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Abstract: Despite recent changes in the relationship between unionism and various indicators of firm performance, there is one seeming constant in the Anglophone countries: unions at the workplace are associated with reduced employment growth of around -2.5%. Using a unique German dataset, we examine the impact of the *works council* – that country's form of workplace representation – on employment change, 1993-2001. Works council plants have 2.2% lower employment growth over this interval, having controlled for wages, changes in demand, industry affiliation, and various worker and establishment characteristics. But works councils do not seem to further slow the tortuous pace of employment adjustment.

Zusammenfassung: Trotz der jüngsten Veränderungen in der Beziehung zwischen Gewerkschaftsbindung und anderen Indikatoren zur Unternehmensperformance scheint ein Zusammenhang in englischsprachigen Ländern Bestand zu haben: Gewerkschaften am Arbeitsplatz sind mit einem reduzierten Wachstum der Beschäftigung von etwa -2,5% verbunden. Auf Basis eines einzigartigen deutschen Datensatzes untersuchen wir die Auswirkungen der Arbeit von Betriebsräten – die in Deutschland vorherrschende Form der Arbeitnehmervertretung – auf Beschäftigungsveränderungen in den Jahren 1993-2001. Betriebe mit Betriebsräten haben in diesem Zeitraum um 2,2 % niedrigere Wachstumsraten der Beschäftigtenzahlen, wobei für Löhne, Nachfrageveränderungen, Branchenzugehörigkeit und andere arbeiter- und unternehmensspezifische Charakteristiken kontrolliert wurde. Allerdings scheinen Betriebsräte den umständlichen Weg der Beschäftigungsanpassung nicht weiter zu verlangsamen.

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The effect of worker representation on employment behaviour in Germany: another case of -2.5%[†]

Contents:

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1. Introduction

In an interesting analysis of the 1984 (1980) WIRS published in this *Journal*, Blanchflower, Millward, and Oswald (henceforth *BMO*) (1991) provided estimates of the union employment differential of -3 (-2.5) percentage points per annum. These first published estimates for Britain immediately attracted controversy. In particular, Machin and Wadhwani (1991) countered that there was no union effect *per se*, arguing that the reduced employment growth in unionized plants was only observed in those establishments that had experienced organizational change. Since they equated organizational change with the elimination of restrictive practices, it follows that Machin and Wadhwani saw something rather positive (however proximate) behind the negative association between union presence and employment growth, where observed. Their interpretation also contrasts with the conventional notion that worker representation has detrimental effects on the number of jobs via the union wage premium.

However, in the years following this localized debate the negative association between unions and employment found by *BMO* (see also Blanchflower and Oswald, 1990) has become more rather than less entrenched. First of all, a number of British studies have confirmed the negative general association between employment change and unionism (e.g. Fernie and Metcalf, 1995, using the 1990 WIRS; Addison and Belfield, 2001, using the 1998 WERS). More especially, Booth and McCulloch (1999) have reported that the union result is robust to the inclusion of an organizational change variable. Using the 1990 WIRS, these authors found that union recognition was associated with a 2.6% (5.7%) reduction in employment 1989-90 (1987-90). The constancy of the union employment effect stands out when compared with seeming shifts in union impact on other firm performance outcomes over the course of the 1980s and 1990s (see the review in Addison and Belfield, 2004). Indeed, for the 1990 WIRS, Blanchflower and Burgess (1996) also find that the union 'effect' (of some -2% per annum) also survives the incorporation of a variable capturing the introduction of new technology as well as changes in work organization, at least in plants employing at least 25 manual and nonmanual employees.

Second of all, studies for other Anglophone countries have not only confirmed the inverse relationship between unions and employment growth but also reported similar point estimates. Thus, for example, in an analysis of the 1995 Australian Workplace Industrial Relations Survey, Wooden and Hawke (2000) reported that Australian unions slowed employment growth by approximately 2.5 percentage points a year. The North American evidence points in the same direction. Thus, in an investigation of some 1,800 Californian manufacturing plants, 1974-1980, Leonard (1992) reports that unionization reduced employment growth by between 2% and 4%. Similarly, Long's (1993) analysis of a sample of 510 Canadian firms indicates that union firms grew a little under 4% less than their nonunionized counterparts between 1980 and 1985.[1]

[1] However, we should note that Blanchflower and Burgesss (1996) do not detect negative union employment growth effects using the 1990 Australian WIRS, while both North American studies referred to suggest that the union effect is concentrated among larger establishments/firms.

In this paper, we provide estimates of the employment effects of workplace representation in Germany. The dual system of industrial relations in that country means that we will be considering the impact of the works council (or *Betriebsrat*) rather than the union. The works council is the vehicle of employee representation at the workplace, while the focus of union activity is the industry-wide or regional collective agreement. Germany is of particular interest for two main reasons. First, the *Betriebsrat* has long been looked upon with favour in European Union counsels, so that it has provided something of a template in the design of policies seeking to increase the involvement of European workers in their companies (for the most recent mandate, see Official Journal, 2002). This policy interest is underscored by recent theoretical support for the German institution on collective voice/contract enforcement grounds (e.g. Freeman and Lazear, 1995). A second, narrower source of interest in the German situation is the availability of a unique data set – the Establishment Panel of the Institute of Labour Market Research of the Federal Labour Office – which contains information on variables such as sales and capital missing from the corresponding datasets for Britain, namely, the WIRS/WERS.

The plan of the paper is as follows. Section II addresses the issue of model specification. Section III provides brief background information the institution of the works council and the longitudinal dataset. Results of fitting our employment change and dynamic labour demand equations are given in section IV. A summary concludes.

2. Methodology

Most employment change analysis has been based on two cross-sections of establishment-level data, collected in periods *t* and *t-j*. Identification of the employment effect of worker representation (typically unionism) has been through an *employment growth differential*, which is the counterpart of the union wage differential in the much larger union wage literature. Employment in period t, l_{it}, is therefore assumed to be a function of union status, demand changes (Δy_{it}), economic conditions (X_i) obtaining at beginning or end of period, and other time-invariant establishment-specific variables.

Denoting worker representation by *U*, we have the general employment equation:

$$l_{it} = \alpha_o + \lambda l_{i\,t-j} + \delta U_i + X_i \beta + \gamma_1 \Delta y_i + e_i, \qquad (1)$$

where λ $(0 < \lambda < 1)$ indicates the degree of employment inertia over the *j*-year interval, and where the lagged employment term l_{it-1} is introduced to account for autoregressiveness in the employment function. Empirical studies typically do not reject the null that $\lambda = 1$, which result has led to the re-interpretation of equation (1) as an employment growth equation, with δ providing the union employment growth differential.

To capture the channels through which worker representation might impact employment, equation (1) is sometimes modified to include other variables and their interaction with the *U* variable. One important example, noted earlier, has been the incorporation of a dummy variable for organizational change, *OC* (i.e. situations in which establishments are reported to have major changes in work organization not involving new plant), giving:

$$l_{it} = \alpha_o + \lambda l_{it-j} + X_i\beta + \gamma_1 \Delta y_i + \gamma_2 OC_i + \delta_1 U_i * OC_i + \delta_2 U_i * (1 - OC_i) + e_i . \qquad (1')$$

Equivalently, model (1) can be estimated using a dependent variable in differences:

$$l_{it} - l_{it-j} = \alpha_o + (\lambda - 1)l_{it-j} + \delta U_i + X_i\beta + \gamma_1 \Delta y_i + e_i , \qquad (2)$$

as is obtained by subtracting l_{it-j} from both sides of (1). Alternatively, averaging the employment change between *t* and *t-j* gives:

$$(l_{it} - l_{it-j}) / j = \alpha_o + (\lambda - 1)l_{i\,t-j} + \delta U_i + X_i\beta + \gamma_1 \Delta y_i + e_i . \qquad (2')$$

In either (2) or (2') the union effect is given by δ under the assumption that $(\lambda - 1) = 0$ is not rejected; otherwise, the union/worker representation effect is on the level of employment.

Clearly, two cross-sections of data do not a time series make. It is therefore dangerous to infer much about long-run dynamics from information on λ alone. But in principle the long-run union effect is given by $\alpha_1 /(1 - \lambda)$, obtained by setting $l_{it} = l_{i\,t-j}$ in equations (1) or (2).

These considerations lead to a third approach: a (pure) model in differences where the *j*-year change in employment is a function of (firm-specific) demand shocks plus any time-invariant effect such as worker representation, industry dummies, and so on (e.g. Booth and McCulloch, 1999). In this approach, there is no lagged employment term on the right-hand side of the equation, so that we have:

$$l_{it} - l_{it-j} = \alpha_o + \delta U_i + X_i\beta + \gamma_1 \Delta y_i + e_i , \qquad (3)$$

or

$$(l_{it} - l_{it-j}) / j = \alpha_o + \delta U_i + X_i\, \beta + \gamma_1 \Delta y_i + e_i . \qquad (3')$$

Again this model may include various interaction terms to capture different channels of union impact on employment growth.

By way of summary, and abstracting from interactions,[2] across equations (1), (2), and (3) the union employment growth differential effect over a *j*-year period is represented by:

$$E(\Delta_j l_{it} \mid Union - \Delta_j l_{it} \mid Nonunion) = \delta .$$

In all circumstances, random assignment of union/worker representation status is assumed and estimation proceeds via OLS. Again, there is a ready parallel in the union relative wage literature.

[2] We also abstract from the issue of which variable most accurately reflects union influence. Although the locus of the British debate has been upon union recognition versus union density, other developments have either involved more explicit modeling of the bargaining power of the union (e.g. Paci, Wagstaff, and Holl, 1993) or deployed additional/alternative measures such as union membership agreements and bargaining fragmentation (see Addison and Belfield, 2004).

A fourth and final approach is panel estimation that takes advantage of the longitudinal structure of a dataset. In this case, employment change is a one-year difference (the frequency of employment observation in the raw database is annual). The standard formulation of an employment adjustment specification in levels of the variables is then given by:

$$l_{it} = \lambda l_{it-1} + \beta'(L)X_{it} + u_i + v_t + e_{it}, \qquad (4)$$

where L is the lag operator, β is the vector of coefficients of explanatory variables X, u_i and v_t represent unobserved firm- and time-specific effects, and e_{it} denotes the noise residual. As in previous formulations, the coefficient of the lagged employment variable captures the degree of sluggishness in labour adjustment – the bigger the coefficient, the lower is the speed of adjustment of employment to exogenous shocks.

OLS estimation of dynamic labour demand models (i.e. with a lagged dependent variable and firm-specific effects) biases (upwardly) the estimated coefficients. First-differencing the dynamic labour demand equation (1) removes the individual effects u_i, but not the lagged (first-difference) employment term, which has to be instrumented using lagged levels of the variables. (Any non-strictly exogenous right-hand-side variable must also be instrumented using instruments in levels while any strictly exogenous variable must be instrumented using lagged differences.) First-differences of model (4) and an instrumental variables method are therefore required. We will use in particular the linear estimator GMM-SYS developed by Blundell and Bond (1998) which is supposed to yield more precise parameter estimates and to reduce potentially important small sample bias stemming from the short sample periods of the typical panel. (This method in first differences does not of course allow the direct estimation of the contribution of any time-invariant regressor.)

To determine whether labor demand adjustment at micro level is sensitive to the presence of worker representation – in our case whether or not works councils imply higher employment inertia – the interaction term $U_{it} * l_{it-1}$ (or $U_i * l_{it-1}$ if we assume no change in the U status of establishment i) is introduced in equation (4). This gives the model:

$$l_{it} = \lambda l_{it-1} + \lambda_1 U_{it} * l_{it-1} + \beta'(L)X_{it} + u_i + v_t + e_{it}, \qquad (4')$$

where U_{it} is a dummy variable set equal to 1 if the establishment reports the presence of a works council, 0 otherwise. Note that we shall ignore any interaction between U_{it} and X_{it}. Under the hypothesis Ho: λ_1=0, employment inertia is given by λ; if Ho is rejected, then employment inertia is equal to $(\lambda + \lambda_1)$ if a works council is present. Clearly, works councils increase employment inertia if λ_1>0.

The employment growth differential, δ, can also be derived from the dynamic model (4) by introducing the interaction term $U_i * t$ (where t represents a time trend):

$$l_{it} = \lambda l_{it-1} + \beta'(L)X_{it} + \delta U_i * t + u_i + v_t + e_{it}, \qquad (5)$$

and then differencing to obtain:

$$\Delta l_{it} = \lambda \Delta l_{it-1} + \beta'(L)\Delta X_{it} + \delta U_i + \Delta v_t + \Delta e_{it}.[3] \qquad (5')$$

3. The Institution and the Dataset

The Works Council

The German works council is mandatory but not automatic in all establishments with five or more employees. That is to say, the body has first to be elected: if workers in an establishment do not petition for a works council election, there will be no council, and if they do it is a *fait accompli*. As a practical matter, fewer than one-fifth of all plants with at least five employees have a works council, even if just over one-half of employees are covered by works councils (Addison, Bellmann, Schnabel, and Wagner, 2003).

The size of the works council is fixed by law and is a function of the establishment's employment level. More particularly, the information, consultation and codetermination rights of the council are also formally laid down under the law. Each is also a stepped function of establishment size. Thus, for example, we can with some justification speak of the formal powers of a council as being a datum between 21 and 100 employees. This particular size range is important in two respects. First, there is the general point that it makes sense to test for the impact of a works council by size categories within which the powers of the institution do not vary – in the absence of further information on works council heterogeneity. Second, and more narrowly, there is the point hinted at earlier that almost all large plants have a works council and small plants seldom do. For our sample in 2001, for example, 40% of establishments with 21-100 employees had works councils. In contrast, only 4% (no less than 94.5%) of plants with less than 21 (more than 100) employees had work councils. Not surprisingly, our empirical focus will be upon employment change in plants with 21-100 employees, although results for the all-plant sample are given in the appendices.

The Dataset

Our data are taken from the Establishment Panel of the Institute for Employment Research of the Federal Labour Office. Each year since 1993 (1996), this panel has surveyed several thousand establishments from all sectors of the economy in western (eastern) Germany. It is

[3] Alternatively, we can employ a time grouping dummy d_T, where $d_T = 1$ if t belongs to period *T*, 0 otherwise, giving $l_{it} = \lambda l_{it-1} + \beta'(L)X_{it} + \delta_T U_i * d_T * t + u_i + v_t + e_{it}$. Taking differences will again capture δ_T, namely, the employment growth differential between establishments with and without works councils in period *T* (i.e. $\Delta l_{it} = \lambda \Delta l_{it-1} + \beta'(L)\Delta X_{it} + \delta_T U_i * d_T + \Delta v_t + \Delta e_{it}$). This particular approach is followed by Nickell, Wadhwani, and Wall (1992).

based on a stratified random sample – strata for 16 industries and 10 size classes – from the population of all establishments with at least one employee covered by social insurance. To correct for panel mortality, exits, and newly-founded units, the data are augmented regularly, yielding an unbalanced panel. Data are collected in personal interviews with the owners or senior managers of the establishments by professional interviewers. The panel is created to serve the needs of the Federal Labour Office, and so its focus is on employment-related matters. Further information on the panel – including information on the questionnaire(s) and how to access the data – are given in Kölling (2000).

Our inquiry uses information for the years 1993 to 2001, thus excluding eastern Germany in the interests of a longer panel of data. Note that some of the information related to year *t* is asked for in the survey conducted in the following year. One such example is the value of sales in year *t*; as a result our demand data will be for seven rather than eight years.

From the raw dataset we identified 912 continuing establishments, 1993-2001, from which we extracted a sample of 802 establishments with at least five employees. Of these plants, 169 had more than 20 and fewer than 100 employees in 2001. We collected information on employment, workforce characteristics (viz. the percentage of part-time and female workers), output demand, gross wages, intermediate inputs, and a variety of other establishment characteristics (a measure of establishment age, and whether or not the establishment exports to foreign markets, uses state-of-the-art technology, invests in ITC, is a single establishment firm, and is publicly listed). These arguments are defined in Appendix Table 1 and are guided by those used in the literature. They are supplemented by 34 industry dummies.

In addition, the Establishment Panel also contains information on the volume of capital investments (including 'expansion' or net investments), even if such data are missing for a large number of units. Since the expansion investment variable is only available from 1996 onwards, we proxied annual changes in the capital stock by total capital investments. The measure does not therefore net out annual depreciation charges. Both it and all nominal variables were deflated by the GDP implicit price level (OECD data).

Finally, information on works council status is available in 1993, 1996, 1998, 1999, 2000, and 2001. In coding this key variable in the missing years, we assumed that the unobserved works council status of establishment *i* in period *t* was the same as that in period *t-1* (or *t-2*) where there was no reported change between *t-1* (or *t-2*) and *t+1*. .

4. Findings

The impact of works council presence on employment growth is presented in Tables 1 and 2. Additionally, Table 2 also gives the effect of works councils on the speed of labour adjustment. In both tables, we focus on those establishments with 21-100 employees, within which sample the potential problems arising from the non-random distribution of works councils and their potential heterogeneity should be mitigated. But corresponding results for the whole sample of establishments with at least five employees are provided in Appendix Tables 2 and 3.

Table 1: Determinants of the Change in Employment, 1993-2001, in Establishments with 21-100 Employees (dependent variable: average annual log employment change)

Variable	Specification (1)	(2)
Works council	-0.0247	-0.0221
	(0.0081)	(0.0102)
Output demand change	0.4391	0.4062
	(0.0566)	(0.0730)
Wage	-0.0276	-0.0200
	(0.0093)	(0.0132)
Establishment size	0.0126	0.0136
	(0.0088)	(0.0105)
Newer establishment	—	-0.0017
		(0.0210)
Share of part-time employees	—	0.0577
		(0.0334)
Share of female employees	—	-0.0447
		(0.0288)
State-of-the-art technology	—	-0.0034
		(0.0050)
ITC	—	0.0115
		(0.0092)
Single establishment firm	—	0.0023
		(0.0117)
Publicly listed firm	—	-0.0123
		(0.0268)
Exporter	—	-0.0208
		(0.0105)
Constant + industry dummies	Yes	Yes
Adjusted R^2	0.56	0.54
F	6.82	4.59
N	129	104

Notes: The model specification is given by equation (3') and was estimated by OLS. The sample was extracted from a raw sample of 912 continuing establishments, 1993-2001. Variables in levels pertain to 2001. Employment change is measured as an eight-year difference (log change) divided by eight, while the output change is a seven-year difference divided by seven because output data are only available for 1993-2000). Establishment size is represented by the number of employees.

Our preferred specification for estimating the effects of works councils on employment growth over the period 1993-2001 is given by equation (3'). As discussed earlier, this exercise uses two cross-sections to characterize an eight-year interval. The results of implementing this model in Table 1 are quite striking. In particular, note the near replication of the *union* employment growth effects of -2.5% to -3% identified by *BMO* (1991). For the parsimonious specification in the first column of the table we do indeed obtain the result that works council plants have 2.5% slower employment growth than their works-council-free counterparts. Adding more controls in the second column yields a slightly reduced works council 'effect' of -2.2%. In both specifications, particularly the former, the works council coefficient estimate is well determined.

With the exception of plant size, the other variables in the parsimonious specification are statistically significant at the .01 level, and of the expected sign. The insignificance of employment-based establishment size variable is not necessarily surprising since we are restricting the sample, but note that the positive association contrasts with some earlier findings for the Anglophone countries. Interestingly, of the new regressors in the second column of the table few are statistically significant at conventional levels, and the main effect of their inclusion is to lower the overall statistical significance of the model.

Appendix Table 2 presents the results using now the whole sample. We have less faith in these results precisely because few plants with less than 21 employees have councils and almost all of those with more than 100 employees do. Nevertheless, it can be seen that the works council 'effect' is much the same as observed for the sub-sample. The directional influence of the other regressors, as well as their statistical significance, is also broadly the same. The exceptions are the effects of plant size and the legal status of the establishment which are much better determined in the full sample.

The above works council effects are based on employment differences between 1993 and 2001. We next turn to evidence based on our longitudinal panel, this time exploiting annual employment differences. The caveat in all of this is that past research points to very sluggish employment adjustment in Germany (e.g. Abraham and Houseman, 1994; Burgess, Knetter, and Michelacci, 2000). In other words, we anticipate that employment inertia will be high and likely dominate the process of employment determination. In terms of models (4') and (5), the parameter λ should approach unity (and be highly statistically significant) while λ_1 should be close to zero (and perhaps insignificant).

Table 2: Employment Determination based on a Dynamic Labour Demand Model, Giving Works Council Effects on the Speed of Labour Adjustment and Employment Growth, Establishments with 21-100 Employees (dependent variable: l_{it}, all variables in first differences)

	Specification	
Variable	(1)	(2)
l_{it-1}	0.9905	0.9877
	(0.0516)	(0.0509)
$Wage_{it}$	-0.1173	-0.1171
	(0.0506)	(0.0505)
$Wage_{it-1}$	0.0672	0.0673
	(0.0486)	(0.0485)
Price of intermediate input$_{it}$	0.0137	0.0134
	(0.0134)	(0.0132)
$Capital_{it}$	-0.0104	-0.0100
	(0.0115)	(0.0115)
$Shock_{it}$	0.1209	0.1212
	(0.0416)	(0.0418)
l_{it-1}* *Works council*$_{it}$ [a]	-0.0085	
	(0.0097)	—
Works council$_{it}$* t [b]		-0.000017
	—	(0.000019)
Constant + time dummies	Yes	Yes
m_1	-4.17	-4.17
m_2	0.38	0.38
Sargan	204.5 [94]	204.8 [94]
Number of observations	678	678
Number of establishments	134	134

Notes: Model specifications in columns (1) and (2) are given by equations (4') and (5), respectively, and were estimated using the GMM-SYS method (1-step) (see text.) The number of observations is given by $O = \sum_i T_i$, where the maximum (useable) length of the time-series is 7 years, 1995-2001. Asymptotic standard errors robust to general cross-section and time-series heteroskedasticity are given in parentheses; m_1 and m_2 are first- and second-order serial correlation tests; and Sargan is a χ^2 test of the over-identifying restrictions from the instruments (degrees of freedom in parenthesis). The Wald test of the overall significance rejects the null in all cases. The instruments used are: $l_{it-2}, l_{it-3}, ..., l_{i1}$; w_{it-2}; p_{it-1}, and k_{it-1} for the differenced equations, and Δl_{it-1}, Δw_{it-1}, Δp_{it-1}, Δk_{it-1} for the levels equations. w denotes the wage level, p the price of the intermediate input, and k the capital stock; the *shock* variable is defined as the first difference of (log) output demand, and p is given by intermediate input divided by total employment. In the estimation, we have used the DPD 1.2 software for OX, version 3.30, available at http://www.nuff.ox.ac.uk/Users/Doornik.

[a] denotes works council effect on the speed of employment adjustment.

[b] denotes works council effect on employment growth.

The results in the first column of Table 2 confirm these expectations. As can be seen, the coefficient estimate of the lagged dependent variable is very large and close to unity, while the value of λ_1 is both small and statistically insignificant. Fitting the same model to data for the whole sample – see the first column of Appendix Table 3 – produces virtually the same results.

The selfsame panel framework also allows us to evaluate the association between works council presence and employment growth although, as we have cautioned, persistence in the employment data and our focus on annual changes may prove limiting in this regard. Indeed, as can be seen from the second column of Table 2, the direction of the works council effect is of the expected sign but the estimate is statistically insignificant. (Again, Appendix Table 3 shows the same result for the all-establishment sample.) Evidently, in the German case the worker representation growth differential is best evaluated using a wider change interval than is permitted by dynamic analysis.

5. Conclusions

There is a remarkable convergence in the literature as to the effects of worker representation on employment change. The conclusion of *BMO* (1991) that worker representation – in their case, union coverage or density – costs jobs has been replicated in subsequent British studies and indeed for Anglophone countries. The central estimate is slowed employment growth of about 2.5% a year. The present exercise shows that this result seems to hold for Germany too, via the agency of the works council. As a practical matter, however, the *generally* sluggish employment adjustment process in Germany limits what may be expected from a truly dynamic model so that this conclusion relies on cross-sectional rather than panel estimates.

This constancy in the effect of worker representation on employment is in one sense unsurprising. It is after all illustrative of classic insider behaviour, also hinted at in analysis of the employment effects of unions using individual data (see Montgomery, 1989). Consequently, we should not be surprised if similar evidence is uncovered for other nations, once allowance is made for the particular collective bargaining and legislative regime.

6. Appendix

Table A.1: Descriptive Statistics and Definition of Variables (establishments with 21-100 employees)

Variable	Obs.	Mean	St. dev.	Definition
Employment	169	3.772	0.437	Total employment (in logs).
Employment change	169	-0.006	0.063	8-year employment change (1993-2000) divided by 8 (log change).
Output demand	157	16.070	0.982	Real sales (nominal sales deflated by the GDP deflator) (in logs).
Output change	140	-0.003	0.072	7-year change (1993-2000) divided by 7 (log change).
Wage	161	8.193	0.415	Real gross wages per employee (in logs).
Works council	162	0.395		Dummy: 1 if there is a works council, 0 otherwise.
Exporter	166	0.307		Dummy: 1 if the establishment exports, 0 otherwise.
Newer establishment	164	0.037		Dummy: 1 if the establishment is less than 5 years old in 1993, 0 otherwise.
Single establishment firm	168	0.756		Dummy: 1 if the establishment is an 'independent, autonomous enterprise' or an 'independent institution without other establishments', 0 otherwise.
Share of female employees	169	0.324	0.265	Percentage of female employees.
Share of part-time employees	145	0.168	0.200	Percentage of part-time employees.
State-of-the-art technology	169	2.083	0.848	1 through 5 index of the state of technical equipment, 1 being thoroughly up-to-date and 5 being very old.
ITC	169	0.639	0.482	Dummy: 1 if the establishment 'has been involved' in investments in information and communication technology, 0 otherwise.
Publicly listed firm	168	0.030		Dummy: 1 if the firm is a publicly listed firm, 0 otherwise.

Notes: The full sample comprises 912 continuing establishments, 1993-2001. From this raw dataset we extracted a sample of 802 establishments with at least 5 employees, 169 of which had more than 20 and fewer than 100 employees in 2001. Variables in levels pertain to 2001. Employment change is measured as an eight-year difference (log change) divided by eight, while the output change is a seven-year difference divided by seven because output data are only available for 1993-2000.

Table A.2: Determinants of the Change in Employment, 1993-2001, in Establishments with at Least 5 Employees (dependent variable: average annual log employment change)

	Specification	
Variable	(1)	(2)
Works council	-0.0226	-0.0208
	(0.0057)	(0.0067)
Output demand change	0.3807	0.3601
	(0.0219)	(0.0248)
Wage	-0.0240	-0.0193
	(0.0042)	(0.0057)
Establishment size	0.0046	0.0052
	(0.0015)	(0.0019)
Newer establishment	—	0.0148
		(0.0101)
Share of part-time employees	—	0.0330
		(0.0173)
Share of female employees	—	-0.0136
		(0.0128)
State-of-the-art technology	—	-0.0060
		(0.0027)
ITC	—	0.0015
		(0.0049)
Single establishment firm	—	-0.0022
		(0.0049)
Publicly listed firm	—	-0.0197
		(0.0063)
Exporter	—	0.0016
		(0.0056)
Constant + industry dummies	Yes	Yes
Adjusted R^2	0.46	0.44
F	15.09	9.94
N	574	471

Note: See Table 1.

Table A.3: Employment Determination Based on a Dynamic Labour Demand Model, Giving Works Council Effects on the Speed of Labour Adjustment and Employment Growth, Establishments with at least 5 Employees (dependent variable: l_{it}, all variables in first differences)

	Specification	
Variable	(1)	(2)
l_{it-1}	0.9933	0.9875
	(0.0300)	(0.0238)
$Wage_{it}$	-0.0846	-0.1250
	(0.0612)	(0.0679)
$Wage_{it-1}$	0.0546	0.1136
	(0.0356)	(0.0416)
Intermediate $input_{it}$	-0.0035	-0.0024
	(0.0148)	(0.0155)
$Capital_{it}$	0.0064	0.0101
	(0.0128)	(0.0094)
$Shock_{it}$	0.0780	0.0801
	(0.0272)	(0.0270)
l_{it-1} Works $council_{it}$* [a]	-0.0131	
	(0.0146	—
Works $council_{it}$ t* [b]		-0.000035
	—	(0.000046)
Constant + time dummies	Yes	Yes
m_1	-4.11	-4.04
m_2	0.36	0.34
Sargan	74.71 [66]	189.1 [66]
Number of observations	2902	2902
Number of establishments	542	542

Notes: See Table 2.

[a] denotes works council effect on the speed of employment adjustment.

[b] denotes works council effect on employment growth.

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