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JOHN T. ADDISON
Queen's University Belfast (U.K.)
and
GEMF, Portugal
and
The Rimini Centre for Economic Analysis, Italy

PAULINO TEIXEIRA
Universidade de Coimbra, Portugal
and
GEMF, Portugal

"ARE GOOD INDUSTRIAL RELATIONS GOOD FOR THE ECONOMY?"

The Rimini Centre for Economic Analysis
Legal address: Via Angherà, 22 – Head office: Via Patara, 3 - 47900 Rimini (RN) – Italy
www.rcfea.org - secretary@rcfea.org

ARE GOOD INDUSTRIAL RELATIONS GOOD FOR THE ECONOMY?

Abstract

Using international data, we investigate whether the quality of industrial relations matters for the macro economy. We measure industrial relations inversely by strikes – which proxy we cross-check with an industrial relations reputation indicator – and our macro performance indicator is the unemployment rate. Independent of the role of other institutions, good industrial relations do seem to matter: greater strike volume is associated with higher unemployment. But these results apply in cross section. Holding country effects constant, the sign of the strikes coefficient is abruptly reversed. Although it does not seem to be the case that the line of causation runs from unemployment to strikes once we control for the endogeneity of strikes, it is also the case that support for the strikes proxy for industrial relations quality is much eroded.

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I. Introduction

The argument that the quality of labor relations matters for economic performance is widely encountered in the industrial relations literature even if it has proven difficult to sustain in practice. The best example is of course the ambiguous role of workplace governance as a determinant of workplace performance. For example, using British WERS data, Fernie and Metcalf (1995) found that authoritarian workplaces performed better on some dimensions of firm performance than did the archetypal employee involvement workplace, while Wood and de Menezes (1998) reported that workplaces assessed to have *high* high commitment management were not more effective than their counterparts with *medium-low*, *low-medium*, and *low* levels of high commitment management along seven dimensions of work performance. Recent British work on social partnership agreements paints a somewhat more optimistic picture, although this may be premature (see, respectively, Metcalf, 2003; Kelly, 2004).

For its part, the U.S. literature has long emphasized the potential of collective voice to improve the functioning of internal labor markets (Freeman and Medoff, 1984). This value-enhancing role of (union) collective voice hinges crucially in the model upon a constructive institutional response from management and a cooperative industrial relations environment. Identification of material pro-productive union effects has proved largely elusive, however, which outcome may of course reflect largely uncooperative labor relations in the United States in the last two decades. But, even abstracting from the union entity, the U.S. evidence on the impact of employee involvement/high performance work practices also provides very mixed results on the effects of labor management cooperation (for a review, see Addison, 2005). As in the British case, however, some recent work presents a more positive picture. Specifically, analyses of strikes – long treated as an outcome indicator rather than an input – have offered some interesting insights into the quality of industrial relations at the workplace and the effect of the latter on productivity (and practices such as TQM) and output quality (see Kleiner et al., 2002; Krueger and Mas, 2004).[1]

There has been almost no attempt to factor the industrial relations climate into the determination of macro outcomes, even if industrial relations *processes* have not been neglected in that literature. Thus, the degree of centralization in collective bargaining and, latterly, the extent of coordination of the bargaining parties/process have recently been entered alongside (the monopoly arguments of) union coverage and union density as determinants of unemployment and employment (see section 2).

In the most recent development, however, a measure of the climate of labor relations has been added to the growing number of collective bargaining variables in macro analysis. Specifically, Blanchard and Philippon (2004) have argued that, in countries where wages are largely determined by collective bargaining, the effects on unemployment of changes in the economic environment will depend in large part on the speed of learning of unions. The latter is seen as a reflection of the quality of the dialogue between the two sides, or the "quality of industrial relations." Proxying the latter by strike intensity (from 1960 to 1967), they report that countries with one standard deviation better quality had about 1 percent less unemployment than the average country in the first decade of the sample period, rising to 2-2.5 percent less in the last two decades. If this result is robust the authors have uncovered an important additional influence of industrial relations – its quality and not just its structure – on a key macro indicator.

The present paper seeks to further examine this intriguing if recherché notion. Our innovations include the use of *annual strike data* (and strike data averaged over the sample period) and the construction and deployment of *time-varying* institutional variables. Further, in order to tackle the important issue of strike endogeneity, we supplement our *country fixed effects* specification with findings from an Arellano-Bond (1991) panel estimator.

The plan of the paper is as follows. We briefly describe the new model and, at somewhat greater length, the broader macro-labor literature within which it is embedded. Next, we introduce the empirical models and the data used in the present inquiry, before presenting our empirical results. A summary and the requirements of a future research agenda conclude.

II. How the Quality of Industrial Relations Might Matter and the Issue of Implementation

Blanchard and Philippon (2004, p. 11) argue that the more unions and firms share a common economic model, or the more they discuss the economic implications of different shocks, the faster learning and adjustment is likely to be. Bayesian learning is thus central to the authors' formal model, in which the effects of shocks on unemployment depend largely on whether and how fast they are perceived by unions.[2]

The authors link this critical speed of union learning and adjustment to the quality of the dialogue that unions have with firms or, equivalently, with the quality of labor relations. The backdrop is the course of unemployment in 18 countries over four decades, 1965-2003. The quality of industrial relations is proxied by strike intensity in the sample period 1960-67, and strike intensity is measured by the maximum of days lost and workers involved, both normalized by the cross country standard deviations.[3] In practice, they also use a second, direct measure based on the survey responses on managers in large firms; specifically, to a 1999 World Economic Forum question seeking to determine the extent to which labor relations in their firms were “cooperative” (on an eight point scale from 0 to 7). Since the outcome indicator might be reflected in this response, Blanchard and Philippon ultimately use the 1960-67 strikes measure to instrument for the 1999 survey measure.

Simple bivariate regressions of unemployment in each of four decades separately on the strikes measure and the direct, survey measure (actual and instrumented) indicate a strong and statistically significant effect of the quality of industrial relations on unemployment – the former positively and the latter negatively. But their preferred specification interacts the measure of the quality of industrial relations with unobservable shocks common to the 18 countries in the sample.

In fact, this indicator of cooperation or the quality of industrial relations is but one of nine ‘institutional’ variables in the model, so that the impact of a common (unobservable) aggregate shock depends on a linear combination of all nine institutions. Apart from the cooperation measure, the other arguments are drawn from the employment protection literature, which it is instructive to review.

Arguably the main impetus behind the now extensive employment protection literature was Lazear’s (1990) cross-country analysis of the determinants of unemployment.[4] Lazear’s key argument is a *time-varying* measure of severance pay;

specifically, the amount of statutory severance pay due to a blue-collar worker with 10 years of service dismissed for reasons unconnected with his or her behavior. The only other independent variables in this sparse empirical representation are a quadratic in time, the growth in per capita GDP (to accommodate the notion that a growing economy vitiates at least in part the probabilistic costs of severance pay), and a demographic control (the population of working age). Lazear's central finding was of course that the more generous a country's severance pay entitlement, the greater its unemployment.

Following Lazear, the literature developed in two main directions. First, there was search for a more inclusive measure of employment protection than just severance pay. This culminated in the well-known OECD (1994) rankings of the 'strictness' of employment protection legislation for regular contracts and fixed-term contracts (and their composite).[5] Rankings for 16 countries were derived, pertaining to the "the late 1980s," so that the price of inclusiveness was a single data point rather than the time-varying measure of Lazear.[6]

The second development was the inclusion of a wider range of regressors than considered by Lazear. Chief among these variables have been union arguments, aspects of the unemployment insurance (UI) system, the tax wedge, and active labor market policies. Thus, collective bargaining arguments such as union density and union coverage have typically been included on the grounds that they are directly associated with pay, and thence unemployment. Additional arguments based on centralization or coordination have a very different pedigree. Initially, it was argued that a more centralized bargaining framework should lead to improved employment outcomes vis-à-vis a less centralized (but not totally decentralized) system because the disemployment and price/tax consequences of excessive wage increases would be more transparent, leading unions to take account of the effects of wage increases on all workers (Calmfors and Driffill, 1988). More recently, researchers have increasingly relied on the notion of *coordination*, ostensibly because the underlying model relies more on behavior than the fact of centralization (e.g. Soskice, 1990; Nickell, 1997; Nickell and Layard, 1999).[7] The spirit of the literature is nicely captured in Nickell's (1997, p. 68) dictum: "[U]nions are bad for jobs, but these bad effects can be nullified if both the unions and the employers can coordinate their wage bargaining activities."

For their part, more generous unemployment benefits lower the opportunity cost of unemployment and elevate wage pressure at the same time that they subsidize search. The upshot is higher equilibrium unemployment because of lengthened jobless duration. Ideally, the unemployment benefits measure should reflect the generosity of the UI system, including the maximum duration of unemployment insurance benefits and any prolongation under separate unemployment assistance benefits. Practically, researchers have been able to draw on a cross-country summary measure provided by the OECD, based on an average of gross replacement rates for individuals with two earnings levels, three family situations, and three duration categories of unemployment (for odd numbered years).[8]

Operating alongside unemployment benefits are measures that may have exactly the opposite effect on unemployment, namely, active labor market programs, operating directly on unemployment by improving search efficiency and indirectly by reducing wage pressure. Equally, they may not, most obviously perhaps where they signal future accommodation by the authorities to inflationary wage demands. Expenditures on active labor market policies are typically expressed as a percentage of GDP or as expenditures per unemployed individual relative to GDP per capita.[9]

Finally, the tax wedge – the gap between the gross labor costs to employers and the consumption wage paid to labor – may have little effect on unemployment because the incidence may be largely shifted on to labor. On the other hand, if markets are imperfect, there may be no offsetting wage cuts, while formal and implicit wage floors (set respectively by minimum wage legislation and social welfare provisions) will make labor taxes harmful to low-productivity workers.[10]

All of the above arguments plus the state of labor relations in 1999, instrumented by strikes in the 1960s, are deployed by Blanchard and Philippon in a specification that, as noted above, allows the impact of a common (unobservable) aggregate shock to depend on a linear combination of all of them. But note that although time varying information is available on most of these arguments (see, for example, Blanchard and Wolfers, 2000) the measures of employment protection, the UI replacement rate, the maximum duration of UI benefits, the tax wedge, active labor market policies, and the three collective bargaining indicators are *fixed*. To repeat, in each case the measures are interacted with the time dummy variables since the maintained hypothesis is that the main route through which institutions impact employment is how well they mediate economic shocks.[11]

With these preliminaries behind us, the more detailed findings of Blanchard and Philippon are threefold. First, cooperation in industrial relations in an equation containing just the cooperation variable and the three decade-long year dummies is negative and well determined. Alternatively put, strikes are positively associated with unemployment.

Second, when the other eight institutional regressors are added to the equation, the point estimate of cooperation in industrial relations falls somewhat in absolute magnitude but remains highly significant. Third, the statistically significant and opposing effects of coordination and union density on unemployment – the former lowering joblessness and the latter elevating it – remain well determined.

III. Models and Data

Let us denote the key labor market performance indicator – unemployment – by y. Assuming that countries in the dataset are observed at different points in time, unemployment in country i in period t is then given by y_{it}. Further assume that in each country, at each data point, we observe a set of country-specific labor market institutions, X_{itj}, $j=1, 2,\ldots, k$; $i=1,2,\ldots, N$; and $t=1, 2, \ldots, T$.

Measuring how institutions impact labor market outcomes has typically been addressed in one of two alternative ways. First, it has been assumed that the role of any given labor market institution can be captured independently of, or in interaction with, other institutions (see, respectively, Nickell, 1997; Belot and van Ours, 2004). Second, institutions may be depicted as interacting with shocks, either ameliorating or aggravating the impact of adverse exogenous shocks (Blanchard and Wolfers, 2000). In this latter case, the impact of a shock can be modelled as a function of given set of institutions, yielding a nonlinear model in the parameters, whereas the former case is linear by definition and can be estimated using standard OLS techniques. Within these two approaches, the present paper assembles a new set of time-varying institutions, while inserting a new *institution*: the quality of labor relations.

Formally, let us first consider the following empirical model[12]

$$y_{it} = X_{it}\beta + c_i + u_{it}\,, \tag{1}$$

where X_{it} includes all the relevant labor market institutions, c_i is the cross-section unobserved effect (or unobserved country heterogeneity), and u_{it} is the idiosyncratic error, or disturbance, term, with $E(u_t \mid X_t, c) = 0$. For convenience, further assume that X_{it} contains both time-invariant and time-varying variables. Calendar time dummies can also be added to the model, as well as interactions between institutions. This model is linear in the parameters and the evaluation exercise will consist in obtaining an estimate of β. Obvious candidates are, respectively, the pooled OLS, fixed-effects, and random-effects estimators $\hat{\beta}_{OLS}$, $\hat{\beta}_{FE}$, and $\hat{\beta}_{RE}$. In the spirit of Blanchard and Philippon (2004), who divided the 1965-2003 period in longer time intervals than a year to avoid contamination from cyclical fluctuations, and if we for the moment neglect the fixed effects case by noting that the data are thin (occasioned by a short sample period – a maximum of six 5-year intervals – and modest changes in institutions through time), the main option is random effects (in the linear version of model (1)). This assumes that all cross-section heterogeneity will be picked up by the array of institutions, and that the unobserved effect c_i is uncorrelated with the observed j labor market institutions. However, results from fitting the standard pooled OLS model will be used to provide a set of initial estimates.[13] In this context, and again in the spirit of Blanchard and Philippon, we will also report results from a simpler exercise regressing the outcome variable (unemployment) on our indicator(s) of the quality of industrial relations in separate cross sections for each of the 5-year intervals making up our sample period.

Within the framework of model (1), the course of unemployment y_{it} is explained by either changes in the X_j institutions or changes in common across-country shocks (proxied by time dummies). Since within country changes in institutions may not be well suited to explain differences in outcomes across time because of the persistence of institutions (and common across-country shocks cannot of course explain differences between countries), it is worthwhile trying to experiment with the interaction between shocks and institutions in order to capture differences in labor market performance. The possibility that 'unfavorable' institutions only reveal their true nature under adverse states of nature requires a different modelling strategy, however, which can be translated into the following model:

$$y_{it} = (\theta_{1t} + \theta_2 d2_t + \ldots + \theta_T dT_t)(1 + \sum_{j=1}^{k} X_{itj} b_j) + u_{it}, \qquad (2)$$

where the variables $d2_t, \ldots, dT_t$ denote time period dummies so that $ds_t = 1$ if s=t. (These variables are proxies for the unobserved common across-country shocks.) As in equation (1), the variables X_j can represent both time-invariant and time-varying institutions. The model does not include any country dummies. Nor does it allow for the 'autonomous' impact of institution j on y_{it}. Rather, by specifying the impact of the time-specific shocks, $ds_t, s = 1,..,T$, as a function of a linear combination of institutions, $\sum_j X_{itj} b_j$, the model concentrates fully on whether, say, a negative shock (one that increases unemployment) translates into more unemployment due the presence of institution j. Under model (2), therefore, if b_j is positive and a given economy is hit by an adverse shock, then institution j 'creates' more unemployment. Correspondingly, if b_j is negative, then institution j

insulates the economy from any adverse shock, or at least softens its impact. Again, estimation on the model requires nonlinear techniques.[14]

Subject – to the caveats entered earlier, we will also examine models (1) and (2) in a fixed-effects framework, which for model (2) – the NLS case – amounts to simply adding country dummies. In the light of the potential endogeneity of strikes, the Arellano-Bond panel estimator will be implemented as well. In this case, the procedure involves both differecing (to eliminate the unobserved time-invariant country-specific effect) and instrumental variables (to solve for any feedback effect between the unemployment rate and strikes). Thus, setting $X_{it} \equiv (Z_{it}, w_{it})$, where Z_{it} is a vector of strictly exogenous variables, while w_{it} contains a lagged dependent variable, first differencing of model (1) yields:

$$\Delta y_{it} = \Delta X_{it}\beta + \Delta u_{it} , \qquad (3)$$

or, in the one lagged dependent variable case,

$$\Delta y_{it} = \Delta Z_{it}\Omega + \delta\Delta y_{it-1} + \Delta u_{it} . \qquad (3')$$

If we further assume that Z_{it} is strictly exogenous (i.e. $E(Z_{it}u_{is}) = 0$ for all s and t), then the set of valid instruments for the lagged dependent term Δy_{it-1} at time t can be represented by $(y_{it-2}, y_{it-3},..., y_{it1})$. Finally, if $E(w_{it}u_{is}) = 0$ for all $s>t$ and where (by reason of omitted variables, measurement error or simultaneity between y_{it} and w_{it}) $E(w_{it}u_{it}) \neq 0$ for all $s \leq t$, then w is no longer endogenous and will need to be instrumented. A valid set of instruments is $(w_{it-1},..., w_{i1})$ if there are no lagged w_{it} terms – or $(w_{it-1},..., w_{i1}, y_{it-2},..., y_{i1})$, for example, for the one lagged dependent variable case.

Our database contains six time-varying institutional indicators (and two alternative measures of the quality of labor relations) for 19 OECD countries: Australia, Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Japan, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United

Kingdom, and the United States. As we have seen, the conventional labor market institutional variables are severance pay, the unemployment insurance replacement rate, union density, union coverage, union and employer coordination, and the tax wedge. (The absence of active labor market policies and benefit duration from this list is explained by the lack of time-series data for these arguments.) The manner in which we obtain six 5-year averages for each variable is outlined in Appendix Table 1. The variables are defined in such a way that an increase in a particular measure is expected to increase unemployment, which means in particular that the coordination measure is multiplied by -1. Table 1 provides the corresponding country means, with the sample period being divided into six 5-year periods from 1970-99 (namely, 1970-74, 1975-79, 1980-84, 1985-99, 1990-1994, and 1995-99).

(Table 1 near here)

We will use two proxies for the quality of labor relations. Our main measure is the strike rate (or 'strike volume' as it is sometimes known), namely, the number of days not worked per thousand paid employees. This ratio is based on revisions to the raw International Labor Office series on strikes (contained in the *Yearbook of Labor Statistics*, Tables 9A-D) kindly made available by Claus Schnabel of the University of Erlangen-Nürnberg. The data is available on an annual basis and is grouped here into 5-year averages. Our second proxy is a direct, survey-based indicator of the quality of industrial relations. It is taken from the *World Competitiveness Yearbook 2000*, published by the International Institute for Management Development (IMD), Switzerland. In the IMD survey, national respondents are asked to rate the state of industrial relations on a scale ranging from 1 ("hostile") to 10 ("productive"). Unlike the indirect measure of the quality of industrial relations, this indicator is solely time invariant – since publication of the IMD index started only in 1989.

(Figure 1 near here)

Figure 1 charts the course of the strike rate/volume over time for all countries in the sample, again for 5-year intervals. Although there is a considerable decrease in strike activity over time, it is also the case that countries show stability in their relative positions. Taking all possible combinations between 5-year periods (15 in total), the Spearman rank correlation coefficients always exceed 0.8, other than for those involving the last five-year interval, where the estimates fall in 0.5-0.7 range.

IV. Findings

Estimates of a simple model in which the dependent variable (unemployment) is solely a function of the selected measure of the quality of labor relations (respectively, 'strike rate' and 'cooperation') is given in Table 2 for six separate cross sections of the data. In the first row of the table, the strikes measure assumes a different value for each 5-year period. In the second row, however, the direct reputational (i.e. survey) measure is fixed at its 2000 reported value throughout, so that only the unemployment rate changes. Since the course of unemployment over the period may influence the perceptions of survey respondents as to the quality of industrial relations (in 2000), we also instrumented the IMD index by the observed strike rate/volume in the 70s, 80s, and early 90s (the third row).

(Table 2 near here)

We find that greater strike volume is associated with heightened unemployment, while the direct survey measure(s) of the quality of labor relations is associated with reduced joblessness. Note that these results accord with those reported by Blanchard and Philippon (2005, Table 1). As can be seen, most of the coefficient estimates are well determined, with the main exception of that for the indirect measure in the most recent 5-year interval. (The same broad findings hold when we ran separate regressions by decade using two clouds of data for each decade.)[15] Alternatively, and taking into account the (sample) standard deviation, we note that the estimated coefficients imply that countries with one standard deviation better quality labor relations have 0.8 to 2.9 percent less unemployment.

(Table 3 near here)

Table 3 provides results from using all of our labor market indicators and for the full sample period, 1970-99. Separate results are given for the strikes proxy and for the direct measure of the quality of industrial relations. Note that the two measures of labor quality are time invariant, strikes being set at their average value over the six 5-year periods (although this restriction will subsequently be relaxed), while all other labor market institutions are time-varying. For the pooled OLS estimates it can be seen that the strike rate is positively associated with unemployment and the survey measure (of the degree of cooperation in industrial relations) is negatively associated with unemployment. (The impact of one standard deviation better quality on unemployment is in the same range as reported above in Table 2.) The coefficient estimates for both arguments are well determined. Of the other institutional influences, the effects of higher replacement rates and greater coordination in collective bargaining are as expected (recall that the coordination score has been multiplied by -1) and the respective coefficient estimates are statistically significant at conventional levels. But observe that, although the effect of higher levels of union coverage (one of the two monopoly union arguments) is of the expected sign, this is not the case for the other monopoly union measure.

The estimates reported in columns (1) through (3) of Table 3 assume away unobserved cross-country heterogeneity. Since application of the standard Breusch-Pagan test rejected the null of constant variance of the error term (homoskedasticity), we re-estimated the base model using random effects. The GLS estimates provided in the next three columns of the table again support the prior that good industrial relations matter: the coefficients on the strike and reputation measures are of the expected sign and remain well determined. The performance of the labor-market institutions proper also improves somewhat, although the perverse effects of union density persist.

The estimates in the last three columns of Table 3 return us to the nonlinear model of equation (2). As can be seen, the effect of the labor market institutions proper further strengthens. And again the two measures of the industrial relations climate operate in the hypothesized manner, with strikes adversely impacting the effect of negative shocks and cooperation in industrial relations ameliorating them.

(Table 4 near here)

Table 4 repeats the regressions in columns (2), (5) and (8) of Table 3, substituting the 5-year, time-varying strikes measure for the measure in which strikes are averaged over the six 5-year periods. It can be seen that the coefficient estimate for strike rate/volume is no longer statistically significant in the random effects GLS specification but remains well determined in the NLS estimates in column (3) of the table).

(Table 5 near here)

Thus far, our results support the notion that good industrial relations – proxied inversely by strike volume and directly via a reputation measure of the degree of cooperation in industrial relations – do matter in influencing unemployment, *either independently or taken in conjunction with economic shocks*. In Table 5, we investigate whether or not the above relationships still hold when we control for country effects. In the first column of the table, we provide fixed effects estimates of the basic model, and in second column we add country dummies to the (NLS) specification in which institutions interact with shocks. The changes in the results are quite dramatic: the strike rate remains highly statistically significant but its sign is reversed, with strike volume now being negatively associated with unemployment. (Also the performance of the institutional variables deteriorates vis-à-vis the results in Tables 3 and 4.)

It is tempting to argue that the cross-section results reported earlier pick up long-run influences while the within estimator provides evidence of the (pro)cyclical nature of strikes reported in the strikes literature proper (see, inter al., Ashenfelter and Johnson, 1969; Hirsh and Addison, 1987; Cramton and Tracy, 2003). An immediate caveat is of course that the strikes measure in the present study is a conflation of frequency and duration, and it may be the case that strike duration is countercyclical – although contrary evidence, at least for large strikes, is provided by Harrison and Stewart (1993).

However, reverse causation requires that we find some instrument for the strikes series. One ambitious approach for the future might be directly to look for changes in labor law or in the rules governing collective bargaining. Here, we instead opt to implement the Arellano-Bond (1991) estimator in which tackling endogeneity involves differencing combined with instrumental variables methods: differencing to get rid of the unobserved time-invariant country-specific effect and instrumental variables to solve for the feedback effect between the unemployment rate and strikes. Observe that the Arellano-Bond GMM estimator has the property of using lagged levels of the endogenous variables as valid instruments for the endogenous regressors, which is of considerable advantage here because of the singular difficulty of finding a variable that is simultaneously correlated with strikes but uncorrelated with unemployment.

Thus far, we have used six 5-year periods, 1970-74 to 1995-99. In order to control for the endogeneity of strikes, we decided to expand the panel by using annual data on the same set of countries. Further, use of annual data makes our results more comparable with the most recent literature on job protection (e.g. Nickell et al., 2005, and Belot and van Ours, 2004). Also in line with this literature, we decided to introduce a lagged dependent term into the model and add a number of baseline (observed shocks) variables.

(Table 6 near here)

The results of our implementation of Arellano-Bond one-step GMM estimator are shown in Table 6. As mentioned, we are using annual observations, and they were obtained by using our raw annual data (or by simple interpolation if no annual data is available). Annual baseline variables – the real interest rate, real import prices, labor demand shocks, total factor productivity shocks, and money supply shocks – were taken from Nickell *et al*. (2005) and cover the period 1970-1995. The model includes two lagged dependent variables and the instruments used are lagged endogenous variables. (Models with two lagged dependent variables tend to perform better in terms of the relevant statistical tests.) The table also includes the tests on first and second order autocorrelation in the first differenced errors, Δu_{it}.[16] Under homoskedasticity, the null hypothesis that the overidentifying restrictions are valid should not be rejected (the *Sargan* test). The *Wald* statistic tests the null hypothesis that all the coefficients (excluding the time dummies) are zero.

The most striking result from Table 6 is that the strikes variable, while still evincing a negative sign in columns (1) and (2), is no longer statistically significant. (In column (3), the sign of the coefficient is positive but again not precisely determined.) In other words, after taking first differences to control for unobserved country heterogeneity and controlling for the endogeneity of strikes, the role of industrial relations quality is no longer evident in the data.

It is true that we are now dealing with a different type of setting – Table 6 uses annual observations and data on observed shocks (viz. aggregate demand shocks, produtivity shocks, and wage shocks) – but this new framework if anything provides improved precision as regards the role of the other institutional variables. Thus, the severance pay, replacement rate,and union density arguments are all statistically significant (pace Table 5, column (1)).[17]

Diagnostic tests in columns (1) through (3) perform as expected; in particular, the null of both the *Sargan* and m_2 tests is not rejected. The coefficients of all shocks or baseline variables also have the expected sign and conform closely with those reported by Nickell et al. (2005, Table 5): specifically, (positive) labor demand and productivity shocks impact unemployment negatively, while (positive) money supply and real import price shocks and higher real (long-term) interest rate generate higher unemployment. In a different experiment, again not reported in the table, we smoothed the strikes series using the Hodrick-Prescott filter. No material changes were detected.

V. Conclusions

In an important departure, it has recently been argued that what is good for industrial relations might after all be good for performance, this time at the macro level. Suggesting that the quality of industrial relations might be (inversely) proxied by strikes, Blanchard and Philippon (2004) adduce strong support for their claim that 'quality' matters in an analysis of unemployment determination in 18 OECD countries, 1965-2003. Thus, for example, they report that countries with one standard deviation better industrial relations enjoyed 2 to 2.5 percent lower unemployment over the course of the last two decades. Moreover, they argue that this quality effect is available over and above any *structural* benefits provided by union and employer coordination in collective bargaining.

In the present treatment, we further investigated the quality issue. Our innovations in ascending order of importance were the derivation of a direct moment-in-time indicator of labor relations quality supported by different survey data, the use of annual strike data (and strike data averaged over the sample period rather than being set at beginning-of-period values or indeed earlier) as well as the construction and deployment of other time-varying institutional variables, and finally the use of instrumental variables.

To begin with, the Blanchard-Philippon hypothesis held up really rather well. That is to say, higher strike volume averaged over the sample period and greater cooperation in industrial relations at end of period were found to be related to the macro performance indicator in the manner these authors hypothesized. And although allowing strike volume to vary through time – and for other institutional innovations – weakened the Blanchard-Philippon result they did not overturn it.

The fly in the ointment first became apparent when we deployed the within estimator. With the introduction of country dummies, the sign of the relation between strikes and unemployment abruptly reversed itself: higher strike activity was now associated with *lower* unemployment. At first blush, and drawing on the micro strikes literature, this result might be interpreted as reflecting cyclical influences, with the results in cross section picking up long-run influences. But what the result really indicated was the need to squarely address the causation issue. To this end, we further deployed the Arellano-Bond panel estimator, using instrumental variables to solve for the feedback effect between the unemployment rate and strike volume. The result was that the strike argument lost significance.

We conclude that in the absence of measurement error (and see Hauk and Wacziarg, 2004, for the superiority of the simple between estimator in such circumstances) the importance of trust between capital and labor has yet to be substantiated in the macro literature (no less than in the micro literature). That said, the rejection of measurement error is heroic when dealing with strikes and other institutional data and we would of course have preferred to use a more direct instrument (e.g. changes in labor law or the rules governing collective bargaining such as those engineered in Britain by Mrs. Thatcher in the 1980s) than the lagged values approach. According, our rejection of the recherché notion industrial relations quality matter is perforce tentative.

Endnotes

1. Strikes are of course not the only measure of industrial relations quality/performance. Another might be *grievances*. Two early studies of General Motors plants and of ten paper mills found that the number of grievances was inversely related to productivity (see, respectively, Katz et al., 1983; Ichniowski, 1984). A review of the earlier literature on the relationship between labor-management conflict and firm performance is provided by Belman (1992).

2. The model assumes an economy-wide union acting as a monopoly and seeking to maximize the wage bill period by period subject to a perceived labor demand. The actual labor demand is derived on the basis of a specific aggregate production function and a particular supply of capital function. The union is depicted as choosing the wage unilaterally on the basis of its perceptions of the parameters of the demand function. The level of employment is then set by firms on the basis of the actual demand schedule. If the union's perceptions are correct, it follows that the economy will proceed along a balanced growth path where capital, output, and real wages grow in line with productivity and employment holds constant. Now, imposing a negative shock to productivity growth, employment will only remain constant if union perceptions adjust fully and wages adjust appropriately. If perceptions do not fully adjust, perceived productivity will exceed actual productivity and employment will be lower until the expected productivity converges back to actual productivity. Assuming stochastic productivity – where actual productivity equals underlying productivity plus white noise and where underlying productivity growth can either be positive or zero – unions will learn and adjust wages at a rate according to the tightness of their prior and the standard deviation of the transitory component. The authors simulate one such path of wage (and hence employment) adjustment for two such values and an assumed fall in underlying total factor productivity growth from 1 percent to 0 percent. For the parameters chosen it takes around seven years for employment to return to its pre-shock value.

3. The use of the max specification is justified on the grounds that both measures are likely to be lower bounds on strike activity.

4. Lazear also examines the employment-population ratio, the labor force participation rate, and average hours worked, using the same regressors.

5. The regular contracts component included not only months of severance pay for no-fault dismissals but also procedural delays and other complications (such as prior authorization) before notice could be activated, as well as the perceived difficulty of dismissal as indexed by the legal conditions defining ‘fair’ and ‘unfair’ dismissals (trial periods, compensation payable, and extent of reinstatement). The fixed-term contract component included the objective grounds for entering into such employment relationships (and permitted derogations), together with the maximum number of successive contracts and their maximum cumulated duration.

6. The OECD (1999) subsequently revised its overall and component measures of employment protection for “the late 1990s,” thus providing researchers with two data points – and for a modestly enlarged sample of 19 countries. Note that other indicators of employment protection are available from surveys of employers (see for example Di Tella and MacCulloch, 1999).

7. Other analysts have deployed both centralization and coordination regressors (see Scarpetta, 1996; Elmeskov et al., 1998; OECD, 1999).

8. There is unfortunately no parallel time series information on the maximum duration of unemployment benefits.

9. Since spending on active measures is endogenous it is conventional to characterize the variable as a fixed effect, instrumenting it by the average spending over the sample period.

10. If the upshot of these post-Lazear innovations is mixed with respect to the impact of employment protection on unemployment (see Addison and Teixeira, 2003, pp.105-107), there is some agreement on the effect of the structure of collective bargaining and several of the other arguments. Thus, most studies report that increased coordination is associated with lower unemployment, either independently or in conjunction with employment protection and adverse shocks (Scarpetta, 1996; Nickell, 1997; Elmeskov et al., 1998; Nickell and Layard, 1999; OECD, 1999; Blanchard and Wolfers, 2000), while greater union coverage and higher union density are often associated with greater unemployment, although the relationships are often weak.

11. Note that all measures of labor market institutions are defined such that an increase in the measure is expected to increase the effect of an adverse shock, requiring in the case of

active labor market policies and degree of coordination that the measures are multiplied by -1.

12. This general specification can be designated as an Unobserved Effects Model (UEM) (Wooldridge, 2002, Chapter 10).

13. Pooled OLS assumes away unobserved effects c_i. Under the assumption that E(X_{it} c_i) = 0, the pooled OLS estimator is consistent but the error term will be serially correlated due to the presence of the time-invariant component c_i. Inference based on pooled OLS will then require robust standard errors. The random effects implementation of model (1) assumes E(X_{it} c_i) = 0 and exploits the serial correlation in the composite error, $e_{it}=c_i+u_{it}$, in a generalized least squares (GLS) framework.

14. From model (2) above, the partial effect of X_j on y is given by: $E(\frac{\partial y}{\partial X_j} \mid X, c) = \theta_s * b_j$, for a given year *s, s = 1, 2, …, T.*

15. The point estimate of the strikes measure was strongly statistically significant in the 1970s and 1990s, although not the 1980s, while the coefficients for the direct measures were well determined throughout.

16. Δu_{it} is necessarily second order serially uncorrelated, otherwise the GMM estimator is not consistent. In other words, E($\Delta u_{it} \Delta u_{it-2}) = 0$ is required.

17. The fixed effects case in Table 5 with annual observations generates virtually the same coefficient statistical significance.

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Table 1: Unemployment and Labor Market Institutions (Country Means, 1970-99)

	(1) Unemployment rate	(2) Replacement rate	(2) Union coverage	(3) Union density	(4) Tax wedge	(5) Union and employer coordination	(6 Severance pay	(7) Quality of labor relations	
								Strike rate/volume	Cooperation in industrial relations
Australia	0.067	20.7	3	45.4	29	1.9	1	369.4	6.2
Austria	0.029	27.7	3	54.1	54.1	3	2.1	5.6	7.7
Belgium	0.082	43.0	3	51.3	59.7	2	0	143.1	5.9
Denmark	0.064	46.7	3	69.8	62.7	2.3	0	202.4	7.9
Finland	0.068	23.0	3	66.0	53.7	2	0	396.1	7.4
France	0.080	30.3	3	16.6	54.7	2	0.9	160.6	4.4
Germany	0.058	28.7	3	33.3	54.0	3	0	30.0	6.7
Ireland	0.112	24.7	3	53.2	52.0	2	1.4	418.9	7.3
Italy	0.092	2.0	3	41.4	54.7	1.7	7.0	764.0	5.0
Japan	0.024	10.0	1	30.2	24.3	3	0	45.5	7.6
The Netherlands	0.065	49.0	3	33.0	56.0	2	0	25.6	8.2
New Zealand	0.043	26.7	1.8	46.8	35.0	1.3	3.3	321.6	7.1
Norway	0.032	24.3	3	55.1	62.3	2.5	0	75.2	7.7
Portugal	0.059	14.3	3	51.3	41.0	2	7.9	97.2	6.2
Spain	0.141	24.3	3	17.5	43.0	2	5.8	581.2	5.4
Switzerland	0.013	12.0	2	29.4	39.3	2	0	1.3	8.6
Sweden	0.039	19.7	3	78.2	60.3	2.3	0	92.4	7.8
United Kingdom	0.070	22.3	2.7	43.8	45.3	1.3	2.5	310.8	7.0
United States	0.064	12.3	1	20.4	36	1	0	223.6	6.6

Sources: The material in columns (1) through (5) is based on the definitions in Appendix Table 1; severance pay in column (6) is based on the Lazear (1990) measure; data on strike volume in column (7) was kindly provided by Claus Schnabel of the University of Erlangen-Nürnberg; and the index of cooperation in industrial relations, also in column (7), was taken from *The World Competitiveness Yearbook 2000* (International Institute for Management Development, Switzerland).

Table 2: Unemployment and the Quality of Labor Relations, Separate Cross-Section Regressions (six 5-year periods, 1970-74, 1975-79, 1980-84, 1985-89, 1990-94, and 1995-99).
(Dependent variable: unemployment rate. The quality of labor relations is proxied by the strike rate/volume and by the IMD index of cooperation in industrial relations.)

	Time period					
	1970-74	1975-79	1980-1984	1985-89	1990-94	1995-99
Strike rate/volume	0.00003 (3.90) F(1,16)=15.2	0.000026 (2.40) F(1,17)=5.75	0.00006 (1.94) F(1,17)=3.76	0.0012 (2.23) F(1,17)=4.99	0.0003 (6.28) F(1,17)=39.4	0.0002 (1.63) F(1,17)=2.66
Cooperation in industrial relations	-0.0071 (2.27) F(1,17)=5.13	-0.011 (2.45) F(1,17)=6.0	-0.0187 (2.63) F(1,17)=6.92	-0.0256 (2.94) F(1,17)=8.67	-0.019 (2.50) F(1,17)=6.26	-0.022 (3.34) F(1,17)=11.16
Cooperation in industrial relations (instrumented)	-0.0172 (2.81) F(1,17)=7.92	-0.0213 (2.71) F(1,17)=7.32	-0.035 (2.79) F(1,17)=7.79	-0.0506 (3.14) F(1,17)=9.86	-0.0377 (2.92) F(1,17)=8.52	-0.0326 (3.08) F(1,17)=9.49

Absolute t-statistics in parentheses.

Notes: The general model specification is given by $y_i = a + bx_i + e_i$, where the dependent variable, unemployment (y_i), is simply a function of the selected index of the quality of labor relations (x_i). In row 3 the IMD index was instrumented by the observed strike volume in the 70s, 80s, and early 90s. The number of countries in the sample is 19 (18 in 1970-74, row 1).

Table 3: Unemployment and the Quality of Labor Relations, 1970-99, 5-Year Averages. (Dependent variable: unemployment rate. The quality of labor relations is proxied by the strike rate/volume and by the IMD index of cooperation in industrial relations.)

	Pooled OLS			Random Effects (GLS)			Nonlinear least squares (NLS)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Severance pay	0.0029 (0.0026)	0.0018 (0.0013)	0.0026 (0.0015)	0.0031 (0.0015)	0.0019 (0.0015)	0.0026 (0.0015)	0.0410 (0.0178)	0.0173 (0.0158)	0.0332 (0.0179)
Replacement rate	0.0004 (0.0004)	0.0008 (0.0004)	0.00058 (0.00024)	0.0003 (0.0003)	0.0007 (0.0003)	0.00052 (0.00027)	0.0070 (0.0042)	0.0138 (0.0039)	0.0111 (0.0045)
Union density	-0.0006 (0.0003)	-0.0006 (0.0002)	-0.00034 (0.00027)	-0.0004 (0.0003)	-0.0005 (0.0002)	-0.00016 (0.00034)	-0.0099 (0.0023)	-0.0093 (0.0020)	-0.0051 (0.0029)
Union coverage	0.0176 (0.0059)	0.0080 (0.0065)	0.0099 (0.0056)	0.0156 (0.0074)	0.0076 (0.0064)	0.0072 (0.0081)	0.2790 (0.0922)	0.1490 (0.0838)	0.1711 (0.1015)
Union and employer coordination	0.0145 (0.0052)	0.0015 (0.0068)	0.0108 (0.0039)	0.0137 (0.0058)	0.0018 (0.0061)	0.0092 (0.0054)	0.2168 (0.0818)	0.0212 (0.0781)	0.1668 (0.0051)
Tax wedge	0.000006 (0.0003)	0.00008 (0.0003)	0.00002 (0.00021)	0.00005 (0.0004)	0.0001 (0.0003)	0.0001 (0.0003)	-0.00009 (0.00509)	0.0002 (0.0044)	-0.0013 (0.0051)
Strike rate (over time average)		0.00009 (0.00003)			0.00009 (0.00003)			0.0014 (0.0003)	
Cooperation in Industrial relations			-0.0093 (0.0036)			-0.0116 (0.0050)			-0.1616 (0.0620)
R^2	0.56	0.66	0.59	0.55	0.66	0.58	0.60	0.72	0.63
Wald χ^2				106.5	121.05	134.38			
F	14.95	41.0	13.37				10.74	16.85	11.25
N	92	92	92	92	92	92	92	92	92

Robust standard errors in parentheses.

Notes: The general specification of the model in columns (1)-(6) is given by equation (1) in the text, while in columns (7)-(9) it is given by equation (2). Sources and definitions of labor market institutions are given in Appendix Table 1. The sample period contains six 5-year data points, ranging from 1970-74 to 1995-99, and (a maximum of) nineteen countries (unbalanced panel).

Table 4: Unemployment and the Quality of Labor Relations, 1970-99, 5-year Averages. (Dependent variable: unemployment rate. The quality of labor relations is proxied by the strike rate/volume.)

	Pooled OLS	Random Effects (GLS)	Nonlinear Least Squares (NLS)
	(1)	(2)	(3)
Severance pay	0.0030 (0.0024)	0.0035 (0.0017)	0.0335 (0.0177)
Replacement rate	0.0005 (0.0004)	0.0004 (0.0003)	0.0071 (0.0041)
Union density	-0.0006 (0.0003)	-0.0005 (0.0002)	-0.0010 (0.0022)
Union coverage	0.0134 (0.0055)	0.0152 (0.0063)	0.2269 (0.0941)
Union and employer coordination	0.0106 (0.0054)	0.0133 (0.0057)	0.1615 (0.8249)
Tax wedge	0.0001 (0.0002)	0.00009 (0.0003)	0.0018 (0.0051)
Strike rate	0.000019 (0.000011)	0.0000003 (0.00001)	0.0005 (0.0002)
R^2	0.57		
Wald χ^2		0.55	0.61
F	21.23	109.3	10.20
N	91	91	91

Robust standard errors in parenthesis.
Notes: See Notes to Table 3.

Table 5: Unemployment and the Quality of Labor Relations, 1970-99, 5-year Averages, Fixed Effects, Nonlinear Least Squares with Country Dummies, and Between Effects Estimation.
(Dependent variable: unemployment rate. The quality of labor relations is proxied by the strike rate/volume.)

	Fixed Effects (FE)	Nonlinear Least Squares (NLS)	Between Effects (BE)
	(1)	(2)	(3)
Severance pay	-0.00018 (0.0038)	0.1628 (0.0640)	0.0017 (0.0018)
Replacement rate	0.0004 (0.0004)	0.0287 (0.0086)	0.0011 (0.00047)
Union density	0.0006 (0.0005)	0.0004 (0.0057)	-0.00059 (0.00026)
Union coverage	0.0118 (0.0126)	0.3053 (0.2313)	0.0059 (0.0104)
Union and employer coordination	0.0181 (0.0116)	0.2552 (0.2026)	-0.00081 (0.0096)
Tax wedge	0.0017 (0.0010)	-0.0147 (0.0113)	0.000009 (0.00056)
Strike rate	-0.00004 (0.000019)	-0.0014 (0.0004)	0.000104 (0.0000281)
R^2	0.62	0.86	0.85
F	7.53	14.16	6.43
N	91	91	19

Robust standard errors in parentheses.
Notes: The general specification of the model in column (1) is given by equation (1) in the text, while in column (2) it is given by equation (2), with country dummies added to the specification. Column (3) presents the between effects estimation. Sources and definitions of labor market institutions are given in Appendix Table 1. The sample period comprises six 5-year data points, from 1970-74 to 1995-99, and (a maximum of) nineteen countries (unbalanced panel).

Table 6: Unemployment and the Quality of Labor Relations, 1970-95, Annual Data, Arellano-Bond GMM Estimator.
(Dependent variable: unemployment rate.)

	First Differences	First Differences Plus Instrumenting Strikes	
	(1)	(2)	(3)
Severance pay	0.00081 (0.00062)	0.00101 (0.00060)	0.00067 (0.00054)
Replacement rate	0.00036 (0.00011)	0.00022 (0.00009)	0.00017 (0.00009)
Union density	0.00021 (0.00010)	0.00017 (0.00009)	0.00006 (0.00009)
Union coverage	-0.00448 (0.00447)	-0.00427 (0.00437)	-0.00426 (0.00430)
Union and employer coordination	0.00028 (0.00442)	-0.00106 (0.00433)	-0.00133 (0.00430)
Tax wedge	0.00049 (0.00027)	0.00054 (0.00025)	0.00059 (0.00025)
Strike rate	-0.0000003 (0.0000018)	-0.0000017 (0.0000019)	0.00000016 (0.0000017)
Baseline variables	Yes	No	Yes
Time dummies	Yes	Yes	Yes
Wald χ^2	2223.41	2036.31	2694.94
m_1	-8.19	-8.24	-8.26
m_2	-0.12	-0.20	-0.13
Sargan	240.65	304.96	271.23
N	323	333	323

Asymptotic standard errors robust to general cross-section and time-series heteroskedasticity in parentheses.

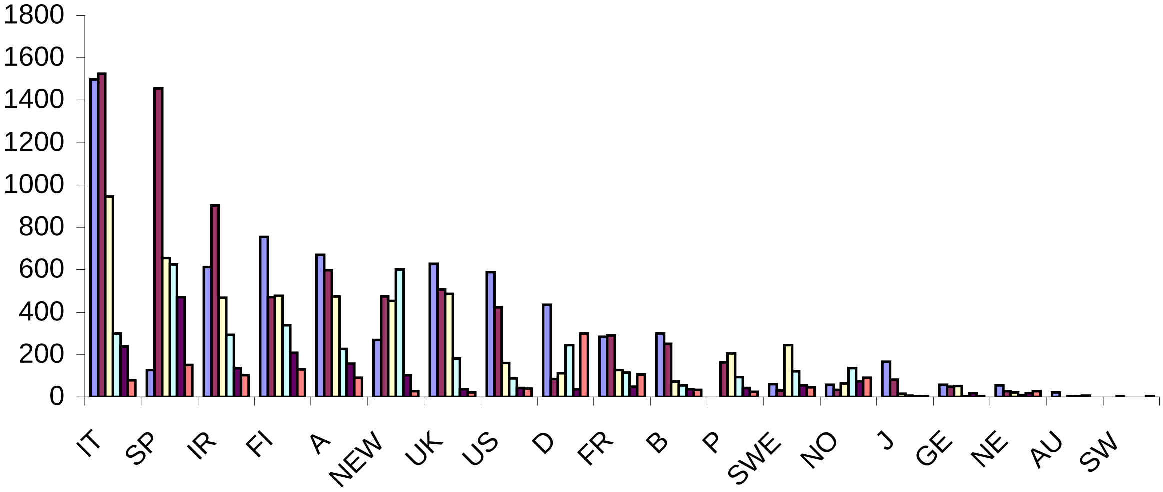
Notes: The general specification of the model is given by equation (3). The model includes two lagged dependent variables and, in columns (1) and (3), five baseline variables (the real interest rate, real import prices, labor demand shocks, total factor productivity shocks, and money supply shocks), taken from Nickell et al. (2005). In columns (2) and (3) the strikes rate/volume is taken as an endogenous variable. Instruments used for the endogenous regressors are lagged endogenous variables. m_1 and m_2 are first and second order autocorrelation tests in the first-differenced residuals. Under homoskedasticity, the null hypothesis that the overidentifying restrictions are valid cannot be rejected (the *Sargan* test). The *Wald statistic* tests the null hypothesis that all the coefficients (excluding the time dummies) are zero. Sources and definitions of the labor market institutions are given in Appendix Table 1. The sample period comprises twenty five annual data points, 1970 to 1995, and (a maximum of) nineteen countries (unbalanced panel).

Appendix Table 1: Description of Institutional Variables

Variable/source		Definition/range	Raw year/period	Interpolated periods
Employment protection (*EPL*)	Fixed measure (OECD, 1994, Table 6.7).	Ranking of employment protection legislation by "strictness". It is an average country ranking based on four different indicators, where 1 denotes the least rigidity.	1985-93	1970-99, five-year periods.
Replacement rate (unemployment insurance replacement rate) (*UIRR*)	Time-varying (OECD, 1994, Table 8.B.1).	Summary measure of benefit entitlements on a gross basis.	1971	1970-74; 1975-79
			1981	1980-84; 1985-89
			1991	1990-94; 1995-99
	Fixed measure (*) (Blanchard and Wolfers, 2000).	Share of past earnings replaced by unemployment benefits.	1983-88 and 1989-94	1970-99, five-year periods.
Union density (*UDEN*)	Time-varying measure (OECD, 1997, Table 3.3).	Trade union density.	1970	1970-74; 1975-79
			1980	1980-84; 1985-89
			1990	1990-94
			1994	1995-99
	Fixed measure (*) (Blanchard and Wolfers, 2000.	Trade union density.	1983-88 and 1989-94	1970-99, five-year periods.
Union coverage (*UCOV*)	Time-varying measure (OECD, 1997, Table 3.3).	Share of workers covered by union bargaining: 1 denotes less than 25 percent; 2 means from 25 to 75 percent; and **3** indicates over 70 percent.	1980	1970-74; 1975-79; 1980-84; 1985-89
			1990	1990-94
	Fixed measure (*) (Blanchard and Wolfers, 2000.		1994	1995-99
			1983-88 and 1989-94	1970-99, five-year periods.
Union and employer coordination (*TCOOR*)	Time-varying measure (OECD, 1997, Table 3.3).	Employer and union coordination in bargaining. It is assigned a value of 1 if there is no economy-wide coordination/centralization up to 3 if the degree of coordination/centralization is very high.	1980	1970-74; 1975-79; 1980-84; 1985-89
			1990	1990-94
			1994	1995-99
	Fixed measure (*) (Blanchard and Wolfers, 2000.	Employer and employee coordination in bargaining. It is coded between 2 and 6 in ascending order (the sum of employer and employee coordination).	1983-88 and 1989-94	1970-99, five-year periods.
Tax wedge (*TXWEDGE*)	Time-varying measure (OECD, 1997, Table 25).	Overall tax wedge (in percentage of average production worker earnings).	1978	1970-74; 1975-79
			1985	1980-84; 1985-89
			1994	1990-94; 1995-99
	Fixed measure (*) (Blanchard and Wolfers, 2000.	Tax burden. It is measured as the sum of the average payroll, income, and consumption tax rates.	1983-88 and 1989-94	1970-99, five-year periods.

Notes: The data on the fixed measures denoted by * was downloaded from http://www.mit.edu/blanchar/www.articles.html. Blanchard and Wolfers (2000) take a simple average of Nickell's (1997) original data over two periods, 1983-88 and 1989-94. Time-varying measures based on authors' own calculations.

Figure 1: Strike Rate/Volume in the Sample of OECD Countries



Notes: Strike rate/volume is given by the ratio of days not worked per thousand paid employees. The raw annual data on strikes is based on a revised version of the ILO series (Yearbook of Labor Statistics, Tables 9A-D), kindly made available by Claus Schnabel. The height of each column gives the average strike rate over five years for each of the six 5-year periods in the sample, beginning with 1970-74 and ending with 1995-99.