, it is plausible to assume that plants with larger expected works council effects are among the first to adopt a council.

Because of their linear dependencies, the joint identification of period, age, and cohort effects is impossible without additional assumptions. To illustrate this point in the context of works councils, consider the following regression model:

Equation for works council plants:

$$\log\left(\frac{VAD}{N}\right)_{it} = \delta_w p\_birth_i + \alpha_2 w\_age_{it} + \beta coho_i + \sum_{t=1}^{T} m_{wt} year_{it} + x_{it}\gamma + \varepsilon_{it} \qquad (3),$$

Equation for plants without works councils:

$$\log\left(\frac{VAD}{N}\right)_{it} = \delta_n p\_birth_i + \sum_{t=1}^{T} m_{nt} year_{it} + x_{it}\gamma + \varepsilon_{it} \qquad (4),$$

where $coho_i$ is the year of works council foundation, and the subscripts *w* and *n* indicate works council existence or non-existence, respectively. In this model, $\beta$ measures how the productivity effect of works councils differs across cohorts, $m_{wt}$ and $m_{nt}$ capture the impact of the aggregate economic conditions in period *t*, and $\alpha_2$ estimates the true age effect for works councils. The

parameters $\alpha_2$, $\beta$, and $m$ are not identified because there is perfect multicollinearity of the form $coho_i = year_t - w\_age_{it}$.

To identify age and cohort effects, we will follow two different strategies. First, we impose the restriction $m_{wt} = m_{nt}$. This means that we assume the same percentage impact of aggregate conditions on labor productivity for plants with and without works councils. Although this may seem very restrictive at first, note that it is sufficient that $m_{wt}$ equals $m_{nt}$ conditional on covariates. Hence, conditioning on a rich set of covariates mitigates concerns with respect to this assumption. The model to be estimated is then

$$\log\left(\frac{VAD}{N}\right)_{it} = woco_{it}\,[\alpha_1 + \alpha_2 w\_age_{it} + \beta coho_i] + \sum_{t=1}^{T} m_t year_{it} + x_{it}\gamma + \varepsilon_{it} \qquad (5)$$

where matrix $x_{it}$ contains all control variables including the year of plant foundation and we are in particular interested in the coefficients $\alpha_1, \alpha_2$, and $\beta$.

The restriction $m_{wt} = m_{nt}$ is not testable, and we therefore check its credibility with a second approach. Several researchers have proposed to identify the underlying economic factors for which period dummies are only indirect proxies and to use variables measuring these factors instead of using period dummies (see, e.g., Heckman and Robb 1985 or Rodgers 1982). This solves the issue of perfect multicollinearity and allows the identification of age and cohort effects. Period effects are typically included in productivity regressions in order to net out aggregate economic conditions influencing productivity. We use yearly information on real value added by sector provided by the German Federal Statistical Office (Statistisches Bundesamt 2013). Specifically, we will use a fixed-base index of value added by sector (where the year 2005 serves as the base period) instead of year dummies and contrast the estimates for council age with the estimates derived from equation (5).

## 4. Data

The data set used is the IAB Establishment Panel of the Institute for Employment Research (*Institut für Arbeitsmarkt- und Berufsforschung*, IAB) of the German Federal Employment Agency (*Bundesagentur für Arbeit*).[5] It is based on a stratified random sample of plants that employ at least one worker covered by social security on June 30th of a given year. The IAB Establishment Panel has surveyed the same plants from all industries in western (eastern) Germany since 1993 (1996). The data are collected in personal interviews with owners or managers. To correct for panel mortality and to reflect the foundation of new establishments, the sample is updated every year. Response rates of units that have been interviewed repeatedly exceed 80 percent. The IAB Establishment Panel has a focus on employment-related topics but also includes value added and many other variables important for productivity regressions.

Deriving information on works council age from the panel structure of the data is generally difficult because works councils status is very persistent within plants. The panel structure of the data can be used to compute council age for plants that are actually observed to establish a new council. Due to the persistence, however, few council foundations can be observed directly and most plants having a council now already had the council when entering the panel. Importantly, the 2012 questionnaire includes a question about the age of the works council. Respondents were asked for the year in which their works council has been established. This information can be used to identify council age even if council foundation is not observed in the data. In order to reduce the impact of recall bias, respondents also had the opportunity to answer that the council is "very old" instead of guessing the exact year. We treat the latter information as missing in order to keep only accurate information regarding works council age. We drop works council plants for which council age is unknown. By combining the information which is directly asked in the questionnaire with information gathered from the panel structure of the IAB Establishment Panel,

[5] Details about the IAB Establishment Panel are given by Fischer et al. (2009).

we are able to crosscheck and to enhance the quality of the data at hand. The data cleaning procedures necessary to arrive at proper age information are described in more detail in the appendix.

In the following, we use information from the IAB Establishment Panel encompassing the years 1998–2013 in order to investigate the effect of works council age on the plant's gross value added per worker. Because the questions on plant sales and intermediate inputs refer to the previous year, our productivity regressions will not cover the year 2013. The vector of control variables $x_{it}$ includes establishment size measured as the log of the number of employees, the percentages of apprentices; skilled, part-time, and female workers in the plant's labor force; coverage by collective wage agreements; a dummy on whether the plant is a single plant or part of a multi-branch firm; and dummies for the plant's legal form, plant cohort, the technical state of equipment, and exporter status.[6,7]

Note that we have few observations on works council age for earlier years because panel attrition and the subsequent sampling of new plants reduce the overlap between the plants answering the 2012 questionnaire and the plants asked in earlier years.[8] What is more, information on collective bargaining agreements, plant ownership, and other control variables are fragmentary in the years before 1998 and we therefore use the years 1993-1997 for the computation of the age variables but not in the regression analysis.

[6] See Table 1 for variable definitions and descriptive statistics. The choice of control variables is guided by theoretical considerations but also dictated by data availability. Some potentially interesting controls are not asked or are asked only in some years and have to be dropped (e.g. foreign ownership). As we will generally condition on plant fixed effects in our regressions, we hope to at least partly control for the influence of many left out variables. Note also that some of the controls we use might themselves be influenced by council existence. In section 6, we will therefore check our results by conditioning on plant fixed effects and year dummies, only.

[7] Capital stock per worker can be approximated using the modified perpetual inventory approach proposed by Mueller (2008). However, plants with missing information on investments will be lost. In order to not lose further observations carrying valuable information on council age (in particular for young plants with instable investment history), we decided not to control for capital intensity in the regressions presented in this paper but always checked the robustness of results with respect to the inclusion of capital intensity. Results are quantitatively very similar and conclusions are qualitatively unchanged. Results are available upon request from the authors.

[8] We discuss potential survivorship bias in section 5.

The sectors included in our sample are manufacturing, trade and repair, transport and communication, and industrial services (excluding real estate activities). We restrict our sample to privately owned West German plants with at least 5 employees. East German plants are excluded as works councils and their learning might be very different in a transition economy because of different traditions of conflict resolution and employee participation (see, e.g., Behrens 2009:278 or Mueller-Jentsch 1995:75).

## 5. Econometric Approach

To investigate the effect of council age on plant's labor productivity, we estimate the following model:

$$\log\left(\frac{VAD}{N}\right)_{it} = woco\,[\alpha_1 + \alpha_2 w\_age_{it} + \beta coho_i] + \sum_{t=1}^{T} m_t year_{it} + x_{it}\gamma + \mu_i + \varepsilon_{it} \quad (6).$$

Compared to model (5), model (6) includes a plant fixed effect $\mu_i$ and therefore highlights the problem of time-invariant unobserved heterogeneity between plants (such as differences in management ability) that may yield spurious relationships between labor productivity and the regressors.

OLS estimations may be biased due to unobserved plant characteristics correlated with the regressors. The classical method to deal with this kind of endogeneity problem in panel data is to include plant fixed effects. The major advantage of the within estimator, which uses deviations from within-plant averages for identification, over OLS is that potential correlations between any right-hand-side variable and the fixed plant effect $\mu_i$ do not bias coefficient estimates. A consequence of the within transformation is a loss in precision because differences between plants are not used for estimation. Time-invariant regressors like plant cohort and works council cohort

will be picked up by the plant fixed effect so that fixed-effect estimation implicitly controls for cohort effects.[9]

**Threats to Identification**

We see three threats to identification in the fixed effects estimation: different counterfactual trends for control and treatment group, regression to the mean, and survivorship bias. We begin with the first threat. If heterogeneous long-run productivity trends exist at the plant level and if councils are more likely to be introduced in plants with above average trends, fixed effects estimates will be biased upwards. We therefore test whether pre-treatment trends in labor productivity exist. Regression to the mean is implied by studies showing that councils are more likely to be introduced when plants face economic problems (Jirjahn 2009, Mohrenweiser et al. 2012). If the plant recovers from the crisis, one can't be sure that the council was causing recovery. If it did not, we have regression to the mean: plants are just returning to their long-run productivity level. Again, we check this by looking at pre-treatment trends.

Survivorship bias means the higher survival chance of successful works council plants compared to unsuccessful council plants (selective panel attrition). While survivorship bias is a potential issue in most panel studies, it may be a particularly severe issue in our study as the propensity of having been observed in the year the works council age question was asked (year 2012) may be systematically related to the performance of works council plants. If council plants close before that year, we have no information on council age for most of these plants and have to exclude them from the entire analysis. Including plant fixed effects may help to overcome survivorship biases potentially arising from selective panel attrition. The fixed effects estimator allows attrition to be correlated with $\mu_i$, which happens if, in the initially random sample, some council plants are more

[9] Power (1998) faced a very similar setting when using fixed effects techniques to assess the effects of plant age and investment vintages on plant productivity.

likely to drop out early than others. Attrition of non-council plants is, of course, also no problem if it is due to any correlation with the fixed effect.

Survivorship bias in our fixed effects estimation may arise if council foundation causally *changes* survival prospects.[10] In this case, council existence and age can be correlated with the idiosyncratic error term causing biased fixed effects estimates. For instance, councils may have negative effects for some plants, i.e. they reduce their productivity. Plants with initially low levels of productivity may not survive this. Then, plants where councils did work or plants where councils did not work but where pre-treatment productivity was such high that the plant nevertheless survived, would be more likely to be observed in our data.[11] However, Mueller (2013) estimates that councils have the most beneficial effects on labor productivity if labor productivity is low, which indicates that this scenario is not very likely. What is more, negative effects of councils for some plants would only result in a spurious levels difference between the productivity of council plants and non-council plants. It wouldn't explain positive council *age* coefficient. For this to be driven by survivorship bias, we would have to assume that council *age* effects are negative for a sizeable fraction of plants so that the final sample becomes more and more selective with council age. From a non-technical point of view, this would mean that workers knowingly and over a long time span deteriorate plant performance and ultimately force plant closure. We see no way how to test for dynamic sample selection with our data but don't consider dynamic survivorship bias as a

[10] We are not aware of studies claiming to estimate the causal effect of councils on exit probability and whether this is related to labor productivity. Using cross sectional variation, Addison et al. (2004) show that council existence is associated with a higher exit propensity. While Jirjahn (2012) confirms this for single-establishment plants without collective wage agreements, he documents that multi-plant establishments and plants facing adverse economic conditions actually face a lower closure risk when having a works council. Neither Addison et al. (2004) nor Jirjahn (2012) provide any direct evidence of whether estimates vary with plant productivity and no study addresses unobserved heterogeneity or is able to fully account for endogeneity issues. Note that there is no bias in our study if any positive association between council existence and exit probability is due to an increased likelihood of council introduction in plants with higher exit probability, the latter being implied by the results of Jirjahn (2009).

[11] We are fully aware of the fact that survival will rather be determined by profits and not by productivity. Looking at profits, however, will not necessarily increase the magnitude of survivorship bias as also plants with positive productivity effects of councils can be pushed out of the market as long as profits decrease. It could even be the case that workers make concessions to save the plant (Freeman and Lazear 1995) so that negative productivity effects are not accompanied by negative profit effects.

plausible scenario from a theoretical perspective. We are therefore confident that our estimates of dynamic effects are not overestimated due to selective attrition.

## 6. Empirical Results

### 6.1 Full Sample

Coefficients estimated using the fixed effects within estimator are identified via within-plant variation over time, only.[12] When estimating equation (6) and conditioning on plant fixed effects, the influence of plant and works council cohort can't be quantified. Both are time-invariant plant characteristics and differences in these characteristics across plants will not affect the estimation of council age effects. As fixed effects estimation of equation (6) controls for cohort effects, the separation of council age effects from period effects proceeds along the same lines as described in section 3. As it turns out, council age coefficients are virtually the same when comparing between a model with period effects as opposed to a model where period effects are replaced by the index of aggregate sector level value added (see section 3). We report in Table 2 the coefficients from estimating equation (6) with year dummies.

We start with a simple linear-quadratic specification of works council age plus shift term for which results are presented in column (1) of Table 2. The shift term is statistically significant and implies a negative productivity effect of 8 percent for young councils. While the estimate for the quadratic term is small and insignificant, the linear term points at a steady yearly increase in productivity of about 1.6 percent.[13]

[12] We observe 91 plants introducing a council, 70 plants switching from young council to 5-9 year old council, 60 changing to 10-14 years, and 46 moving into the highest age category.

[13] We also performed OLS regressions where the introduction effect is large 0.213*** (0.068) and the coefficients for the linear-quadratic term are both insignificant (results available upon request from the authors). OLS results with introduction effect differ considerably from the results presented in Jirjahn et al. (2011), who reported an insignificant and negative council introduction effect on sales per worker and a highly significant inverted U-shape in council age. A number of differences between the two papers might explain the different results. While Jirjahn et al. (2011) were able to utilize some information regarding union influence on and workforce support for the council not contained in our data, we have a much larger sample and information on intermediate input and are therefore able to regress on

The dummy variable specification in column (2) also shows a negative introduction effect. The set of council age dummies measures the productivity difference between plants with a works council in the respective age category and plants with a newly founded council. Results show that the within-plant productivity improvements are strongest after a 5 year implementation period: plants having a 5-9 year old council have *ceteris paribus* 11 percent higher productivity compared to when the same plants newly introduced the council. Subsequent improvements are somewhat smaller but still substantial so that the overall age pattern suggests a positive causal long-run effect of works councils on labor productivity.[14] Our results are in line with Kato and Morishima (2002) who show for Japan positive long-run productivity effects of participatory employment practices and slightly negative introduction effects. Jones and Kato (1995) report a similar pattern for the effect of employee stock-ownership plans.

As a robustness check, we changed the number of years spanned by the age categories from 5 years to 3 years. We obtain a negative and statistical significant introduction effect (-5.4 percent) for councils being no more than 2 years old. The first positive coefficient shows up at an age of 6-8 years. Coefficients further increase monotonically with age and plants having councils being 15 years or older experience a 25 percent productivity increase compared to the introduction period. Hence, recoding of categories leaves our insights unchanged but uncovers that the negative introduction effect lasts up to 5 years.

value added. It is possible that the 222 works councils (over all age classes) in the data set used by Jirjahn et al. (2011) are too few to identify the introduction effect of works councils properly. If the introduction effect in Jirjahn et al. (2011) is underestimated, then their inverted U-shape might be artificial. What is more, Jirjahn et al. (2011) include East German plants despite the much shorter and very different history of codetermination in East Germany. In any case, our results demonstrate the importance of confronting results obtained with different data sets as proposed by Hamermesh (2007).

[14] About twenty percent of our observations belong to plants being part of a multi-plant firm and are, thus, either headquarters, local or specific administrative units, or branches. While the sample is too small to derive reasonable results for headquarters, local or specific administrative units, or branches, respectively, focusing only on single-plant firms yields even stronger long-run council effects.

**Examination of Possible Threats to Identification**

As discussed in more detail in section 5, there are situations where it could be argued that the observed positive dynamics in council productivity are spurious: first, positive long-run productivity trends in plants introducing a works council and no such trends (or weaker trends) in non-council plants and, second, regression to the mean. We start with the first potential issue. Counterfactual trends in labor productivity for council plants are unknown but any upward pre-treatment productivity trend in the treatment group would be alarming. We therefore add pre-treatment time dummies to equation (6) and report results in column (3) of Table 2. Both pre-treatment dummies can be interpreted relative to the introduction period.[15] The coefficients are both positive and thus provide no evidence for any upward pre-treatment productivity trend in the treatment group. If at all, treated plants experience a negative development prior to council introduction.

Whether the long-run increase in productivity shown in our study should be fully attributed to council actions or whether there is some role for regression to the mean can't be tested directly. In any case, the pre-treatment dip is much smaller than the post-treatment recovery and, thus, there is evidence for positive effects of councils even if part of the post-treatment increase should reflect a return to pre-treatment productivity levels and is not caused by the council. It is, of course, possible that other unobserved measures are systematically introduced parallel to council introduction. To explain the positive long-run trend, these measures would themselves have to have increasing long-run effects on productivity. Even then, however, one could be willing to attribute part of the effects associated with these unobserved changes to works councils if the changes are a consequence of council introduction (e.g. an employer reaction to council foundation).

[15] Adding pre-treatment dummies yields a type of fixed-effects regression models frequently applied in the job displacement literature (see e.g. Jacobsen et al. 1993).

One last issue is potential over-controlling in the sense that conditioning on time-variant control variables, which themselves might be influenced by council age, picks up effects that should be attributed to councils. Using exactly the same observations but conditioning only on year dummies yields council age patterns that are very similar to the pattern reported in column (2) of Table 2. The only sizeable difference shows up in the council introduction dummy, which becomes -0.078 (instead of -0.054). The difference to our main results is, however, hard to interpret as we don't know whether the parallel changes in covariates leading to different introduction effects are caused by the council or not. In our eyes, it is most important that the increasing long-run productivity profile is unaffected by dropping control variables.

### 6.2 Subgroup Analysis

Table 3 repeats the analysis but splits the sample by coverage by union-negotiated collective wage agreements and by plant size. Both characteristics have been found to be important moderating factors for the effectiveness of codetermination in previous studies. German industrial relations are characterized by a distinct dual system, with industry-level collective wage bargaining on the one hand and plant-level negotiations over working conditions often conducted by works councils on the other hand.[16] Theoretical arguments (Freeman and Lazear 1995) as well as empirical evidence (Huebler and Jirjahn 2003, Frick and Möller 2003, Mueller 2011, Wagner et al. 2006, Wagner 2008) indicate the importance of the interactions between works councils and collective bargaining. Freeman and Lazear (1995) argue that works councils are more likely to engage in rent-seeking activities and adversarial bargaining if wages are negotiated individually or at the plant level. If wages are negotiated at a higher level than the plant level, however, works councils can only increase workers' rents by increasing the overall pie, i.e., by increasing productivity. Behrens (2009:277) summarizes that collective bargaining by unions "… enabled works councils

[16] Works councils have no formal right to negotiate wages but they may use codetermination rights in other areas to enforce higher compensation (Addison et al. 2010).

to engage more or less peacefully in company-level negotiations with management about the remaining issues…". In line with this reasoning, Huebler and Jirjahn (2003) and Mueller (2011) show that beneficial works council effects are higher if the plant is covered by collective wage agreements.

Moreover works council effects are typically estimated to be less pronounced in smaller plants (e.g. Addison et al. 2001, Mueller 2012). Addison et al. (2001) argue that representational participation is of lesser importance in small plants as eliciting worker preferences is easier there. Another potential explanation for this finding is that, according to the law, the amount of works councils' rights is a positive function of firm size. For small plants, we find negative short-term effects and a noisy long-run pattern suggesting that council effects are generally non-positive within the group of small plants. Small plants having an old council do, however, at least not experience negative council effects. The council effect is much more beneficial in plants having at least 100 employees, i.e. there is no introduction effect and a rising productivity-age profile. All in all, our results confirm earlier findings regarding plant size differences.[17]

The results of previous studies on the interaction between bargaining coverage and council effects are confirmed only in the sense that dynamic council effects are more positive in plants covered by collective wage agreements.[18] Due to negative introduction effects in these plants, however, the overall effect of councils in codetermined plants never exceeds the effect in plants with individual bargaining. Both the lack of long-run advantages of covered plants and their apparent short-run disadvantage are rather surprising given the sound theoretical argumentation in Freeman and Lazear (1995). Note that our results are not necessarily at odds with previous empirical evidence as, to the best of our knowledge, there exists no study comparing the effects of works council

[17] This conclusion also holds if we use an employment threshold of 50 to distinguish smaller and larger plants.
[18] We do not distinguish between sectoral-level and firm-level collective wage agreements. Firm-level agreements are typically negotiated between management and a sector-level union. The key feature of collective wage agreements is that the council is less engaging in rent-seeking activities, which should be fulfilled when wages are negotiated with union participation.

existence by bargaining regime while controlling for plant fixed effects.[19] We nevertheless refrain from pushing results too far as the number of plants switching between council age categories is limited in the subgroup analysis.

## 7. Discussion

Germanys' powerful works councils have been subject of many empirical studies examining plant productivity but unambiguous evidence on the *causal* effect of councils is hard to pin down. This paper provides a methodological framework for the analysis of dynamic effects of worker representation. We use this framework to estimate dynamic effects of German works councils on labor productivity by employing a large panel data set and newly available information on council age. The idea is that the evolution of council effects within plants over time can provide strong evidence against or in favor of positive causal productivity effects.

Results show a productivity deterioration during the first five years after council introduction but a steady increase in productivity thereafter. As we show that there is no positive pre-treatment trend in productivity, our results suggest that the long-run within-plant increase in productivity after council foundation is caused by the council. The long-run pattern is very similar if we drop control variables likely to be influenced by the council. Council effects are not only statistically highly significant, they are also of an economically relevant magnitude. For instance, we estimate a 11 percent within-plant increase in labor productivity when comparing five to nine year old councils with up to four year old councils.

Negative short-term effects are either the consequence of difficulties and adaption problems during the period of council introduction or reflect a continuation of negative pre-treatment trends in productivity, the latter being previously suggested by Jirjahn (2009) and Mohrenweiser et al.

[19] Our conclusions remain qualitatively unchanged if we split the sample by initial bargaining status instead of current status. While the effects for initially covered plants are also quantitatively unaffected, the coefficients for the initially uncovered plants are somewhat more pronounced and marginally significant for the 5-9 and the 10-14 year categories.

(2012) and confirmed in our study. We conclude that councils are no policy measure to immediately boost productivity and think that our findings rather highlight the importance of long-term relations for productivity. Our results add to a recent strand of literature that, with a variety of different empirical approaches, more and more converges to the view that German works councils have a positive impact on productivity (Jirjahn 2009, Mohrenweiser et al. 2012, Mueller 2012). None of these studies, however, uses within-plant changes in council status or council age to identify effects.

One of the cornerstones in the works council literature is that the broader industrial relations system matters for the effectiveness of councils. Theoretical papers (Freeman and Lazear 1995) and empirical papers (e.g. Huebler and Jirjahn 2003, Mueller 2011) suggest a more positive role of councils if wages are negotiated at a higher level than the plant level. This has been used as a central argument on why works councils may not work in other industrial relations settings, e.g. in the United States. However, so far no evidence on the interaction between collective wage agreements and plant level codetermination is available that conditions on plant fixed effects. Our fixed effects results provide a somewhat nuanced view on this issue: in line with expectations, we do find positive long-run effects after an introduction period in plants covered by collective wage agreements and somewhat weaker evidence within uncovered plants. While acknowledging the possibility that our data lacks sufficient within-plant variation in council age to give definite answers for subgroups, the substantial negative introduction effect in covered plants, however, contradicts the standard paradigm. This finding should be subject of further exploration as a sound understanding of the interplay with other mechanisms of the industrial relations system is important for optimal policy design.

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**Table 1:** Variable Definitions and Descriptive Statistics *(N = 23,138)*

| *Variable* | *Definition* | *Mean, SD* |
|---|---|---|
| Log(Labor Productivity) | Log of value added (in Euro) per employee. | 11.49, .925 |
| Log(Employment) | Log of the number of employees. | 3.29, 1.26 |
| Woco Age | Works council age (figure refers to the year 2012). | 22.39, 16.80 |
| Woco Age Dummies | Dummy variables for categories in the works council age. | |
| < 5 years | | .023, .149 |
| 5-9 years | | .014, .118 |
| 10-14 years | | .012, .110 |
| >=15 years | | .052, .222 |
| Skilled Employees | Skilled blue-collar and white-collar employees as a share of total employees. | .599, .264 |
| Part-time Employees | Part-time employees as a share of total employees. | .208, .222 |
| Apprentices | Apprentices as a share of total employees. | .049, .076 |
| Women | Female employees as a share of total employees. | .376, .268 |
| Single Plant | The dummy equals 0 if the plant is part of a multi-plant firm and 1 if it is not. | .798, .402 |
| Limited Liability | The dummy equals 0 if the plant is individually owned or a partnership and 1 if it is a limited liability company or a company limited by shares. | .665, .472 |
| Exporter | Dummy equals 1 if the establishment exports | .344, .475 |
| Collective Bargaining | Dummy equals 1 if the establishment is covered by a collective bargaining agreement. | .405, .491 |
| Technology | Five dummy variables for the vintage of production technology used (1 = state of the art technology, …, 5 = very old). | |
| Technology = 1 | | .216, .412 |
| Technology = 2 | | .487, .500 |
| Technology = 3 | | .268, .443 |
| Technology = 4 or 5 | | .029, .167 |

**Table 2:** Labor Productivity Regressions, Fixed Effects Within Estimator

| | (1) | (2) | (3) |
|---|---|---|---|
| Works Council | -0.081** | -0.054** | --- |
| | (0.026) | (0.025) | |
| Works Council Age Dummies | | | |
| > 2 years prior to introduction | --- | --- | 0.069* |
| | | | (0.036) |
| 1-2 years prior to introduction | --- | --- | 0.046 |
| | | | (0.028) |
| 5-9 years | --- | 0.109*** | 0.108*** |
| | | (0.027) | (0.027) |
| 10-14 years | --- | 0.162*** | 0.161*** |
| | | (0.035) | (0.035) |
| >= 15 years | --- | 0.226*** | 0.225*** |
| | | (0.042) | (0.042) |
| Works Council Age | 0.016*** | --- | --- |
| | (0.004) | | |
| (Works Council Age)^2 / 100 | -0.001 | --- | --- |
| | (0.001) | | |
| Log(Employment) | -0.425*** | -0.427*** | -0.427*** |
| | (0.009) | (0.009) | (0.009) |
| Skilled Employees | 0.143*** | 0.142*** | 0.142*** |
| | (0.014) | (0.014) | (0.014) |
| Part-time Employees | -0.152*** | -0.153*** | -0.153*** |
| | (0.019) | (0.019) | (0.019) |
| Apprentices | -0.122** | -0.116** | -0.117** |
| | (0.051) | (0.051) | (0.051) |
| Women | -0.021 | -0.023 | -0.023 |
| | (0.025) | (0.025) | (0.025) |
| Collective Wage Agreements | 0.006 | 0.007 | 0.007 |
| | (0.007) | (0.007) | (0.007) |
| Within R-Squared | 0.142 | 0.142 | 0.142 |

Dependent variable: Log(Labor Productivity). Years 1998-2012, West Germany, private sector, plants with more than 5 workers. 23,138 observations from 5,759 plants. Dummies for year, single plants, legal status, exporter status, and technological status are included. Standard errors in parentheses. * denotes statistical significance at the 10% level, ** denotes statistical significance at the 5% level, and *** at the 1% level, respectively.

**Table 3:** Labor Productivity Regressions, Fixed Effects Within Estimator, Subgroup Analysis

| | Collective Wage Bargaining | Individual Wage Bargaining | < 100 employees | >= 100 employees |
|---|---|---|---|---|
| Works Council | -0.104*** | -0.001 | -0.100*** | -0.004 |
| | (0.035) | (0.039) | (0.032) | (0.041) |
| Woco Age Dummies | | | | |
| 5-9 years | 0.120*** | 0.083 | 0.038 | 0.098*** |
| | (0.031) | (0.055) | (0.039) | (0.035) |
| 10-14 years | 0.180*** | 0.097 | 0.021 | 0.168*** |
| | (0.040) | (0.074) | (0.054) | (0.044) |
| >= 15 years | 0.234*** | 0.177** | 0.101 | 0.177*** |
| | (0.050) | (0.087) | (0.076) | (0.053) |
| Log(Employment) | -0.453*** | -0.428*** | -0.440*** | -0.403*** |
| | (0.016) | (0.012) | (0.011) | (0.025) |
| Skilled Employees | 0.076*** | 0.192*** | 0.141*** | 0.053 |
| | (0.022) | (0.019) | (0.015) | (0.042) |
| Part-time Employees | -0.137*** | -0.153*** | -0.143*** | -0.163*** |
| | (0.031) | (0.025) | (0.020) | (0.061) |
| Apprentices | -0.087 | -0.069 | -0.126** | 0.465 |
| | (0.078) | (0.071) | (0.051) | (0.396) |
| Women | -0.025 | -0.026 | -0.014 | 0.004 |
| | (0.042) | (0.034) | (0.027) | (0.008) |
| Collective Wage Agreement | --- | --- | 0.002 | 0.014 |
| | | | (0.008) | (0.020) |
| Constant | 13.053*** | 12.647*** | 12.653*** | 13.555*** |
| | (0.065) | (0.051) | (0.040) | (0.151) |
| Observations | 9,376 | 13,762 | 19,750 | 3,388 |
| R-squared (within) | 0.147 | 0.149 | 0.130 | 0.185 |

Dependent variable: Log(Labor Productivity). Years 1998-2012, West Germany, private sector, plants with more than 5 workers. Dummies for year, single plants, legal status, exporter status, and technological status are included. Standard errors in parentheses. * denotes statistical significance at the 10% level, ** denotes statistical significance at the 5% level, and *** at the 1% level, respectively.

## Appendix

As the question on council age was asked in 2012, the information is missing for plants not observed in 2012. In the following, we describe how we computed the council age information for these plants and for plants that are observed in 2012 but have a missing value for council age or have a response of “old council” without an exact age. Before turning to council age, we cleaned the information regarding works council existence. For plants that have a missing value in the council status in one year or two subsequent years, the missing information was replaced by the council status in the previous and the subsequent year if the status was the same in both years. In the years 1994, 1995, and 1997 council information was asked only for newly sampled plants. We replaced the missing values for all incumbent plants of 1994 and 1995 with the 1993 information and the 1997 missing value with the 1996 information. Note that we don’t use information prior to 1998 directly for estimation. The above cleaning is also not generating any changes in council status (this would be relevant as a starting point for council age). It however smoothes interrupted works council time series, which would otherwise be dropped during the next steps of cleaning.

Plants that previously had a council but have none today would contaminate the control group if councils actually have lasting impact on production processes and firm culture. We thus drop all observations belonging to years with council closure and thereafter so that we keep observations prior to council closure, only. We end up with a sample of plants having at most one status change in the council variable. After this restrictive cleaning procedure, we end up with relatively few but clean within plant time series in council existence. Based on the corrected works council histories, we computed works council age. If we observed council foundation directly, we set the year of foundation accordingly.