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Works Councils in the Production Process

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Abstract

This paper uses data from a nationally representative panel of establishments to estimate the effects of German works councils on firm performance, 1997 – 2000. We analyze the impact of this institution on sales and sales growth using OLS and fixed effect estimates of a translog production function as well as by employing a model in first differences. With cross-sectional and pooled data, the strong pro-productivity effects of works councils noted in the recent literature prove sensitive to disaggregation – most notably for plants with 21 to 100 employees, where the powers of the council are a datum – even if the coefficient estimates for the works council variable are often substantive. However, the fixed effects estimator yields much smaller works council effects that are (weakly) statistically significant in only one instance, while productivity changes do not differ between plants with and without a works council in the first differences specification. We conclude that reports of positive works council effects on productivity have been much exaggerated. That said, there is no evidence that works councils adversely affect firm performance, as suggested by earlier empirical literature based on small samples of firms.

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1. Introduction

Although the German system of co-determination has increasingly been attacked on efficiency grounds by businessmen and their representatives, recent research – such as that of Frick and Möller (2003) in this journal – has suggested that establishments with works councils have considerably higher productivity than establishments in which they are absent. In seeming anticipation, in the last reform of the German Works Constitution Act the stated rea-

* We are indebted to two anonymous referees for their helpful remarks on a previous version of this paper. All computations were undertaken at the Institute for Employment Research of the Federal Labor Agency, using Stata / SE 8.0. In the interests of replication, the do-files are available from *thorsten.schank@wiso.uni-erlangen.de*

sons for strengthening worker rights and extending works council coverage included for the first time an efficiency rationale (see Addison / Bellmann / Schnabel / Wagner, 2004).

Yet, as a practical matter, there is no consensus on the effects of this type of employee representation on firm performance or, more generally, of the contribution of various employee involvement mechanisms, including board representation. Indeed, when the changes to the German legislation were first mooted, the commission set up to review the operation of the existing codetermination machinery and to make recommendations for its improvement concluded that the available econometric evidence was "equivocal" (Kommission Mitbestimmung, 1998, 61; English summary, 13).

In the years since the commission reported, there has been a dramatic increase in research on the economic effects of works councils (see Addison / Schnabel / Wagner, 2004). This development chiefly reflects the availability of improved data sets. But it is also indicative of the heightened international interest in the German institution at a time when unionism – the conventional form of workplace representation in other nations – is in severe decline.

The present paper is offered as a contribution to the debate on the economic impact of worker representation, with particular reference to this institution in Germany and focusing on the determinants of labor productivity. Although the productivity effects of works councils have been investigated in the past, most studies have lacked data on the capital stock, raising a potential omitted variables problem. In this inquiry, we use a data set containing a (crude) proxy-variable for capital, and provide works-council-in-the-production-function estimates as well as an approach based on first differences (analyzing productivity growth). We investigate whether the strong pro-productivity effects reported in the most recent works council literature using this data set are robust with respect to unobserved plant heterogeneity and works council heterogeneity / differences in works council powers.

The structure of the paper is as follows. Section 2 sets the scene for our empirical inquiry by reviewing the case for works councils and offering a thumbnail sketch of the developing empirical literature. The estimating framework is detailed in Section 3, and a description of the unique data set is given in Section 4. Our findings are reported in Section 5. An interpretative section concludes.

2. Nature of the Works Council Effect and the Extant Empirical Evidence

Despite differences between the two entities, the efficiency case for works councils has largely relied on exactly the same set of arguments used to make the economic case for unionism, namely, notions of collective voice (Freeman, 1976; Freeman / Medoff, 1984). Collective voice emphasizes the information problems that arise in real world labor markets and seeks to explain how collective voice via the union agency may outperform other (inferential or direct) means of eliciting private information. One reason is the public goods problem of preference (under-) revelation caused by the non-rival consumption of shared working conditions and common workplace rules. By collecting information on the preferences of all workers and aggregating them, unions can determine the social demand for such goods and enable the firm to choose a more efficient mix of personnel policies.[1]

A second key feature of collective voice is governance, namely, the policing or monitoring of incomplete employment contracts through specialized procedural arrangements (e.g. grievance and arbitration procedures). Since such rules are not unique to union settings, the argument must be that unions make it easier to negotiate and administer them. A union that specializes in information about the contract and in the representation of workers can prevent employers from engaging in opportunistic behavior.[2] Workers may be expected to withhold effort and cooperation when the employer cannot credibly commit to take their interests into account. In other words, if the reputation effects mechanism is weak, there is scope for unionism to be pro-productive (i.e. facilitate long-term efficient contracting).

Although the collective voice model emphasizes governance, it contains virtually no discussion of bargaining power. Yet if the union is to make credible the employer's *ex ante* promises, theory tells us that there must be some threat of credible punishment by the union (see Malcomson, 1983). The model simply sidesteps the potential holdup problem implicit in governance by treating the exertion of bargaining power and the expression of voice as distinct and offsetting facets of unionism (referring to the "two faces" of unionism; see Addison / Belfield, 2004).

Nevertheless, recognition of the bargaining problem motivates a purpose-built model of the works council offered by one of the architects of collective voice. In a model that retains many of the features of collective voice, Freeman and Lazear (1995) argue that participation / codetermination will be underprovided by the market because institutions that give power to workers will affect the distribution as well as the size of the surplus. The authors argue that

1 The model also contends that in addition to providing a direct channel of communication between the two sides, collective voice offers an alternative to exit (i.e. quitting) as a way of expressing discontent. Both factors are said to reduce labor turnover, permitting lower hiring and training costs and stimulating firm-specific human capital investments. This effect may be underwritten by the governance aspect discussed below.

2 For example, it has been argued that unions may counteract the tendency of the employer to hold-up the sunk investments of workers in training (see Menezes-Filho / Van Reenen, 2003, 299).

the joint surplus of the enterprise will increase with the progression from information exchange through consultation to codetermination rights. Unless these rights are constrained, however, they may be expected to rise to the level of a bargaining problem as manifested in the model by a worker share in the joint surplus that is increasing in that surplus and by a capital share that is declining absolutely as well as relatively. The workers' share rises because knowledge and involvement are power, so that the very factors that cause the surplus to rise also cause profitability to fall, with the result that workers will demand too much power / involvement because their share will continue to rise after the joint surplus has peaked. Symmetrically, employers will either oppose works councils or vest them with too little power because profits decline even as the surplus is increasing.

Accordingly, some means of third-party regulation limiting bargaining power has to be found if the potential efficiency gains of worker voice are to be realized. Here, Freeman and Lazear (1995) see the German institution as attractive in two respects: first, because under the 'peace obligation' it cannot strike; and, second, because it cannot formally engage in bargaining over wages and other conditions of employment unless expressly authorized to do so under the relevant industry-level or regional collective bargaining agreement. The key element in the works council model, then, is the potential "decoupling" of the factors that determine the size of the surplus from those that determine its distribution. Left open is whether or not there is a *sufficient* decoupling in practice. Even if the works council is formally an exemplary collective voice institution, therefore, this model of the works council does not provide an unambiguous answer as to its consequences for efficiency.

If the efficiency of works councils is ultimately an empirical question, the applied literature has produced extremely divergent results. The literature can be divided into three stages of analysis, beginning with studies based on small samples of plants, through investigation of much larger manufacturing data sets covering either a single region or sector, to analyses of nationally representative samples of establishments (for a detailed survey see Addison / Schnabel / Wagner, 2004). Studies of the first stage contain a wide range of performance outcomes. These include objective and subjective measures of profits, product innovation and R&D, investment in physical capital, and (excessive) quits,[3] even if few studies investigate works council effects on total factor productivity or labor productivity.[4] Despite their often high sophistication, all these studies are potentially hamstrung by problems associated with small sample size.

3 See, respectively, FitzRoy / Kraft (1985); Addison / Kraft / Wagner (1993); Addison / Wagner (1997); FitzRoy / Kraft (1990); Addison / Wagner (1997); Schnabel / Wagner (1994); Addison / Kraft / Wagner (1993); and Kraft (1986).

4 But see FitzRoy / Kraft (1987); Addison / Kraft / Wagner (1993).

Studies of the second stage mainly exploit the Hanover Firm Panel and the NIFA-Panel. The former is a stratified random sample of all manufacturing plants with at least five employees in the German state of Lower Saxony, while the latter covers all establishments in the German machine tool industry.[5] Sample sizes typically exceed 900 establishments, as compared with less than 100 plants in the case of the studies of the first stage. Other advantages include the panel nature of the new data sets (covering 1994–97 and 1989–99, respectively), richer information on employee involvement / high performance work practices, and, in the case of the NIFA panel, actual information on works council 'type' and 'degree of works council involvement' as assessed by management. Unfortunately, neither data set includes a measure of capital, while the advantage of the longitudinal capacity of the data is undercut by attrition and a small number of changes in works council status.

Studies of the third stage exploit a truly nationally representative data set, namely, the Establishment Panel of the Institute for Labor Market Research of the Federal Labor Agency. This data set is used in the present inquiry and is described in Section 4. Unlike its counterparts of the second phase, in containing an indirect measure of the capital stock this panel allows the researcher to estimate formal production functions. Furthermore, its longitudinal capacity can be better exploited.

Pronounced differences in findings characterize the developing literature. Results from the first phase of the empirical literature are flatly pessimistic with respect to the ability of representative participation to improve establishment performance.[6] On the other hand, the results from the next phase of research are decidedly less pessimistic, for an admittedly narrower range of outcome indicators. Not only is the overall 'effect' of works councils no longer adverse – with the principal exception of profitability, where the observed negative association might still represent an efficiency-neutral transfer – but some pro-productive outcomes are also observed. But it is the third stage of research that provides the most positive evaluations to date of works council impact on firm performance. Indeed, reminiscent of Brown and Medoff's (1978) pioneering union-in-the-production-function tests for the United States,

5 Studies using the Hannover Firm Panel include Addison / Schnabel / Wagner (2001); Jirjahn (2003); and Hübler / Jirjahn (2003). Among the smaller number of studies exploiting the NIFA-Panel are Frick (2002a) and Dilger (2002).

6 The studies by FitzRoy and Kraft (1985, 1987, 1990) are particularly noteworthy in this regard, suggesting that efficient managers are able to elicit greater effort from their workforces without interference from works councils. They are also portrayed as being able to institute adequate systems of communication and decision-making and avoid (the delays associated with) works councils – in part by paying higher wages. Other negative results from this stage include reduced investment in works council regimes (Addison / Kraft / Wagner, 1993) and the seeming failure of collective voice through the works council – versus *individual* voice – to significantly lower excess quits (Kraft, 1986).

the first two such studies of this phase point to 25 to 30 per cent *higher* labor productivity in works council regimes (Frick, 2002b; Frick / Möller, 2003).[7]

The goal of the present exercise is to establish whether these new OLS findings on productivity are robust with respect to establishment size and unobserved plant heterogeneity. One reason to stratify the sample by employment is that that the power of works councils – as indexed by their information, consultation, consent, and codetermination rights – is a stepped function of establishment size under the Works Constitution Act. Ideally, we should like to take this diversity into account in testing for works council effects. This is achieved here by differentiating between all plants and those with between 21 and 100 employees where the formal powers of the works council are to all intents and purposes a datum. This approach is also justified by a strictly practical concern. In Germany most establishments above a certain size have works councils, while most plants below a certain size do not: in 2000, for example, 9.1 (91.7) percent of establishments employing between 5 and 20 (over 500) employees had a work council (Addison / Bellmann / Schnabel / Wagner, 2003). In other words, within certain ranges of firm size, one cannot hope to obtain a reliable measure of works council impact using a measure based on works council presence alone (all that is available in the IAB Establishment Panel). Also observe that works councils are found in roughly one-third of our preferred sub-sample of establishments with 21 to 100 employees. The issue of omitted firm effects on productivity is potentially no less pressing a concern since the ambitious pro-productive effects estimates of works councils may reflect a positive correlation between firm characteristics that do not change quickly over time and works council presence *and* the productivity of the firm. For this reason, we also provide fixed-effects estimates of the production function and a model in first differences in which these relatively stable differences between firms drop out.

3. The Production Function Framework

A straightforward way to characterize the technology of a firm is the production function, defined as the maximum output of y attainable with a given set of inputs x and a given technology. We will use two production function specifications in our empirical analysis: the more general translog and its nested Cobb-Douglas specification.

7 However, the two most recent studies using the IAB Establishment Panel fail to detect statistically significant differences in efficiency between establishments with and without works councils (see Schank / Schnabel / Wagner, 2004; Addison / Bellmann / Schnabel / Wagner, 2004). We might also mention that other research using the IAB Panel points to higher rates of plant closings among works council establishments, hinting at upward bias in cross-section estimates of works councils' productivity effects (see Addison / Bellmann / Kölling, 2004).

The translog production function – introduced by Christensen, Jorgenson, and Lau (1971, 1973) – belongs to the family of flexible functional forms. These are local approximations of an arbitrary, twice continuously differentiable production function. The translog function is defined as:

$$\ln y = \beta_0 + \sum_{i=1}^{n} \beta_i \ln x_i + \frac{1}{2} \sum_{i=1}^{n} \sum_{j=1}^{n} \beta_{ij} \ln x_i \ln x_j \ , \tag{1}$$

with $\beta_{ij} = \beta_{ji}$. Note that although this formulation is linear-in-parameters and conceptually simple, it does not impose any restrictions on returns-to-scale and the substitution elasticities.

In our empirical application, we use labor (N) and capital (K) as inputs, so that equation (1) becomes:

$$\ln y = \beta_0 + \beta_1 \ln N + \beta_2 \ln K + \beta_{11} \frac{(\ln N)^2}{2} + \beta_{22} \frac{(\ln K)^2}{2} + \beta_{12} \ln N \ln K + \gamma Z + \varepsilon \ , \tag{2}$$

where the vector Z captures additional control variables and ε is an error term. If the translog function is viewed as an approximation of the underlying production technology, the higher than second-order terms will be absorbed in the error term. And since these terms depend on x (in our case N and K), the parameter estimates will be biased and inconsistent. Consequently we follow a strategy common to many empirical investigations and assume that the translog is not an approximation, but rather an exact functional relationship. This allows us to consider the disturbance ε in equation (2) as a white noise error term, comprising random variation of (a) the technology of the production unit, (b) the environment of each firm, (c) the behavior of the production unit, and (d) observational errors (measurement or aggregation errors).

The output elasticities with respect to employment and capital – namely, the percentage change in output following one per cent change in employment and capital, respectively – are given by the expressions:

$$\frac{\partial \ln y}{\partial \ln N} = \beta_1 + \beta_{11} \ln N + \beta_{12} \ln K \tag{3}$$

$$\frac{\partial \ln y}{\partial \ln K} = \beta_2 + \beta_{22} \ln K + \beta_{12} \ln N \ . \tag{4}$$

If we insert the coefficient estimates and values of the sample means for $\ln N$ and $\ln K$. we obtain estimates of the respective elasticities. Note that constant returns-to-scale at all input levels impose the following restrictions on the parameters: first, that the coefficient estimates for (log) employment and (log) capital sum to unity ($\beta_1 + \beta_2 = 1$); and, second, that the coefficient estimate for the interaction term between (log) employment and (log) capital sums to zero with that of squared (log) employment, and similarly for the coefficient estimate on squared (log) capital ($\beta_{11} + \beta_{12} = 0$, $\beta_{22} + \beta_{12} = 0$).

A (nested) variant of the more general translog production function is the Cobb-Douglas function. Although returns-to-scale are still allowed to be below, equal to, or above unity, the elasticity of substitution is restricted to one. The model is obtained by restricting the coefficients $\beta_{11}, \beta_{22}, \beta_{12}$ to be zero. Accordingly, the hypothesis that the specification of a Cobb-Douglas technology is appropriate can be examined by testing the joint significance of these three coefficients.

4. The Data Set and Estimation Strategies

Our data are taken from the IAB Establishment Panel of the Institute for Employment Research of the Federal Labor Agency. Each year since 1993 (1996), this panel has surveyed several thousand establishments from all sectors of the economy in western (eastern) Germany. It is based on a stratified random sample – strata for 16 industries and 10 size classes – from the population of all establishments with at least one employee covered by social insurance. To correct for panel mortality, exits, and newly-founded units, the data are augmented regularly, yielding an unbalanced panel. Data are collected in personal interviews with the owners or senior managers of the establishments by professional interviewers. The panel is created to serve the needs of the Federal Labor Agency, so its focus is on employment-related matters. Note that the IAB panel is the only nationally representative longitudinal sample of establishments that can be used to investigate the impact of works councils. Further details regarding the IAB panel are given in Kölling (2000).

Our inquiry uses information for the years 1997 to 2000. Since some of the information relating to year *t* is asked for in the survey conducted in the following year – an example being total sales in year *t* – we will actually use data from five surveys. The early years of the panel were excluded because one focus of the present exercise is to compare western and eastern Germany and because, as previously noted, establishments in eastern Germany were only surveyed from 1996 onward. Further, we do not employ data for 1996 because we use information on replacement investment to measure capital, and this question was asked for the first time only in 1997.

All for-profit establishments in the manufacturing and service sectors are considered, other than those in banking and insurance where output is measured differently. Establishments in agriculture, forestry, and fisheries were excluded for two reasons: first, the production process in this branch differs from that in other sectors; and, second, councils are present in just three percent of all such establishments, as compared with 12 percent in the rest of the economy. The remaining exclusion is establishments with fewer than five employees. This is because the German legislation only makes provisions for works council elections in establishments with at least five employees. Observe that, consistent with the terms of the legislation, we include part-timers and apprentices in this total.

The empirical models are estimated for all establishments, and separately for establishments with 21 to 100 employees, as has become standard practice in investigations of works council impacts (see, e.g., Addison / Schnabel / Wagner, 2001; Dilger, 2002; Jirjahn, 2003). The reasons for this strategy are twofold: first of all, and more important, works council rights under the law tend to increase with firm size but are a datum for plants with 21 – 100 employees. Looking at establishments in this category is a means of accounting for the heterogeneity of works councils in the absence of other measures. Second of all, works councils are rare among very small establishments while establishments without a works council are rare in higher size classes. For our sub-sample of establishments, however, there is much greater balance of the two workplace regimes. As can be seen from the lower panel of Table 1, one-third of plants with 21 – 100 employees have works councils. It is of no small interest – and one form of robustness check – to determine whether or not the empirical results for all establishments can be replicated in this sub-sample.

Table 1

Works Councils Presence in Establishments (Percentages)

	Total		Manufacturing		Services	
	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$
	Weighted					
Germany	11.7	28.5	14.0	31.6	11.0	26.3
western Germany	12.0	29.0	15.3	34.3	10.8	25.8
eastern Germany	10.6	26.0	9.3	22.7	12.0	28.9
	Unweighted					
Germany	42.7	33.5	49.3	34.8	34.3	30.2
western Germany	49.7	36.6	64.1	44.6	34.7	28.2
eastern Germany	34.1	30.3	34.6	27.9	33.9	32.9

Source: IAB-Establishment Panel, 1997 – 2000.

In investigating the impact of works councils on establishment productivity, we apply three different estimation strategies, the first two of which estimate the determinants of the output levels while the third focuses on productivity growth. On occasion, as will be noted, variable definition will differ between the first two and the third estimation strategies.

Our first approach uses pooled data for 1997 to 2000 to estimate translog production functions by OLS. As remarked in Section 3, the translog specification is preferred because it is the least restrictive production function, nesting Cobb-Douglas and other specifications (such as CES). The endogenous variable, output, is measured as the volume of total sales of the establishment in a year. (Ideally, output should be measured in physical units, but this information is not available in the IAB panel.) Recent studies using the panel by Frick (2002b) and Wolf and Zwick (2002) deploy a more conventional measure of output, namely, value added, which is computed by subtracting the costs of materials from sales. Although conceptually superior to total sales, this value-added measure suffers from two limitations. First, survey respondents are asked to estimate the percentage share of total sales represented by materials cost, and (1 minus) this share is used in conjunction with sales volume to derive value added. Unfortunately, unlike the sales measure, these share-in-sales values seem to be little more than "informed guesstimates."[8] Moreover, the respondents often fail to answer the materials cost question in the survey, so that use of value added involves a large reduction in the number of observations. For example, the sample of all establishments with five or more employees would be reduced by 20 percent (from 11,464 to 9,361 units) if we used value added. Given that log total sales and log value-added (as constructed) are strongly correlated for the establishments in the IAB panel and for all the sub-samples considered here,[9] we opted not to lose a large part of the sample and hence will work with sales volume as our proxy for output.

Turning to the exogenous variables, the key argument in the augmented function is of course the works council variable, as measured by the presence or absence of this council. Although information on most variables is collected for each wave of the panel, this is not the case for our workplace representation covariate. Specifically, the works council question was asked of all establishments in 1993, 1996, 1998 and 2000, and in the 'missing' years only of panel accessions – although the IAB provides interpolations for 1999 based on information from 1998. Given our sample period, however, we are only lacking information on works council status for 1997 among those establishments that were not panel accessions in that year. In these cases, we used interpolation if the reported works council status was identical either side of this miss-

8 Almost two-thirds of the observations are in multiples of 5 percent, implying measurement error, while the reported share of material costs changes on average by 11.6 percent in any two years, which is unrealistically high

9 Using pooled data for 1997 to 2000, the correlation coefficients for the all-establishment sample and for manufacturing and services are 0.942, 0.962, and 0.891, respectively. The corresponding values for western (eastern) Germany are 0.950 (0.918), 0.970 (0.937), and 0.900 (0.875).

ing year. But establishments reporting different works council regimes in 1996 and 1998 were excluded from the sample in the missing year.

We measured labor input by the total number of employees. We did not correct this total by computing full-time equivalents for part-time workers or by adjusting for the lower input per head of apprentices, lacking information on hours and productivity of the two groups. But we did seek to take account of these compositional effects by entering the percentage employment shares of part-time workers and apprentices as separate control variables. Furthermore, we also included the proportion of skilled workers in an attempt to control for differences in the quality of labor inputs. While hours worked would be a better measure of labor input, the panel does not allow us to construct an 'annual hours worked' variable.

Nor for that matter does the panel provide direct information on the establishment's capital stock. This is a familiar problem when working with firm-level survey data. As our measure of capital input in year t, we use the average amount of replacement investment reported for years $t-1$ and t. The idea here is that the known amount of replacement investment is expected to be proportional to the unknown amount of capital stock. We should caution that about 30 percent of all establishments in the sample report a value of zero for replacement investment at least once. Frankly, this problem is not flagged in contemporary research using replacement investment in t as a proxy for capital stock in that year (e.g. Frick, 2002b). Our solution was as follows: all firms reporting zero replacement investment in both year $t-1$ and year t were excluded from the sample for year t. The effect was to reduce the size of the sample by 2,665 observations, or about 17 percent. While admitting that our investment variable is a rather imprecise proxy variable and that the exclusion of such a large number of observations could pose a selectivity problem, we would nevertheless regard this approach as an improvement compared to previous analyses that did not use capital variables at all.

We used two variables to proxy differences in the quality of the capital stock: first, a dummy variable indicating whether or not the establishment invested in information and communication technologies in year t; and, second, an index indicating the state of technology in year t (ranging from $1 =$ "state-of-the-art" down to $5 =$ "obsolescent"). The remaining variables comprised a bargaining dummy – whether or not the establishment is contemporaneously covered by a collective agreement – and controls for industry, year and eastern Germany. The translog production functions were estimated by OLS based on an unbalanced panel of pooled data for the four years 1997 to 2000.[10]

[10] Stata / SE 8.0 was used to estimate the empirical models, using the *cluster (establishment)* option because observations are considered to be independent across but not necessarily within establishments. For the estimation of the variance-covariance matrix of the coefficient vector the Huber / White estimator was used.

The appropriateness of OLS might be called into question because of unobserved firm heterogeneity and works council endogeneity. The former issue is addressed by an estimation strategy that more fully exploits the longitudinal capacity of our data; that is, we apply the fixed effects estimator which controls for unobserved (time-invariant) differences between establishments. Unobserved establishment heterogeneity – due, say, to differences in management quality – leads to inconsistent OLS (but not fixed effects) estimates of the impact of works councils on productivity only if these unobserved characteristics are correlated both with productivity (as we would expect to be the case) and also with the existence of a works council. On the other hand, as is well known, identification of fixed effects estimates of works council impact rests on within-plant changes in the works council regime. For our non-stratified regression sample, which comprises 5,684 establishments, only 117 plants (or 2 percent of the total) changed their works council status in either direction. For these reasons, it will be interesting to see whether fixed effects and pooled OLS deliver different parameter estimates.

As far as the endogeneity issue is concerned, we know that works council presence is not random but is associated with establishment size and the structure of the work force among other things (see Addison / Bellmann / Schnabel / Wagner, 2003). However, the introduction or presence of a works council is not the result of purposive action on the part of the employer based on a comparison of costs and benefits; rather, it is the results of actions taken by employees. Now employee action might well be related to the past performance of the establishment, but we argue that the latter is mainly related to the formation or dissolution of a works council. For example, large increases in a plant's productivity may induce the work force to set up a works council in order to access some of the rents. Conversely, there may also be a strong incentive to form a works council during a downturn in the economic situation of an establishment when employees are fearful of losing their jobs. As noted above, however, the vast majority of plants (98 percent) do not change their works council status. Hence, for almost all plants in our regression sample, we have no reason to believe that the existence of a works council is caused by contemporaneous productivity. Therefore, endogeneity should not pose a severe problem in the present context. Our argumentation may not hold, however, for the fixed effects regressions. In this case, the parameter estimate for the works council dummy is identified only by those plants that change their works council regime, which developments might be correlated with changes in productivity. Yet, as a practical matter, empirical evidence on the determinants of works council formation suggests that neither the profit situation of the plant nor its productivity level matters for the decisions of workers to install a works council (see Addison / Bellmann / Schnabel / Wagner 2003; Dilger 2003).[11]

[11] Nevertheless, we also provide summary (and unrealistic) results of the effects of instrumenting the works council dummy in Section 5 below.

Our third approach differs from the first two estimation strategies in focusing on productivity growth. The output variable is the difference in an establishment's log total sales between 1997 and 2000. Works council status is indicated by a dummy variable indicating whether an establishment had a works council in both 1997 and 2000, the reference group comprising establishments without a works council in either year.[12] The change in labor input is measured by the difference in the log number of employees in an establishment between 1997 and 2000. We also include changes in the percentage shares of part-time workers, apprentices, and skilled employees to control for compositional changes in the work force. Given the lack of information on the capital stock in the establishment in 1997 and 2000, the change in this input was obtained as follows. We first estimated the capital stock in 1997 by multiplying the two-year average of replacement investment in 1996 and 1997 by six, the assumption being that capital depreciates over six years at a constant rate. For each of the following years, the capital stock was measured as the sum of the capital stock in the previous year plus the amount of extension (i.e. net) investment reported for the current year. In other words, the capital stock in 2000 is given by the two-year average of replacement investment in 1996 and 1997, multiplied by six, plus the sum of extension investments over each of the next three years.[13] We used a (augmented) Cobb-Douglas production function because F-tests did not reject this functional form in favor of the more general translog specification at the .05 level. To control for (changes in) the quality of the capital stock, we included the average value of the dummy variable indicating investment in information and communication technology over the sample period, and the average of the index indicating the state of technology in each year. Finally, coverage by a collective agreement or otherwise is indicated by three dummy variables: establishments covered by a collective agreement in both 1997 and 2000; those without a bargaining agreement in 1997 but with one in 2000; and those with such an agreement in 1997 but without one in 2000. The reference group thus comprises establishments without a collective agreement in either year. This model in first differences was estimated by OLS using a heteroscedasticity-consistent covariance matrix estimator.

5. Findings

We next provide results for each of our three estimation strategies. It will be recalled that the first procedure uses unbalanced panel data, 1997 – 2000, to

[12] Establishments that changed their works council regime in this period were excluded from the analysis.

[13] In its official estimates of the capital stock, the German Federal Statistical Office uses depreciation periods of six and thirteen years (see Schmalwasser, 2001). By way of a sensitivity analysis, we repeated our calculations using a thirteen-year rule. The results were scarcely affected and we therefore report (and discuss) estimates based on a six-year depreciation cycle alone.

estimate translog production functions by OLS. Separate regressions were run for all establishments, for establishments with 21 to 100 employees, and separately for establishments by broad sectors (manufacturing and services) for Germany as a whole and for western and eastern Germany. Our findings are reported in Tables 2, 3, and 4.

For the full sample, comprising 11,464 observations and 5,684 plants, a highly statistically significant works council coefficient estimate of 0.232 is reported in column 1 of Table 2. This implies a beneficial effect of works council presence on plant productivity of 26.1 percent (viz. $\exp^{0.232}-1$). This effect is somewhat lower (higher) if we look at establishments from western (eastern) Germany only (see column 1, Tables 3 and 4); and it is lower (higher) in establishments from manufacturing (services) in each of the three geographical areas (see columns 3 and 5, Tables 2, 3, and 4). All of these positive coefficient estimates are statistically different from zero at a significance level of .01 or better, and range from 12 percent (manufacturing establishments in western Germany) to 34.2 percent (services in eastern Germany).

As was discussed in Section 3, the flexibility of the translog specification derives from the inclusion of squared employment and capital terms and the interaction of employment and capital, allowing output elasticities to vary with employment and capital. For the full sample of all establishments with five or more employees (reported in columns 1, 3 and 5 of Tables 2, 3, and 4), these three coefficient estimates are in all cases jointly statistically significant at the .10 level or better. Hence, the more restrictive Cobb-Douglas formulation is always rejected. For the sub-samples comprising establishments with 21 to 100 employees (reported in columns 2, 4, and 6 of Tables 2, 3, and 4) the Cobb-Douglas specification is not rejected in five out of nine cases. This result is not surprising, however, because there is less reason to believe that the output elasticities will vary within a narrowly defined size class.[14]

The output elasticities with respect to employment and capital *cannot* be directly read from the coefficient estimates and are derived according to equations (3) and (4). Since they vary with the amounts of labor and capital used, we report elasticities at sample means (of the logarithm of each variable) at the base of Tables 2, 3, and 4. The employment elasticity of output varies by sector, size class, and region between 75 and 95 percent. The capital elasticity is much lower, ranging between 8.2 and 13.4 percent (which is not surprising considering the roundabout way in which our capital variable had to be con-

14 It should be noted that, in the full sample regressions, the squared employment term and the interaction term between employment and capital are in all cases jointly significant (even if neither is statistically significant at conventional levels when tested separately). Moreover, in six out of nine cases, each has a negative sign, unambiguously implying that the output elasticity is decreasing in employment. See equation (3) in Section 3: if β_{11} and β_{12} are negative (and employment and capital are positively correlated), the output elasticity with respect to employment rises if employment increases.

structed). In both western and eastern Germany, the employment elasticity is greater for manufacturing than for services. And in all but one case, the elasticities are larger in the full sample than for the sub-sample of establishments with 21 to 100 employees. There are no systematic differences in this regard between the two regions.

We should add that the estimated coefficients for the control variables indexing the quality of the capital stock (investment in information and computer technology and the state of technology) and of the labor force (percentage share of part-time workers, apprentices, and skilled workers), as well as the dummy variable identifying establishments in eastern Germany, have the expected signs and are statistically significant at a conventional level in most cases. On the other hand, the variable indicating coverage by collective bargaining turns out to be either insignificant or, at best, only marginally significant.

Returning to the question that motivates this inquiry, we do not find the *magnitude* of the works council productivity effect reported for establishments with five or more employees to be credible. Even if there are good theoretical reasons in the collective voice tradition (see Section 2) to suggest that works councils might have a positive net impact on firm performance, one would not expect effects of this size given that establishments with and without works councils compete in the same market. And there are a number of reasons to suspect that estimates from this first step in our empirical investigation of works council effects on productivity are artefacts of the data. One possibility might be that size effects are insufficiently controlled for. As pointed out in Section 4, works councils tend to be rare in small establishments and ubiquitous in large ones. If productivity tends to increase with establishment size due to economies of scale, a positive relationship between works council presence and productivity will show up in the data even if there is no causal link between works councils and productivity.[15]

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15 We also sought to instrument the works council dummy to take account of any endogeneity. As is well known, the standard problem confronting the investigator in selecting proper identifying instruments is that they should be highly correlated with the (potentially) endogenous right-hand side variable but uncorrelated with the error term. In our case, a Sargan test rejected the latter requirement for the plant's profit situation (a variable suggested by an anonymous reviewer). We instead chose as instruments the share of female employees in the work force and a dummy variable indicating whether or not the establishment was a branch plant, which variables fulfilled both requirements. Unfortunately, we obtained implausibly large parameter estimates for the (instrumented) impact of the works council on plant productivity. Moreover, the estimate was extremely volatile; for example, including the share of female employees alone yielded a coefficient estimate of 0.64, which climbed to 2.37 with the addition of the branch plant dummy. Although strictly speaking both instruments do satisfy the two requirements noted above, we would conjecture that their incremental influence – over and above the contribution of the variables already included in the productivity equation – is probably not strong enough to properly identify the works council effect.

Table 2: **OLS Regressions of a Translog Production Function**

(pooled estimates, 1997 – 2000;dependent variable: total sales $(\log Y)$)

	Total		Manufacturing		Services	
Establishment size	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$
	(1)	(2)	(3)	(4)	(5)	(6)
Works council (dummy: 1 = yes)	0.232*** [8.88]	0.118*** [3.22]	0.177*** [5.71]	0.046 [1.03]	0.275*** [6.31]	0.183*** [3.05]
Number of employees $(\log N)$	1.041*** [17.80]	0.446 [0.70]	1.117*** [15.92]	0.228 [0.30]	1.075*** [11.92]	0.629 [0.60]
Capital stock $(\log K)$	–0.075 [1.59]	–0.168* [1.70]	–0.098* [1.89]	–0.063 [0.41]	–0.072 [0.96]	–0.257** [2.01]
$(\log N)^2/2$	–0.025 [1.59]	0.132 [0.79]	–0.023 [1.20]	0.171 [0.77]	–0.029 [1.32]	0.092 [0.35]
$(\log K)^2/2$	0.017*** [2.83]	0.026*** [3.30]	0.019*** [2.71]	0.013 [1.25]	0.02** [2.25]	0.037*** [3.32]
$\log N \log K$	–0.004 [0.43]	–0.009 [0.44]	–0.006 [0.54]	0.001 [0.04]	–0.011 [1.03]	–0.018 [0.63]
Investment in ICT (dummy: 1 = yes)	0.124*** [7.55]	0.096*** [3.72]	0.105*** [5.75]	0.051* [1.72]	0.145*** [4.88]	0.147*** [3.38]
State of technology (index: 1 = state-of-the art; 5 = obsolescent)	–0.082*** [7.71]	–0.083*** [4.62]	–0.076*** [6.36]	–0.077*** [3.74]	–0.085*** [4.57]	–0.101*** [3.29]
Parttime workers (percentage)	–0.992*** [15.26]	–1.02*** [9.24]	–1.455*** [15.36]	–1.544*** [9.28]	–0.735*** [9.09]	–0.817*** [6.28]

Apprentices (percentage)	–0.892*** [7.46]	–0.761*** [3.23]	–1.114*** [8.72]	–1.506*** [6.24]	–0.567*** [2.62]	0.162 [0.37]
Skilled workers (percentage)	0.344*** [9.61]	0.379*** [6.16]	0.234*** [5.43]	0.305*** [3.60]	0.432*** [7.73]	0.391*** [4.49]
Collective agreement (dummy: 1 = yes)	0.037* [1.88]	0.034 [1.10]	0.019 [0.92]	0.034 [0.91]	0.086** [2.27]	0.06 [1.10]
eastern Germany (dummy)	–0.355*** [16.63]	–0.33*** [9.27]	–0.298*** [12.55]	–0.267*** [5.99]	–0.417*** [11.37]	–0.42*** [7.24]
Constant	11.976*** [51.05]	13.914*** [10.06]	11.986*** [48.35]	13.803*** [8.40]	12.597*** [33.48]	14.665*** [6.58]
Output elasticities at sample means						
ε_{YN} *p*-value	0.892 [0.000]	0.835 [0.000]	0.945 [0.000]	0.894 [0.000]	0.833 [0.000]	0.763 [0.000]
ε_{YK} *p*-value	0.122 [0.000]	0.105 [0.000]	0.114 [0.000]	0.093 [0.000]	0.124 [0.000]	0.113 [0.000]
No. of observations	11465	3595	6870	1987	4595	1608
No. of plants	5684	1839	3413	1032	2271	807
R^2	0.90	0.60	0.93	0.57	0.83	0.62

Notes: Regressions also include sector and year dummies.

***, **, * denote significance at the .01,. 05, and .10 levels, respectively.

Table 3: **OLS Regressions of a Translog Production Function – Western Germany**

(pooled estimates, 1997 – 2000; dependent variable: total sales ($\log Y$))

	Total		Manufacturing		Services	
Establishment size	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$
	(1)	(2)	(3)	(4)	(5)	(6)
Works council (dummy: 1 = yes)	0.206*** [5.21]	0.098* [1.80]	0.113*** [2.59]	-0.042 [0.71]	0.27*** [4.14]	0.186** [2.04]
Number of employees ($\log N$)	1.028*** [12.05]	0.952 [0.95]	1.29*** [14.32]	1.118 [0.93]	0.993*** [7.71]	1.036 [0.69]
Capital stock ($\log K$)	–0.058 [0.81]	–0.327** [2.13]	–0.119* [1.67]	0.001 [0.00]	–0.053 [0.46]	–0.49*** [2.68]
$(\log N)^2/2$	–0.026 [1.17]	–0.09 [0.36]	0.011 [0.56]	–0.034 [0.10]	–0.056* [1.89]	–0.123 [0.34]
$(\log K)^2/2$	0.015* [1.67]	0.032*** [3.11]	0.03*** [3.31]	0.012 [1.11]	0.011 [0.82]	0.046*** [2.79]
$\log N \log K$	–0.001 [0.10]	0.015 [0.40]	–0.03** [2.50]	–0.01 [0.24]	0.007 [0.46]	0.014 [0.24]
Investment in ICT (dummy: 1 = yes)	0.104*** [4.18]	0.115*** [2.82]	0.088*** [3.18]	0.075* [1.67]	0.112*** [2.63]	0.15** [2.29]
State of technology (index: 1 = state-of-the-art; 5 = obsolescent)	–0.053*** [3.62]	–0.067** [2.49]	-0.036** [2.26]	–0.038 [1.29]	–0.064** [2.56]	–0.102** [2.39]
Part-time workers (percentage)	–1.049*** [11.91]	–1.018*** [6.43]	–1.317*** [9.89]	–1.598*** [7.36]	–0.862*** [7.97]	–0.724*** [3.67]

Apprentices (percentage)	–1.175*** [5.56]	–1.042** [2.30]	–1.555*** [6.18]	–2.51*** [6.19]	–0.786** [2.45]	0.189 [0.27]
Skilled workers (percentage)	0.446*** [8.80]	0.412*** [4.46]	0.293*** [4.92]	0.291** [2.56]	0.576*** [7.52]	0.475*** [3.69]
Collective agreement (dummy: 1 = yes)	0.008 [0.24]	0.012 [0.21]	-0.021 [0.62]	-0.035 [0.55]	0.073 [1.34]	0.043 [0.49]
Constant	11.837*** [29.28]	13.256*** [5.85]	11.642*** [27.51]	11.091*** [4.43]	12.572*** [21.78]	15.283*** [4.71]
Output elasticities at sample means						
ε_{YN} *p*-value	0.900 [0.000]	0.789 [0.000]	0.943 [0.000]	0.860 [0.000]	0.872 [0.000]	0.748 [0.000]
ε_{YK} *p*-value	0.127 [0.000]	0.109 [0.000]	0.134 [0.000]	0.102 [0.000]	0.106 [0.000]	0.102 [0.000]
No. of observations	5843	1646	3347	789	2496	857
No. of plants	3120	944	1717	439	1403	505
R^2	0.91	0.55	0.94	0.61	0.84	0.54

Notes: Regressions also include sector and year dummies.

***, **, * denote significance at the .01,. 05, and .10 levels, respectively.

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Table 4: **OLS Regressions of a Translog Production Function – Eastern Germany**
(pooled estimates, 1997 – 2000; dependent variable: total sales $(\log Y)$)

	Total		Manufacturing		Services	
Establishment size	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$
	(1)	(2)	(3)	(4)	(5)	(6)
Works council(dummy: 1 = yes)	0.266*** [7.77]	0.137*** [2.84]	0.222*** [5.25]	0.111* [1.85]	0.294*** [5.17]	0.154** [1.98]**
Number of employees $(\log N)$	1.149*** [12.57]	0.237 [0.30]	1.085*** [9.89]	–0.143 [0.16]	1.19*** [8.43]	0.448 [0.31]
Capital stock $(\log K)$	–0.096 [1.42]	–0.121 [1.00]	–0.131* [1.84]	–0.242 [1.19]	–0.039 [0.39]	–0.01 [0.06]
$(\log N)^2/2$	–0.047** [2.07]	0.154 [0.73]	–0.068** [2.22]	0.164 [0.65]	-l0.034 [1.01]	0.142 [0.38]
$(\log K)^2/2$	0.021** [2.31]	0.016 [1.49]	0.016* [1.66]	0.016 [0.96]	0.023* [1.80]	0.013 [0.89]
$\log N \log K$	–0.007 [0.65]	0.007 [0.27]	0.01 [0.71]	0.037 [0.82]	–0.023 [1.53]	–0.01 [0.32]
Investment in ICT (dummy: 1 = yes)	0.144*** [6.66]	0.09*** [2.81]	0.123*** [5.19]	0.049 [1.34]	0.18*** [4.34]	0.162*** [2.76]
State of technology (index: 1 = state-of-the-art; 5 = obsolescent)	–0.11*** [7.28]	–0.093*** [4.07]	–0.105*** [6.19]	–0.091*** [3.39]	–0.107*** [3.89]	–0.092** [2.20]
Part-time workers (percentage)	–0.904*** [9.39]	–0.963*** [6.46]	–1.596*** [11.59]	–1.769*** [6.02]	–0.557*** [4.58]	–0.812*** [4.78]

Apprentices (percentage)	–0.759*** [5.48]	–0.732*** [2.80]	–0.939*** [6.18]	–1.12*** [3.69]	–0.418 [1.55]	–0.086 [0.17]
Skilled workers (percentage)	0.253*** [4.99]	0.295*** [3.64]	0.214*** [3.40]	0.331*** [2.77]	0.235*** [2.81]	0.264** [2.34]
Collective agreement (dummy: 1 = yes)	0.062** [2.42]	0.068* [1.80]	0.046* [1.73]	0.061 [1.36]	0.086* [1.67]	0.08 [1.16]
Constant	11.685*** [39.25]	13.933*** [8.14]	12.052*** [39.49]	15.448*** [7.12]	11.937*** [24.23]	13.144*** [4.30]
Output elasticities at sample means						
ε_{YN} *p*-value	0.881 [0.000]	0.900 [0.000]	0.941 [0.000]	0.904 [0.000]	0.793 [0.000]	0.860 0.000
ε_{YK} *p*-value	0.118 [0.000]	0.091 [0.000]	0.095 [0.000]	0.082 [0.000]	0.142 [0.000]	0.104 0.000
No. of observations	5622	1949	3523	1198	2099	751
No. of plants	2564	895	1696	593	868	302
R^2	0.87	0.64	0.90	0.58	0.82	0.71

Notes: Regressions also include sector and year dummies.

***, **, * denote significance at the .01,. 05, and .10 levels, respectively.

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The results would be more convincing if they were replicated in the case of firms with 21 to 100 employees, where both types of workplace are well represented and where the formal rights of the works council do not vary with establishment size. Results for this sub-sample of establishments are reported in columns 2, 4, and 6 of Tables 2, 3, and 4. Compared with the findings earlier reported for the all-establishment sample, the point estimates for the works council effect are much lower – and even negative for manufacturing establishments in western Germany – and they are either insignificant or only marginally significant for four out of nine cases. If we look at the two regions and the two broad sectors, the estimated coefficients for the works council dummy variable are positive and significant at the .05 level or better only among establishments in the services sector.

It might be objected that slicing up the sample in this way is bound to produce statistically insignificant estimates. We therefore sought to determine whether the results fall apart significantly more than one would expect based on pure sampling error. Our test amounted to determining whether the results for the sub-sample of 21 to 100 employees were significantly different from the rest. In all cases, F-tests rejected aggregation; that is, an interaction term of works council presence and establishment size interval was statistically significant, and the production functions differed between the samples.

As a further test of robustness and stability, results for repeated cross sections of the data are given in Table 5. Although the plant samples are not identical because of panel attritions and accessions, had the sometimes quite large estimates from the pooled data indicated true productivity differentials we might have expected the works council coefficients to be of a similar order of magnitude across each of the four cross sections. But the point estimates are not only often statistically insignificant at conventional levels but also rather volatile from year to year. Looking at service-sector establishments, for example, we observe that the estimated works council effects are statistically insignificant in three out of four years in western and eastern Germany, while they vary from –7.2 percent to 36 percent in western Germany and lie between 7.5 and 27.4 percent for eastern Germany. This sensitivity again cautions against uncritical interpretation of standard production function estimates.

We turn now to the fixed effects estimates which are reported in Table 6. Other than controlling for unobserved time-invariant plant heterogeneity, the regressions are based on the same specifications as used earlier.[16] The coefficient estimate for the works council variable is always positive, but has shrunk considerably. It now ranges between 1.4 and 5.7 percent. Moreover, that influence is poorly determined: it is (weakly) statistically significant in just one

16 We could not pursue each of the sample stratifications adopted in the OLS estimations because the number of plants changing their works council status (and which identify the parameter estimate) would then be too small.

Table 5

Works Council Effects on Productivity from OLS Regressions of a Translog Production Function, Individual Years

			1997	1998	1999	2000	Pooled
Germany	Total	$N \geq 5$	0.259*** [5.76] 2024	0.235*** [5.92] 2520	0.207*** [5.82] 2969	0.238*** [7.35] 3952	0.232*** [8.88] 11465
		$100 \geq N \geq 21$	0.123* [1.86] 546	0.107* [1.94]* 734	0.056 [1.16] 942	0.172*** [3.60] 1373	0.118*** [3.22] 3595
	Manu-facturing	$N \geq 5$	0.305*** [5.31] 1204	0.172*** [3.55] 1497	0.131*** [3.31] 1807	0.161*** [4.05] 2362	0.177*** [5.71] 6870
		$100 \geq N \geq 21$	0.119* [1.73] 291	0.063 [0.94] 394	–0.011 [–0.18] 549	0.07 [1.22] 753	0.046 [1.03] 1987
	Services	$N \geq 5$	0.212*** [2.88] 820	0.283*** [4.46] 1023	0.288*** [4.60] 1162	0.291*** [5.56] 1590	0.275*** [6.31] 4595
		$100 \geq N \geq 21$	0.098 [0.82] 255	0.145 [1.61] 340	0.139* [1.75] 393	0.269*** [3.41] 620	0.183*** [3.05] 1608
Western Germany	Total	$N \geq 5$	0.2*** [2.83] 1003	0.133** [2.02] 1234	0.227*** [3.69] 1373	0.229*** [5.15] 2233	0.206*** [5.21] 5843
		$100 \geq N \geq 21$	0.083 [0.79] 227	–0.039 [–0.43] 310	0.074 [0.83] 367	0.177*** [2.64] 742	0.098* [1.80] 1646
	Manu-facturing	$N \geq 5$	0.242*** [3.16] 615	0.049 [0.73] 727	0.101 [1.52] 805	0.102* [1.89] 1200	0.113*** [2.59] 3347
		$100 \geq N \geq 21$	0.073 [0.58] 116	–0.055 [–0.54] 155	–0.111 [–1.06] 190	–0.038 [–0.47] 328	–0.042 [–0.71] 789
	Services	$N \geq 5$	0.191* [1.68] 388	0.203* [1.82] 507	0.32*** [3.03] 568	0.306*** [4.45] 1033	0.27*** [4.14] 2496
		$100 \geq N \geq 21$	0.076 [0.37] 111	–0.075 [–0.45] 155	0.215 [1.39] 177	0.311*** [3.05] 414	0.186** [2.04] 857

Continued Table 5

			1997	1998	1999	2000	Pooled
Eastern Germany	Total	$N \geq 5$	0.305*** [5.13] 1021	0.304*** [6.18] 1286	0.202*** [4.83] 1596	0.277*** [5.90] 1719	0.266*** [7.77] 5622
		$100 \geq N \geq 21$	0.17* [1.88] 319	0.198*** [2.75] 424	0.047 [0.81] 575	0.171** [2.54] 631	0.137*** [2.84] 1949
	Manu-facturing	$N \geq 5$	0.338*** [4.43] 589	0.234*** [3.62] 770	0.149*** [2.96] 1002	0.233*** [3.93] 1162	0.222*** [5.25] 3523
		$100 \geq N \geq 21$	0.164* [1.89] 175	0.142* [1.69] 239	0.038 [0.52] 359	0.184** [2.21] 425	0.111* [1.85] 1198
	Services	$N \geq 5$	0.253** [2.54] 432	0.361*** [4.76] 516	0.241*** [3.38] 594	0.3*** [3.93] 557	0.294*** [5.17] 2099
		$100 \geq N \geq 21$	0.11 [0.61] 144	0.242** [2.09] 185	0.072 [0.80] 216	0.13 [1.12] 206	0.154** [1.98] 75

Notes: Each cell is from a separate regression. The cell entries give coefficient estimate, t-value, and number of observations in the regression. Dependent variable and explanatory variables as in Tables 2 through 4.

case. The suggestion is, then, that the large coefficients found in the OLS estimations are (at least partly) due to unobserved plant heterogeneity. On the other hand, the downside of the fixed effects estimates is that they rely on a small number of plants that experienced works council formation or dissolution. (There is also the issue that fixed effects typically elevate the importance of measurement error that will bias the estimate towards zero.)

We should also note that the output elasticity with respect to employment is 49 percent for the full sample, just half the value reported in the corresponding OLS estimates (see Tables 2 through 4). It is larger in manufacturing than in services and, somewhat surprisingly, it is also larger for eastern than for western Germany. The estimate for the output elasticity with respect to capital is always near zero and poorly determined in five out of the six regressions. On the other hand, the coefficient estimates of the variables capturing the quality of the capital stock have the expected signs (i.e. we find a positive impact of investment in ICT and of a plant's technology on productivity) and are mainly well determined. For their part, the coefficient estimates of the control variables are statistically zero in all but one instance. Their insignificance may reflect insufficient variation in the respective variables as well as measurement error (most relevant perhaps in the case of the capital stock). As a result, the within-plant variation in employment accounts for almost all of the explained within-plant variation in output.

Our third empirical strategy differs from the first two in tackling productivity *growth,* namely, changes in sales 1997 – 2000.[17] Results of our first-difference approach are reported in Table 7. Note that establishments without a works council in both the starting year (1997) and in the end year (2000) form the reference group. It can be seen from the first row of the table that sales in works council plants grew neither more quickly nor more slowly than in counterpart establishments without councils. None of the coefficient estimates for the works council dummy is statistically different from zero at conventional levels.

The output elasticity with respect to employment has shrunk (as compared to the OLS estimates) and amounts on average to 64 percent, while the coefficient estimate for the change in the capital stock is positive and statistically significant in just two out of six models.[18] Also similar to the fixed effects results, the coefficient estimates of the control variables are insignificant in almost all cases, but see above for caveats on this finding. We should finally note that controlling for other factors, the average growth in output is found to be larger for western Germany. The bottom line of this exercise is that the works council has neither a positive nor a negative effect on productivity growth.

In summary, we have investigated works councils' productivity effects from three different perspectives. Contrary to some recent findings of sturdy pro-productive effects of works councils, our own estimates suggest that their effects on plant productivity and productivity growth appear to be more or less neutral.

[17] Note that we cannot on this occasion further disaggregate by manufacturing and services for the two broad regions due to the small number of cases.

[18] Strictly speaking, the output elasticities from the fixed effects regressions and from the model in first differences imply decreasing returns to scale. This result follows directly from the finding that changes in the capital stock have virtually no effect on output. That said, the elasticity with respect to employment conforms to what is typically found in studies using aggregate data on employment and the capital stock.

Table 6: **Fixed Effects Regressions of a Translog Production Function**

(1997 – 2000; dependent variable: total sales $(\log Y)$)

Sample	All	$100 \geq N \geq 21$	West	East	Manufacturing	Services
	(1)	(2)	(3)	(4)	(5)	(6)
Works council (dummy: 1 = yes)	0.032	0.056	0.014	0.032	0.045	0.025
	[1.28]	[1.82]*	[0.42]	[0.88]	[1.31]	[0.71]
Number of employees $(\log N)$	0.522	-0.017	0.441	0.36	0.655	0.395
	[9.68]***	[0.04]	[6.34]***	[4.16]***	[8.73]***	[4.83]***
Capital Stock $(\log K)$	0.011	0.017	0.006	0.016	0.037	–0.009
	[0.49]	[0.28]	[0.17]	[–0.52]	[1.13]	[0.29]
$(\log N)^2/2$	0.010	0.196	0.002	0.066	-0.014	0.021
	[0.80]	[1.66]*	[0.13]	[3.18]***	[0.92]	[1.02]
$(\log K)^2/2$	0.001	0.005	0.002	0.000	–0.002	0.003
	[0.51]	[1.18]	[0.51]	[–0.14]	[0.42]	[1.04]
$\log N \log K$	–0.006	–0.015	–0.007	–0.002	–0.003	–0.007
	[1.69]*	[1.24]	[1.54]	[-0.3]	[0.57]	[1.60]
Investment in ICT (dummy: 1 = yes)	0.028	0.007	0.034	0.021	0.033	0.019
	[3.73]***	[0.61]	[3.65]***	[1.87]*	[3.29]***	[1.70]*
State of technology (index: 1 = state-of-the-art; 5 = obsolescent)	–0.014	–0.016	–0.014	–0.014	–0.011	–0.019
	[2.64]***	[1.81]*	[2.18]**	[1.67]*	[1.41]	[2.43]**
Part-time workers (percentage)	–0.056	–0.078	–0.050	–0.037	–0.032	–0.061
	[1.47]	[1.11]	[1.08]	[–0.63]	[0.49]	[1.34]

Apprentices (percentage)	–0.016	0.166	–0.190	0.077	–0.036	–0.011
	[0.19]	[0.99]	[1.60]	[-0.65]	[0.31]	[0.09]
Skilled workers (percentage)	0.004	0.000	0.052	–0.042	–0.022	0.023
	[0.21]	[0.00]	[2.22]**	[–1.38]	[0.75]	[0.90]
Collective Agreement (dummy: 1 = yes)	–0.003	–0.019	–0.015	–0.002	–0.013	0.017
	[0.26]	[1.11]	[0.92]	[-0.1]	[0.88]	[0.96]
Constant	14.115	14.711	14.977	13.817	13.655	14.493
	[93.29]***	[15.61]***	[68.70]***	[62.06]***	[63.30]***	[67.89]***
Output elasticities at sample means						
ε_{YN}	0.491	0.541	0.358	0.596	0.556	0.390
p-value	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
ε_{YK}	0.003	0.016	0.000	.004	.003	.004
p-value	[0.38]	[0.01]	[0.98]	[0.43]	[0.62]	[0.39]
No. of observations	11465	3595	5843	5622	6870	4595
No. of plants	5684	1839	3120	2564	3413	2271
No. of plants with works council formation / dissolution	117	62	46	71		53
R^2	0.13	0.11	0.12	0.16	0.16	0.09

Regressions also include year dummies.

***, **, * denote significance at the .01,. 05, and .10 levels, respectively.

Table 7: **OLS Regressions of a Cobb-Douglas Production Function in Differences**
(1997 – 2000; dependent variable: Δ log total sales)

	Total		Western Germany		Eastern Germany	
	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$	$N \geq 5$	$100 \geq N \geq 21$
	(1)	(2)	(3)	(4)	(5)	(6)
Works council (dummy) 1997: yes; 2000: yes	0.013 [0.46]	–0.011 [0.25]	0.053 [1.43]	–3.0e – 4 [0.01]	–0.010 [0.23]	0.012 [0.16]
Number of employees ($\log N$)	0.639*** [6.87]	0.553*** [5.84]	0.531*** [4.42]	0.301* [1.76]	0.718*** [5.98]	0.638*** [5.06]
Capital stock ($\Delta \log K$)	0.014 [0.91]	0.081*** [3.73]	0.011 [0.33]	0.081 [1.40]	0.015 [0.74]	0.082*** [3.29]
Investment in ICT (4-year-average of dummy)	0.049 [1.14]	0.062 [0.94]	0.033 [0.62]	–0.021 [0.22]	0.047 [0.73]	0.060 [0.61]
State of technology (4-year-average of index)	0.005 [0.20]	0.028 [0.93]	–0.052** [2.35]	0.009 [0.23]	0.047 [1.12]	0.041 [0.81]
Part-time workers (Δ percentage)	0.134 [0.61]	–0.173 [1.11]	0.049 [0.36]	0.258 [0.96]	0.243 [0.59]	–0.508** [2.17]
Apprentices (Δ percentage)	–0.186 [0.64]	–0.392 [1.02]	–0.323 [0.71]	0.085 [0.12]	–0.185 [0.48]	–0.567 [1.02]
Skilled workers (Δ percentage)	0.062 [0.69]	0.079 [1.24]	0.081 [1.23]	0.155* [1.70]	0.085 [0.55]	0.059 [0.62]

Collective agreement (dummy)	–0.003	–0.059	–0.071	–0.208**	0.039	–0.007
1997: yes; 2000: yes	[0.09]	[1.10]	[1.31]	[2.56]	[0.81]	[0.10]
1997: no; 2000: yes	–0.087	–0.228**	–0.097	0.003	–0.066	–0.271**
	[1.30]	[2.00]	[0.83]	[0.01]	[0.77]	[2.06]
1997: yes; 2000: no	0.037	0.054	-0.079	-0.072	0.078	0.103
	[0.73]	[0.90]	[1.02]	[0.67]	[1.20]	[1.33]
Dummy eastern Germany	–0.070***	–0.061				
	[2.75]	[1.61]				
Constant	–0.133	0.036	–0.265**	0.050	–0.217	–0.097
	[0.66]	[0.30]	[2.52]	[0.34]	[0.85]	[0.49]
No. of observations	1063	299	492	121	571	178
R^2	0.31	0.34	0.30	0.42	0.35	0.41

Notes: Δ denotes the difference between 2000 and 1997 of the respective variable. Plants which changed their works council regime have been excluded from the analysis. The regressions also include sector dummies. ***, **, * denote significance at the .01, .05, and .10 levels, respectively.

6. Conclusions

We are now well into the third stage of research charting the effects of German works councils on firm performance. The evidence provided here suggests that harsh dismissal of the institution based on rent seeking considerations or slowed decision making is just as misplaced as the view that sees the works council as a productivity cure-all while also delivering on industrial democracy. Arguably, the former position was commonplace by the end of the first stage of economic research on work councils, while the latter has recently been gaining currency as a result of some very optimistic estimates of works council impacts using the IAB data.

In addressing works council effects on productivity, 1997–2000, we pursued three estimation strategies. The first two approaches sought to recoup the works council effect directly by estimating works-council-augmented translog production functions, using both OLS and fixed effects methods. The third approach offered a modified first difference model linking productivity growth to works council presence.

For a sample comprising all establishments, the results of the first exercise pointed to positive and well-defined works council 'effects' on plant productivity of around 25 percent. These estimates are broadly in line with those reported in recent studies using the same data set. But neither takes account of differences in works council type or in works council coverage, both of which are influenced by establishment size. Since productivity will also be related to establishment size where there are economies of scale, there is the possibility that this productivity differential may be an artefact of the data. As a partial solution to this problem, given the lack of plausible identifying instruments for works council presence and no independent information on works council type, we presented results for a sub-sample of plants with between 21 and 100 workers. Over this employment range, the legal powers of the council are a datum – so that to this extent we are standardizing on works council type – and works councils plants are well represented (around one-third of the sample). Had the all-establishment results carried over to this sub-sample, they would be compelling. But in fact the coefficient estimates for the works council dummy fell sharply, were actually negative albeit statistically insignificant for the manufacturing sector in western Germany, and were statistically insignificant in three out of four of the cross sections. In addition, when controlling for time-invariant unobserved plant heterogeneity by fixed effects – our second test procedure – the positive impact of works councils on productivity almost vanished and was insignificant in all but one case. Further, the results of our third test based on changes in productivity, as well as the recent production frontier estimates by Schank, Schnabel, and Wagner (2004), support the notion that there are likely to be few differences on average between plants with works councils and plants without them.

The implication that there are no material works council effects *on average* is not unimportant in its own right given the history of German research in this area. Thus, for example, it may suggest that the pessimistic findings of the early 'first stage' empirical literature, as we have termed it, were overdrawn. But for the future it will be necessary to look at what lies behind these average effects. In this connection, we might usefully note that in re-estimating their early production function study which had pointed to sharply lower total factor productivity in works council firms, FitzRoy and Kraft (1987, 1995) were to report that such effects could be undone by profit sharing. It will be interesting to see if this potential for productivity improvement is also discernible in the IAB panel when future waves containing more data points on profit sharing and other high performance work practices become available.

In some sense, our results and insights mirror those obtained by empirical studies on union productivity effects. Almost three decades ago Brown and Medoff (1978) reported that worker representation in unions was associated with a large positive productivity differential of 25 to 27 percent – even more (35 percent) if the productivity differential of unionized establishments derived solely from labor inputs. But, to quote Hirsch (2004, 445), "subsequent evidence suggests an average union productivity effect near zero and at most modestly positive." In Germany, by contrast, estimates of the effect on productivity of worker representation in work councils have progressed from negative values in the early literature to Brown-Medoff levels in some of the more recent studies employing nationally representative data. Our own findings based on fixed effect and first difference estimators, and controlling for works council heterogeneity, seem much closer to Hirsch than to Brown and Medoff. The finding of small to insignificant works council effects is nontrivial in view of the heated debates on unions and codetermination alike. To repeat: the findings presented here direct us to inquire more closely into the black box of potential productivity augmenting mechanisms in works council (and of course non-works council) regimes.

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