DISCUSSION PAPER SERIES

IZA DP No. 1188

The Effect of Worker Representation on Employment Behavior in Germany: Another Case of -2.5%

John T. Addison
Paulino Teixeira

June 2004

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

The Effect of Worker Representation on Employment Behavior in Germany: Another Case of -2.5%

John T. Addison
University of South Carolina and IZA Bonn

Paulino Teixeira
Universidade de Coimbra and GEMF, Portugal

Discussion Paper No. 1188
June 2004

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
Email: iza@iza.org

IZA Discussion Paper No. 1188
June 2004

ABSTRACT

The Effect of Worker Representation on Employment Behavior in Germany: Another Case of -2.5%*

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% a year. Using German data, 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 to 3 percent lower employment growth having controlled for wages, changes in demand, industry affiliation, various worker and establishment characteristics, and survival bias. That said, works councils do not seem to further slow the tortuous pace of employment adjustment in Germany.

JEL Classification: J23, J51

Keywords: unions, works councils, employment change, employment dynamics, survival bias

Corresponding author:

John T. Addison
Department of Economics
Moore School of Business
University of South Carolina
1705 College Street
Columbia, SC 29208
USA
Email: ecceaddi@moore.sc.edu

* We thank, without implicating, David Blanchflower for his helpful comments on an earlier draft of this paper.

1. Introduction

In an interesting analysis of the 1984 (1980) WIRS, 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 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 percent (5.7 percent) 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 -2.0 percent 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 non-manual 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 percent less than their nonunionized counterparts between 1980 and 1985.[1]

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 favor 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 Labor Market Research of the Federal Labor Office (now Federal Labor Agency) – which contains information on variables such as sales and capital missing from the corresponding datasets for Britain, namely, the WIRS/WERS. Since the German data also contain information on plant closings, we can address the issue of possible selection bias in employment growth equations based on survivors.

The plan of the paper is as follows. Section 2 addresses the issue of model specification. Section 3 provides brief background information the institution of the works council and the longitudinal dataset. Results of fitting our employment change (and dynamic labor demand) equations are given in section 4. A summary concludes.

2. Methodology

2.1 The standard employment growth equation

Most British 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.

Let us assume that employment level of establishment i in period t, l_{it}, is a function of union status, economic conditions, and other establishment-specific variables, such as industry dummies and so on. Then, denoting worker representation by U_i (a fixed variable between *t-j* and *t*) and the other establishment characteristics by X_i, we have

$$l_{it} = \alpha_o + \lambda l_{i\,t-j} + \delta U_i + X_i \beta + e_i \,, \tag{1}$$

where λ $(0 < \lambda < 1)$ indicates the degree of employment inertia over the *j*-year interval. In this framework, the (long-run) union effect will be then given by $\delta/(1-\lambda)$, obtained by setting $l_{it} = l_{it-j}$.

Empirical studies typically do not reject the null that $\lambda = 1$, which result has led to the employment growth equation

$$l_{it} - l_{it-j} = \alpha_o + \delta U_i + X_i \beta + e_i \,. \tag{2}$$

Alternatively, the employment change may be averaged between *t* and *t-j*. In either case, the 'union' employment growth differential is given by δ, under the assumption of random assignment of union/worker representation status.

2.2. Survival bias

Implementation of model (2) is based on a sample of surviving establishments (in our case establishments observed in both 1993 and 2001). But we also have information on closures, that is, on establishments that have failed between *t-j* and *t*. We are therefore in a position to evaluate the presence of any 'survival bias' in OLS estimation of model (2). Formally, this amounts to investigating whether the unobserved determinants of establishment failure are correlated with the unobserved determinants of employment change. If the hypothesis of no correlation between the error terms in the two equations is rejected, then the works council effect estimated using the standard model will either over- or under-estimate the true effect on employment growth. For example, if the correlation is negative, then establishments less likely to fail will have lower employment growth; the marginal effect of any regressor present in the two equations (selection and outcome regression) on employment growth will then depend on the impact of that regressor on the probability of survival. In the case of the works council variable, a variable that presumably explains both survival and employment growth, a negative correlation between error terms, combined with a negative impact of works council on survivability (Addison, Bellmann, and Kölling, 2004)**,** will result in a bigger employment reduction in the OLS estimation. The intuition in this case is that works councils contribute to the failure of establishments less prone to reduce employment.

More formally, and denoting the vector of all independent variables in model (2) by Ω, the problem can be re-formulated as

$$g_i = \Omega_i \omega + u_{1i}, \tag{3}$$

where $g_i \equiv (l_{it} - l_{it-j}) / j$. Average employment growth g_i is observed if $T_i = 1$ (i.e. if establishment *i* is a survivor); g_i is not observed if $T_i = 0$ (i.e. if establishment *i* failed). In turn, survivability is a function of vector *W* of explanatory variables as specified by the (latent) selection equation

$$T_i^* = W_i \gamma + u_{2i}, \tag{4}$$

where $T_i = 1$ if $T_i^* \geq 0$ and $T_i = 0$ if $T_i^* < 0$. In this framework, it follows that

$E(g_i \mid g_i \textit{ is observable})$
$= \Omega_i\omega + E(u_{1i} \mid T_i^* \geq 0) = \Omega_i\omega + E(u_{1i} \mid u_{2i} \geq -W_i\gamma) = \Omega_i\omega + \rho\sigma_{u1}(\phi / \Phi),$

where ρ is the correlation between u_1 and u_2, ϕ is the standard normal density function, and Φ is the standard normal cumulative distribution function. Clearly, rejection of no correlation (viz. $\rho = 0$) implies that ω_{OLS} is biased (and inconsistent). In other words, only by controlling for the correlation between u_1 and u_2 (the 'omitted' variable in the standard OLS estimation of model (3) using survivors) can one obtain the true effect of works council on employment growth. The marginal effect of regressor *k* on employment growth is given by (see Greene, 1993, p. 710)

$$\frac{\partial E(g_i \mid T_i^* \geq 0)}{\partial w_{ik}} = \omega_k - \gamma_k(\rho\sigma_{u_1} / \sigma_{u_2})\tau_i .$$

2.3 Panel estimation

Our 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[2]

$$l_{it} = \lambda l_{it-1} + \beta'(L)X_{it} + u_i + v_t + e_{it}, \tag{5}$$

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. The coefficient of the lagged employment variable captures the degree of sluggishness in labor adjustment: the bigger the coefficient, the lower is the speed of employment adjustment to exogenous shocks.

OLS estimation of dynamic labor demand models (i.e. with a lagged dependent variable and firm-specific effects) upwardly biases the estimated coefficients. First-differencing the dynamic labor demand equation (5) 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 (5) 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.

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 $U_i * l_{it-1}$ (we assume no change in the U status of establishment i) is introduced in equation (5). This gives the model

$$l_{it} = \lambda l_{it-1} + \lambda_1 U_i * l_{it-1} + \beta'(L) X_{it} + u_i + v_t + e_{it}, \quad (5')$$

where U_i is a dummy variable set equal to 1 if the establishment reports the presence of a works council, 0 otherwise.[3] 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 (5) by introducing the interaction term $U_i * t$ (where t represents a time trend)

$$l_{it} = \lambda l_{it-1} + \beta'(L)X_{it} + \delta U_i * t + u_i + v_t + e_{it}, \tag{6}$$

and then differencing to obtain[4]

$$\Delta l_{it} = \lambda \Delta l_{it-1} + \beta'(L)\Delta X_{it} + \delta U_i + \Delta v_t + \Delta e_{it}. \tag{6'}$$

3. The Institution and the Dataset

3.1 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 percent of establishments with

21-100 employees had works councils. In contrast, only 4 percent (no less than 94.5 percent) of plants with less than 21 (more than 100) employees had work councils. Findings for the subsample of establishments with 21-100 employees therefore merit special attention.

3.2 The Dataset

Our data are taken from the Establishment Panel of the Institute for Employment Research of the Federal Labor Agency. Each year since 1993 (1996), this panel has surveyed several thousand establishments from all sectors of the economy in western (eastern) Germany. It is based on a stratified random sample – strata for 16 industries and 10 size classes – from the population of all establishments with at least one employee covered by social insurance. To correct for panel mortality, exits, and newly-founded units, the data are augmented regularly, yielding an unbalanced panel. Data are collected in personal interviews with the owners or senior managers of the establishments by professional interviewers. The panel is created to serve the needs of the Federal Labor Agency, 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 waves. In turn, information on works council status is available in 1993, 1996, 1998, 1999, 2000, and 2001 which requires some manipulation. 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*.[5]

The full sample (i.e. establishments with at least 5 employees) in the beginning period (1993) comprises 2,959 establishments of which 771 were also observed in 2001. The remaining establishments exited the panel either by reason of closure (248 cases) or attrition (panel rotation, non-response, etc.). Missing data on certain key variables resulted in a further loss of some 270 observations. This problem is always confronted to a greater or lesser degree in longitudinal datasets, and in this case there was no discernible pattern in the missing data.

(Table 1 near here)

As shown in Table 1, employment growth across all establishments over the sample period 1993-2001 averaged -1.5 percent. This was the result of employment contraction of -2.7 percent a year in the slightly more than one-half of plants with works councils and very modest growth in employment of 0.1 percent a year in plants without works councils. The corresponding values for the subsample of plants with 21-100 employees were -0.7, -3.7 and 1.4 percent respectively. These figures make the *prima facie* case for the proposition that work councils retard employment growth. Also shown in the table is the distribution of plants closings by works council status. For the entire sample, roughly 55 percent of the shut-downs occurred in establishments with works councils.

We also collected information on employment, workforce characteristics (namely, the percentage of part-time and female workers), output demand, gross wages, intermediate inputs, and a variety of other establishment characteristics (specifically, a measure of establishment age, and whether or not the establishment is an exporter, uses state-of-the-art technology, invests in ITC, is a single establishment firm, and is publicly listed). The selection of these arguments was guided by their use in the literature, and they are supplemented by five industry dummies (see Appendix Table 1).

Note finally that 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 onward, 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, using OECD data.

4. Findings

The impact of works council presence on employment is presented in Tables 2 through 4. In each table, we consider two cases: the subsample of establishments with 21-100 employees and the full sample of all establishments with at least five employees.[6]

(Table 2 near here)

Consider first implementation of the standard employment growth model, given by equation (2). As discussed earlier, this exercise uses two cross-sections to characterize employment growth over our eight-year sample period, 1993-2001. The results of are quite striking. In particular, note the similarity between our findings of the effect of worker representation on employment growth and those reported by *BMO* (1991). In the first column of the table we obtain the result that works council plants record 2.8 percent slower employment growth than their works-council-free counterparts. The other statistically significant covariates for the subsample are: output demand change, establishment size, and the shares of part-time and female workers. Each is significant at the .05 level and is of the expected sign; although we should note that, in contrast with some earlier findings for the Anglophone countries, the coefficient estimate for the employment-based establishment size variable is positive.

The second column of the table presents results for the whole sample. It will be recalled that the incidence of works councils is spotty in small establishments (with less than 21 employees) and near universal in larger establishments (with more than 100 employees), so that in principle we prefer the results for the subsample where there is a balanced representation of works councils and where additionally works council powers are datum (thus controlling in part for the heterogeneity of the institution). In any event, it can be seen that the works council 'effect' is still negative and well determined even if somewhat reduced in absolute magnitude, at -2.1 percent. The directional influence of the other regressors is mostly the same but note that the wage, establishment size, and technology variables are much better determined than before while the influence of labor force structure/worker characteristics is much attenuated. In addition, note that publicly listed firms now grow at a materially slower rate than their counterparts.

(Table 3 near here)

The extent to which these results over- or under-estimate the true effect of works council is addressed in Table 3. The framework is that of models (3) and (4) in section 2. We assume here that the vector W (in the selection equation) includes all observable characteristics relevant to survival, namely works council status, the wage level, establishment size, the shares of part-time and female workers, and indicators of whether or not the establishment uses state-of-the-art technology, invests in ITC, is a single establishment firm, and is publicly listed. Six industry dummies were also included.

The two columns of the table show the impact of works councils, inter al., on employment change after accounting for survivability. As it can be seen, in the case of the subsample there is no evidence suggesting the presence of a statistically significant survival bias. Indeed, the likelihood ratio test does not reject the null ($H_O : \rho = 0$) of independence of the outcome and the selection models. At face value, the suggestion is that there is no reason for concern in providing OLS estimation of model (2). On the other hand, the obvious limitation of the identification strategy is revealed by the fact that none of the right-hand side variables in the selection equation is statistically significant.

Using data on the whole sample increases the number of surviving and non-surviving establishments (see Table 1). Specifically, the number of closings doubles and the number of survivors quadruples. The results are given in the second column of Table 3. It can be seen that now five of the nine variables in the selection model are statistically significant at .05 level, and the null ($\rho = 0$) can be rejected. Moreover, the correlation between the error terms in panels (a) and (b) is negative. Taken in conjunction with the negative impact of works councils on survival, a negative correlation implies that the true effect of works councils on employment growth is somewhat weaker than was predicted by the OLS estimation, namely, -1.74 percent vis-à-vis -2.1 percent.

The evidence on the presence of a survival bias in OLS estimates is therefore not marked. We would have preferred to have obtained cleaner-cut results from the subsample, where the problems arising from heterogeneity are mitigated. In any event, unobserved factors affecting both selection and outcome equations are likely to prove elusive in the absence of properly designed datasets, so that we are perhaps forced to rely on standard OLS methods in greater degree than we would like.

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 therefore likely dominate the process of employment determination. Expressed in terms of models (5') and (6), the parameter λ should approach unity (and be highly statistically significant) while λ_1 should be close to zero (and perhaps insignificant).

(Table 4 near here)

The results in the first column of Table 4 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 – in the third column of the table – produces virtually the same results.

The model also includes time dummies, to capture macroeconomic events specific to a given year, the input price of labor**,** the price of intermediate input, and a measure of the stock of capital. Firm-specific demand shocks (the *shock* variable) are proxied by (log) changes in establishment output demand. Regarding the regression diagnostic statistics, they nowhere point to any specification problems: the errors are, as expected, negatively first order serially correlated, with no evidence of second order serial correlation; the set of selected instruments is valid (the *Sargan* test); and the joint significance of the coefficients included in the regression is clearly rejected.

The selfsame panel framework also allows us to evaluate the association between works council presence and employment growth (the parameter δ in equation (6)) although, as we have cautioned, persistence in the employment data and our focus on annual changes may prove limiting in this regard. As can be seen from the second and fourth columns of the table, the direction of the works council effect is of the expected sign but the estimate is statistically insignificant. (As before, the respective regression statistics are within the expected range.) 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 job growth has been replicated in subsequent British studies and indeed for Anglophone countries. The central estimate is slowed employment growth in the order of 2.5 percent a year. The present exercise shows that this result seems also to hold for the very different institutional arrangements of Germany. Using data from two cross sections we found that works councils were associated with reductions in employment growth of between 2.1 and 2.8 percent a year.

In a new departure, we also attempted to assess the contribution of survival bias to these outcomes and to look for supportive evidence using panel estimation methods. In the former case, where we detected evidence of such bias (i.e. for the full sample) it was of an unexpected direction: the unobserved factors associated with survival were also those that lead to slower employment growth. The fuller implication was that OLS methods overstate the negative effect of works councils on employment growth. Taking account of selection, the works council effect on employment growth in the full sample was reduced from -2.1 to -1.74 percent per year. Given this result, and the seeming absence of selection bias for our preferred sample, we would conclude either that employment growth is not an appropriate maximand after all, or that the modeling of selection is especially fraught with difficulty.

Exploiting the longitudinal nature of our dataset, using an employment adjustment specification in levels of variables, we reported a negative association between works council presence and a time trend. Unfortunately, while fully consistent with the cross-section results, this growth effect was not statistically significant at conventional levels. This was not altogether surprising given the very high levels of employment inertia in the annual employment data, even if there was no suggestion that works councils actually added to this sluggish employment adjustment process.

Pending more work on potential survival bias and the use of longer time series permitting improved analysis of the employment adjustment process – as well as variation through time in the crucial worker representation variable – we must perforce rely more on the cross-section results. As noted, these conform closely with international findings on the employment effects of unions at the workplace. This commonality of outcome may be illustrative of classic insider behavior, also hinted at in analysis of the employment effects of unions using individual data (see Montgomery, 1989).

Endnotes

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.

2. Of course in this case the findings are only valid for surviving establishments.

3. For expositional convenience we ignore any interaction between U_i and X_{it}. Note, too, the absence of a U_i term as estimation in first differences eliminates any time-invariant regressor.

4. 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).

5. Less than 2 percent of all establishments changed works council status over the eight-year interval. Accordingly, we chose to drop them from the sample.

6. For the subsample, the population is defined by employment levels obtaining at end of period, namely, 2001. Ensuring that establishments had between 21 and 100 employees in both 1993 and 2001 had a negligible impact on the regression results.

References

Abraham, K.G. and Houseman, S.N. (1994). 'Does employment protection inhibit labor market flexibility? Lessons from Germany, France and Belgium.' In *Social Protection versus Economic Flexibility – Is There a Tradeoff?* (ed. R.M. Blank). Chicago, IL: University of Chicago Press, pp. 59-93.

Addison, J.T. and Belfield, C.R. (2001). 'Updating the determinants of firm performance: estimation using the 1998 WERS.' *British Journal of Industrial Relations*, vol. 39, pp. 341-366.

Addison, J.T. and Belfield, C.R. (2004). 'Unions and establishment performance: evidence from the British Workplace Industrial/Employee Relations Surveys.' In *The Changing Role of Unions: New Forms of Representation* (ed. P.V. Wunnava). Armonk, New York: M.E. Sharpe.

Addison, J.T., Bellmann, L., Schnabel, C. and Wagner, J. (2003). 'German works councils old and new: incidence, coverage, and determinants.' *Schmollers Jahrbuch*, vol. 123, pp. 339-358.

Addison, J.T., Bellmann L., and Kölling, A. (2004). 'Works councils and plant closings in Germany.' *British Journal of Industrial Relations*, vol. 42, pp. 125-148.

Blanchflower, D.G. and Burgess, S.M. (1996). 'New technology and jobs: comparative evidence from a two country study.' Centre for Economic Performance Discussion Paper 1996-04, London School of Economics.

Blanchflower, D.G., Millward, N. and Oswald, A.J. (1991). 'Unionism and employment behavior.' *Economic Journal*, vol. 101, pp. 815-834.

Blanchflower, D.G. and Oswald, A.J. (1990). 'Unions and jobs: further evidence.' Dartmouth College Working Paper.

Blundell, R. and Bond, S. (1998). 'Initial conditions and moment restrictions in dynamic panel data models.' *Journal of Econometrics,* vol. 87, pp. 115-143.

Booth, A.L. and McCulloch, A. (1999). 'Redundancy pay, unions, and employment.' *Manchester School*, vol. 67, pp. 346-366.

Burgess, S., Knetter, M. and Michelacci, C. (2000). 'Employment and output adjustment in the OECD: a disaggregate analysis of the role of job security provisions.' *Economica*, vol. 67, pp. 419-435.

Fernie, S. and Metcalf, D. (1995), 'Participation, contingent pay, representation and workplace performance: evidence from Great Britain.' *British Journal of Industrial Relations*, vol. 33, pp. 379-415.

Freeman, R.B. and Lazear, E.P. (1995), 'An economic analysis of works councils'. In *Works Councils: Consultation, Representation, and Cooperation in Industrial Relations* (eds. J. Rogers and W. Streeck). Chicago, IL: University of Chicago Press, pp. 27-50.

Greene, W. (1993). *Econometric Analysis*. New York, NY: Macmillan (2ed.).

Kölling, A. (2000). 'The IAB Establishment Panel.' *Schmollers Jahrbuch*, vol. 120, pp. 291–300.

Leonard, J.S. (1992). 'Unions and employment growth.' *Industrial Relatio*ns, vol. 31, pp. 80-94.

Long, R.J. (1930. 'The impact of unionization on employment growth of Canadian companies.' *Industrial and Labor Relations Review*, vol. 46, pp. 691-701.

Machin, S. and Wadhwani, S. (1991). 'The effects of unions on organisational change and employment.' *Economic Journal*, vol. 101, pp. 835-854.

Montgomery, E. (1989). 'Employment and unemployment effects of unions.' *Journal of Labor Economics*, vol. 7, pp. 170-190.

Nickell, S.J., Wadhwani, S. and Wall, M. (1992). 'Productivity growth in United Kingdom companies, 1975-1986.' *European Economic Review*, vol. 36, pp. 1055-1085.

Official Journal (2002), 'Directive 2002/14/EC of the European Parliament and the Council of 11 March 2002 Establishing a General Framework for Informing and Consulting Employees in the European Community.' *Official Journal of the European Communities* L 80 of 23.3.2003: 29-33.

Wooden, M. and Hawke, A. (2000). 'Unions and employment growth; panel data evidence.' *Industrial Relations*, vol. 39, pp. 88-107.

Table 1
Percentage annual growth rates in employment in continuing establishments and numbers of plant closings, 1993-2001

	Establishment size	
	21-100 Employees	All establishments (≥5 Employees)
(a) Employment growth		
All establishments	-0.7 (n =169)	-1.5 (n =771)
Works council establishments	-3.7 (n =64)	-2.7 (n =429)
Non-works council establishments	1.4 (n =98)	0.1 (n =308)
(b) Plant closings		
All establishments	68	248
Works council establishments	32	135
Non-works council establishments	36	112

Table 2
Determinants of the Change in Employment, 1993-2001 (Dependent variable: average annual log employment change)

Variable	Establishment size: 21-100 Employees	Establishment size: All Establishments (≥5 Employees)
Works council	-0.0280 (0.0085)	-0.0206 (0.0060)
Output demand change	0.3689 (0.0536)	0.3566 (0.0231)
Wage	-0.0121 (0.0111)	-0.0223 (0.0052)
Establishment size	0.0193 (0.0090)	0.0046 (0.0016)
Share of part-time employees	0.0645 (0.0259)	0.0247 (0.0156)
Share of female employees	-0.0433 (0.0199)	-0.0143 (0.0103)
State-of-the-art technology	-0.0066 (0.0043)	-0.0075 (0.0025)
Single establishment firm	0.0025 (0.0090)	-0.0030 (0.0045)
Publicly listed firm	0.0086 (0.0206)	-0.0178 (0.0058)
Constant + industry dummies	Yes	Yes
Adjusted R^2	0.54	0.45
F	10.43	28.67
N (surviving establishments)	110	496

Notes: The model specification is given by equation (2) and was estimated by OLS. The sample was extracted from a raw sample of 771 continuing establishments with at least 5 employees, 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. Standard errors are given in parenthesis.

Table 3
Determinants of the Change in Employment, 1993-2001, Controlling for Potential Survival Bias (dependent variable: average annual log employment change)

	Establishment size	
	21-100 Employees	All Establishments (≥5 Employees)
Change in employment		
Works council	-0.0272 (0.0082)	-0.0174 (0.0064)
Output demand change	0.3694 (0.0498)	0.3499 (0.0236)
Wage	-0.0132 (0.0107)	-0.0253 (0.0055)
Establishment size	0.0196 (0.0085)	0.0024 (0.0018)
Share of part-time employees	0.0587 (0.0271)	0.0126 (0.0165)
Share of female employees	-0.0425 (0.0188)	-0.0127 (0.0107)
State-of-the-art technology	-0.0065 (0.0041)	-0.0079 (0.0027)
Single establishment firm	0.0018 (0.0087)	-0.0067 (0.0049)
Publicly listed firm	0.0121 (0.0207)	-0.0174 (0.0062)
Constant + industry dummies	Yes	Yes
Selection		
Works council	-0.1272 (0.2429)	-0.3223 (0.1960)
Wage	0.2162 (0.3003)	0.4303 (0.1672)
Establishment size	-0.0695 (0.2675)	0.2159 (0.0573)
Share of part-time employees	1.344 (0.9233)	1.8203 (0.5322)
Share of female employees	-0.1098 (0.5128)	-0.3993 (0.3174)
State-of-the-art technology	-0.0130 (0.1263)	0.0629 (0.0831)
Single establishment firm	0.1320 (0.2693)	0.3867 (0.0415)
Publicly listed firm	-0.6072 (0.4971)	0.0415 (0.2043)

Newer establishment	—	1.0486 (0.4733)
Constant + industry dummies	Yes	Yes
ρ	-0.3104 (0.6238)	-0.6460 (0.1457)
LR [$\chi^2(1)$]	0.15 [0.699]	3.65 [0.056]
Lambda	-0.0106 (0.0224)	-0.0279 (0.0075)
Wald χ^2	144.46	369.64
Surviving Non-surviving establishments	110 54	482 104

Notes: See models (3) and (4). They were implemented using the Heckman procedure in STATA, version 8. LR is the likelihood ratio test on the independence of the outcome and selection models.

Table 4
Employment Determination Based on a Dynamic Labor Demand Model Giving Works Council Effects on the Speed of Employment Adjustment and Employment Growth (dependent variable: l_{it}; all variables in first differences)

Variable	Establishment size: 21-100 Employees		All Establishments (≥5 Employees)	
l_{it-1}	0.9905 (0.0516)	0.9877 (0.0509)	0.9933 (0.0300)	0.9875 (0.0238)
$Wage_{it}$	-0.1173 (0.0506)	-0.1171 (0.0505)	-0.0846 (0.0612)	-0.1250 (0.0679)
$Wage_{it-1}$	0.0672 (0.0486)	0.0673 (0.0485)	0.0546 (0.0356)	0.1136 (0.0416)
Price of intermediate $input_{it}$	0.0137 (0.0134)	0.0134 (0.0132)	-0.0035 (0.0148)	-0.0024 (0.0155)
$Capital_{it}$	-0.0104 (0.0115)	-0.0100 (0.0115)	0.0064 (0.0128)	0.0101 (0.0094)
$Shock_{it}$	0.1209 (0.0416)	0.1212 (0.0418)	0.0780 (0.0272)	0.0801 (0.0270)
l_{it-1}* Works $council_{it}$ [a]	-0.0085 (0.0097)	—	-0.0131 (0.0146	—
Works $council_{it}$* t [b]	—	-0.000017 (0.000019	—	-0.000035 (0.000046
Constant + time dummies	Yes	Yes	Yes	Yes
m_1	-4.17	-4.17	-4.11	-4.04
m_2	0.38	0.38	0.36	0.34
Sargan	204.5 [94]	204.8 [94]	74.71 [66]	189.1 [66]
Number of observations	678	678	2902	2902
Number of establishments	134	134	542	542

Notes: Model specifications in columns (1) and (3) are given by equation (5'), while columns (2) and (4) are given by equation (6), 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.

Appendix Table 1

Descriptive Statistics and Definition of Variables (establishments with at least 5 employees)

Variable	Obs.	Mean	St. dev.	Definition
Employment	771	4.702	2.024	Total employment (in logs).
Employment change	771	-0.015	0.063	8-year employment change (1993-2000) divided by 8 (log change).
Output change	621	0.003	0.081	7-year change (1993-2000) divided by 7 (log change).
Wage	716	8.232	0.518	Real gross wages per employee (in logs).
Works council	737	0.582		Dummy: 1 if there is a works council, 0 otherwise.
Newer establishment	750	0.052		Dummy: 1 if the establishment is less than 5 years old in 1993, 0 otherwise.
Single establishment firm	764	0.621		Dummy: 1 if the establishment is an 'independent, autonomous enterprise' or an 'independent institution without other establishments', 0 otherwise.
Share of female employees	768	0.332	0.268	Percentage of female employees.
Share of part-time employees	678	0.164	0.200	Percentage of part-time employees.
State-of-the-art technology	769	2.159	0.754	1 through 5 index of the state of technical equipment, 1 being thoroughly up-to-date and 5 being very old.
Publicly listed firm	765	0.142		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 771 establishments with at least 5 employees. 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. Industries were aggregated into six groups: extractive; manufacturing using mineral and other resources; manufacturing of investment goods; manufacturing of consumer goods and construction; trade and transport, storage, and communications; and other services. Agriculture, financial services, and insurance were excluded from the sample.