Mandatory Works Councils in Germany: Their Effects on Productivity and Profits

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Introduction

1.1 The Employee Representation Debate

Should employees be involved in establishment-level decision-making? If yes, to what extent? Should such participation be a legal right of employees or should it be left open to the individual firm to negotiate this question with its employees? What are the economic consequences of legally-based formal employee representation, e.g. via mandatory works councils?

The political debate on the usefulness of non-union employee representation has been going on for decades in many industrialized countries. In Germany, the amendment of the Works Constitution Act in the year 2001 aims to reduce a "representation-free zone" in the economy. In 1994, the European Union paved the way for European works councils in multi-national firms.[1] Also in the mid-nineties, a high-ranking commission (the so called Dunlop Commission) was initiated by the U.S. government to examine what

[1] See Kotthoff (2006) for an evaluation of this policy.

can be learned from European-style employee representation (see U.S. Department of Labor (1994)).

While proponents of employee representation also use non-economic arguments like workplace democracy and high-quality labor relations and see employee representation as an important counterweight to a pure shareholder value approach or even as a basic political right (Wever (1994)), its opponents predict damaging economic consequences such as efficiency losses and rent-seeking behavior if employee representation is a legal right of workers. However, in the scientific debate there is considerable uncertainty about whether mandated works councils, which are the most common institutions that organize legally-based employee representation at the establishment level, indeed reduce productivity and decrease profits. In this dissertation, the two latter questions are examined empirically for Germany.

Before starting with theoretical arguments on the economic consequences of mandatory works councils, one has to explain why works councils have to be mandated. If works councils are not forbidden[2] and if they, nevertheless, do not evolve in absence of a legal mandate – why should they have desirable economic consequences? The classical arguments against a legal mandate for works councils can be found in Jensen and Meckling (1979). Jensen and Meckling (1979, 472) argue that "firms are free to write any kind of contracts they wish with their employees" and, consequently, they could voluntarily give their workers any right that a legislation could give to a works council. Jensen and Meckling (1979, 473) assume that firms would offer such contracts "if the benefits exceeded the costs". From this statement

[2]They are forbidden, for instance, in the United States.

and from the assumption that workers value the existence of a works council, they conclude that the fact that virtually no works council exists in absence of a legal mandate is a strong case for the inefficiency of employee participation. Hence, they derive a conclusion about efficiency from a statement about "benefits and costs", which is a statement about profits. Thus, an implicit assumption of Jensen and Meckling (1979) is that a profit-decreasing institution can not be efficient.

By contrast, Freeman and Lazear (1995) develop a model where works councils are able to increase productivity and, at the same time, decrease profits. In their model, Freeman and Lazear (1995) argue that the creation and the distribution of economic rents can not be decoupled in case of works council presence. The reasoning is that works councils can only have economic effects if they have rights but as soon as they have rights they could use them for rent-seeking purposes as well. Following this assumption, a works council affects productivity and the distribution of rents. They assume that works councils are able to increase productivity but also increase workers' share in total firm surplus and, hence, may decrease profits. Based on this assumption, they show that even if works councils were the most efficient solution, managers and workers would not agree on the optimal power of such councils. They argue further that fixed costs of councils could even completely hinder their foundation although the existence of councils would be efficient. Hence, if works councils have some rent-seeking and redistributive power, the introduction of efficiency-increasing councils may not be optimal for employers and this explains why councils do not evolve voluntarily. As a result, as councils are assumed to be efficiency-increasing, a legal mandate

for them increases social surplus.[3]

In many countries, mandated works councils have the legal right to be informed and to be heard by the management. In Germany, they additionally have veto rights and codetermination rights on some issues. Given mandated works councils, what are their effects on productivity and the distribution of rents? Do they increase or decrease productivity and profits? Or do they increase productivity but redistribute rents to an extent that decreases profits? These are the central questions I address in this dissertation.

It is hardly controversial that works councils induce costs: informing them takes time, consulting them delays decision-making, and codetermination or veto rights may lead to inefficient solutions. The first questions to answer are whether there are also positive productivity effects and whether they outweigh the costs and disadvantages councils cause.

There are numerous economic arguments for positive productivity effects of good firm-labor relations that can be achieved by works councils. These arguments are classified as follows into two main categories – trustful communication and longer-term relations.

In the first place, more frequent and more **trustful communication** is attributed to works councils. While more frequent information exchange between managers and workers may improve efficiency to some extent, the more interesting point is trustful communication. Communication goes into two directions; from managers to workers and vice versa. A works council is able to screen information provided by the management and this reduces man-

[3]However, a legal mandate that prescribes councils' rights can not take into account that different firms have a different optimal degree of codetermination and can therefore not maximize social surplus.

agers' possibility of opportunistic behavior and fosters the trust of workers in such information. This point becomes important if the management reports economic difficulties and demands higher worker effort. In absence of works councils, workers would not know whether this is opportunistic behavior or a real threat to the firm's survival. Freeman and Lazear (1995) show why this screening mechanism can be beneficial for both sides.

Another aspect of trustful communication is touched when communication goes in the other direction. If workers have exclusive efficiency-increasing information on an issue, they will not reveal it to managers if the latter cannot credibly assure not to use this information against workers. If works councils have the right to co-determine *how* a certain information is used, they help to overcome this problem. Supporting this view, studies show that innovative work practices like flexible working time (Dilger (2002)) or variable payment schemes (Heywood et al. (1998)) are more likely in firms that have a works council.

Both elements of trustful communication, i.e. screening and codetermination, facilitate the introduction of new technologies: screening may increase acceptance within the work force and the consultation/codetermination process eases optimal implementation.[4]

A second important field of councils' contribution to efficiency refers to **longer-term relations** between workers and firms. The longer-term perspective comes from lower personnel fluctuation,[5] caused by the employment

[4]Wever (1995) reports that also managers see this as a major advantage of works councils.

[5] For example, Addison et al. (2001) and Hirsch et al. (2009) find lower personnel fluctuation.

protection function works councils have, by the higher wages paid in council firms, and by the voice option they provide.[6] Less fluctuation decreases firms' hiring and search costs and leads to repeated interactions that also cuts transaction costs. Longer-term relations are necessary to safeguard specific co-investments like, e.g., investments in firm-specific human capital[7] and some authors therefore argue that the German works council system pushes firms to adopt competitive strategies that rely on high skills, high technology, and high labor productivity (Sorge and Streeck (1988)).

As they are employee associations, it is typically presumed that works councils engage in rent-seeking activities. Consequently, if they have no positive effect on productivity, councils are expected to decrease profits. If there are productivity gains through councils, the councils' effect on profits depends on the extent of rent-seeking. The extent of rent-seeking depends on the legal rights councils have with respect to remuneration and on the degree to which distributional conflicts are worked out at the establishment level or at a more aggregated level. In Germany, works councils have no right to call a strike and the wage level is typically negotiated between employer associations and unions at the industry level. Therefore, the rent-seeking possibilities of councils are limited and decrease further if their firm is covered by an industry-level collective bargaining agreement. Nevertheless, councils could use their veto and codetermination rights in other areas to increase the wage level in their establishment.

[6] See Hirsch et al. (2009) for a detailed discussion of the impact of works councils on separations.

[7] Bellmann and Ellguth (2006) find increased training intensity in firms with a works council.

1.2 Empirical Evidence and Shortcomings

It rarely happened in German history that a government introduced or fostered a mandate for works councils to improve productivity.[8] Works council legislations fostering councils have rather been politico-economic decisions to, e.g., canalize radical worker movements in the Weimar Republic, to weaken powerful unions after World War II, or to accompany a general policy of social reforms as in the 1970s.[9] Against that background, it is little surprising that until the 1980s the productivity (and profitability) impact of German works councils was widely ignored by the empirical literature. Starting in that decade, the interest in employee-representation rose as union density declined in many industrialized countries. The higher frequency of new empirical studies since the mid 1990ies may have additionally been pandered by the newly available large-scale data sets including information on works councils.

While early studies on the councils' influence on productivity and profits yield rather pessimistic results, more recent studies find positive productivity effects and negative (but insignificant) profit effects.[10] An important difference between early and recent studies is the data base. Until the mid-nineties, only small and non-representative samples existed. After that, the large-scale data sets of the Hannover Panel and the IAB Establishment Panel became available. Another explanation for the more positive results of recent studies may be the improved functioning of councils and a process of adaptation

[8] One exception may be a special law during World War I, providing a mandate for works councils in medium and large sized firms in strategic industries.

[9] For an abstract of the history of German works councils see Müller-Jentsch (1995).

[10] See Addison et al. (2004) for a summary of existing studies.

and increasing acceptance that leads to better cooperation of councils with employers and unions. Underlining these arguments, Kotthoff (1994) finds in his 1990 follow-up study that unions' acceptance of councils is substantially higher than in his 1975 study (which was conducted only three years after the far-reaching 1972 reform of the works council legislation was enacted). In the 1990 study, he further finds that, compared with the situation in 1975, an initially highly ideological confrontation between managers and works councillors turned into more businesslike and professional attitudes on both sides.

The positive productivity effects found in most recent studies, weakens the position of the councils' opponents. However, the empirical evidence suffers from serious shortcomings. A first shortcoming is that the data includes no information on physical capital stock. This has two implications: productivity estimates can only be interpreted with the reservation that capital does not matter and researchers are not able to account for substitution processes between the input factors capital and labor.

The second shortcoming refers to the endogeneity of works council existence. Few studies attempt to control for the self-selection mechanisms that lead to the observed situations of having a council or having no council.[11] If the workers of more productive firms tend to establish a council, the observed positive correlation of works councils and productivity is not caused by works councils and the councils' effect on productivity is lower than the reported correlation and possibly negative. Hence, although the tendency in results is favorable to councils, the results leave open the possibility that

[11] See Hübler and Jirjahn (2003) for an exception.

councils reduce productivity.

The negative profitability effects are in line with the arguments of council opponents and the standard assumption that employee associations pursue rent-seeking goals. However, both main criticisms of the productivity studies hold also for the profit estimations: first, capital stock is missing and second, if councils are established, e.g., in times of bad profits, the observed negative correlation might cloud a possibly positive causal effect . Additional problems arise due to a lack of information on profits. To explain the actual profit situation of firms, recent studies used self-reported subjective evaluations of managers as dependent variables. Unfortunately, such evaluations depend on the particular respondent and his reference point regarding what a "very good", "good" or "bad" profit is and do therefore not necessarily reflect actual profits. As the estimated negative effect is insignificant in several profit studies, doubts on whether having estimated the correct sign of the causal effect or not are even larger than with respect to the productivity estimates.

1.3 Main Results of this Dissertation

This dissertation consists of three self-explaining essays. A new method for capital stock approximation is proposed in the first paper. Using this method, the two other papers attempt to assess the productivity effect and the profitability effect of works councils, respectively.

1.3.1 Capital Stock Approximation with Short Panels

Recognizing the problem of no direct information on capital stock in German large-scale establishment data sets, this study proposes an intuitive solution. The basic idea is to apply the well known perpetual inventory approach and to construct a starting value for that approach from averages of replacement investments. In a preparatory step, data on average economic lives of capital goods is used to approximate depreciation rates.

The method is then tested by re-estimating the paper by Addison et al. (2006) that examines the productivity effect of works councils. Using simple OLS techniques, the results with respect to the output elasticity of capital are slightly better with the new method. The advantage becomes substantial when panel estimators are used. The reason is that the new method works much better in the within-firm dimension of the data. As it induces problems in the between dimension, the new method is the most appropriate if panel estimators are used.

1.3.2 The Productivity Effect of Non-Union Representation

This paper estimates a production function to assess the sign of the causal effect of works councils on establishment productivity. In contrast to previous studies, capital stock is controlled for. Further, the simultaneity of the output and input decisions is addressed using a GMM-SYS estimator. Residuals from this regressions are used to estimate the council effect. I find a positive

effect of 6.5 percent and show that this is a downward-biased estimate of the true council effect. The use of an endogenous switching regression model and the construction of instruments from linked employer-employee data allows such a causal interpretation.

1.3.3 Are the Firm Owners really worse off with a Works Council?

A positive productive effect is only in line with a negative effect on profits if the amount of rents, that is redistributed from firm owners to workers, exceeds the additional rents from the productivity increase.

Applying two self-constructed objective measures and a frequently used subjective profit measure, I reproduce previous results for the subjective measure and find that the objective measures are more adequate approximations for actual profits. The councils' effect on quasi rents (i.e. value added minus labor costs) as an objective measure of profits is clearly positive in firms that are covered by a collective bargaining agreement and close to zero otherwise. Surprisingly, and in contradiction to approaches that leave aside the implications of the duality of unions and councils in Germany, I find that labor's share in total rents *decreases* in case of the joint existence of a works council and collective bargaining. Obviously, councils are not able to increase labor's share as remarkably as they increase productivity in that large group of firms.

Chapter 2

Capital Stock Approximation with Short Panels

2.1 Introduction

In recent years new establishment panel surveys have been created. For Germany, these are e.g. the establishment panel of the IAB,[1] the Mannheim Innovation Panel (both since 1993) and the Hannover Panel (since 1994).[2] Establishment panel data has the advantage to provide detailed micro data in a panel structure and thus is an excellent base for working on some of the most interesting questions in labor economics. Some of these questions are:

- What are the determinants of firm-level labor demand?

[1] Institute for Employment Research of the Federal Labor Service in Germany (Institut fuer Arbeitsmarkt und Berufsforschung der Bundesagentur fuer Arbeit)

[2] Descriptions of these data sets are provided in the European Data Watch Series published in the Journal of Applied Social Science Studies (Schmollers Jahrbuch) and available for download on the sites of the German Council for Social and Economic Data (www.ratswd.de).

- What is the influence of institutional regularities (works councils, collective agreements) on productivity?
- What are direction and extent of the substitution relations between capital and (different kinds of) labor?
- Is there any skill biased technological change?

To answer these questions, the parameters of the firm's production or cost function (or of the derived factor demand function) have to be estimated. One requirement for estimating production or cost functions is the correct measurement of inputs. The major difficulty here is to measure capital input, because in many establishment panel data sets, e.g. in those mentioned above, direct information on capital stock is unavailable. Measurement errors in capital stock will lead to biased estimates and any inference based on such estimates could be misleading. This paper provides a method that, in comparison to previous studies, improves the approximation of capital stock substantially for the case that no direct information on capital stock is given and that the time-series dimension is short.

Many researchers use the investment expenditures of firms to approximate the unknown capital stock, i.e. investment expenditures are inserted directly into the production or cost function. Depending on the data, a variety of specifications is used. It is common to apply all available information about total investments (van Reenen (1997)), only the replacements of the current year (Bellmann and Schank (2000)), or the sum of the last two years (Addison et al. (2006)). All these attempts entail the implicit assumption that investment expenditures reflect the necessary depreciation. If this is given,

they are proportional to the unknown capital stock and control for the capital effect when estimating production functions, cost functions or conditional factor demand functions. I call this approach the *proportionality approach*. The idea behind the proportionality approach is that depreciations are proportional to the capital stock, assuming a linear depreciation rate. Given that replacement investments are undertaken to replace depreciated capital, they are proportional to capital, too. Obviously, the proportionality assumption can only be fulfilled if replacement investments are used instead of total investments or net investments. In section 2.3 the problems that arise with the proportionality approach are investigated:

1. If the proportionality assumption holds, the approximated capital stock (the replacement investment expenditures) is proportional - with an unknown factor of proportionality - to the real capital stock. Adding net investments (reported in absolute value) to the replacement investments is not possible because this would destroy the proportionality. Thus, information on net investments can not be used to approximate the capital stock.

2. The implicit assumption that replacements equal the unknown depreciation and are therefore proportional to the capital stock cannot be verified by data. Huge variations in the investment expenditures of a firm will lead to implausible variations in the (by assumption) proportional capital stock variable, e.g. zero investments in one year would cause a capital stock measure of zero for that year. This could cause measurement errors and a considerable attenuation bias.

To avoid these drawbacks, a method is necessary that provides absolute values of capital stock and does not rely on the proportionality assumption of investments and capital stock. Perpetual inventory, i.e. continued adjustment of an inventory value by balancing inflows with outflows, is such an approach. In this paper, it is modified to be feasible in short panels, too. Modified perpetual inventory consists of two steps with the first step as the *modification*. First, the *absolute* value of capital stock is computed, using the proportionality approach and information about average economic lives of capital goods. To provide a reliable alternative to the inventory value, these single capital stock values are than averaged to smooth variations caused by the investment expenditures they are based on. Second, traditional perpetual inventory is started from this average. To apply the new method, only little information is necessary.

This paper is organized as follows: in section 2.2 the results of recent studies that use direct information on capital stock are compared with the results of analyses lacking this information and applying the proportionality approach. In section 2.3, the substantial differences found in section 2.2 are traced back to the problems of the proportionality approach and as a solution the modified perpetual inventory approach is proposed. In section 2.4, the necessary information and assumptions for implementation are sketched. The proposed method is used to replicate a recent paper that applied the proportionality approach and the results are compared in section 2.5. The main conclusions are summarized in the final section.

2.2 Literature

In this section, the results of studies with direct information on capital stock are compared with studies lacking this information. The latter applied the proportionality approach. The idea is to take the results of studies with information on capital stock as a benchmark and to see whether the proportionality approach is able to produce similar results without the direct capital stock information. The central measure for comparison is the output elasticity with respect to capital ($\epsilon_{Y,K}$) because:

- this elasticity is a standard measure,
- it is reported in nearly any production function estimation and
- it is a capital related result that, conditional on the fact that the proportionality assumption is fulfilled, is estimated correctly with the proportionality approach.

In general, the analyses without capital stock information listed in table 2.1 report lower $\epsilon_{Y,K}$, e.g. because of measurement errors in the capital variables due to simplifying assumptions made in the proportionality approach.[3]

For all studies the fixed effects estimation results are lower than the OLS results.[4] One interpretation is that, as the variance in the capital variable is reduced in the within estimation, other misspecifications overwhelm the

[3]It is possible, of course, that the proportionality approach estimates are correct and the others are biased upwards. Note, that for industry or country level data these elasticities were usually estimated between 0.2 to 0.3, too (see, e.g., Levy (1994) or Hsing (1996)).

[4]In table 2.1, I listed no study that deals with the problem of simultaneity (see, for example, Olley and Pakes (1996)). The reason is that no analysis was found that approaches the simultaneity problem *and* uses the proportionality approach. For the sake of comparability, the selected studies with information on capital stock do neither.

remaining signal in the data. Besides others, the lack of information on time-varying variables, e.g. on output prices or on capacity utilization for capital and labor, is a possible misspecification. Another interpretation is that an existing measurement error problem increases in the case of within estimation if the serial correlation in the measurement error is lower than the one in the true capital stock.[5] Given the empirical results of this paper it is suggested that the major reason for the sharp downfall in $\epsilon_{Y,K}$ found with the proportionality approach and illustrated in table 2.1 is the inaccurate measurement of the capital stock.

A convincing production function estimation can only be done if inputs and output are measured correctly. Presumably, there exist only so few production function estimations based on establishment panels without direct information on capital stock *because* the capital stock information is missing and existing capital stock approximation methods are not reliable. A reliable approximation method for the capital input would make a considerable number of recently established, rich firm-level panel data sets accessible for more sophisticated studies in this field.

2.3 The Modified Perpetual Inventory Approach

This section starts with an inspection of the two major problems of the proportionality approach and concludes with the modified perpetual inventory approach as the solution to this problems.

[5] See, for instance, Bound et al. (2001).

Table 2.1: Previous studies with different capital stock approximation methods using firm-level panel data

Direct information on capital stock			
Authors	Description	Data	Results
Mairesse and Griliches (1990)	Estimation of Production Function Parameters, Cobb Douglas Production Function	Manufacturing firms from France, US and Japan	OLS: $\epsilon_{Y,K}$ between 0.19 to 0.27
Griliches and Mairesse (1998)	Estimation of Production Function Parameters, Cobb Douglas Production Function	French manufacturing firms	OLS: $\epsilon_{Y,K} = 0.3$ FE: $\epsilon_{Y,K} = 0.15$
Mairesse and Jaumandreu (2005)	Estimation of Production Function Parameters, Cobb Douglas Production Function	Data on Spanish and French manufacturing firms	OLS: $\epsilon_{Y,K}$ between 0.22 and 0.25 FE: $\epsilon_{Y,K}$ between 0.06 and 0.11
Proportionality Approach			
Frick and Möller (2003)	Impact of works councils on firm performance using various production functions, Replacements of the current year as capital input	IAB Establishment Panel	OLS: $\epsilon_{Y,K} = 0.012$
Addison et al. (2006)	Impact of works councils on firm performance, Translog production function, Replacement of two previous years as capital input	IAB Establishment Panel	OLS: $\epsilon_{Y,K} = 0.122$ FE $\epsilon_{Y,K} = 0.003$

Notes: OLS = Ordinary least squares, FE = fixed effects (within) estimation, $\epsilon_{Y,K}$ = output elasticity with respect to capital

2.3.1 Proportionality

A first drawback of working only with a proportional value is that information about the absolute value of the net investment cannot be taken into consideration. Adding net investments to the replacement investment expenditures would obviously destroy the assumed proportionality. The second problem is that if this proportional value is used to estimate cost- or production function parameters or e.g. to derive estimates from conditional factor demand functions, then the estimates for capital stock will be biased upwards.[6] Assuming linear depreciation, the unknown factor of proportionality with respect to the absolute value of capital stock is the average depreciation rate. This information is not given in many establishment data sets.

One promising way to estimate it is to use industry level information. In this paper, data from the German Federal Statistical Office (Statistisches Bundesamt) is used. It provides the composition of each industry capital stock into structures and equipment (Statistisches Bundesamt (2008a)) and data on the average economic lives of structures and equipment[7] (the reciprocal of the linear depreciation rate). Combining both by constructing the harmonic mean of the economic lives of structures and equipment gives the reciprocal of the average linear depreciation rate in a given industry:

$$\frac{1}{DR} = \frac{K_{total}}{(K_{structure}/L_{structure}) + (K_{equipment}/L_{equipment})}, \tag{2.1}$$

with K for the capital stock, L for average economic life and DR for the

[6] Of course they are not biased if a logarithmic functional form is used for estimation.
[7] Available on request from the German Federal Statistical Office.

depreciation rate. To combine industry data with firm data, one has to assume that every firm within an industry has an identical composition of capital stock (assumption 1) and thus the same average depreciation rate. For competitive industries, this assumption seems to be realistic because in the long run only those firms with the superior production technology will survive. Non-competitive sectors such as the public sector or non-profit organizations may not tend to fulfill assumption 1 and should be excluded from the sample.

Multiplying replacements with the reciprocal of the average depreciation rate gives the absolute value of the capital stock:

$$K_t = IR_t * \frac{1}{DR} + IN_t \tag{2.2}$$

with IR for replacement investments, t as a time index and IN for net investments, whereas most former studies had to apply proportional values.[8] I call this the augmented proportionality approach.

2.3.2 Moving averages

In the previous subsection, the problem of the unknown factor of proportionality is solved by using industry level data, but the approach so far is still similar to the proportionality approach. This is the case because equation 2.2 depends heavily on the assumption that the reported expenditures for replacement investments equal the unobserved depreciation. One has to assume *that every firm completely replaces its depreciated capital every year*

[8] A similar approach can be found in Ornaghi (2006).

(assumption 2). This is the major problem of the proportionality approach because if the assumption is not valid, the classical measurement error problem occurs[9] and estimates will be attenuated to zero.[10]

Several studies (for instance, Caballero et al. (1995), Doms and Dunne (1998), Power (1998), Nilsen and Schiantarelli (2003)) clearly show that assumption 2 is very unlikely to hold at the establishment level. The authors claim that investments are lumpy and many firms report zero investments for certain years. A capital stock computed with the proportionality approach would be zero if investments are zero. In fact, capital is a rather stable input for most firms. High variations in replacements are thus a clear sign that assumption 2 fails. Some authors recognize this problem and use the average or sum of some past investment vintages to smooth the volatility in a firm's investment expenditures. Using averages of investment vintages means that assumption 2 has to hold only on average, i.e. the sum of depreciations equals the sum of replacements in a period that is longer than one year.

2.3.3 Perpetual inventory

Clearly, the use of averages alone is not a satisfying solution to the measurement error problem. The measurement error problem is still present, maybe it is weakened. To overcome this general problem, one has to drop assumption 2. One common capital stock approximation method that does

[9]One might argue that the classical measurement error logic (see, e.g., Bound et al. (2001)) is not applicable because of the potential endogeneity of capital in the true model. However, as I'm comparing capital approximation methods this potential problem touches all methods in the same way.

[10]Whether violations of assumption 2 induce the measurement error is tested and discussed in section 2.5.

not rely on assumption 2 is the perpetual inventory method. Unfortunately, this method is only feasible for long time series that are not available in most establishment panel data sets. The central idea of this paper is to modify perpetual inventory in a way that makes it feasible for short panels.

Perpetual inventory is an accounting method. Capital inflows (total investments) are balanced with capital outflows (depreciation, sales from capital stock)

$$\Delta K = (IR_t - D_t) + (IN_t - D_t^{IN}) - SALES. \tag{2.3}$$

The resulting difference is then added to the capital stock of the previous year

$$K_{PIM} = K_{t-1} + \Delta K \tag{2.4}$$

with D_t as the expected depreciation of the capital stock at the beginning of the year, D_t^{IN} as the depreciation regarding the net investments of the current year and K_{PIM} as the capital stock at the end of the current year. Because perpetual inventory is a type of accounting rather than an approximation method, it should lead to more reliable results.

Because perpetual inventory always rests on the capital stock of the previous year, this is an endless chain and the fundamental problem is to find a *starting value*. One solution is to account for investment vintages over a long period and to evaluate the depreciation rate of every vintage. If one knows the depreciation rate, one knows the amount of capital of a certain investment vintage that is left in the capital stock at a certain point in time.

$$K_1 = IR_0 + (1 - DR_{-1})IR_{-1} + (1 - DR_{-2})^2 IR_{-2} + ... \tag{2.5}$$

This procedure is often not feasible if the unit of observation is the firm rather than the industry because if e.g. , the depreciation rate of a certain firm's capital stock is 0.04, one has to observe a firm over 25 years before only known investment vintages are left in the capital stock and thus the latter is computable. Most establishment panels cannot provide this data; first because only few firms are observed over 25 years (panel mortality, plant closure) and second because some panel data sets have been established only 10 or 15 years ago.

One way to compute a starting value for perpetual inventory is proposed by Hall and Mairesse (1995). Their initial assumptions are a constant depreciation rate DR for all firms and times and a growth rate g of investments that is constant over time and firms as well. The assumption of a constant depreciation rate allows rewriting equation 2.5:

$$K_1 = \sum_{s=0}^{\infty} IR_{-s}(1 - DR)^s. \tag{2.6}$$

The constant growth rate implies a deterministic relation between the investments of a certain firm over time and thus allows drawing the investment variable out of the sum. Equation 2.7 shows the final approximation proposed by Hall and Mairesse (1995)

$$K_1 = IR_0 \sum_{s=0}^{\infty} \left[\frac{1 - DR}{1 + g}\right]^s = \frac{IR_1}{g + DR}. \tag{2.7}$$

It follows directly from the deterministic relation between within firm investments over time that one can write the investment history in terms of current investments and its growth rate. While the incorporation of the

growth factor has useful theoretical interpretations, it causes no change in the mechanism of the method. The approach can be reduced to dividing current investment by a factor containing assumptions over investment properties. The proposal so far has the two obvious drawbacks that the combined factor $(g + DR)$ is a guestimate and that all relies on the question whether current investments indeed mirror the firm's investment history.

Modified perpetual inventory overcomes these problems by using the moving average of equation 2.2 as a starting value for the perpetual inventory procedure. Equation 2.2 is based on different depreciation rates for every industry and year and the moving average increases the chance for the investments to equal depreciations (i.e. to mirror the firm's investment history).[11] Further, the distinction between net investments and replacements allows for taking into account an increase in capacity that may occur in the period under consideration.[12] In other words, modified perpetual inventory uses an improved proportionality approach to derive the starting value and applies classical perpetual inventory to compute the capital stocks of subsequent years.

2.4 Implementation of Modified Perpetual Inventory

In general, information on the amount of replacements and net investments at the firm level, the composition of capital stock at the industry level and

[11] The importance of the latter difference is illustrated in table 2.2.

[12] As described in section 2.4, disentangling net and replacements investment is necessary for the computation of firm specific depreciation rates, too.

the knowledge about the average depreciation rate of capital structures and equipments are necessary to apply modified perpetual inventory. After an average depreciation rate at the industry level is computed, see section 2.3, one has to know to which industry a certain firm belongs. To assign the industry level information to the single firm of this industry, assumption 1 is necessary. This information is sufficient to compute the absolute value for every firm's capital stock in a given year.

To construct moving averages as described in section 2.3, no further information is necessary. The crucial point here is to determine a reasonable number of periods of support for the moving average. The more periods a panel contains the longer the moving average can be chosen. Recall that the simple moving average is only a starting point for perpetual inventory, i.e. the average is the first observation for capital stock. When increasing the number of periods of support for the moving average, there is a tradeoff between loss of observations (the first years in the panel) and the intended degree of smoothing the single investment vintages used for the computation of the starting value. Table 2.2 shows the magnitude of smoothing when extending the number of periods of support for the starting value (SMA), computed from the IAB Establishment Panel.[13] SMAn is a simple moving average of the augmented proportionality approach (equation 2.2) with n years of support. From the table, one can see that e.g. extending the period of support from two (SMA2) to three years (SMA3) causes a decrease in the within standard deviation by more than 50 percent. At the same time the number of observations for capital stock (applying modified perpetual

[13]For details on the establishment panel see http://betriebspanel.iab.de/.

inventory) decreased only from 11,175 to 11,114.[14]

After choosing a particular SMA as a starting value for perpetual inventory, the latter is applied. The additional information necessary for it is every firm's depreciation. Information on the sales of capital goods would be useful, too. For my analysis, the latter was not given and therefore has not been considered. The depreciation has to be computed for net investments and the capital stock at the beginning of the period, see equation (2.3). Using information on average economic lives of capital goods, the depreciation of net investments are easy to compute if their type (e.g. plants, buildings or IT) is known. Information on depreciation of the previous years' capital stock are more difficult to get. I computed it by multiplying previous years' capital stock by its average depreciation rate, again assuming a linear depreciation rate. Clearly, this depreciation rate has to be found before. For the first year of perpetual inventory, the depreciation rate of a firm equals that of the industry level by assumption 1. From the second year on, it is a weighted average of the depreciation rate of the capital stock at the beginning of the previous year and the depreciation rate of previous year's net investment.[15]

[14]The number of observations for capital stock can not be seen from the table. What can be seen is the maximum number of starting values computable from this data.

[15]In practice the computation of firm specific depreciation rates is difficult. Due to implausible variations in the investment variables in the examined data the depreciation rate turned out to be negative in few cases. To avoid this problem, I decided to use the industry level information for the depreciation rate (see section 2.3) for all firms instead of firm specific values. The results differ only slightly.

2.5 Empirical Results

Section 2.3 showed why the method presented in this paper is superior to previous attempts from a theoretical point of view. Now the plausibility of the method is examined empirically. Therefore, in this section the modified perpetual inventory is applied in a replication study of Addison et al. (2006).

2.5.1 Replication estimation

The original paper uses the IAB Establishment Panel to examine whether the presence of a works council influences the firm's productivity and the works council effect is controlled for capital input. Capital input is approximated using the replacement investments of the current and the previous year, i.e. the proportionality approach. In the replication study, capital input will be approximated using the modified perpetual inventory approach. The aim of the replication study is to compare the estimated coefficients of the capital related variables using both approaches. The central criteria for comparison are the elasticity of output with respect to capital and the statistical significance of the capital regressors. The two different capital stock approximation methods lead to different samples. To compare the methods directly, only observations that appear in both samples are used for estimation.[16]

Following Addison et al. (2006), a translog production function with total sales (Y), inputs labor (N) and capital (K) and a vector of other explanatory variables (Z) is used:

[16] I am especially grateful to Thorsten Schank for providing the ID's of the firms that Addison et al. (2006) used for estimation.

$$ln\,Y = \beta_0 + \beta_1 lnN + \beta_2 lnK + \beta_{11}\frac{(lnN)^2}{2} + \beta_{22}\frac{(lnK)^2}{2} + \beta_{12} lnN lnK + \gamma Z + \epsilon, \quad (2.8)$$

and the elasticity of output with respect to capital is computed as follows:

$$\epsilon_{Y,K} = \frac{\partial ln\,Y}{\partial lnK} = \beta_2 + \beta_{22} lnK + \beta_{12} lnN. \quad (2.9)$$

The estimation results are reported in table 2.3. The first two columns show the results of pooled OLS estimations based on the two different capital stock approximation methods. Columns three and four show the results for fixed effects estimation. According to Addison et al. (2006) I used a within estimator that only takes account of the deviations from the within firm mean.

While the OLS estimation show no fundamental differences, for the fixed effects estimation both capital coefficients, i.e. $\ln K$ and $1/2(\ln K)^2$, are zero with Addison et al. 's (2006) method and significantly different from zero in the replication analysis. The estimate for $\epsilon_{Y,K}$ obtained with Addison et al. 's (2006) capital stock approximation is also near zero while modified perpetual inventory gives an $\epsilon_{Y,K}$ of 0.110 using exactly the same observations.[17] Mairesse and Jaumandreu (2005) estimated a production function with firm-level data and information on book values of capital stock. They estimated

[17]The elasticities are estimated at sample means. The sample mean of labor is 354 employees. Addison et al.'s (2006) capital stock approximation method gives a sample mean of 5.3 million German Mark for capital, with modified perpetual inventory this amounts to 149 million. Note that, as described in section 2.3, the difference in the means reflects the fact that the modified perpetual inventory approach provides absolute values for capital stock while the proportionality approach leads to proportional values, only.

output elasticities between 0.06 and 0.11; results that are very similar to the results I found with the modified perpetual inventory approach. Taking the studies with information on capital stock as a benchmark (see table 2.1), the empirical results of the replication study are more plausible than the results of Addison et al. (2006). This supports the new method applied in this study.

Although not in the focus of this study, the impact of the new method on the other parameters is interesting as well. Many studies attempted to estimate productivity effects of works councils within a production function context. Many of them did not control for capital stock at all, some applied the proportionality approach (see, for example, Wagner et al. (2006)). Because of this (potentially) inappropriate capital control, the authors had to interpret their works council parameter with the reservation that capital does not play a role. From the results in table 2.3 I conclude that at least for the particular data set and the applied estimators of my study the conclusions drawn from the works council parameter have not to be changed when using the new capital stock approximation method instead of the proportionality approach. Obviously neither the council dummy nor other control variables picked up an observable part of the influence of the capital stock on value added. This in turn supports the interpretation that the improvement in the results of the capital regressors mirror a decrease in measurement error and, as a observable consequence, a decrease in the attenuation bias.

2.5.2 Interpretation and Robustness

When comparing the proportionality approach with modified perpetual inventory, one is interested in isolating the reasons behind the different results of the original and the replication estimation. There are three differences in the methods that could explain the different results:

1. information on net investments are exclusively used in the replication study,

2. in the replication study a three years moving average is applied while only two years are used in the original study and

3. the drop of assumption 2, what is equivalent to the transition from a proportionality approach to a (modified) perpetual inventory approach.

The first two issues could be handled within a proportionality framework, too. If they explain the difference in the results, no modified perpetual inventory approach is necessary. The third point characterizes the methodological innovation in this paper. Only if the transition to perpetual inventory explains a substantial part of the improvement in the results the new method is an improvement, too. To isolate the effect of the third point, one has to compare the results of the modified perpetual inventory approach with the results of a method that differs only in this point from modified perpetual inventory. For these purposes, the proportionality approach is augmented in way that it uses information on net investments (point 1) and the same number of supporting years for the moving average as the modified perpetual inventory approach (point 2). The estimation is repeated with this augmented

proportionality approach (SMA) and modified perpetual inventory (MPI) to compare the results.

To additionally check the robustness of the *difference in the results*, both methods are varied in the number of supporting years for the moving average. As a result I compare three approximation methods: first, modified perpetual inventory based on a three year moving average (MPI3) compared to the augmented proportionality approach based on a three years moving average (SMA3), second, MPI2 versus SMA2 and third, MPI1 versus SMA1. If the results of MPIn are always superior to the corresponding results of SMAn, the transition to perpetual inventory explains the different results.

Table 4.1 shows the estimated $\epsilon_{Y,K}$ for different capital stock approximation and estimation methods. MPIn is the modified perpetual inventory approach with a starting value that is computed from a moving average of length n (MPI3 is thus the method applied to compute the capital value for the replication study discussed above). SMAn is a simple moving average of K_t (the augmented proportionality approach from equation 2.2) with length n. Recall that the SMA are in absolute value and use information about net investments and that the switch to perpetual inventory done in the MPIn but not done in the SMAn is therefore the only difference. The four cells are numbered to ease discussion. The switch to perpetual inventory can be seen when going from the right column to the left. Little difference exists between cells IV and III, while the major contribution of modified perpetual inventory can be seen when cell II is compared to cell I. Due to the setting, one can conclude that the transition to perpetual inventory is the only reason for the improved results in fixed effects estimation. What is the econometric expla-

nation for the improvement? The transition to perpetual inventory causes the drop of the proportionality assumption. The proportionality assumption is expected to cause substantial measurement errors (see section 2.3). Thus, to renounce the proportionality assumption reduces the measurement error and this explains the improved results. Further, from the table it can been seen that pooled OLS estimates (cells III and IV) are generally higher than the fixed effects estimates, supporting the findings in the replication study.[18] When applying the fixed effects estimator instead, the decrease in $\epsilon_{Y,K}$ is higher for the SMA approximation.[19]

One last question to discuss is why the difference in the results of modified perpetual inventory and the augmented proportionality approach nearly completely vanishes in the OLS estimation case. The OLS estimator considers the within and the between dimension of the data. We have seen above that modified perpetual inventory is superior to the augmented proportionality approach in the within dimension. What could cause the noise in the between dimension that offsets the better approximation in the within firm dimension?

To explain this I have to go back to the creation of the starting values for perpetual inventory. The starting value consists of an average of capital stock

[18]The improvements induced by modified perpetual inventory increase with the volatility of the investment expenditures within firms. Therefore, the gains from this method may vary with the data used. However, the general patterns in table 4.1 are expected to persist, i.e. the difference between the columns remains as long as modified perpetual inventory decreases measurement errors and the difference between the rows remains as long as the between dimension of the data adds reliable information.

[19]Note that MPI1 is similar to the approach proposed by Hall and Mairesse (1995) and that the increase in the elasticity when using a three years average for the starting value is substantial.

values that is computed with the augmented proportionality approach, i.e. averages of equation 2.2. Keeping in mind the problems of the proportionality approach one has to expect measurement errors in the starting value, too. Given this fact, perpetual inventory starts from an inaccurate value. While with perpetual inventory there is convergence against the true value in the long run, the error persists in the short run. Hence, I cannot hope to overcome this error because of the shortness of the panels under consideration. The consequence is a noisy measure of the *level* of capital stocks of different firms, inducing noise to the between dimension of the OLS estimation. However, due to the fact that modified perpetual inventory is based on a three years average it is expected to compute more reliable starting values than the method proposed by Hall and Mairesse (1995) that is discussed in section 2.3.

In opposition to modified perpetual inventory, the proportionality approach uses *moving* averages of investment vintages. Hence there is hope that an error in one year is followed by an error in the opposite direction, i.e. the errors are not persistent but random. Therefore, on average, the *level* of capital stock can be expected to have the correct magnitude. Thus, the between firm information derived with the proportionality approach is expected to be more correct.

To sum up: the proportionality approach causes a measurement error problem. For the within dimension of the data, the measurement error problem can be alleviated via the switch to perpetual inventory. When considering the between and the within dimension, i.e. the OLS case, the advantage of modified perpetual inventory decreases.

2.5.3 Extensions

As mentioned above, the new method may only have little advantage if the between dimension of the data is an important source of information, as typically the case in OLS estimates. In such cases, a way to improve the method is to use all available investment vintages to compute the starting value. This has the advantage of intensified smoothing of outliers and may therefore be a better estimate of the capital stock level. However, if there is a trend in a firm's capital stock, using more investment vintages to compute the starting value comes at the cost of destroying within firm information. For instance, consider an increase in investments over time. Using all investment vintages instead of only using those at the beginning of the period will overstate the starting value. As a consequence, computed depreciations are too high and hence, the difference between investments and depreciations is biased to zero. As this holds also for negative trends, using all investment vintages to compute a starting value reduces information in the within dimension. Hence, the optimization of the computation of the starting value depends on the relative importance of the between dimension compared to the within dimension.

In order to quantify these consideration one would have to simulate the performance of the method under controlled circumstance, e.g. in a Monto Carlo experiment. Such simulations could also be helpful to compare modified perpetual inventory with other methods and to derive the conditions that influence their (relative) performance. However, this is left for further research.

2.6 Summary

A more accurate method for firm-level capital stock approximation is proposed for establishment panel data sets that contain only short investment time series and no direct information on capital stock. The utility of the new method is twofold; results of existing studies can be reviewed critically and a lot of rich establishment panel data sets become accessible to more sophisticated production function and factor demand analyses.

In former studies and for short establishment panel data sets, often the firms' investments were used to approximate their capital stock, i.e. the proportionality approach. In this paper, it is shown that the proportionality approach relies on the assumption that observed replacements equal unobserved depreciations. This assumption has to be rejected given highly volatile investment expenditures and reported zero investments. Violations of this assumption indicate the presence of the classical measurement error problem.

Modified perpetual inventory augments the proportionality approach to compute a starting value that allows one to perform traditional perpetual inventory, an accounting method that is not based on the doubtful assumption of investments equaling depreciations and that therefore is expected to yield more reliable results. In a replication study, the modified perpetual inventory approach yields significant and plausible coefficients for the capital regressors and the output elasticity with respect to capital input. Applying the old method to the same observations gives insignificant estimates and a zero output elasticity with respect to capital.

2.7 Tables

Table 2.2: Variations in moving averages of different length

variable	variation	Mean	Standard Deviation	Observations
SMA1	overall	$7.14 * 10^7$	$5.27 * 10^8$	Obs = 20495
	between		$4.28 * 10^8$	Firms = 5474
	within		$2.42 * 10^8$	T-bar = 2.27
SMA2	overall	$8.81 * 10^7$	$5.57 * 10^8$	Obs = 14264
	between		$4.70 * 10^8$	Firms = 5527
	within		$2.06 * 10^8$	T-bar = 2.58
SMA3	overall	$9.51 * 10^7$	$5.66 * 10^8$	Obs = 8737
	between		$5.05 * 10^8$	Firms = 4102
	within		$1.18 * 10^8$	T-bar = 2.13

Notes: Data from IAB Establishment Panel, waves 1996 - 2000, SMAn is a simple moving average of equation 2.2 with n years of support, SMA is measured in million German marks, own calculations.

Table 2.3: Results of the replication study

Regression Method	Ordinary Least Squares		Fixed Effects	
Studies	Addison	Mueller	Addison	Mueller
Works council (dummy: 1=yes)	0.219	0.200	-0.010	-0.009
	[5.77]***	[5.21]***	[0.30]	[0.26]
Number of employees ($\ln N$)	1.161	1.210	0.574	0.957
	[13.47]***	[10.56]***	[8.68]***	[7.32]***
Capital stock ($\ln K$)	-0.099	-0.187	0.019	-0.319
	[1.43]	[1.87]*	[0.69]	[3.09]***
$1/2\ (\ln N)^2$	0.001	-0.005	0.012	0.042
	[0.05]	[0.23]	[0.84]	[2.31]**
$1/2\ (\ln K)^2$	0.025	0.026	0.002	0.035
	[2.74]***	[2.82]***	[0.70]	[4.21]***
$\ln N \ln K$	-0.021	-0.020	-0.010	-0.040
	[1.90]*	[1.70]*	[2.30]**	[3.72]***
Investment in ICT	0.111	0.123	0.022	0.022
(dummy:1=yes)	[4.79]***	[5.29]***	[2.39]**	[2.41]**
State of technology	-0.084	-0.080	-0.004	-0.005
(index: 1=state of the art;	[5.59]***	[5.31]***	[0.75]	[0.69]
5=obsolescent)				
Parttime workers (percentage)	-1.052	-1.038	-0.089	-0.087
	[10.38]***	[10.29]***	[1.73]*	[1.68]*
Apprentices (percentage)	-1.034	-1.034	0.120	0.157
	[5.78]***	[5.76]***	[1.04]	[1.36]
Skilled workers (percentage)	0.312	0.298	0.033	0.033
	[6.03]***	[5.73]***	[1.37]	[1.35]
Collective Agreement	0.090	0.090	-0.009	0.009
(dummy: 1=yes)	[3.16]***	[3.17]***	[0.63]	[0.64]
Constant	12.595	13.055	14.084	15.376
	[41.15]***	[22.42]***	[74.36]***	[20.93]***
Output elast. at sample means				
$\epsilon_{Y,N}$	0.839	0.814	0.496	0.446
$\epsilon_{Y,K}$	0.158	0.186	-0.003	0.110
Observations	6077			
Firms	2459			
Year Dummies	yes		yes	
R^2 overall	0.91	0.91	0.84	0.84

Notes: Merged Sample, Translog Production Function (1997 - 2000; dependent variable: total sales (log Y)); firms with at least five employees; t values in brackets; *,**,*** denote significance at the 0.1, 0.05 and 0.01 level, respectively.

Table 2.4: Summary for different capital stock approximation methods

Estimation of $\epsilon_{Y,K}$	Modified Perpetual Inventory (MPI)	Simple Moving Averages (SMA)
Fixed Effects Estimation	**I** MPI1: $\epsilon_{Y,K} = 0.06$ MPI2: $\epsilon_{Y,K} = 0.08$ MPI3: $\epsilon_{Y,K} = 0.11$	**II** SMA1: $\epsilon_{Y,K} = 0.02$ SMA2: $\epsilon_{Y,K} = 0.01$ SMA3: $\epsilon_{Y,K} = 0.01$
Pooled OLS Estimation	**III** MPI1: $\epsilon_{Y,K} = 0.15$ MPI2: $\epsilon_{Y,K} = 0.17$ MPI3: $\epsilon_{Y,K} = 0.19$	**IV** SMA1: $\epsilon_{Y,K} = 0.15$ SMA2: $\epsilon_{Y,K} = 0.15$ SMA3: $\epsilon_{Y,K} = 0.17$

Notes: The numbers after MPI and SMA denote the number of periods of support for the moving average (in years) the capital approximation is based on.

Chapter 3

The Productivity Effect of Non-Union Representation

3.1 Introduction

The present system of labor relations in the United States is part of the "New Deal", initiated between 1933 and 1936 by President F.D. Roosevelt as a reaction to the Great Depression. While strengthening workers' rights when engaging in collective bargaining, the National Labor Relations Act of 1935 (and its amendment, the Taft-Hartley act of 1947) prohibits the formation of any form of employer-employee committee that has the power to decide on working conditions and labor-management relations.[1] As a result, the present system of industrial relations contains union bargaining but no mandatory or voluntary form of workplace representation. Low union

[1]See section 8(a)(2) of the National Labor Relations Act.

density[2] recently raised doubts about whether this system still does a good job and increased interest in alternative ways of employee representation at the workplace. In the mid-nineties, economists[3] and politicians[4] analyzed the industrial relations system of European countries to learn which institutions may help to improve the competitive position of the United States. The debate passed without strengthening employees' participation rights in the National Labor Relations Act – possibly due to the uncertainty about the economic consequences of formalized participation.

The most prominent example of non-union workers' participation in European countries is employee representation via works councils. Rogers and Streeck (1995) define works councils as "institutionalized bodies for representative communication between a single employer and the employees of a single plant or enterprise". Works councils are designed to give workers a collective voice and to increase workplace democracy. But they do more: many studies show that they positively contribute to a society's regulatory performance by enforcing commitment to legal standards regarding, e.g., environmental protection (Askildsen et al. (2006)), gender equality (Heywood and Jirjahn (2009)), and health and safety (Weil (1999)).

Given the positive social effects of works councils as mentioned above, a society is better of with works councils if councils additionally increase pro-

[2]The share of union members among private sector workers decreased from 24.2 percent in 1983 to 7.6 percent in 2008, see Hirsch and Macpherson (2009).

[3]Rogers and Streeck (1995) published an influential book on works councils. This book is one outcome of the comprehensive NBER project "Working and Earning under Different Rules: What the United States Can Learn from Labor Market Institutions in Other Developed Countries" by Richard B. Freeman.

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

ductivity. Even if works councils are permitted by law and even if employers and workers knew that a works council would increase productivity, expected distributional conflicts at the establishment level can obstruct their foundation (Freeman and Lazear (1995)). Hence, if a society desires to have strong works councils, it has to make them mandatory.

The hypothesis of increased establishment productivity through works councils mainly rests on the assumption that they improve communication between management and workers. Works councils can be an important source of information for managers helping them to improve the quality of their decisions. Councils may also be able to motivate both parties to make longer-term commitments (Freeman and Lazear (1995)) and, hence, increase the probability of workers' concessions in economically difficult times and of higher investments in firm-specific human capital. Smith (1991) argues that employee participation may reduce opportunistic behavior of managers. However, a works council may also worsen performance. Depending on the specific design of the council's rights, managers may have to consult it or have to come to an agreement with it in situations where fast decision-making is necessary. If councils have codetermination rights, they can block decisions. Therefore, from a theoretical point of view, the productivity effect of works councils is unclear.

Because mandated works councils do not exist in the United States, the productivity effect of councils can only be estimated for other countries. Nevertheless, if the effect is positive there – maybe the United States can learn from that. German works councils belong to the most powerful ones in Europe (see Streeck and Vitols (1995 p. 270) or Streeck (1995) for a comparison

of European works councils) and most empirical studies found a non-negative productivity effect (see Addison et al. (2004) for a survey). But, since existing data do not include direct information on capital stock, inference regarding the productivity effect of works councils is conditional on the assumption that capital stock does not matter. Previous results contain a wide variety in the estimated council effect, including obviously implausible "productivity effects" of up to 30 percent.

This contribution studies whether works councils increase or decrease the productivity of German establishments. To estimate the productivity effect, in a first step, value added is regressed on capital and labor inputs with a GMM-SYS estimator to address endogeneity issues. In a second step, the residuals of step 1 are regressed on establishment characteristics. At this second stage, an Oaxaca-Blinder-decomposition of the output differential of establishments with and without a council is used to estimate the productivity effect. Finally, an endogenous switching regression model is applied to check for unobserved mechanisms that, simultaneously, may explain productivity and self-selection into the observed works council status.

I find a positive impact of councils on establishment productivity of 6.5 percent. However, I do not claim this figure to be the causal effect of council existence on productivity. Rather I present empirical evidence and additional theoretical arguments that this figure is the lower bound to the causal productivity effect. The conclusion is that works councils, embedded in an appropriate system of industrial relations, have no negative impact on productivity.

3.2 German Works Councils

In Germany, the Works Constitution Act (*Betriebsverfassungsgesetz*) is the legal base for works councils. Workers have the right to establish a council in establishments with at least five employees. Hence, works councils are mandatory but do not evolve automatically. The employer bears the expenses for the election and other costs the council causes. Works council members are elected for four years and enjoy strong employment protection. For establishments with more than 200 employees, at least one councillor acts as a full-time councillor. The larger the establishment, the higher is the number of works councillors and the stronger are their rights.

In general, council rights are weaker with regard to financial and economic affairs and stronger in personnel matters and social concerns. Explicitly, the Works Constitution Act (WCA) gives councils codetermination rights in the field of workers' health and safety and of social and personnel matters such as the introduction of new payment methods, the regulation of overtime, recruitment guidelines, transfers, and dismissals. Furthermore, they have information and consultation rights in financial matters, personnel planning, and with respect to changes in the work environment and the adoption of new technologies.

The WCA not only determines the rights of councils, it also obliges councillors to cooperate with the management. Councils and management should act in "a sprit of mutual trust", "in cooperation with union and employer organizations" and "to the benefit of the employees and of the establishment" (WCA, Section 2). It is also determined that councils have no wage bargain-

ing power and no right to call a strike and that the work of the union is not restricted by the WCA. Hence, the German system of industrial relations consists of two parts. While unions have the exclusive right of industry-wide collective bargaining, works councils are the designed to be the collective voice with respect to work place conditions for *all* workers in a specific plant or establishment, regardless of whether they are union members or not or whether their establishment is covered by collective bargaining. Although works councils and unions are formally independent, most works councillors are union members or have close ties to a union. Unions devote expertise and financial resources to councils, while works councillors often actively recruit new union members (Streeck (1995) p. 335).

3.3 Literature

3.3.1 How can works councils affect productivity?

Councils act as the collective voice and as the ear of the workers and are able to reduce information asymmetries between labor and management. Management may, e.g., misinform workers about the true economic state of the firm to extract higher effort. Anticipating such strategies, workers may distrust management information, even if it is truthful. Councils with legal **information rights** are able to act as workers' ear by verifying such information and thus may be able to solve or at least reduce the communication problem (see e.g. Rogers and Streeck (1995)).

As their collective voice, councils communicate workers' preferences to

the management. **Consultation rights** commit the employer to listen to this voice and to consult the council prior to intended changes. Additionally, formal consultation provides a forum for both sides to find new solutions to problems and this may help managers to improve the quality of their decisions. If a council has **codetermination rights** on a particular matter, its agreement is necessary for a decision.

Giving workers a voice and letting them articulate dissatisfaction will reduce costly quits (see Hirschman (1970)). Several studies found a decrease in personnel turnover in case of works council presence (see e.g. Frick (1996) or Addison et al. (2001)) supporting a reduced exit propensity.

Section 80 of the WCA calls upon councils to enforce the legal rights of workers at the establishment level. The strong **employment protection** legislation,[5] codetermination rights, and the council's support to workers (e.g. legal advice) have the potential to considerably hinder displacements.[6] As a result, workers and management may make longer-term commitments, which would, for instance, decrease the hold-up problem of investing in firm-specific human capital.

Of course, all those positive effects can be costly. If councillors do not work in a "spirit of mutual trust" and "to the benefit of the establishment" as demanded by the WCA, they are able to deteriorate establishments' performance. But even if councils are cooperative, some of their characteristics

[5] According to the overall employment protection index of the OECD (see OECD (2004) chart 2.1), 18 out of 28 countries have less strict regularities than Germany.

[6] A methodological implication of increased employment protection through works councils is that workers could tend to erect a works council in times of bad establishment performance to save their jobs. This kind of self selection will lead to a downward bias in the estimated effect of council existence on establishment productivity.

are inherently productivity decreasing. Informing a council takes time, and, worse, consultation takes time *before* a decision can be made and this can result in the loss of profitable opportunities. Codetermination can lead to a suboptimal allocation of an establishment's resources and, of course, having a secure working place also may create incentives for moral hazard. In sum, whether the benefits of councils outweigh the costs is an empirical question.

3.3.2 Empirical Results

To asses the productivity effects of German works councils, mainly two large-scale data sets have been used so far,[7] the IAB Establishment Panel[8] (e.g. Addison et al. (2006), Frick and Möller (2003), Schank et al. (2002), Wolf and Zwick (2002)) and the Hannover Panel[9] (e.g. Addison et al. (2001), Hübler and Jirjahn (2003), Jirjahn (2003)).

Generally, the estimated productivity effect of councils is non-negative, ranging from insignificant effects close to zero (Addison et al. (2006); Schank et al. (2002)) to large effects (i.e. around 15 percent in Addison et al. (2001) and Wolf and Zwick (2002); up to 30 percent in Frick and Möller (2003) and Addison et al. (2006)). All studies with large productivity estimates applied OLS, while those which estimated a zero effect used the fixed effects estimator. The difference can be explained with unobserved heterogeneity that leads to upwards biased OLS estimates.

However, this does not mean that the insignificant estimates close to zero are necessarily correct. A fixed effects estimator only uses within-

[7] See Addison et al. (2004) for an overview over the results from small-scale data sets.
[8] See http://www.iab.de/de/erhebungen/iab-betriebspanel.aspx.
[9] Observations from 1994–1997, meanwhile part of the IAB Establishment Panel.

establishment variations to identify partial effects. Few establishments establish or close a council (see e.g. Addison et al. (2006)) and this may explain the insignificance of the effect. Also, changes in the council regime may have no immediate effects on productivity.

Existing studies on councils' productivity effect only crudely control for capital stock.[10] Further problems with respect to the sample selection of previous studies are described below. Addressing these problems, I test whether works councils deteriorate establishment productivity. My empirical strategy allows for causal inference.

3.4 Data

This analysis is based on the Linked Employer-Employee Panel of the Institute for Employment Research (IAB). In the data set, administrative information on employees is matched with survey information on establishments. The survey unit is the establishment or local production unit, rather than the legal and commercial entity of the company.

3.4.1 Sample Selection

I restrict the analysis to the manufacturing sector. Since works council rights increase if an establishment has more than 20 employees, I drop all establishments that have less than 21 employees in at least one of the periods under consideration. The probability of works council existence increases with establishment size: while only about half of the establishments with 21

[10] See Mueller (2008) for a discussion of capital stock approximation.

to 100 employees have a council, this share is about 99 percent in the group of manufacturing establishments with more than 300 employees. An objection against former studies that neglect the correlation between establishment size and council probability is that the measured productivity effect is biased due to unobserved effects that are correlated with establishment size. To avoid this potential weakness, my analysis is confined to establishments with, at the maximum, 300 workers. A dummy for establishments with less than 101 workers is also included in regression.

Since the reform of the WCA in 2001 implies substantial changes in the council rights, I only consider the period from 2001 to 2005 and end up with 2,879 establishment-year observations on 1,086 different establishments.

3.4.2 Variables

Because different establishments will produce output using different shares of intermediate inputs, value added is a better approximation for economic performance than total sales and is used as the dependent variable in the production function. Value added is regressed on works council presence, the number of employees, and the value of the capital stock.[11] The other control variables are the percentages of part-time workers, apprentices and skilled workers[12] in total employment, whether the establishment is covered by collective bargaining, the number of persons participating in employer-supported training programs, industry affiliation, location in East or West

[11]The data does not contain direct information on the capital stock. I use an approach by Mueller (2008) to compute the capital stock from investment data.

[12]Skilled workers are craftsmen who have at least two years of formal professional education, or other employees who perform qualified tasks, i.e. also university graduates are included in that group.

Germany, a dummy indicating whether the establishment has between 21 and 100 employees, the state of technology, the indication whether the establishment exports and whether it belongs to a group of affiliated companies.[13]

The theoretical considerations in the previous section indicate that losses of firm-specific human capital due to personnel fluctuation may be important for productivity and related to council existence. The regressor "number of employees" controls for changes in the total amount of labor used in production. However, it does not control for fluctuations that leave the level of total employment unaffected. To deal with this, the churning rate is taken as an additional measure of fluctuation (see Burgess et al. (2000)).[14] The churning rate is a measure for separations that lead to replacement hirings and thus indicates fluctuations that do not affect total employment.

3.5 Empirical Model

3.5.1 The Production Function

I base my analysis on a Cobb-Douglas production function that contains value added (Y), labor (L), capital (K), works council presence, and the other above mentioned control variables.

The static Cobb-Douglas specification is:

[13]Summary statistics of the establishment-specific means are presented in table 3.2 on page 71.

[14]The churning rate is the difference between the total work flow rate (WF) minus the absolute value of the net change rate (NET) in employment. WF is the share of hired (WIF) plus the share of displaced workers in total employment (WOF), and $NET = WIF - WOF$.

$$ln(Y_{it}) = \alpha ln(L_{it}) + \beta ln(K_{it}) + \delta' Z_{it} + \nu_i + m_t + \eta_{it} \quad (3.1)$$

with

$$\eta_{it} = \rho \eta_{i,t-1} + \epsilon_{it}$$

where ν_i is a establishment-specific fixed effect, m_t captures time effects that are common to all establishments, η_{it} is an idiosyncratic and possibly autoregressive productivity shock, ϵ_{it} is a white noise error term, α and β are the output elasticities with respect to labor and capital, and δ' is a vector of coefficients on Z_{it}, the vector of control variables. Note that some of the control variables are time-invariant or at least nearly time-invariant.

A dynamic representation of equation 3.1 is

$$\begin{aligned} ln(Y_{it}) = {} & \alpha ln(L_{it}) - \rho\alpha ln(L_{i,t-1}) + \beta ln(K_{it}) - \rho\beta ln(K_{i,t-1}) + \\ & \rho ln(Y_{i,t-1}) + \delta ln(Z_{it}) - \rho\delta ln(Z_{i,t-1}) + \\ & (1-\rho)(\nu_i) + (m_t - \rho m_{t-1}) + \epsilon_{it} \end{aligned} \quad (3.2)$$

where the possibly autoregressive nature of productivity shocks is explicitly modeled and therewith removed from the error term. The static specification in equation 3.1 is a special case of equation 3.2. Both equations coincide if ρ is zero. To consider the more general case, I estimate the dynamic specification.

3.5.2 Endogeneity and Time-Invariance

When estimating a production function, one generally faces the problems of simultaneity and unobserved heterogeneity. Additionally, when estimating works council effects, one has to be aware of the time invariance of the council status.

From an econometrician's perspective, the simultaneity problem is a correlation of the time-varying part of the error term with one or more explanatory variables (typically with labor and capital). Similarly, unobserved heterogeneity can be viewed as a correlation of the time-invariant part of the error term with one or more explanatory variables. One way around both problems is to use lagged differences of the endogenous regressors to instrument their levels. Unfortunately, lagged differences are often only weakly correlated with the original regressor. To overcome this problem, Arellano and Bover (1995) proposed to additionally estimate the model in first differences and instrument with lagged levels. In that case, problems arise if the researcher is interested in estimating the coefficients of nearly time-invariant regressors (e.g. works council existence) or, even worse, totally time-invariant regressors because any differences of time-invariant regressors are zero and, therefore, no variation remains to identify their coefficients.

However, the situation of endogeneity and time-invariance is manageable using a two-staged approach as applied, for example, in Black and Lynch (2001). In a first step, value added is regressed on the variable inputs while – as described above – internal instruments are used to deal with the potential endogeneity of the regressors. The residuals of that first step regression are

averaged within establishments and used as dependent variable in a second step. In that second step, the averaged residuals are regressed on the remaining time-invariant regressors. As a result, coefficients of time-invariant regressors can be estimated while one has controlled for the endogeneity of the variable inputs. However, if the time-invariant regressors are themselves endogenous, their coefficients may be still biased.

In my study, the regressors in Z_{it} are time-invariant or nearly time-invariant. I construct the within-establishment averages of the nearly time-invariant regressors and use these averages together with the time-invariant regressors as explanatory variables in the second step. Next, both steps are explained in more detail.

First-Step Estimation

Natural candidate instruments for the variable input factors labor and capital are lagged differences of the regressors because they are correlated with the regressor but are assumed to be exogenous. The more lags are used, the more efficient is the estimate but the smaller is the sample size. Because I have only a short panel of five years, classical IV-style instruments are inadequate. A way around this problem is to use GMM style instruments as proposed by Holtz-Eakin et al. (1988).

With these instruments, equation 3.2 can be estimated consistently with the system GMM (GMM-SYS) estimator proposed by Arellano and Bover (1995) and which was first applied to a production function estimation by Blundell and Bond (2000). To improve efficiency, the GMM-SYS estimator estimates a system of a first-differenced and a level equation. It uses lagged

levels of the endogenous variables as instruments in the first differenced equation. Additionally, lagged differences are used to instrument the regressors in the level equation.

Second Step Estimation

The dependent variable for the second step is the fixed effect of each establishment. To obtain it, I first generate the predicted values for value added and subtract it from the observed values[15]

$$ln(Y_{it}) - [\widehat{ln(Y_{it})}] = (1-\rho)(\delta' Z_i + \nu_i) + error_{it}. \tag{3.3}$$

I then average that value over the period 2001–2005 for each establishment to get an estimate of the establishment-specific time-invariant component of the first step residual, i.e. $(1-\rho)(\delta' Z_i + \nu_i)$. If *error* is a zero mean error term, averaging over time will eliminate or at least substantially reduce its contribution to the residual.

The second step estimation equation is

$$R_i = \delta' Z_i + \nu_i + \widetilde{error}_i \tag{3.4}$$

with

$$R_i = \frac{\frac{1}{T}\sum_t ln(Y)_{it} - [\widehat{ln(Y)_{it}}]}{1-\rho}.$$

[15]Note, because Z contains now only time-invariant regressors, it varies only between establishments and the equation simplifies.

Oaxaca-Blinder Decomposition

An interesting alternative to estimating equation 3.4 directly with OLS is the Oaxaca-Blinder decomposition, introduced by Oaxaca (1973) and Blinder (1973): the output differential between two groups can be decomposed into explained and unexplained components. The output differential between establishments with a council (C) and establishments without (N) can be partitioned in either of the two following ways:

$$R_C - R_N = \delta_C(Z_C - Z_N) + Z_N(\delta_C - \delta_N) + (error_C - error_N) \qquad (3.5)$$

or

$$R_C - R_N = \delta_N(Z_C - Z_N) + Z_C(\delta_C - \delta_N) + (error_C - error_N) \qquad (3.6)$$

where, for clarity, the establishment subscript has been dropped. $R_C - R_N$ is the mean output differential, Z_C and Z_N are vectors of mean values of the independent variables (including an intercept) and δ_C and δ_N are estimated coefficient vectors. Equation 3.5 says that the output differential can be decomposed into a part due to differences in endowments evaluated at the council establishments' coefficients and a part due to differences in coefficients evaluated at the means of the group without councils. The first part of equation 3.5 can be interpreted as the difference in output the council group would achieve if it had the other group's endowments, i.e. the explained part of the output gap. The second part represents the difference

in output the group without councils would experience if it had the same productivity as the council group, i.e. the unexplained part or, if assuming random assignment of councils to establishments, the average treatment effect on the non-treated.

In the second term of equation 3.6, the productivity differences are evaluated at the council group's means. Assuming random assignment of councils to establishments, this term is the average treatment effect on the treated, indicating the difference in output the council group would experience if it had no council. This is the effect I estimate below.

Following an idea of Winsborough and Dickenson (1971), the treatment effect on the treated can be estimated using a threefold decomposition of the output differential:

$$\begin{aligned} R_N - R_C &= \delta_C(Z_N - Z_C) \\ &\quad + Z_C(\delta_N - \delta_C) \\ &\quad + (\delta_N - \delta_C)(Z_N - Z_C). \end{aligned} \tag{3.7}$$

While the unexplained part in the second line is the desired treatment effect on the actually treated establishments,[16] the third term indicates whether, e.g., the council establishments accumulate more of such endowments for which they have a productivity advantage (compared to the other group) or not.

[16]Note, this decomposition leads to a negative output differential. Hence, the second term is negative if the council establishments have a productivity advantage. This could be interpreted as the output reduction that they would experience if they close the council.

Self-Selection into Works Council Regimes

Workers have the right to establish or close a works council. Hence, they select their establishment into one of two possible regimes, i.e. into having a council or not. If the self-selection mechanism is systematically related to the establishments' productivity, OLS estimates of the council effect and the Oaxaca-Blinder decomposition are biased. However, for at least some self-selection patterns the direction of the OLS bias is clear.

Consider the case where there are unobserved factors that increase the incentives of workers to establish or maintain a works council and, at the same time, are negatively correlated with productivity. In that case, random assignment of councils to establishments would increase the output differential. Applying the Oaxaca-Blinder decomposition to an output differential that is too small leads to underestimation of the council effect. In the following, it is briefly shown how to adjust the output differential.

The self-selection into a works council regime can be described by an endogenous switching regression model.[17] If the utility of having a council is higher than its costs, workers will choose to maintain a works council. Even though the utility cannot be observed by the researcher, the workers' choices are observed. The endogenous switching regression model can be estimated using the Heckman two-step estimator (see Heckman (1979)). The latent utility of having a council is

[17] Also called "Roy Model", see, e.g., Maddala (1983).

$$W_i^* = \gamma' Z_i + \tau' I_i + u_i \tag{3.8}$$

with Z_i as the vector of second step regressors from equation 3.4, I_i as a vector of external instruments, γ' and τ' as coefficient vectors and u_i as a random error. The observed choices are

$$W = 1 \quad if \quad W^* > 0$$
$$W = 0 \quad if \quad W^* \leq 0$$

with W as a dummy indicating the presence of a works council. The output equations can be estimated consistently with

$$R_{Ci} = \alpha_C + \delta_C' Z_i + \sigma_C \left(\frac{\phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)}{\Phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)} \right) + \epsilon_{Ci} \quad if \quad W = 1 \tag{3.9}$$
$$R_{Ni} = \alpha_N + \delta_N' Z_i + \sigma_N \left(-\frac{\phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)}{1 - \Phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)} \right) + \epsilon_{Ni} \quad if \quad W = 0 \tag{3.10}$$

where $\hat{\gamma}' Z_i + \hat{\tau}' I_i$ is the predicted probability of having a works council from equation 3.8, $\phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)$ is the density function evaluated at $\hat{\gamma}' Z_i + \hat{\tau}' I_i$ and $\Phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)$ is the cumulative distribution function at this point.[18] Hence, the expressions after σ_C and σ_N are the inverse Mills' ratios, accounting for the non-random selection of works council regimes.

Having estimated equations 3.9 and 3.10, the output differential $(R_N - R_C)$ can be adjusted by subtracting $\widehat{\sigma}_C \left(\frac{\phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)}{\Phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)} \right)$ and $\widehat{\sigma}_N \left(-\frac{\phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)}{1 - \Phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)} \right)$

[18] For consistency of the endogenous switching regression model one has to assume that u_i, ϵ_{Ci} and ϵ_{Ni} follow a trivariate normal distribution.

from both sides of the respective equation. Hence, the selectivity-corrected dependent variables are $R^*_{Ci} = R_{Ci} - \widehat{\sigma}_C \left(\frac{\phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)}{\Phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)} \right)$ and $R^*_{Ni} = R_{Ni} - \widehat{\sigma}_N \left(- \frac{\phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)}{1 - \Phi(\hat{\gamma}' Z_i + \hat{\tau}' I_i)} \right)$. Imitating random assignment of works councils, the corrected output differential $(R^*_N - R^*_C)$ is decomposed using equation 3.7.[19]

Although the endogenous switching regression model is identified through nonlinearities, additional instruments will improve identification. Appropriate instruments have to be uncorrelated with the errors in equations 3.8, 3.9, and 3.10 but should explain as much variation in W as possible.

Acknowledging that valid instruments are hard to find, I choose the industry share of establishments having a works council as a technical instrument. Additionally, I construct one instrument from the data's employee dimension that reflects worker heterogeneity within establishments. The heterogeneity measure is the within establishment standard deviation in workers age. As a homogenous work force is assumed to agree more easily on electing and running a works council,[20] I expect the heterogeneity measure to be negatively correlated with council existence. The industry share is by construction positively correlated with council existence. Empirical evidence is presented in the next section.

Assuming valid instruments, the productivity effect from estimating equation 3.7 with the adjusted dependent variables is the average treatment effect of council existence, while estimating equation 3.7 with the unadjusted variables gives the treatment effect on those firms who actually choose to have a

[19] See Neuman and Oaxaca (2004) for a methodological analysis of decompositions with selectivity corrected equations.

[20] See Freeman and Medoff (1984) and Demsetz (1993) for a discussion of worker heterogeneity and union representation elections.

council. However, using non-experimental data, one can rarely be sure that an instrument is uncorrelated with the error terms in the outcome equations. To check robustness of the estimated selectivity pattern, I will apply multiple specifications of the endogenous switching model using a variety of different instruments to equation 3.8. Of course, each specification will produce its own selectivity-corrected output differential and, therefore, its own estimate for the average treatment effect. Instead of interpreting one of the resulting point estimates as the average treatment effect, I will only check whether they are higher or lower than the estimate from the unadjusted decomposition. If they are higher, I interpret the estimate from the unadjusted decomposition as the lower bound to the unknown average treatment effect.

3.6 Results

3.6.1 First-Step Results

The results in table 3.1 show short-run output elasticities of 0.75 with respect to labor and 0.49 with respect to capital.[21] The long-run elasticities are 0.75 for labor and 0.26 for capital, and all estimated parameters except the one for the lagged labor input are highly significant.

After having controlled for capital and labor, the (unadjusted) output

[21] I used twice and more lagged levels of the output variable, threefold lagged levels of the input variables and simple lagged levels of time dummies as instruments in the first differenced equation. For the levels equation, once lagged first differences of the output variable and twice lagged first differences of the input regressors are used as instruments. To improve efficiency, instruments are drawn from all past waves of the IAB Establishment Panel starting in 1993. The total number of instruments is 96 and the Sargan test of overidentifying restrictions suggests no misspecification of the model ($Prob > chi^2 = 0.20$). The Arellano-Bond test for autocorrelation in the first-differenced errors does not reject the hypothesis of no autocorrelation ($Prob > z = 0.21$).

differential $R_C - R_N$ amounts to 19.4 percent, indicating that establishments with a works council produce ceteris paribus on average 19.4 percent more value added. This is not interpretable as a productivity effect of works councils because other establishment characteristics have not been controlled for so far.

3.6.2 Second-Step Results

Oaxaca-Blinder Decomposition

The second-step estimations are used to obtain the productivity effect. Table 3.2 shows descriptive statistics of the second step variables.

The decomposition results are presented in table 3.3.[22] Two third of the output differential can be explained by different endowments and interaction effects. The unexplained part amounts to 6.5 percent and reflects a higher productivity of works council establishments; i.e. after having controlled for all available information (but not for self-selection), council establishments create on average 6.5 percent more value added.[23]

The central result is the positive 6.5 percent productivity effect of works councils on the actually treated establishments.[24] In the following it is discussed why this is the lower bound to the average treatment effect.

[22]The decomposition is conducted using the "Oaxaca" command in Stata. For the computation of the standard errors see Jann (2008).

[23]Being aware of the criticism of Jones (1983), I will not interpret the contribution of each regressor to the unexplained part.

[24]As a robustness check, I dropped one or more second step regressors arbitrarily and repeated the decomposition. The resulting productivity effects are of similar magnitude and never below 6.5 percent.

Underestimation of the Council Effect

There are econometric and economic arguments for considering the estimated productivity effect on the treated of 6.5 percent as a lower bound to the true effect. The economic argument stems from the well-known phenomenon of decreasing productivity during cyclical downturns due to labor hoarding. Labor hoarding means that establishments do not fully adjust their labor input to decreasing production. The consequence is a lower capacity utilization and therefore a lower productivity. The period under examination in this study (2001–2005) is characterized by a cyclical downturn of the German economy with an average annual growth rate of real GDP in the manufacturing sector of 1.5 percent, while in 2000 and 2006 growth rates were around 7 percent.[25] Assuming that stronger employment protection in works council establishments increases labor hoarding, the productivity effect of councils is higher in cyclical upturns and is therefore underestimated in this study.

From an econometric point of view, self-selection into the works council regime is the main reason why my results are only lower bounds to the true average treatment effect. The results with selectivity correction presented in table 3.4 show that random assignment of works councils to establishments increases the output differential and the estimated productivity effect. These results are estimated with the following instruments in the selection equation 3.8 (the first figure in parentheses is the pairwise correlation coefficient with council existence and the second figure is the associated p-value for a test of the hypothesis that this correlation coefficient is equal to zero):

[25]Statistisches Bundesamt (2008a)

1. SHARE: = industry share of works council establishments (0.160; 0.000)

2. AGE: = within-establishment standard deviation of employees' age (-0.194; 0.0000)

As discussed in more detail in the previous section, the first instrument is a standard technical instrument and the second reflects worker heterogeneity within establishments. The reported signs for the correlation coefficients with council existence coincide with a priori expectations: establishments with a high degree of heterogeneity in workers age are significantly less likely to have a council.

Table 3.4 shows the adjusted output differentials from endogenous switching regression models and the corresponding decomposition results for the two instruments. While there is some variation in the productivity effect, the general direction is obvious. The adjusted output gap as well as the productivity effect are clearly higher than the estimates from the unadjusted case (for the latter see table 3.3). This change in the results occurs because both σ_i are estimated to be negative.[26] With negative σ_i, it can directly be seen from equations 3.9 and 3.10 that the average establishment in the council group would perform poorer than the average establishment in the whole sample would do, provided that both have a council or both have no council.[27] This is an important additional insight and can be explained by the councils' offer of employment protection – workers of poorly performing

[26] Jirjahn (2009) finds the same selection pattern.

[27] In contrast to the interpretation of the classical Roy Model it is impossible to conclude from both σ_i being negative that both types of establishments are better of with the council regime they are in. This is because in the Roy Model the agents behave optimal with respect to their outcomes (earnings) while workers in my model do not necessarily care about productivity when deciding about works council existence.

establishments may choose to maintain a council to protect their rents (for a discussion see Jirjahn (2009)).[28]

Nevertheless, keeping in mind the difficulties in finding appropriate instruments, there is good reason to be very careful in interpreting the figures presented in table 3.4. All I claim here is that the true productivity effect is higher than the not selectivity-corrected productivity estimate of 6.5 percent and I do not claim to what extent this may be the case.

3.7 Summary

Most economists expect non-union participation of employees in establishment-level decision-making to have desirable social effects, such as workplace democracy or the enforcement of legal standards in working conditions and environmental protection. However, there is no unambiguous empirical evidence about the economic efficiency of such participation. I examine German works councils as a prominent example of non-union participation to assess their influence on establishment productivity.

Data on roughly 1,050 small to medium-sized manufacturing establishments is taken from the 2001–2005 waves of the Linked Employer-Employee Panel of the Institute for Employment Research (IAB). A GMM-SYS estimator addresses the endogeneity of capital and labor in the production function. After controlling for capital and labor, I decompose the remaining output differential between establishments with a council and establishments without a

[28] As another check for robustness, the endogenous switching regression model is estimated via full information maximum likelihood. Regardless of the combination of instruments, the correlations of the error terms of the output equations with the error of the selection equation are negative and hence, both σ_i in equations 3.9 and 3.10 are negative.

council into explained and unexplained parts and estimate a positive productivity effect of council existence of 6.5 percent. However, this is the effect for establishments whose workers actually *choose* to maintain a works council. An endogenous switching regression model controls for self-selection into the council regime and mimics random assignment of councils to establishments. Its results and further economic and empirical arguments indicate that the estimated effect of 6.5 percent is the lower bound to the average treatment effect.

This study shows that it is possible to design a system of industrial relations where works councils improve the productivity of establishments. Theoretical studies (Freeman and Lazear (1995)) and empirical studies (Hübler and Jirjahn (2003)) showed that the productivity effect of works councils increases if distributional conflicts are worked out on a higher level than the establishment level. Hence, industries in the Unites States that have centralized wage bargaining for a group of establishments or an industry (such as e.g. the automobile industry) could benefit from mandatory works councils – given that unions concentrate on bargaining and works councils focus on working conditions.

3.8 Tables

Table 3.1: Production function estimation of manufacturing establishments for the years 2001–2005 using the GMM-SYS estimator

Variable	**Coefficient**	(Std.Error)
L1.log(value added)	0.290***	(0.065)
log(number of employees)	0.754**	(0.350)
L1.log(number of employees)	-0.228	(0.377)
log(capital stock)	0.487***	(0.163)
L1.log(capital stock)	-0.304**	(0.134)
Intercept	5.700***	(1.267)
Observations	2879	
establishments	1086	

Note: Robust standard errors. **,*** denote significance at the 0.05 and 0.01 level, respectively, and L1 is the one-period lag operator. Year dummies are included.

Table 3.2: Summary statistics of 2nd-step variables

	Works Council		No Works Council	
Variable	**Mean**	**Std.Dev.**	**Mean**	**Std.Dev.**
average residual from first-step GMM-SYS estimation	0.07	0.60	-0.13	0.61
Churning rate (percent)	2.66	2.29	4.29	7.09
Covered by collective bargaining (yes = 1)	0.72	0.39	0.30	0.38
East Germany (yes = 1)	0.39	0.49	0.57	0.50
State of technology (index: 1 = state of the art; 5 = obsolescent)	2.27	0.65	2.13	0.58
Percentage of part-time workers	5.60	7.17	9.14	11.40
Percentage of temporary workers	3.44	6.58	2.29	4.62
Percentage of apprentices	4.25	3.62	6.22	5.90
Percentage of skilled workers	76.96	22.72	77.53	23.79
Exporter (yes = 1)	0.75	0.38	0.55	0.45
Does not belong to a group of affiliated firms (yes = 1)	0.69	0.41	0.84	0.31
Percentage of workers participating in training programs	18.73	18.15	14.82	16.27
Observations	560		488	

Note: The means are averages over the years 2001 to 2005. The average residual is measured in logs. Compared to the 1086 observations in the first step I lose 38 observations due to missing values in the second step variables.

Table 3.3: Oaxaca-Blinder decomposition

Variable	**Coefficient**	**(Std.Err.)**
Unadjusted Differential		
Prediction with council	0.066***	(0.026)
Prediction without council	-0.127***	(0.028)
Output Differential	0.194***	(0.038)
Decomposition		
Endowments	0.142***	(0.046)
Coefficients	0.065	(0.062)
Interaction	-0.013	(0.068)

Notes: *** denotes significance at the 0.01 level. Positive numbers for the decomposition results indicate advantages for the council group. Decomposition evaluated at the council establishments' endowments.

Table 3.4: Oaxaca Blinder decomposition after selection adjustment

Instrument	**SHARE**		**AGE**	
Adjusted output gap	0.31	(0.00)	0.52	(0.00)
Productivity effect	0.26	(0.00)	0.65	(0.00)

Notes: p-values in parentheses.

Chapter 4

Are the Firm Owners really worse off with a Works Council?

4.1 Introduction

Mandatory works councils are widely considered as employers' costly concessions to workers and to the society's political left wing. And indeed, if one assumes perfect markets, a legislation that creates powerful and mandatory works councils must cause efficiency losses and, ceteris paribus, decrease profits. Additionally, as works councils represent workers, it seems to be plausible to presume rent-seeking behavior and, as a result, a redistribution of economic rents from capital owners to workers, which implies a further reduction in profits.

In contrast to this view, recent empirical studies suggest that German

works councils have a non-negative impact on establishment productivity (e.g. Addison et al. (2004), Mueller (2009)). This opens up four possibilities for the councils' effect on profits. First, if councils nevertheless reduce profits then they redistribute more than the additional rents they have created. Second, councils may confine themselves to redistribute only the additional rents and, thus, leave profits unaffected. Third, they do not influence the income shares of capital and labor and are therefore positively related to establishment profits. Fourth, if they are unable to increase wages as strongly as they increase productivity (both in percentages), labor's share shrinks while profits absolutely increase.

Empirical studies support the first possibility and find a negative association of works councils and establishments' profits (e.g., Addison and Wagner (1997), Addison et al. (2001)). Hence, in Germany works councils appear as efficiency-increasing institutions that redistribute economic rents from capital owners to workers. This result is in line with theoretical predictions (Freeman and Lazear (1995)) and explains why powerful works councils do not evolve without a legal mandate.

While the productivity impact of works councils is examined frequently, the literature on its profitability effects is comparatively sparse and dated. Moreover, studies use very different concepts of profitability. The majority uses self-reported subjective evaluations of current profits by the establishments' managers and finds negative council effects (e.g., Addison et al. (2001), Dilger (2002; 2003)). Others construct objective measures from value added and costs (Hübler and Jirjahn (2001)) and some use accounting information (Addison and Wagner (1997)). Only few studies address the endo-

geneity of works councils (Addison and Wagner (1997), Hübler and Jirjahn (2001)), which may arise from self-selection.

In order to obtain a more comprehensive and up to date picture of the councils' impact on profits, I construct several profit measures from the LIAB[1] and use data on medium-sized establishments from manufacturing and service sectors for the years 1996 to 2006. I further estimate the direction of potential endogeneity biases and add the establishment's capital stock as an explanatory variable. As the extent of rent-seeking and productivity-enhancing activities of works councils is likely to depend on whether wages are determined inside or outside the establishment (see Freeman and Lazear (1995) or Hübler and Jirjahn (2003)), the interaction with industry-wide collective wage bargaining is explicitly taken into account.

The results vary with the profitability concept used. While councils' impact on the managers' evaluation of the current profit situation is negative (as reported in previous studies), utilizing objective profit measures yields more positive results. Works councils do not influence whether there is an actual (objective) profit or a loss. They are positively associated with the establishments' quasi rents, i.e. with the difference between value added and labor costs. For both objective measures, the presence of a collective bargaining agreement positively influences the impact of works councils on establishment profits. Furthermore, the results for the objective measures become more positive if potentially endogenous council existence is controlled for. I present evidence that the subjective profit measure is not a reliable proxy for

[1]The Linked Employer-Employee panel data set of the Institute for Employment Research (IAB) in Nuremberg.

actual profits and conclude that works council existence is unrelated to profits in case of no collective wage bargaining and positively related otherwise.

Given higher wages in council establishments (as, for instance, reported in Addison et al. (2001) and Addison et al. (2009)), the positive results for the objective measure indicate that additional rents due to the councils' productivity gains are shared by firm owners and workers. The opposing results from the subjective profit measure could reflect managers' dissatisfaction with works council existence and not works councils' effect on actual profits.

4.2 Institutional Background

The Works Constitution Act (*Betriebsverfassungsgesetz*) is the legal base for German works councils. It codifies the rules for council elections and regulates the rights of the council. A works council is a board of workers who are elected by their colleagues for a period of four years. Councils can be elected in establishments with five or more permanent employees of which at least three have to be eligible for election. A worker is eligible for election if she is with the company for at least six months. Firms that have more than one establishment, additionally have central works councils, which consist of members of establishment-level councils.[2]

German works councils are mandatory because workers have the legal right to establish councils. However, councils do not exist automatically but depend on the initiative of the employees. As the number of employees rises,

[2]For a more in depth treatment of Germany's works council legislation see, for instance, Müller-Jentsch (1995).

the probability of works council existence increases. There are only few small businesses that have a council, while nearly each establishment with more than 300 employees has one.

In general, councils have information rights, consultations rights, veto rights and codetermination rights. With respect to remuneration, the Works Constitution Act gives councils codetermination rights in the introduction of new payment methods, the regulation of overtime, changes of pay groups, and transfers. Additionally, councils may use their veto and codetermination rights in other areas to enforce higher remuneration.

Works councils have no right to organize strikes and the exclusive right of industry-wide collective bargaining remains with unions. Hence, the German system of industrial relations consists of two parts. While unions operate at the industry level, works councils represent *all* workers in a specific plant or establishment, regardless whether they are union members or not or whether their establishment is covered by collective bargaining. Although works councils and unions are formally independent, both are closely linked in reality: most works councillors are union members and often actively recruit new union members while unions devote expertise and financial resources to councils (Behrens (2009)).

4.3 Theory and Literature

Freeman and Lazear (1995) developed a useful framework to model works councils' effects on profits. They assume that the economic rent (R) generated by an organization depends an the rights (x) attributed to the workers.

They further assume that $R(x)$ has an inverted U-Shape. This means that some power of workers increase total rents but too much power decreases it. The parameter τ with $0 < \tau < 1$ is the share in (R) that is given to the workers and $1 - \tau$ is the share of the firm owners. Workers are supposed to use increasing power to achieve a higher share in total revenue, i.e. $\tau'(x) > 0$. Hence, whether profits increase or decrease depends on how strongly $R(x)$ increases (due to increased productivity) *and* on how strongly firm owners' share in rents decreases.

As the number of council rights (x) is determined by law, $\tau(x)$ can only take two values: $\tau(1)$ in case of council presence and $\tau(0)$ in the other case.[3] If $\tau(1) = \tau(0)$ councils do not redistribute economic rents and increased profits are shared by workers and firm owners according to $\tau(0)$. As German works councils may have some rent-seeking power, $\tau(1) > \tau(0)$ is assumed by most researchers.[4]

According to Freeman and Lazear (1995) and empirically supported by Hübler and Jirjahn (2003), collective bargaining outside the establishment – e.g., at the industry level – reduces councils' rent-seeking opportunities. Consequently, $\tau(1) - \tau(0)$ is expected to be smaller for establishments that are covered by such an agreement. Hübler and Jirjahn (2003) and other studies further show that collective bargaining raises $R'(x)$, i.e. increases the

[3]For simplicity I neglect the fact that council rights rise with the number of employees. As I will only consider establishments with more 20 and less than 300 workers, the difference in rights is moderate and this may justify the simplification. Further, the major difference in workers' rights depends on the existence or non-existence of a council and not on employment thresholds. Addison et al. (2001) provide information on employment thresholds and council rights.

[4]Addison et al. (2001) and Addison et al. (2009) find higher wages in establishments with a works council.

productivity effect of councils. Both effects of collective agreements point into the same direction and hence, as a first hypothesis, I expect the works councils' effect on profits to be more positive in the presence of collective bargaining.

A second testable hypothesis refers to the potential endogeneity of works councils due to self-selection. Self-selection exists because the workers have the right to establish a council. Workers will do this if the utility they gain from having the council exceeds the costs they face. If there are unobserved factors that influence both workers' utility and establishments' profits, works councils are endogenous and the estimated council coefficient is biased. Jirjahn (2009) finds that works councils are introduced for rent-protection purposes in times of poor establishment performance, and Mueller (2009) draws a similar conclusion. Hence, as a second hypothesis, it is expected that correcting for self-selection leads to more positive council coefficients.

Previous studies using a self-reported subjective evaluation of profitability as the dependent variable (e.g., Addison and Wagner (1997), Addison et al. (2001), Dilger (2002; 2003)) find a negative relationship between works councils and profits. As the council coefficient in Addison and Wagner (1997) becomes more positive when the works council dummy is instrumented, this result supports my second hypothesis. However, an obvious drawback of the subjective profit measure is its dependence on some unknown reference point of the respondent. I will argue that this measure is not a good measure for actual profits and present evidence for this view at the end of this paper. Hübler and Jirjahn (2001) compute an objective profit measure by taking the difference between value added and labor costs as the dependent variable.

They find that works council existence is not associated with this so called 'quasi rent'. Their results seem to support my first hypothesis but give mixed results with respect to the second one.

Although capital stock is a potentially important omitted variable in all studies mentioned, its omission may be most problematic in Hübler and Jirjahn (2001) because the quasi rent quantifies the income of firm owners and foreign capital lenders jointly.[5] Hence, if capital is not controlled for, the council parameter estimates the councils' effect on the sum of firm owners' and capital lenders' income and not the desired effect on firm owners' profits.

To purge the drawbacks of the approach by Hübler and Jirjahn (2001), I approximate capital stock following the method proposed by Mueller (2008) and use it as a control variable. Much more observations than in any existing study are used, attempts are made to control for the self-selection into the observed council regime, and the interaction with collective bargaining is taken into account. Additionally, I run regressions that are based on the self-reported measure (the same as used by the aforementioned studies) and on two objective profit measures, respectively. In order to discuss the appropriateness of the three measures, I compare the plausibility of their results.

4.4 Data and Descriptive Evidence

This analysis is based on twelve annual waves (1996–2007) of the Linked Employer Employee Panel data set (LIAB) of the Institute for Employment

[5]There are studies with direct information on profits and capital stock (see Addison et al. (1993); FitzRoy and Kraft (1985)). However, these studies are based on very small samples.

Research (IAB). The survey unit is the establishment or local production unit, rather than the legal and commercial entity of the company. Information on employees is provided for June 30 each year.

As works councils in establishments with fewer than 21 employees have few legal rights and because very small establishments may operate systematically different from larger ones, I exclude them from the sample. Nearly all establishments with more the 300 workers have a works councils. To not estimate unobservable size effects instead of works council effects, those establishments are also excluded. Furthermore, only profit-oriented manufacturing and service sectors are considered.[6] I exclude the real estate sector, because capital stock is the predominant input factor in that sector and is difficult to interpret there. As value added, capital stock and other variables have a different meaning in banks and insurance companies than in other sectors, these two sectors are also dropped, and I end up with 6,500 establishment-year observations.

I estimate regressions in which the dependent variable is a self-reported ordinal profit measure: establishments are asked to classify their profit situation into the categories 'very good', 'good', 'satisfying', 'sufficient', and 'insufficient'. This is transformed into a binary variable that is one if the establishment is at least in a good profit situation (35.8 percent of observations) and zero otherwise. Note that the respondents are not asked to compare the profit situation of their firm to the profit situation of firms of similar size, region or industry.

[6]This leads to the exclusion of the following industries: health and social work, sports, culture, entertainment, educational services, sewage, refuse disposal, sanitation, and the unspecified group of "other services".

The difference between value added and labor costs in 1,000 Euros, divided by the number of workers is used as an objective profit measure. Following Hübler and Jirjahn (2001), the difference is called the quasi rent. Value added is total sales minus the costs of intermediate goods and services bought to produce output. Labor costs consist of gross wages and non-wage labor costs. Non-wage labor costs contain employers compulsory contributions to the social security insurance system and accident insurance as well as other costs that may differ by establishments and sectors. As the data contain no information on non-wage labor costs, the latter has to be approximated. I used information about gross income per worker and total labor costs per worker by sector from the national accounts (see Statistisches Bundesamt (2008b)) to compute the average non-wage cost as a percentage of gross income per worker. This percentage is then added to the LIAB information on gross wages to approximate total labor costs.

As an additional objective measure, the 2007 wave of the panel contains information on whether the establishment experienced a profit, a loss or a balanced result in the year 2006. Profit is here defined as revenue minus costs, which is the pre-tax profit. However, it is not explicitly asked for pre-tax profits. The variable is recoded as a binary variable. As more than 80 percent of the establishments report a profit, the variable is 1 if there is a profit and 0 otherwise.

One drawback of this measure, compared to the quasi rent, may arise from the imprecise survey question. If respondents view taxes as costs and therefore report after-tax profits, the objective binary measure is only loosely related to actual profits because firms optimize their tax burden over time.

If the optimizing behavior is randomly distributed over establishments and time, the binary objective profit measure is a noisy but still unbiased measure of actual profits in the year 2006. By contrast, if tax optimization is not randomly distributed over firms and time, then some establishments, for example the establishments with a works council, may move tax burdens to other periods and report less profits in 2006 than they actually had. In this example, the estimated council coefficient would be biased downwards.

The profit variables are regressed on a works council dummy and a set of control variables as described in Table 4.7. Capital stock is approximated using the approach proposed by Mueller (2008). The collective bargaining dummy is 1 if the establishment is covered by an industry-wide collective bargaining agreement or a establishment-level collective agreement that is negotiated between a single employer and an industry-wide union and 0 otherwise.

In order to provide a first grasp on the relationship between $R(1) - R(0)$, $\tau(1) - \tau(0)$,[7] and collective wage bargaining agreements, in Table 4.1 total factor productivity (TFP) and labor's share in value added are compared by collective bargaining agreement for establishments with and without a works council. TFP is calculated for the whole sample of approximately 6,500 establishment-year observations from OLS regression of log value added on the full set of regressors in Table 4.7 omitting the variables works council and bargain. From this regression, log value added is predicted for the establishments in each group of Table 4.1, respectively, and subtracted from observed log value added. Hence, TFP estimates the percentage advan-

[7] $R(x)$ and $\tau(x)$ are introduced in section 4.3.

Table 4.1: Total factor productivity and labor's share

	Works Council	No Works Council
Collective Bargaining	**I** TFP: 6.8 Labor's share: 62.7 Observations: 2339	**II** TFP: -7.8 Labor's share: 65.5 Observations: 1467
No Collective Bargaining	**III** TFP: -1.1 Labor's share: 68.3 Observations: 786	**IV** TFP: -1.7 Labor's share: 65.3 Observations: 2044

Notes: Total factor productivity (TFP) and labor's share in percentages. TFP measures the productivity difference of the average establishment in the respective group to the average establishment in the sample. It is calculated from OLS regression of log value added on the full set of regressors in Table 4.7 omitting the variables works council and bargain (PDImin and PDImax are also excluded because they are not observed each year). From this regression, log value added is predicted for the establishments in each group, respectively and subtracted from observed log value added. Labor's share is the total wage bill (including non-wage labor costs as described above) divided by value added.

tage/disadvantage in productivity that the average establishment in the respective group has, compared to the average establishment in the sample. Labor's share is the total wage bill (including non-wage labor costs as described above) divided by value added.

TFP of group I establishments, i.e. of establishments with both a works council and collective bargaining, is 6.8 percent higher than that of the average establishment in the sample while TFP in group II, where establishments have a collective agreement but no works council, is 7.8 percent lower than in the average establishment. $R(1) - R(0)$ is positive and highest in group I. As labor's share is lowest in that group, establishments that have both a works council and are covered by collective wage bargaining are expected to have the highest profits.

Comparing TFP and labor's share between the two groups without collective bargaining (i.e. cells III and IV) yields the results that one would expect from the Freeman and Lazear (1995) model and previous empirical studies: while TFP is somewhat higher in case of council existence, labor's share is also higher. This indicates some rent-seeking behavior of councils but leaves open the sign of the profitability effect.

Comparing labor's share between the two groups with collective bargaining gives $\tau(1) - \tau(0) < 0$, which indicates redistribution of economic rents toward firm owners. This does, of course, not mean that wages are absolutely lower in the council group – it reflects that the percentage increase in productivity dominates the increase in wages. In case of collective agreements, it follows that if total wage costs in council establishments are higher by some percentage, than this percentage is lower than the productivity advantage of council establishments. Presumably, this is the case because wages are to a considerable extent exogenously determined at the industry level and are therefore not fully adjusted to productivity changes within a group of establishments in that industry that is covered by such an agreement (i.e. the council establishments). Assuming that workers cannot influence whether they are covered by such an agreement and because other firms with similar productivity in that industry are also covered by a collective agreement, workers have few or no higher-wage outside options, and a stable situation with wages below productivity is possible.[8]

[8] As Freeman and Lazear (1995) implicitly assume that a rise in labor productivity translates into an equivalent rise in wages, the situation $\tau(1) - \tau(0) < 0$ is not considered in their model. In absence of a collective agreement, the figures in Table 4.1 do not conflict with their assumption of $\tau(1) - \tau(0) > 0$.

The descriptive evidence in Table 4.1 indicates that, in case of collective bargaining, the amount of rights given to the workers by the German works council legislation leads to a situation where both productivity and profits are higher than in the situation without councils. This supports my first hypothesis from section 4.3 and challenges the hypothesis of works councils' rent-seeking, redistributive, and profit-decreasing nature. As the latter hypothesis is confirmed by previous studies that are based on the subjective profit measure, the results in Table 4.1 also call the appropriateness of this measure into question.

4.5 Methods

The model to estimate is

$$profit_i = \alpha + \beta W_i + \pi X_i + \eta_i \tag{4.1}$$

where $profit$ is a profit measure, W is a dummy indicating works council presence, β is the parameter of interest, X is vector with the control variables described in Table 4.7 and η is an error term.[9]

The parameter β cannot be consistently estimated with ordinary least squares techniques (OLS) if W_i is correlated with η_i. This is the case if unobserved mechanisms influence both the probability of works council existence and the profit measure. A correlation between W_i and η_i exists if

[9] I do not exploit the panel character of the data because the works council dummy is nearly time-invariant and fixed effects estimators would therefore yield imprecise estimates of β. However, fixed effects results and a solution to the time-invariance problem are presented at the end of this section.

the workers' decision to erect or maintain a works council depends on the profit situation. It is assumed that workers erect or maintain a council if the (latent) utility they obtain from doing so (i.e. the difference between benefits and costs) is greater than zero. The latent utility for the workers in establishment i can be described as

$$W_i^* = \gamma Z_i + u_i \tag{4.2}$$

where W_i^* is the latent utility, Z_i contains the regressors X_i from equation (4.1) and, not necessarily, external instruments, γ is the corresponding coefficient vector and u_i is a random error. The observed works council regimes W are

$$W = 1 \quad if \quad W^* > 0$$
$$W = 0 \quad if \quad W^* \leq 0.$$

A model where agents endogenously choose the regime ($W = 1$ or $W = 0$) is an endogenous switching regression model. Endogenous switching regression models can be estimated consistently employing the logic of the Heckman Two Step estimator (see Heckman (1979)). The basic idea in the Heckman Two Step estimator is to add the inverse Mills Ratio as a regressor in the profit equation in order to control for selectivity. However, as endogenous switching regression models estimate one profit regression for establishments with a council and one profit regression for establishments without a council, no council parameter will be estimated. In this situation, an estimate of the

council's effect on profits can be obtained from an Oaxaca-Blinder decomposition (Oaxaca (1973), Blinder (1973)) of the mean difference in profits between both works council regimes.

Alternatively, the approach by Vella and Verbeek (1999) is suitable and will be applied in this study. Vella and Verbeek (1999) argue that an endogenous switching regression model can be estimated with a single output equation containing the regressor of interest and the inverse Mills Ratios that are interacted with the treatment dummy. The profit equation is then

$$\begin{aligned} profit_i = \alpha + \beta W_i + \pi X_i + W \cdot \sigma_1 \left(\frac{\phi(\hat{\gamma} Z_i)}{\Phi(\hat{\gamma} Z_i)} \right) \\ +(1 - W) \cdot \sigma_2 \left(-\frac{\phi(\hat{\gamma} Z_i)}{1 - \Phi(\hat{\gamma} Z_i)} \right) + \epsilon_i \end{aligned} \tag{4.3}$$

where $\hat{\gamma} Z_i$ is the predicted works council probability from equation (4.2). Evaluating the density function $\phi(\cdot)$ and the cumulative distribution function $\Phi(\cdot)$ of the standard normal distribution at $\hat{\gamma} Z_i$ gives the inverse Mills Ratios (in large parentheses).[10] In equation (4.3), σ_1 is the coefficient of the inverse Mills Ratio for the establishments that have a council. The coefficient measures the covariance between the error in the selection equation (4.2) and the error in the profit equation (4.3). On the other hand, σ_2 refers to the establishments that have no council. Both coefficients can be used to describe the selectivity mechanisms.

Although the Heckman Two Step estimator is identified by nonlinearities, valid instruments in the selection equation improve precision. The instrument

[10]For consistency one has to assume that the errors in the selection equation and the outcome equation follow a bivariate normal distribution.

has to be exogenous to the council state but correlated with it, and it is allowed to influence profits via works councils, exclusively. I use the industry share of establishments with a works council and the within-establishment standard deviation of workers' age – both instruments are computed from the sample that is used for regression. While the first instrument is a standard technical instrument, the latter mirrors work force heterogeneity within establishments. I assume that a homogenous workforce is more likely to successfully organize a council election and to maintain the council. On the other hand, the dispersion of workers' age within a establishment is assumed to be uncorrelated with the profit situation. The hypothesis that the coefficients of both instruments are jointly zero is rejected with $\chi^2(2) = 49.96$ and a corresponding p-value $p = 0.000$.

Unobserved time-invariant heterogeneity that is correlated with right-hand side variables may bias the results regardless of whether it is controlled for self-selection into the council regime or not. Unobserved heterogeneity can, for instance, be a difference in the quality of management or in the establishments' worker-management relations. In case of continuous dependent variables (here quasi rent per worker) and panel data, a fixed effects estimator can be used to deal with unobserved heterogeneity. A fixed effects within-estimator uses deviations from within-establishment averages to identify parameters. Hence, time-invariant unobserved heterogeneity is removed from the error term. As this principle applies to all right-hand side variables, the effects of (nearly) time-invariant regressors like works council or collective bargaining cannot be estimated (precisely).

To obtain estimates for time-invariant regressors and to control for po-

tential correlations between time-variant regressors and time-invariant unobserved heterogeneity, a fixed-effects profit regression for all time-varying regressors is carried out in a first step. Then, the within-establishment average of the residual from that regression is computed and these average residuals are used as dependent variables in a second step OLS regression with the time-invariant regressors.[11]

In that approach, the first step equation is

$$quasi_{it} - \overline{quasi}_i = \pi(X_{it} - \overline{X}_i) + error_{it} \tag{4.4}$$

where $quasi_{it}$ is the quasi rent of establishment i in year t, $error_{it}$ is a zero mean white noise error term, and an upper bar denotes the within-establishment average. The influence of W and the influences of other time-invariant regressors in X on profits are now part of the establishment-specific fixed effect.[12] The establishment-specific fixed effect is, in expectation, equal to the average establishment-specific residual from equation (4.4). To obtain the average residual, first, the predicted values for the quasi rent are subtracted from the observed values. This gives the first-step residuals

$$quasi_{it} - (\widehat{quasi_{it}}) = \delta C_i + \nu_i + error_{it}. \tag{4.5}$$

[11] See Black and Lynch (2001) for a very similar approach.

[12] As some variables are not fully time-invariant, they would appear in equation (4.4) although the basic idea of the applied two step approach is to estimate their coefficients in the second step. Consequently, I exclude the following variables that have no or very small within-establishment variation from equation (4.4): works council, bargain, exporter, single, east, size100, size200, industry affiliation, and old. As training and minor and major product innovations are observed only for some panel waves, these variables are also excluded from equation (4.4).

where C_i are the regressors that have been excluded from equation (4.4) and ν_i is the remaining establishment-specific fixed effect. Next, I average the first-step residuals over the whole sample period for each establishment to get an estimate of the establishment-specific time-invariant component of the first step residual R_i with $R_i = \frac{1}{T}\sum_t quasi_{it} - [\widehat{quasi_{it}}]$.[13] The second step equation is then

$$R_i = \delta C_i + \nu_i + \widetilde{error}_i. \tag{4.6}$$

The virtue of this approach is to obtain estimates for the time-invariant regressors while controlling for potential correlations between unobserved heterogeneity and time-variant regressors. Nevertheless, an estimation of equation (4.6) suffers from the same potential self-selection problem that arises if equation (4.1) is estimated directly. To control for the selectivity, the same procedure as applied to equation (4.1) can also be applied to equation (4.6). The only differences are that the dependent variable is now the average first-step residual R_i instead of the quasi rent, and that X_i is now C_i and contains only the time-invariant regressors, and that Z_i, in addition to C_i, contains the within-establishment averages of the two instruments described above.

An Oaxaca-Blinder decomposition (Oaxaca (1973), Blinder (1973)) reveals additional insights like the share in the total quasi rent difference between the two groups of establishments with and without a council that cannot be explained by different endowments. To estimate this and to use

[13] As $error_{it}$ is assumed to have zero mean, it goes to zero when T increases.

the advantages of the two step procedure, the Oaxaca-Blinder decomposition is applied to equation (4.6), too. Additionally, selectivity is controlled for by subtracting $\hat{\sigma}_1 \left(\frac{\phi(\hat{\gamma} Z_i)}{\Phi(\hat{\gamma} Z_i)} \right)$ from the quasi rent residuals of council establishments (i.e. R_{1i}) and $\hat{\sigma}_2 \left(-\frac{\phi(\hat{\gamma} Z_i)}{1-\Phi(\hat{\gamma} Z_i)} \right)$ from the quasi rent residuals of establishments without a council (i.e. R_{2i}) before carrying out the decomposition.[14] Here, $\left(\frac{\phi(\hat{\gamma} Z_i)}{\Phi(\hat{\gamma} Z_i)} \right)$ and $\left(-\frac{\phi(\hat{\gamma} Z_i)}{1-\Phi(\hat{\gamma} Z_i)} \right)$ are computed from equation 4.2 where Z_i consists of C_i and the within-establishment averages of the two instruments described above. The $\hat{\sigma}_1$ and $\hat{\sigma}_2$ are obtained by estimating

$$R_{1i} = \alpha_1 + \delta_1 C_i + \sigma_1 \left(\frac{\phi(\hat{\gamma} Z_i)}{\Phi(\hat{\gamma} Z_i)} \right) + \epsilon_{1i} \quad if \quad W = 1 \tag{4.7}$$

$$R_{2i} = \alpha_2 + \delta_2 C_i + \sigma_2 \left(-\frac{\phi(\hat{\gamma} Z_i)}{1 - \Phi(\hat{\gamma} Z_i)} \right) + \epsilon_{2i} \quad if \quad W = 0. \tag{4.8}$$

As a result, the Oaxaca-Blinder decomposition decomposes the difference between the means of the selectivity adjusted average first-step residuals (which reflect the quasi rent) of the group with a council and the group without a council. Assuming valid instruments and that ϵ_{1i}, ϵ_{2i}, and the error of the selection equation are trivariate normally distributed, the selectivity-adjusted Oaxaca-Blinder decomposition gives a consistent estimate of the profitability effect of works councils. Acknowledging that these assumptions may not be completely fulfilled, I will only rely on the estimated sign of the effect and not on its magnitude.

[14] See Mueller (2009) for a more in depth treatment of this issue.

4.6 Results

To estimate the effect of works councils on profits and to test the two hypotheses derived in section 4.3, for each dependent variable four regressions are reported:

1. a baseline regression without the council–bargaining interaction and without selectivity control,

2. a regression without the interaction but with control for the endogeneity of the council dummy that may arise due to self-selection (tests the second hypothesis), and

3. two regressions with selectivity controls that are separated by the respective collective bargaining status (tests the first hypothesis).

4.6.1 Subjective Profit Measure

The results for the estimations using the self reported measure are presented in Table 4.2. Just like, for instance, in Addison et al. (2001), works council presence is negatively correlated with this profit measure. Surprisingly, the negative effect is stronger after controlling for selectivity. Also unexpectedly, works councils have more negative effects on profits in case of collective bargaining. Hence, both hypotheses are rejected with the subjective measure. The marginal effect of works councils is insignificant though, regardless of the specification.

According to the coefficient of the East/West dummy, East German managers are significantly more satisfied with the profit situation of their estab-

lishment than West German managers. Obviously, this does not reflect the actual profit situation of the establishments. My results for the objective measures in Table 4.3 and Table 4.4 show that East German establishments earn less. The same is true for the establishment size indicators: smaller establishments are more satisfied with their profit situation although, according to the results in Table 4.3, their quasi rent per worker is significantly lower.

The coefficients of the East/West dummy and the establishment size dummies reveal a fundamental problem with this self-reported measure: The researcher does not know the reference point of the respondent. Respondents may condition their answer, for example, on the general situation of their establishment, on the situation of their nearby competitors, on last years profits or on something else that is unknown to the researcher. Hence, an East German manager may report a good profit situation because the establishment earns more than other East German establishments and not because its earnings are really 'good' – whatever 'good' means. Because of these problems, and the puzzling coefficients for the East/West and establishment-size dummies, and because the results of the selectivity control and the bargaining interaction are not in line with theoretical expectations, I conclude that the self-reported evaluation of the profit situation is a poor measure for the establishments' real profit situation.[15]

[15]Studies that compare subjective and objective measures of firm performance typically find a strong positive correlation between both types of measures and conclude that both are valid indicators for firm performance (see, for instance, Dess and Robinson (1984) or Wall et al. (2004)). Note that in these studies the survey questions that produce the subjective measures of firm performance dictate reference points to the respondent – questions typically ask for performance "in comparison to firms of similar size, region and industry". Recall that these reference points are not given in the IAB establishment panel

4.6.2 Objective Profit Measures

Table 4.3 provides regression results for the quasi rent per worker as a measure of profitability. The quasi rent is that part of value added that is earned by capital owners, i.e. by the firm owners and by outside capital providers. Assuming the same costs per unit of capital for each establishment, the ceteris paribus effect of works councils on the quasi rent per worker is equal to the effect on establishment profits.[16]

The council effect is positive throughout all specifications. The effect is of significant magnitude and, except for column 4, statistically highly significant. As expected from theoretical considerations, the council coefficient is highest in the presence of a collective bargaining agreement.[17] The selectivity control, as expected, considerably increases the positive effect of councils.[18] However, because of the well-known weaknesses of all non-experimental instrumental variable approaches, the magnitude of the estimates in columns

survey. I conjecture that the absence of reference points in the survey question is the main reason for the poor performance of the subjective profit measure in my study.

[16]Of course, the shares of borrowed capital and equity in total capital differ across establishments. One could argue that equity is priced differently than borrowed capital and object that the ceteris paribus effect of councils on quasi rent may therefore be a biased estimate of councils effect on profits as soon as the debt to equity ratio is systematically different for establishments with and without a council. However, the assumption of equal costs per unit of capital becomes less restrictive if equities are viewed as investments of firm owners that are priced with a competitive interest rate. In that view, the ceteris paribus effect of councils (i.e. holding capital fixed) measures the influence of councils on that part of profits that exceeds the revenues from competitively priced capital. Finally, assuming competitively priced borrowed capital, the ceteris paribus effect measures the councils' effect on that part of firm owners' surplus that goes beyond the revenue from competitively priced capital.

[17]This also holds if separate regressions are performed for East and West Germany and for the subgroups of establishments with no more than 100 employees and with more than 100 employees, respectively.

[18]The hypothesis that the coefficients of both instruments in the selection equation are jointly zero is rejected with $\chi^2 = 49.96$.

2 to 4 should not be taken to literally. What can be learned from these estimates is the unambiguous direction of the selectivity bias.

The results for the binary objective measure are presented in Table 4.4. The estimates are generally very imprecise. This may be caused by the small sample size or noise in the dependent variable. Such noise may come from tax optimizing behavior of establishments, e.g., from postponing or moving forward tax burdens as discussed in section . Because of the imprecision of the results, I will only comment on their general tendencies rather than interpreting single estimates. In general, the results resemble those obtained from the quasi rent regressions: The works council effect increases if endogeneity issues are taken into account[19] and works councils are more positively related to profits in case of collective wage bargaining.

4.6.3 Unobserved Heterogeneity

In the previous section it is argued that the subjective measure is not appropriate because, among other things, answers depend on unknown reference points. To defend the results obtained by previous studies that used the subjective measure, one could argue that managers may, for instance, report to not be satisfied with actually good profits because they, for some reason, *know* that their establishment would perform better if there would be no council. This means that the unknown reference point for managers in council establishment is the situation without a council. In that case, the subjective measure may reflect the causal effect of councils while any objective measure

[19]The hypothesis that the coefficients of both instruments in the selection equation are jointly zero is rejected with $\chi^2 = 13.91$.

is biased upwards due to unobserved establishment characteristics that lead to higher productivity and increase council probability.

The following results are obtained after controlling for potential correlations between time-variant regressors and unobserved heterogeneity as described in section 4.5. The second step results, which are based on equation (4.6), are presented in Table 4.5.[20] Supporting the results in Table 4.3, works council existence is positively associated with quasi rent per worker through all specifications. However, the effect is now insignificant. Similar to the results in the previous section, the interaction with collective bargaining is important, and the selectivity correction slightly increases the effect.

The results of the Oaxaca-Blinder decomposition of the unadjusted mean difference in the averaged first step residuals between the group of establishments with a council and the group of establishments without a council are presented in Table 4.6.[21] The mean difference in the residual is approximately 9,100 Euro per worker in favor of establishments with a council and approximately 40 percent of that difference cannot be explained by different endowments. Hence, a quasi rent of approximately 3,800 Euro per worker can not be explained by the establishments' endowments and is therefore attributed to council existence.

Conducting the selectivity correction of the mean difference in the averaged first step residuals according to the procedure presented in the previous section leads to the results presented in the lower part of Table 4.6. I use the

[20]Variables that do not appear in the table but appear in Tables 4.2, 4.3 and 4.4 are time-variant and therefore used in the first step fixed effects regression. As all variables in Table 4.5 are within-establishment averages, information on product innovation, which is available only for two waves of the panel, is included without loss of observations.

[21]I apply the threefold decomposition following Daymont and Andrisani (1984).

same instruments as before, and, as expected, the difference in the quasi rents increases (11,700 Euro). Now the unexplained part of the difference amounts to 13,300 Euro per worker. For the same reasons as in the previous section, I will not rely on the point estimates of the adjusted decomposition but on the direction of the bias. To sum up, the positive influence of councils on profits remains if unobserved heterogeneity and self-selectivity of establishments is taken into account.

I conclude that the sign of the council effect on the quasi rent and my conclusions with respect to the two hypotheses remain unchanged after both controlling for correlations between unobserved establishment characteristics and time-varying regressors and, at the same time, correcting for self-selection into the council regime.

4.7 Summarizing Discussion

Are the firm owners really worse off with a works council? Are works councils a rent-seeking and redistributive part of Germany's labor relations legislation?

Many researcher take the assumptions of Freeman and Lazear's (1995) works council model and predict that councils may probably increase productivity but surely decrease profits. The latter prediction is supported by the observation of higher wages in council establishments and may also be inspired by the widespread notion that automatically links employee associations with rent-seeking behavior. But what if, on average, works councils *surely* increase productivity and *maybe* engage in rent-seeking activities? In

that case, one would expect that councils increase the pie and leave the piece of the firm owners unaffected or even make it larger.

A further look at the Freeman and Lazear (1995) model shows that the extent of rent-seeking very likely depends on whether distributional conflicts are solved inside or outside the establishment, e.g., at the industry level. If councils operate in an establishment that is covered by an industry-wide collective bargaining agreement, their profitability effect may be positive because in that case, wages are (to some extent) exogenous at the establishment level and councils have little rent-seeking possibilities. Additionally, their engagement in productivity-enhancing practices is increased (see Hübler and Jirjahn (2003)).

To shed light on the councils' effect on profits, I estimate regressions for three different profit measures as dependent variables. Further, I account for the self-selection with respect to works councils, unobserved heterogeneity, and the interaction with the existence of a collective bargaining agreement.

The first finding is that the widely used subjective profit evaluation by the establishments' managers is a poor measure of actual profits and hardly appropriate as a dependent variable in a profit regression. It yields clearly implausible results – for example, the estimated effect of being located in East Germany is positive and significant – and the *changes* in results that occur due to endogeneity corrections and the interaction with collective bargaining differ diametrically from changes obtained with objective profit measures. This casts doubt on the results of previous studies in which this measure is applied and which are used to show the rent-seeking behavior of works councils.

Estimations with objective measures yield mainly non-negative coefficients for the councils' influence on profits, which strongly increases if the establishment is covered by a collective bargaining agreement. As expected from previous studies (Jirjahn (2009), Kraft and Lang (2008), Mueller (2009)), the correction for potential self-selection with respect to council existence further increases the positive council parameter.

The results of this study stand in contrast to the widespread notion according to which councils redistribute economic rents from firm owners to workers. I suggest an explanation for *why* previous studies found other results: It is plausible to assume that the subjective profit measure, applied in previous studies, is a bad measure for actual profits.

4.8 Tables

Table 4.2: Dependent variable: managers' evaluation of previous year's profits, 1=good or very good; 0=otherwise

Variable Name	baseline	Heckman Two Step	bargain = 1	bargain = 0
Works council	-0.022	-0.049	-0.072	0.000
Bargain	-0.025	-0.003	-.-	-.-
IMR*Works council	-.-	0.036	0.038	0.015
IMR*(1-Works council)	-.-	-0.001	0.030	-0.036
Employees	0.054**	0.082**	0.074	0.088*
Capital Stock	0.001	-0.003	-0.003	-0.004
Tech2	-0.053***	-0.047**	-0.082***	-0.011
Tech3	-0.142***	-0.150***	-0.188***	-0.119***
Tech4	-0.167***	-0.214***	-0.193***	-0.295***
Exporter	0.059***	0.063***	0.035	0.086**
Single	-0.001	0.007	-0.002	0.030
Temporary workers	0.004**	0.003*	0.002	0.006**
Female workers	0.001	0.000	-0.000	0.000
Part-time workers	0.000	0.001	0.000	0.001
Apprentices	0.002	0.001	-0.003	0.005*
Skilled workers	0.000	-0.000	-0.001	0.000
Churning	0.001	0.001	0.001	0.000
Training	0.058	0.063	0.046	0.044
East	0.056**	0.083***	0.098**	0.060
Size100	0.073	0.073	0.062	0.083
Size200	0.028	0.021	0.000	0.042
Old	-0.011	0.007	0.049	-0.029
Overtime	0.067***	0.059***	0.009	0.120***
Work hours	-0.003	-0.006	-0.015	0.008
Observations	6,648	4,735	2,546	2,184

Notes: All results are marginal effects after Probit. Year and sectoral dummies included. *;**;*** denote significance at the 10 percent, 5 percent or 1 percent level, respectively. Standard errors are clustered by establishments. IMR is the inverse Mills Ratio. Columns 2 – 4 are second step probit regressions (equation (4.3)) from an endogenous switching regression following Vella and Verbeek (1999) – column 2 for the pooled sample, columns 3 and 4 separated according to collective bargaining regime.

Table 4.3: Dependent variable: quasi rent per worker in 1,000 Euro

Variable Name	**base-line**	**Heckman Two Step**	**bargain = 1**	**bargain = 0**
Works council	3.2***	12.4**	22.1***	3.7
Bargain	2.0**	2.7**	-.-	-.-
IMR*Works council	-.-	-6.1**	-8.1*	-4.5
IMR*(1-Works council)	-.-	-5.6	-14.2**	-0.3
Employees	-7.0***	-7.9***	-8.8***	-6.8**
Capital Stock	4.3***	4.1***	3.7***	4.7***
Tech2	-3.0**	-3.9***	-2.4	-5.5***
Tech3	-5.6***	-7.7***	-7.4***	-7.6***
Tech4	-12.4***	-17.9***	-18.0***	-18.2***
Exporter	5.2***	5.8***	5.2***	7.0***
Single	-7.5***	-5.9***	-8.7***	0.2
Temporary workers	0.2**	0.1	0.1	0.1
Female workers	-0.1***	-0.1*	-0.1	-0.1
Part-time workers	-0.0	-0.0	0.0	-0.1**
Apprentices	-0.2***	-0.2**	-0.2	-0.2
Skilled workers	0.0	-0.0	-0.1**	0.0
Churning	-0.1***	-0.1**	-0.0	-0.0
Training	4.6*	-1.4	0.6	-6.1*
East	-4.0***	-4.3***	-2.6	-6.3***
Size100	-8.5***	-7.7***	-8.5**	-6.4
Size200	-6.1***	-6.8***	-7.6***	-7.1*
Old	-2.6**	-3.9***	-3.6	-4.4**
Overtime	-0.6	-0.5	0.7	-2.1
Work hours	-0.4*	0.2	0.6	-0.2
Observations	6,648	4,735	2,548	2,187

Notes: Year and sectoral dummies included. *;**;*** denote significance at the 10 percent, 5 percent or 1 percent level, respectively. Robust standard errors. IMR is the inverse Mills Ratio. Columns 2 – 4 are second step OLS regressions (equation (4.3)) from an endogenous switching regression following Vella and Verbeek (1999) – column 2 for the pooled sample, columns 3 and 4 separated according to collective bargaining regime.

Table 4.4: Dependent variable: managers report on previous years' profits, 1=positive; 0=negative or balanced

Variable Name	**base-line**	**Heckman Two Step**	**bargain = 1**	**bargain = 0**
Works council	-0.10***	0.00	0.26	-0.16
Bargain	-0.03	-0.02	-.-	-.-
IMR*Works council	-.-	-0.03	-0.23**	0.12
IMR*(1-Works council)	-.-	-0.10	-0.21	-0.06
Employees	0.08**	0.06	-0.04	0.12*
Capital Stock	0.01	0.02	-0.00	-0.00
Tech2	-0.02	-0.01	-0.00	-0.04
Tech3	-0.13**	-0.13**	-0.16***	-0.14
Tech4	0.02	-0.04	-0.21*	-.-
Exporter	0.03	0.05	0.04	0.08*
Single	-0.01	0.00	0.04	0.00
Temporary workers	0.01***	0.01***	0.01***	0.00***
Female workers	0.00	0.00	0.00	-0.00
Part-time workers	0.00	0.00	0.00	-0.00
Apprentices	0.00	0.00	0.00	0.01
Skilled workers	-0.00	-0.00	-0.00	0.00
Churning	-0.00	0.00	0.00	-0.00
Training	0.02	0.03	-0.07	0.10
East	-0.01	-0.03	-0.04	-0.06
Size100	0.05	0.05	-0.00	0.10
Size200	-0.08	-0.08	-0.12	-0.01
Old	-0.00	-0.02	0.03	-0.09
Overtime	0.02	-0.01	-0.05	-0.00
Work hours	-0.01	0.00	0.01	-0.00
Observations	1,037	956	486	449

Notes: All results are marginal effects after Probit. Year and sectoral dummies included. *;**;*** denote significance at the 10 percent, 5 percent or 1 percent level, respectively. IMR is the inverse Mills Ratio. Columns 2 – 4 are second step probit regressions (equation (4.3)) from an endogenous switching regression following Vella and Verbeek (1999) – column 2 for the pooled sample, columns 3 and 4 separated according to collective bargaining regime.

Table 4.5: Second step results of two step approach, dependent variable: quasi rent per worker in 1,000 Euro

Variable Name	**base-line**	**Heckman Two Step**	**bargain = 1**	**bargain = 0**
Works council	2.9	9.8	11.0	8.0
Bargain	2.3	-0.2	-.-	-.-
IMR*Works council	-.-	-6.5	-6.8	-4.4
IMR*(1-Works council)	-.-	-0.9	-0.7	-3.3
Exporter	11.8***	11.1***	9.0**	15.0***
Single	-11.5***	-10.8***	-13.3***	-3.7
Training	8.7*	7.6*	10.0	0.7
East	-1.6	-1.8	-2.1	-1.6
Size100	-13.1***	-10.7***	-13.1***	-8.4
Size200	-5.6	-5.6	-7.3	-5.1
Old	-1.0	-1.3	-2.9	-0.1
PDImax	-4.8**	-4.9*	-3.6	-5.6*
PDImin	2.4	2.7	3.1	2.6
Observations (= establishments)	1,722	1,703	956	747

Notes: OLS; all variables are within-establishment averages. Year and sectoral dummies included. *;**;*** denote significance at the 10 percent, 5 percent or 1 percent level, respectively. Robust standard errors. IMR is the inverse Mills Ratio. Columns 2 – 4 are second step OLS regressions (equation (4.3) applied to the within establishment average of the residuals from equation (4.4)) from an endogenous switching regression following Vella and Verbeek (1999) – column 2 for the pooled sample, columns 3 and 4 separated according to collective bargaining regime.

Table 4.6: Oaxaca-Blinder decomposition of second step differential, dependent variable: quasi rent per worker in 1,000 Euro

Variable		**(Std.Err.)**
Unadjusted Differential (1,722 Obs.)		
Prediction with council	5.9***	(1.4)
Prediction without council	-3.2***	(1.0)
Output Differential	9.1***	(1.7)
Decomposition	**Coefficient**	
Endowments	9.4***	(2.0)
Coefficients	3.8	(2.6)
Interaction	-4.1	(2.7)
Adjusted Differential (1,703 Obs.)		
Prediction with council	2.8**	(1.4)
Prediction without council	-8.9***	(1.0)
Output Differential	11.7***	(1.7)
Decomposition	**Coefficient**	
Endowments	13.2***	(2.1)
Coefficients	13.3***	(2.7)
Interaction	-14.8***	(2.8)

Notes: Robust standard errors in parentheses. *,**,*** denotes significance at the 10, 5 or 1 percent level, respectively. Positive numbers for the decomposition results indicate advantages for the council group. Decomposition evaluated at the council establishments' endowments.

Table 4.7: Variable description

Variable Name	Mean	Description
Works council	0.471	1 if works council exists
Bargain	0.574	1 if covered by a collective bargaining agreement between employer and union
Employees	4.273	log of the total number of employees
Capital Stock	14.583	log of capital stock value
Tech1 (reference)	0.232	1 if capital stock is state of the art, self-evaluated relative to competitors
Tech2	0.496	1 if technology of capital stock is one category worse than Tech1
Tech3	0.249	1 if technology of capital stock is two categories worse than Tech1
Tech4	0.023	1 if technology of capital stock is three categories worse than Tech1
Exporter	0.480	1 if the establishment exports
Single	0.723	1 if the establishment does not belong to a group of affiliated companies
Temporary workers	2.03	in percent of total employment
Female workers	33.09	in percent of total employment
Part-time workers	12.80	in percent of total employment
Apprentices	5.43	in percent of total employment
Skilled workers	69.30	in percent of total employment
Churning	5.17	in percent of total employment
Training	0.196	persons participating in employer-provided training programs, share in total employment
East	0.410	1 if located in Eastern Germany
Size100	0.658	1 if at most 100 employees
Size200	0.239	1 if at most 200 employees but more than 100
Old	0.656	1 if the establishment existed prior to 1990
Overtime	0.636	1 if overtime work
Work hours	38.74	regular weekly full-time hours
PDImax	0.612	0 to 2; indicates major product innovations
PDImin	0.128	0 to 2; indicates minor product innovations

Notes: Statistics for 6,648 observations of the regressions in column 1 of Tables 4.2 and 4.3. The capital stock is computed following Mueller (2008). Skilled workers are craftsmen who have at least two years of formal professional education, or other employees who perform qualified tasks, i.e. university graduates. The churning rate is computed as in Burgess et al. (2000) and measures personnel fluctuations that leave total employment unaffected. Product innovations are asked retrospective for two years in 2001 and 2004 (statistics reported for 1722 observations). The index for PDImax is 2 if the establishment had major product innovations in both periods, it is 1 if it has it in one period and zero if there was no major product innovation. PDImin is coded equally for minor product innovations.

Chapter 5

Conclusive Remarks

5.1 Summary

This dissertation studies the consequences that German works councils have on firm-level productivity and profits. It provides new insights and up-to-date empirical evidence on these questions and, additionally, suggests a new method for capital stock approximation with short establishment panel data. The focus is on the endogeneity of works councils, the interaction effects between councils and collective bargaining, and on errors in the measurement of capital and profits. The dissertation consists of three self-explaining studies: the first proposes the new capital stock approximation method, the second analyses the productivity effect of works councils, and the third examines their impact on profits and the distribution of economic rents.

The study "Capital Stock approximation with Short Panels" shows that the often practiced capital stock approximation using only previous years replacement investments is inappropriate. This is because reported replace-

ment investments are extremely volatile. Hence, although replacement investments may be a good proxy for capital stock in the long run, their volatility in the short run will enter estimation as measurement error. Not surprisingly, previous capital-related estimates that are based on this measure are insignificant and close to zero. I propose a new method that produces much better results in cases where only the within-firm dimension of the data is used for estimation. The advantage vanishes if the between dimension is added, such as in OLS estimations. This study implies two aspects. First, previous studies using panel estimators and the old approximation method do virtually not control for capital stock. Second, as soon as panel estimators are used, the new method should be applied.

The two studies on works councils have implications for both politics and further research. The discussion on policy implications is given at the end of this chapter. In "The Productivity Effect of Nonunion Representation", I show that medium-sized manufacturing firms with a works council are on average 6.5 percent more productive than its counterparts without a council. Existing literature is extended by using a better capital stock approximation method as described above and the joint application of empirical tools that allow to deal with different econometric problems; namely: the estimation of the coefficient of a time-invariant firm characteristic in presence of unobserved heterogeneity, simultaneity and a (self-)selection into treatment that is based on unobservables. Further, and to my best knowledge for the first time in the works council literature, instruments are drawn from linked employer-employee data. These instruments aggregate such employee characteristics at the firm level that may explain council existence but not productivity. An

endogenous switching regression model based on these instruments shows that the productivity estimate of 6.5 percent underestimates the causal effect and, hence, leads to the conclusion that the causal effect of councils on productivity is positive. This direction of the self-selection bias is found for every combination of instruments and indicates that works councils are founded in firms that face difficult economic circumstance. Jirjahn (2009) supports this view and explains it with rent-protection motives of workers.

The last study sheds light on the connection between productivity and profits in case of works councils existence. Previous studies with large-scale data sets almost exclusively rely on the managers' subjective evaluation of profits as a dependent variable and uniformly report a negative association with works councils. The first result of my study is that this measure is an inappropriate measure of actual profits. The implications of this result are straightforward: the results of previous studies can only be interpreted as a negative association of managers subjective views and council existence and do not show a negative council effect on profits. A second and surprising result comes from simple descriptive analysis: labor's share in total income is lowest in the group of firms that have both a works council and are covered by a collective bargaining agreement. As those firms are also the most productive ones, profits must be highest in that group. This supports previous studies that find that the coexistence of councils and unions in Germany assures that works councils have little rent-seeking opportunities in firms that are also covered by collective agreement and rather focus on productivity-increasing practices in such firms (Hübler and Jirjahn (2003)). New is the insight that in the group of firms that are covered by both institutions, works

councils are not able to increase wages by the same percentage as they increase productivity. Regression analysis strongly supports the inference of the descriptive part. The dependent variable is the quasi rent, which is value added minus total labor costs, i.e. the share in value added that capital earns. Councils increase quasi rents in firms covered by collective bargaining and leave it unaffected otherwise. As it is controlled for capital stock, this result for quasi rent as dependent variable would also hold for profit as the dependent variable as long as council firms face the same user costs per unit of capital.

5.2 Limitations, Implications, and Extensions

What are the political implications of the evidence presented in the two works council studies? The value of political advices crucially depends on whether the advice can be given on grounds of a causal effect or not. In empirical research, a causal effect is an unbiased estimate of the true impact that a treatment has on the outcome of a treated subject. In this dissertation, the treatment is works council existence, the subjects are the establishments, and the outcomes are productivity and profits. Even if a causal effect has been identified for a certain subpopulation (or period in time), it is not always clear whether the effect is the same for other subpopulations (or time periods). To appraise the value of the political advice given later in this section, I first discuss the limitations of my study with respect to causality and external validity.

Although one important aim of this dissertation is to assess the causal

effect of works councils on productivity,[1] there are severe limitations for this kind of empirical research. Generally, an experiment that randomly assigns works councils to establishments would allow causal interpretation of the estimated council parameter. As there was no such experiment, the researcher has to rely on econometric techniques that try to mimic random assignment. The identification of a causal effect is possible if there are no *unobserved* factors that are correlated with council existence – something that is assured by random assignment of councils to firms. As these factors are unobserved, the identifying assumption of uncorrelatedness can not be verified in absence of random assignment. Hence, as long as there is no randomized experiment, no unambiguous causal effect of works councils can be estimated.

In this dissertation, I mimic random assignment via instrumental variables. In that approach, one has to assume that the instrument is uncorrelated with unobserved factors that also influence productivity.[2] As the identifying assumptions can not be tested, I corroborate their validity by using different instruments that have been selected on the grounds of plausibility considerations. I then check whether the results obtained with the various instruments always differ in the same way from the result that is obtained without imitating random assignment. For the productivity and profit estimates, the results with instrumental variables are always more positive than the results without instrumental variables. I conclude to have found

[1] In principle, this holds also true for the profitability estimates. However, in the profit study I primarily focus on finding the correct proxy for profits than on estimating the causal effect.

[2] For identification in my approach, there are also distributional assumptions with respect to the error terms that have to be fulfilled.

robust evidence for a positive productivity effect and a non-negative effect on profits.

The external validity of my results may be limited. The effects are estimated for a sample of establishments with more than 20 but no more than 300 employees. It is not answered whether the results are the same for smaller or larger establishments. Further, there are indications for a change in council effects over the last three decades. This conjecture is based on the results of qualitative studies (see Kotthoff (1994)) and the observation that recent empirical studies found more positive results than older ones. It is possible that council effects will change further in future periods.

The following political advice is subject to the limitations and criticisms mentioned above.

One central result of my dissertation is that the employee representation via works councils has (on average) a positive effect on establishment productivity in Germany. Therefore, this result supports the proponents of mandated works councils and employee-representation – from an efficiency perspective, the existing legal and institutional framework has to be evaluated as useful and worth preserving. However, it is not possible from my results to assess which gradual changes in the framework would improve or deteriorate the productivity effect of councils. Further, I found no negative effect on profits. It is, generally, difficult to evaluate distributional outcomes. However, as long as profits are viewed as a source of investments and future prosperity, this outcome is desirable from an economic point of view.

The works council effects on productivity and profits crucially depend on whether the establishment is also covered by a collective bargaining agree-

ment or not. If it is covered, both works council effects are substantially more positive. Hence, policies that affect the coverage by collective agreements[3] do also affect the effectiveness of the works councils apparatus.

The dual system of German industrial relations, consisting of works councils and unions, should also be evaluated against the background of international competitiveness. Qualitative research (Sorge and Streeck (1988)), supported by previous empirical studies and this dissertation suggests that the German system of industrial relations sets incentives for German firms to invest in training (Gerlach and Jirjahn (2001)) and labor productivity, which leads to a competitive strategy with high skills, high technology, high productivity, and high wages. A policy that strengthens works councils and unions supports this competitive strategy of the German economy.

Future research should look deeper into the consequences that globalization has on the work and the efficiency of German works councils. Are works councils complementary or conflictive with respect to foreign ownership of German establishments? Further, there is little evidence on the effects councils have in the service sector[4] and in very small establishments. Little is known on whether works council effects are more positive in more productive or in less productive establishments.[5] However, whether works councils really *cause* all the beneficial effects is still the most pressing question. To come closer to an answer, more research should be devoted to the exact circumstances of works council formation and to the changes in the establishment that accompany and follow this event.

[3]These may e.g. be policies that affect the power of unions.
[4]See Wagner et al.(2006) for a study on that issue.
[5]See Wagner et al. (2006) for a quantile regression approach.

This dissertation also revives an old question: Why must there be a mandate in order to have works councils? The standard answer so far was "because they decrease profits and are efficiency increasing" and bases on the model of Freeman and Lazear (1995) and previous empirical studies. As I found that councils do not affect profits negatively, this answer can not be taken to explain the need for a legal mandate.

I suggest two alternative economic explanations: the first is risk aversion of managers and the second is a short-term horizon of managers. Although the council effect on profits is positive on average, it can be negative for certain establishments. Hence, a risk-averse manager may refuse the introduction of councils. A refusal becomes more likely the smaller the average profit gain of councils is, the higher the variance in the profit effect is, and the stronger the degree of risk aversion is. If managers have a short-term horizon and if councils have non-positive effects in the short run, managers will refuse the introduction of councils. To give answers to the mandate-puzzle, future research should distinguish between short-term and long-term effects of councils – e.g. by the use of distributed lag models.

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