Institutional Determinants of Unemployment in OECD Countries:
Does the Deregulatory View Hold Water?

Lucio Baccaro

MIT

Diego Rei

ILO

Address correspondence to Lucio Baccaro:
baccaro@mit.edu

November 2006

Forthcoming in *International Organization*, Fall 2007

Institutional Determinants of Unemployment in OECD Countries:
Does the Deregulatory View Hold Water?

Lucio Baccaro
Diego Rei

abstract

The view that unemployment is caused by labor market rigidities and should be addressed through

systematic institutional deregulation has gained broad currency and has been embraced by national and

international policy-making agencies alike. It is unclear, however, whether there really is robust empirical

support for such conclusions. This paper engages in an econometric analysis comparing several estimators

and specifications. It does not find much robust evidence either of labor market institutions' direct effects on

unemployment rate, or of a more indirect impact through the magnitude of adverse shocks. At the same time,

it finds little support for the opposite, pro-regulatory position as well: the estimates show a robust positive

relationship between union density and unemployment rates; also, there is no robust evidence that the within-

country variation of bargaining coordination is associated with lower unemployment (as frequently argued),

nor is it clear that bargaining coordination moderates the impact of other institutions. All in all, restrictive

monetary policies enacted from an independent central bank and other determinants of real interest rates

appear to play a more important role in explaining unemployment than institutional factors.

Institutional Determinants of Unemployment in OECD Countries:
Does the Deregulatory View Hold Water?

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 labor market

institutions and should be addressed through systematic institutional deregulation.[1] The policy implications

are succinctly summarized in one of the IMF's policy papers: "Countries with high unemployment [are]

urged to undertake comprehensive structural reforms to reduce 'labor 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."[2] Is the empirical evidence

really supportive of such strong policy conclusions?

To address this question, we conduct a time-series cross-section (TSCS) analysis of 18 OECD

countries between 1960 and 1998. We pay considerable attention to the statistical properties of models, and

this leads us to compare several estimators and specifications. Our preferred model, which we arrive at by

testing down, estimates the direct effect of institutions with data averaged over five-year periods. It includes

only one macroeconomic control (the interest rate), six institutional variables: employment protection,

unionization rate, a measure of generosity of unemployment benefits, tax wedge, central bank independence,

as well as wage coordination, and no interactions. Such a parsimonious model, which we consider both in

levels and first differences, gives changes in labor market institutions a fair chance to explain changes in

unemployment. Yet little support for the deregulatory view emerges from the analysis: not just employment

protection, but also, and more surprisingly, the generosity of unemployment benefits and the size of the tax

wedge do not seem to be associated with higher unemployment. We also test for possible indirect effects of

---

[1] See Siebert, 1997; Saint Paul, 2004.

[2] IMF, 2003, 125.

institutions on unemployment, operating by magnifying the size of adverse external shocks,[3] and find similarly inconclusive results.

Our analysis also fails to provide empirical support for the opposite view that labor market institutions are good for unemployment. Indeed, our models display a robust (albeit small) positive association between unemployment and the union density rate. We also find that, focusing on the within-country variation, bargaining coordination does not reduce unemployment (despite frequent claims to the contrary), even though it does seem to be associated with lower real wage growth and to increase the responsiveness of real wages to unemployment. In addition, wage coordination does not seem to modify the impact of other institutions in any robust way. Overall, our findings suggest that the impact of labor market institutions on unemployment is, for the most part, indeterminate. Their effect is probably dependent on country-specific institutional configurations. There seems to be no one best way to organize and govern the labor market. Restrictive monetary policies implemented by an independent central bank and other determinants of high real interest rates appear to be more robustly associated with greater unemployment than institutions.

The remainder of the paper is organized as follows: the next section reviews the theory and evidence on the relationship between labor market institutions and unemployment. Section three introduces models and data. Section four lays out a set of hypotheses on the impact of labor market institutions, based on the available literature. Section five presents the results of an econometric analysis testing the direct impact of institutions on unemployment. Section six discusses the findings and their implications for the desirability of labor market deregulation. Section seven tests for possible non-linear effects of institutions on the magnitude of adverse shocks, as well as for the impact of institutions on wage growth. Section eight concludes with an overall summary of the evidence.

2. Labor Market Institutions and Unemployment

---

[3] Blanchard and Wolfers, 2000.

If one looks at the evolution of unemployment in the US, on the one side, and the major continental European countries, France, Germany, and Italy, on the other, the differences stand out immediately (Figure 1). In the US, unemployment starts off at a higher level than in the other countries in the first half of the 1960s (and well above the OECD median). It falls briefly in the second half of the 1960s, and then grows steadily until the early 1980s, when it peaks. From then on, it declines continuously throughout the late 1990s, returning to lower levels than at the beginning of the period. In the continental countries, too, unemployment grows sharply in the 1970s. Rather than peaking and declining, however, it continues to grow well beyond the early 1980s and is at the end of the 1990s not only higher than the US rate, but also higher than the OECD median[4]. This contrast between American dynamism and European sluggishness has inspired a series of commentaries, both in the popular press and in the scholarly literature (beginning with the OECD Job Study of 1994), focusing on Europe's labor market rigidities – particularly high trade union density and collective bargaining coverage rates keeping wages above market-clearing levels; strict employment protection regulation limiting the employers' ability to hire and fire at will; and generous unemployment systems reducing the incentives for the unemployed to bid down the wages of those currently employed. Implicit and sometimes explicit in these analyses is the idea that reforming European labor markets to levels of regulation comparable to the US would produce sizeable reductions in the unemployment rate.

Figure 1 about here

Even at first blush, however, there are numerous problems with this diagnosis. First, if it were true that unemployment is determined by labor market rigidities, one would expect the evolution of US labor market institutions to match the path of unemployment in that country.[5] Thanks to data collected by the

---

[4] Howell et al., 2006, Fig. 1, 56.

[5] We use the term "institution" somewhat imprecisely in this paper as a shortcut for non-demand and supply factors impinging on the labor market. However, the label may not be appropriate for some of the factors we consider. For

OECD and other researchers,[6] it is now possible to test these and other related hypotheses. It seems that the trajectory of US labor market institutions does not follow the same path as unemployment, with the exception, perhaps, of the unemployment benefit replacement rate. This increases from the late 1960s to the early 1980s and then declines, more or less as unemployment does. Other institutions (e.g. the level of employment protection and of collective bargaining coordination) do not change at all over the period. Still others follow their own path, rather uncorrelated with unemployment. For example, union density begins to fall in the mid-1950s, continues to decline in the 1970s, when unemployment was increasing, but then keeps declining well past the 1980s, when unemployment was also decreasing.

Second, the evolution of labor market institutions in continental European countries also fails to match the trajectory of unemployment. Indeed, as argued by Solow, "there are good empirical reasons for rejecting this convenient belief that the labor market *by itself* provides an adequate account of the sad story of European unemployment. At the crudest level, the timing is wrong. One of the two big increases in unemployment took place in the early eighties, although there was no change in labor-market regulation to account for it … The large continental economies do not seem to have suffered from noticeably more rigid labor markets during the high-unemployment 1980s than they did in the low-unemployment 1970s."[7]

Third, it would be unwise to generalize from the experience of the largest continental countries to Europe as a whole. Some European countries <u>did</u> manage to bring back down their unemployment rates. Yet the labor market in these same countries is overall considerably more regulated than in the US.[8] For the labor market rigidity thesis to hold water, the decline in the unemployment rates should be consistently

---

example, benefit replacement rates and tax rates are probably more accurately characterized as the result of policies; and union density rates are the result of social processes.

[6] Nickell and Nunziata, Baker et al., and others, see the Appendix.

[7] Solow, 2000, 4-5 (emphasis the author's). According to Solow, alternative explanations are to be sought, having to do with the low rate of job creation (potentially caused by the rigid employment protection, but also by limited product market competition) and with the more demand-friendly US macroeconomic policies.

accompanied (or, even better, preceded) by reductions in institutional protections in these countries. This does not seem to be the case. Indeed, if one looks at the four of the most successful European countries as far as unemployment reduction is concerned, Ireland, Denmark, the Netherlands, and the UK, the lack of correspondence is quite clear (Figure 2).

Figure 2 about here

In all these countries, the unemployment rate increased in the 1970s, began declining in the second half of the 1980s (later in Ireland), and continued to decline in the 1990s. Yet the behavior of labor market institutions was far from univocal: employment protection declined in two countries (in Denmark and, slightly, in the Netherlands) but increased in the other two (Ireland and the UK). Unemployment benefit replacement rates declined in Denmark,[9] Ireland, and the UK, but increased in the Netherlands.[10] Far from decreasing, unemployment benefit duration increased considerably in Ireland and Denmark, and slightly in the UK. The tax wedge also increased in Ireland and Denmark. The one institutional variable whose trajectory seems to match unemployment is union density, which decreases in all countries from the second half of the 1970s-early half of the 1980s on (in Denmark only slightly) at the same time as unemployment starts coming down. Interestingly enough, this same conclusion emerges from our multivariate analysis, illustrated below, which identifies unionization as the only robust institutional predictor of unemployment.

An additional reason to mistrust the rigidity thesis is that it is not clear that similar institutional reforms have comparable effects in different countries. For example, a marked increase in collective bargaining coordination, bringing about wage moderation, is often regarded as key for the employment

---

[8] See Pontusson, 2005; Howell et al. 2006.

[9] Apparently, the most relevant aspect of reform of the unemployment insurance system in Denmark had to do with tightening of the eligibility criteria (Nickell et al. 2005, 4).

[10] The major reform of the unemployment insurance system in the Netherlands involved the tightening of eligibility criteria (Saint Paul 2004, 52).

successes of Ireland[11] and the Netherlands.[12] However, collective bargaining coordination also increased in Italy in the 1990s.[13] Yet Italian unemployment performance continued to be dismal, unlike the other two countries. In addition, case study evidence casts doubt on the argument that different levels of institutional rigidities matter for differences in unemployment. Schettkat, for example, compares employment-miracle Holland with laggard Germany and shows that, even after the various reforms, labor market "regulations [we]re stricter and transfers more generous in the Netherlands than in Germany."[14]

Considering the heterogeneous experience of countries, it is not particularly surprising that the results of econometric tests using time-series cross-section data on OECD countries appear not especially robust. Dean Baker, Andrew Glyn, David Howell, and John Schmitt have been tracking the evolution of the literature in this domain for quite some time.[15] A recent article of theirs provides un up-to-date comparison of findings from 11 econometric studies between 1997 and 2005,[16] all regressing the unemployment rate on essentially the same set of institutional variables (an employment protection index, unemployment benefit replacement rates, union density, a bargaining coordination index, the magnitude of the tax wedge, and less frequently a measure of unemployment benefit duration, collective bargaining coverage, and expenditures in active labor market policies) using time-series cross-section data on OECD countries. Their review shows that no single institutional variable is consistently found to be significantly different from zero across all studies. To be sure, most studies do find that labor market rigidities are important determinants of

---

[11] Baccaro and Simoni, 2007.

[12] Visser and Hemerijck, 1997.

[13] Baccaro, 2000. Between the second half of the 1980s and the second half of the 1990s, the index measuring bargaining coordination increased from 2 to 4 in Italy (on a five-point scale), from 2.3 to 4 in Ireland, and from 3 to 4 in the Netherlands.

[14] Schettkat, 2003, 773.

[15] See Baker et al. 2003; 2005.

[16] Howell et al. 2006, 19-28, and Table 3, 68.

unemployment,[17] but they disagree as to exactly which institutions matter. Also, the size of estimated impacts varies considerably across specifications.

In brief, both case study evidence and econometric testing suggest that the case for the rigidities-cause-unemployment thesis is probably less robust and unambiguous than it should be, particularly considering its wide popularity and its huge influence on policy-makers. Yet the current debate among mainstream economists is not whether or not changes in labor market institutions explain movements in unemployment. This is more or less taken for granted.[18] The question that is being asked is instead exactly through which channels labor market institutions impact unemployment, whether directly, by pushing up the equilibrium unemployment rate,[19] or indirectly, by magnifying the adverse consequences of exogenous shocks, as argued by Blanchard and Wolfers.[20]

Within the recent literature, the work of Stephen Nickell and co-authors carries particular weight, having inspired several other authors as well. Nickell et al., 2005, conduct a time-series cross-section analysis of unemployment patterns in 20 OECD countries between 1961 and 1995 using annual data. The unemployment rate is regressed on: 1) its own first lag; 2) a vector of labor market institutions (employment protection, benefit replacement rate, benefit duration, the change in union density, bargaining coordination, and the tax wedge); 3) a vector of institutional interactions (benefit replacement × benefit duration;

---

[17] See particularly Nickell, 1997; Elmeskov et al, 1998; IMF, 2003; Nickell et al., 2005. One exception is Baker et al, 2005.

[18] Indeed, Saint Paul has argued in a recent review of the literature (2004, 53) that "evidence supports the traditional view that rigidities that reduce competition in labor markets are typically responsible for high unemployment. Reducing these rigidities across the board seems to work." Freeman, 2005, has recently argued that economists' priors concerning the role of labor market rigidities in unemployment are so strongly held that they are unlikely to be changed simply by the results of time-series cross-section econometric testing, and that different, more micro data and analyses are needed.

[19] See Nickell, 1997; Elmeskov et al, 1998; IMF, 2003; Belot and Van Ours, 2004, Nickell et al., 2005.

[20] Blanchard and Wolfers, 2000; see also Blanchard, 1999.

coordination $\times$ union density; coordination $\times$ tax wedge); 4) a vector of macroeconomic controls (labor demand shock, total factor productivity shock , terms of trade shock, money supply shock, plus the real interest rate); 5) country and time effects; and 6) country-specific time trends.

The authors find that for all institutional predictors except employment protection the null hypothesis of zero coefficient is rejected, by ample margins in most cases.[21] Changes in institutions alone are found to explain 55 percent of the variation in unemployment, the generosity of the unemployment benefit system being the most important factor, followed by taxes and union density.[22] The authors also test the plausibility of the Blanchard and Wolfers's argument,[23] by estimating another model in which institutions are allowed to have not just direct effects on unemployment but also to modify the coefficients of time dummies capturing external shocks.[24] They find that institutions remain significant predictors of unemployment in this alternative specification, while the interacted time effects make no contribution to the explanation of unemployment once direct effects are controlled for.

The next section examines whether these strong conclusions are robust to small changes in data and specifications. In light of the findings of Nickell et al.,[25] summarized above, we devote most attention to testing the direct effects of institutions on unemployment rates. Later in the paper, however, we also examine the plausibility of interactive effects of institutions on the size of adverse macro shocks.

3. Models and Data

We make no attempt at modeling unemployment in an innovative way. Instead, we rely on existing models, whose empirical testing has been found to provide strong support for the thesis that institutional

---

[21] Nickell et al., 2005, Tab. 5, 14, col. 1.

[22] Nickell et al., 2005, 21.

[23] Blanchard and Wolfers, 2000.

[24] Nickell et al., 2005, Tab. 8, 21, col. 2.

[25] Nickell et al., 2005.

rigidities cause unemployment. In particular we draw heavily on IMF, 2003, which is, in turn, a modification of Nickell et al., 2001 – itself an early version of Nickell et a, 2005l.[26] The model we test hypothesizes that the unemployment rate depends on a series of labor market institutions determining the equilibrium level, and on a series of macroeconomic variables explaining short terms deviations from the equilibrium level.[27]

In static form, the model we use 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) fixed effects, capturing country-specific, but time-invariants, unmeasured determinants of unemployment, the α_ts are (T-1) year dummies, accounting for time-varying annual shocks affecting all countries simultaneously, and $\varepsilon_{i,t}$ is the stochastic residual. With yearly data we estimate dynamic models, namely 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 sometimes estimated in first differences, with first differencing wiping out the country fixed effects. The vector of institutional variables is the following:

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

---

[26] We focus on IMF (2003) because this allows us to use data for three additional years (1996-98). However, in Table 1, columns seven and eight, using a shorter panel, we also estimate models in which the macroeconomic controls are the same as in Nickell et al., 2001; 2005.

[27] See Layard et al., 1991; Nickell et al, 2001; see also Blanchard, 1999, 9; Nickell et al., 2001, 2-4.

where *EP* is an employment protection index, *BRR* is the benefit replacement rate,[28] *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. 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 labor productivity,[29] *DCPI* is the change in the consumer price index, and *TOTS* is the terms of trade shock measure. Finally, every institutional variable is interacted with the bargaining coordination measure:

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

Also, every interactive term is expressed as deviation from the sample average ψ.[30] This allows us to interpret the coefficient of each institutional variable as the coefficient of the hypothetical country

---

[28] The benefit replacement variable should be combined with a *benefit duration* variable (as well as with other variables capturing which percentage of the workforce is eligible for benefits and how tight the rules of access are – measures which, to our knowledge, are presently unavailable for many countries). However, the benefit duration variable available in our data set is the one elaborated and used by Baker et al. 2003. Following Nickell and Nunziata, 2001, they constructed the series proxying the duration of unemployment benefits with the ratio of benefits available after the first year to benefits available in the first year of unemployment. The drawback of this methodology is that the variable assumes the value of zero whenever there are no benefits available after the first year of unemployment, and hence does not capture the shorter lengths. For this reason we omit the benefit duration variable and use instead the OECD summary measure of benefit entitlements (BENOECD) in some specifications. This averages the gross unemployment benefit replacement rates for two earnings levels, three family situations, and three durations of unemployment. The advantage of this variable is that it considers both the dimension of duration and the dimension of income replacement. Its disadvantage is that it does not distinguish between the two. In a related paper (Baccaro and Rei, 2005), we also ran estimates that included benefit duration variables and their interactions with the wage coordination variable, and found that results did not vary much from the ones presented here.

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

characterized by the average level of a given institutional measure (in this case the interaction term equals zero):

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

While our specifications are similar to others in the literature,[31] they also present a few peculiarities. First, unlike Nickell et al., 2001, 2005, Nunziata, 2001, and IMF, 2003 , we do not include country-specific time trends in the model.[32] Inserting country-specific trends alters the purpose of the exercise from explaining how institutions affect unemployment to explaining how institutions affect movements in unemployment around a (country-specific) time trend. Also, country-specific time trends introduce an inordinate amount of multicollinearity, and this may render the estimates highly sensitive to the particular specification selected.

Second, unlike other papers, which contain a rather eclectic array of interactions,[33] we focus on the interaction between the degree of wage coordination in the economy and the institutional variables. This modeling choice is in line with a basic intuition of the variety of capitalism literature,[34] 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.

Our main interest is to see whether the η's (the coefficients of the institutional variables) are significantly different from zero across the 18 OECD countries included in our analysis and over the entire period (1960-1998). We do not explore parameter heterogeneity over time. As far as cross-sectional heterogeneity is concerned, we do not allow for country-specific parameters but do let the effects of

---

[30] See Nunziata, 2002, 8.

[31] Specifically, IMF, 2003; Nickell et al, 2001, Tab. 13, Col. 1, 37.

[32] See Baker et al., 2003, 15, for a similar choice.

[33] One exception is Belot and Van Ours (2004), who provide an explicit rationale for their choice of interaction terms.

[34] See Hall and Soskice, 2001.

institutions to vary systematically across bargaining regimes. As is common practice in this literature, we

assume exogeneity of the predictors. As for data, we rely on the time-series cross-sectional dataset

assembled by Baker et al.,[35] which, in turn, is very similar to the one used by the IMF.[36] Both datasets are

extensions of the Nickell/Nunziata database of labor market institutions,[37] which, in turn, relies on OECD

measures. We use, however, a different, and arguably better, measure of wage bargaining coordination,

elaborated by Lane Kenworthy.[38] Because Spain and Portugal were not democratic for a large part of this

period, these two countries were not coded by Kenworthy and are therefore excluded from our sample.

Information on the various measures can be found in the Appendix.

4. Specific Hypotheses

Research on specific labor market institutions has shown that the channels through which these may

impact unemployment are multiple and possibly contradictory. For example, the effects of *employment

protection (EP)* legislation have been found to be theoretically and empirically ambiguous.[39] Employment

protection simultaneously reduces both flows from unemployment into employment and flows from

employment into unemployment.[40] A portion of the literature also underscores the possible positive effects

of employment protection legislation on worker productivity, by providing for greater job stability and

---

[35] Baker et al., 2003.

[36] 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, and

United States.

[37] Nickell and Nunziata, 2001.

[38] See Kenworthy, 2003.

[39] See Bertola, 1990; OECD, 1999, 2004a.

[40] See Nickell, 1997, 66; Blanchard, 1999, 10; Bertola et al., 2001, 30.

employee morale, such that wage increases possibly caused by employment protection may finance

themselves through increased productivity.[41]

The *union density (UD)* measure is intended to capture union bargaining power.[42] Unionization may

affect unemployment through two channels: higher average wages or a more compressed wage structure. If

unionization leads to labor 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

labor supply.[43] In addition, if unionization leads to a more compressed wage structure,[44] some workers, those

with lower productivity, are likely to be priced out of the labor market. Freeman and Medoff, 1984, argued

that unionization has two faces: a labor monopoly face and a "voice" face. The latter compensates for the

former by increasing workplace productivity. If this view is correct, then the effects of unionization may be

indeterminate and depend on which of the two effects prevails. It has also been argued that when collective

bargaining is coordinated, unions tend to internalize the externalities associated with their wage policies.[45]

For this reason we expect the interaction between unionization rate and bargaining coordination to be

negative.

The *benefit replacement rate (BRR)* variable expresses the percentage of gross earning replaced by

unemployed benefits in the first year of unemployment and, in so doing, seeks to capture the degree of

generosity of the unemployment insurance system. More generous insurance systems may cause

unemployment to rise through multiple channels,[46] for example by making "unemployment less painful and

---

[41] See Nesporova and Cazes, 2003, 87, relying on the findings of Ichniowski et al., 1995, and Nickell and Layard, 1999.

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

[43] E.g., for women, youth, and old workers, see Bertola et al., 2001.

[44] See Fortin and Lemieux, 1997; Pontusson et al., 2002; Rueda and Pontusson, 2000.

[45] Soskice, 1990; Nickell, 1997, 68.

[46] See Holmlund, 1997, for a general review.

thus strengthen[ing] the hand of workers in bargaining",[47] or by "reduce[ing] the 'effectiveness' of unemployed individuals as potential fillers of vacancies, by allowing them to be more choosy".[48] The findings of Feldstein and Poterba,[49] and Katz and Meyer,[50] based on US micro data, suggest that the generosity and duration of unemployment benefits increases the reservation wage and the duration of unemployment, respectively. At the same time, 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 (TW)* variable is the sum of the payroll, income, and consumption tax rates. Clearly, the tax wedge is potentially an additional cost for enterprises. However, the impact of this variable on unemployment depends on who shoulders the burden of these taxes,[51] 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 labor demand should be unaffected. The net impact would then depend on what happens to labor supply. If workers increase their 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 labor demand is likely to be negatively affected and unemployment likely to increase.

The *wage bargaining coordination (BC)* variable is generally hypothesized to be associated with lower unemployment, because of the tendency of coordinated bargaining to internalize the externalities of

---

[47] Blanchard, 1999, 12.

[48] Nickell, 1997, 67.

[49] Feldstein and Poterba, 1984.

[50] Katz and Meyer, 1990.

[51] Nickell, 1997, 69.

wage bargaining and lead to lower real wage settlements than uncoordinated bargaining.[52] However, a positive relationship between bargaining coordination and unemployment is also plausible if one considers that coordination potentially enhances the monopoly power of unions.[53] A recent OECD analysis finds no robust association between wage coordination and unemployment.[54]

The *central bank independence (CBI)* index is intended to capture the degree to which the monetary authority is able to resist political pressures to inflate the economy. It is not clear what kind of impact this variable should have on unemployment when considered in isolation. In a rational expectation framework, for example, central bank independence does not directly impact employment or unemployment, but reduces inflation.[55] 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 behavior accordingly, than actors in an uncoordinated bargaining system.[56] Based on this reasoning, the interaction between central bank independence and wage bargaining coordination should be negatively associated with unemployment, other things being equal.

With regard to the macroeconomic controls, we expect the *real interest rate* to be positively associated with unemployment, mostly because high interest rates reduce capital accumulation (at least until wages adjust downwards, and an increase in the profit rates compensates for the greater cost of capital). According to Blanchard, the effect of interest rates should be limited to the short-term, and should be small in the long run.[57] Ball, however, argues that protracted periods of restrictive monetary policies, with high

---

[52] See Tarantelli, 1986; Soskice, 1990; Flanagan, 1999, 1157ff.; Hall and Franzese, 1998; Franzese, 2001, 112-120.

[53] See Saint-Paul, 2004, 51; Traxler and Kittel, 2000, 1156.

[54] OECD, 2004b.

[55] Bleney, 1996; see also Eijfingeer and De Haan, 1996, for a general overview.

[56] See Hall and Franzese, 1998; Soskice and Iversen, 2000; Cuckierman and Lippi, 1998; Iversen, 1998.

[57] Blanchard, 1999, 3.

real interest rates, do not just increase current unemployment, but end up increasing equilibrium

unemployment as well.[58] Nickell et al. point out that real interest rates may positively affect unemployment

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 labor.[59]

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.[60] In line with most macroeconomic theory, we expect this effect to hold in

the short- but not in the medium- or long-term.

The *terms of trade shock* variable, defined as first log-difference of the terms of trade multiplied by

trade openness (in turn defined as the ratio between imports plus exports to GDP), 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 should rise.[61] The

duration of this effect depends of the speed of adjustment and is likely to disappear when longer time frames

are considered.

The *change in labor productivity* variable is also expected to have a negative sign due to real wage

resistance. If the rate of productivity growth suddenly decelerates, and workers continue to obtain similar

rates of growth in real wages as in the past, unemployment should rise. As argued by Bertola et al.,[62] "in the

long run, there is no reason for unemployment to be affected by the particular level of [productivity] growth

---

[58] Ball, 1999.

[59] Nickell et al. 2001, 3.

[60] See Baker et al. 2005; Belot and Van Ours, 2004; Nickell, 1997.

[61] Nickell et al., 2001, 5

[62] Bertola et al., 2001, 17.

a country has settled upon, but it may take a long time for real wage growth to decelerate to its new equilibrium level".[63]

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.[64] To the extent that a more coordinated bargaining system helps economic actors internalize the systemic consequences of their actions, the sign of these interactive terms should be negative.

5. Econometric Analysis[65]

5.1. Annual Data

We begin the econometric analysis by estimating dynamic fixed effects models using annual data.[66] This is the kind of models from which issue some of the strongest conclusions about the desirability of deregulation.[67]

Table 1 about here

---

[63] See also Layard et al., 1991

[64] Hall and Soskice, 2001.

[65] All analyses were performed with StataSE 8.0., except for the cointegration tests, for which Eviews 4.1 was used.

[66] In a dynamic model the lagged dependent variable is included in the regressors. Hence 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.

[67] See IMF, 2003, Nickell et al., 2001; 2005; Nunziata, 2001; Elmeskov et al, 1998. Based on Beck and Katz's, 2004, and others' advice (see Nunziata, 2001, 11; Judson and Owen, 1999), we ignore the possible bias due to the correlation between the (demeaned) lagged dependent variable and the (demeaned) error term in the fixed effect estimator, known as Nickell bias (Nickell, 1981; Kiviet, 1995; Baltagi, 2001, 130). Monte Carlo evidence reported in Beck and Katz, 2004, 33-4, suggests that, with T=39, the fixed effects estimator should perform as well as others, if not better.

Column one in Table 1 reports the results of OLS estimation. Columns two and three model country-specific heteroskedasticity and residual serial correlation (which persists even in the dynamic specification) by using a Panel Weighted Least Squares estimator with a Prais-Winsten transformation of the data. Model two uses a common estimated rho (first-order autocorrelation coefficient), as recommended by Beck and Katz;[68] model four a country-specific one, as in Nickell et al.[69] Column four estimates the same model as column three with the summary measure of benefit entitlements elaborated by the OECD instead of the benefit replacement rate variable. Column five uses OLS with panel corrected standard errors (PCSEs).[70] Column six estimates the same model as in column five with the OECD benefit entitlements variable in lieu of the benefit replacement rate variable. In columns seven (PWLS with Prais-Winsten transformation) and eight (OLS/PCSE) we replace the macroeconomic control variables in our specification with the macro control variables used by Nickell et al.[71] (labor 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, these models should attribute a greater role to the institutional variables in explaining equilibrium unemployment than the previous. Since the data in the Nickell et al.'s database run until 1995, this model is estimated on a shorter panel.

---

[68] Beck and Katz, 1995, 640.

[69] Nickell et al., 2001; 2005. It should be noted that, just like other regression coefficients, the estimated rho, too, could be biased in a dynamic model with serial correlation.

[70] The PCSEs correct for country-specific heteroskedasticity and cross-country correlation of the errors – both typical features of TSCS datasets – thus providing more reliable estimates of the standard errors (Beck and Katz, 1995 and 1996).

[71] Nickell et al. 2001; 2005.

The coefficients of the lagged dependent variable are all worryingly very high (close to 0.9 in some cases). Test results on stationarity show that the series are for the most part non-stationary.[72] However, the cointegration tests at the bottom of the table reject the null of non-cointegration.[73] Our tests show that data transformations do not eliminate serial correlation in the residuals, which remains non-negligible in all models (estimated $\rho \cong .3$). Due to the presence of serial correlation, we cannot exclude that the estimates in these models are not both biased and inconsistent. Monte Carlo evidence suggests that the magnitude of the bias may not be great.[74] However, because they are conducted with N much larger than ours, the experiments do not match the specific features of our dataset.

Ignoring the possible bias, the results reported in Table 1 suggest that unemployment clearly depends on macroeconomic conditions. All macroeconomic predictors are significantly associated with unemployment, even though the terms of trade variable is statistically insignificant when OLS is used. The

---

[72] See the Appendix 2 in Baccaro and Rei, 2005.

[73] The test checks for stationarity of the regression residuals. Drawing on Nunziata , 2001, 12-3, we used the test for co-integration proposed by Maddala and Wu (1999), which is suitable for an unbalanced panel like ours. This test combines the results of country-specific tests with p-value p_i, in the statistic: $-2(\sum_{i} \log p_i)$, which is χ^2 distributed with 2N degrees of freedom, under the null of non co-integration (see Smith and Fuertes, 2004, 35-6). To perform the country-specific tests, we adopted the Augmented Dickey Fuller and Philips Perron tests. For the ADF, the appropriate country-specific 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 PP test the Newey West bandwidth was selected using the Bartlett Kernel. The p-values are Mackinnon approximations. The Maddala and Wu (1999) test assumes that there is no cross-sectional correlation of the errors. Because we control for cross-country correlation through the insertion of time dummies, we assume (somewhat incorrectly) that the test statistics follows the χ^2 . Concerns about the critical values are likely to arise only in case the margin of acceptance/rejection is thin (which is not our case).

[74] See Gaduh, 2002.

effects of institutional variables are, instead, much less clear-cut. Only union density is robustly positive and significant in all models. Central bank independence is positively correlated with unemployment, but often insignificant when OLS is used. Bargaining coordination is found to be positively, not negatively, associated to unemployment, and sometimes significantly so. The only robust interaction seems to be that between unionization and wage coordination, which is negative as expected.[75] It is not possible to reject the hypothesis that the interaction terms are jointly equal to zero. The models in columns seven and eight, which seek to approximate the Nickell et al.'s specification,[76] confirm the results of previous models. However, the coefficient of the real interest rate variable becomes smaller with the new macro controls in place and insignificant with OLS. Both the coefficient and the z statistics for central bank independence are greater than in other models. In addition, benefit replacement emerges as negatively correlated with unemployment (although insignificantly) in the FGLS specification, and positively (and weakly significant) in the OLS/PCSE one. In the FGLS model two new interactions emerge, the one between coordination and tax wedge (negative) and the one between coordination and employment protection (positive).[77]

5.2. Five-Year Data

---

[75] This conclusion also holds when interaction terms are entered one at a time.

[76] Nickell et al., 2001; 2005.

[77] We also estimated a battery of models in first differences (not shown here). This allowed us to deal with the problem of remaining serial correlation in the residuals and associated bias in a dynamic model. In addition, differencing served another important purpose: since our series are integrated of order 1, it allowed us to ignore the question whether or not the variables in levels are co-integrated or not. In this regard, Kittel and Winner (2001, footnote 10, p. 22) observe, based on Maddala and Wu (1999), that "time series with $T \cong 30$ are too short for the estimation of reliable parameters in the co-integrating framework." The results were in line with those from models in levels. Among the institutional variables, only unionization was both positive and significant across models. Its impact seemed to be partially counterbalanced by its interaction with the coordination variable.

The next step in the analysis is to estimate models with data averaged over 5-year intervals.[78] Since institutions vary little over time, an analysis with averaged data should be preferable to one with yearly data. Also, five-year aggregates should be more appropriate than annual data for indicators like the employment protection index, which is based on interpolation from a few observations.[79] In addition, averaging should mop out the effects of business cycles on unemployment, thus leading to more reliable causal interpretations. Moreover, it 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 capture them by averaging the data.

Table 2 about here

In columns one and two in Table 2 , we estimate a static fixed effects model, using two alternative methods: one is OLS with the Newey-West robust standard errors, which correct for both first order autocorrelation and panel heteroscedasticity; the other is PWLS with a Prais-Winsten transformation, modeling both heteroskedasticity and serial correlation in the residuals.[80] The remaining columns in Table 2 exclude, for the sake of greater efficiency, first the interaction variables in columns three and four, and then the macroeconomic variables that do not appear to be significant according to a Wald test, that is, all except the real interest rate (columns five and six). Columns seven and eight estimate the same models as in the previous two columns, by using the OECD benefit entitlements measure instead of the benefit replacement rate. We also estimated a dynamic model (not reported here). With T=8, this suffers from Nickell bias, which

---

[78] As in, for example, Daveri and Tabellini, 2000; Nickell, 1997; Baker et al., 2005.

[79] See Baker et al., 2003, 6 and ft. 4 as well as the Data Appendix to this paper.

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

leads to underestimation of the lagged dependent variable,[81] and possibly overestimation of the coefficients of the exogenous variables.[82] Despite the negative bias, the coefficient of the lagged dependent variable was very high (0.94), as well as highly significant. Unfortunately, tests of (non-) stationarity and cointegration assume a T much larger than ours,[83] or, with small T, a larger N than ours,[84] so they would not make much sense in this context. We interpret the significance of the lagged dependent variable as a sign that the static model with five-year data is underspecified, and its high value as a warning that we are likely to have a problem of non-stationarity and, linked to that, possibly a danger of spurious regressions.

Ignoring these statistical problems, the results are similar across estimators and specifications. Among the macroeconomic variables, only the real interest rate is significant and signed according to prediction (i.e., positive). The other macroeconomic predictors are mostly positive rather than negative. All are, however, insignificant. Among the institutional variables, only union density and the central bank independence index are robustly different from zero. The interactions terms are individually and jointly insignificant,[85] except the interaction between coordination and employment protection, which is positive and significant at the 10 percent level with FGLS (but whose sign appears to jump depending on specification).[86]

Table 3 about here

Table 3 compares fixed effects and random effects specifications. We chose fixed effects on both theoretical and methodological grounds: the model is better specified when country dummies are inserted. Indeed, country dummies seem to capture a large share of the variation in the unemployment rate. In a

---

[81] Hsiao, 2004, 71-2.

[82] See Judson and Owen, 1999, 12.

[83] See Smith and Fuertes, 2004, 51.

[84] Binder et al., 2000.

[85] This conclusion also holds if the interaction terms are entered one by one in the specification.

[86] See Tab. 8 in Baccaro and Rei, 2005.

random-effects model it is assumed that the countries are random draws from a population, about which inferences are being made – an assumption that does not seem especially realistic in this case. We tested for fixed vs. random effects through a Hausman test.[87] The random effect specification appears borderline acceptable for the model with the summary measure of benefit entitlements (column four). However, non-randomness of the sample and better specification still make one prefer the fixed effects model to the random effects one. 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. The greatest change concerns the coordination variable, which is negative and significant with random effects (consistent with most literature),[88] but not with fixed effects. Also, the magnitudes of the other institutional variables (except those of unemployment benefits) are considerably smaller with random effects.

Table 4 about here

Table 4 moves from models in levels to models in first differences.[89] The rationale behind this choice is the following: first, the fact that we cannot exclude that the data in levels are non-stationary and non-cointegrated suggests first differencing as a safety device. Second, a t-test on the lagged dependent variable (not reported here) shows that this should not be included in a model in first differences, unlike a model in levels. Therefore, a model in first differences is better specified than a model in levels. Third,

---

[87] In a Hausman test, if the null hypothesis cannot be rejected, then random effects are warranted since one can assume no correlation between the covariates and the error term.

[88] See the review in Aidt and Tzannatos, 2002.

[89] 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 fact that a Wald test on the insertion of country dummies in the first difference estimator (not reported) reveals that they are not jointly significant signals that country-specific time-trends are not warranted in our models.

differencing provides a solution to the problem of serial correlation of the error (we reject the null at the five percent level in all cases). Finally, with first differences the ratio between parameters and observations is much lower than in levels, because first differencing wipes out the fixed effects. Therefore, coefficient estimates are probably more precise.[90] The major drawback of first differencing is that we risk exacerbating the problem of measurement error, which may be less severe in the levels equation.[91]

We present two sets of estimates: one is PWLS (heteroskedasticity-consistent), the other is OLS with White-robust standard errors. The coefficients have to be interpreted as the effect of changes in independent variables (averaged over five-year spans) on change in unemployment in the same period, controlling for other determinants, including time-specific shocks. This interpretation does not seem at odds with the basic policy question underlying this and other studies, namely understanding how average unemployment would change compared with the average of the previous five years if institutions were to change over the same period. Columns one and two include the full battery of macroeconomic controls and interactions. Columns three and four only include the real interest rate among macroeconomic controls and no interactions. Columns five and six estimate the same models as in the preceding two tables but with the OECD measure of benefit generosity instead of the benefit replacement rate. Columns seven and eight estimate an all-institution specification, omitting the real interest rate.

One would expect similar coefficient estimates from models in differences and levels. This is indeed the case with most variables (real interest rate, union density, benefit replacement rate, and tax wedge), but there are a few exceptions, as revealed by comparing Tables 2 and 4. Not surprisingly, variables based on indicators, which change little over time, 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. This variable is

---

[90] 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.

[91] See Arellano, 2003, 50.

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.

Similarly, wage coordination is often positive (albeit insignificant) with models in levels, while it is always negative in differences and significant with FGLS when only the interest rate is included as macroeconomic control. The countries in which the wage coordination index is time-unvarying and which do not participate in the determination of 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 may tilt the estimate in levels towards a positive sign. Interestingly enough, the other index, that of central bank independence (which is time-invariant for Australia, Austria, Canada Denmark, Germany, Sweden, and USA), has similar coefficients and standard errors in both levels and differences.

6. Discussion of Findings

Our strategy in the previous section has been to compare a number of different estimators as well as specifications. None of the models we have considered has been found to be exempt from statistical problems: by comparing them we have sought to ensure that our conclusions were robust across estimators and specifications. Our findings suggest that macroeconomic conditions (captured by real interest rates) and macroeconomic policies (specifically, restrictive monetary policies implemented by an independent central bank) do matter for unemployment. It is much less clear, instead, that the same can be said for labor market rigidities in block.

The real interest rate is almost always a positive and significant predictor of unemployment across models, especially with five-year data but often also with annual data. Our findings suggest that the depressing effect of real interest rates on unemployment, which is in all likelihood mediated by reduced

27

capital accumulation, are not just limited to the short-run, but also impact the medium-to-long term, and are, in this respect, a confirmation of Ball's argument that "determinants of aggregate demand have […] effects on long-run as well as short-run movements in unemployment."[92]

Unsurprisingly, 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, signaling 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 models with yearly data, possibly indicating the presence of short-run 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.

Among the institutional variables, the sign of the employment protection variable varies considerably across specifications and is generally positive in levels and negative (even weakly significant in some specifications) in first differences. The absence of a robust positive association with unemployment is in line with most theoretical arguments, according to which the impact of employment protection on the unemployment rate is indeterminate as employment protection reduces employment and unemployment flows simultaneously and these effects tend to cancel each other out. We attribute at least a portion of the shift in sign between levels and first differences to the influence of fixed effects when the measure, as is the case for some countries, is time-invariant.

In contrast to theoretical predictions, the benefit replacement rate variable is almost always negative (albeit often insignificant) with both annual and, especially, five-year data. It may be that if benefit replacement is a form of insurance,[93] the cost of such insurance is borne by workers through lower real wages. 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

---

[92] Ball, 1999, 189.

[93] See Agell, 1999; 2000.

jobs and worker skills when benefit replacement rates are higher.[94] The sign of a summary measure of benefit entitlements, including both the dimensions of income replacement and benefit duration, varies across models and is never significantly different from zero.

The tax wedge estimates are also surprising, since they are negative with both annual data and five-year data, and sometimes even significant. If the impact of the tax wedge depends on what portion of it is not paid for by workers through lower real wages and contributes, therefore, to increase the real labor cost faced by employers, then one may conclude that the tax wedge is, on average, entirely paid for by workers, controlling for other variables in the model. The negative sign may depend on the fact that lower take-home pay shifts the labor supply curve rightward, i.e., for given wage levels workers increase their labor 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.[95] The union density coefficient is normally 0.1 with five-year data (implying a one percent increase in unemployment for every 10 percent increase in unionization); the long-term coefficient with yearly data is slightly higher. 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 internalizes 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

---

[94] In a related paper (Baccaro and Rei, 2005, Tables 6, 7, 12), we also estimated specifications that included a benefit duration variable, whose sign appears unstable and dependent on the particular measure used to operationalize the construct.

[95] It bears noting, however, that we are assuming (as is common practice in this literature) exogeneity of the institutional predictors, and that such assumption may be unwarranted in the case of the union density rate, which is considered by the industrial relations literature to depend on unemployment among other things (see Checchi and Luifora, 2002; Goldfield, 1990, 102-3).

determinants. 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 of more than 4 percentage points on average. Interestingly enough, the effect of central bank independence is net of the effect of the real interest rate in our models. The two measures are weakly correlated (the correlation coefficients are around 0.14). We interpret the results as follows: the central bank independence index captures the more or less restrictive monetary policy stance of the country in the particular year. Its coefficient reflects the effect on unemployment of restrictive monetary policies: these lead to a temporary increase in unemployment, which then becomes permanent probably because some form of hysteresis intervenes, as argued by Ball.[96] The coefficient of the real interest rate variable captures instead those effects of the real interest rate on unemployment that do not depend on the particular stance of the central bank, but on other factors (e.g. perceived country risk or supply and demand factors). In this regard, it would be interesting to unpack the real interest rate variable in future research, and seek to determine what conditions or policies contribute to higher interest rates, and hence to unemployment.

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 modeling choice. If fixed effects are included in the model, either directly or indirectly by taking first differences, then this variable does not seem to have a significant impact on unemployment. If, however, fixed effects are not included (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 is likely to suffer from omitted variable bias. We conclude that the effects of wage coordination that seem to matter most for unemployment are the cross-sectional ones, while the within-country variation in wage coordination does not significantly reduce unemployment. Cross-sectional

---

[96] Ball, 1999.

differences probably reflect the rest of the institutional structure (e.g. social democracy and associated economic policies) in which wage coordination is often embedded. From a policy perspective, simply increasing the level of bargaining coordination, in the absence of parallel changes in the rest of the institutional and policy structure, would probably not reduce unemployment, according to our results.

Among the interaction variables, it seems that coordination increases, in the short-term, union capacity to internalize externalities.[97] However, our findings with yearly data provide do not support the thesis of Hall and Franzese and others that the employment-depressing effects of restrictive monetary policies enacted by an independent central bank decrease with greater coordination.[98] With five-year averages, it seems that virtually no interaction holds. This may imply that wage coordination mediates the impact of other institutions, at best, only in the short-term. This suggests that the hypothesized institutional differences between liberal and coordinated market economies may be less entrenched than argued by the variety of capitalisms view, at least as far as unemployment is concerned.[99]

7. Additional Tests of Institutional Effects

7.1. Non-Linear Model

Our analysis has failed to uncover a direct impact of labor market institutions on unemployment, except for unionization. However, as suggested by Blanchard,[100] and Blanchard and Wolfers,[101] rather than increasing unemployment directly, institutions may exacerbate the adverse effects of external shocks. Indeed, it is conceivable that once the unemployment rate is brought above the equilibrium level by adverse

---

[97] See Soskice, 1990; Nickell, 1997, 68.

[98] Hall and Franzese, 1998. Some support for this thesis emerges from models in first differences with yearly data (not shown here).

[99] Hall and Soskice, 2001.

[100] Blanchard, 1999.

[101] Blanchard and Wolfers, 2000.

external forces, it may be prevented to return to its (previous) equilibrium level by institutional rigidities hindering the adjustment process – interfering, for example, with real wage flexibility. In such case, unemployment could remain at high levels even several years after the shocks.

To text for this eventuality, we estimate essentially the same model as in Blanchard and Wolfers:[102]

$$u_{i,t} = \beta_0 + \phi_t(1 + \sum_j \gamma_j X_{j,it}) + \delta_i + \varepsilon_{i,t}$$

Unemployment in country i at time t is presented as a function of the same j institutional variables considered before (Employment Protection, Union Density, Benefit Replacement Rate, Tax Wedge, Central Bank Independence, and Wage Coordination), (n-1) country-specific fixed effects (δ_i), and (t) year dummies (φ_t). The coefficients of institutions (γ_j) expresses the linkage between the unobservable time shocks captured by the country-unvarying time dummies (φ_t) and the set of institutions (X_j). In this specification, the effects of institutions on unemployment are non-linear and are estimated through Non-Linear Least Squares. In line with our previous analysis (and consistent with Blanchard and Wolfers) we use data grouped in five-year averages.[103]

Our tests differ from Blanchard and Wolfers's in the following respects: 1) the institutional variables we use are not exactly the same, as their list also includes collective bargaining coverage, a measure of active labor market policies, and benefit length, but does not contain central bank independence; 2) the data we use

---

[102] Blanchard and Wolfers, 2000, Table 1, p. C20.

[103] This specification makes it impossible to understand exactly which shocks are at play and how they vary by country. However, this is acceptable to us as our focus is on institutions, not shocks. Blanchard and Wolfers make an attempt at modeling the shocks explicitly by including three macroeconomic variables: the real interest rate, the rate of total factor productivity growth, the change in labor demand (2000, Table 5, p. C28). They find, however, that country-invariant time dummies provide a better fit than observable measures of shocks.

are different, as our institutional measures are time-varying, unlike theirs;[104] 3) while Blanchard and Wolfers express their institutional measures as deviations from the sample means;[105] we compare results from data in levels and in deviations;[106] 4) since the assumption of i.i.d. errors is untenable with this data structure,[107] we use Rogers robust standard errors for hypothesis testing.[108] These correct for country-specific heteroskedasticity and within-country serial correlation.[109]

---

[104] Blanchard and Wolfers also estimate a model with time-varying institutional measures for benefit replacement rates and employment protection (2000, Table 3, p. C24) and find that results are generally stronger with time-unvarying institutions.

[105] Blanchard and Wolfers, 2000, C20.

[106] It has to be considered that with fixed effects (country and time) the data are already transformed in deviations from country and period means, so the choice to express them as deviations implies demeaning the variables even further.

[107] See Blanchard and Wolfers, 2000, footnote 21, p. C20.

[108] See Rogers, 1993.

[109] The standard errors in question are equivalent to the Huber/White heteroskedasticity consistent standard errors, adjusted to account for possible correlation within a cluster. They are consistent in the absence of cross-sectional correlation. This estimator has not been frequently used in small samples because of concerns about its performance. However, recent Monte-Carlo evidence by Kezde (2003, Table 2.2 and Table 2.5) shows that in a fixed effects contest with a serial correlation of about 0.3 (similar to our situation) its absolute performance in terms of efficiency is acceptable and the relative efficiency with respect to the White estimator (which corrects for heteroskedasticity only) depends on the degree of autocorrelation of the variable considered (the more sluggish the variable the better the cluster estimator). Also, Petersen (2005, Figures 3 to 5) shows (using, however, a sample where N is much bigger than in our case) that even with time and country dummies included, the performance of the Rogers standard errors is much better than the (Panel) Newey West standard errors and the Fama-MacBeth standard errors and that its bias with a small N is little. In brief, the experimental evidence seems to suggest the Rogers standard errors as acceptable. Also, the short time dimension suggests that modeling heteroskedasticity and serial correlation with a feasible weighted least square estimator would not greatly improve efficiency. The problem of cross-sectional correlation is dealt with in the models,

In the first four columns of Table 5 data are expressed as levels; in the latter four as deviations from

sample means. The interpretation of coefficients is different. In columns one to four the coefficients of the

time dummies (not shown) represent the impact of "pure" exogenous shocks on unemployment in a

hypothetical country in which all institutional variables are set to zero, so that $\sum_j \gamma_j X_{j,it} = 0$. In such

hypothetical country there is no employment protection, no unionization, no unemployment benefits, no

taxes, the central bank is politically dominated, and bargaining is completely uncoordinated. The coefficients

of the institutional variables in columns one to four represent the additional impact of the time shocks on

unemployment when the institution in question grows by one unit. In columns five to eight, instead, the

coefficients of the time dummies (not shown) are to be interpreted as the impact of time shocks on

unemployment in a hypothetical country in which all institutions are equal to the sample mean (so

that $\sum_j \gamma_j (X_{j,it} - \overline{X}_j) = 0$; while the coefficients of the institutional variables represent the extra impact of

the time shocks on unemployment when the institution in question grows by one unit above the sample

mean.[110]

As the table clearly shows, substantive conclusions about the impact of institutions vary dramatically

depending on the particular way in which the data are expressed (levels or deviations) and the choice of

standard errors for hypothesis testing. Overall, we do not find unequivocal evidence of institutional impact.

---

as is common praxis, through the insertion of time dummies, even though we doubt that all spatial correlation is

eliminated in this way.

[110] Each of the time dummies is insignificant in models with data in levels, while several of the time dummies are

significant in models with data in deviations. In both cases the time dummies show a growing trend. The difference

could be explained as follows: in a completely deregulated hypothetical country (data in levels), exogenous time shocks

do not significantly affect unemployment, but they do in a country with average values of institutions (data in

deviations).

When data are in levels, no institutional effect is significantly different from zero. When data are in

deviations only two institutions seem to alter significantly the impact of exogenous shocks on

unemployment, using robust standard errors for hypothesis testing (columns five and seven): the overall

generosity of the unemployment insurance system (as captured by the summary measure *BEOECD*) and the

coordination of the wage bargaining system. The former increases the impact; the latter decreases it.

7.2. Impact of Institutions on Wage Growth

We now consider the impact of institutions on wage growth. Indeed, if labor institutions have an

impact on unemployment, such impact should work its way through wages: in an imperfect market scenario,

in which wages are determined as the outcome of bargaining between firms and workers, institutions should

raise equilibrium unemployment by increasing the bargaining power of workers and reducing the willingness

of the unemployed to bid down the wages of the employed. As a check on previous results, then, we estimate

a model in which the dependent variable is a measure of wage growth. The model tested is:

$$\Delta WEU_{i,t} = \beta_0 + \sum_j \gamma_j x_{j,it} + \sum_n \eta_n z_{n,it} + \delta_i + \alpha_t + \varepsilon_{i,t}$$

where ΔWEU is the 5-year average of the annual percentage change of the wage expressed in efficiency

units in country *i* at time *t* (roughly, a measure of permissible real wage increases given total factor

productivity gains attributable to labor).[111] The vector of institutional variables ($\sum_j \gamma_j x_{j,it}$) includes

Employment Protection, Union Density, Benefit Replacement Rate or the Benefit Generosity measure,

Change in the Tax Wedge,[112] Central Bank Independence, and Wage Bargaining Coordination. The vector of

macroeconomic controls ($\sum_n \eta_n z_{n,it}$) includes the Unemployment Rate (instrumented with the real interest

---

[111] See Blanchard, 1997; Blanchard and Philippon, 2004.

[112] See Layard et al., 1991, 33, for this modeling choice.

rate and other variables in the model) and a change in terms of trade variable. The alphas (α_t) are time dummies; the deltas (δ_i) country dummies.

Table 6 about here

Results from the wage growth model are largely compatible with those from models estimating the direct effects of institutions on unemployment. Wage change in efficiency units responds negatively (as expected) to unemployment and terms of trade shocks. Benefits replacement rate and tax wedge are not significantly associated with higher wage increases, consistent with the unemployment models. Benefit generosity is (surprisingly) even significantly negatively associated with wage growth (in line with previous results). Central bank independence per se does not seem to lead to wage moderation.[113] Consistent with the unemployment models, union density is significantly positively associated with wage growth. The main peculiarities of the wage change models concern the employment protection index and the wage coordination index. It looks as though employment protection does lead to higher wages in efficiency units even though this does not seem to translate into increases in unemployment. Interestingly enough, employment protection seems to have no significant impact when the dependent variable is change in unit labor costs (model not shown). This may be due to the fact that the higher wages induced by employment protection spur capital-labor substitution processes which, in turn, generate productivity increases that compensate for them, such that unit costs are unaffected.[114]

Another peculiarity of the wage growth model is that bargaining coordination is significantly associated with lower wage growth, even controlling for country fixed effects. Hence coordination seems effective in bringing about wage moderation, but wage moderation does not necessarily translate in lower unemployment, on average, as suggested by the lack of a robust direct effect of bargaining coordination on unemployment in previous models. This finding, combined with the ones above on real interest rates and

---

[113] In contrast with arguments in Scharpf, 1991; Streeck, 1994; Hassel, 2003.

[114] See Blanchard, 1997; Blanchard and Philippon, 2004.

36

central bank independence (which does not significantly affect wages, but does seem to lead to higher unemployment), points to the importance, in future research, of understanding how the various predictors affect different components of aggregate demand.

We also test whether institutions reduce the responsiveness of wage growth to unemployment. Indeed, this is a key channel through which institutions may affect the persistence of exogenous shocks over time. The argument is illustrated by Blanchard and Wolfers as follows "Take an adverse shock which leads to higher unemployment. The normal adjustment mechanism is then for unemployment to put downward pressure on wages until unemployment has returned to normal. To the extent that some labor market institutions reduce the effect of unemployment on wages, they will increase the persistence of unemployment in response to shocks."[115]

Hence we add to the previous model of wage growth in efficiency units a term interacting (instrumented) unemployment with each institution expressed as deviation from the sample mean. The interaction terms are entered in the equation one by one.[116] The coefficient of the unemployment variable has to be interpreted as the impact on wage change of a one percent increase in unemployment when the value of the interacted variable is equal to the sample mean. If institutions reduce the responsiveness of wage movements to unemployment, the interactions should be positive and significant.

Table 7 about here

Table 7 shows that most interactions are negative. The interaction between wage coordination and unemployment is not only negative but also significantly different from zero at standard confidence levels. Thus, it seems that wage coordination not only leads to lower real wage growth directly, but also increases the effect of unemployment on wage growth. The interactions between benefit replacement and benefit generosity, respectively, with unemployment are instead positive but insignificant. This result, combined

---

[115] Blanchard and Wolfers, 2000, C17.

[116] This is to limit the bias associated with the omission of implicit interaction terms (i.e. variables that are interacted with the same variable, but not entered explicitly as interaction terms, see Braumoller, 2004),

with a negative direct effect of benefits on wage growth (Table 6), suggests that the finding of a significant positive impact of benefit generosity on unemployment-increasing exogenous shocks in models with variables expressed as deviations from the sample mean (see Table 5) may have been a statistical incident.

8. Concluding Remarks

In this paper, we have examined whether pooled time-series cross-section data on OECD countries provided empirical support for the thesis that unemployment is caused by labor market rigidities. In conducting our analysis, we have sought to pay attention to a series of statistical problems generally associated with time-series cross-section data, and compare results from multiple estimators and specifications.

While no model we estimate is entirely problem free, our main results seem robust to changes in estimation methods as well as changes in specification. Our preferred model, which we estimate both in levels and differences, tests the direct effects of institutions with data averaged over five-year periods. It is a simple and parsimonious model, leaving ample room for institutional measures to explain changes in unemployment. We find no systematic support for the deregulatory view. Indeed, employment protection, benefit replacement rates, and tax wedge do not seem to have a significant impact on unemployment. At the same time, we find a robust positive association between union density and unemployment. Also, in contrast with most literature that attributes to it a negative impact on unemployment, wage coordination seems an insignificant predictor of unemployment when fixed effects are controlled for (even though it does seem to moderate wages) and does not moderate the impact of other institutions in a significant way in the medium-to-long term.[117]

---

[117] We cannot exclude that, since some of the institutional variables (especially employment protection) are probably measured with error, the coefficients of the imperfectly measured variables are not biased downwards. Other coefficients may be biased, too, as a result of measurement error. This, however, is a problem that applies to all analyses using these data.

Our results suggest that, at least as far as pooled data allow one to tell,[118] the impact of labor market institutions is, for the most part, not robust and that unemployment is mostly increased by high real interest rates and independent central banks. Obviously there could be more fine-grained effects of institutions that are not captured by our models. For example, labor market institutions may affect different demographic groups in different ways, such that even though there is no average effect on unemployment, there are distinct effects on group-specific employment and unemployment rates, e.g. for women and the youth.[119] We cannot assess these more nuanced effects with our specification. However, the claim that it would be possible to reduce unemployment simply by getting rid of labor market rigidities appears unwarranted based on our results.

References

Agell, Jonas. 1999. On the Benefits from Rigid Labor Markets: Norms, Market Failures, and Social Insurance. Economic Journal 127 February: F143-64.

Agell, Jonas. 2000. On the Determinants of Labor Market Institutions: Rent-Sharing vs. Social Insurance. Working Paper Series, 16.Uppsala: Department of Economics: Uppsala University.

Aidt, Toke, and Zafiris Tsannatos. 2002. Unions and Collective Bargaining. Washington, D.C.: The World Bank.

Arellano, Manuel. 2003. Panel Data Econometrics. Oxford: Oxford University Press.

Baccaro, Lucio. 2000. Centralized Collective Bargaining and the Problem of 'Compliance': Lessons from the Italian Experience. Industrial and Labor Relations Review 53 (4):579-601.

Baccaro, Lucio and Diego Rei. 2005 Institutional Determinants of Unemployment in OECD Countries: A Time-Series Cross-Section Analysis (1960-1998). Discussion Paper No. 160. Geneva: International Institute for Labor Studies.

Baccaro, Lucio and Marco Simoni. 2007. Centralized Wage Bargaining and the 'Celtic Tiger' Phenomenon. Forthcoming in Industrial Relations.

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

---

[118] See Freeman, 2005, for a discussion of the limitations of this kind of analysis.

[119] See Bertola et al, 2001.

Baltagi, Badi.2001. Econometric Analysis of Panel Data. New York: Wiley.

Baker, Dean , Andrew Glyn, David R. Howell, and John Schmitt. 2005. Labor Market Institutions and Unemployment: A Critical Assessment of the Cross-Country Evidence. In Fighting Unemployment: The Limits of Free Market Orthodoxy edited by David R.Howell, 72-118. New York: Oxford University Press.

Baker, Dean , Andrew Glyn, David R. Howell, and John Schmitt. 2003 Unemployment and Labor Market Institutions: The Failure of the Empirical Case for Deregulation. Working paper, 43. Geneva, ILO, Policy Integration department.

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

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

Beck, Nathaniel and Jonathan N. Katz. 1996. Nuisance Vs. Substance: Specifying and Estimating Time-Series-Cross-Section Models. Political Analysis 6:1-36.

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

Belot, Michèle and Jan Van Ours. 2004. Does the Recent Success of Some OECD Countries in Lowering their Unemployment Rates Lie in the Clever Design of their Labor Market Reforms?. Oxford Economic Papers 56:621-42.

Bertola, Giuseppe 1990. Job Security, Unemployment and Wages. European Economic Review 34:851-886.

Bertola, Giuseppe , Francine Blau, and Lawrence Kahn. 2002. Comparative Analysis of Labor Market Outcomes: Lessons for the US from International Long-Run Evidence. In The Roaring Nineties: Can Full Employment Be Sustained? Edited by Krieger, Alan and Robert Solow, 159-218 (citations in this paper refer to the manuscript version of 2001). New York: Russell Sage and Century Foundations.

Binder, Michael, Cheng Hsiao, and M.Hashem Pesaran. 2000. Estimation and Inference in Short Panel Vector Autoregressions with Unit Roots and Cointegration. Working Papers in Economics 0003. Cambridge: Department of Applied Economics, University of Cambridge.

Blanchard, Olivier.1997. The Medium Run. Paper prepared for the Brookings Panel, September.

Blanchard, Olivier.1999. The Role of Shocks and Institutions in the Rise of European Unemployment. Unpublished paper, Baffi Lecture.

Blanchard, Olivier and Justin Wolfers. 2000. The Role of Shocks and Institutions in the Rise of European Unemployment: The Aggregate Evidence. Economic Journal 110 (462) March: C1-33.

Blanchard, Olivier and Thomas Philippon. 2004. The Quality of Labor Relations and Unemployment. NBER Working Paper, 10590

Braumoller Bear. 2004. Hypothesis Testing and Multiplicative Interaction Terms. International Organization 58 (4):807-820.

Checchi, Daniele and Claudio Lucifora. 2002. Unions and Labour Market Institutions in Europe. Economic Policy 17 (2): 362-401.

Cuckierman, Alex, and Francesco Lippi. 1999. CBI, Centralization of Wage Bargaining, Inflation and Unemployment - Theory and Some Evidence. European Economic Review 43:1395-1434

Daunfeldt, Sven Olov, and XavierDe Luna. 2002. Central Bank Independence and Price Stability: Evidence from 23 OECD-countries. Umeå Economic Studies, 589. Umeå: Umeå University.

Daveri, Francesco and Guido Tabellini. 2000. Unemployment, Growth and Taxation in Industrial Countries. Economic Policy 30: 47-104 (citations refer to a 1997 manuscript version).

Ebbinghaus, Bernard and Jelle Visser. 2000. Trade Unions in Western Europe. London: Macmillan.

Eijffinger, Sylvester and Jakob de Haan. 1996. The Political Economy of Central-Bank Independence Princeton Studies in International Economics, 19. Princeton,NJ: International Economics Section, Departement of Economics, Princeton University.

Elmeskov, Jorgen, John Martin, and Stefano Scarpetta. 1998. Key Lessons for Labor Market Reforms: Evidence from OECD Countries' Experiences. Swedish Economic Policy Review 5(2) (Autumn): 205-52.

Feldstein, Martin and James Poterba.1984. Unemployment Insurance and Reservation Wages. Journal of Public Economics 23: 141-167.

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

Fortin, Nicole, and Thomas, Lemieux. 1997. Institutional Changes and Rising Wage Inequality: Is There a Linkage? Journal of Economic Perspectives 11:75-96.

Franzese, Robert, J., Jr.2001.Institutional and Sectoral Interactions in Monetary Policy and Wage-Price Bargaining," In Varieties of Capitalism: The Institutional Foundations of Comparative Advantage., edited by Hall, Peter and David Soskice, 104-44.

Freeman, Richard and James Medoff. 1984. What Do Unions Do? New York: Basic Books.

Freeman, Richard. 2005. Labor Market Institutions without Blinders: The Debate over Flexibility and Labor Market Performance. NBER Working Paper, 11286.

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

Goldfield, Michael. 1990. The Decline of Organized Labor in the United States. Chicago: University of Chicago Press.

Hall, Peter .and Robert .J. Franzese Jr. 1998. Mixed Signals: CBI, Coordinated Wage-Bargaining, and European Monetary Union. International Organization, 52 (summer):505-535.

Hall, Peter , and David Soskice. 2001 Varieties of Capitalism: The Institutional Foundations of Comparative Advantage. Oxford/NewYork: Oxford University Press.

Hassel, Anke. 2003. The Politics of Social Pacts. British Journal of Industrial Relations 41: 707–26.

Holmlund, Bertil. 1997. Unemployment Insurance in Theory and Practice. Working Paper Series, 25. Uppsala: Uppsala University, Department of Economics.

Howell, David R., Dean Baker, Andrew Glyn, A., and John Schmitt, J. 2006. Are Protective Labor Market Institutions Really at the Root of Unemployment? A Critical Perspective on the Statistical Evidence. CEPR Papers, July 14.

Hsiao, Cheng.2004. Analysis of Panel Data. Second Edition. New York: Cambridge University Press.

Ichniowski, Casey, Kathryn Shaw, and Giovanna Prennushi.1995. The Effects of Human Resource Management Practices on Productivity. NBER Working Papers 5333.

IMF. 2003. World Economic Outlook. Chapter 4

Iversen, Torben.1998.Wage Bargaining, CBI and the Real Effects of Money. International Organization 52 (summer):469-504.

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

Katz, Lawrence, and Bruce Meyer.1990.The Impact of the Potential Duration of Unemployment Benefits on the Duration of Unemployment. Journal of Public Economics 41:45-72.

Kenworthy, Lane.2003. Quantitative Indicators of Corporatism. International Journal of Sociology 33 (3): 10-44

Kézdi Gabor 2003. Robust Standard Error Estimation in Fixed-Effects Panel Models. Mimeo, Central European University (URL is http://ideas.repec.org/p/wpa/wuwpem/0508018.html).

Kittel, Bernhard, and Hannes Winner. 2005. How Reliable is Pooled Analysis in Political Economy. The Globalization-Welfare State Nexus Revisited. European Journal of Political research 44 (2):269-293 (references in the paper are from the Discussion Paper 02/3. Max-Planck-Institut für Gesellschaftsforschung)

Kiviet, Jan F.1995. On Bias, Inconsistency and Efficiency of Various Estimators in Dynamic Panel Data Models. Journal of Econometrics 68: 53-68.

Layard, Richard, Stephen Nickell, and Richard Jackman.1991.Unemployment. Oxford: Oxford University Press.

Lazear, Edward P. 1990. Job Security Provisions and Employment. Quarterly Journal of Economics 105: 699-726

Maddala, G.S., and Shaowen Wu. 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, Stephen.1981.Biases in Dynamic Models with Fixed Effects. Econometrica. 49:1417-1426.

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

Nickell, Stephen, and Richard Layard. 1999. Labour Market Institutions and Economic Performance..In Handbook of Labour Economics edited by Ashenfelter, Orley. and David Card, ,(eds), Vol 3:3029-84. Amsterdam and New York: Elsevier:.

Nickell, Stephen,Luca Nunziata,, Wolfgang Ochel, and Glenda Quintini. 2001. The Beveridge Curve, Unemployment and Wages in the OECD. CEP Discussion Papers 0502. London: Centre for Economic Performance, LSE.

Nickell Stephen, and Luca Nunziata. 2001. Labour Market Institutions Database. London: London School of Economics, CEP.

Nickell, Stephen, Luca Nunziata, and Wolfgang Ochel. 2005. Unemployment in the OECD since the 1960s. What Do We Know?. Economic Journal 115:1-27.

Nunziata, Luca. 2001. Institutions and Wage Determination: a Multi-Country Approach. Working Papers in Economics, W29. Oxford, Nuffield College.

Nunziata, Luca. 2002. Unemployment, Labor Market Institutions and Schocks. Working Papers in Economics, W16. Oxford: Nuffield College

OECD. 1994. Jobs Study. Paris: OECD.

OECD. 1999. Employment Protection and Labour Market Performance. In OECD Employment Outlook 1999, 49-132. Paris: OECD.

OECD. 2004a. Employment Protection Regulation and Labour Market Performance. In OECD Employment Outlook 2004, 61-126. Paris: OECD.

OECD. 2004b.Wage-Setting Institutions and Outcomes. In OECD Employment Outlook 2004, 127-81.Paris: OECD.

Petersen, Mitchell. 2005. Estimating Standard Errors in Finance Panel Data Set: Comparing Approaches. NBER Working Papers 11280.

Pontusson, Jonas.2005. Inequality and Prosperity: Social Europe versus Liberal America. Ithaca, NY: Cornell University Press.

Pontusson, Jonas., David Rueda, and Christopher Way. 2002. Comparative Political Economy of Wage Distribution: The Role of Partisanship and Labour Market Institutions. British Journal of Political Science 32 (April):281-308.

Rueda, David and Jonas Pontusson. 2000. Wage Inequality and Varieties of Capitalism. World Politics 52 (3):350-383.

Schettkat, Ronald . 2003. Are Institutional Rigidities at the Root of European Unemployment? Cambridge Journal of Economics 27: 771-87.

Saint-Paul, Gilles.2004.Why Are European Countries Diverging in Their Unemployment Experience? Journal of Economic Perspectives 18(4):49-68.

Scharpf, Fritz 1991. Crisis and Choice in European Social Democracy. Ithaca, NY: Cornell University Press.

Siebert, Horst.1997. Labor Market Rigidities: At the Root of Unemployment in Europe. Journal of Economic Perspectives 11 (3):37-54.

Smith, Ronald, and Ana Maria Fuertes. 2004. Panel Time-Series. Mimeo, April 29. London: Birkbeck College, London and Sir John Cass Business School, City University, London.

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

Soskice, David and Torben Iversen. 2000. The Nonneutrality of Monetary Policy with Large Price or Wage Setters. Quarterly Journal of Economics 115 (1):265-284.

Streeck, Wolfgang. 1984. Pay Restraint without Incomes Policy: Institutionalized Monetarism and Industrial Unionism in Germany. In The Return of Incomes Policy edited by Dore, Ronald, Robert Boyer, and Zoe Mars, 118-40. London: Pinter.

Tarantelli, Ezio.1986. Economia Politica del Lavoro. Turin: Utet.

Traxler, Franz. and Bernhard Kittel. 2000. The Bargaining System and Performance: A Comparison of 18 OECD Countries. Comparative Political Studies 33:1154-1190.

Visser, Jelle and Anton Hemerijck. 1997. A Dutch Miracle. Amsterdam: Amsterdam University Press.

Figure 1: The Trajectory of Unemployment in France, Germany, Italy and the US (1960-1998)
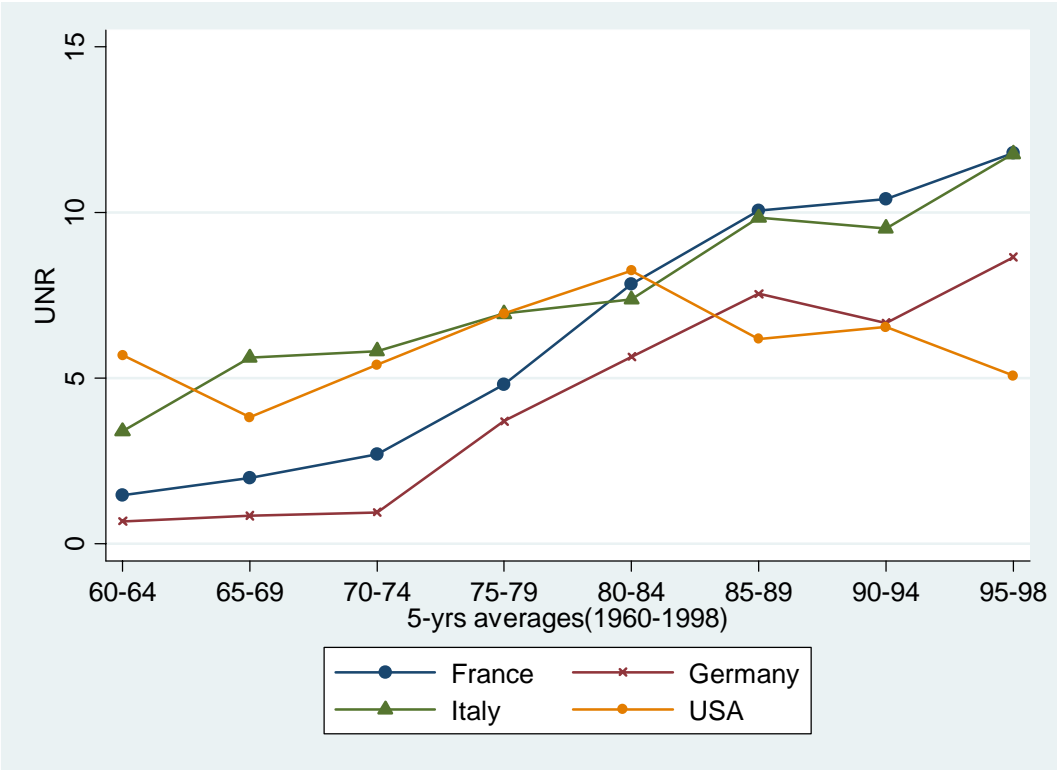
Source: See Appendix

Figure 2: The Trajectory of Unemployment and Labor Market Institutions in Denmark, the Ireland, the Netherlands, and the UK

Source: see Appendix

Table 1. Annual Data. Dynamic Models in Levels (Intercept, Country, and Time Dummies Omitted)

Table 1	OLS	FGLS het. and autocorr..(common panel RHO)	FGLS het. and autocorr. (panel-spec.RHO)	Column III, with BENOECD	OLS with PCSEs	OLS/PCSE with BENOECD	FGLS het. and autocorr..(common panel RHO) .	OLS with PCSEs. .Alternative Macro Controls
Dependent Variable	unr	unr	unr	unr	unr	unr	unr	unr
Lagged Dep.variable	0.901(44.85)**	0.829 (35.72)**	0.832 (36.34)**	0.831 (36.48)**	0.901 (28.92)**	0.897 (28.63)**	0.844(38.31)**	0.898(28.58)**
Real interest rate	0.056 (3.77)**	0.059 (4.54)**	0.055 (4.31)**	0.055 (4.28)**	0.056 (3.44)**	0.058 (3.55)**	0.022(1.84) ♦	0.016(0.97)
Change in Inflation	-0.041 (2.44)*	-0.030 (2.61)**	-0.033 (3.05)**	-0.033 (3.06)**	-0.041 (2.15)*	-0.040 (2.12)*	-0.018(1.82) ♦	-0.017(1.04)
Terms of trade shocks	-0.052 (1.49)	-0.060 (2.30)*	-0.063 (2.45)*	-0.061 (2.38)*	-0.052 (1.33)	-0.051 (1.30)		
Lagged Productivity Δ	-0.081(4.25)**	-0.049 (3.61)**	-0.050 (3.96)**	-0.050 (4.02)**	-0.081 (3.64)**	-0.079(3.54)**		
EP	-0.096 (0.52)	0.120 (0.66)	0.013 (0.07)	0.034 (0.18)	-0.096 (0.62)	-0.042 (0.28)	0.107(0.73)	0.020(0.16)
UD	0.013 (2.10)*	0.021 (2.85)**	0.018 (2.32)*	0.016 (2.08)*	0.013 (1.65) ♦	0.014 (1.70) ♦	0.016(2.37)*	0.014(2.01)*
BRR	0.006 (1.62)	-0.004 (1.06)	-0.000 (0.12)		0.006 (1.55)		-0.001(0.38)	0.007(1.93)♦
TW BENOECD	-0.006 (0.59)	-0.018 (1.64)	-0.014 (1.31)	-0.015 (1.36) 0.002 (0.32)	-0.006 (0.59)	-0.005 (0.51) 0.007 (1.14)	-0.022(2.16)*	-0.016(1.53)
CBI	0.501 (1.27)	0.828 (2.26)*	1.100 (2.85)**	1.121(2.91)**	0.501 (1.34)	0.490 (1.29)	1.042(2.04)*	1.171(2.08)*
BC	0.035 (0.74)	0.093 (2.01)*	0.075 (1.70) ♦	0.071(1.62)	0.035 (0.70)	0.045 (0.92)	0.119(2.65)**	0.036(0.80)
*BC*UD*	-0.005 (1.87)♦	-0.006 (1.96)*	-0.006 (2.35)*	-0.006(2.15)*	-0.005 (1.66) ♦	-0.005 (1.58)	-0.005(1.80)♦	-0.006(2.19)*
*BC*TW*	-0.002 (0.48)	-0.001 (0.23)	-0.002 (0.44)	-0.001(0.26)	-0.002 (0.42)	-0.002 (0.40)	-0.009(2.20)*	-0.002(0.45)
*BC*EP*	0.095 (1.25)	0.108 (1.44)	0.115 (1.58)	0.110(1.48)	0.095 (1.21)	0.106 (1.33)	0.115(1.70)♦	0.036(0.56)
*BC*BRR*	-0.000 (0.24)	0.002 (0.81)	0.001 (0.73)		-0.000 (0.24)		0.002(0.96)	0.000(0.25)
*BC*CBI*	0.079 (0.36)	-0.126 (0.66)	-0.158 (0.81)	-0.120 (0.62)	0.079 (0.38)	0.104 (0.52)	-0.258(1.07)	-0.070(0.27)
*BC*BenOECD*	0.000 (0.14)		-0.001 (0.45)					
Labour Demand Shocks							-19.495(9.44)**	-22.231(7.26)**
Money Supply Shocks							0.436(2.53)*	0.419(1.39)
Real Import Prices							4.379(2.76)**	3.838(1.58)
Total Factor Productivity Shocks							-18.451(11.43)**	-20.308(9.72)**
Observations	620	620	620	620	620	620	559	559
ADJ- R-squared	0.96				0.96	0.95		0.96
Number of Countries	18	18	18	18	18	18	18	18
Estimated Rho	.30	.36			.30	.31	0.33	..26
(Durbin M) Test for Remaining Serial Correlation of the residuals	Coeff: .39 P- value $\cong$ 0.0	Coeff: . .44 P- value $\cong$ 0.00	Coeff: . .44 P- value $\cong$ 0.00	Coeff: . .44 P- value $\cong$ 0.00	Coeff: .39 P- value $\cong$ 0.000	Coeff: ..4 P- value $\cong$ 0.000	Coeff: .34 P- value $\cong$ 0.000	Coeff: .32 P- value $\cong$ 0.0
Wald test on Country dummies	F(17, 550) = 2.21 P-val = 0.0036	$\chi_{(17)} = 53.85$ P- value $\cong$ 0.000	$\chi_{(17)} = 58.06$ P-value $\cong$ 0.000	$\chi_{(17)} = 55.46$ P-value= 0.000	$\chi_{(17)} = 29.91$ P-value = 0.027	$\chi_{(17)} = 28.04$ P-value = 0.044	$\chi_{(17)} = 59.6$ P-value = 0.00	$\chi_{(17)} = 47.5$ P-value = 0.00
Wald test on Time dummies	F(34, 550) = 4.98 P-value=0.0000	$\chi_{(35)} = 211.46$ P-value = 0.0000	$\chi_{(35)} = 228.11$ P-value = 0.0000	$\chi_{(34)} = 193.28$ P-value = 0.00	$\chi_{(24)} = 25217.44$ P-value = 0.0000	$\chi_{(24)} = 49348.28$ P-value=0.00	$\chi_{(32)} = 216.7$ P-value = 0.00	$\chi_{(32)} = 2.2e+05$ P-value = 0.00
Wald test on Interactions	F(5, 550) = 1.53 P-value = 0.18	$\chi_{(5)} = 5.94$ P-value = 0.31	$\chi_{(5)} = 7.41$ P-value = 0.19	$\chi_{(5)} = 7.17$ P-value = 0.2	$\chi_{(5)} = 5.79$ P-value = 0.3269	$\chi_{(5)} = 6.12$ P-value = 0.29	$\chi_{(5)} = 7.38$ P-value = 0.19	$\chi_{(5)} = 9.64$ P-value = 0.08
Multicollinearity Tests	Mean VIF 5.41 Condition Nmb: 19.0066	Mean VIF 5.41 Condition Number 19.0066	Mean VIF 5.41 Condition Nmb: 19.0066	Mean VIF 4.84 Condition Nmb: 18.66	Mean VIF 5.41 Condition Number 19.0066	Mean VIF 4.84 Condition Number 18.66	Mean VIF 5.1 Condition Number 82.7	Mean VIF 5.1 Condition Number 82.7

Cointegration test for the model in Column 1: Augmented Dickey Fuller: 83.62, Phillips Perron: 101.22 – which implies rejection of Ho of non-cointegration at 1%. (see ft 21)
Here and in the other tables: Absolute value of z statistics in parentheses.♦ significant at 10%; * significant at 5%; ** significant at 1%

47

Table 2. Five-Year Data. Static Models in Levels (Intercept, Country, and Time Dummies Omitted).

Table 4	OLS with Newey-West s.e.	FGLS heterosc. and autocorrelation (common rho)	OLS Newey-West s.e.	FGLS heterosc. and autocorrelation (common rho)	OLS Newey-West s.e.	FGLS heterosc. and autocorrelation (common rho)	OLS Newey-West s.e. with BENOECD	FGLS heterosc./aut. (common rho) with BENOECD
Dep. Variable	Unr	unr	unr	unr	unr	unr	Unr	unr
Real Interest rate	0.315 (3.31)**	0.282 (3.71)**	0.262 (3.10)**	0.216 (2.98)**	0.252 (3.31)**	0.240 (4.00)**	0.255 (3.38)**	0.237 (3.95)**
Change in inflation	0.065 (0.38)	0.028 (0.20)	-0.013 (0.08)	-0.071 (0.52)				
Terms of trade shocks	0.092 (0.28)	0.038 (0.16)	0.063 (0.19)	-0.158 (0.61)				
Lagged Productivity change	0.202 (1.15)	0.136 (1.19)	0.194 (1.10)	0.111 (0.97)				
EP	1.259 (1.02)	0.652 (0.72)	0.925 (0.76)	0.510 (0.61)	1.518 (1.47)	0.977 (1.46)	1.452 (1.31)	0.935 (1.37)
UD	0.090 (1.73)♦	0.067 (2.20)*	0.083 (2.00)*	0.077 (2.75)**	0.103(3.28)**	0.101 (4.11)**	0.105(3.21)**	0.102 (4.12)**
BRR	-0.011 (0.63)	-0.013 (0.88)	-0.019 (1.09)	-0.020 (1.47)	-0.020 (1.20)	-0.021 (1.64)		
BENOECD							-0.028 (0.97)	-0.021 (0.98)
TW	-0.092 (1.44)	-0.103 (2.23)*	-0.064 (1.05)	-0.069 (1.45)	-0.044 (0.89)	-0.051 (1.30)	-0.046 (0.89)	-0.054 (1.38)
CBI	3.798 (1.75) ♦	4.689 (2.92)**	4.053 (2.36)*	4.142 (2.81)**	4.286 (2.49)*	4.102 (2.83)**	4.261 (2.36)*	4.162 (2.82)**
BC	0.195 (0.79)	0.085 (0.41)	0.120 (0.53)	-0.109 (0.63)	0.015 (0.08)	-0.162 (1.07)	-0.014 (0.08)	-0.188 (1.24)
BC*UD	0.001 (0.03)	-0.008 (0.63)						
BC*TW	0.005 (0.19)	-0.008 (0.43)						
BC*EP	0.451 (0.80)	0.658 (1.74)♦						
BC*BRR	-0.016 (1.32)	-0.014 (1.49)						
BC*CBI	-0.843 (0.79)	-0.667 (0.71)						
Observations	*121*	*121*	*121*	*121*	*134*	*134*	*134*	*134*
Number of countries	*18*	*18*	*18*	*18*	*18*	*18*	*18*	*18*
Wald test on Country Dumm	*F(17, 81) = 9.50 P- value ≅ 0.000*	$\chi_{(17)} =$ *151.29 P- value ≅ 0.000*	*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*	*F(17, 102) = 11.94 P- value ≅ 0.000*	$\chi_{(17)} = 135.12$ *P- value ≅ 0.000*
Wald test on time Dummies	*F(7, 81) = 6.32 P- value ≅ 0.000*	$\chi_{(7)} = 85.44$ *P- value ≅ 0.000*	*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*	*F(7, 102) = 6.31* **P- value ≅ 0.000**	$\chi_{(7)} = 77.04$ **P- value ≅ 0.000**
Wald test on Interaction terms	*F(5, 81) = 0.72 P-value = 0.6114*	$\chi_{(1)} = 7.25$ *P-value = 0.2029*						
Wald test on all the macro variables but Real Int. Rate	*F(3, 81) = 0.48 P-value = 0.699*	$\chi_{(3)} = 1.55$ *P-value = 0.67*	*F(3, 81) = 0.50 P-value = 0.6855*	$\chi_{(3)} = 1.55$ *P-value = 0.67*				
Estimated Rho	*.3*	*.26*	*.32*	*.25*	*.32*	*.44*	***.39***	
Multicollinearity Tests	*Mean VIF 12.86 Condition Number 28.1789*	*Mean VIF 12.86 Condition Number 28.1789*	*Mean VIF 12.12 Condition Number 26.5339*	*Mean VIF 12.12 Condition Number 26.5339*	*Mean VIF 7.20 Condition Number 17.1595*	*Mean VIF 7.20 Condition Number 17.1595*	Mean VIF 6.73 Condition Number 16.99	Mean VIF 6.73 Condition Number 16.99
LM Remaining serial correlation test	$\chi_{(1)} = 6.79$ *P-val=.009*	$\chi_{(1)} = 10.95$ *P-value=.0000*	$\chi_{(1)} = 7.39$ *P-value = .006*	$\chi_{(1)} = 12.$ *P-value = .0000*	$\chi_{(1)} = 13.86$ *P-value = .00*	$\chi_{(1)} = 18.47$ *P-value = .0000*	$\chi_{(1)} = 14.28$ *P-value=0.00*	$\chi_{(1)} = 18.87$ *P-value = .0000*
Adj R Square	0.79		.79		.81		.81	

Table 3. Five-Year Data. Fixed and Random Effects in Levels (Intercept, Country dummies, Time dummies omitted)

	Fixed Effects (OLS Newey-West s.e.)	Random Effects	Fixed Effects with benefit entitlements measure (OLS Newey-West s.e.)	Random Effects with benefit entitlements measure
Dependent Variable	Unr	unr	unr	unr
Real Interest rate	0.252 (3.31)**	0.234 (2.69)**	0.255 (3.38)**	0.239 (2.72)**
EP	1.518 (1.47)	0.480 (0.76)	1.452 (1.31)	0.506 (0.79)
UD	0.103 (3.28)**	0.055 (2.71)**	0.105 (3.21)**	0.055 (2.68)**
BRR	-0.020 (1.20)	-0.019 (1.39)		
BENOECD			-0.028 (0.97)	-0.020 (0.89)
TW	-0.044 (0.89)	-0.022 (0.65)	-0.046 (0.89)	-0.030 (0.90)
CBI	4.286 (2.49)*	2.925 (1.84)♦	4.261 (2.36)*	2.889 (1.81) ♦
BC	0.015 (0.08)	-0.465 (2.35)*	-0.014 (0.08)	-0.485 (2.44)*
Hausman Test results. *Ho: difference in coefficients not systematic*	$\chi_{(15)} =$ 21.56 P-value = 0.11		$\chi_{(15)} =$ 24.77 P-value = 0.05	
Observations	134	134	134	134
Number of CNTRY		18		18
R squared	.76	.49	.76	.48

Table 4. Five-Year Data. Models in First Differences (No Intercept; Time Dummies Omitted).

	FGLS modeling heteroskedasticity (I)	OLS with White robust standard errors (II)	FGLS modeling heteroskedasticity (I)	OLS with White robust standard errors (IV)	FGLS-heterosk. with benefit variable from oedc (V)	OLS with White robust standard errors with benefit variable from oedc(VI)	FGLS-heterosk. with benefit variable from oedc (VII)	OLS with White robust standard errors with benefit variable from oedc(VIII)
Dep. Var	Unr	unr	unr	unr	unr	unr		
Real Interest rate	0.224 (2.42)*	0.219 (2.20)*	0.265 (4.49)**	0.273 (3.73)**	0.266 (4.49)**	0.273 (3.71)**		
Inflation	-0.077 (0.32)	-0.160 (0.64)						
Terms of trade shocks	0.161 (0.77)	0.031 (0.12)						
Lagged Productivity change	-0.152 (1.31)	-0.101 (0.75)						
EP	-1.715 (1.78) ♦	-1.747 (1.99)*	-1.083 (1.35)	-1.121 (1.58)	-1.157 (1.47)	-1.162 (1.87) ♦	-1.19 (1.46)	-1.317 (1.95) ♦
UD	0.095 (2.44)*	0.110 (2.12)*	0.102 (2.99)**	0.108 (2.53)*	0.100 (2.90)**	0.110 (2.49)*	0.094 (2.69)**	0.093 (2.01)*
BRR	-0.013 (0.67)	-0.004 (0.19)	-0.007 (0.44)	-0.005 (0.23)				
BENOECD					0.005 (0.14)	-0.012 (0.29)	0.008(0.26)	-0.017 (0.44)
TW	-0.065 (1.31)	-0.063 (1.21)	-0.064 (1.42)	-0.071 (1.70) ♦	-0.074 (1.67) ♦	-0.071 (1.75) ♦	-0.054 (1.20)	-0.043 (0.99)
CBI	4.301 (2.14)*	4.340 (2.09)*	4.121 (2.29)*	4.364 (1.99)*	4.174 (2.29)*	4.295 (1.95)♦	3.867 (2.01)*	4.141 (1.80) ♦
BC	-0.079 (0.37)	-0.061 (0.23)	-0.239 (1.80) ♦	-0.162 (1.05)	-0.238 (1.80) ♦	-0.166 (1.09)	-0.238 (1.53)	-0.173 (0.95)
*BC*UD*	-0.018 (1.36)	-0.004 (0.28)						
*BC*TW*	-0.011 (0.59)	-0.019 (0.97)						
*BC*EP*	0.507 (1.38)	0.604 (1.34)						
*BC*BRR*	-0.001 (0.13)	-0.002 (0.16)						
*BC*CBI*	-0.892 (0.84)	-0.691 (0.68)						
Observations	*103*	*102*	*116*	*116*	*116*	*116*	*121*	*121*
Number of CNTRY	*18*	*18*	*18*	*18*	*18*	*18*		
Adj. R-squared		*0.39*		*0.42*		*0.41*		*0.17*
LM Serial correlation test[120]	$\chi_{(1)} = 2.62$ P-value =.11	$\chi_{(1)} = 1.9$ P-value =.15	$\chi_{(1)} = .33$ P-value =.56	$\chi_{(1)} = .39$ P-value =.52	$\chi_{(1)} = .32$ P-value =.56	$\chi_{(1)} = .40$ P-value =.552	$\chi_{(1)} = .13$ P-value =.7	$\chi_{(1)} = ..228$ P-value =..663
Wald test on Macro variables (but RIR)	$\chi_{(3)} = 2.6$ P-value= 0.4532	F(3, 87) = 0.55 P-value = 0.6506						
Wald test on interactions:	$\chi_{(3)} = 4.67$ P-value = 0.4580	F(5, 82) = 0.45 P-value = 0.8153						
Multicollinearity Tests	*Mean VIF 2.37 Condition Number 6.4712*	*Mean VIF 2.37 Condition Number 6.4712*	*Mean VIF 8.39 Condition Number 16.1470*	*Mean VIF 8.39 Condition Number 16.1470*	*Mean VIF 8.84 Condition Number 15.83*	*Mean VIF 8.84 Condition Number 15.83*	*Mean VIF 9.52 Condition Number 15.81*	*Mean VIF 9.52 Condition Number 15.81*
	Time dummies	*Time*	*Time dummies*	*Time*	*Time dummies*	*Time dummies*	*Time*	*Time dummies*

---

[120] Baltagi and Li (1995) LM test for autocorrelation in the residuals:

$$v_{i,t} = \rho v_{i,t-1} + \varepsilon_{i,t}, \; H0 \; : \rho = 0$$

	inserted	dummies inserted	inserted	dummies inserted.	inserted	inserted	dummies inserted	inserted

Table 5: Estimation of the Impact of Institutions on the Magnitude of Time Effects on Unemployment. Non Linear Least Squares. Intercept, Time, and Country Dummies Not Shown

	Levels, Rogers standard errors	Levels, l.s standard errors.	Levels, Rogers standard errors	Levels, l.s standard errors.	Deviations, Rogers standard errors	Deviatiosn, l.s standard errors	Deviations, Rogers standard errors	Deviations, l.s standard errors
Dep.Var.	Unr	Unr	Unr	Unr	unr	Unr	unr	Unr
EP	0.922	0.922	0.973	0.973	0.427	0.427	0.348	0.348
	(0.70)	(1.01)	(0.68)	(0.85)	(1.29)	(2.18)*	(1.52)	(1.97)*
UD	0.026	0.026	0.026	0.026	0.012	0.012	0.009	0.009
	(0.65)	(0.92)	(0.59)	(0.75)	(1.23)	(2.52)*	(1.02)	(1.96)*
BRR	0.017	0.017			0.008	0.008		
	(0.66)	(0.93)			(1.55)	(1.87) ♦		
BEOECD			0.073	0.073			0.026	0.026
			(0.85)	(0.94)			(3.24)**	(5.01)**
TW	-0.014	-0.014	-0.011	-0.011	-0.006	-0.006	-0.004	-0.004
	(0.49)	(0.89)	(0.48)	(0.64)	(0.48)	(0.84)	(0.42)	(0.57)
CBI	1.097	1.097	1.121	1.121	0.508	0.508	0.401	0.401
	(0.60)	(0.71)	(0.59)	(0.59)	(1.07)	(1.42)	(1.06)	(1.18)
BC	-0.442	-0.442	-0.637	-0.637	-0.205	-0.205	-0.228	-0.228
	(0.84)	(1.10)	(0.91)	(0.95)	(2.71)*	(3.34)**	(4.28)**	(3.97)**
Observations	139	139	139	139	139	139	139	139
R-squared	0.95	0.95	0.96	0.96	0.95	0.95	0.96	0.96

Table 6. Institutional Determinants of Wage Growth in Efficiency Units. 2SLS. Intercept, Country, and Time Effects not Shown

	2SLS, Newey West standard errors with Benefit Replacement Rate	2SLS, Newey West standard errors with BENOECD
Dep.Var.	WEI	WEI
Unr	-1.166	-1.103
	(2.30)*	(2.29)*
Terms of trade shocks	-1.001	-1.249
	(1.88) ♦	(2.39)*
EP	2.694	2.530
	(2.38)*	(2.44)*
UD	0.100	0.111
	(2.01)*	(2.36)*
BRR	-0.022	
	(1.01)	
BEOECD		-0.073
		(2.30)*
ΔTW	0.000	0.000
	(0.66)	(1.40)
CBI	0.928	0.521
	(0.27)	(0.14)
BC	-0.589	-0.638
	(1.69) ♦	(1.97)*
Observations	121	121
	Exogenous instrument for unr is the real interest rate.	Exogenous instrument for unr is the real interest rate.

Table 7. Analysis of Interactions between UNR (instrumented) and Institutional Predictors in a Model of Wage Growth in Efficiency Units

	2SLS, Newey West standard errors with BRR	2SLS, Newey West standard errors with BENOECD
*EP*UNR*	-0.255	-0.222
	(1.76)♦	(1.57)
*UD*UNR*	-0.005	-0.004
	(0.83)	(0.77)
*BRR*UNR*	0.009	
	(1.46)	
*BEOCD*UNR*		0.016
		(1.39)
*DTW*UNR*	-0.000	-0.000
	(1.77)♦	(1.84)♦
*CBI*UNR*	0.231	0.076
	(0.45)	(0.15)
*BC*UNR*	-0.226	-0.204
	(3.53)**	(3.28)**
Observations	121	121

Each of the interaction terms was entered one by one to the specifications in Table 6, Columns 1 and 2, respectively

Data Appendix

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 OECD data and is publicly available at http://cep.lse.ac.uk/pubs/number.asp?number=502. 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 the measure elaborated and made available to us by Lane Kenworthy.

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 1960-1998. The panel is unbalanced.

Macroeconomic variables

Unemployment Rate (UNR): All data are from historical OECD databases for standardized unemployment rate

Real Interest Rates: This is the NN series updated for 1995-99 by the IMF based on OECD Economic Outlook series for long-term interest rates and consumer price deflators. 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: 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): 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: 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 to GDP (at constant prices). Raw data on export prices, import prices and trade openness are from OECD databases.

Wage Change in Efficiency Units ($\Delta WEU_{i,t}$): corresponds to the five year average of the change in real wage (*RW*) minus the change in an index of labor productivity (ILP): $\Delta WEU=\Delta RW-\Delta ILP$. The ILP is constructed, following Blanchard and Wolfers (2000), as the percentage change in total factor productivity (ΔTFP) minus the percentage change in the labor share of current GDP at current market prices (a): $\Delta ILP=\Delta TFP-\Delta a$. Both measures (TFP and a) are taken from the AMECO database, http://ec.europa.eu/economy_finance/indicators/annual_macro_economic_database/ameco_en.htm (downloaded 6/2005).

Additional Macroeconomic Controls (Table 1, Columns 7 and 8)

Money Supply Shock: 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, 2001, 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.

Institutional variables

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

Union Density (UD): This is the NN series updated for 1995-98 by Baker et al. (2003) based on Ebbinghaus and Visser (2000) as well as other sources. Data are expressed in percentage points.

Benefit replacement rate (BRR): This is the NN measure as modified by Baker et al. (2003), namely "benefit entitlement before tax as a percentage of previous earnings before tax. Data are averages over replacement rates at two earnings levels (average and two-thirds of average earnings) and three family types (single, with dependent spouse, with spouse at work). They refer to the first year of unemployment" (Nickell et al., 2001: 46). Baker et al. introduce minor modifications for three Scandinavian countries in the 1970s and update the series to 1998. The data are in percentage points.

Benefit entitlements (BENOECD): This is the OECD summary measure of benefit entitlements. It is publicly available at:
http://www.oecd.org/document/0/0,2340,en_2649_34633_34053248_1_1_1_1,00.html#statistics.
It is defined as the average of the gross unemployment benefit replacement rates for two earnings levels, three family situations and three durations of unemployment. As the measure for the benefit replacement rate, it is available every second year. We interpolated linearly the value for the missing years, considering the first year of our range, 1960, equal to 1961, the first year of data availability.

Taw Wedge (TW): Baker et al. (2003: 27) 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)." Data are in percentage points.

Central Bank Independence index (CBI): This is a 0-1 continuous CBI index. The IMF borrowed the series from Rob Franzese (see Hall and Franzese, 1998) and updated it based on information on more recent reforms in Daunfeldt and de Luna (2002).

Index of Co-ordination in wage setting (BC): The variable is taken from Kenworthy (2003). It is available at: http://www.emory.edu/SOC/lkenworthy/WageCoorScores.xls. 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 (Baccaro and Simoni, 2007; Baccaro, 2000).