INTERNATIONAL INSTITUTE FOR LABOUR STUDIES

Discussion paper

DP/160/2005

Decent Work Research Programme

Institutional determinants of unemployment in OECD countries: A time series cross-section analysis (1960-98)

Lucio Baccaro
Diego Rei

P.O. Box 6
CH-1211 Geneva 22
Tel. 00 41 22 / 799 6128
Fax. 00 41 22 / 799 8542
E-mail: inst@ilo.org
http://www.ilo.org/inst

Institutional determinants of unemployment in OECD countries: A time series cross-section analysis (1960-98)

Lucio Baccaro
Diego Rei

International Institute for Labour Studies Geneva

ISBN Print: 92-9014-761-X
Web/pdf: 92-9014-762-8

First published 2005

Requests for this publication should be sent to: IILS Publications, International Institute for Labour Studies, P.O. Box 6, CH-1211 Geneva 22 (Switzerland).

TABLE OF CONTENTS

LIST OF TABLES

1. Introduction*

The aim of this paper is to assess the empirical plausibility of a vastly popular view of the unemployment problem, endorsed by international organizations like the OECD and the IMF, as well as policy-makers in several countries. In a nutshell, this holds that unemployment is caused by labour market institutions and should be addressed through systematic institutional deregulation (see Siebert, 1997). The policy implications are succinctly summarized in IMF (2003: 125): "Countries with high unemployment [are] urged to undertake comprehensive structural reforms to reduce 'labour market rigidities' such as generous unemployment insurance schemes; high employment protection, […] high firing costs; high minimum wages; non-competitive wage-setting mechanisms; and severe tax distortions." Is the empirical evidence really supportive of such strong policy conclusions?

Like others before us (Elmeskov et al., 1998; Nickell et al., 2001; Nunziata, 2001; 2002; IMF, 2003; Baker et al. 2002; 2003), we conduct a time-series cross-section analysis of OECD countries to address this question.[1] We use a standard dataset of institutional measures, originally assembled by Nickell and Nunziata (2001), mostly on the basis of OECD data and measures, and then updated/improved by other authors afterwards. Our specification is also fairly standard, and follows closely on the one in IMF (2003) – the paper with perhaps the strongest evidence supporting the deregulatory view. Both our data and models present, however, what we consider plausible changes, which are illustrated below. We pay a fair amount of attention to the statistical properties of models, especially as concerns non-stationarity of the data, serial correlation and other sources of bias in dynamic TSCS models with fixed effects, and violations of other standard assumptions concerning the error terms, which may impact the reliability of standard errors and, hence, of tests of hypotheses. We also test the robustness of our results against small specification and/or data changes, or changes in estimation methods.

Our preferred model is a reduced model in first differences with data averaged over five-year periods, which we arrive at by testing down. This includes only one macroeconomic control (the interest rate), the six institutional variables for which reasonably complete measurement series are available (employment protection, unionisation rate, benefit replacement rate, tax wedge, central bank independence, and wage coordination), and no interactions. Such a parsimonious model gives changes in labour market institutions more than a fair chance to explain changes in unemployment rates. It turns out that both changes in real interest rates and in an index of central bank independence are positively associated with changes in unemployment. All other institutional variables are instead generally insignificant or negatively signed, except the unionisation rate, whose impact on unemployment is significantly different from zero, and which has a modest effect according to our point estimates. Overall, we find little support for the deregulatory view. There seems to be no generalized unemployment-increasing effect of institutions in OECD countries in the period under consideration (1960-98). Restrictive macroeconomic policies appear to play a more important role.

The remainder of the paper is organized as follows. Section two lays out both the dataset and the models, and discusses the possible causal links between dependent and independent variables. Section three presents the results of various models based on both and five-year data. Section four provides an overview of findings. Section five concludes.

* Many thanks to participants in the 2nd Coffee Europe Workshop, as well as Peter Auer, Rob Franzese, Andrew Glyn, Bernhard Kittel, Naren Prasad, and Marco Vivarelli for comments on a previous version of this paper.

[1] We use the denomination of "time-series cross-section" analysis, rather than "panel" analysis, because, unlike in typical panel data, the time dimension *T* of our data is greater than the cross-sectional dimension *N*, so most of the asymptotic results that are of interest are in *T* rather than *N*.

2. Data and models

The time-series cross-sectional dataset on which we conduct our analysis is the one used by Baker et al. (2003).[2] It contains a series of macroeconomic and institutional measures for 18 OECD countries between 1960 and 1998.[3] This database is, in turn, very similar to the one used by IMF (2003), except for a few extra variables as well as small changes in the data (in particular concerning the years 1996-98, and/or specific countries).[4] Both these dataset are extensions of the original Nickell/Nunziata (2001) database of labour market institutions.[5] Information on the various measures is reported in Appendix 1.

Our basic model, in static form, is the following:

$$u_{i,t} = \beta_0 + \sum_j \gamma_j x_{j,it} + \sum_n \eta_n z_{n,it} + \sum_p \sigma_p h_{p,it} + \delta_i + \alpha_t + \varepsilon_{i,t}$$

where u_{it} is the unemployment rate in country i at time t, the xs are j institutional variables, the zs are n macroeconomic controls, the hs are p interactions, the δ_is are (N-1) country-specific fixed effects, the α_ts are (T-1) year dummies, and $\varepsilon_{i,t}$ is the stochastic residual. With yearly data, we add the lagged unemployment rate to the predictors:

$$u_{i,t} = \beta_0 + \beta_1 u_{i,t-1} + \sum_j \gamma_j x_{j,it} + \sum_n \eta_n z_{n,it} + \sum_p \sigma_p h_{p,it} + \delta_i + \alpha_t + \varepsilon_{i,t}$$

For reasons explained in the text, the models are often estimated in first differences, where first differencing wipes out the country fixed effects. The vector of institutional variables is the following:

$$\sum_j \gamma_j x_{j,it} = \gamma_1 EP_{i,t} + \gamma_2 UD_{i,t} + \gamma_3 BRR_{i,t} + \gamma_4 TW_{i,t} + \gamma_5 CBI_{i,t} + \gamma_6 BC_{i,t}$$

where *EP* is an employment protection index, *BRR* is the benefit replacement rate, *UD* is union density, *TW* is the tax wedge, *CBI* is an index of central bank independence, *BC* is an index of wage bargaining coordination. Some specifications also include $\gamma_7 BD_{i,t}$ for the duration of unemployment benefits, or $\gamma_8 BenefitGenerosity_{i,t}$ for a variable that multiplies BRR and BD. The vector of macroeconomic variables includes:

$$\sum_n \eta_n z_{n,it} = \eta_1 RIR_{i,t} + \eta_2 PROD_{i,t-1} + \eta_3 DCPI_{i,t} + \eta_4 TOTS_{i,t}$$

where *RIR* is the real interest rate, *PROD* is the (lagged) change in labour productivity,[6] *DCPI* is the change in the consumer price index, and *TOTS* is the terms of trade shock measure. Finally, the vector of interactions includes:

---

[2] Many thanks to John Schmitt and his colleagues for making the dataset available to us.

[3] The countries we consider in this analysis are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, United Kingdom, United States.

[4] We would also like to thank Xavier Debrun for making the IMF (2003) dataset available to us.

[5] This is publicly available at http://cep.lse.ac.uk/pubs/number.asp?number=502.

[6] As in IMF (2003), we lag the productivity variable due to possible endogeneity with unemployment.

$$\sum_p \sigma_p h_{p,it} = \sigma_1 EP_{i,t} * BC_{i,t} + \sigma_2 UD_{i,t} * BC_{i,t} + \sigma_3 BRR_{i,t} * BC_{i,t} + \sigma_4 TW_{i,t} * BC_{i,t} + \sigma_5 CBI_{i,t} * BC_{i,t}$$

and $\sigma_6 BD_{i,t} * BC_{i,t}$ or $\sigma_7 BenefitGenerosity_{i,t} * BC_{i,t}$ where indicated. Every institutional variable is interacted with the bargaining coordination measure. Also, every interactive term is expressed as deviation from the sample average ψ:

$$VARX_{i,t}VARY_{i,t} = \{VARX_{i,t} - \psi_{\mathrm{var}\,X}\} * \{VARY_{i,t} - \psi_{\mathrm{var}\,Y}\}$$

This allows us to interpret the coefficient of each institutional variable as the coefficient of the hypothetical country characterized by the average level of a given institutional measure (see Nunziata, 2002: 8).

Our specification follows closely on the one in IMF (2003) – which is, in turn, an extension of that of Nickell et al. (2001) (see IMF, 2003: 146). The IMF stands out among others because it includes both indicators of labour market institutions and a central bank independence index, and hence aims at investigating the effects of both labour market and monetary institutions. Our specifications present a number of relatively minor changes compared with others:

1) We use a different, and arguably better, measure of wage bargaining coordination, elaborated by Lane Kenworthy.[7] Unlike other indexes, this one "does not attempt to capture the degree of actual wage coordination in each country," which tends to give rise to impressionistic and possibly endogenous assessments (in the sense that the assessment of the degree of wage coordination in a particular country may be influenced by how well or badly the country in question performs), but rather is based on "a set of expectations about which institutional features of wage setting arrangements are likely to generate more or less coordination." (Kenworthy, 2003: 5). Two features of the bargaining system are likely to generate coordination: the degree of centralization of bargaining <u>and</u> the degree of employer coordination. The countries are scored year by year from one to five based on a range of secondary sources, covering the 1960-98 period. However, because Spain and Portugal were not democratic for a large part of this period, these two countries are not coded and are therefore excluded from our sample.

2) Unlike the IMF (2003), Nickell et al. (2001), and Nunziata (2001) we do not include country-specific time trends in the model. We agree with Baker et al. (2003: 15) that there seems to be "little theoretical justification … for allowing for a separate time trend for each country. To the extent that unemployment in the OECD economies is trended over time, … the role of … modelling [should] be precisely to explain such a trend, not to control for it."

3) Unlike the IMF (2003), we do not allow for country-specific terms capturing the trade-off between change in inflation and unemployment, but only a common term. Again, we agree with Baker et al. (2003: ft. 21, p. 15) that once the decision to pool is taken, i.e. to impose common coefficients across countries, we see no particular reason to allow only one coefficient to vary across countries and not others.

4) Our set of interactions is different from the IMF (2003) and others, because, unlike other specifications, which contain a rather eclectic set of interactions, we focus on the interaction between the degree of wage coordination in the economy and the various institutional variables. The reasons why we do so are explained below. It should be noted that in

[7] Many thanks to Lane Kenworthy for permission to use his index. This is available at: http://www.emory.edu/SOC/lkenworthy/WageCoorScores.xls

doing so, we are for all purposes estimating a semi-pooled model because the effects of institutions are allowed to vary systematically across bargaining regimes.

5) All of our models include time dummies to control for exogenous shocks affecting all countries simultaneously, unlike in IMF (2003).

If the deregulatory view of unemployment were true, we would see the coefficients of our labour market institutional variables (employment protection, union density, benefit replacement rate, tax wedge, and bargaining coordination) to be positively signed (except the coordination variable) and statistically different from zero. This is because, in an imperfect market scenario, in which wages are determined as the outcome of bargaining between firms and workers, the labour market institutions either directly increase the bargaining power of unions (e.g. the unionisation rate or employment protection), or reduce the willingness and capacity of the unemployed to bid down the wages of the employed (unemployment benefit replacement and duration), and, in so doing, indirectly increase the bargaining power of workers (see Nickell et al, 2001; see also Blanchard, 1999: 9; Nickell et al., 2001: 2-4; Schettkat, 2003: 772).

The empirical effects of *employment protection* legislation are, however, theoretically ambiguous. Employment protection should increase the bargaining power of unions because it becomes more difficult or costly for firms to lay off workers. The effects on flows are unclear, though. According to Nickell (1997:66), "laws that raise the cost of employment adjustment, notably those relating to employment protection, […] tend to reduce the inflow into unemployment and, because they make firms more cautious about hiring, […] also reduce the flow out of unemployment into work. This […] almost certainly reduce[s] the short-term unemployment (via the reduced inflow) and raise[s] long-term unemployment (via the reduced outflow). The overall impact on unemployment is likely to be rather small as these effects tend to cancel out" (see also Blanchard, 1999: 10; Bertola et al, 2001:30).

The *union density* measure is intended to capture union bargaining power and should be positively associated with the unemployment rate.[8] If unionisation leads to a wage premium, i.e., to labour costs above their market-clearing levels, some workers who are willing to work at the prevailing wage level do not find employment. This effect is likely to be greater the more elastic the labour supply (e.g., for women, youth, and old workers, see Bertola et al., 2003). Similarly, if unionisation leads to a more compressed wage structure, some workers, those with lower productivity, are likely to be priced out of the labour market. Freeman and Medoff (1984) argued, however, that unionisation has two faces: a labour monopoly face and a "voice" face. The latter compensates for the former by increasing workplace productivity. If this view is correct, then the effect of unionisation is theoretically indeterminate and depends on which of the two effects prevails. It has also been argued that when collective bargaining is coordinated, unions tend to internalise the externalities associated with their wage policies (Soskice, 1990; Nickell, 1997: 68). For this reason we expect the interaction between unionisation rate and bargaining coordination to be negative.

The *benefit replacement rate* variable captures the degree of generosity of the unemployment insurance system. This variable should be combined with a *benefit duration* variable. However, because the latter is not available for some countries and some years, several models presented below only include the former (see IMF, 2003; Nunziata, 2001, for a similar choice). More generous insurance systems may cause unemployment to rise through multiple channels. Generous unemployment benefits may "make unemployment less painful and thus strengthen the hand of workers in bargaining" (Blanchard, 1999: 12). Also, generous employment benefits may "reduce the 'effectiveness' of unemployed individuals as potential fillers of vacancies, by allowing them to be more choosy." (Nickell, 1997: 67) At the same time,

[8] The collective bargaining coverage rate would be preferable as a measure. This series is, however, largely incomplete.

a generous unemployment system may lead to a more efficient matching between the unemployed and available jobs, in which case the sign of the coefficient may be theoretically indeterminate.

The *tax wedge* variable is the sum of the payroll, income, and consumption tax rates. The impact of this variable on unemployment depends on who shoulders the burden of taxes (Nickell, 1997: 69), which in turn depends on the relative bargaining power of the parties. If taxes are entirely paid for by workers through lower post-tax wages, then the costs for employers are unaffected and labour demand should be unaffected as well. If workers increase their labour supply at existing wage levels to compensate for lower take-home pay, the relationship may even be negative, i.e. higher taxes may be associated with lower unemployment. If, however, taxes cannot be shifted onto wages, because of union bargaining power, or because of wage floors or compressed wage structures, then labour demand is likely to be negatively affected and, consequently, employment is likely to be negatively affected as well. Daveri and Tabellini (2000) argue that the effect of taxes is contingent on the nature of the wage setting institutions, and that the effect of the interaction between tax wedge and bargaining centralization is non-linear. Very centralized and very decentralized systems, those in which unions either do not have much bargaining power or are led to internalise the systemic consequences of their choices, are able to counterbalance the impact of tax growth, but intermediately centralized systems are unable to do so.[9]

The *wage bargaining coordination* variable is generally hypothesized to be associated with lower unemployment.[10] This is because centralized bargaining should lead to lower real wage settlements than uncoordinated bargaining (see Flanagan, 1999: 1157ff.; Hall and Franzese, 1997: 7-11; Franzese, 1997: 5-9). As explained by Franzese (1997: 5), "when wage/price negotiations occur in very fragmented (atomistic) units, the externality of one bargaining unit's wages (prices) lowering the real value of another's is ignored; thus atomistic wage and price settlements will be higher than need be since they must include increments to offset expected increases elsewhere in the economy … If, contrarily, bargaining occurs in encompassing or coordinated units, the externality is internalised and such increments are neither necessary nor desirable. Thus [coordinated wage bargaining] induces wage/price restraint and therefore lowers unemployment and inflation." The effect of bargaining coordination may be non-linear. Calmfors and Driffil (1988), for example, argued that the relationship between real wage growth and bargaining structure is hump-shaped, with very centralized/coordinated and very decentralized/uncoordinated systems performing best in terms of lowering the real wage and hence the unemployment rate. This is due to weaker market power of unions and firms in the decentralized than in the intermediately centralized setting (if workers in enterprise bargaining do not exercise restraint they are likely to lose their jobs to competitors, unlike workers in industry level bargaining). In the centralized/coordinated setting, unions have incentives to internalise externalities, so they spontaneously moderate their wage demands to avoid negative consequences on unemployment.

The *central bank independence* index is intended to capture the degree to which the monetary authority is able to resist political pressures to inflate the economy. The introduction of this variable in a model of the institutional determinants of unemployment is a peculiarity of the IMF (2003) paper, compared with similar papers (e.g. Nickell et al., 2001; Elmeskov et al., 1998). It is not clear what kind of impact this variable should have on unemployment when

[9] Daveri and Tabellini (2000)'s measure of labour taxes is, however, different from the measure advocated by Nickell (1997) and used in this and other papers.

[10] It also possible to hypothesize a positive relationship between bargaining coordination and unemployment. This is if one takes a "neoliberal position that considers any collective regulation of the labour market to be a performance-inhibiting rigidity." (Traxler and Kittel, 2000: 1156).

considered in isolation. In a rational expectation kind of framework, for example, central bank independence does not directly impact employment or unemployment. It reduces inflation with no real costs (Franzese, 1997: 2; see also Eijfingeer and De Haan, 1996, for a general overview). Political economists have devoted a great deal of attention to the interaction between central bank independence and bargaining structure. In an economy characterized by coordinated bargaining, the bargaining actors are more likely to heed the monetary policy announcements issuing from an independent central bank (and adjust their behaviour accordingly) than actors in an uncoordinated bargaining system. Hence, as explained by Hall and Franzese (1997: 10), "when bargaining is coordinated, the central bank may be able to influence the level of settlements and reduce inflation simply by signalling its policy intentions so that monetary policy does not raise the level of unemployment. Where wage bargaining is uncoordinated, however, such that small bargaining units have no reason to expect a direct response to their settlements and disincentives to exercise general moderation lest others fail to do so, the central bank may have to apply very tight monetary policies that induce substantial increases in unemployment before wage and price contracts will respond." Based on this reasoning, the interaction between central bank independence and wage bargaining coordination should be negatively associated with unemployment, other things being equal.[11] Also, because the effect of central bank independence is $\beta_{CBI} + \beta_{INT}$CBI*BC, and this is supposed to be positive for small values of bargaining coordination and negative for high values, β_{CBI} should be positive enough to compensate for the negative value of β_{INT}CBI*BC when BC equals one (i.e. when bargaining is completely uncoordinated).

As far as the macroeconomic controls are concerned, we expect the following relationships to hold. The *real interest rate* should be positively associated with unemployment. As explained by Blanchard (1999: 3), "other things equal, an increase in the real rate increases the user cost of capital. Investment decreases, leading over time to lower capital accumulation, and a decrease in employment. This goes on until the wages have adjusted and the increase in the profit rate matches the increase in user costs."[12] Nickell et al. (2001:3) point to another channel through which real interest rates may positively affect unemployment, that is, by increasing the returns on non-human wealth, which, in turn, increases the reservation wage of the unemployed and reduces their willingness to bid down the price of labour. According to Blanchard (1999:3), the effect of interest rates should be limited to the short-term, or should be small in the long run. Ball (1999), however, argues that protracted periods of restrictive monetary policies, with high real interest rates, do not just increase actual unemployment, but end up increasing equilibrium unemployment as well.

The change in the *inflation rate* variable should capture a possible trade-off between inflation and unemployment of the Phillips curve-type, i.e. higher values of this variable are expected to be associated with lower unemployment values.[13] In line with most macroeconomic theory, we expect this effect to hold in the short- but not in the medium- or long-term. In other words, the coefficient of this variable should not be significantly different from zero when data are averaged over five-year periods.

The *terms of trade shock* variable is expected to have a negative sign and is supposed to operate through real wage resistance. If there is a fall in terms of trade and the real wage does not adjust downwardly due to real wage resistance, unemployment rises. Vice versa, if terms of trade rise and real wages fail to follow suit, unemployment falls (see Nickell et al., 2001: 5).

[11] For a different model of the relationship between central bank independence and bargaining structure, and slightly different empirical results, see Iversen (1998); see also Cuckierman and Lippi (1999).

[12] On the role of interest rates, see also Fitoussi et al. (2000).

[13] Authors like Baker et al. (2002); Belot and Van Ours (2000); Nickell (1997), and others also insert the change in inflation variable in their models.

The duration of this effect depends of the speed of adjustment and is likely to disappear with data averaged over longer time frames.

The change in *labour productivity* variable is also expected to have a negative sign due to real wage resistance. As in the IMF's model (2003: footnote 35, p. 48), this variable is lagged due to possible endogeneity problems. If the rate of productivity growth suddenly decelerates, and workers continue to demand (and obtain) similar rates of growth in real wages as in the past, unemployment rises. Similarly, if the rate of productivity growth suddenly accelerates and workers fail to review upwards their wage demands, unemployment falls. As argued by Bertola et al. (2001: 17), "in the long run, there is no reason for unemployment to be affected by the particular level of [productivity] growth a country has settled upon, but it may take a long time for real wage growth to decelerate to its new equilibrium level."

Our models also include *interactions* between every institutional/organizational variable (employment protection, union density, benefit replacement, benefit duration, tax wedge, and central bank independence) and the wage bargaining coordination variable. This modelling choice is in line with a basic intuition of the variety of capitalism literature (see Hall and Soskice, 2001), that institutions function differently in different types of economic systems, and that a key factor distinguishing among types of capitalism is the degree of coordination in economic transactions – of which wage bargaining coordination is of paramount importance. Except for those cases (discussed above) for which clear theoretical predictions exist, we do not have strong priors concerning the sign of these interactions. To the extent that institutions affect unemployment by strengthening the bargaining power of workers, thus leading to higher real wage settlements, the sign of the institutional variables should be positive, and to the extent that a more coordinated bargaining system helps economic actors internalise the systemic consequences of their actions, the sign of these interactive terms should be negative.

3. Results

3.1 Models with Annual Data [14]

We begin our data analysis with OLS estimation of the full static model (table 1).[15] The first column in the table only contains the institutional variables (employment protection, union density, benefit replacement rate, tax wedge, central bank independence, and bargaining coordination) and their interactions with bargaining coordination as predictors. The second column adds in the macroeconomic controls (real interest rates, change in inflation, terms of trade change, and change in labour productivity). The third column also controls for unobserved (and time-unvarying) country-effects through the insertion of country dummies. The fourth column controls for unobserved time effects affecting all countries simultaneously, through the insertion of time dummies. The fifth column controls for both country and time effects as well as for a step increase in unemployment from 1973 on. A battery of tests is included at the bottom of the table, with two purposes: understanding whether the structure of the error term conforms to OLS assumptions and verifying the appropriateness of inserting country- and time-specific fixed effects, as well as the interactions.

[14] The estimation strategy with annual data draws heavily on Kittel and Winner (2001).

[15] All statistical analyses have been performed using STATA 8.0, except when explicitly indicated in the text.

Table 1. Annual data. Estimates in levels. Static models (intercept, country, and time dummies omitted).

	OLS	OLS controlling for macro-economic shocks	OLS controlling for country effects FE(c)	OLS controlling for time effects FE(t)	OLS with FE(c) and FE(t) + 1973-and-beyond dummy
Dependent var.	**Unr**	**Unr**	**Unr**	**Unr**	**Unr**
EP	-0.046 (0.14)	-0.195 (0.63)	-0.459 (1.03)	-0.059 (0.22)	0.313 (0.84)
UD	0.028 (2.95)**	0.020 (2.17)*	0.049 (3.55)**	0.032 (4.01)**	0.090 (7.25)**
BRR	0.017 (2.39)*	-0.000 (0.03)	0.013 (1.46)	-0.014 (2.49)*	-0.021 (3.00)**
TW	0.075 (5.03)**	0.035 (2.31)*	0.131 (6.55)**	-0.015 (1.12)	-0.050 (2.39)*
CBI	2.365 (3.41)**	1.077 (1.60)	5.067 (5.67)**	0.710 (1.22)	3.275 (4.14)**
BC	-1.100 (9.44)**	-0.976 (9.01)**	0.019 (0.17)	-1.082 (11.68)**	-0.025 (0.27)
*BC*UD*	-0.027 (3.58)**	-0.018 (2.57)*	-0.010 (1.35)	0.003 (0.42)	-0.008 (1.44)
*BC*TW*	-0.021 (2.12)*	-0.016 (1.64) ♦	-0.038 (3.84)**	0.000 (0.01)	0.000 (0.04)
*BC*EP*	-0.158 (0.80)	-0.411 (2.20)*	0.178 (0.94)	-0.608 (3.82)**	-0.201 (1.31)
*BC*BRR*	0.002 (0.46)	0.001 (0.27)	-0.001 (0.16)	-0.006 (1.43)	-0.002 (0.59)
*BC*CBI*	-1.699 (2.71)**	-1.029 (1.75) ♦	-0.985 (1.81) ♦	-0.717 (1.43)	-1.107 (2.54)*
Real Interest Rate		0.401 (11.51)**	0.304 (11.51)**	0.306 (7.11)**	0.190 (6.46)**
Change in Inflation		-0.079 (1.50)	-0.069 (1.88) ♦	-0.032 (0.61)	-0.076 (2.25)*
Terms of trade shocks		-0.126 (1.05)	0.020 (0.24)	-0.069 (0.62)	-0.012 (0.17)
Lagged Productivity change		-0.069 (2.58)**	-0.112 (1.24)	0.238 (2.72)**	0.097 (4.35)**
Observations	688	621	621	621	621
No. of countries	18	18	18	18	18
Adj.R-squared	0.26	0.41	0.72	0.58	0.80

Table 1. Annual data *(continued)*.

	OLS	OLS controlling for macro-economic shocks	OLS controlling for country effects FE(c)	OLS controlling for time effects FE(t)	OLS with FE(c) and FE(t) + 1973-and-beyond dummy
Test for Spatial Correlation:	$\chi_{(153)} = 1210$ P-value $\cong$ 0.00	$\chi_{(153)} = 579.7$ P-value $\cong$ 0.00	$\chi_{(153)} = 634.7$ P-value $\cong$ 0.00	$\chi_{(153)} = 708.3$ P-value $\cong$ 0.00	$\chi_{(153)} = 865.9$ P-value $\cong$ 0.00
LM Test for Autocorrelation In the residual	$\chi_{(1)} = 565.8$ P- value $\cong$ 0.000	$\chi_{(1)} = 429.2$ P- value $\cong$ 0.000	$\chi_{(1)} = 366.5$ P- value $\cong$ 0.000	$\chi_{(1)} = 436$ P- value $\cong$ 0.000	$\chi_{(1)} = 404$ P- value $\cong$ 0.000
Test for Group Wise Heteroskedasticity	$\chi_{(18)} = 11166$ P-value $\cong$ 0.00	$\chi_{(18)} = 737$ P-value $\cong$ 0.00	$\chi_{(18)} = 361.73$ P-value $\cong$ 0.00	$\chi_{(18)} = 1030.08$ P-value $\cong$ 0.00	$\chi_{(18)} = 404.04$ P-value $\cong$ 0.00
Wald test of joint significance of the country dummies			F(17, 588) = 38.80 P- value $\cong$ 0.000		F(17, 552) = 47.29 P- value $\cong$ 0.000
Wald test of joint significance of the time dummies				F(35, 569) = 6.56 P- value $\cong$ 0.000	F(34, 552) = 6.53 P- value $\cong$ 0.000
Wald Test of joint significance of all the dummies					F(51, 552) = 20.4 P- value $\cong$ 0.000
Wald Test on Joint Significance of the interactions	F(5,676)= 4.88 P-value= 0.0002	F(5,605)= 4.74 P-value = 0.002	F(5,588)= 5.85 P-value $\cong$ 0.00	F(5,569)= 4.38 P-value $\cong$ 0.0003	F(5,552)= 2.19 P-value $\cong$ 0.000
Estimated Rho	.92	.85	.78	0.86	0.84

Note: Absolute value of t statistics in parentheses. ♦significant at 10%* significant at 5%; ** significant at 1%.

The results of these OLS specifications are presented here for exploratory purposes only. It is clear from the tests that straightforward OLS is not appropriate for these data because the errors present all three classic problems of time-series cross-sectional data (TSCS), namely autocorrelation of the residuals, contemporaneous correlation of the errors across countries, and country-specific heteroskedasticity (i.e., the variance of the errors varies country by country). Nevertheless, the table reveals some interesting patterns in the data. The model in column one, with no macroeconomic controls, gives the institutional variables their best shot at explaining variation in unemployment. All institutional variables, except employment protection, are significant and signed according to theoretical predictions (i.e. positive). Three interactions, those between union density and coordination, tax wedge and coordination, and central bank independence and coordination, are also significant and signed according to predictions, i.e., negatively. However, the fit of the model is low, and the model is more than likely to suffer from omitted variable bias. Also, the errors clearly present numerous problems, in particular very high autocorrelation, at least part of which is likely to stem from unobserved heterogeneity. The model in column one is about the only one in which empirical support for the deregulatory view, i.e. that labour market institutions are associated with greater unemployment, is found in this paper.

The insertion of macroeconomic controls in column two alters the coefficients and standard errors of some variables. In particular, the benefit replacement rate becomes insignificant. The insertion of country dummies in column three improves the fit of the model (the adjusted R-squared statistics rises from .41 to .72) and reduces serial correlation in the residuals (probably by capturing some unobserved heterogeneity). However, the error term is still very highly autocorrelated, among other things.

Some of the coefficients change in interesting ways. In particular, the coordination variable, which was negative and highly significant when country dummies were not controlled for, now becomes insignificant as well as positively signed. This implies that the within-country variation of bargaining coordination does not seem to significantly affect the variation in unemployment. If there is an impact of wage coordination, this has to do with the cross-sectional variation across countries, not the longitudinal variation within countries. It should also be emphasized that when country dummies are included in the model, countries in which the coordination index does not vary over time (Austria, Germany, Japan, Switzerland) do not participate in the determination of the coefficient estimate. These are countries that over the entire time period present above average coordination and below average unemployment. Also, countries where the coordination index varies little, like the US and Canada, with low coordination and greater than average unemployment, have a limited impact on the determination of the coordination coefficient. This may contribute to explain the positive sign of the coordination coefficient in column three. The central bank independence variable behaves in the opposite way. It is only significant when country dummies are in the model. This index is time-invariant for several countries (Australia, Austria, Canada Denmark, Germany, Sweden, and USA), which therefore do not participate in the determination of the coefficient when country dummies are in, and varies very little in a number of other countries. The coefficient estimate, both positive and significant when country dummies are controlled for, relies therefore on within-country variation in the countries where the index changes over time. Controlling for time effects (in column four), that is, focusing on the cross-sectional variation, the coefficient of this variable is greatly reduced and its error increased. In other words, contrary to bargaining coordination, it is the variation of central bank independence over time that seems to matter most for unemployment, not its cross-sectional variation. Employment protection is also measured through an index, which is time-invariant for Australia, Canada, Japan, New Zealand, Switzerland, and the US. These are for the most part countries with low protection and higher than average unemployment (except Japan, for which the opposite holds).

Column five, where both country- and time-specific intercepts are inserted, presents a model where all the unobserved country -and time-specific heterogeneity is removed. The fit of model five is highest and both time- and country-dummies appear to belong in the model according to the Wald test performed. Some of the institutional variables in this table (i.e., benefit replacement and tax wedge) are negatively signed, contrary to predictions. However, due to the methodological problems discussed above, it is not worthwhile putting too much weight on these results.

Table 2. Annual data. Estimates in Levels.
Dynamic models (intercept, country, and time dummies omitted).

	OLS	FGLS corrected for hetero-skedasticity	FGLS corrected for het. and autocor. (common panel RHO)	FGLS corrected for het. and autocor. (panel specific RHO)	OLS with Panel Corrected Standard Errors	FGLS-het. with country-specific CPI and time trends
Dependent var.	**Unr**	**Unr**	**Unr**	**Unr**	**Unr**	**Unr**
Lagged dependent Var	. 0.899 (44.92)*	0.903 (48.84)*	0.828 (35.70)*	0.831 (36.40)**	0.899 (29.06)**	0.793 (35.74)**
Real Interest Rate	0.056 (3.76)**	0.056 (4.30)**	0.058 (4.49)**	0.054 (4.25)**	0.056 (3.44)**	0.048 (5.05)**
Change in Inflation	-0.041 (2.45)*	-0.043 (2.95)**	-0.030 (2.65)**	-0.033 (3.10)**	-0.041 (2.16)*	Country specific
Terms of trade shocks	-0.052 (1.50)	-0.060 (2.09)*	-0.060 (2.32)*	-0.063 (2.46)*	-0.052 (1.34)	-0.069 (2.54)**
Lagged Productivity change	-0.082 (4.31)**	-0.071 (4.42)**	-0.050 (3.76)**	-0.052 (4.16)**	-0.082 (3.70)**	-0.068 (4.74)**
EP	-0.090 (0.49)	0.080 (0.56)	0.125 (0.68)	0.032 (0.16)	-0.090 (0.59)	-0.619 (2.53)*
UD	0.014 (2.13)*	0.011 (1.93) ♦	0.021 (2.85)**	0.018 (2.31)*	0.014 (1.67) ♦	0.052 (5.52)**
BRR	0.006 (1.70) ♦	0.002 (0.86)	-0.004 (0.98)	-0.000 (0.01)	0.006 (1.63)	0.011 (2.57)*
TW	-0.007 (0.65)	-0.013 (1.48)	-0.019 (1.73) ♦	-0.015 (1.38)	-0.007 (0.65)	-0.019 (1.82)♦
CBI	0.511 (1.30)	0.621 (1.96)*	0.845 (2.30)*	1.133 (2.94)**	0.511 (1.37)	-0.081 (0.24)
BC	0.035 (0.76)	0.112 (2.65)**	0.092 (1.98)*	0.075 (1.70) ♦	0.035 (0.72)	0.087 (1.64)
*BC*UD*	-0.005 (1.89) ♦	-0.006 (2.47)*	-0.006 (1.98)*	-0.006 (2.33)*	-0.005 (1.69) ♦	-0.009 (2.46)*
*BC*TW*	-0.002 (0.48)	-0.003 (0.78)	-0.001 (0.22)	-0.002 (0.47)	-0.002 (0.41)	-0.001 (0.29)
*BC*EP*	0.089 (1.18)	0.110 (1.65) ♦	0.103 (1.37)	0.111 (1.52)	0.089 (1.15)	0.194 (2.23)*
*BC*BRR*	-0.000 (0.25)	0.001 (0.49)	0.002 (0.79)	0.001 (0.71)	-0.000 (0.25)	0.01 (0.58)

Table 2. Annual data *(continued).*

	OLS	**FGLS corrected for hetero-skedasticity**	**FGLS corrected for het. and autocot. (common panel RHO)**	**FGLS corrected for het. and autocor. (panel specific RHO)**	**OLS with Panel Corrected Standard Errors**	**FGLS-het. with country-specific CPI and time trends**
*BC*CBI*	*0.073* *(0.34)*	*0.022* *(0.13)*	*-0.122* *(0.64)*	*-0.151* *(0.78)*	*0.073* *(0.35)*	*-0.147* *(0.76)*
Obser-vations	*620*	*620*	*620*	*620*	*620*	*620*
ADJ-R Squared	*0.96*				*0.96*	
Wald Test on all the coefficients	*F(69, 550) = 211.17* *P-value ≅ 0.000*	$\chi(69) =$ *20421.76* *P-value ≅ 0.000*	$\chi(69) =$ *10186.68* *P-value ≅* 0.000	$\chi(69) =$ *10823.16* *P-value ≅ 0.000*	$\chi(41) =$ *134773* *P-value ≅ 0.000*	$\chi(68) =$ *18430* *P-value ≅ 0.000*
Estimated Rho	*.30*	*.31*	*.36*		*.30*	*.32*
No. of countries	*18*	*18*	*18*	*18*	*18*	*18*
LM Test for Autocorrelation In the residuals	$\chi(1) =$ *55.80* *P- value ≅ 0.000*	$\chi(1) =$ *57.37* *P- value ≅ 0.000*	$\chi(1) =$ *89.35* *P- value ≅ 0.000*	$\chi(1) =$ *83.89* *P- value ≅ 0.000*	$\chi(1) =$ *55.8* *P- value ≅ 0.000*	$\chi(1) =$ *60.2* *P- value ≅ 0.000*
Test for Autocorrelation (Durbin M)	*Coeff: .39* *P-value ≅ 0.000*	*Coeff: . .39* *P- value ≅ 0.000*	*Coeff: . .44* *P- value ≅ 0.000*	*Coeff: . .44* *P- value ≅ 0.000*	*Coeff: .39* *P- value ≅ 0.000*	*Coeff: .46* *P- value ≅ 0.000*
Wald test on Country dummies	*F(17, 550) = 2.21* *P-value ≅ 0.0036*	$\chi(17) =$ *46.78* *P-value ≅ 0.0001*	$\chi(17) =$ *53.85* *P- value ≅ 0.000*	$\chi(17) =$ *58.06* *P- value ≅ 0.000*	$\chi(17) =$ *29.91* *P-value ≅ 0.027*	$\chi(17) =$ *58.41* *P- value ≅ 0.000*
Wald test on Time dummies	*F(34, 550) = 4.98* *P-value ≅ 0.0000*	$\chi(35) =$ *210.40* *P-value ≅ 0.0000*	$\chi(35) =$ *211.46* *P-value ≅ 0.0000*	$\chi(35) =$ *228.11* *P-value ≅ 0.0000*	$\chi(24) =$ *25217.4* *P-value ≅ 0.0000*	
Wald test on Inter-actions	*F(5, 550) = 1.53* *P-value ≅ 0.1800*	$\chi_{(5)} = 9.31$ *P-value ≅ 0.1*	$\chi_{(5)} = 5.94$ *P-value ≅ 0.31*	$\chi_{(5)} = 7.41$ *P-value ≅ 0.19*	$\chi_{(5)} = 5.79$ *P-value ≅ 0.3269*	$\chi_{(5)} = 11.35$ *P-value ≅ 0.0448*

Note: Absolute value of t statistics in parentheses. ♦significant at 10%* significant at 5%; ** significant at 1%.

Table 2 moves from static to dynamic specification, that is, inserts a lagged dependent variable among the predictors. In so doing, we follow the example of IMF (2003), Nickell et al. (2001), and Nunziata (2001), who estimate dynamic models with yearly data, all not very different from ours. We do not have strong theoretical reasons to insert a lagged dependent variable, except that with yearly data it probably makes sense to assume that exogenous shocks are not absorbed in one year. The variable coefficients have to be interpreted as the effects of the regressors on the partial adjustment process of unemployment, and are short-term coefficients. Based on the results of table 1, all models were estimated with country and time dummies. We also provide tests of the statistical significance of these fixed effects.[16]

From an econometric point of view, the inclusion of a lagged dependent variable in the fixed effects estimator may be problematic (see Baltagi, 2001: 130 for a discussion). Nickell (1981) showed that, with a lagged dependent variable term in the model, the fixed effects (or "within") estimator is biased due to the correlation between the (demeaned) lagged dependent variable and the (demeaned) error term. He also showed that the bias becomes less important as T grows.[17] In theory, the lagged dependent variable should be instrumented. Monte Carlo evidence reported in Beck and Katz (2004: 33-4) shows, however, that the fixed effects estimator (also known as "LSDV") performs relatively well, compared with other estimators, in typical TSCS contexts. In particular, for datasets of similar dimensions to ours (N=20, T=40), and for high values of the lagged dependent variable coefficient (φ=0.9), as in our case, it performs as well as the Kiviet correction estimator (for which there would not be ready-made routines that would be applicable to an unbalanced panel like ours) and is more efficient than the Anderson and Hsiao IV estimator (which is, however, unbiased). These experiments suggest that LSDV slightly underestimates φ (by 2.6 per cent) and slightly overestimates the coefficient of the exogenous variable β (by 1.1 per cent) – both negligible proportions. Here we follow Beck and Katz's (2004), as well as others' advice (Nunziata, 2001: 11; Judson and Owen, 1999) and assume away the Nickell/Kiviet bias, since our panel seems long enough (T=39) to warrant this assumption.

Column one in table 2 reports the results of a dynamic OLS estimation. As clearly indicated by the tests, this model is plagued by serial correlation (which, in the case of a dynamic model, should be detected via Durbin m test).[18] Serial correlation in a dynamic model may lead to biased and inconsistent estimates. Column two to five in the table, which estimate the same model with different methods, seek to correct for this problem – unsuccessfully as it turns out. Column two presents a FGLS estimation that corrects for heteroskedasticity only. This seems to be the approach used in the IMF (2003) paper.[19] Column three and four add to the

---

[16] An alternative estimation strategy would be a random effects model (see Elmeskov et al., 1998). Random effects estimation relies on the number of groups minus the number of coefficients to go to infinity to get a sound estimate of the variance of the group error component. If the number of groups is small, or not much bigger than the number of coefficients, poor estimates of the group specific error components are obtained. If the number of groups is less than the number of coefficients (as is the case with our model), the whole procedure reduces to pooled OLS (see Baltagi, 2001: 18). Many thanks to Mark Shaffer for directing our attention to this phenomenon.

[17] See Nickell (1981: 1420-23). Nickell's results assume that no other exogenous variables are included in the model, but Kiviet (1995) showed that the bias in the complete coefficient vector in small samples has a $O\left(N^{-1}T^{-3/2}\right)$ approximation error. Hence, this bias becomes less important as T and N grow and does not depend on the actual number of exogenous variables.

[18] With a lagged dependent variable, the Baltagi test presented before is no longer adequate. We hence performed the Durbin m test for the dynamic models (see Dezhbakhsh, 1990). The Durbin m test consists in performing a regression of the residuals on the lagged residuals and the other independent variables, and then checking for the magnitude, sign and significance of the coefficient of the lagged residuals.

[19] Our own replication of the IMF (2003) estimates seems to suggest that only the correction for heteroskedasticity was performed. However, since we could not match all of the IMF's models, we cannot be sure about this.

FGLS heteroskedasticity-consistent estimate a Prais-Winsten transformation of the data as well, to seek to eliminate the residual serial correlation that persists even in the dynamic specification. Model three uses a common estimated rho (first-order autocorrelation coefficient); model four a country-specific one. The latter is the approach adopted by Nickell et al. (2001) and by Nunziata (2001); the former is recommended by Beck and Katz (1995: 640) on both statistical and substantive grounds – the former being that experiments show the efficiency of estimation to be greater with a common rho than with country-specific ones, even when the panel is relatively long (T=40); the latter being that once the decision to impose common parameters to different countries has been made, it does not make much sense to allow country-specific variation in the errors' autocorrelation term only.[20] In both cases, however, our tests show that data transformations do not eliminate serial correlation, which remains non negligible in all models ($\rho \cong .3$). Due to the presence of serial correlation, we cannot exclude that the estimates in our models are not both biased and inconsistent. Monte Carlo evidence suggests that the magnitude of the bias may not be great. However, because the experiments do not match the specific features of our dataset (especially N), we cannot be sure of this and we proceed with an estimation strategy seeking to eliminate serial correlation from the dynamic model.[21]

Column five presents OLS estimation of the coefficients with panel corrected standard errors (PCSEs). The PCSEs should correct for panel heteroskedasticity and spatial correlation of the errors, thus providing for more reliable estimates of the standard errors. Beck and Katz (1995 and 1996) argue forcefully for this method, rather than FGLS, on the ground that simulations show FGLS estimates of the standard errors and significance levels to be overly optimistic and to lead to rejection of the null hypothesis that regression coefficients are equal to zero in the population more often than warranted.[22] However, they recommend eliminating serial correlation before applying their preferred method for calculating "robust" standard errors. The specification reported in column five is still marred by autocorrelation, despite inclusion of the lagged dependent variable; hence, as for the models reported in column two, three, and four, we do not know how reliable the coefficient estimates really are. Column six reports the results of a FGLS specification with no time dummies, country-specific time trends and country-specific terms for change in inflation. It is intended to approximate the debatable, in our opinion, modelling choices in IMF (2003). EP appears as a significantly negative predictor of unemployment and BRR as a significantly positive predictor. Also, the interaction between employment protection and bargaining coordination is positive and significant. Other results do not vary much.

Ignoring the possible bias, the results reported in table 2 are quite similar across different estimation methods. The coefficients of the lagged dependent variable are all very high (close to 0.9) – which, from a substantial point of view, seems to indicate a high degree of unemployment inertia and, from a statistical point of view, make us worry about unit roots (see infra). All

[20] It should be noted that, just like other regression coefficients, the estimated rho, too, could be biased in a dynamic model with serial correlation.

[21] Gaduh (2002) performs Monte Carlo experiments with dynamic models where data present serial correlation of the error term. His dataset is slightly different from ours, as N=50. He finds that with T=30, the bias of the LSDV on the coefficient estimates of the exogenous explanatory variable is negligible for levels of residual serial correlation of .25 (see table 3). However, since this simulation is performed not only with an N bigger than ours, but also assuming only one value for the exogenous variable (0.15), it is not clear whether this Monte Carlo evidence is applicable to our panel.

[22] The Beck and Katz's critique is directed at the FGLS correction for spatial correlation of the errors in particular. However, it also takes issue with FGLS with correction for panel heteroscedasticity (see Beck and Katz, 1996). "This is because the weights used in the procedure are simply how well the observations for a unit fit the original OLS regression plane. The second round of FGLS simply downweights the observations for a country if that country does not fit the OLS regression plane well." (Beck, 2001: 277)

macroeconomic variables seem to be significant predictors of unemployment. The real interest rate has a clear positive effect in all specifications. The change in the inflation rate has instead a negative impact – which seems to reflect a short-term trade-off between change in inflation and unemployment. Terms of trade changes seem to have a negative effect on unemployment. This should reflect real wage rigidity. In other words, when terms of trade change, real wage levels do not promptly adjust, thus causing unemployment to rise (if terms of trade decline) or fall (if terms of trade rise). The effect is, however, statistically insignificant when OLS is used. The lagged change in productivity is also negative and significant in all models. It, too, should capture real wage resistance in the face of changes in productivity growth.

Differently from the macroeconomic variables, the effects of institutional variables are much less clear. Employment protection is never significantly different from zero. Union density is positive and significant in all models. Benefit replacement is positive and significant (at the 90 per cent level) when OLS is used, but not when FGLS is used. Tax wedge is surprisingly negative but only significant in two FGLS models (columns three and six). Central bank independence is positively correlated with unemployment, but insignificant when OLS is used. Wage bargaining coordination is, surprisingly, always positive, but significant only with GLS-based estimation. The only robust interaction seems to be that between unionization and wage coordination, which is negative as expected. It is hardly ever possible to reject the hypothesis that the interaction terms are jointly equal to zero. In general, and in line with Beck and Katz's critique (1995 and 1996), FGLS standard errors appear more optimistic than PCSEs (or even OLS). These dynamic models in levels, which are not very different from those presented in IMF (2003), Nickell et al. (2001), and Nunziata (2001), have to be taken with caution, due to the problem of residual autocorrelation mentioned above. However, they hardly seem to support the deregulatory view of unemployment.

Table 3 deals with the problem of serial correlation by differencing the data. In addition, differencing serves two other important purposes: first, since our series are for the most part non-stationary but integrated of order 1, it ensures stationarity of the series (see tests in Appendix 2). We can therefore ignore the question whether the variables in levels are cointegrated or not. Problems with the low power of the tests for panel data co-integration may suggest this as a suitable path to take. Our co-integration test for annual data in levels suggests the existence of a co-integrating relationship. However, as noted by Kittel and Winner (2001: footnote 10, p. 22) on the basis of Maddala and Wu (1999), time series with dimensions similar to ours ($T \cong 30$) are "too short for the estimation of reliable parameters in the co-integrating framework." One possible alternative would be to deal with non-stationarity through some ad hoc tools, like the insertion of country-specific time trends or period and step dummies, and then proceed with estimation in levels if a cointegrating relationship is found. This seems to be the approach taken by Nickell et al. (2001), Nunziata (2001) and IMF (2003). In keeping with Baker et al. (2003: 15), we have chosen not to follow this approach. We do not want to control for trends. If anything, we would like to explain them through our model. By differencing the data we now explore how yearly changes in institutions affect yearly changes in unemployment, after controlling for changes in macroeconomic variables.[23]

[23] While the first difference model is similar to a fixed effect model (within-group estimator) in which first differencing wipes out the country-specific fixed effects, the first difference transformation may come at a price. On one hand, we risk to exacerbate the problem of measurement error, which may be less severe in the levels equation (see Arellano, 2003: 50). On the other hand, since " the information about the Betas in the regression in first differences will depend on the ratio of the variances Δv and Δx (where v is the error term and X is an explanatory variable) ... if Var ΔX is small, regressions in changes may contain very little information about the parameters of interests" (Arellano, 2003: 10).

Table 3. Yearly data. Models in first differences. Static and dynamic (country and time dummies omitted).

	OLS	OLS dynamic	OLS dynamic with country dummies	OLS dynamic with time dummies	OLS dynamic with both country and year dummies
Dependent var.	**unr**	**unr**	**unr**	**unr**	**unr**
Lagged dependent var.		0.387 (9.92)**	0.361 (9.04)**	0.345 (8.39)**	0.337 (8.02)**
Real Interest Rate	0.059 (4.16)**	0.051 (3.89)**	0.050 (3.78)**	0.023 (1.61)	0.023 (1.55)
Change in Inflation	-0.093 (6.01)**	-0.042 (2.79)**	-0.043 (2.80)**	-0.032 (1.95) ♦	-0.033 (1.95) ♦
Terms of trade shocks	0.028 (0.97)	0.014 (0.54)	0.015 (0.55)	-0.033 (1.25)	-0.034 (1.28)
Lagged Productivity change	-0.029 (1.98)*	-0.056 (4.03)**	-0.054 (3.85)**	-0.035 (2.60)**	-0.034 (2.52)*
EP	-0.192 (0.20)	0.104 (0.12)	-0.447 (0.47)	-0.679 (0.80)	-1.029 (1.10)
UD	0.134 (5.02)**	0.111 (4.45)**	0.122 (4.55)**	0.095 (3.94)**	0.104 (3.93)**
BRR	0.011 (0.94)	0.005 (0.47)	0.004 (0.32)	-0.002 (0.21)	0.000 (0.01)
TW	-0.021 (0.91)	-0.038 (1.79) ♦	-0.052 (2.33)*	-0.019 (0.92)	-0.017 (0.80)
CBI	0.390 (0.62)	0.387 (0.67)	0.118 (0.20)	0.935 (1.61)	0.913 (1.53)
BC	-0.008 (0.12)	-0.029 (0.48)	-0.024 (0.39)	-0.018 (0.32)	-0.015 (0.26)
*BC*UD*	-0.013 (2.94)**	-0.012 (2.82)**	-0.012 (2.86)**	-0.008 (2.17)*	-0.008 (2.11)*
*BC*TW*	0.005 (0.65)	0.009 (1.35)	0.009 (1.33)	0.006 (0.91)	0.005 (0.82)
*BC*EP*	-0.011 (0.10)	-0.026 (0.25)	-0.023 (0.21)	-0.002 (0.02)	0.006 (0.06)
*BC*BRR*	-0.000 (0.09)	-0.001 (0.25)	-0.001 (0.22)	0.001 (0.52)	0.001 (0.55)
*BC*CBI*	-0.947 (3.07)**	-0.798 (2.79)**	-0.810 (2.80)**	-0.462 (1.75) ♦	-0.450 (1.68) ♦
Observations	603	602	602	602	602
R-squared	0.15	0.27	0.28	0.41	0.47

Table 3. Yearly data *(continued).*

LM Test for Autocor-relation in the residuals	$\chi(1)$ = *61.89* *P-val* ≅ *0.000*	$\chi(1)$ = *.1* *P-value* ≅ *.74*	$\chi(1)$ = *.06* *P-value* ≅ *.79*	$\chi(1)$ = *.006* *P-value* ≅ *.93*	$\chi(1)$ = *.042* *P-value* ≅ *.84*
Durbin M test	*Coeff:.3 .* *P-value* ≅ *0.000*	*Coeff:.04* *P-val* ≅ *0.49*	*Coeff: .03* *P-v* ≅ *0.687*	*Coeff: .004* *P-value* ≅ *0.98*	*Coeff: -.01* *P-value* ≅ *0.87*
Wald test on country dummies			*F(17,550)= 0.01* *P-value* ≅ *1.0000*		*F(15, 515) = 0.00* *P-value* ≅ *1.0000*
Wald test on time dummies				*F(36, 550) = 5.46* *P-value* ≅ *0.0000*	*F(33, 515) = 0.01* *P-value* ≅ *1.0000*
Wald test on both Country and time dummies					*F(48, 515) = 0.01* *P-value* ≅ *1.0000*
Wald test on Interactions' coefficients	*F(5, 588) = 3.10* *P-value* ≅ *0.0090*	*F(5, 585) = 3.31* *P-value* ≅ *0.0059*	*F(5, 550) = 0* *P-value* ≅ *1*	*F(5, 550) = 1.42* *P-value* ≅ *0.21*	*F(5, 550) = 0* *P-value* ≅ *1*
Note: Absolute value of t statistics in parentheses. ♦significant at 10%* significant at 5%; ** significant at 1%.					

Table 3 builds an OLS model in first differences. It starts with a static model (column one). This appears somewhat underspecified, as revealed by the t-test on the lagged dependent variable in column two, which shows the latter to be highly significant. The dynamic model in column two turns out to be free from autocorrelation. Although first differencing wipes out the country fixed effects, which are the source of the Nickell bias in the dynamic specification, the lagged dependent variable, $\Delta y_{it-1} = (y_{it-1} - y_{it-2})$, becomes correlated with the error term, $(u_{it} - u_{it-1})$, by construction with this transformation. We should therefore instrument it in order to obtain consistent estimates. However, following Beck and Katz (2004), as well as Kittel and Winner (2001: 27), we ignore this source of bias because our sample seems long enough. In other words, we do not instrument the lagged dependent variable in the majority of our models.[24]

In column three we insert country dummies. An F-test rejects the hypothesis that these country dummies are jointly different from zero. This is not surprising as first differencing wipes out the time-invariant country-fixed effects.[25] In column four we insert time dummies, which control for variations in contemporaneous shocks, and find that they are jointly

[24] In table 4, for comparison purposes, we estimate our model with the Anderson and Hsiao (AH) instrumental variable (IV) estimator, instrumenting the lagged dependent variable with y_{it-2} (see Baltagi, 2001: 130; Greene, 2003: 308). Beck and Katz (2004) argue on the basis of simulations that AH should not be used in typical TSCS contexts, i.e. with T long enough, because of its lack of efficiency in small samples.

[25] Note that country fixed effects, in a model in first differences, can be interpreted as capturing country-specific time trends (see Daveri and Tabellini, 1997; 26); the results of the F-test may thus be interpreted as implying that country-specific time trends are not appropriate in this model in first differences.

significant. In column five we estimate a model with both country and time dummies. Based on the results of the various specifications and tests, we decide to proceed with a dynamic model in first differences with period effects. The model is, however, still more than likely to be marred by panel heteroskedasticity and cross sectional correlation. To be able to make correct inferences, we need better estimates of standard errors than straightforward OLS.

In column three we insert country dummies. An F-test rejects the hypothesis that these country dummies are jointly different from zero. This is not surprising as first differencing wipes out the time-invariant country-fixed effects.[26] In column four we insert time dummies, which control for variations in contemporaneous shocks, and find that they are jointly significant. In column five we estimate a model with both country and time dummies. Based on the results of the various specifications and tests, we decide to proceed with a dynamic model in first differences with period effects. The model is, however, still more than likely to be marred by panel heteroskedasticity and cross sectional correlation. To be able to make correct inferences, we need better estimates of standard errors than straightforward OLS.

Table 4 presents three estimates of our basic model in first differences (this time with a constant aimed at capturing a time trend). We use different estimation methods. FGLS estimation with correction for heteroskedasticity is reported in column one. In this model, spatial correlation of the errors is taken into account through the time dummies (although these do not suffice). This procedure is used by Nunziata (2001:12), who provides a theoretical justification for it, which explicitly deals with the Beck and Katz critique (1995).[27] Column two contains an OLS estimation with PCSEs. This should provide for robust standard errors that take into account both panel heteroskedasticity and cross-sectional correlation. This method is suitable for datasets like ours, where T (the time dimension) is greater than N (the cross-sectional dimension), and where serial correlation in the disturbances is eliminated through dynamic specification and/or transformation of the data (in our case, both were necessary). Column three presents the results of an Anderson and Hsiao (1981) instrumental variable (IV) estimation, consisting in eliminating the correlation between the lagged dependent variable and the error term by instrumenting the lagged dependent variable with the second lag of itself (in levels). In estimating this model (and others), we assume that the lagged dependent variable is the only endogenous variable in the model. All other variables are assumed to be exogenous. The Anderson and Hsiao estimator is presented here for the purposes of cross-validation of results. Its estimates are consistent when $N \rightarrow \infty$ or $T \rightarrow \infty$ or both, and in the absence of serial correlation in the errors (the M-test for the model in column three rejects autocorrelation by a tiny margin).[28] We focus on the models where the lagged dependent variable is not instrumented. The OLS/PCSE model in table 4 is our preferred model with annual data. By assuming exogeneity of the independent variables, we accept a small bias in the coefficients (we will partially relax this assumption in table 5). In what follows, we mostly compare results from the FGLS and OLS/PCSE models.

---

[26] Note that country fixed effects, in a model in first differences, can be interpreted as capturing country-specific time trends (see Daveri and Tabellini, 1997; 26); the results of the F-test may thus be interpreted as implying that country-specific time trends are not appropriate in this model in first differences.

[27] The results of an LM test (not presented here) show, however, that the insertion of time dummies (controlling for shocks that are common to each country in a particular year) does not necessarily eliminate all spatial correlation, which remains present.

[28] We do not use other kinds of instrumental variable estimators for dynamic panel data models (e.g., the Arellano and Bond (1991) GMM "differences" estimator or the Blundell and Bond (1998) GMM "system estimator") because these are designed for panels with N>T or, at any rate, with a larger N then ours. Judson and Owen (1999) and Gaduh (2002)'s Monte Carlo simulations both suggest that with panel dimensions similar to ours there are no gains (and, possibly, even losses) from using this sort of GMM estimation.

Table 4. Yearly data. Alternative estimates in differences. Dynamic models (intercept and time dummies omitted).

	FGLS corrected for heteroscedasticity	OLS with Panel Corrected Standard Errors (PCSE).	Anderson and Hsiao IV estimation
Dependent var.	**unr**	**unr**	**unr**
Lagged dependent var.	0.396 (10.13)**	0.345 (5.35)**	0.142 (1.10)
Real Interest Rate	0.048 (3.76)**	0.023 (1.26)	0.019 (1.31)
Change in Inflation	-0.041 (3.04)**	-0.032 (1.69) ♦	-0.025 (2.20)*
Terms of trade shocks	-0.050 (2.33)*	-0.033 (1.02)	-0.030 (1.07)
Lagged Productivity Change	-0.028 (2.70)**	-0.035 (2.31)*	-0.018 (1.16)
EP	-0.592 (0.98)	-0.679 (0.90)	-1.049 (1.16)
UD	0.079 (3.84)**	0.095 (3.53)**	0.104 (4.10)**
BRR	-0.013 (1.66) ♦	-0.002 (0.19)	-0.001 (0.12)
TW	-0.024 (1.36)	-0.019 (0.75)	-0.023 (1.10)
CBI	1.433 (2.98)**	0.935 (1.49)	0.932 (1.56)
BC	0.043 (0.85)	-0.018 (0.29)	-0.016 (0.30)
*BC*UD*	-0.005 (1.33)	-0.008 (1.90) ♦	-0.008 (2.15)*
*BC*TW*	-0.001 (0.16)	0.006 (0.68)	0.004 (0.70)
*BC*EP*	0.115 (1.32)	-0.002 (0.02)	-0.013 (0.13)
*BC*BRR*	0.003 (1.26)	0.001 (0.48)	0.002 (0.68)
*BC*CBI*	-0.226 (1.02)	-0.462 (1.77) ♦	-0.522 (1.91) ♦
Observations	602	602	601
No. of countries	18	18	18

Table 4. Yearly data (continued).

	FGLS corrected for heteroscedasticity	OLS with Panel Corrected Standard Errors (PCSE).	Anderson and Hsiao IV estimation
Observations	602	602	601
Number of CNTRY	18	18	18
Estimated common Rho		0.0836	
LM Autocorrelation test:	$\chi_{(1)} =$ 1.93 P-value $\cong$ .16	$\chi_{(1)} =$ .06 P-value $\cong$ 93	$\chi_{(1)} =$ .23 P-value $\cong$ 86
Durbin M test:	Coeff: -.037455 P-value $\cong$ 0.693	Coeff: . 004 P-value $\cong$ 96	Coeff:-.289534 P-val $\cong$ 058
Adj R square		0.44	
Wald test on Interactions coefiicients	$\chi_{(5)}$ = 6.70 P-value $\cong$ 0.2437	$\chi_{(5)}$ = 5.93 P-value $\cong$ 0.3135	$\chi_{(5)}$ = 6.71 P-value $\cong$ 0.2429
	Time dummies inserted	Time dummies inserted	Time dummies inserted. Lagged dependent var. is instrumented with the second lag of the level.

Note: Absolute value of z statistics in parentheses.♦ significant at 5%; * significant at 5%;** significant at 1%.

The main differences across columns in table 4 have to do with the significance of the macroeconomic variables, while the magnitudes of the coefficient estimates are largely similar. While the FGLS model implies that each one of the macroeconomic variables is significantly different from zero, the OLS/PCSE model is more restrictive and shows that only the lagged productivity change and change in inflation are significant predictors of unemployment (the latter at the 90 per cent significance level).

The values of the institutional variables confirm the picture emerging from previous models in levels. Employment protection is never significant (and its sign is negative). Unionisation is again both positive and significant, with a magnitude below 0.1, which implies that a change of ten units in this variable is associated with a change of less than one unit in the dependent variable. The impact of unionisation seems to be partially counterbalanced by its interaction with the coordination variable (at least in the OLS/PCSE and IV models). Benefit replacement is either insignificant, according to the OLS/PCSE model, or negatively related to the change in unemployment, according to the FGLS model. Tax wedge is negatively signed but insignificant. Central bank independence is larger and significant with FGLS but fails to achieve significance at the 90 per cent level in the OLS/PCSE and IV models. In line with previous research (Hall and Franzese, 1998), the interaction between central bank independence and coordination is found to be negative, and significant in the OLS/PCSE and IV models. Coordination is insignificant in all models. The major peculiarity of the Anderson and Hsiao IV estimation concerns the much smaller magnitude, and statistical insignificance, of the lagged dependent variable coefficient. Similar to results of the models in levels (table 3), these models in first differences, which are arguably better behaved than models in levels because they do not present serial correlation, fail to support the argument that an increase in institutional regulation is associated with an increase in unemployment.

Table 5 provides a series of alternative specifications, by introducing additional regressors and by using different measures of the coordination index. In keeping with table 4, we estimate each model in first differences with both FGLS (corrected for heteroskedasticity) and OLS/PCSE. The results are generally similar to our basic model. While virtually all macroeconomic variables are significant predictors of unemployment in the FGLS models, only change in inflation and change in lagged productivity are significant in the OLS/PCSE models. Similarly, central bank independence is generally significant in FGLS models, while it is not significant in most OLS/PCSE specifications. Union density is positive and significantly different from zero in both types of models. Among the interactions, the most robust are those between coordination and union density, and coordination and central bank independence, both negative.

In columns one and two, the dependent variable is the employment to population ratio. As expected, most of the signs are inverted and results are compatible with the unemployment model. A few things are noteworthy about this model of (changes in) employment to working-age population. The negative effect of the unionisation variable on employment seems greater than the positive effect of the same variable on unemployment. It may be that a portion of those who are priced out of the market (presumably those with a more elastic labour supply curve, i.e. women, older workers, and youth) leave the labour market rather than join the pool of unemployed (see Bertola et al., 2003). Strangely enough, employment protection, which leaves the unemployment rate unaltered, seems to significantly increase the employment rate (i.e the ratio between employees and population between 15 and 64 years of age). This result is in contrast with other literature (see OECD, 1998) and difficult to account for. It could be that the countries in which the level of employment protection is increasing are simultaneously the ones in which the non-active part of the working age population is shrinking (for example, women that do not participate in the labour force exit the working age population for demographic reasons), so that the employment to working-age population ratio is growing even though the ranks of employed and unemployed (the labour force) remain constant or change at the same rate, and vice versa. The other peculiarity of the employment to population model is that no interaction seems significant.

Columns three, four, five, and six return to our basic model of unemployment, but substitute the Kenworthy wage coordination index with the other coordination indexes included in the Nickell et al. (2001) database, the Nickell(1) and Nickell(2) variables. The results of these models are more or less the same as the basic model. The only peculiarity is that the interaction between coordination and union density is negative when using Nickell(1) and positive when using Nickell(2). Similarly, the interaction between coordination and central bank independence is negative when using Nickell(1) and positive when using Nickell(2). Both Nickell et al. (2001) and the IMF (2003) use Nickell(1), which yields results similar to our basic model with the Kenworthy (2002) variable.

Columns seven and eight instrument union density (both alone and in interaction) by taking the first lag of the series. There are reasons to suspect that the unionisation rate depends on unemployment (see Checchi and Lucifora, 2002: 382 and the literature cited therein in footnote 27, p. 387; see also Western, 1997). Taking the first lag is intended to address possible endogeneity. The unionisation variable remains significant, even though its coefficient is slightly smaller than before. Other results do not seem to vary much.

Table 5a. Yearly data. Alternative estimates of the basic model in differences. Dynamic versions (intercept and time dummies omitted).

	Model with Employment to Population as dependent variable. FGLS with heterosk. correction	Model with Employment to Population as dependent variable. OLS with PCSE.	Model with alternative coordination variable. Nickell 1. FGLS (heterosk.)	Model with alternative coordination variable. Nickell 1. OLS with PCSE.	Model with alternative coordination variable. Nickell 2. FGLS (heter.)	Model with alternative coordination variable. Nickell 2. OLS/PCSE	Model with UD instrumented taking the first lag of the series. FGLS with heterosk. correction	Model with UD instrumented taking the first lag of the series. OLS with PCSE
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Dependent var.	**EPop**	**EPop**	**unr**	**unr**	**unr**	**unr**	**unr**	**unr**
Lagged dependent var.	0.299 (7.74)**	0.237 (3.39)**	0.394 (10.10)**	0.342 (5.26)**	0.382 (9.64)**	0.326 (4.97)**	0.382 (9.63)**	0.329 (4.97)**
Real Interest Rate	-0.043 (2.44)*	-0.042 (1.52)	0.044 (3.51)**	0.020 (1.10)	0.044 (3.49)**	0.019 (1.03)	0.044 (3.43)**	0.021 (1.13)
Change in Inflation	0.084 (4.19)**	0.083 (2.72)**	-0.041 (2.98)**	-0.030 (1.56)	-0.039 (2.82)**	-0.031 (1.62) ♦	-0.043 (3.15)**	-0.033 (1.72)
Terms of trade shocks	0.023 (0.66)	0.003 (0.05)	-0.040 (1.91) ♦	-0.022 (0.69)	-0.040 (1.86) ♦	-0.026 (0.80)	-0.038 (1.73) ♦	-0.020 (0.63)
Lagged Productivity	0.001 (0.07)	-0.010 (0.39)	-0.030 (2.83)**	-0.031 (2.03)*	-0.026 (2.48)*	-0.031 (2.01)*	-0.030 (2.79)**	-0.033 (2.14)*
EP	2.287 (2.71)**	2.964 (3.05)**	-0.121 (0.16)	-0.451 (0.49)	-0.796 (1.21)	-1.215 (1.42)	-0.479 (0.79)	-0.732 (0.97)
UD	-0.167 (5.30)**	-0.208 (4.06)**	0.066 (3.08)**	0.088 (2.84)**	0.081 (3.87)**	0.107 (3.84)**	0.059 (2.87)**	0.077 (2.80)**
BRR	0.019 (1.62) ♦	0.014 (1.05)	-0.011 (1.33)	-0.001 (0.12)	-0.011 (1.25)	-0.001 (0.08)	-0.014 (1.72) ♦	-0.002 (0.20)
TW	-0.001 (0.03)	-0.001 (0.03)	-0.027 (1.52)	-0.019 (0.74)	-0.023 (1.25)	-0.009 (0.34)	-0.027 (1.50)	-0.016 (0.63)
CBI	-0.822 (1.34)	-0.828 (0.74)	1.553 (3.23)**	0.958 (1.49)	1.452 (3.05)**	0.905 (1.46)	1.313 (2.73)**	0.687 (1.08)
BC	0.059 (0.99)	0.083 (1.05)					0.002 (0.06)	-0.053 (1.08)
BC (Alternative) Nickell2					-0.622 (0.56)	-0.007 (0.00)		
BC (Alternative) Nickell1			0.371 (0.22)	0.530 (0.23)				

Table 5a. Yearly data *(continued)*.

	Model with Employment to Population as dependent variable. FGLS with heterosk. correction	**Model with Employment to Population as dependent variable. OLS with PCSE.**	**Model with alternative coordination variable. Nickell 1. FGLS (heterosk.)**	**Model with alternative coordination variable. Nickell 1. OLS with PCSE.**	**Model with alternative coordination variable. Nickell 2. FGLS (heter.)**	**Model with alternative coordination variable. Nickell 2. OLS/PCSE**	**Model with UD instrumented taking the first lag of the series. FGLS with heterosk. correction**	**Model with UD instrumented taking the first lag of the series. OLS with PCSE**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Dependent var.	**EPop**	**EPop**	**unr**	**unr**	**unr**	**unr**	**unr**	**unr**
*BC*UD*	0.002 (0.41)	0.002 (0.38)	-0.069 (2.37)*	-0.046 (1.16)	0.026 (1.34)	0.053 (2.06)*	-0.004 (1.05)	-0.001 (0.26)
*BC*TW*	0.003 (0.48)	0.005 (0.63)	0.004 (0.14)	-0.002 (0.06)	-0.002 (0.07)	-0.024 (0.71)	0.000 (0.08)	0.004 (0.56)
*BC*EP*	-0.030 (0.32)	0.007 (0.06)	-0.219 (0.22)	-0.275 (0.21)	0.570 (0.87)	0.429 (0.47)	-0.080 (1.09)	-0.055 (0.63)
*BC*BRR*	-0.001 (0.49)	-0.001 (0.45)	-0.005 (0.32)	0.006 (0.31)	-0.005 (0.50)	0.005 (0.37)	-0.003 (1.51)	-0.002 (0.97)
*BC*BRR*	0.395 (1.31)	0.643 (1.17)	-1.099 (1.40)	-1.802 (1.92) ♦	0.240 (1.13)	0.259 (1.00)	0.151 (0.65)	0.386 (1.37)
Observations	*610*	*610*	*602*	*602*	*602*	*602*	*602*	*602*
ADJ. R Square		*0.31*		*0.44*		*0.44*		*0.44*
Number of countries	*18*	*18*	*18*	*18*	*18*	*18*	*18*	*18*

Note: Absolute value of z statistics in parentheses ♦ significant at 10%,* significant at 5%; ** significant at 1%.

Fearing possible reversed causation between unemployment and the institutional predictors, columns nine and ten use predetermined (i.e., lagged one period) values of all the institutional variables, while columns eleven and twelve do the same with all variables, both institutional and macro (see Fitoussi et al., 2000, for a similar choice). Overall, our main results hold even with these alternative specifications, with a few exceptions (for example, the interaction between wage coordination and employment protection is negative and significant (with FGLS), while the interaction between wage coordination and benefit replacement (with FGLS) is positive and significant at the 10 per cent level).

Table 5b. Yearly data. Alternative estimates of the basic model in differences. Dynamic versions (intercept and time dummies omitted). ***(Continuation of table 5a.)***

	Model with all the institution-nal variables instru-mented taking the first lag of the series. FGLS	*Model with all the institution-al variables instru-mented taking the first lag of the series. OLS with*	*Model with all the variables instru-mented taking one lag. FGLS with heterosk. correction*	*Model with all the variables instru-mented taking one lag. OLS with PCSE*	*Model without real interest rate. FGLS with heterosk. correction*	*Model without real interest rate. OLS with PCSE*	*Model with alternative macro Control variables. FGLS with heterosk. correction*	*Model with alternative macro Control variables. OLS with PCSE)*
	(9)	*(10)*	*(11)*	*(12)*	*(13)*	*(14)*	*(15)*	*(16)*
Depen-dent var.	*unr*	*unr*	*unr*	*unr*	*unr*	*unr*	*unr*	*unr*
Lagged dep.var.	0.435 (9.97)**	0.344 (4.84)**	0.436 (10.1)**	0.350 (5.08)**	0.399 (10.3)**	0.345 (5.35)**	0.449 (11.0)**	0.383 (5.58)**
Real Interest Rate	0.046 (3.59)**	0.023 (1.26)	0.053 (4.05)**	0.061 (3.35)**			0.007 (0.58)	-0.008 (0.43)
Change in Inflation	-0.048 (3.45)**	-0.036 (1.87) ♦	0.011 (0.74)	0.005 (0.23)	-0.026 (2.06)**	-0.026 (1.44)	-0.028 (3.49)**	-0.02.2 (1.91) ♦
Terms of trade shocks	-0.037 (1.74) ♦	-0.015 (0.49)	-0.019 (0.85)	-0.048 (1.51)	-0.044 (2.10)*	-0.033 (1.02)		
Lagged Produc-tivity change	-0.031 (2.93)**	-0.033 (2.19)*	-0.029 (2.72)**	-0.032 (2.04)*	-0.03 (2.97)**	-0.035 (2.43)*		
EP	0.039 (1.62) ♦	-0.006 (0.20)	0.035 (1.43)	-0.006 (0.19)	-0.648 (1.07)	-0.689 (0.90)	-0.743 (1.36)	-0.883 (1.37)
UD	0.061 (2.88)**	0.077 (2.82)**	0.065 (3.07)**	0.078 (2.83)**	0.083 (4.12)**	0.095 (3.51)**	0.059 (2.72)**	0.078 (2.98)**
BRR	-0.013 (1.56)	-0.008 (0.71)	-0.015 (1.82) ♦	-0.010 (0.87)	-0.012 (1.48)	-0.002 (0.19)	-0.018 (2.37)*	-0.004 (0.32)
TW	0.018 (1.01)	0.024 (0.93)	0.024 (1.34)	0.031 (1.22)	-0.024 (1.39)	-0.02 (0.79)	-0.004 (0.20)	-0.024 (0.82)
CBI	-0.520 (1.10)	-0.437 (0.72)	-0.521 (1.10)	-0.426 (0.67)	1.509 (3.19)**	0.939 (1.49)	1.803 (3.05)**	2.320 (3.08)**
BC	0.021 (0.49)	0.039 (0.81)	0.001 (0.02)	0.017 (0.35)	0.049 (0.97)	-0.013 (0.2)	0.069 (1.38)	0.007 (0.11)
*BC*UD*	0.001 (0.28	0.002 (0.52)	-0.000 (0.00)	0.002 (0.36)	-0.004 (1.1)	-0.008 (1.85) ♦	-0.005 (1.45)	-0.008 (1.93)
*BC*TW*	-0.000 (0.07)	-0.002 (0.41)	0.000 (0.10)	-0.001 (0.18)	-0.001 (0.11)	0.005 (0.61)	-0.009 (1.69)	-0.002 (0.17)
*BC*EP*	-0.015 (2.72)**	-0.012 (1.56)	-0.012 (2.23)*	-0.011 (1.47)	0.103 (1.19)	0.002 (0.02)	0.195 (2.28)*	0.087 (0.76)
*BC*BRR*	0.004 (1.75) ♦	0.002 (0.89)	0.005 (2.25)*	0.003 (1.37)	0.001 (0.67)	0.001 (0.33)	0.002 (0.91)	0.001 (0.28)
*BC*CBI*	-0.017 (0.08)	-0.096 (0.37)	-0.041 (0.19)	-0.105 (0.41)	-0.193 (0.88)	-0.460 (1.76) ♦	-0.126 (0.52)	-0.199 (0.70)
Labour Demand Shocks							-9.837 (5.92)**	-12.042 (5.17)**

Table 5b. Yearly data *(continued.)*

	Model with all the institution-nal variables instru-mented taking the first lag of the series. FGLS (heterosk.)	Model with all the institution-al variables instru-mented taking the first lag of the series. OLS with PCSE	Model with all the variables instru-mented taking one lag. FGLS with heterosk. correction	Model with all the variables instru-mented taking one lag. OLS with PCSE	Model without real interest rate. FGLS with heterosk. correction	Model without real interest rate. OLS with PCSE	Model with alternative macro Control variables. FGLS with heterosk. correction	Model with alternative macro Control variables. OLS with PCSE)
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Depen-dent var.	unr	unr	unr	unr	unr	unr	unr	unr
Money Supply Shocks							0.252 (1.85) ♦	0.309 (1.61)
Real Import Prices							1.160 (0.99)	1.922 (0.97)
Total Factor Produc-tivity Shocks							-7.935 (5.23)**	-10.492 (4.42)**
Terms of trade shocks	-0.037 (1.74) ♦	-0.015 (0.49)	-0.019 (0.85)	-0.048 (1.51)	-0.044 (2.10)*	-0.033 (1.02)		
Obser-vations	*620*	*620*	*620*	*620*	*602*	*602*	*525*	*525*
AdJ. R Square		*0.45*		*0.43*		*0.44*		*0.47*
No. of Countries	*18*	*18*	*18*	*18*	*18*	*18*	*18*	*18*

Note: Absolute value of z statistics in parentheses. ♦ significant at 10%* significant at 5%; ** significant at 1%.

In columns thirteen and fourteen, we take out the real interest rate variable from the control variables. This is to check whether the positive impact of central bank independence on unemployment is channelled through higher real interest rates. This does not seem to be the case. The CBI's coefficient estimate is almost unchanged. Other variables do not change either. In columns fifteen and sixteen we substitute the macroeconomic control variables in our specification, which correspond to the control variables included in the IMF (2003) specification, with the macro control variables in the Nickell et al. paper and database (2001) (labour demand shock, total factor productivity shock, money supply shock, real interest rate, and terms of trade shock). These alternative controls are all mean-reverting, with the exception of the real interest rate, and can therefore only explain short-run deviations of unemployment from its equilibrium path. In other words, this model attributes a greater role to the institutional variables in explaining equilibrium unemployment (see also Nunziata, 2002). Since the data in the Nickell et al. (2001) database run until 1995, this model is estimated on a shorter panel. This alternative specification confirms that both union density and central bank independence are positively and significantly related to unemployment in both the FGLS and OLS/PCSE specifications. Both the coefficient and the z statistics for central bank independence are much greater than before. In addition, benefit replacement emerges as negatively correlated with unemployment in the FGLS specification, but not in the OLS/PCSE one. Among the interactions, that between unionisation and coordination is negative and significant as before (in the OLS/PCSE model). In the FGLS model two new interactions emerge, that between coordination and tax wedge (negative at the 10 per cent level) and that between coordination and employment protection (positive).

In table 6, columns one to four, an additional predictor of the unemployment rate, i.e., a measure of benefit duration, is inserted among the independent variables. We use two measures of benefit duration, one from the Nickell et al. (2001) database; another from a database kindly provided by Baker et al. (2003). The first series is incomplete, especially for Austria, Finland, Norway, and Sweden (particularly for the 1960-75 years) – which seems to be the reason why it was not included in the IMF (2003) models. The series of benefit duration provided by Baker et al. (2003), based on OECD data (p. 26), appears slightly different from that in the Nickell et al.'s database. In particular, it has less missing values, because it attributes 0 values when the OECD series showed no benefits after year one.

For Japan, Italy, and Switzerland, both series include a large number of zero values. In columns five to eight, we construct a summary measure of benefit generosity by interacting benefit replacement rate and benefit duration. This is important to capture the combined effect of these two dimensions, namely replacement rate and duration of unemployment benefits. The results of these models are interesting. When benefit replacement and benefit duration variable are entered separately in columns one to four, they are almost always negative, and, in column three, even significant at the 10 per cent level. The other coefficients remain relatively stable. However, the interactions between employment protection and coordination, and between benefit duration and coordination, become positive and often significant, too. If both employment protection and benefit duration are to be considered as forms of insurance (Agell, 1999; 2000; Bertola, 2004), then it seems that in more coordinated systems (that is, in systems in which the unions' bargaining power is presumably greater) the costs of such insurance are, at least partially, borne by the employers – which may explain the positive impact on unemployment. In models five to eight, the benefit generosity variable is positive but insignificant, both alone and in interaction with wage coordination.

By looking at all models in tables 5 and 6 simultaneously, it is clear that the only robust institutional determinant of yearly unemployment is the union density rate, whose positive effect seems to be tempered by wage coordination. Less frequently, the degree of central bank independence also emerges as a positive and significant institutional determinant of unemployment. The effect of central bank independence seems to depend on the particular estimation method used (for example, the coefficient of this variable is often significant with FGLS, but not with OLS/PCSE) and on the particular specification adopted. Perhaps, the unemployment-augmenting effects of an independent monetary policy authority take more than one year to manifest. For this reason, among others, in the next section we shift from yearly data to five-year averages.

Before we do that, we present in table 7 the results of a random coefficient model (RCM) in levels. This is a compromise between assuming perfect homogeneity and perfect heterogeneity of the parameters. Generally speaking, the RCM is a special type of "shrinkage estimator". It shrinks the OLS estimates of the β_i to pooled sample mean estimate β, with the degree of shrinkage a function of the poolability of the data. The RCM extends the logic underlying the random effects model to all parameters of interest, not just the intercept, treating parameter heterogeneity as stochastic variation around a mean parameter.

Table 6. Yearly data. Alternative estimations of the dynamic model in differences with benefit duration and benefit generosity (intercept and time dummies omitted).

	Nickel series. FGLS	Nickell Series. PCSE	Baker series. FGLS	Baker series. PCSE	Nickell Series. Benefit Generosity FGLS	Nickell Series. Benefit Generosity PCSE	Baker series. Benefit Generosity FGLS	Baker series. Benefit Generosity PCSE
Dependent var.	**unr**	**unr**	**unr**	**unr**	**unr**	**unr**	**unr**	**unr**
Lagged dependent var.	*0.382* *(9.17)***	*0.322* . *(4.56)***	*0.403* *(10.38)***	0.352 (5.53)**	0.376 (8.99)**	0.312 (4.39)**	0.399 (10.17)**	0.347 (5.40)**
Real Interest Rate	0.044 (3.27)**	0.021 (1.05)	0.048 (3.79)**	0.023 (1.24)	0.041 (3.07)**	0.021 (1.08)	0.046 (3.67)**	0.023 (1.26)
Change in Inflation	-0.048 (3.30)**	-0.046 (2.14)*	-0.045 (3.26)**	-0.038 (2.02)*	-0.046 (3.16)**	-0.041 (1.91) ♦	-0.042 (3.04)**	-0.033 (1.74) ♦
Terms of trade shocks	-0.056 (2.42)*	-0.035 (1.04)	-0.056 (2.56)*	-0.042 (1.29)	-0.052 (2.25)*	-0.029 (0.84)	-0.051 (2.32)*	-0.037 (1.13)
Lagged Productivity change	-0.028 (2.55)*	-0.030 (1.99)*	-0.029 (2.74)**	-0.035 (2.35)*	-0.028 (2.54)*	-0.029 (1.89)♦	-0.029 (2.76)**	-0.035 (2.35)*
EP	-0.899 (1.35)	-1.268 (1.54)	-0.569 (0.93)	-0.682 (0.90)	-1.005 (1.56)	-1.207 (1.53)	-0.653 (1.08)	-0.673 (0.91)
UD	0.087 (3.98)**	0.107 (3.63)**	0.078 (3.80)**	0.095 (3.53)**	0.083 (3.88)**	0.108 (3.69)**	0.078 (3.77)**	0.094 (3.50)**
BRR	-0.011 (1.29)	-0.002 (0.16)	-0.014 (1.69) ♦	-0.006 (0.53)				
TW	-0.019 (0.92)	-0.016 (0.52)	-0.021 (1.20)	-0.013 (0.51)	-0.025 (1.23)	-0.023 (0.78)	-0.027 (1.50)	-0.019 (0.74)
BD	0.143 (0.19)	-0.053 (0.05)	-0.983 (1.92) ♦	-1.112 (1.41)				
Benefit generosity					0.013 (0.94)	0.021 (1.13)	-0.007 (0.55)	0.003 (0.20)
CBI	1.856 (3.18)**	2.088 (2.87)**	1.431 (3.02)**	0.964 (1.51)	1.884 (3.19)**	2.043 (2.83)**	1.455 (3.04)**	0.948 (1.50)
BC	0.058 (1.07)	0.013 (0.20)	0.052 (1.05)	0.005 (0.08)	0.043 (0.79)	-0.012 (0.18)	0.031 (0.62)	-0.016 (0.26)
*BC*UD*	-0.007 (1.77) ♦	-0.012 (2.33)*	-0.007 (1.89) ♦	-0.012 (2.63)**	-0.004 (1.07)	-0.009 (1.77) ♦	-0.005 (1.25)	-0.010 (2.08)*
*BC*TW*	0.000 (0.01)	0.008 (0.87)	0.000 (0.01)	0.007 (0.86)	-0.001 (0.17)	0.004 (0.55)	0.001 (0.16)	0.006 (0.77)
*BC*EP*	0.144 (1.56)	0.075 (0.64)	0.157 (1.85) ♦	0.076 (0.71)	0.128 (1.35)	0.021 (0.18)	0.101 (1.20)	0.006 (0.06)
*BC*BD*	0.282 (1.69) ♦	0.498 (2.36)*	0.306 (2.15)*	0.434 (2.45)*				

Table 6. Yearly data *(continued).*

	Nickel series. FGLS	Nickell Series. PCSE	Baker series. FGLS	Baker series. PCSE	Nickell Series. Benefit Generosity FGLS	Nickell Series. Benefit Generosity PCSE	Baker series. Benefit Generosity FGLS	Baker series. Benefit Generosity PCSE
Depen-dent var.	unr	unr	unr	unr	unr	unr	unr	unr
*BC*BRR*	*0.002 (0.83)*	*0.000 (0.04)*	*0.002 (1.01)*	*0.001 (0.33)*				
*BC*CBI*	*-0.185 (0.74)*	*-0.135 (0.44)*	*-0.177 (0.80)*	*-0.382 (1.47)*	*-0.194 (0.80)*	*-0.315 (1.09)*	*-0.185 (0.86)*	*-0.456 (1.79) ♦*
BC benefit generosity					*0.000 (0.03)*	*0.003 (0.67)*	*0.002 (0.90)*	*0.004 (1.33)*
Observatio ns	*524*	*524*	*602*	*602*	*524*	*524*	*602*	*602*
ADJ-R-Squ ared		*0.39*		*0.4*		*.39*		*.39*
No. of Countries	*18*	*18*	*18*	*18*	*18*	*18*	*18*	*18*

Note: Absolute value of z statistics in parentheses. ♦significant at 10%,* significant at 5%; ** significant at 1%.

Representing the model as:

$$Y_{i,t} = X_{i,t}\beta_i + \varepsilon_{i,t} \text{ where } \quad \beta_i = \beta + \upsilon_i , E(\upsilon_i) = 0 , \; E(\upsilon_i\upsilon_i') = \mathrm{T}$$

the goal of the RCM is to find both $\hat{\beta}$, the estimated (weighted) mean of the cross sectional specific coefficient vector β_i and $\hat{\mathrm{T}}$, the estimated covariance matrix.

We estimated a RCM for two of our models with annual data, implementing the two-step approach suggested by Swamy (1971).[29] To reduce the number of parameters to be estimated with T observations, our specifications do not include the interaction terms. This is because the coefficient parameters are allowed to vary (randomly) across countries in this specification. The test of parameter constancy rejects the hypothesis of constancy of the parameters across the different cross-sections. Both models show more or less the same degree of serial correlation as before, which is somewhat surprising because we would expect that allowing for coefficient variation across countries would lead to a better specified model and would, through that channel, alleviate the problem of serial correlation. The models in table 7 are not very different from those in table 2. The major differences concern the lagged dependent variable, which has a smaller coefficient in the RCM, and the union density coefficient, which is bigger, but insignificant in column one (the basic model). Also, the CBI coefficient is much bigger in column

[29] Beck and Katz (2001) argue against using RCMs "in normal circumstances," because, based on Monte Carlo evidence, they find that pooled OLS performs better in terms of efficiency, unless there is a large amount of parameter heterogeneity. RCMs are especially inefficient with small T (which is not our case, however). In particular, they find that the Swamy's method tends to overestimate the covariance matrix in small samples, even though it is consistent. Here we present results based on this estimator for the purpose of comparison with other models.

Table 7. Yearly data. Random coefficient estimates (Swamy model), in levels.

	Basic model without interactions	Basic model without interactions with benefit duration (Nickell series #1)
	(1)	**(2)**
Dependent variable	**unr**	**unr**
Lagged dependent variable	0.739 (17.12)**	0.702 (11.32)**
Real Interest rate	0.082 (3.70)**	0.072 (2.60)**
Change in the inflation rate	-0.048 (2.35)*	-0.055 (1.75) ♦
Terms of trade shocks	-0.112 (1.18)	-0.101 (1.06)
Lagged productivity change	-0.101 (4.84)**	-0.106 (4.30)**
EP	-0.064 (0.05)	-1.126 (0.53)
UD	0.052 (1.35)	0.088 (1.90) ♦
BRR	-0.023 (0.41)	-0.030 (0.50)
BD		-0.804 (0.32)
TW	-0.010 (0.33)	-0.013 (0.25)
CBI	-0.031 (0.01)	7.042 (0.39)
BC	0.007 (0.10)	-0.014 (0.15)
Constant	1.270 (0.32)	0.840 (0.12)
Observations	620	542
Number of countries	18	18
Test of parameter constancy	$\chi_{(204)} = 619.74$ P. val ≅ 0.0000	$\chi_{(221)} = 783.66$ P. Val ≅ 0.0000
Durbin M test for autocorrelation of the residuals	.458971(0.000)	.42434 (0.000)

Note: Absolute value of z statistics in parentheses. ♦significant at 10%* significant at 5%; ** significant at 1%.

two (which includes benefit duration) than in the models in table 2. It is, however, insignificant. The macroeconomic predictors are instead all significant and correctly signed with the exception of the terms of trade shocks variable. No labour market institution is a statistically significant predictor of unemployment in these RCM models, with the exception of union density in column two. It should be recalled, however, that the Swamy model tends to overestimate the true variability of the parameters in small T samples (see Beck and Katz, 2001).

3.2. Models with Five-Year Data

In this section, in keeping with other literature on the institutional determinants of unemployment (see, for example, Daveri and Tabellini, 1997; Nickell, 1997; Baker et al., 2002), we use data averaged over 5 years intervals. The advantages of five-year averages are multiple: averages should mop out the effects of business cycles on unemployment, thus leading to more reliable causal interpretations. Also, five-years aggregates should be more appropriate than annual data for indicators like the employment protection index, which is based on interpolation from a few observations (see Baker et al., 2003: 6 and ft. 4). In general terms, since the institutional variables vary little over time, an analysis with averaged data should be preferable. Moreover, averaging the data is likely to reduce the degree of first-order serial correlation in the error term. The obvious drawback of this approach is a lower number of observations over time for each country, which also implies lower statistical power. Also, if some of the effects are purely short-term, we may not be able to pick them up by averaging the data.

Similar to the previous section, table 8 begins with a simple pooled OLS model in levels (column one), to which country dummies (column two), time dummies (column three), and then both country and time dummies are added (column four). The Wald test shows that both should be included in the model. The models in columns one and three show a high degree of serial correlation. This seems closely linked with unobserved heterogeneity. Proof is that when country dummies are included, the degree of serial correlation (and the estimated rho) drops considerably. From the tests conducted on the model in column four, it emerges, however, that serial correlation of the errors is still a problem. In a static model, serial correlation leads to inefficient OLS estimates, but unbiased, if the model is correctly specified. From a substantial point of view, it is worth noting again the change in sign (and significance) of the wage coordination index (similar to models with yearly data). This is negative and highly significant when country dummies are not inserted, and becomes positive and insignificant when these are inserted. The central bank independence variable behaves in the opposite way. This coefficient is much greater, and significant, when country fixed effects are controlled for. We postpone discussion of the other institutional predictors until later. However, it is already worth emphasizing one peculiarity of five-year data models, which emerges clearly by examining columns one to four: the whole set of interactions does not appear to come even close to significance, with the exception, perhaps, of the interaction between coordination and employment protection.

In columns five and six in the table, we present the same fixed effects models estimated with two alternative methods: one is OLS with the Newey-West robust standard errors, which should take into account both first order autocorrelation and panel heteroskedasticity; the other is FGLS with corrections for heteroskedasticity and serial correlation (with a Prais Winsten transformation). In both cases, we assume that the spatial correlation of the errors has been removed via the insertion of time dummies. The Beck and Katz's panel corrected standard errors (PCSEs), used in models with annual data, are not appropriate in this case because this estimator is recommended for panels where T>N.[30] The FGLS estimates correct for serial correlation by estimating a common rho (rather than country-specific ones). Panel specific rhos would be especially problematic in this case given the low number of data points. Among the macroeconomic variables, only the real interest rate is significant and signed according to prediction (i.e., positive). The other macroeconomic predictors are surprisingly positive rather than negative. All are, however, insignificant. This insignificance is not unexpected with five-year averages. In fact, variables like changes in consumer price indexes, in terms of trade,

[30] In particular, Beck (2001: 174) recommends against using them when T<10 since they depend on asymptotic assumptions about T.

Table 8. Five-year data. Full models in levels. Static (intercept, country and time dummies omitted)

	OLS.	**OLS with country dummies**	**OLS with time dummies**	**OLS with country and time dummies**	**OLS robust with country and time dummies**	**FGLS corrected for hetero-skedas-ticity and autocor-relation (common Rho)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Dependent var.	**unr**	**unr**	**unr**	**unr**	**unr**	**unr**
Real interest rate	0.558 (5.31)**	0.348 (4.28)**	0.447 (3.21)**	0.315 (3.53)**	0.315 (3.40)**	0.282 (3.71)**
Change in Inflation	-0.141 (0.52)	-0.081 (0.47)	0.292 (0.87)	0.065 (0.34)	0.065 (0.37)	0.028 (0.20)
Terms of trade shocks	-0.455 (0.69)	-0.189 (0.46)	-0.136 (0.22)	0.092 (0.26)	0.092 (0.27)	0.038 (0.16)
Lagged Productivity change	0.106 (0.57)	-0.149 (1.00)	0.409 (2.09)*	0.202 (1.46)	0.202 (1.17)	0.136 (1.19)
EP	-0.115 (0.15)	1.227 (1.14)	0.107 (0.15)	1.259 (1.31)	1.259 (1.04)	0.652 (0.72)
UD	0.024 (1.17)	0.063 (1.93) ♦	0.036 (1.83) ♦	0.090 (3.05)**	0.090 (1.76) ♦	0.067 (2.20)*
BRR	-0.002 (0.11)	0.021 (1.16)	-0.012 (0.93)	-0.011 (0.73)	-0.011 (0.62)	-0.013 (0.88)
TW	0.012 (0.34)	0.135 (2.69)**	-0.027 (0.80)	-0.092 (1.75) ♦	-0.092 (1.45)	-0.103 (2.23)*
CBI	0.723 (0.46)	5.680 (2.70)**	0.600 (0.41)	3.798 (1.99)*	3.798 (1.73) ♦	4.689 (2.92)**
BC	-1.239 (4.82)**	-0.070 (0.24)	-1.244 (5.34)**	0.195 (0.82)	0.195 (0.78)	0.085 (0.41)
*BC*UD*	-0.011 (0.60)	0.004 (0.23)	0.006 (0.37)	0.001 (0.05)	0.001 (0.03)	-0.008 (0.63)
*BC*TW*	-0.007 (0.31)	-0.023 (1.00)	0.002 (0.08)	0.005 (0.26)	0.005 (0.19)	-0.008 (0.43)
*BC*EP*	-0.720 (1.49)	0.480 (0.93)	-0.776 (1.78) ♦	0.451 (1.06)	0.451 (0.82)	0.658 (1.74) ♦
*BC*BRR*	-0.009 (0.76)	-0.016 (1.22)	-0.015 (1.34)	-0.016 (1.50)	-0.016 (1.37)	-0.014 (1.49)
*BC*CBI*	-0.851 (0.56)	-0.204 (0.15)	-0.534 (0.39)	-0.843 (0.75)	-0.843 (0.80)	-0.667 (0.71)
Observations	*121*	*121*	*121*	*121*	*121*	*121*
No. of countries	*18*	*18*	*18*	*18*	*18*	*18*

and in productivity are likely to only affect short-term adjustment processes of the unemployment rate to its long-term equilibrium and it is not unusual that they are not significantly different from zero when longer time frames are considered. According to mainstream macroeconomic theory, for example, if there is a trade off between unemployment and inflation, this is, at best, limited to the short run and should disappear in the medium to long term.

Table 8. Five years data *(continued)*.

	OLS.	OLS with country dummies	OLS with time dummies	OLS with country and time dummies	OLS robust with country and time dummies	FGLS corrected for hetero-
Wald test on country Dumm		$\chi_{(17)}$ = 219.1 P- value ≅ 0.000		F(17, 81) = 9.50 P- value ≅ 0.000	F(17, 81) = 9.50 P- value ≅ 0.000	$\chi_{(17)}$ = 151.29 P- value ≅ 0.000
Wald test on time Dumm			F(7, 98) = 4.95 P-value ≅ 0.0001	F(7, 81) = 6.32 P- value ≅ 0.000	F(7, 81) = 6.32 P- value ≅ 0.000	$\chi_{(7)}$ = 85.44 P- value ≅ 0.000
Wald test on Interactions coefficients	F(5, 105) = 1.07 P-value ≅ 0.3814	F(5, 88) = 0.61 P-Value ≅ 0.6956	$\chi_{(5)}$ = 8.34 P-value ≅ 0.1382	F(5, 81) = 0.72 P-value ≅ 0.6114	F(5, 81) = 0.72 P-value ≅ 0.6114	$\chi_{(1)}$ = 7.25 P-value ≅ 0.2029
Estimated Rho	.64	.43	.7	.3	.3	.26
LM autocor-relation test	$\chi_{(1)}$ = 32.4 P- value ≅ 0.000	$\chi_{(1)}$ = 13.26 P- value ≅ 0.002	$\chi_{(1)}$ = 38.78 P- value ≅ 0.000	$\chi_{(1)}$ = 6.79 P-value ≅ .009		$\chi_{(1)}$ = 10.95 P-value ≅ .0009
Wald hetero-skedasticity test	$\chi_{(18)}$ = 452.42 P- value ≅ 0.000	$\chi_{(18)}$ = 460.97 P- value ≅ 0.000	$\chi_{(18)}$ = 163.96 P- value ≅ 0.000	$\chi_{(18)}$ = 97.22 P- value ≅ 0.000		$\chi_{(18)}$ = 26.2 P- value ≅ 0.09
Adj R Square	.4	.7	.52	0.79	0.79	

Note: Absolute value of z statistics in parentheses. ♦significant at 10%,* significant at 5%; ** significant at 1%.

Among the institutional variables, employment protection is positive and insignificant. As argued above, this variable is measured through a time-invariant index for Australia, Canada, Japan, New Zealand, Switzerland, and the US. These are for the most part countries with low protection and higher than average unemployment (except Japan, for which the opposite holds). The fact that they do not participate in the determination of the employment protection coefficient in models with country fixed effects may explain the positive sign. Union density is, as in models with yearly data, both positive and significant. Benefit replacement rate is, contrary to predictions, negative but insignificant. Tax wedge is also negative, again contrary to predictions, and significant with FGLS. This somewhat surprising result could be an indication that taxation is entirely paid for by wages and, for this reason, does not increase unemployment. Because it reduces take-home pay, taxation may even increase labour supply for given wage levels, which may explain the negative sign. Central bank independence is positive and significant. Wage coordination is also positive but insignificant in the models reported in columns five and six, which include country dummies. The interactions terms are all not significantly different from zero, unlike in models with yearly data, except the interaction between coordination and employment protection, which is positive and significant at the 10 per cent level with FGLS (but whose sign appears to jump depending on specification). When comparing the interaction coefficients in models with five-year and one-year data, one is led to conclude that the degree of wage coordination moderates the effects of institutions only in the short-term, that is, with yearly data, and not when data are averaged over longer time frames. As is the case with models with yearly data, FGLS appears to produce more optimistic estimates of standard errors and significance levels than robust OLS.

Table 9 estimates a similar model in levels, again by using two different techniques (OLS robust and FGLS), but excluding, for the sake of greater efficiency, first the interaction variables – the impact on the adjusted R-Square is minimal in columns two and three – and then the macroeconomic variables that do not appear significant according to a Wald test performed on them, that is, all except the real interest rate (columns four and five). In the latter case, the adjusted R-Square even increases. Column one also reports the results of a dynamic model. With T=8, this is more than likely to suffer from Nickell/Kiviet bias. We report these results as a reminder that the static model with five-year data is possibly underspecified (unlike the model in first differences, as we will see below), as shown by the coefficient of the lagged dependent variable in column one, which is highly significant.

As was the case with models with annual data, few variables appear robust throughout (not considering the dynamic model in column one): the real interest rate, union density, and the central bank independence index. The magnitude of all coefficients is quite similar across models, even though the effects of some institutional variables like employment protection, union density, and central bank independence seem to become greater in the more parsimonious models with only one macroeconomic control, i.e., the real interest rate. It is interesting to note that even with reduced models – that is with only one macroeconomic control, the institutional variables, and no interactions – no systematic support is found in the data for the deregulatory view. Benefit replacement and tax wedge are negative. Employment protection is insignificant. Only union density is in line with theoretical predictions. Its magnitude is around 0.1. The coefficients of the real interest rate and central bank independence variables, both positive and highly significant, seem to point in the direction of restrictive macroeconomic policies as determinants of unemployment.

Table 9. Five-year data. Models in levels. Alternative estimation methods (intercept, country, and time dummies omitted).

	Dynamic model. OLS robust	OLS robust	FGLS corrected for heteroskedasticity and autocorrelation	OLS robust	FGLScorrected for heteroskedasticity and autocorrelation
Dependent var.	**unr**	**unr**	**unr**	**unr**	**unr**
Lagged unemployment rate	0.94 (35.3)**				
Real interest rate	0.02 (0.73)	0.262 (3.19)**	0.216 (2.98)**	0.251 (3.41)**	0.240 (4.00)**
Change in inflation	-0.13 (1.71) ♦	-0.013 (0.08)	-0.071 (0.52)		
Terms of trade shocks	-0.04 (0.32)	0.063 (0.18)	-0.158 (0.61)		
Lagged Productivity change	0 .022 (0.32)	0.194 (1.14)	0.111 (0.97)		
EP	0 .13 (0.41)	0.925 (0.76)	0.510 (0.61)	1.493 (1.50)	0.977 (1.46)
UD	.009 (1.06)	0.083 (2.01)*	0.077 (2.75)**	0.103 (3.28)**	0.101 (4.11)**
BRR	0.007 (1.57)	-0.019 (1.11)	-0.020 (1.47)	-0.019 (1.19)	-0.021 (1.64) ♦
TW	0.000 (0.01)	-0.064 (1.01)	-0.069 (1.45)	-0.044 (0.89)	-0.051 (1.30)
CBI	0.35 (0.79)	4.053 (2.27)*	4.142 (2.81)**	4.284 (2.45)*	4.102 (2.83)**
BC	-0.03 (0.42)	0.120 (0.53)	-0.109 (0.63)	-0.001 (0.01)	-0.162 (1.07)
Observations	*120*	*121*	*121*	*134*	*134*
No. of countries	*18*	*18*	*18*	*18*	*18*
Wald test on country Dumm	*F(17, 84) = 0.83* *P- value ≅ 0.6*	*F(17, 86) = 9.14* *P- value ≅ 0.000*	$\chi_{(17)} = 148.10$ *P- value ≅ 0.000*	*F(17, 102) = 10.13* *P- value ≅ 0.000*	$\chi_{(17)} = 135.51$ *P- value ≅ 0.000*
Wald test on time Dummies	*F(7, 84) = 5.05* *P- value ≅ 0.001*	*F(7, 86) = 6.97* *P- value ≅ 0.000*	$\chi_{(7)} = 80.14$ *P- value ≅ 0.000*	*F(7, 102) = 6.81* *P- value ≅ 0.000*	$\chi_{(7)} = 81.14$ *P- value ≅ 0.000*
Wald test on all the macro variables but Real Interest Rate	*F(3, 83) = 0.98* *P-value = 0.4*	*F(3, 81) = 0.50* *P-value = 0.6855*	$\chi_{(3)} = 1.55$ *P-value = 0.6714*		
Estimated Rho	*.3*	*.32*	*.25*	*.32*	*.44*
LM Autocorrelation test	$\chi_{(1)} = 7.05$ *P-value = .006*	$\chi_{(1)} = 7.39$ *P-value = .006*	$\chi_{(1)} = 12.$ *P-value = .0005*	$\chi_{(1)} = 13.86$ *P-value = .0001*	$\chi_{(1)} = 18.47$ *P-value = .0001*
Wald hetero-skedasticity test	$\chi_{(18)} = 305$ *P- value ≅ 0.000*	$\chi_{(18)} = 122.6$ *P- value ≅ 0.000*	$\chi_{(18)} = 27.1$ *P- value ≅ 0.07*	$\chi_{(18)} = 212$ *P- value ≅ 0.000*	$\chi_{(18)} = 25.3$ *P- value ≅ 0.11*
Adj R Square	*.97*	*.79*		*.81*	

Note: Absolute value of z statistics in parentheses. ♦significant at 10%,* significant at 5%** significant at 1%.

Table 10 compares fixed effects and random effects specifications. Columns one and three in the table are the same as in the previous table. Columns two and four report the corresponding random effect estimates. In the present paper, we opted for fixed effects, rather than random effects. It is, however, interesting, to compare the results of the two at this point. There are both theoretical and methodological reasons behind the choice for a fixed effects specification. In a fixed effect model, we introduce a country-specific intercept δ_i, which is intended to capture country-specific and time-invariant unobservable determinants of unemployment, and can also serve as a country-specific fix for possible misspecification.31 In this way, a fixed effects model catches at least a portion of the cross-country heterogeneity, which could not be captured by the slope coefficients, since these are constrained to be the same for all countries (except in so far as they are allowed to vary by different levels of wage coordination). The use of fixed effects is legitimate if the goal is to draw "inferences that are going to be confined to the effects in the model." (Hsiao, 1986: 43) The previous statement implies that inferences have to be limited to the set of OECD countries included in the sample. In the random effects model, it is assumed that the intercept is a random variable that is a function of a mean value (the constant) plus a random error. It is also assumed that the groups (in this case, countries) are random draws from a population, about whose parameters inferences are being made. The baseline hypothesis for consistent estimates from a random effects model is the absence of correlation between the unit specific effects (which are considered part of the error term) and the other covariates.

The model appears better specified when country dummies are inserted, as shown by the R-squared statistics in table 8. Indeed, country dummies seem to capture a large share of the variation in the unemployment rate. We also tested for fixed vs. random effects specification through a Hausman test, which is based on the null hypothesis that the coefficients estimated by the random effects estimator are the same as the ones estimated by the consistent fixed effects estimator. If the null hypothesis cannot be rejected, then it is safe to use random effects since we can assume no correlation between the covariates and the error term. When the full models with all macroeconomic predictors were compared, the null hypothesis was rejected. However, when the reduced models were considered (columns one and two), we could not reject the null hypothesis at the 5 per cent but only at the 10 per cent level. The random effect specification appears thus borderline acceptable compared with the fixed effects one, based on the Hausman test. However, non-randomness of the sample and better specification still make one prefer the fixed effects model to the random effects one.

[31] There could be other variables, not included in the model, which could influence the unemployment rate. Degree of competition in the goods and services markets, degree of labour mobility, demography, etc., are all examples of additional control variables that could have been inserted in our models. Lack of data, or of complete time series for some countries, prevented us from estimating a more comprehensive model. We aimed for a specification that was as close as possible to those used by IMF (2003) and others.

Table 10. Five-year data. Fixed and random effects models in levels (intercept omitted).

	5 years static model (with time dummies). OLS. Robust s.e. Fixed effects	5 years static model (with time dummies).[1] OLS. Random effects	5 years model. With macro variables. OLS. Robust s.e. FE	5 years model. With macro variables. OLS. RE
Dependent var.	**unr**	**unr**	**unr**	**unr**
Real interest rate	0.251 (3.41)**	0.234 (2.68)**	0.262 (3.19)**	0.255 (2.36)*
Change in inflation			-0.013 (0.08)	0.073 (0.30)
Terms of trade shocks			0.063 (0.18)	0.008 (0.02)
Lagged productivity change			0.194 (1.14)	0.215 (1.34)
EP	1.493 (1.50)	0.447 (0.71)	0.925 (0.76)	-0.180 (0.23)
UD	0.103 (3.28)**	0.053 (2.67)**	0.083 (2.01)*	0.034 (1.49)
BRR	-0.019 (1.19)	-0.019 (1.36)	-0.019 (1.11)	-0.017 (1.17)
TW	-0.044 (0.89)	-0.021 (0.62)	-0.064 (1.01)	-0.007 (0.17)
CBI	4.284 (2.45)*	2.818 (1.79) ♦	4.053 (2.27)*	2.086 (1.24)
BC	-0.001 (0.01)	-0.496 (2.53)*	0.120 (0.53)	-0.379 (1.76) ♦
Observations	134	134	121	121
No. of countries	18	18	18	18
Adj. R-squared	0.75	.5	.74	.51
Hausman Test results. Ho: difference in coefficients not systematic	$\chi_{(14)} =$ 22.74 P-value = 0.0646		$\chi_{(17)} = 211.45$ P-value = 0.0000	

Note: Absolute value of z statistics in parentheses. ♦significant at 10%,* significant at 5%. ** significant at 1%.

One of the reasons why the random effects specification is worth considering is that, dispensing with country dummies, it allows all countries to contribute to the determination of the coefficient estimates, including for those variables like employment protection, central bank independence and wage coordination, which are based on time-unvarying (for some countries) or sluggish indices. Some of the changes between the two specifications are remarkable. The employment protection variable is generally positive and insignificant in both fixed and random effects reduced-model specifications. However, the magnitude is much lower in the random than in the fixed effects model. Employment protection is even negative in column four, which uses a random effect specification. Similarly, the central bank independence coefficient has greater magnitude and lower standard error with fixed than with random effects. The greatest change concerns the coordination variable, which is negative and significant when random effects are considered (consistent with most literature, see, for example, Aidt and Tzannatos, 2002) but not when country dummies are inserted. Also, the magnitude of the union density coefficient is cut by about half when one shifts from fixed to random effects models.

Table 11 moves from models in levels to models in first differences. The reasons behind this choice are the following: first, the results of integration tests show that five-year data are non-stationary and the models we are estimating do not appear to be cointegrated (see Appendix 2).[32] The data in five-year averages are integrated of order 1, which justifies first-differencing. Second, differencing the data provides a solution to the problem of serial correlation of the error (we reject the null at the 5 per cent level in all cases). Also, a t-test on the lagged dependent variable (in column five) shows that this should not be included in a model in

first differences, unlike a model in levels. Therefore, a static model in first differences seems better specified than a static model in levels. We present two sets of estimates: one is FGLS corrected for heteroskedasticity, the other is OLS with the White standard errors. The coefficients have to be interpreted as the effect of average five-year changes in independent variables on change in unemployment in the same period, controlling for other determinants. This interpretation does not seem at odds with the basic policy question underlying this and other studies, namely understanding how unemployment would change over five years if institutions were to change (over five-years).

One would expect similar coefficient estimates from models in differences and in levels (even though the estimators are not exactly the same when $T>2$). This is indeed the case with most variables, but there are a few exceptions, as revealed by comparing the results reported in tables 8, 9, and 11. Not surprisingly, variables based on indicators, which change little over time, and especially employment protection and wage coordination, are the ones for which coefficient estimates vary the most. For example, employment protection is positive (albeit insignificant) when the models are estimated in levels, and negative (at times even significant) when the same models are estimated in differences. As argued above, this index is time-invariant for Australia, Canada, Japan, New Zealand, Switzerland, and the US. These are countries with low protection (high in the case of Japan) and high (low in the case of Japan) unemployment. They do not participate in the determination of the coefficient when the models are in levels and there are country dummies. This tilts the estimate towards a positive association. Similarly, wage coordination is positive (albeit insignificant) in two models in levels, while it is always negative in differences. The countries in which the wage coordination index is time-unvarying and which do not affect the coordination coefficient in levels (with country dummies) are Austria, Germany, Japan, and Switzerland, all characterized by high coordination and low unemployment on average across the time period. This, again, biases the estimate towards a positive sign. Interestingly enough, the other index, that of central bank independence, has similar coefficients and standard errors in both levels and differences.

Other coefficients do not vary much. The real interest rate variable is positive and significant in both levels and differences models, and its magnitude similar. The other macroeconomic variables are not significant, either in levels or in differences. Union density is positive and significant, and its magnitude is around 0.1. The benefit replacement rate is negative and (almost always) insignificant in both kinds of models. Tax wedge is negative and (mostly) insignificant. Interactions are not significantly different from zero (with one exception, that between coordination and employment protection, which is positive and significant at the 10 per cent level in column 6 of table 8). The Wald test reveals that their removal does not significantly reduce the fit. The co-ordination coefficient increases its absolute magnitude when interactions are omitted – it probably captures the effects of the omitted interactions – and is negative and significant at the 10 per cent level with FGLS estimation (table 11, column 3).

[32] We suspect, however, that the latter result may be due to low statistical power of the test.

Table 11. Five-year data. Full and reduced models in first differences. (intercept and time dummies omitted).

	FGLS static	OLS static with White robust standard errors	FGLS static	OLS static with White robust standard errors	Dynamic with White robust standard errors
Dependent var.	**unr**	**unr**	**unr**	**unr**	**unr**
Lagged Unemployment rate					0.15 (0.96)
Real interest rate	0.224 (2.42)	0.219 (2.20)*	0.265 (4.49)**	0.273 (3.73)**	(0.22) (2.28)*
Change in inflation	-0.077 (0.32)	-0.160 (0.64)			-0.19 (0.73)
Terms of trade shocks	0.161 (0.77)	0.031 (0.12)			0.02 (0.08)
Lagged productivity change	-0.152 (1.31)	-0.101 (0.75)			-0.14 (0.94)
EP	-1.715 (1.78) ♦	-1.747 (1.99)*	-1.083 (1.35)	-1.121 (1.58)	-1.75 (1.96)*
UD	0.095 (2.44)**	0.110 (2.12)*	0.102 (2.99)**	0.108 (2.53)*	0.11 (2.09)*
BRR	-0.013 (0.67)	-0.004 (0.19)	-0.007 (0.44)	-0.005 (0.23)	0.003 (0.14)
TW	-0.065 (1.31)	-0.063 (1.21)	-0.064 (1.42)	-0.071 (1.70) ♦	-0.06 (1.28)
CBI	4.301 (2.14)*	4.340 (2.09)*	4.121 (2.29)*	4.364 (1.99)*	4.8 (2.26)*
BC	-0.079 (0.37)	-0.061 (0.23)	-0.239 (1.80) ♦	-0.162 (1.05)	-0.03 (2.15)**
*BC*UD*	-0.018 (1.36)	-0.004 (0.28)			-.007 (0.53)
*BC*TW*	-0.011 (0.59)	-0.019 (0.97)			-0.019 0.04
*BC*EP*	0.507 (1.38)	0.604 (1.34)			0.59 (1.32)
*BC*BRR*	-0.001 (0.13)	-0.002 (0.16)			-.002 (0.22)
*BC*CBI*	-0.892 (0.84)	-0.691 (0.68)			-0.33 (0.32)
Observations	103	103	116	116	102
No. of countries	18	18	18	18	18
Adj R-square		0.36		0.35	
LM Autocorrelation test	$\chi_{(1)} = 2.62$ *P-value = .11*	$\chi_{(1)} = 1.9$ *P-value = .15*	$\chi_{(1)} = .33$ *P-value = 56*	$\chi_{(1)} = .39$ *P-value = 52*	$\chi_{(1)} = .36$ *P-value = 54*
Wald test on Macro variables (but RIR)	$\chi_{(3)} = 2.6$ *P-value =0.4532*	*F(3, 87) = 0.55* *P-value = 0.6506*			*F(3, 80) = 0.47* *P-value = 0.7*
Wald test on interactions:	$\chi_{(3)} = 4.67$ *P-value = 0.4580*	*F(5, 82) = 0.45* *P-value = 0.8153*			*F(5, 80) = 0.45* *P-value = 0.81*
Wald test on time dummies	$\chi_{(5)} = 15.57$ *P-value = 0.0082*	*F(5, 82) = 1.07* *P-value = 0.3838*	$\chi_{(7)} = 14.79$ *P-value = 0.0388*	$\chi_{(7)} = 7.60$ *P-value = 0.3692*	*F(5, 82) = 124* *P-value = 0.29*

Note: Absolute value of z statistics in parentheses. ♦significant at 10%,* significant at 5%; ** significant at 1%.

Table 12. Five-year data. Alternative estimates of the reduced model with the insertion of the benefit duration and benefit generosity variables. Data in first differences (intercept and time dummies omitted).

	FGLS	FGLS alternative bd	FGLS b generosity	FGLS alternative b generosity	OLS ROBUST	OLS ROBUST alternative bd	OLS ROBUST b generosity	OLS ROBUST alternative b generosity
Dependent var.	unr	unr	unr	unr	unr	unr	unr	unr
Real interest rate	0.275 (4.66)**	0.277 (4.70)**	0.267 (4.60)**	0.264 (4.42)**	0.285 (3.80)**	0.280 (3.69)**	0.281 (3.77)**	0.280 (3.75)**
EP	-1.387 (1.65) ♦	-1.101 (1.37)	-1.235 (1.48)	-0.917 (1.13)	-1.331 (1.77) ♦	-1.120 (1.53)	-1.282 (1.90) ♦	-1.090 (1.67) ♦
UD	0.101 (2.74)**	0.087 (2.49)*	0.101 (2.84)**	0.088 (2.52)*	0.125 (2.69)**	0.101 (2.30)*	0.126 (2.71)**	0.101 (2.34)*
BRR	-0.003 (0.18)	-0.009 (0.52)			-0.004 (0.21)	-0.006 (0.29)		
BD	0.225 (0.13)	-1.433 (1.33)			0.093 (0.05)	-0.439 (0.38)		
Benefit generosity			0.041 (1.64) ♦	0.030 (1.18)			0.026 (1.02)	0.022 (0.85)
TW	-0.048 (0.98)	-0.063 (1.40)	-0.066 (1.47)	-0.082 (1.88) ♦	-0.059 (1.24)	-0.069 (1.65) ♦	-0.072 (1.65) ♦	-0.081 (2.08)*
CBI	4.450 (2.44)*	4.093 (2.29)*	4.580 (2.56)*	4.413 (2.50)*	4.456 (2.04)*	4.346 (2.01)*	4.579 (2.05)*	4.615 (2.14)*
BC	-0.271 (2.05)*	-0.267 (2.01)*	-0.259 (2.00)*		-0.200 (1.26)	-0.166 (1.07)	-0.186 (1.17)	-0.154 (0.97)
Observations	*110*	*114*	*110*	*114*	*110*	*114*	*110*	*114*
No. of countries	*18*	*18*	*18*	*18*	*18*	*18*	*18*	*18*
Adj. R square			*0.26*	*0.26*	*0.27*	*0.27*		

Note: Absolute value of z statistics in parentheses ♦ significant at 10%,* significant at 5%; ** significant at 1%.

Table 12 presents specifications that include the benefit duration variable. As with models with yearly data, two different measures of benefit duration are used: the one appearing in the Nickell et al. (2001) database (columns one, three, five, and seven), and the one kindly provided by Baker et al. (2003), respectively (columns two, four, six, and eight). The latter comprises more complete series for countries over time. In columns one, two, five, and six, the benefit replacement rate variable and the benefit duration variable are entered separately; in columns three, four, seven, and eight the two are combined (by multiplication) in a single variable, which we call "benefit generosity." Columns one to four are estimated by FGLS with correction for heteroskedastic errors; columns 5 to eight, by OLS with White robust standard errors.

None of the two benefit-related variables is significant when entered individually. Benefit replacement rate is negative as before, while the sign of benefit duration depends on the measure used: it is positive with the Nickell et al. (2001)'s one and negative with the Baker et al. (2003)'s measure, which is possibly a better measure. Benefit generosity is instead positive, but it is significant (at the 10 per cent level) only with the first measure of benefit duration, and with FGLS, not with the second measure, or OLS. The other coefficients do not change much, with one exception: the wage coordination measure, which is negative across models, becomes significant with FGLS (but not with OLS). Tax wedge has a negative sign and it is often significant with OLS.[33]

In Appendix 3, we provide two robustness checks of our favourite model in first differences (table 11, columns three and four). First, we report the results of jack-knife analysis. This shows that no coefficient estimate seems to be overly influenced by outliers. Second, following Beck (2001: 282-3), we test for cross-sectional homogeneity through cross-validation. In other words, we estimate the model leaving out one country at a time, use the model to predict the values of the dependent variable for the excluded country, and then examine the (mean absolute) forecast error. The objective of this practice is to understand how well a country fits (or does not fit) a given pooled specification. The results of cross-validation show that our model predicts variation in unemployment changes in Ireland, Norway, and Sweden less well than in other countries. However, no country is so poorly predicted as to be considered an outlier.

4. Overview of findings

In this paper, we estimated various kinds of models. We started off with static models in levels using annual data, which we then turned into dynamic fixed effects models. We then shifted to dynamic models in first differences with yearly data. We then grouped our data in five-year averages and estimated fixed effects models in levels, random effects models in levels, as well as models in first differences. We generally used different estimators: FGLS, OLS (normally with some form of robust standard errors), and, sometimes, instrumental variable (IV) estimators. In this section, we provide a summary of results.

The dynamic models in levels with yearly data are <u>not</u> our preferred models. First, they all display serial correlation of the error term, which is potentially a source of biased estimates in dynamic models. Second, there does not seem to be any strong theoretical ground for a dynamic specification. We estimate this type of models because this is the kind appearing in a portion of the literature, especially IMF (2003), Nickell et al. (2001), and Nunziata (2001) – from which perhaps the strongest conclusions about the desirability of deregulation issue. Dynamic models with yearly data in first differences appear better behaved statistically, because they do not present serial correlation. However, there, too, is a potential source of bias represented by the correlation between the lagged dependent variable and the error term (by construction). When we use the Anderson and Hsiao (IV) estimator to correct for this problem we find similar results. Models with five-year data are to be preferred in our opinion, for reasons stated above, even though the number of observations is dramatically reduced. We estimated models in both levels and differences. First difference models are to be preferred because the five-year series are non-stationary and do not seem to be cointegrated. Also, five-year models in first differences do not seem to require a lagged dependent variable, unlike the models in levels. Another reason is that with first differences the ratio between parameters and observations is

[33] We also estimated a full model, with the whole set of macro controls, and of interactions, adding the benefit duration and benefit generosity measures. The results are not reported. However, they do not vary much. The interactions are mostly insignificant with three exceptions: the interaction between coordination and benefit duration is negative and significant at the 10 per cent level with FGLS; the interaction between coordination and benefit generosity is negative and significant at the 10 per cent level with FGLS; the interaction between coordination and employment protection is positive and significant at the 10 per cent level with FGLS when benefit generosity and its interaction are inserted.

much lower, because first differencing wipes out the fixed effects; therefore, coefficient estimates are probably more precise.[34]

The real interest rate is almost always a positive and significant predictor of unemployment especially with five-year data but often also with annual data. Its long-term coefficient is around 0.5 in models in levels with annual data, and about half in models with five-year data, both in levels and in differences. The latter value implies that for every four per cent increase in the real interest rate, there is a corresponding one per cent increase in unemployment. Real interest rates affect demand, especially for consumer durables, investment goods, and exports. Our findings suggest that these depressing effects are not just limited to the short-run, but also impact medium-to-long term unemployment and are, in this respect, a confirmation of Ball (1999: 189)'s argument that "determinants of aggregate demand have […] effects on long-run as well as short-run movements in unemployment." Other macroeconomic variables seem to have a more fleeting impact. For example, changes in consumer price indexes are negatively correlated with unemployment in models with one-year data, signalling the presence of a short-term Phillips-curve trade off. However, they are insignificant with five-year data. Lagged changes in productivity and terms of trade changes are also negatively associated with unemployment in yearly data models, possibly indicating the presence of real wage resistance interfering with the adjustment of real wages to shocks. However, with five-year data, changes in terms of trade and changes in productivity appear insignificant. This may imply that if the effect of these variables is mediated by real wage resistance, this effect is purely short-term and can no longer be captured when longer time frames are considered.

Among the institutional variables, employment protection is hardly ever significantly different from zero with annual data. This is in line with theoretical arguments, according to which the impact of employment protection on unemployment stocks is indeterminate as employment protection reduces employment and unemployment flows simultaneously and these effects tend to cancel each other out (see, for example, Blanchard and Wolfers, 1999: 8; Nickell, 1997: 66). It is also to be noted that the employment protection index is based on a limited number of observations, which are interpolated, and that the yearly data framework does not seem most appropriate for this variable (see Baker et al., 2003). With five-year data, employment protection estimates vary considerably between levels and differences. The coefficient is generally positive, but insignificant, in levels, and generally negative, and even significant, in differences. We attribute at least part of this shift to the influence of fixed effects when the measure, as in the case of some countries, is time-invariant. With random effects in levels, the sign of employment protection is indeterminate and depends on specification. We conclude that this variable does not seem to have a robust impact on aggregate unemployment.

In contrast with theoretical predictions, the benefit replacement rate variable is almost always negative and almost always insignificant with both annual and five-year data. It may be that if benefit replacement is a form of insurance, the cost of such insurance is borne by workers through lower real wages (Agell 1999; 2000). It could also be that the positive impact of benefit replacement on unemployment (for example, by increasing the reservation wage) is counterbalanced by a negative effect linked with a better match between jobs and worker skills when benefit replacement rates are higher. A few specifications also include a benefit duration variable, whose sign appears unstable and dependent on the particular measure used to operationalize the construct. A composite measure of benefit generosity, obtained by interacting both duration and replacement rate of benefits, is instead more robustly positive in sign, even though always insignificant except in one specification, with FGLS and at the 10 per cent

[34] With reduced models, there are approximately 4 observations for each parameter to be estimated when the model is in levels, and 8 observations per parameter in first differences.

confidence level. Overall, it seems that benefits do not impact unemployment. If they do, it is the combination of high replacement and high duration that seems to matter most.

The tax wedge estimates are also somewhat surprising, in that they are negative in all specifications with annual data, except the ones in which the variable is instrumented with its lag – which are, however, non significant.[35] With five-year data, tax wedge is still negative and more often significant than with yearly data, especially when the models control for benefit generosity. If the impact of the tax wedge depends on what portion of it is not paid for through lower real wages and contributes, therefore, to increase the real cost of labour faced by the employers, then one has to conclude that, on average, the whole tax wedge is paid for by workers, controlling for other variables in the model. The negative effect on unemployment may depend on the fact that lower take-home pay shifts the labour supply curve rightward, i.e., for given wage levels, workers increase their labour supply.

Union density is the one institutional variable that appears to have a robust positive impact on unemployment, independent of specification or estimation method used.[36] This may be due to unionisation driving wages above the market clearing level. According to our models, however, this increase seems of limited magnitude. The union density coefficient is normally 0.1 with five-year data and is cut by about half when a random effects model is estimated. This implies that a 10 per cent increase in union density is, on average, associated with a 1 per cent increase in overall unemployment, controlling for other determinants.[37] With annual data, there is evidence that the positive effect of union density declines with growing coordination, i.e. that a more encompassing bargaining system partially internalises the externalities caused by wage pressure.

The central bank independence variable is positive with annual data, but sometimes not significantly different from zero. With five-year data, its coefficient is much larger and always significant, which suggests that an increase in central bank independence leads to greater unemployment, controlling for other determinants of unemployment and especially for the degree of wage coordination. Our point estimates with five-year data – greater than four – suggest that the transition from a totally independent to a totally politically dependent monetary authority is associated with a decrease in unemployment by more than 4 percentage points.[38] Interestingly enough, central bank independence does not increase unemployment through greater real interest rates in our model, since the latter are controlled for, and operates through other channels. The two measures, central bank independence and real interest rates, are weekly correlated with one another (the correlation coefficient is around 0.14 with both annual and five-year data). It seems that high real interest rates in our sample do not depend on the particular stance of the central bank, but on other factors, e.g. perceived country risk (which may depend on budget deficits and debts).

The wage coordination variable is insignificant in most specifications, and often even "wrongly" signed, i.e., positively rather than negatively. The effect of coordination is, according to our models, ultimately the result of a modelling choice. If fixed effects are accounted for, either directly, or indirectly by taking first differences, then this variable does not seem to have a

[35] Daveri and Tabellini (1997: 24) mention the possibility of endogeneity between unemployment and tax wedge because high unemployment may induce countries to increase taxes to pay for unemployment benefits. Endogeneity may be responsible for the change of sign.

[36] It should be noted, however, that when our basic specification is estimated as a random coefficient model, the coefficient is not significantly different from zero.

[37] The effect could, however, vary across demographic groups and be higher for workers with more elastic supply curves, like women and youth, and lower for workers with less elastic supply curves, like prime age males (see Bertola et al., 2003).

[38] With random effects, the coefficient is somewhat lower.

significant impact on unemployment. If, however, fixed effects are not included in the model (for example, in random effects models), the coefficient of the coordination variable is negative and significant. It is possible that with better specified models we could be able to dispose of country dummies (which are nothing more than labels) and be able to appreciate the cross-sectional effect of the wage coordination variable. For the time being, however, a model without fixed effects seems more than likely to suffer from omitted variable bias. We conclude that the effects of wage coordination that seem to matter for unemployment are the cross-sectional ones, while the within-country variation in wage coordination, controlling for cross-sectional differences, does not significantly reduce unemployment. Cross-sectional differences probably reflect the rest of the institutional structure (e.g. social democracy and associated economic policies) in which wage coordination in embedded. From a policy perspective, simply increasing the level of bargaining coordination, in the absence of parallel changes in the rest of the institutional structure, would not reduce unemployment, based on our results.[39]

Among the interaction variables, the ones between union density and wage coordination, and between central bank independence and wage coordination, have the expected negative sign and appear significantly different from zero with annual data (although not always, especially with the latter). As argued above, it seems that coordination increases union capacity to internalise externalities – which explains the negative sign of the interaction between union density and wage coordination. Also, as argued by Hall and Franzese (1998), the employment-depressing effects of restrictive monetary policies enacted by an independent central bank are likely to decrease with greater coordination, because when the bargaining system is coordinated, it can more easily adapt its wage behaviour to the particular monetary stance adopted by the central bank than when the system is uncoordinated. Our results provide some support for this thesis, but the coefficients are often not significantly different from zero. When five-year averages are considered, it seems that no interaction holds. This may imply that wage coordination mediates the impact of institutions only in the short-term.

5. Concluding remarks

In this paper, we examined what kind of support data on OECD countries provide for the deregulatory view of unemployment, according to which variations in unemployment are explained by variations in labour market and other institutions. In proceeding to estimation, we paid attention to a series of statistical problems generally associated with this kind of time-series cross-section data:

1) Non-stationarity of the series.
2) Possible sources of bias in dynamic models.
3) Violations of other standard assumptions concerning the error term.

Our preferred model is a static fixed effects model in first differences with data averaged over five-year periods. We arrived at it by testing down from our initial specification. It is a parsimonious model, in which only the interest rate appears as macroeconomic control alongside the institutional variables, and there are no interaction terms. This specification gives changes in institutions more than a fair chance to explain changes in unemployment. Yet this model (just like the others we estimate in this paper) provides very little support for the view that one could reduce unemployment simply by getting rid of institutional rigidities. We find that an increase in interest rates raises unemployment and that countries that augment the level of independence of their central bank end up augmenting the unemployment rate as well.

[39] Many thanks to Andrew Glyn for suggesting this explanation.

Changes in employment protection, benefit replacement rates, and tax wedge seem negatively associated with changes in unemployment, even though the coefficients are (mostly but not always) insignificant. The one institutional variable we find to be positively associated with changes in unemployment is the union density change variable. Other interesting results from our analysis concern the bargaining coordination variable, which turns out to be mostly an insignificant predictor when fixed effects are controlled for, in contrast with most literature that attributes to it a negative impact on unemployment.

What transpires from these findings is that unemployment is mostly increased by policies and institutions that lead to restrictive macroeconomic policies. Obviously, there could be more fine-grained effects of institutions that are not captured by our models. For example, labour market institutions may affect different demographic groups in different ways, so that even though there is no average effect on unemployment, there are distinct effects of group-specific employment and unemployment rates, e.g. for women and the youth (see Bertola et al, 2001). Similarly, as argued by Blanchard and Wolfers (1999), institutions may impact unemployment not so much directly as by magnifying the effects of adverse macroeconomic shocks. We cannot assess these effects with our specification. However, the claim that systematic deregulation of labour markets would solve the unemployment problem faced by several advanced countries appears unwarranted based on our results.

References

Aidt, T. and Tsannatos Z. (2002), Unions and Collective Bargaining. Washington, D.C.: The World Bank.

Anderson, T. W. and Hsiao, C. (1981), "Estimation of Dynamic Models with Error Components." Journal of American Statistical Association **76**(375), 598-606.

Arellano, M. (2003), Panel Data Econometrics. Oxford: Oxford University Press.

Arellano, M. and Bond, S. (1991), "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application of Employment Equations." Review of Economic Studies **58**: 277-297.

Arellano, M. and Bover, O. (1995), "Another Look at the Instrumental Variable Estimation of Error Component Models." Journal of Econometrics **68**: 29-51

Ball L. (1999), **"**Aggregate Demand and Long-Run Unemployment." Brookings Papers on Economic Activity **2**: 189-251.

Baltagi, B. (2001), Econometric Analysis of Panel Data. New York: Wiley.

Baltagi, B. and Li, Q. (1995), **"**Testing AR(1)Against MA(1) Disturbances in an Error Component Model." Journal of Econometrics **68**(1), 133-151.

Baker, D., Glyn, A., Howell, D., and Schmitt, J. (2002), "Labor Market Institutions and Unemployment: A Critical Assessment of the Cross-Country Evidence." New York: Center for Economic Policy Analysis, New School University, WP 2002-17.

Baker, D., Glyn A., Howell, D., and Schmitt, J. (2003), "Unemployment and Labour Market Institutions: The Failure of the Empirical Case for Deregulation." Paper prepared for the International Labour Organization.

Beck, N. (2001), "Time-Series-Cross-Section Data: What have we learned in the past few years?" Annual review of Political Science **4**: 271-293.

Beck, N. and Katz, J. (2004), "Time-Series–Cross-Section Issues: Dynamics". Unpublished paper, available at http://www.nyu.edu/gsas/dept/politics/faculty/beck/beckkatz.pdf

Beck, N. and Katz, J. (2001), "Random Coefficient Models for Time-Series–Cross-Section Data: The 2001 Version". Paper prepared for delivery at the 2001 Annual Meeting of the Society for Political Methodology, Emory University.

Beck, N. and Katz, J. (1996), "Nuisance vs. Substance: Specifying and Estimating Time-Series-Cross-Section Models." Political Analysis **6**: 1-36.

Beck, N. and Katz, J. (1995), "What to do (and not to do) with time series cross section data." American Political Science Review **89**(3), 634-647.

Bertola, G. (2004), "Labor Market Institutions in a Changing World." Working paper originally prepared for "Rethinking Economic and Social Policies (XVI Symposium Moneda y Credito, Madrid, November 26-27, 2003).” Revised draft available at: http://www.personalweb.unito.it/giuseppe.bertola/LMIinCW.pdf

Bertola, G., Blau, F. D., and Kahn, L. M. (2002), “Comparative Analysis of Labor Market Outcomes: Lessons for the US from International Long-Run Evidence.” In Krieger, A. and Solow, R. (eds.), The Roaring Nineties: Can Full Employment Be Sustained? New York: Russell Sage and Century Foundations: 159-218 (citations in the paper are from the 2001 manuscript version).

Bertola, G., Blau, F. D., and Kahn, L. M. (2003), “Labor Market Institutions and Demographic Employment Patters.” Unpublished paper, August.

Belot, M. and Van Ours, J. (2000), “Does the Recent Success of Some OECD Countries in Lowering their Unemployment Rates Lie in the Clever Design of their Labour Market Reforms?” IZA Discussion Paper, No. 147.

Blanchard, O. (1999), **“**European Unemployment: The Role of Shocks and Institutions.” Unpublished paper, MIT.

Blanchard, O. and Wolfers, J. (2000), “The Role of Shocks and Institutions in the Rise of European Unemployment: The Aggregate Evidence.” The Economic Journal **462**.

Blundell, R. and Bond, S. (1998), “Initial Conditions and Moment Restrictions in Dynamic Panel Data Models.” Journal of Econometrics **87**: 115-143.

Checchi, D. and Lucifora, C. (2002), “Unions and Labour Market Institutions in Europe.” Economic Policy **35**: 363-408.

Calmfors, L. and Driffill, J. (1988). “Centralisation of Wage Bargaining, Corporatism and Macroeconomic Performance.” Economic Policy **6**.

Cuckierman, A. and Lippi, F. (1999**)**. “CBI, Centralization of Wage Bargaining, Inflation and Unemployment – Theory and Some Evidence,” European Economic Review **43**, 1395-1434

Daveri, F. and Tabellini, G. (2000), “Unemployment, growth and taxation in industrial countries.” Economic Policy **30**: 47-104 (citations refer to a 1997 manuscript version).

Daunfeldt, S.-O. and de Luna, X. (2002), “Central Bank Independence and Price Stability: Evidence from 23 OECD-countries.” Umeå Economic Studies **589**, Umeå University.

Dezhbakhsh, H. (1990), “The Inappropriate Use of Serial Correlation Tests in Dynamic Linear Models.” The Review of Economics and Statistics **72**(1), 126-32.

Dickey, D. and Fuller, W. (1979), “Distribution of the estimators for autoregressive time series with a unit root.” Journal of the American Statistical Association **74**.

Drukker, D. M. (2003), “Testing for serial correlation in linear panel-data models.” Stata Journal **3** (2), 168-177.

Ebbinghaus, B. and Visser, J. (2000), Trade Unions in Western Europe. MacMillan: London.

Eijffinger, S-C-W & de Haan, J. (1996), "The Political Economy of Central-Bank Independence," Princeton Studies in International Economics 19.

Elmeskov, J., Martin J. P., and Scarpetta S. (1998), "Key Lessons for Labour Market Reforms: Evidence from OECD Countries' Experiences". Swedish Economic Policy Review, **5**(2), 205-52, Autumn.

Fitoussi, J-P, Jestaz, D., Phelps, E., and Zoeg.a, G. (2000), "Roots of the Recent Recoveries: Labor Reforms or Private Sector Forces?" Brookings Papers on Economic Activity 1. 2000.

Flanagan, Robert J. (1999), "Macroeconomic Performance and Collective Bargaining: An International Perspective. "Journal of Economic Literature **37**:1150-75.

Franzese, R.J. (2001), "Institutional and Sectoral Interactions in Monetary Policy and Wage-Price Bargaining**,"** In Varieties of Capitalism: The Institutional Foundations of Comparative Advantage. P. Hall and D. Soskice, eds., Cambridge University Press, pp. 104-44.

Freeman, R., Medoff, J. (1984), What Do Unions Do? New York: Basic Books.

Gaduh, A. (2002), "Properties of Fixed Effects Dynamic Panel Data Estimators for a Typical Growth Dataset." CSIS Economics Working Paper Series. WPE062, Centre for Strategic and International Studies, Jakarta, Indonesia.

Greene, W. (2003), Econometric Analysis. 5th Edition, Prentice Hall.

Hall, P. A. and Franzese, R. J. Jr. (1998), "Mixed Signals: CBI, Coordinated Wage-Bargaining, and European Monetary Union." International Organization, Summer 1998.

Hall, P. A., Soskice D., (2001), Varieties of Capitalism: The Institutional Foundations of Comparative Advantage. Oxford/New York: Oxford University Press.

Hsiao (1986), Analysis of Panel Data. Cambridge, Cambridge University Press

IMF (2003), World Economic Outlook. Chapter 4.

Iversen, T. (1998), "Wage Bargaining, CBI and the Real Effects of Money." **52** International Organization (summer).

Judson, R. Owen, A. (1999), "Estimating Dynamic Panel Data Models: A Guide for Macroeconomists." Economics Letters **65**: 9-15 (October 1999).

Kenworthy, L. (2002), "Corporatism and Unemployment in the 1980s and 1990s." American Sociological Review **67(**3), 367-388.

Kenworthy, L. (2003), "Quantitative Indicators of Corporatism." International Journal of Sociology **33(**3), 10-44

Kittel, B. and. Winner, H. (2001), "How Reliable is Pooled Analysis in Political Economy. The Globalization-Welfare State Nexus Revisited." Max Planck Institut fur Gesellschaftsforschung Discussion Paper.

Kiviet, J. F. (1995), "On Bias, inconsistency and Efficiency of various estimators in dynamic panel data models." Journal of Econometrics **68**: 53-68.

Maddala, G. S. and Wu, S. (1999), "A comparative study of unit root tests with panel data and a new simple test." Oxford Bulletin of Economics and Statistics. **Special Issue**: 631-652.

Nickell, S. (1981), "Biases in dynamic models with fixed effects." Econometrica. **49**: 1417-1426.

Nickell, S. (1997), "Unemployment and labor market rigidities. Europe vs North America" Journal of Economic Perspectives **11**(3), 55-74.

Nickell, S. J., Nunziata, L, Ochel, and W, Quintini, G. (2001), "The Beveridge Curve, Unemployment and Wages in the OECD." CEP Discussion Papers 0502, Centre for Economic Performance, LSE.

Nunziata, L. (2002), "Unemployment, Labour Market Institutions and Schocks". Nuffield College Working Papers in Economics, 2002-W16.

Nunziata, L. (2001), "Institutions and Wage Determination: a Multi-Country Approach." Nuffield College Working Papers in Economics, 2001-W29, under revision for Oxford Bulletin of Economics and Statistics.

OECD (1998), Employment Outlook. Paris: OECD.

Phillips, P. C. and Perron, P. (1988), "Testing for a unit root in time series regression." Biometrika **75**: 335-346.

Siebert, H. (1997), "Labor market rigidities: at the root of unemployment in Europe." Journal of Economic Perspectives **11**(3), 37-54.

Schettkat, R. (2003), "Labour market flexibility and unemployment regulation and flexibility of markets, taking the Netherlands and the Federal Republic of Germany as an example." In: U. Blien and F. den Butter (Hrsg.) Labour participation and unemployment: a comparison of developments and institutions in Germany and the Netherlands. The Hague: S. 123-138 Reihe / Serie: Scientific Council for Government Policy. Working documents Nr. 133.

Soskice, D. (1990), "Wage Determination: The Changing Role of Institutions in Advanced Industrialized Countries." Oxford Review of Economic Policy **6**(4), 36- 61.

Swamy, P. (1970), "Efficient Inference in a Random Coefficient Model." Econometrica **38**: 311-323.

Traxler, F. and Kittel, B. (1999), "The Bargaining System and Performance: A Comparison of 18 OECD Countries." Comparative Political Studies.

Western, B. (1997), Between Class and Markets: Postwar Unionization in the Capitalist Democracies. Princeton, NJ: Princeton University Press.

Annex I

The Data

We use the time-series cross-section (TSCS) dataset made available to us by Baker et al. (2003). This is based on the IMF (2003) dataset with some modifications. The IMF dataset, in turn, updates the Nickell and Nunziata (2001) (henceforth NN) dataset. The latter is mostly based on information gathered by the OECD. The modifications introduced by Baker et al. concern specific countries and/or the years 1996-1998, and are drawn from other OECD databases (for details, see Baker et al., 2003: 27). The bargaining coordination (BC) index we use is a new measure elaborated and made available to us by Lane Kenworthy (2003).

The countries included in the sample are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, United Kingdom and United States. The years covered are 1960-98.

Macroeconomic variables

Unemployment Rate (UNR), from IMF (2003). All data are from historical OECD databases for standardized unemployment rate.

Real Interest Rates: from IMF (2003). This is the NN series updated for 1995-99 based on OECD Economic Outlook series for long-term interest rates and consumer price deflator. The measure is defined as nominal returns on long-term government bond minus the actual inflation rate over the following year.

Change in Inflation Rate: from IMF (2003), yearly changes in Consumer Prices Indexes, based on OECD databases. The formula for country *i* is CPI*t*-CPI*t-1*

Labor Productivity Growth (lagged), from IMF (2003). The series is based on OECD data. Productivity growth for country *i* is defined as: 100*(Prod*t*-Prod*t-1*/Prod*t-1*).

Terms of Trade Shocks: from IMF (2003), with raw data on export prices, import prices and trade openness from OECD databases. The measure is defined as first log-difference of the terms of trade multiplied by trade openness. The trade openness of the country is defined as the ratio between imports plus exports and GDP (at constant prices).

Money Supply Shock: alternative measure from NN database. Defined as ln(money supply) from the OECD Economic Outlook database.

Real Import prices: alternative to terms of trade shocks series, from NN database. Defined as the import price deflator normalized by the GDP deflator. Source: OECD, National Accounts and Main Economic Indicators. The real import price shock is the change in the log of real import prices times the share of imports in GDP (from OECD Main Economic Indicators).

Total Factor Productivity Shocks: from NN database. Based on the Solow residual for each country (see Nickell and Nunziata, 2000, for details). The measure here is the cyclical component of TFP, i.e. the deviation of the Solow residual from its Hodrick-Prescott filter trend.

Labour Demand Shocks: from NN database. Residuals from country-specific employment equations, each being a regression of employment on lags of employment and real wages.

Employment to Population Ratio: from Baker et al. (2003). Total civilian employment divided by the working age population (15-64), based on OECD dataset.

Institutional variables

Employment Protection Legislation (EP), from NN**.** This variable presents some peculiarities that undermine its strength as an indicator (see Baker et al., 2003: 6 and ft. 4). The NN measure draws on Blanchard and Wolfers (2000). It is based on two data points for the late 1990s and late 1980s. From these, Blanchard and Wolfers created two other data points interpolating the previous measure for the early 90s and another one for the early 1980s, simply taking the late 1980s figures which were assumed to be constant. For the years 1960-1979, the data come from another source (Lazear, 1990). The measure is a 0-2 index where 2 is the highest level of employment legislation protection.

Union Density (UD), from Baker et al. (2003). This is the NN series updated for 1995-99 based on Ebbinghaus and Visser (2000) as well as other sources.

Benefit replacement rate (BRR), from Baker et al. (2003). They make minor modifications to the NN series for three Scandinavian countries in the 1970s. Data are expressed in percentage points.

Benefit Duration (BD). We use two series. The first is taken from the NN database. The second was provided by Baker et al. (2003) and is based on the OECD database. Both series proxy the duration of unemployment benefits with to the ratio of benefits available after the first year to benefits available in the first year of unemployment. The second series is slightly different from the one in the Nickell et al.'s database. In particular, it has less missing values, because it attributes 0 values when the OECD series showed no benefits after year one.

Taw Wedge (TW), from Baker et al. (2003), who update the NN series "based on changes in the sum of individual (income) tax, social security contributions (employer and employee), payroll taxes, VAT, sales taxes, excise taxes and customs duties, all over GDP ([…] from OECD data)." (p. 27) Data are in per centage points.

Central Bank Independence index (CBI), from IMF (2003). The CBI index is borrowed from Rob Franzese (see Hall and Franzese (1997), and the IMF expanded the time series after 1991, based on information on more recent reforms in Daunfeldt and de Luna (2002). The index ranges from 0 to 1, where one is the maximum independence level.

Index of Co-ordination in wage setting (BC), the variable is taken from Kenworthy (2003). The index ranges from 1 to 5 where 1 is the minimum co-ordination. We introduced minor changes for Ireland between 1988 and 1992 and Italy in the 1990s, based on our previous work.

Co-ordination Indexes from NN database (1 and 2). Series 1 is based on interpolations of OECD data (OECD Employment Outlook 1994, 1997) and data made available by Michèle Belot, described in Belot and van Ours (2000). Series 2 is based on various secondary sources. Both these series, unlike the Kenworthy's index, are up to 1995. In the words of Nickell et al. (2001: 8) the first index ignores transient changes, whereas the second tries to capture more nuanced variations in the institutional structure.

Annex II

Tests for stationarity and co-integration

In order to test whether the series we used in our models were stationary or not, we checked for unit roots in each country series, using both the Augmented Dickey Fuller test (ADF) and the Philips Perron test (PP), as suggested by Nunziata (2001:12).[40] This was to double-check the results, considering the limited power of the ADF tests. The results were in the large majority of cases consistent between the two tests.

Table B1. Results of stationarity tests on annual data

country	unr	ep	Bargain-ing Coordi-nation	Union density	Benefit Repacement rate	TW	ud_bc	Ep_bc	Brr_bc	Tw_bc	Cbi	*Cb_.bc*	rir	tots	Prductivity change	Inflation change
Australia	1,2	0,0	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	0,0	1,2	1,2	0,0	1,2	0,0
Austria	1,2	1,2	0,0	1,2	1,2	1,2	1,2	0,2	1,2	1,2	0,0	0,0	1,2	0,0	1,2	1,0
Belgium	1,2	1,2	1,2	0,0	1,2	1,2	1,2	0,0	1,2	1,2	1,0	1,2	1,2	0,0	1,2	1,2
Canada	1,2	1,2	0,0	0,0	1,2	1,2	1,2	0,0	1,2	1,2	0,0	1,0	1,2	0,0	1,2	1,2
Denmark	1,2	1,2	1,2	1,2	1,2	0,0	1,2	1,2	1,2	0,0	0,0	1,2	1,2	0,0	1,2	1,2
Finland	1,2	1,2	1,2	0,0	1,2	1,2	1,2	1,2	1,2	1,2	0,2	1,2	1,2	0,0	1,2	1,0
France	1,2	0,0	1,2	1,2	1,2	1,2	0,0	1,2	0,0	1,2	0,2	0,0	1,2	0,0	1,2	1,2
Germany	1,2	0,0	1,0	1,2	1,2	1,2	1,2	1,2	1,2	1,2	0,0	0,0	1,2	0,0	0,0	1,2
Ireland	1,2	0,2	0,0	0,0	1,2	1,2	0,0	1,2	1,2	1,2	1,0	1,2	1,2	0,0	1,2	1,2
Italy	1,2	1,2	0,2	1,2	1,2	0,0	0,0	1,2	1,2	1,2	1,2	1,2	1,2	0,0	0,0	1,2
Japan	1,2	1,2	1,2	1,2	1,2	0,2	1,2	0,0	0,0	1,2	0,0	0,0	1,2	0,0	0,0	1,2
Netherlands	1,2	1,2	1,2	1,2	1,2	1,2	1,0	1,2	1,2	1,2	1,0	1,2	1,2	0,0	1,2	1,2
Norway	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	0,0	1,2	1,2	1,2	0,0	0,0	1,2
New Zealand	1,2	1,2	1,2	1,2	1,2	1,2	0,2	1,2	1,2	0,0	1,2	1,2	1,2	0,0	0,0	1,2
Sweden	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	0,0	1,2	1,2	0,0	0,0	1,2
Switzerland	0,2	0,0	0,0	1,2	1,2	1,2	1,2	1,2	1,2	1,2	0,0	0,0	0,0	0,0	1,2	1,2
UK	1,2	0,0	1,2	0,0	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	0,0	0,0	1,2
USA	1,2	0,0	0,0	0,0	1,2	1,2	0,0	1,2	1,2	1,2	0,0	0,0	1,2	1,0	0,0	1,2

Note: 1,2={Ho: Unit Root} not rejected at 10% level, according respectively to the ADF and PP tests, 0 =Ho rejected (according respectively to the ADF and PP tests).

[40] The appropriate specifications were determined by trial and error based on the three possible alternatives (with trend and constant, constant only, no constant-no trend), and the optimal number of lags was selected according to the Akaike Information Criterion for the ADF (choosing among max 2 lags with the 5-year data). For the PP test the Newey West bandwidth was selected using the Bartlett Kernel.

Table B2. Results of stationarity tests with five-year data[41]

Country	unr	ep	Bargaining coordination	Union density	Benefit Replacement rate	TW	ud_bc	Ep_bc	Brr_bc	Tw_bc	cbi	Cbi_bc	rir	tots	Productivity change	Inflation change
Australia	1,2	0,0	1,2	1,2	0,0	1,2	0,0	1,2	1,2	1,2	0,0	1,2	1,2	0,0	1,2	0,0
Austria	1,2	0,2	0,0	1,2	1,2	1,2	1,2	0,2	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0
Belgium	1,2	0,0	0,2	1,2	1,2	0,0	0,0	0,0	1,2	1,2	1,2	1,2	1,2	0,0	1,2	0,0
Canada	1,2	0,0	0,0	1,2	1,2	1,2	1,0	0,0	1,2	1,2	0,0	1,0	1,2	0,0	1,2	1,0
Denmark	1,2	1,2	0,0	1,2	1,2	1,2	0,0	0,0	0,2	0,0	0,0	0,0	1,2	0,0	1,2	0,0
Finland	1,2	1,2	1,2	0,0	0,0	0,0	1,2	0,0	1,2	1,2	0,2	1,2	1,2	0,0	0,0	1,0
France	1,2	0,0	0,0	1,2	1,2	1,2	1,2	1,2	0,0	1,2	1,2	1,2	1,2	0,0	1,2	1,2
Germany	1,2	0,0	0,0	1,2	1,0	1,2	1,2	0,0	1,0	1,2	0,0	1,2	1,2	0,0	0,0	1,2
Ireland	1,2	0,0	0,0	1,2	1,2	0,0	0,0	0,0	1,2	0,0	0,0	0,0	1,2	0,0	1,2	1,2
Italy	0,0	1,2	1,2	1,2	1,2	1,2	0,0	1,2	1,2	1,2	0,0	1,2	1,2	0,0	0,0	1,2
Japan	1,0	0,0	0,0	1,2	0,0	0,0	1,2	0,0	1,2	1,2	0,0	0,0	1,2	0,0	1,2	0,0
Netherlands	1,2	0,0	0,0	1,2	0,0	0,0	0,0	0,0	0,0	0,0	0,0		1,2	1,0	1,2	1,2
Norway	1,2	0,2	1,0	1,2	1,2	0,0	1,0	0,0	0,0	0,0	1,0	0,0	1,2	0,0	1,2	0,0
New Zealand	0,1	0,0	1,2	1,0	0,0	1,2	1,2	1,2	1,2	1,2	1,2	1,2	1,2	0,0	0,0	1,2
Sweden	1,2	0,0	0,0	1,2	1,2	0,0	1,2	0,0	1,2	0,0	0,0	1,0	1,2	0,0	1,2	0,0
Switzerland	1,2	0,0	0,0	1,2	1,0	1,2	0,2	1,2	1,2	1,2	0,2	1,0	0,0	0,0	1,2	0,0
UK	1,2	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	0,0	1,2	0,0	1,2	0,0	0,2	0,0
USA	0,2	1,2	0,0	0,0	1,2	0,0	0,2	0,0	1,2	0,0	0,0	0,0	0,0	0,0	0,0	1,2

Note: 1,2 = {Ho: Unit Root} not rejected at 10% level, according respectively to the ADF and PP tests, 0 = Ho rejected (according respectively to the ADF and PP tests).

From the previous tables, it appears that performing regressions in levels may be problematic, because at least some of the variables, in primis the unemployment rate, do not seem to be stationary, despite being integrated of order 1.[42] For the full models in levels with yearly data, we hence checked if a co-integrating relationship between the series exists.

The test consists in controlling the stationarity of the residuals of the regression of the non-stationary variables. Several tests have been proposed in the literature but none appears to be particularly powerful, especially in the case of unbalanced panels. We implemented the test for co-integration proposed by Maddala and Wu (1999), which is explicitly designed for unbalanced panels such as the one we have.[43] As Nunziata notes (2001: 13), "this test combines the results of N by country unit roots tests of any kind, each with P-value Pi, in the statistic:

[41] The ADF test and the PP tests are based on critical values for 20 observations, which may not be appropriate for a sample size of eight. We present these results being aware of their problems.

[42] The results of the unit root tests on the differenced series are omitted.

[43] A possible drawback of this method is that the critical values for these tests, and, by consequence, the p-values, are

$$-2(\sum_i \log P_i)$$

which was shown to be χ^2 distributed with 2xN degrees of freedom."[44] To perform this test, we adopted the Augmented Dickey Fuller stationarity tests and Philips Perron tests with the appropriate option in terms of trend.[45] The P-values are Mackinnon approximations.
Here are the results of this test:

Table B3. Results of the Maddala and Wu test with ADF unit root test

	Basic dynamic model. Annual data.	**Dynamic Model with Benefit Duration. Annual Data**
MW Test statist	83.62 P val: 0.0001	70.53 P val: 0.0002
Degrees of freedom	2*18 = 36	2*17 = 34
Note: Ho is presence of Unit root in the residuals i.e. no co-integration amongst the variables.		

And the same test carried out by performing the Philips Perron unit root tests:[46]

Table B4. Results of the Maddala and Wu test with Philips Perron Unit Root test.

	Basic dynamic model. Annual data.	**Basic static model, 5-year data.**	**Model with Benefit Duration. Annual data (dynamic)**	**Model with Benefit Duration. 5- year data (static)**
MW Test statistic	101.22 P. Val: 0.0001	16.94 P. Val: 0.9971	86.86* P. Val: 0.0001	21.61 P. Val: 0.8679
Degrees of Freedom47	2*18 = 36	2*18 = 36	2*17 = 34	2*15 = 30
Note: Ho is presence of unit roots in the residuals i.e. no co-integration amongst the variables.				

According to these results, our proposed model in levels should not present any problem concerning the existence of a co-integrating relationship when estimated with annual data. Both the tests performed strongly reject the null of a unit root in the residuals. There may be however some concerns about the models estimated with 5-year data, since these do not appear to present a stationary relationship. These results should be taken with caution, given the small size of the sample. Nevertheless, we addressed the possible methodological problems by estimating a model in first differences.

---

normally defined for samples larger then some of the ones we were testing for. For the smaller sample (with five-year data), it could be particularly problematic to perform the Augmented Dickey Fuller Test; hence we only performed he Philips Perron test.

[44] This test assumes that there is no cross-sectional correlation of the errors. Because we control (like Nunziata: 13) for cross-country correlation through the insertion of time dummies, we assume (somewhat incorrectly) that the test statistics follow the Chi square distribution and, therefore, use its critical values (the alternative would be bootstrapping to determine the critical values). Concerns about the critical values are likely to arise only in case the margin of acceptance/rejection is thin (which is not our case).

[45] The maximum lag length was selected according to the Akaike Information Criterion (note that the use of other criteria may slightly alter the results). All the MW tests, as well as the stationarity tests, have been performed with E-views 4.1.

[46] The stationarity of the series was also checked with a third type of control: we performed a Kwiatkowski-Phillips-Schmidt-Shin test on the single series, to cross-check the results of the ADF and PP tests (which may lack power, specially in small samples as some of the ones we tested for). This test confirmed the results of the other two in the large majority of cases.

[47] Intuitively, the degrees of freedom should be equal to the number of cross sections for which the unit root test is performed (times two). Because of lack of observations for a few countries, it was not possible to perform the test on all countries. This explains the different d.f. between the tests including benefit duration and the others.

Annex III

Robustness tests

Jacknife analyisis

Table C1. Five-year data. Jacknife analysis. Reduced model in differences. FGLS estimation with correction for heteroskedasticity.

	Minimum Coefficient	Country Excluded	Estimate	Maximum Coefficient	Country Excluded	Number of non significant estimations (at 10%l evel)
Dependent var.	**UNR**	**UNR**	**UNR**	**UNR**	**UNR**	**UNR**
Real interest rate	0.24 (4.02)	Ireland	0.265 (4.49)	0.31 (5.07)	Japan	0
Employ-ment protection	-1.303 (1.56)	Belgium	-1.083 (1.35)	-0.86 (1.14)	France	17
Union density	0.091 (2.57)	Austria	0.102 (2.99)	0.116 (3.37)	Netherlands	0
Bargaining coordi-nation	-0.277 (2.04)	Belgium	-0.239 (1.80)	-0.16 (1.15)	Australia	5
BRR	-0.02 (1.4)	Denmark	-0.007 (0.44)	0 (0)	Austria	18
TW	-0-08 (1.87)	Australia	-0.064 (1.42)	-0.02 (0.59)	Canada	13
CBI	2.78 (1.44)	New Zealand	4.121 (2.29)*	4.78 (2.3)	Belgium	1

Note: Z-statistics in parentheses.

Cross-validation

Table C2. Five-year data, out of sample forecast errors by country. Reduced model in first differences (OLS, no time effects in the model)

Country	Mean Absolute Forecast Error (%)
Australia	1.47
Austria	0.33
Belgium	1.85
Canada	1.48
Denmark	1.87
Finland	1.64
France	1.40
Germany	1.13
Ireland	2.53
Italy	1.08
Japan	0.86
Netherlands	1.91
Norway	2.07
New Zealand	1.00
Sweden	2.05
Switzerland	0.76
UK	1.77
USA	1.99