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Do Works Councils Inhibit Investment?

John T. Addison*

Thorsten Schank[†]

Claus Schnabel[‡]

Joachim Wagner**

*University of South Carolina, Queen's University (Belfast, U.K.), and University of Coimbra/GEMF, Portugal,

[†]Institute of Economics, University of Erlangen-Nuernberg,

[‡]Institute of Economics, University of Erlangen-Nuernberg,

**Institute of Economics, University of Lueneburg,

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John T. Addison, Thorsten Schank, Claus Schnabel, and Joachim Wagner

Abstract

Theory suggests that firms confront a hold-up problem in dealing with workplace unionism: unions will appropriate a portion of the quasi-rents stemming from long-lived capital. As a result, firms may be expected to limit their exposure to rent-seeking by reducing investments. The U.S. evidence points clearly in this direction. The authors of this paper investigate whether the same is true for German works councils, the analogue of workplace unionism in that nation. Using establishment panel data for the years 1998-2003, they find no evidence that a works council's formation adversely affected investment or that its dissolution favorably affected investment.

KEYWORDS: works councils

DO WORKS COUNCILS INHIBIT INVESTMENT?

JOHN T. ADDISON, THORSTEN SCHANK,
CLAUS SCHNABEL, and JOACHIM WAGNER*

Theory suggests that firms confront a hold-up problem in dealing with workplace unionism: unions will appropriate a portion of the quasi-rents stemming from long-lived capital. As a result, firms may be expected to limit their exposure to rent-seeking by reducing investments. The U.S. evidence points clearly in this direction. The authors of this paper investigate whether the same is true for German works councils, the analogue of workplace unionism in that nation. Using establishment panel data for the years 1998–2003, they find no evidence that a works council's formation adversely affected investment or that its dissolution favorably affected investment.

In the present paper, we investigate the impact of the German works council on investment. The works council is the counterpart of the workplace union in other countries, where considerable attention has focused on the hold-up question: the insight that unions may reduce and distort investments in physical capital if investors anticipate union rent-seeking. But the very different institutional context in which the works council is embedded may make for important differences between it and workplace unionism in Anglo-Saxon countries. In particular, as we shall see, it has been argued that the dual system of industrial relations in Germany—collective agreements between unions and employers at the sectoral level and participation in decision making (co-determination) at the plant level through

works councils—may allow a decoupling of the factors that govern production on the one hand and distribution on the other. Moreover, representative participation in the decision-making process via the works council might encourage workers to take a longer-run view of their companies. That being said, the wide jurisdiction enjoyed by the works council means that it has real bargaining power. And the fact that wages have been shown to be higher in establishments with works councils (for example, Addison, Schnabel, and Wagner 2001; Hübler and Jirjahn 2003) might imply that rent-seeking is alive and well; in other words, any attenuation of the hold-up problem may be modest at best.

Although there is a growing literature on the effects of works councils on various aspects of firm performance (for a survey, see Addison, Schnabel, and Wagner 2004),

*John T. Addison is Hugh C. Lane Professor of Economic Theory, Department of Economics, University of South Carolina, Professor of Economics, Queen's University, Belfast, U.K., and Research Fellow, University of Coimbra/GEMF, Portugal; Thorsten Schank is Lecturer, Institute of Economics, University of Erlangen-Nuernberg, Germany; Claus Schnabel is Professor of Economics, Institute of Economics, University of Erlangen-Nuernberg, Germany; and Joachim Wagner is Professor of Economics, Institute of Economics, University of Lueneburg, Germany. Addison acknowledges

research support from the Center for International Business Education and Research, University of South Carolina. All computations were performed at the Institute for Employment Research of the Federal Employment Service, using Stata/SE 8.2.

To facilitate replication and extensions, the do-files are available from thorsten.schank@wiso.uni-erlangen.de.

there has been almost no investigation of the works council–investment nexus, a relation that is fundamental to the performance question. Moreover, there is the unsettled issue of unobserved plant heterogeneity. Most studies have examined the effect of works council presence on the outcome indicators without considering the stable but unobserved characteristics of the firm that may be associated with performance and the frequency of the works council institution. This omission may be of particular importance in the case of investment, underscoring the need to provide within-plant estimates of works council impact. Our use of changes in works council status in the present treatment gives us more confidence in drawing inferences as to causality.

The theoretical portion of this paper begins with an outline of the simplified hold-up model as applied to unions and a consideration of its possible modification in the light of a purpose-built model of the works council. Using panel data for a nationally representative sample of German establishments, we later present findings for a matching model in which the treatment is the formation (dissolution) of a works council between 1998 and 2000 and the control group consists of establishments without (with) a works council over the entire sample period. We supplement this nonparametric evaluation with estimation strategies that involve fitting reduced-form investment functions, but which again compare investment behavior in plants setting up (abandoning) a works council with that of establishments that never (always) had a works council.

Theory

Theoretical considerations suggest that unions can have positive as well as negative effects on investment. In the traditional model, the union-set wage is represented as an exogenous change in the price of labor, and the firm adjusts employment along the labor demand curve. In this case, the union premium or tax is levied on labor. Union firms substitute away from expensive labor, but the net effect on investment is unclear: it depends on the degree of substitutability

between capital and labor and the magnitude of the scale effect as the premium filters through into higher product prices and output falls.

By contrast, the modern view is that unions tax *capital*, that firms respond unambiguously by cutting investment, and that the wage is endogenous. The idea is that unions expropriate quasi-rents that represent a part of the normal (that is, competitive) returns to capital that is vulnerable to capture once investment in specialized plant and equipment has been made. Once installed, even sizable reductions in the revenue stream to such fixed-cost capital will not cause it to be scrapped or sold. Standard theory predicts that such assets will continue in use as long as they earn a return above their opportunity cost. Accordingly, the more specific is the asset, the bigger the scope for union rent-seeking. Of course, with the relation-specific capital *in situ*, higher wages are unlikely to influence the use of the asset, but firms will anticipate reduced returns to such capital and invest less.

This is the so-called “hold-up” problem, first carefully analyzed by Grout (1984). Consider a simple one-shot two-stage game. In the first stage the firm chooses a level of capital (high/low), and in the next the union chooses the wage (high/low). By backwards induction, the union will always choose a high wage at stage two and, knowing this, the firm will always choose a low investment strategy at stage one. As noted, the union tax on investment will vary directly with the specificity of the asset and its longevity. The tax would vanish were the union able to commit itself to a low wage strategy by posting a bond or hostage to a third party, or if there were bargaining over investment as well as wages. However, neither arrangement is more than sporadically encountered in the real world.¹

¹There is also a potential hold-up problem on the other side of the employment relation. The usual example given is capture by the employer of workers’ quasi-rents stemming from their (co-)investments in firm-specific human capital. However, reputation effects that cause wage costs to rise and future cash flows to fall are usually viewed as effectively checking any such employer rent-seeking behavior.

Collective bargaining is of course repeated over time rather than a one-shot exercise and, abstracting from an end-game scenario, repeated games offer a solution to the hold-up problem, since opportunistic behavior can in principle be appropriately punished (for example, van der Ploeg 1987). An important issue in the literature has been the degree to which unions discount the future. In particular, it has been argued that because union members do not have property rights in the union, they will be rationally myopic and discount the future at a higher rate than shareholders. And this tendency will be reinforced by the greater influence of older workers in union councils (Hirsch and Prasad 1995). Accordingly, in a repeated game context, much hinges on the union's discount factor and the success of firms in extending the union's horizon (including greater recourse to debt), as well as inefficient defensive strategies such as the maintenance of inefficient capital or plants to facilitate substantial cuts in employment as a short-run profit-maximizing response to wage demands (for a discussion, see Baldwin 1983).

There is a strong presumption in the unions-and-investment literature, therefore, that greater worker representation will depress investments in physical and intangible capital—the Grout result—and lead to greater reliance on debt financing. However, two theoretical caveats and a qualification are in order. First, Addison and Chilton (1998) have shown that union opportunism alone does not necessarily underpin suboptimal investments in physical capital and that—irrespective of the union's horizon or the productive life of capital—sufficient patience on the part of the firm can yield self-enforcing contracts that are efficient with respect to employment and investment. In other words, the efficient outcome may depend crucially on the firm's discount factor. Second, uncritical extension of the hold-up model to investments in intangible capital (that is, R&D) should be resisted. As noted by Menezes-Filho and Van Reenen (2003:299), the tendency toward strategic R&D “undermines the analytical clarity of the Grout result.” The context is a Cournot duopoly model in which separate, firm-specific unions bargain *ex post* (that is,

there is no bargaining over R&D in the first stage) over employment and wages. Provided the union places sufficient weight on employment vis-à-vis wages, an increase in union bargaining power can lead to higher market share by virtue of the growth in employment. The firm is then supposed to raise its R&D investment so as to protect this enhanced market share.

The qualification is that the above discussion nowhere considers the potential productivity-enhancing function of unionism emphasized by proponents of collective voice (Freeman and Medoff 1984). Besides being the counterpart of workplace unionism in Germany, works councils can be viewed as the exemplary collective voice institution. The problem is that the standard collective voice model abstracts from these bargaining considerations (Addison and Belfield 2004). However, Freeman and Lazear (1995) provided a voice-theoretic model of the works council that recognizes the bargaining problem. They contended that the machinery of the works council holds out the prospect of an improvement in the joint surplus of the enterprise because of information exchange, consultation, and participation/co-determination. Thus management's use of the works council as a communicator to workers about the state of nature can lead them to work harder in adverse states, since in the absence of credible information (equated by the authors with a legal requirement to disclose financial information) workers might be tempted to discount management claims as to the seriousness of the situation. These claims may be viewed as opportunistic or strategic, leading workers to adopt an antagonistic stance that lowers the joint surplus. Having access to information that can verify management claims, the works council can render those claims credible to the work force and preempt antagonistic behavior, resulting in increased effort flexibility. For its part, consultation allows new solutions to production problems and other workplace problems by virtue of the non-overlapping information sets of the two sides and the creativity of discussion. Finally, participation or co-determination rights—which signally distinguish the German institution from its

continental European counterparts—generate an improvement in the joint surplus by providing workers with greater security, encouraging them to take a longer-run view of the prospects of the firm. The issue of the time horizon of workers is, it will be recalled, an important consideration in investment models.

Freeman and Lazear argued that the joint surplus of the enterprise will increase cumulatively with the progression from information exchange through consultation to co-determination. That said, the surplus will not continue to grow with the competence of the works council institution. This is more than a question of management prerogative, because it is posited that the workers' share in the joint surplus grows with the surplus, while profit falls both relatively and absolutely. The workers' share rises because knowledge and involvement are power. The same factors that cause the surplus to grow also cause profit to fall. Workers are thus depicted as demanding too much involvement precisely because their share will continue to rise after the joint surplus has peaked; equivalently, firms will either resist works councils or vest them with insufficient power. It is at this point that the wider industrial relations context in which the German works council operates commends itself to Freeman and Lazear because of the limits placed on the rent-seeking. Thus, works councils are excluded from reaching agreement with the employer on wages and working conditions that are settled or normally settled by collective agreements between unions and employers at the sectoral level, unless the latter authorize works agreements. Further, the enabling legislation forbids works councils from going on strike under a so-called peace obligation enjoining them to work together with the employer in a spirit of mutual trust for the good of the employees and the establishment. As a result, it is conjectured that the German system allows a decoupling of the factors that determine the size of the enterprise surplus from factors that determine its distribution.

Unfortunately the bargaining problem is not so easily sidestepped. German works councils have power. We know that the contents of work agreements range consid-

erably beyond the terms prescribed by law (Müller-Jentsch 1995:60–61) and that wages are higher and profits lower in works council regimes (Addison, Schnabel, and Wagner 2001). The question becomes one of whether there is a sufficient decoupling in practice of production from distribution issues. A final issue has nothing to do with rent-seeking: does the power of works councils to *delay* decision making—most obviously perhaps because of their consultation rights over the introduction of new technology—serve as a further constraint on investment? If so, this mechanism provides an additional direct source of reduced investment under works councils, which supplements any indirect effect operating through reduced profitability (an important source of investment funds) emphasized by bargaining power and delay theories alike. In any event, the conclusion is that the effect of the works council on investment is ultimately an empirical question.

Literature Review

There is a small U.S. literature on the investment effects of worker representation at the firm level. One study, Hirsch (1991), set the standard in deriving by survey its own measure of union presence, applying the distinction between the direct effects of unionism and the indirect effects stemming from the wage premium—the latter necessitating the estimation of an auxiliary profits equation—and identifying nonlinearities in union effects. Hirsch's study also tested the robustness of its principal results, and looked to supportive findings for other investment outcomes and other indicators suggested by the theory (for example, intangible capital and higher debt-equity ratios, respectively). Hirsch analyzed data on 572 publicly traded manufacturing companies from the NBER R&D Master File 1968–80, and reported solid evidence of the distorting long-run effects of union rent-seeking. Specifically, union companies invest around 20% less in plant and equipment than do similar nonunion companies (see also Hirsch 1992). Broadly consistent but somewhat weaker results—at least for physical capital—were reported

by Bronars and Deere (1993) and Bronars, Deere, and Tracy (1994) using Compustat firm data (which were also used by the remaining U.S. studies considered here).

A study by Cavanaugh (1998), using data on 722 publicly traded firms averaged over 1973–82, is of interest because it sought to test the argument that unions reduce *asset-specific investments*. Recall that the more specialized the plant or equipment, the greater the scope for the extraction of quasi rents. Assuming that asset specificity is directly associated with investment (as well as with excess market value and employment growth), Cavanaugh looked to the union effect on investment being amplified by asset specificity and hence included an interaction term between the two variables in his investment equation. He obtained strongly negative coefficient estimates for both the union measure and the interaction term.

Finally, just one U.S. firm-level study has been able to exploit a time series for unionization. In an approach that has found favor in the profit literature, Fallick and Hassett (1999) considered the effects of union representation elections on firm performance in 1962–84. In addition to a standard tax-adjusted q model, the authors employed a vector autoregression model to predict investment and then regress the forecast errors on union certification. Among their fixed effects results was a finding that unions reduced the investment-capital ratio by 29%. In their “surprises” model, investments in firms with successful certification elections were over-predicted. By way of underscoring the magnitude of the union tax, Fallick and Hassett computed that its effect was equivalent to a 33 percentage point hike (from .34 to .67) in the corporation tax.

There are two main German studies. In the first, Addison, Kraft, and Wagner (1993) considered the impact of the German works council on capital investment, the subject of our larger inquiry. They found that gross investment was between one-fifth and one-third lower in a works council plant than in its counterpart without the institution, which at first blush suggests that the effects of workplace representation in Germany were no less severe than those of the work-

place union institution in the United States. However, the study was based on a small sample of likely unrepresentative plants, and the authors duly noted the fragility of their investment equations. In the second study, using a much larger cross-section sample of medium-sized German establishments, Hübler (2003) reported that works council presence was associated with higher levels of ICT investment, other investment, and expansion investment. Results from a three-equation model indicated that this positive overall effect stemmed exclusively from the indirect effects of works councils operating through reorganization and training. On the other hand, the direct effect of works councils on investment was negative, albeit statistically insignificant (but see the companion study by Gerlach, Hübler, and Meyer 2002, where this inverse association was statistically significant in one specification). Note that both German studies relied on single cross-sections of data and only made use of information on works council presence (rather than changes in works council status), so that any attribution of causality is dubious.

Statistical limitations notwithstanding, virtually every firm- or establishment-level study points to reduced investment in tangible capital under unionism. Indeed, the only such studies pointing to statistically insignificant or positive union effects have not had objective data on investment (see, respectively, Machin and Wadhvani 1991; Benson 1994). For their part, industry studies for Canada and the United Kingdom have pointed in the same direction (see, respectively, Odgers and Betts 1997; Denny and Nickell 1992). Although aggregation problems limit the usefulness of industry-level studies, one intriguing result contained in the British analysis is the finding of a positive influence of joint consultative committees (that is, joint meetings of employers and managers concerned with consultation rather than negotiation) on investments in plant and machinery (Denny and Nickell 1992:882, fn. 9). Given that these bodies are akin to the German institution, might not the works council be sufficiently concertative *and* removed from negotiation to have a positive influence on investment?

Data

Our data are taken from the IAB Establishment Panel of the Institute for Employment Research of the Federal Employment Service. Each year since 1993 (1996), this panel has surveyed several thousand establishments from all sectors of the economy in western (eastern) Germany. It is based on a stratified random sample—the strata are for 16 industries and 10 size classes—from the population of all establishments with at least one employee covered by social insurance. To correct for panel mortality, exits, and newly founded units, the data are augmented regularly, yielding an unbalanced panel. Participation of establishments is voluntary, but the response rates (which exceed 70%) are high compared with other, non-official German firm panel studies. The first wave of the IAB panel (1993) included 4,265 plants for western Germany; and in 1996, the establishment panel for eastern Germany started up with 4,313 plants. Overall, the panel has increased in size every year to facilitate regional analysis at state (*Länder*) level. As of 2004 it covered approximately 16,000 establishments.

Data are collected in personal interviews with the owners or senior managers of the establishments by professional interviewers. Since the panel is created to serve the needs of the Federal Employment Service, its focus is on employment-related matters. Accordingly, information on the work force, its decomposition, and development through time are central elements of the panel questionnaire. Additional questions cover, among other things, the establishment's sales, investment in physical capital, wage bill, exports, technology, age, and corporate form and legal status, as well as whether or not it applied a collective agreement (at either sectoral or firm level). Note that the IAB panel is the only nationally representative longitudinal sample of establishments in Germany that can be used to investigate works council impact. Further details on the panel (to include information on the questionnaire(s) and how to access the data) are given in Kölling (2000).

In investigating the impact of works councils on investment, our study uses information

on the presence or absence of a works council in a plant during the interval 1998 to 2003. The question on works council presence or absence was asked of all establishments in 1998, 2000, 2002, and 2003.² The crucial information is whether the plant's works council status changed through time, which allows us to identify the impact of the institution on investment through *within-plant* changes rather than relying on between-plant comparisons. Therefore, we differentiate four groups of establishments: (i) plants that never had a works council across all years for which information is available; (ii) plants that did not have a works council in 1998, but subsequently reported having one in 2000 that was still operational in 2002 and 2003; (iii) plants that always had a works council; and (iv) plants in which the works council was dissolved (that is, plants that reported the existence of a works council in 1998 but not in 2000 through 2003). Observe that all plants changing their works council status after 2000 were excluded from the sample. Our focus, then, is on how a regime shift (the setting up or abandonment of a works council) occurring between 1998 and 2000 affected investment in the following years (2000 to 2003). As we shall be exploiting changes in works council status, it should be noted that although works councils are mandatory in all plants with five or more permanent employees, they are not automatic.³ For a works council to be elected, just three employees with voting rights (that is, aged at least 18 years) must first call for an electoral board to be convened. Once this procedure has been set in motion, the establishment of a works council is a *fait accompli*, with the electoral board holding the election. By the same token, works councils can equally easily lapse. This will occur if no re-election takes place, or if works councilors quit the

²In the missing years, the question was only asked of panel accessions, which perforce cannot be used in our longitudinal analysis.

³Only one in six establishments in the private and public sectors actually had a works council in 2000. In particular, it is the small firms that typically do not elect or need works councils (for details and explanations, see Addison et al. 2003).

plant or cease actively to meet. The entity can therefore be set up or dissolved at any point in time by the plant's employees. By the same token, we shall see that both events are rather rare.

The dependent variable in our analyses is investment divided by sales averaged over the period 2000 to 2003.⁴ We have information both on total investment and on the share of that investment that is expansion investment, as opposed to replacement investment. We normalize investment by sales to avoid scale effects. Because sales are measured differently for establishments from the banking and insurance sectors—being recorded in balance sheet terms and premium income, respectively—they are excluded (in addition to public sector companies) from the sample, which is further reduced because of attrition and missing values. Nevertheless, depending on the particular estimation strategy pursued, several hundred to several thousand plants can still be observed.

Methodology and Findings

In the subsequent empirical analysis, we make use of three alternative estimation strategies to determine the effect of works councils on investment. In each case, we investigate whether the setting up (dissolution) of a works council between 1998 and 2000 inhibited (increased) investment between 2000 and 2003. To do so, we compare the investment behavior of plants setting up (abandoning) a works council with that of establishments that never (always) had a works council. Each approach is carried out separately for our two standardized investment measures: total investment and expansion investment.

A first look at the descriptive data in the top panel of Table 1 indicates that both to-

tal investment and replacement investment between 2000 and 2003 were higher in those establishments in which a works council was introduced and lower in those establishments where the institution was abandoned—even if the differences with comparator establishments (those that, respectively, never had or always had a works council) are only statistically significant in the latter case. However, there are some marked differences within the two pairs of works council regimes in 1998 (that is, before a works council was set up or abandoned). The lower panel of Table 1 examines these differences for several workplace characteristics that have been found to be associated with works council presence (see Addison et al. 2003). Thus, for example, it can be seen that establishments abandoning their works councils between 1998 and 2000 had on average fewer employees than those that had always had a works council. In addition, establishments abandoning a works council were less likely to be branch plants, to be located in western Germany, to apply a bargaining agreement, and to report either a good or very good profit situation than plants that had always had a works council. When we compare plants that set up a works council with those that never had one, it appears that the former group were more likely than the latter to be branch plants, to be located in western Germany, to be non-family-owned businesses, to report good or very good profits, and to apply a sectoral-level bargaining agreement.

The implication of this descriptive material is that the introduction or dissolution of a works council is not a random occurrence. The different "starting conditions" imply that observed differences over the sample period in the investment behavior of establishments that did or did not set up or abandon a works council cannot be interpreted unambiguously as a causal effect of that entity. If establishments from both pairs of groups differed significantly at a point in time when none of them had yet set up or abandoned a works council, one would expect them also to display differences in investment some years later. These different starting conditions are explicitly taken into account and eliminated in the matching approach below.

⁴Information on both total sales and investment in year t is contained in the survey for the following year. If there were missing values for investment or sales in any one or two of the four years, we calculated the average using information from the remaining (three or two) years. Plants for which we had usable information on establishment investment (divided by total sales) for just one year were dropped from the sample.

Table 1. Mean Values of Variables for Plants
with Different Works Council Regimes: All Plants.

Variable ^a	Works Council Regime					
	(1) Formation (1998: No; 2000: Yes; 2002: Yes; 2003: Yes)	(2) Never Present (1998: No; 2000: No; 2002: No; 2003: No)	P-Value ^b	(3) Dissolution (1998: Yes; 2000: No; 2002: No; 2003: No)	(4) Always Present (1998: Yes; 2000: Yes; 2002: Yes; 2003: Yes)	P-Value ^b
N	29	1,668		33	765	
Total Investment Divided by Total Sales (Average of 2000–2003)	0.065	0.045	.15	0.029	0.079	.00
Expansion Investment Divided by Total Sales (Average of 2000–2003) ^c	0.025	0.017	.32	0.014	0.032	.02
Number of Employees	304.1	27.5	.20	50.5	629.9	.00
Branch Plant (Dummy: 1 = Yes)	0.345	0.058	.00	0.152	0.292	.04
Economic Performance (Dummy: 1=Very Good/Good)	0.552	0.376	.07	0.182	0.358	.02
Share of Blue-Collar Workers	0.596	0.652	.37	0.728	0.610	.03
Share of Part-Time Employees	0.128	0.154	.49	0.055	0.087	.11
Share of Female Employees	0.292	0.365	.12	0.299	0.298	1.00
Eastern Germany (Dummy)	0.379	0.600	.02	0.697	0.408	.00
Collective Agreement:						
at Sectoral Level (1 = Yes)	0.690	0.399	.00	0.515	0.763	.01
at Firm Level (1 = Yes)	0.138	0.062	.26	0.030	0.152	.00
Establishment Formation in the Last Five Years (1 = Yes)	0.241	0.335	.26	0.182	0.156	.71
Legal Form of Firm (Dummy: 1 = Family Owned Firm)	0.069	0.553	.00	0.242	0.124	.13

^aUnless otherwise stated, information refers to the year 1998.

^bTwo-sample t-test with unequal variance of the hypothesis that the difference in the means between columns (1) and (2) (and between columns 3 and 4) is zero.

^cThe number of observations for groups (1), (2), (3), and (4) is 29, 1,652, 32, and 758, respectively.

A Matching Approach

In our first strategy we employ the matching method, which is a nonparametric (or semi-parametric) approach to identify the impact of a specific treatment on certain outcomes and which allows a causal interpretation of treatment effects.⁵ In our case, the treatment is the formation of a works council between 1998 and 2000, and establishments without a works council over the

entire sample period from our control group. Our outcome variable is average investment divided by total sales in the years following the treatment.⁶ The (average) treatment effect is identified by choosing a subset of the control group of untreated plants (plants that never had a works council) having observable characteristics in 1998 as similar as possible to those of the treated group (plants setting up a works council after 1998). Condition-

⁵Matching analysis and the causal interpretation of the effects identified can be traced back to Rubin (1974). Latterly, the approach has become very popular in the evaluation of labor market programs; see, for example, Heckman, LaLonde, and Smith (1999).

⁶Analogously, we also investigate the treatment effect of abandoning the works council, where the control group now consists of all those plants that always had a works council. For expositional convenience, however, we will only refer to the formation of a works council as a treatment.

ing on the observables, the method assumes that the only remaining difference between the two groups of plants is the treatment status. Accordingly, the average impact of the treatment can be recovered through a comparison of means of the investment rates of both groups. The matching analysis has two advantages over the traditional OLS regressions on which our second and third empirical strategy will be based. First, it does not rely on a linear relationship in the investment equation. Second, the analysis is restricted to the region of common support, which means that the estimated treatment effect is restricted to the so-called region where data on the treated plants as well as those on the control group are observed.⁷

In the particular context we study, the identifying assumption when using matching methods (that is, the conditional independence assumption) is that plants setting up a works council would have displayed the same investment behavior as the control group in the (hypothetical) case that they had not done so. To satisfy this condition, we must take into account all variables that are expected to exert an influence on the decision to set up a works council and on investment.

We apply propensity score matching, wherein the selection of the control group is carried out on the basis of the probability that a unit has received treatment, conditional on the observed variables. The propensity score is obtained from a probit regression of a dummy variable indicating whether or not a works council was formed between 1998 and 2000 on a vector of covariates consisting of the following arguments: the number of employees (and the squared number of employees); the percentages of part-time employees, of female employees, and of blue-collar workers; dummy variables for collective agreements concluded at either of two levels (sectoral or firm); dummy variables denoting branch-plant status, the profit situation, and the legal form of the enterprise; and

dummies for the age of the establishment (formed within the last five years, or earlier), the region (western versus eastern Germany), and industry affiliation.⁸ For each treated plant we search in the same region (West versus East) and in the same sector (from a total of nine) for the most similar plant in terms of the propensity score (obtained from the probit regression) in the control group, thus performing a nearest neighbor matching.⁹ Note that in this process each matched establishment from the control group is never used more than once to form a statistical twin (one-to-one matching), so that the results reported below are based on totally different pairs of treated and non-treated plants. The matched non-treated establishments form the new control group. Finally, the mean investment of the treated establishments is compared with the mean investment of the matched non-treated establishments.

Table 2 indicates that the matching was successful. A comparison of the mean values of variables in 1998 for the two pairs of treated and control plants (works council formation versus “never present,” and works council abandonment versus “always present”) shows no statistically significant differences at conventional levels.¹⁰ In other words, the respective treated and control groups are very similar. However, the sample of plants setting up a works council now numbers only 25, while that identified as abandoning a works council falls to 21 (20 in the case of expansion investment). This reduction occurs because no comparable twin (in the same sector and

⁷Note that both regression and matching analysis require the decision to set up a works council, conditional on the covariates, to be independent of the unobservables.

⁸For an econometric analysis of the determinants of works council presence and their introduction that is also based on the IAB panel and uses these covariates, see Addison et al. (2003).

⁹Since our control groups consist of 1,668 and 765 plants, respectively, and are thus much larger than our groups of treated plants, nearest neighbor matching without replacement is feasible and appropriate. Matching was performed in Stata 8.2 using the PSMATCH2 command (Leuven and Sianesi 2003).

¹⁰The sole exception is where the plant was established during the previous five years, which is now more likely (albeit statistically significant only at the .10 level) for plants that always had a works council than for those that abandoned their works councils between 1998 and 2000.

Table 2. Mean Values of Variables for Plants with Different Works Council Regimes: Matched Plants.

Variable ^a	Works Council Regime					
	(1) Formation (1998: No; 2000: Yes; 2002: Yes; 2003: Yes)	(2) Never Present (1998: No; 2000: No; 2002: No; 2003: No)	P-Value ^b	(3) Dissolution (1998: Yes; 2000: No; 2002: No; 2003: No)	(4) Always Present (1998: Yes; 2000: Yes; 2002: Yes; 2003: Yes)	P-Value ^b
N	25	25		21	21	
Number of Employees	97.7	84.7	.77	67.2	72.5	.77
Branch Plant (Dummy: 1 = Yes)	0.280	0.320	.76	0.095	0.143	.64
Economic Performance (Dummy: 1=Very Good/Good)	0.520	0.480	.78	0.190	0.095	.39
Share of Blue-Collar Workers	0.589	0.570	.85	0.704	0.700	.95
Share of Part-Time Employees	0.141	0.128	.84	0.053	0.094	.45
Share of Female Employees	0.299	0.338	.61	0.263	0.300	.60
Eastern Germany (Dummy)	0.440	0.440	1.0	0.619	0.619	1.0
Collective Agreement: at Sectoral Level (1 = Yes)	0.680	0.800	.34	0.571	0.571	1.0
at Firm Level (1 = Yes)	0.120	0.040	.31	0.048	0.000	.33
Establishment Formation in the Last Five Years (1 = Yes)	0.240	0.280	.75	0.095	0.333	.06
Legal Form of Firm (Dummy: 1 = Family Owned Firm)	0.080	0.040	.56	0.238	0.143	.44

^aInformation refers to the year 1998.

^bTwo-sample t-test with unequal variance of the hypothesis that the difference in the means between columns (1) and (2) (and between columns 3 and 4) is zero.

region) could be found for the plants that dropped out of the analysis. More technically, the propensity score of the treated unit was outside the estimated propensity score distribution of the untreated plants.

Mean values of our two investment outcome variables for the respective plant-types are reported in Table 3. All probability values imply that the observed differences between the treated and untreated groups are statistically insignificant. Thus, we do not find evidence either that setting up a works council inhibited investment in subsequent years or that abandoning a works council increased future investment. This result casts doubt on the validity of the unions-reduce-investment hypothesis as it applies in a German context.

Estimating Investment Functions

In the next two approaches we estimate

a reduced form investment function that employs the standard covariates used in the literature, insofar as these are available in our data set. Our first explanatory variable is an establishment's growth in sales between 2000 and 2003, since a rising demand for goods and services may only be satisfied through an increase in the capital stock, via expansion investment. The economic performance of an establishment is also expected to have a positive impact on investment: the better the profit situation, the higher the expected return on investment, and the more plentiful the financial resources available for investment. Therefore, a dummy variable indicating whether or not the profit situation in 1999 was either very good or good is used here. Although we have normalized the investment measure by total sales, we include establishment size as a further independent variable. According to Schumpeterian arguments, there should exist a positive relation-

Table 3. Mean Values of Investment for Plants with Different Works Council Regimes. Matched Plants.

Variable ^a	Works Council Regime					
	(1) Formation (1998: No; 2000: Yes; 2002: Yes; 2003: Yes)	(2) Never Present (1998: No; 2000: No; 2002: No; 2003: No)	P-Value ^b	(3) Dissolution (1998: Yes; 2000: No; 2002: No; 2003: No)	(4) Always Present (1998: Yes; 2000: Yes; 2002: Yes; 2003: Yes)	P-Value ^b
Total Investment Divided by Total Sales (Average of 2000–2003)	0.061	0.036	.12	0.040	0.046	0.73
N	25	25		21	21	
Expansion Investment Divided by Total Sales (Average of 2000–2003)	0.021	0.011	.25	0.020	0.019	0.94
N	25	25		20	20	

^aTwo-sample t-test with unequal variance of the hypothesis that difference in the means between columns (1) and (2) (and between columns (3) and (4)) is zero.

ship between a firm's size and its innovation as well as investment activity. In addition, investment per unit of sales may be greater in large firms due to their easier access to financial markets and greater willingness to accept risk. We use a quadratic in the number of employees to pick up any non-linearity in the relationship between establishment size and the investment ratio. We also deploy two dummy variables reflecting formal collective bargaining arrangements: whether or not the establishment has concluded a collective agreement (either through the employers association at sectoral level or with the union directly at firm level) and is thus subject to union wages. In both cases, investment might be higher if firms substitute away from expensive labor; but if collective bargaining takes place at firm level, the firm also faces an increased risk of rent capture, thereby inhibiting investment. Next, as a crude representation of the dynamic adjustment of the capital stock, we include investment divided by sales averaged over the two years 1997 and 1998 as a quasi lagged dependent variable.¹¹ Finally,

we deploy ten industry dummies to capture sectoral differences in investment behavior (possibly due to different positions in the business cycle), and also enter a dummy for eastern Germany. Although the economic situation remains worse in post-communist eastern Germany—which should therefore serve to depress investment—the heavy subsidization of capital in the east should act as spur to investment.

Two caveats are in order before turning to our fitted investment equations. First, we do not have information on the price of capital, the investment constraints, or the financial constraints confronted by individual firms, even if some of those determinants may in part be indirectly captured by certain of our other explanatory variables (such as firm size and industry affiliation). Second, because our econometric specifications are not explicitly derived from optimal investment behavior, given some specified structure of adjustment costs, the reduced form models used here only “represent an empirical approximation to some complex underlying process that has generated the data” (Bond and Van Reenen 2003:34). Extended discussions of theoretical and empirical investment equations can be found in Jorgensen (1963), Hirsch (1991), and Bond and Van Reenen (2003). Recent investment studies using German firm-level data include Neubäumer and Kohaut (2002),

¹¹In order to make a clean break between prior investment and investment after the formation or dissolution of a works council, we did not include the year 1999 in the lagged dependent variable because in this year investment might already have been influenced by a change in works council status.

Bond, Harhoff, and Van Reenen (2003), and Kirchesch (2004).

As before, our main focus is on the various works council regimes that are here represented by three dummy variables indicating whether a works council was set up, dissolved, or always in existence. Establishments that never had a works council now form the reference group. A statistically significant negative coefficient estimate in respect of works council formation, for example, would imply that these plants invest less than do their counterparts that remained free of works councils. It is also of interest to determine whether the effect of works council dissolution significantly differs from that of continuous works council presence, and whether plants that have recently abandoned a works council behave similarly to those that never had one.

OLS results for our reduced form investment equation allowing for separate intercepts in the case of each works council regime are presented in Table 4. It can be seen that the coefficient estimates for most of our control variables have the expected signs, but not all of them are statistically significant. In particular, a good profit situation of the plant and the plant's location in eastern Germany are associated with higher investment, whereas establishment size and sales growth do not seem to play a role. For its part, average investment between 1997 and 1998 makes the expected positive contribution to the explained variation of investment between 2000 and 2003, and the relationship is stronger for total investment. This seems plausible since total investment is composed of expansion investment and replacement investment, and the latter—reflecting the depreciation of the capital stock—should evince stronger autocorrelation through time.

Focusing on our works council dummies, we see that establishments that always had a works council recorded significantly higher investment than reference group plants. This difference in levels should, however, not be overemphasized. Although this relation may reflect plants' reactions to higher labor costs associated with permanent works council presence (as found by Addison, Schnabel, and Wagner 2001), it also could indicate that

the two groups of plants are so different in many respects that we have non-overlapping distributions of the covariates (see above). More interesting, and more important for a causal interpretation of works council effects, are the point estimates of the dummies indicating a *change* in works council status. We see that the coefficient estimate for the dummy indicating the formation of a works council is altogether statistically insignificant for both total and expansion investment. Similarly, plants that abandoned their works council did not record higher investment than those in which a works council continued in operation. Neither result favors the hypothesis that works councils inhibit investment.¹² That said, we should note that our results are based on only a small number of changers: the regression sample includes 1,186 (557) plants that never (always) had a works council, but only 14 (23) reporting the formation (abandonment) of a works council. (Further note that the numbers of observations now fall below those reported in Table 1 because of missing values for the lagged dependent variables and sales growth in the investment equations.)

The preceding estimation strategy attributes any changes in investment behavior between the four different works council regimes to intercept shifts, thereby assuming that the other covariates have the same impact throughout. This restriction is relaxed in our second approach, in which our investment equations only use data pertaining to plants that did *not* experience a change in works council status (that is, we fit separate investment equations to data from plants that either never had or always had a works council). The estimated coefficients are then used to predict investment in plants that changed their works council status. More precisely, the estimated parameters from the regression containing plants that never (always) had a works coun-

¹²This insight and the following results do not change if—following the suggestions of a referee—the standardized investment variables in our regressions are expressed in logarithms, or averaged over the two-year interval 2002–3 (consonant with our lagged dependent variable, which is also a two-year average covering 1997–98). Full results are available from the authors on request.

Table 4. OLS Regressions of Investment for all Plants.
(Endogenous Variable: Investment Divided by Total Sales, Average of 2000–2003.)

Independent Variable	Investment Measure	
	Total	Expansion
Works Council Dummies:		
Works Council Never Present (1998: No; 2000: No; 2002: No; 2003: No)	Reference Group	Reference Group
Works Council Formation (1998: No; 2000: Yes; 2002: Yes; 2003: Yes)	0.004 (0.23)	0.002 (0.18)
Works Council Dissolution (1998: Yes; 2000: No; 2002: No; 2003: No)	-0.004 (0.51)	0.001 (0.17)
Works Council Always Present (1998: Yes; 2000: Yes; 2002: Yes; 2003: Yes)	0.028*** (3.82)	0.01** (2.56)
Sales Growth (Between 2000 and 2003)	0.004 (1.02)	0.004 (1.59)
Profit Situation of the Establishment in 1999 (Dummy: 1=Good/Very Good)	0.011** (2.15)	0.008** (2.30)
Number of Employees in 2000	3.39e-06 (1.07)	4.30e-07 (0.23)
Number of Employees in 2000 Squared	-6.95e-11 (0.90)	-5.32e-12 (0.12)
Collective Agreement in 2000 (Reference Category: No Collective Agreement):		
at Sectoral Level (Dummy)	0.011* (1.82)	0.005 (1.16)
at Firm Level (Dummy)	0.011 (1.08)	0.010 (1.24)
Eastern Germany (Dummy)	0.018*** (2.87)	0.011*** (2.93)
Total Investment Divided by Total Sales (Average of 1997–98)	0.148** (2.02)	
Expansion Investment Divided by Total Sales (Average of 1997–98)		0.081* (1.69)
10 Industry Dummies	Yes***	Yes***
Constant	0.081*** (3.84)	0.041** (2.27)
Number of Observations (N)	1,780	1,746
Works Council Formation	14	14
Works Council Never Present	1,186	1,171
Works Council Dissolution	23	23
Works Council Always Present	557	538
R ²	0.22	0.08

The Huber/White/sandwich estimator is used to calculate the standard errors of the estimates.

Itl-statistics in parentheses.

*Statistically significant at the .10 level, **at the .05 level;*** at the .01 level.

cil are used to simulate the investments that would have been forthcoming in plants that set up (abandoned) a works council had they not in fact changed their status. Assuming that the unobserved factors are uncorrelated with works council presence, any difference

between the average predicted value and the actual value of investment in plants setting up or abandoning a works council is due to different parameters in the investment function of the groups of plants that changed or did not change their works council status.

Table 5. OLS Regressions of Investment for Groups of Plants with No Change in Works Council Regime.
(Endogenous Variable: Investment Divided by Total Sales, Average of 2000–2003)

<i>Independent Variable</i>	<i>Investment Measure:</i>	<i>Works Council Regime</i>			
		<i>Works Council Never Present (1998: No; 2000: No; 2002: No; 2003: No)</i>		<i>Works Council Always Present (1998: Yes; 2000: Yes; 2002: Yes; 2003: Yes)</i>	
		<i>Total</i>	<i>Expansion</i>	<i>Total</i>	<i>Expansion</i>
Sales Growth (Between 2000 and 2003)		0.004 (1.37)	0.003* (1.67)	0.028 (1.16)	0.012 (0.72)
Profit Situation of the Establishment in 1999 (Dummy: 1 = Good/Very Good)		0.016*** (2.60)	0.010** (2.25)	0.007 (0.73)	0.009 (1.66)
Number of Employees in 2000		-8.24e-05 (1.25)	-5.98e-05 (1.16)	2.76e-06 (1.03)	-8.37e-07 (0.36)
Number of Employees in 2000 Squared		2.10e-08 (0.29)	3.82e-08 (0.69)	-4.21e-11 (0.61)	3.17e-11 (0.58)
Collective Agreement in 2000 (Reference: No Collective Agreement):					
at Sectoral Level (Dummy)		0.005 (0.68)	0.004 (0.71)	0.015 (0.84)	0.006 (0.92)
at Firm Level (Dummy)		0.004 (0.54)	0.003 (0.56)	0.008 (0.41)	0.002 (0.23)
Eastern Germany (Dummy)		0.007 (1.09)	0.006 (1.38)	0.016 (1.53)	0.013* (1.99)
Total Investment Divided by Total Sales (Average of 1997–98)		0.063 (1.62)		0.388*** (3.69)	
Expansion Investment Divided by Total Sales (Average of 1997–98)			0.037 (1.35)		0.347** (2.60)
10 Industry Dummies		Yes***	Yes***	Yes**	Yes***
Constant		0.110*** (4.92)	0.051** (2.57)	0.020 (0.75)	0.007 (0.27)
Number of Observations (N)		1,186	1,171	557	538
R ²		0.12	0.06	0.46	0.22

The Huber/White/sandwich estimator is used to calculate the standard errors of the estimates. *t*-statistics in parentheses.

This could be due to an intercept shift or to different effects of the other covariates, both of which elements may be regarded as the effect of a works council formation (abandonment) on investment.

The results of estimating our investment equation separately for establishments that either never had or always had a works council are given in Table 5. They are more or less in line with the results in Table 4. It can be seen, however, that the explained variation of investment is much higher for those plants that always had a works council. (Such plants are considerably larger on average; see Table 1.)

Using the estimated parameters from these regressions, we can predict investment (normalized by sales) for those plants that set up (abandoned) a works council and compare these values with the actual investment of the respective groups. If works councils do indeed inhibit investment, we would expect to observe a lower (greater) actual than predicted value for plants setting up (abandoning) a works council. Table 6 shows that the difference between predicted and actual investment is not significantly different from zero for plants that have set up a works council. Actual total investment was even significantly smaller than the predicted

Table 6. Difference between Observed and Predicted Investment Divided by Total Sales, Average of 2000–2003.

Statistic	Investment Measure:	Group			
		Works Council Formation (1998: No; 2000: Yes; 2002: Yes; 2003: Yes)		Works Council Dissolution (1998: Yes; 2000: No; 2002: No; 2003: No)	
		Total	Expansion	Total	Expansion
N		14	14	23	23
Mean Observed		0.070	0.031	0.037	0.019
Mean Predicted		0.074	0.098	0.051	0.021
Mean Difference		-0.004	-0.067	-0.014	-0.003
T-Statistic		-0.131	-0.897	-2.256**	-0.339

Predictions are based on parameter estimates reported in Table 5. Estimates for plants that never (always) had a works council were used to predict the investment divided by total sales measure for plants setting up (abandoning) a works council.

value for plants *abandoning* a works council, although the difference in expansion investment is statistically insignificant. This final estimation strategy thus reconfirms the findings of our first and second approaches and does not indicate any evidence of a negative impact of works councils on investment.

Conclusions

Using establishment panel data, we have developed three complementary estimation strategies—involving parametric and nonparametric methods—to determine the impact, if any, of works councils on firm investments in tangible capital. We found no evidence that the formation (dissolution) of a works council had an unfavorable (beneficial) effect on investment. Nor for that matter did we find that changes in works council status had a positive effect on the investment bottom line. These are important results that fill a hole in the developing German literature on the effects of works councils on firm performance. They are also important more generally in addressing the hold-up problem, and in pointing to institutional arrangements that might attenuate that problem, as suggested by Freeman and Lazear (1995).

Care must be taken in interpreting our results. One issue is that only a small number of plants recorded a change in works council

regime over the sample period, making it more difficult to detect statistically significant differences in investment behavior. Second, there is the issue of the pedigree of the model. We have noted that the statistically insignificant overall impact of works councils on investment might in practice reflect opposing effects of the institution on investment, with capital-labor substitution being offset by opposing effects on capital formation stemming from rent-seeking.

Finally, there is the issue of perception. Despite our findings, and some potentially corroboratory evidence on investments in intangible capital (for example, Schnabel and Wagner 1994; Addison, Schnabel, and Wagner 2001; Kraft and Stank 2004), both anecdotal evidence and surveys of businessmen indicate that international investors are seemingly reluctant to invest in Germany because of that country's (little understood) system of codetermination. If this is the case, if codetermination does put off foreign investment, then works councils may indeed reduce investment in a way that our methodology cannot detect. That said, we suspect that the perceptions have more to do with codetermination at the enterprise level (where unions and employees can get half of the seats on the supervisory board) than with the establishment-level codetermination investigated here.

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