

Center for European Studies

Working Paper Series #98

Labor Market Institutions and Unemployment:
A Critical Assessment of the Cross-Country Evidence*

by

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Abstract

In the last twenty five years, there has been a sharp divergence in trends in the unemployment rate among OECD countries, with some seeing much larger increases in unemployment than others. This divergence is usually explained by institutions that lead to labor market inflexibility – generous unemployment benefits, employment protections, and strong unions – in countries with high unemployment rates. This paper examines the evidence for this view. It shows that there is no simple bivariate relationship between standard measures of labor market institutions and unemployment rates across countries. It then critically examines several of the most often cited studies that support the labor market inflexibility view. It finds that these studies present relatively weak and to some extent contradictory support for the labor market inflexibility view. Finally, the paper presents the results of a set of tests designed to replicate some of the earlier multivariate analyses with more current data. These tests consistently fail to find robust evidence to support the labor market inflexibility view.

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The rigidities imposed by labor market institutions and policies are widely held to play a key role in the explanation of the European unemployment crisis of the 1980s and 1990s.

*We thank the MacArthur Foundation and the Center for Economic Policy Analysis of New School University for financial support. We are greatly indebted to Michèle Belot, Olivier Blanchard, Steve Nickell, Luca Nunziata, Justin Wolfers, and Jan C. van Ours for making their data and documentation available to us, and to Laura Bardone, Andrea Bassanini, John Martin, Dominique Parturot, Stefano Scarpetta and Paul Swaim for OECD data and their advice about it. We are also grateful to Andrew Martin, Wendy Carlin, John Morley, Steve Nickell and Luca Nunziata for most helpful comments and discussion. None of the above are responsible for the results and interpretations reported in this paper. Our data are available upon request

This was the central message of the OECD's *Job Study* (1994), and a recent follow-up report on the implementation of the *Job Study*'s recommendations confirms that this rigidity explanation remains the conventional wisdom: "Previous OECD work... and a growing body of academic research suggests a direct link between structural reform and labor market outcomes (see Box 2.3)" (OECD, 1999, p. 52-3).¹ A recent paper in the *Swedish Economic Policy Review* by three noted OECD researchers (Elsmeskov, Martin and Scarpetta, 1998) provides a good example of the broad consistency between OECD and academic research on the determinants of OECD unemployment. Comparing their results with those to be published in Nickell and Layard (1999), a prominent academic paper, they conclude that "Both studies assign significant roles to unemployment benefits, collective bargaining structures, active labor market policies ... and the tax wedge – even if the variables in question are defined somewhat differently between the two studies." While the final assessment on the importance of "bad" (unemployment-creating) institutions shows some variation across the leading studies (see section 3 below), the broad consensus has been that labor-market institutions and policies² lie at the heart of the unemployment problem.

This paper evaluates the empirical evidence for this labor market rigidity account. Our approach is distinctive in that we begin from an admittedly skeptical stance and proceed to ask whether the available evidence, from both the literature and our own analysis of the standard data, can provide a compelling case for the conventional account. In the first section we set the macroeconomic and institutional stage. Section 2 then takes an initial look at the data, by presenting simple scatter plots in which conventional measures of the most commonly referenced labor market institutions are set, one at a time, against the standardized unemployment rate for nineteen OECD countries for the 1980-99 period. These figures show that standardized rates over time by country show little or no statistical association with conventional measures of institutions and policies.

Such straightforward statistical evidence appears rarely in the leading papers. Rather, empirical support for the labor market rigidity explanation comes almost exclusively from multivariate analyses that have become increasingly complex since the pioneering work of Layard, Nickell, and Jackman (1991, 1994). In Section 3, we survey the leading papers in this literature. While these studies tend to conclude that institutions are a key part of the story, the

actual empirical results appear far less robust and uniform across studies than is commonly believed. Indeed, while the OECD policy position has stressed the “direct links” between labor-market institutions and the unemployment problem, a careful reading of their own survey of the cross-country evidence turns up “no evidence” for union density and only “mixed evidence” for the effects of unemployment benefits, active labor market policies, and employment protection laws (OECD, 1999, Box 2.3, p. 55). At the same time, the standard interpretation systematically downplays the empirical support that exists for a beneficial role of collective-bargaining coordination (typically large effects) and active-labor-market policies (more mixed results). It should also be noted that an important part of the explanatory power of the institutional approach, in fact, derives from these two institutions’ apparent ability to *reduce* unemployment.

We then present, in Section 4, our own multivariate results. With data for twenty OECD countries organized into five-year periods and extended to 1999, we present results of regression tests of the effects of institutions on unemployment across different time periods with different combinations of variables. We show, first, how sensitive one of the best known results in the literature is to the particular set of institutional measures used. We then show that the most comprehensive available measures of institutions and policies can only account for a minor part of the differences in the evolution of unemployment across these twenty OECD countries over the past forty years and that the impacts of institutions on unemployment do not appear to be stable over time. The upshot is that our multivariate results provide little or no more support for the labor market rigidity explanation than did our simple scatter plots. These results lend support to Tony Atkinson’s (2001, pp 48-9) view that “Aggregate cross-country evidence, interesting though it may be, cannot on its own provide a reliable guide to the likely consequences of rolling back the welfare state.”

1. Macroeconomic and Institutional Settings

Unemployment and Inflation

As the first columns of Table 1 show, both the average rate of unemployment and its dispersion increased dramatically from the early 1970s to the early 1990s, a pattern that many comparative studies of OECD unemployment have attempted to explain. The unweighted average unemployment rate quadrupled between the late 1960s and the early 1990s and disper-

sion (as measured by the standard deviation) rose practically as fast, a development reflected in the fanning out of the country points in Figure 1 of the Introduction to this volume clearly illustrates. The second half of the 1990s saw modest declines in both average unemployment and its dispersion, falling even more sharply in 2000-2001. Thus, after peaking at 10.9 percent in 1994, unemployment in OECD-Europe fell to 7.6 percent in 2001. This compares to a decline in U.S. unemployment from 7.5 percent in 1992 to 4.0 percent in 2000, which then rose sharply to 4.8 percent in 2001 (OECD, 2002, Table A).

The course of inflation shows a striking contrast to that of unemployment. Average inflation rates (again with annual fluctuations smoothed out) began rising earlier than unemployment and reached their peak in the late 1970s, with a great deal of variation across countries. As Table 1 shows, inflation then subsided, at first slowly and then precipitately during the 1980s. By the late 1990s the inflation rate was half the level prevailing in the early 1960s and dispersion was lower as well.

Table 1: Unemployment and Inflation Trends for Nineteen OECD Countries, 1960-99

	Unemployment Rate		Inflation Rate	
	Mean	Std. Dev.	Mean	Std. Dev.
1960-64	2.1	1.7	3.6	1.3
1965-69	2.1	1.3	4.1	1.0
1970-74	2.5	1.7	8.2	1.9
1975-79	4.3	2.3	10.1	4.3
1980-84	6.9	3.9	9.0	3.6
1985-89	7.7	4.8	4.0	2.6
1990-94	8.8	4.1	3.6	1.3
1995-99	8.2	4.0	1.8	0.8

Source: see Appendix 2

This broad pattern for inflation trends has been widely interpreted as supporting a view that the economy has an equilibrium unemployment rate, or NAIRU, which has fluctuated both across time and across countries over the last four decades. Factors other than the labor market are involved in determining inflation, notably prices of imports from outside the OECD. However, the rising inflation from the late 1960s through to the end of the 1970s is broadly consistent with unemployment having been typically below the NAIRU, with the sub-

sequent disinflation suggesting that unemployment had overshoot and was somewhat above the NAIRU. By the turn of the century, inflation was both low and generally steady. The continued high degree of dispersion of unemployment, therefore, suggests corresponding variations in country level NAIRUs. In terms of explaining cross-country patterns of unemployment, the change in inflation is frequently taken as a rough indicator of how far each economy is away from equilibrium unemployment.

Within the NAIRU framework, the experience reviewed above is interpreted as showing that increases in the NAIRU up to the early 1990s differed greatly across countries and that there were some interesting decreases in the NAIRU in the 1990s. Two sets of influences have been suggested to explain these patterns – macroeconomic developments and labor-market institutions.

Macroeconomic Developments

A number of macroeconomic influences can affect equilibrium unemployment. These revolve around the “space” for real wage gains. The essential point is that if real wages have to decline (or more plausibly have to rise more slowly than the rate to which workers have become accustomed) then a higher level of unemployment will be required to weaken workers’ bargaining power and thus prevent “excessive” wage increases and rising inflation. A host of complicated issues surround the form and permanence of such effects. Does a slower growth of “feasible” real wages lead to a temporarily higher NAIRU until workers’ expectations have adjusted? Or, is the effect much longer lived as expectations adjust very slowly or if the higher level of unemployment itself generates other labor market changes that perpetuate the higher joblessness.³ The literature contains a range of views on the subject. Blanchard and Wolfers (2000) treat the “shocks” such as slower productivity growth as having permanent effects, whereas Nickell et al (2001) explicitly model most of these shocks as having only a temporary impact.

The favorite candidate for a macroeconomic shock, or change in trend, is indeed slower productivity growth after 1973, which reduced the extent to which real wages could grow without reducing profitability. Table 2 shows the sharp deceleration in Total Factor Productivity (TFP) growth through the 1970s and first half of the 1980s.⁴ But this slowdown in pro-

ductivity growth could only contribute to explaining high unemployment in the 1990s if real wage bargaining was very slow to adapt.

The feasible growth of real wages also depends on the country's terms of trade. An increase in the real cost of imports relative to domestic output squeezes the feasible growth of real wage increases (Table 1 shows the impact of the terms of trade on living standards). The terms of trade of most OECD countries deteriorated in the first half of the 1970s and again in first half of the 1980s (associated with the two oil shocks) and this factor played a major role in the pioneering account of rising unemployment by Bruno and Sachs (1985). However as Table 2 shows, the terms of trade for OECD countries improved after the mid-1980s, so it would require very strong persistence mechanisms from earlier negative shocks for this factor to still be explaining high unemployment in the 1990s.

The "tax wedge" on average incomes means that real take-home pay is lower than the pre-tax real wage; if that wedge increases, then take-home pay and thus the feasible growth of real consumption grows more slowly. Changes in the tax wedge may affect not only the bargaining stance of organized workers but also individual labor-supply decisions, since a high tax level may decrease the incentive to work, particularly if unemployment benefits are generous. Table 1 indicates that there were substantial increases in the tax wedge in the 1970s, followed by relative stability in the 1980s and 1990s.

Finally, among the widely used macroeconomic variables is the real interest rate. High real interest rates may raise unemployment through several possible channels. Most obviously, higher real interest rates can increase unemployment by depressing aggregate demand. However the underlying cause of the higher unemployment could still lie elsewhere, with higher real interest rates simply being the weapon used by the authorities to ensure that unemployment adjusts to a rise in the NAIRU – which, for instance, may have occurred because of developments in the labor market, as suggested by Blanchard (1999). Secondly higher real interest rates may signal cases where the government deliberately pushes unemployment above the NAIRU in order to reduce the inflation rate. Finally, there are ways in which high real interest rates can affect the NAIRU itself. For example, higher real interest rates may push up profit-markups as firms seek to maintain profits after interest payments. Higher markups mean lower real wages and higher unemployment may then be required to achieve a corresponding reduction in wage pressure (a higher NAIRU). The pattern shown in Table 2 is

of modest real interest rates in the 1960s, very low or zero rates in the 1970s, followed by real interest rates averaging 5 percent or more through to the early 1990s and some decline at the end of the decade. So higher real interest rates could clearly help explain continuing high unemployment through the 1990s, though the difficulty in unraveling their causal role should be kept in mind.

Table 2: The Macroeconomic Background

	Total Factor Productivity Growth %pa		Impact of Terms of Trade %pa		Tax Wedge (% of Incomes)		Real Interest Rates %		Structural Budget balance % GDP	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
1960-64	4.0	1.6	0.7	0.6	19.2	5.5	2.1	1.4	Na	
1965-69	3.8	1.6	0.5	0.5	21.7	6.8	2.2	0.9	Na	
1970-74	3.1	1.3	-0.7	0.6	25.5	8.2	-0.2	1.9	0.6	3.1
1975-79	2.0	1.0	0.4	0.5	28.7	8.8	0.4	3.0	-2.6	3.6
1980-84	1.6	0.9	-0.5	1.2	31.0	7.7	4.4	2.2	-4.0	4.3
1985-89	1.5	0.9	1.3	0.9	31.4	7.7	5.2	1.4	-3.0	3.5
1990-94	1.5	0.9	0.7	0.7	31.9	7.6	5.9	1.6	-3.8	2.9
1995-99	1.5	0.9	0.1	1.0	31.9	7.5	3.7	1.4	-1.4	1.8

Source: see Appendix 2

The movements in the structural budget balance could also affect the unemployment rate, although the primary impact would be from a traditional Keynesian demand-side perspective. Other things equal, a smaller deficit would be associated with less demand and higher unemployment. Table 2 shows a large rise in the structural deficits in the seventies, and then a sharp falloff in the deficit in the nineties. The latter was associated with the Maastricht accord, which laid down strict deficit limits as a condition for being admitted into the euro zone. The rapid pace of deficit reduction required by this agreement could partially explain high unemployment in the nineties.

In considering the deficit figures shown in the table, it is important to keep in mind that they are based on an estimate of the deficit, *at the NAIRU*. This point is important, because if the NAIRU is itself misestimated, then the measures of the structural deficit would be wrong as well. Much of the increase in the structural deficit from the early seventies to the eighties coincided with a rise in the estimated NAIRUs for most countries. A higher unem-

ployment rate is of course associated with a higher deficit. If the NAIRU did not rise as much as the OECD assumed, then the structural deficits did not increase as much as is indicated in the table. In other words, the assessment of fiscal policy over this period is itself dependent on the view one holds of the NAIRU.

Institutions

A conventional set of “institutional variables” has been developed in the literature to capture various aspects of the labor market that affect either collective wage setting (for example union strength) or individual labor-supply conditions (such as active labor-market policy) or both (unemployment benefit levels, for example). Because of the constraints of data availability and comparability across countries, this set of measures is not usually claimed to be comprehensive.

Union strength is a notoriously difficult variable to capture quantitatively, and this problem is compounded in cross-country studies by the differing contexts. The most commonly cited variable is the proportion of employees in unions – union density. The data for the nineteen OECD countries considered (see Table 3) suggests modestly rising density from the early 1960s until the early 1980s. By the late 1990s average density had fallen back to its original level, though the variability in union membership across countries has considerably increased. In some countries, many more workers are covered by collective agreements than belong to unions, and this extension of union agreements should strengthen unions’ bargaining position. But data for the coverage of collective bargaining agreements is much patchier, especially for the earlier periods. The available data suggest a fairly small decline in collective bargaining coverage since the early 1980s.

Finally, much attention since Calmfors and Driffil (1988) has been devoted to the degree of centralization of bargaining, later adjusted to co-ordination in wage bargaining by unions and by employers (Soskice, 1990). A great deal of effort has been devoted to constructing internationally comparable measures of coordination, which also try to reflect variations within a country over time in bargaining practices (examples of this appear in later chapters, notably the cases of Ireland and Netherlands, two of the “success stories” of the 1990s). The most comprehensive co-ordination index, reported by Nickell et al (2001), suggests some slight increase up to the late 1960s, with a definite decline subsequently. This movement was

far from uniform however. In five countries (Ireland, Netherlands, Finland, Italy and Portugal) bargaining coordination is shown as increasing between the early 1980s and late 1990s.

Table 3: Measures of Union Strength and Bargaining Stance

	Union Density %		Bargaining Coverage %		Bargaining Coordination (1-3)	
	Mean	(SD)	Mean	(SD)	Mean	(SD)
1960-64	38.8	14.2			2.21	0.65
1965-69	39.1	14.2			2.21	0.64
1970-74	41.4	15.2			2.24	0.60
1975-79	44.8	18.0	71.4	21.9	2.29	0.50
1980-84	44.6	19.8	71.6	22.7	2.16	0.55
1985-89	42.2	20.3	70.6	23.8	2.03	0.57
1990-94	41.2	21.1	69.2	25.5	2.01	0.66
1995-99	38.7	22.5	68.5	27.2	1.92	0.65

Source: see Appendix 2

Overall, then, the bargaining variables tell a pretty consistent story, in which union strength and in bargaining coordination rise until the end of the 1970s or the early 1980s, followed by a rather modest decline on average. It is important to appreciate that the radical reductions in union strength seen in some countries (the United Kingdom and New Zealand, for example) are not typical for OECD countries.

Two measurable dimensions of unemployment benefits, the replacement rate and the duration of benefits, are widely seen as affecting labor supply decisions and therefore (voluntary) unemployment. The data in Table 4 show the average (pre-tax) replacement ratio for the first year of unemployment together with an index for duration based on the proportion of these benefits still being received in later years of unemployment. The average replacement ratio increased by one half between the early 1960s and late 1970s, after which there were further small increases before the hint of a decline at the end of the 1990s. The duration index shows a rather steady rise throughout the whole period. It is well recognized that these measures should be supplemented by data on eligibility, since the harshness of work-tests and other requirements vary widely across countries.

The second set of institutional variables is more focused on the microeconomic conditions in the labor market. First, there is employment protection legislation, which has many

dimensions (see OECD, 1999) and which is the central target in many discussions of labor-market flexibility. A high degree of employment protection is widely thought to inhibit hiring, though the parallel constraints on firing make the overall impact on unemployment somewhat ambiguous. The data in Table 4 suggest a steady rise in the index up until the early 1980s, after which there was a slow decline, reversing about one-third of the earlier increase.

Table 4: Measures of Labor Market Policies

	Employment Protection Legislation		Unemployment Benefit Replacement Ratio (%)		Duration of Benefits (index 0-1)		Active Labor Market Policies (% of GDP)	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
1960-64	0.79	0.62	28.0	11.1	0.34	0.40		
1965-69	0.85	0.61	31.0	15.8	0.35	0.39		
1970-74	0.99	0.59	34.6	18.2	0.37	0.35		
1975-79	1.09	0.59	43.2	20.3	0.42	0.34		
1980-84	1.11	0.59	45.4	21.4	0.45	0.34		
1985-89	1.11	0.58	48.1	21.9	0.46	0.33	0.78	0.48
1990-94	1.05	0.53	48.5	20.0	0.49	0.32	0.97	0.60
1995-99	.92	0.43	47.9	17.5	0.53	0.33	0.99	0.54

Source: see Appendix 2

Finally, unemployment rates may be affected by a range of active labor market policies, which include the provision of information, counseling and training to the unemployed. The OECD has gathered data on expenditure on these policies since the mid 1980s. Table 3 shows some increase in this spending in the 1990s and with divergences between countries tending to increase.

Assessment

This discussion of the patterns institutions and macroeconomic background suggests plenty of candidates that could help to explain the trend in OECD unemployment over recent decades. The macroeconomic environment deteriorated after 1974 with consistently slower TFP growth and periodic terms of trade shocks. Higher real interest rates took over as a depressing factor in the 1980s, with a modest reversal at the end of the period. The continued rise in the share of taxation until the early 1980s put further pressure on take-home pay. Union strength increased noticeably until the end of the 1970s, after which there was some reversal, but this coincided with a declining degree of co-ordination within the bargaining process, which could have brought adverse bargaining outcomes including higher unemployment. Em-

ployment protection legislation strengthened until the early 1990s, after which there was a partial reversal; replacement ratios rose till the late 1980s; and the duration of benefits seems to have increased rather steadily. Rising inflation up till the late 1970s is consistent with the view that unemployment was somewhat below the NAIRU in the 1970s. The trend towards lower inflation thereafter suggests actual unemployment somewhat above the NAIRU.

This broad story has appealed to a wide range of economists approaching these issues from a variety of perspectives (see Bruno and Sachs (1985), Armstrong et al (1984 and 1991), Layard, Nickell and Jackman (1991, 1994), OECD (1994) and Siebert (1997)). However, as noted above, there are significant differences within the empirical literature over the exact ways in which different sources of labor market rigidity are believed to increase unemployment rate. The third section of this paper examines this literature in more detail. In order to provide background for this more detailed empirical work, the next section relates unemployment levels for OECD countries to six standard measures of labor market institutions and policies.

2. Institutions and Unemployment: An Initial Look

If a “direct link” exists between labor market institutions and policies and unemployment (OECD, 1999, p. 52-3), a first place to look for it is in the simple correlations between the variables. Figures 1-6 present scatter plots of six conventional measures of institutions against the OECD’s standardized unemployment rates for twenty countries (see Appendix TK for definitions and sources). Since these institutional measures tend to show little annual change and we are interested in longer-term determinants of the pattern of unemployment, the data are organized into five-year averages – a common practice in this literature (see Nickell, 1997; Elsmekov, et.al., 1998; and Blanchard and Wolfers, 2000). We focus on the last two decades (1980-84, 1985-89, 1990-94, and 1995-99), the period during which unemployment reached extremely high levels in many OECD member countries.

We begin with the two commonly used, OECD-derived, measures of the generosity of the unemployment benefits system, the replacement rate (the level of benefits relative to earnings) and the index of duration of benefits for which averages across countries were shown in Table 4 in the previous section. Among the institutions held to have the greatest adverse employment effects, these measures of benefit generosity are also among the least controversial.

As a follow-up report to the Jobs Study (OECD, 1997, p. 52) puts it, “There is broad consensus that unemployment rates across time and countries are related to the generosity of income support available to the unemployed.” It is worth noting that there may be a problem of reverse causation in simple tests of association between unemployment and unemployment benefit generosity, since governments are likely to respond to higher unemployment with greater generosity of benefits.

Despite both the widely accepted view that unemployment benefit generosity lies at the heart of the unemployment problem and the likelihood of at least some reverse causation, Figures 1 and 2 show virtually no association between the standard measures of unemployment benefit generosity and unemployment over the 1980-99 period. Figure 1 shows a slight positive relationship between the unemployment rate and the replacement rate (the coefficient of the regression line is not statistically significant at the 10-percent level [$t=1.5$] and this measure accounts for less than 3 percent of the variation in the unemployment rate over these twenty countries and four periods). Spain is an outlier, and without it, the trend line is absolutely flat. Directly below the four Spanish points are those for Sweden; while both countries had similar replacement rates (ranging from 65-76 percent), the five-year average unemployment rates in Spain ranged from 16-20 percent while Swedish unemployment rates ranged from 2-8 percent. Another example of the lack of correspondence between replacement rates and unemployment can be seen with France and the Netherlands. While French replacement rates were about 58 percent from 1980-99 and Dutch rates were much higher, at 70 percent, French unemployment rose from 8 to 12 percent while Dutch unemployment fell from 8 to 5 percent. Nor do differences in the duration of benefits appear to explain the perverse (from the Rigidity View perspective) replacement rate-unemployment trends for France and the Netherlands – the duration of benefits was substantially higher in the Netherlands for the first three periods (1980-95) and about the same in the last (1995-99).

Indeed, as Figure 2 shows, there is also no simple association between unemployment benefit duration and unemployment levels across these twenty countries and four time periods. With similar unemployment rates, New Zealand (1.04) and the U.S. (.15 -.19) are at opposite ends of the spectrum on this measure of duration. On the other hand, the quintessential welfare state, Sweden, with a strong commitment to active labor market policies (training and job placement services), gets a duration score (.04-.05) that is far smaller than that of even the

United States. Spain's duration score since 1985 (.25-.28) is slightly above that of the United States, but far below that of the United Kingdom (.70-.73); nevertheless, Spain has had unemployment rates two to three times higher than the United Kingdom (20.1 vs 7.3 for 1995-99).

An index of the strength of employment protection laws is plotted against unemployment in Figure 3. An OECD survey (1999) found that "empirical results are somewhat mixed. . . ." Bertola (1992), Nickell and Layard (1997), and OECD (1999b) were unable to find a statistically significant relationship between EPL and the unemployment rate. . . . This is, indeed, precisely what Figure 3 indicates. With similar unemployment rates, at least through 1994, Sweden and Portugal had far higher EPL scores than the United States. Spain, however, had far higher official unemployment rates than Portugal (and Sweden) despite similar EPL scores.

Figures 4 and 5 present plots of union density and bargaining coordination against unemployment, both of which again show no statistically meaningful relationship. As the OECD (1999, Box 2.3, p. 55) concludes, "Notably there is little evidence of an effect of union density . . . on unemployment once other features of the collective bargaining system are taken in to account." In fact, our Figure 4 shows no effect even *without* taking these features into account. One of the key collective bargaining features is coordination, which appears in Figure 5. Bargaining coordination is often found to be among the stronger variables in cross-country multivariate tests – the more coordination (greater institutional intervention) the *lower* the unemployment rate. Our simple plot does not indicate this for the full set of country-time points, but it is worth noting that both Ireland and the Netherlands do show both greater coordination and lower unemployment over time.

On the role of taxes, the OECD (1999, Box 2.3, p. 55) concludes that "Recent studies seem to suggest a significant effect of taxes on labor on unemployment. . . ." Again, no simple bivariate relationship appears in our data. Figure 6 shows that Sweden had extremely high tax levels and relatively low unemployment (although it increased substantially in the 1990s), whereas Spain reports fairly low taxes but extremely high unemployment. Ireland is again of interest: relatively low taxes and very high unemployment, which fell sharply in the last half of the 1990's. France and Belgium also show high and rising unemployment at the same time that tax levels were relatively high and rising. So while no cross-country relationship appears in Figure 6, the tax-unemployment relationship, like that of coordination, may be more con-

sistent with conventional expectations (coordination lowers unemployment, taxes raise it) over time for particular countries (effects which are picked up in the multiple regressions discussed below once country dummies are included).

In sum, Figures 1-6 offer no hint that labor market institutions and policies could explain even a small part of the post-1980 pattern of unemployment for these nineteen countries.

**Fig. 1: The Unemployment Benefit Replacement Rate and Unemployment, 1980-99
(20 countries, 4 five-year periods)**

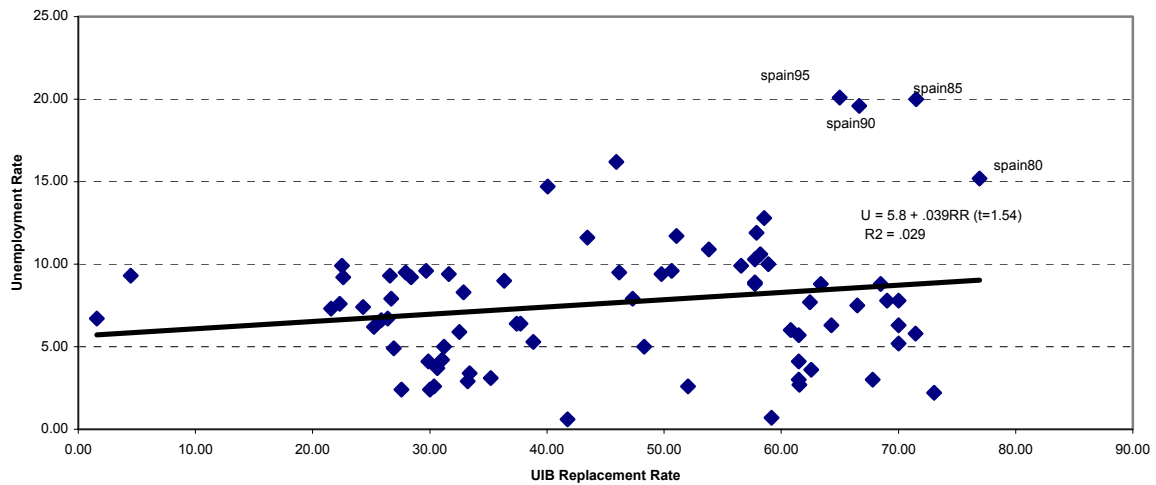


Fig 2: Unemployment Benefit Duration and Unemployment, 1980-99
(20 countries, 4 five-year periods)

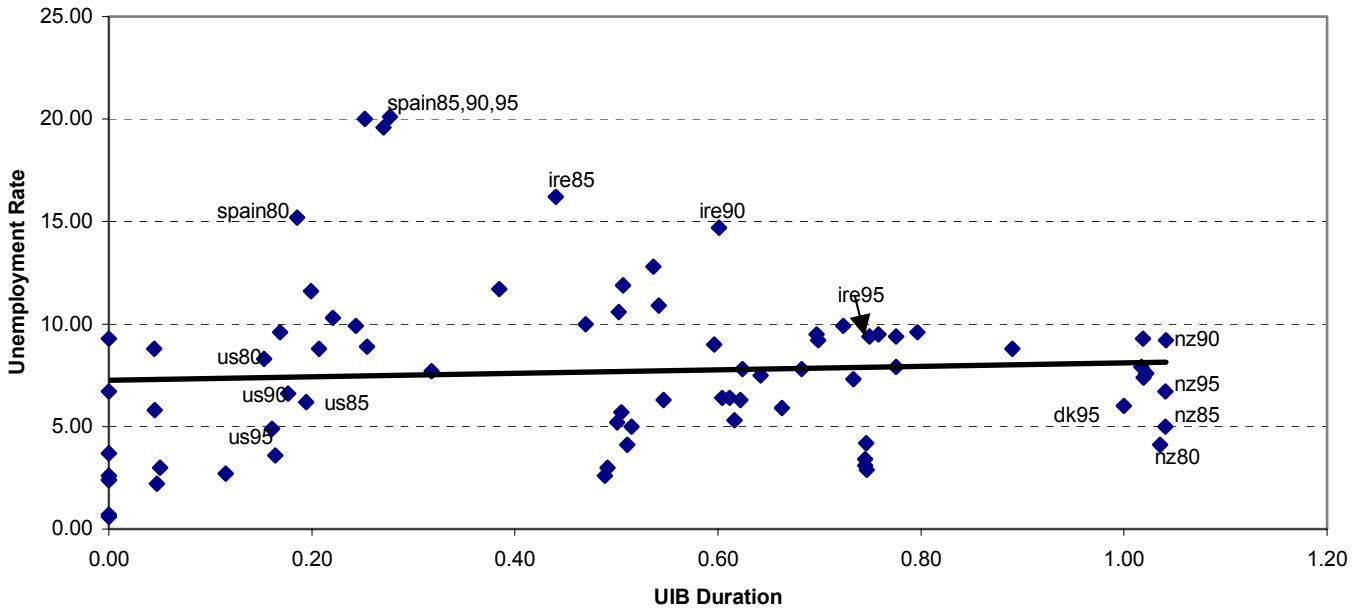


Fig 3: Employment Protection Laws and Unemployment, 1980-99
(20 countries, 4 five-year periods)

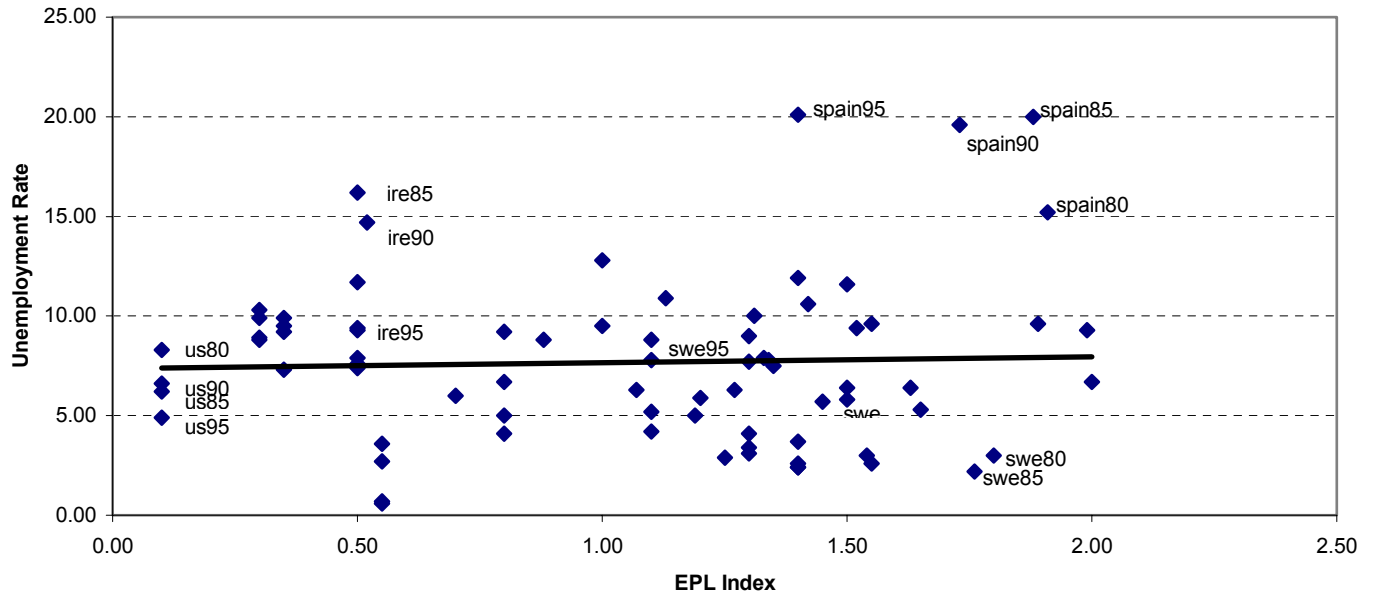


Fig 4: Union Density and Unemployment
(20 countries, 4 five-year periods)

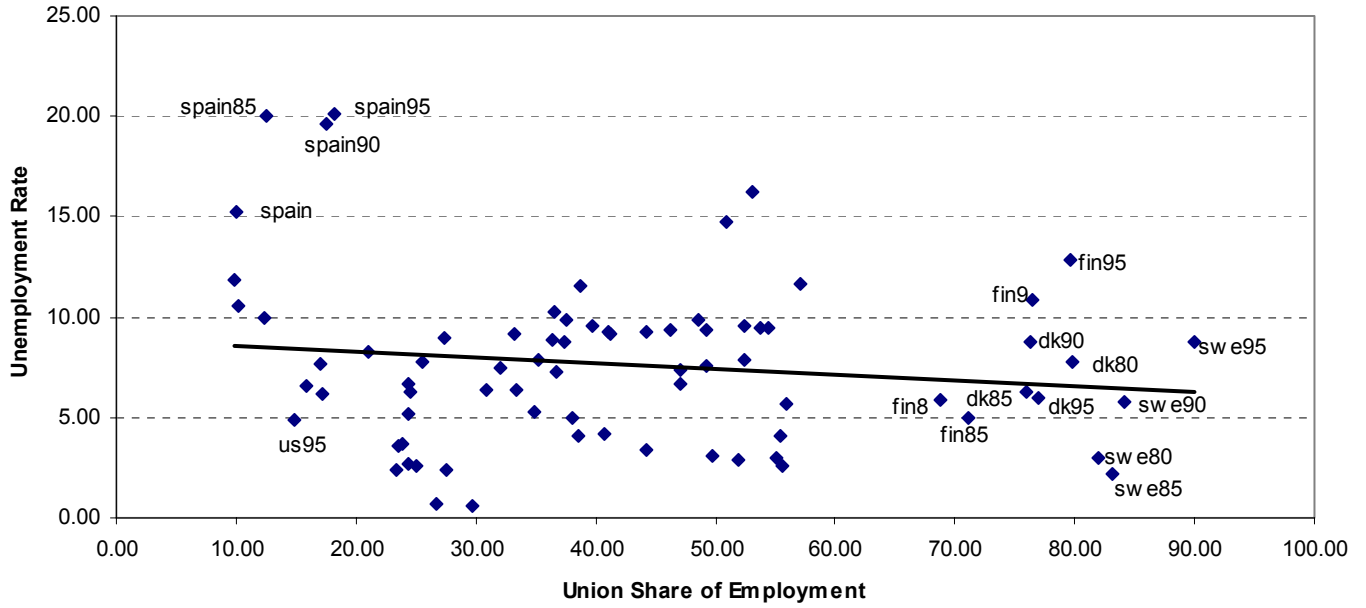


Fig 5: Bargaining Coordination and Unemployment
(20 Countries, 4 five-year periods)

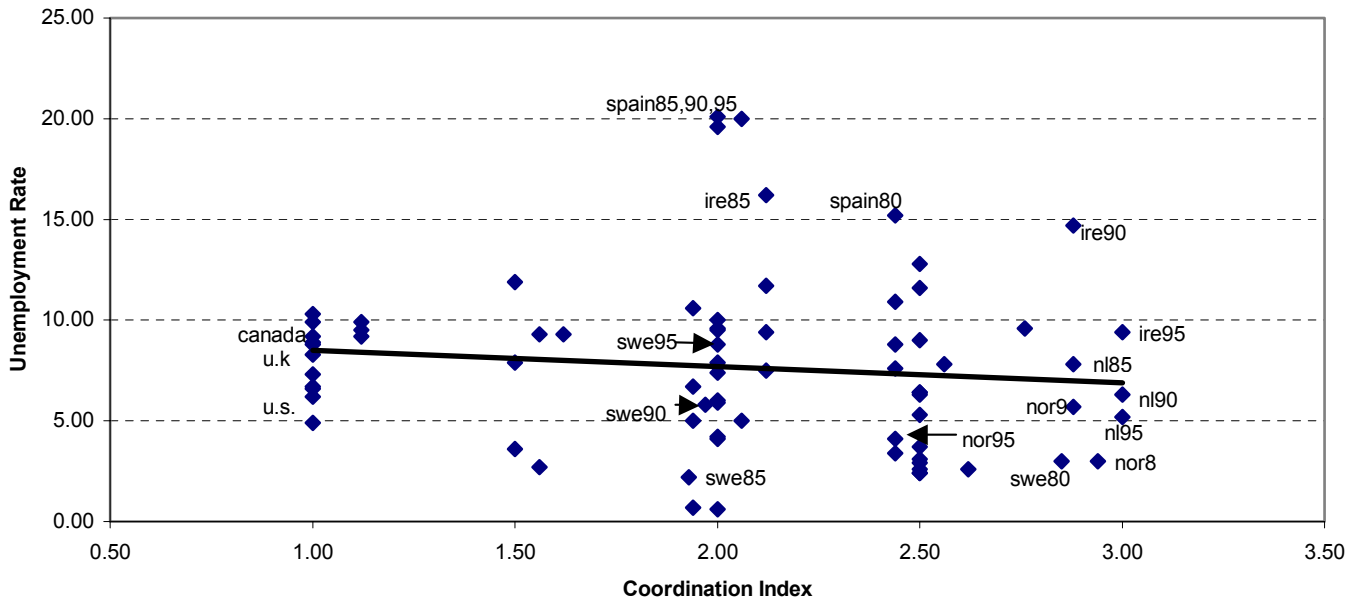
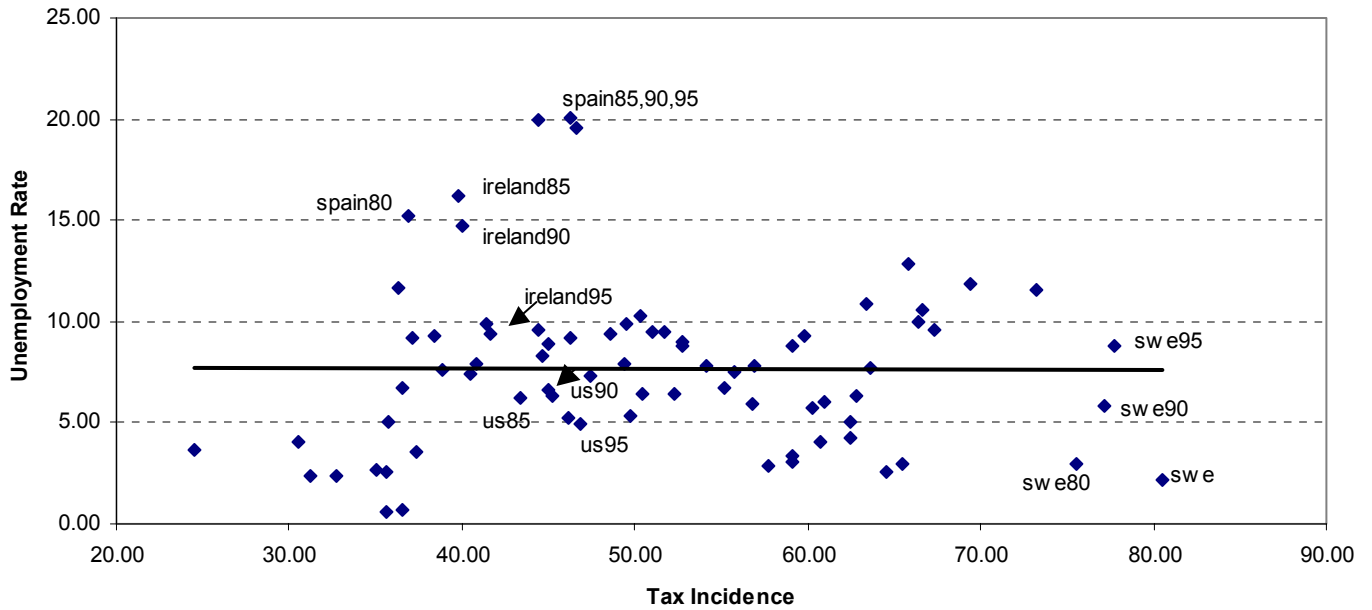


Fig 6: Taxes and Unemployment, 1980-99
(20 Countries, 4 five-year periods)



We conclude this section by focusing on the relationship between labor market deregulation (sometimes referred to as “structural reform”) and declining unemployment rates for some countries in the mid to late 1990s. The OECD (1999) claimed a strong link between labor market deregulation and the extent to which structural unemployment (the NAIRU) fell in the 1990s. They used the degree to which a country complied with their policy prescriptions (such as the reduction of replacement ratios) as their indicator of labor market reform. But this would imply that, ignoring different weights attached to different measures, one policy proposal fully acted represents twice as much reform as ten measures half followed.

Figure 7: Change in the NAIRU and Labor Market Deregulation for 20 Countries in the 1990s

Source: change in NAIRU: OECD Economic Outlook June 2002; the index of deregulation measures authors' calculation based on measures listed & weightings in OECD (1999) Appendices. Note: the measures considered cover unemployment benefits, wage formation and EPL and working time arrangements.

A more appropriate measure would be the “volume” of reforms, so in the example above ten reforms half carried out would be worth five times the one reform that was fully implemented. Such an index of the extent of labor marker deregulation can be constructed from the OECD's listing of their reform proposals, their weighting of the comparative importance of different policies, and their tabulation of the extent to which each reform suggestion had been followed (OECD 1999). We confined the index to the core labor market deregulation proposals concerning employment protection, unemployment benefits and wage determination (leaving aside taxation levels and ALMP). Figure 7 plots this index of labor market deregulation against the OECD's latest estimates of changes in the NAIRU over the 1990s. As the figure indicates, there is no significant relation between the extent to which countries took policy actions to reduce labor market regulation and shifts in the NAIRU. This conclusion holds even if the outlying case of Ireland is excluded (which would be hard to justify).

Despite the evidence reported in these seven figures, the conventional wisdom has remained firmly entrenched: the rigidities imposed by labor market institutions are the center-

piece of the explanation for the persistence of high levels of unemployment in the OECD. The empirical evidence for this claim rests largely on research characterized by increasingly complex multivariate tests. We survey this literature in the next section.

3. Institutions and Unemployment: The Recent Cross-Country Literature

Since the late 1980s a considerable literature has developed on the extent to which differences in unemployment rates between nations and over time can be explained by labor-market institutions. This section examines some of the most influential of these studies. We do not present an exhaustive review of the literature. Our goal is to present some of the main findings of this research and to highlight the main methodological issues that have arisen.

To facilitate our assessment of these papers, Table 5 presents the results from six representative studies of a set of regressions designed to measure the relationship between labor market institutions and unemployment. The key differences in the structure of the regressions are noted below.⁵ The construction of the variables, which differs somewhat across regressions, is explained more fully in the appendix.

Nickell

Building on his earlier work with Layard and Jackman (1991), Nickell (1997) lays out a clear and simple framework for examining the link between institutions and unemployment with a sample of 20 OECD countries for two six year periods, 1983-88 and 1989-1994. The study calculates the average rate of unemployment, long-term unemployment, and short-term unemployment for each country in each period, which then appear (in log form) as the dependent variables in a set of regressions.⁶ The independent variables intended to capture the impact of key labor market institutions and regulations are employment protection (rank 1-20), the replacement rate (percent of working wage), unemployment benefit duration (years), active labor market policy (spending per unemployed worker as a percentage of GDP per employed worker), union density (percent), union coverage (index 1-3), bargaining coordination (2-6), and the total tax rate (percent). The regressions also include the change in the average inflation rate during the period and a dummy for the second time period. The regressions were run using generalized least squares, allowing for random effects of country variables.

The first column of Table 5 shows the projected impact of the specified changes in the institutional variables, based on Nickell's regression results. Since the dependent variable is the log of the unemployment rate, rather than the unemployment rate itself, the results imply that the effect of each institution will be proportionate to the unemployment rate in a particular country at a given point in time. The results show the effect on a country that has an 8 percent unemployment rate, approximately the average for the sample period. All of the variables are significant with the expected sign, with the exception of employment protection legislation.

The implied impact of the hypothetical changes in some of the institutional variables on the unemployment rate is quite large. For example, the regression results imply that an increase of 1 unit in the 3-unit index used to measure union coverage would lead to a 3.6 percentage point rise in the unemployment rate. Similarly, the results imply that an increase of 1 unit in the 4-unit index measure of bargaining coordination would lead to a 3.7 percentage point drop in the unemployment rate. The estimated impact of active labor market policy also appears quite large, with an increase of 10 percentage points in the measure of spending on active labor market policy leading to a 2.4 percentage point drop in the unemployment rate.⁷ The impacts of the other labor market institutions implied by the regression results are also substantial, although for the policies changes specified in the table, they are smaller than for these three measures.

Despite the apparent strength of these results, Nickell's interpretation is cautious, commenting at the outset that the labor market rigidities explanation for high European unemployment "is not *totally* wrong." His concluding discussion points out that many of the institutional features that are thought of as labor market rigidities are no more prevalent among the group of high unemployment countries than among the low unemployment countries. He also points out that some of these features, such as bargaining coordination, appear to reduce unemployment. The paper closes with the warning that, "the broad-brush analysis that says that European unemployment is high because European labor markets are too 'rigid' is too vague and probably misleading."

Elmeskov, Martin and Scarpetta

“Key Lessons for Labor Market Reforms” by Elmeskov, Martin and Scarpetta (1998) (hereafter, EMS) is an assessment of the effectiveness of the recommendations from the OECD Jobs Study [OECD, 1994] by three OECD economists. The tests run by EMS are characterized by several important methodological differences with Nickell (1997), the most significant of which is that it uses annual data, which is central to the purpose of the paper: explaining the recent declines in unemployment rates in many OECD nations. EMS also use a different data set, relying on OECD measures for the labor market institutions. The second column of table 5 shows projections of the impact of the specified changes in the institutional variables based on the regression results from the study.

EMS’s results differ from those obtained by Nickell (1997) in several noteworthy ways, even though the period covered is almost identical. They find a large significant positive relationship between employment protection and unemployment. The results indicate that an increase of 4.3 units (one standard deviation) on an index with a possible range from 0 to 18, is associated with 1.4 percentage-point rise in the unemployment rate, which contrasts to Nickell’s finding that there was no relationship between employment protection legislation and the unemployment rate. This result may reflect the fact that the employment protection legislation index in EMS is quite different from the one used in Nickell. The EMS measure assigns values for several different features of the employment protection legislation, while the Nickell measure is a simple ranking. Similarly, the coefficients of the coordination variables, while highly significant, are considerably smaller in absolute value than the estimates reported in Nickell. This result could be in part attributable to the difference between the OECD coordination index and the Nickell index. EMS also use a separate index of centralization, which refers to the level at which bargaining takes place (firm, industry, or nation-wide).

Unlike Nickell (1997), EMS do not find a statistically significant relationship between union density and unemployment, and their estimate of the impact of taxation is only half as large. In other tests, EMS estimate the impact of bargaining coordination more carefully, distinguishing between coordination among workers and employers and centralization in the bargaining process. They also interact these measures with other labor market variables. A finding that shows up in a variety of specifications is that countries with intermediate levels of coordination and centralization tend to have the highest rates of unemployment, and that coun-

tries with highly coordinated and centralized bargaining tend to have even lower unemployment rates than those with the most decentralized and least coordinated bargaining systems. The regressions with interacted terms also show this pattern (EMS, 1998, tables 2 and 4). Through both direct and interacted effects, their results show that the unemployment increasing effects of employment protection legislation and taxes are concentrated in countries with an intermediate level of coordination.

The use of annual data allows for a test of Granger causality from unemployment rates to benefit generosity and the tax wedge. The causality issue is important because if countries are raising their benefits as a result of high unemployment, or increasing taxes to cover the cost of providing benefits to a larger population of unemployed workers, this *reverse causation* might also result in a significant relationship between higher benefits or tax rates and higher unemployment. While the authors make little note of it, the reported test results show solid evidence of Granger causality from higher unemployment to higher unemployment benefits for three of the countries with high levels of unemployment during this period, Belgium, France, and Italy, as well as for two countries with lower unemployment levels, the United Kingdom and the United States (EMS, 1998, table A.3). They also find evidence for Granger causality from higher unemployment to higher tax rates in three of the nineteen countries examined. While clearly not universal, this evidence of reverse causation provides serious grounds for viewing test results showing a correlation between high unemployment and long benefit duration with caution.

Using their regression results, EMS examine the extent to which changes in the unemployment rates in the OECD countries over this period can be explained by the changes in labor-market institutions. They find that for most countries, the vast majority of the change in the unemployment rate can be attributed to country-specific effects rather than any identified change in labor market institutions (EMS, 1998, Table 3). For example, according to their estimates, institutional changes can account for only 0.3 percentage points of a 2.1 percentage point drop in the structural rate of unemployment in Ireland, 1.3 percentage points of a 4.2 percentage point increase in the structural unemployment rate in Sweden, and -0.2 percentage points of a 2.2 percentage point rise in the structural unemployment rate in Spain. EMS explicitly acknowledge this limitation of their model: “it should be stressed at the outset that an

important fraction of the structural change in unemployment cannot be accounted for by changes in the explanatory variables included in our analysis” (p. 11).

Yet, in spite of these rather weak findings, particularly in comparison with Nickell (1997), EMS are much less cautious and strongly argue for the importance of labor-market institutions in the explanation for high unemployment in the OECD. They conclude by urging nations to reform their labor markets along the lines recommended by the OECD:

“Some of the medicine prescribed under the OECD recommendations is bitter and hard for many countries to swallow, especially insofar as it appears to raise concerns about equity and appears to threaten some of the rents and privileges of insiders. As a result, there is a natural tendency in many countries to delay needed reforms in certain areas and/or search for alternative, ‘sweeter’ remedies. It requires strong political will and leadership to convince electorates that it is necessary to swallow all of the medicine and that it will take time before this treatment leads to improved labor market performance and falling unemployment. But the success stories show that it can be done!”

Belot and van Ours

Belot and van Ours (2002) extend the approach of EMS (1998) by exploring a wider set of interactions between variables. They also extend the period of analysis, using five-year periods from 1960 to 1996. The study reports the results of four regressions that test the direct impact of institutions on unemployment over this period. The only regression in which most of the direct effects of the institutional variables have significant coefficients with the expected sign does not include time or country fixed effects. In this “successful” regression, the coefficient for the tax rate, the replacement rate and union density variables are all positive and statistically significant, as the conventional labor market rigidity view predicts. On the other hand, the coefficients on the coordination and employment protection variables are negative and significant, the latter being a perverse result for the conventional view, since it implies that employment protection legislation lowers the unemployment rate.

The authors then present the results of a number of alternative specifications. When fixed effects and a time variable are included, the coefficients of all the institutional variables become insignificant, although the coefficient for the change in the inflation rate is negative and significant in every specification. In a regression that includes interacted variables, a positive and significant interaction is found between the tax rate and the replacement rate, implying that taxes will have a larger effect on the unemployment rate if the replacement rate is

high, and that raising the replacement rate will have a larger impact on the unemployment rate if the tax rate is high. Both high replacement rates and high tax rates reduce the gap between take-home pay and the benefits available to unemployed workers. The implication of this finding is that increasing either the replacement rate or the tax rate has a larger impact on the unemployment rate if the other one is already high, meaning that the gap between take home pay and unemployment benefits is already low.

Ultimately these regressions produce largely inconclusive results about the impact of the interactions. For example, an interaction variable between employment protection and centralized bargaining has a significant positive coefficient in one regression (table 7, column 5), but the coefficient of the employment protection variable becomes negative and significant in a regression in which bargaining is held at a decentralized level (table 7, column 6). The coefficient of an interacted union density variable and centralization variable is negative and significant in one regression (table 7, column 5), implying that higher unionization rates are associated with lower levels of unemployment. However, this coefficient also changes sign in the case of decentralized bargaining. The only variable to have a significant coefficient for its direct effect in the preferred regression (table 7, column 5) is the replacement rate. However, the estimated effect of the replacement rate is negative, suggesting the surprising conclusion that in low tax countries, raising the replacement rate would actually lead to lower unemployment (table 4, column 3).

Like Nickell (1997), and in sharp contrast to EMS, Belot and Van Ours are cautious in their interpretation of these results. They conclude by noting that “institutions matter and institutions interact” (p 18), warning that policies that lead to lower unemployment in some countries may not have the same effect on countries with a different institutional structure.

Nickell et al.

Like Belot and van Ours (2002), the Nickell et al. (2002) study tries to explain trends in unemployment rates in the OECD over the longer period, in this case from 1961 to 1995. But like EMS, this paper uses annual data and takes into account the interactions between institutions. The interacted institutions include coordination and employment protection, benefit duration and the replacement rate, coordination and union density, and coordination and the tax rate. Like Blanchard and Wolfers (2000), this study also measures the effects of several

macroeconomic shocks, including changes in labor demand, total factor productivity growth, real import prices, the money supply, and the real interest rate.⁸ Nickell et al also look at a broader set of labor market outcomes, including regressions for the inflow into unemployment (proxied by short-term unemployment), real compensation growth, and employment-to-population rates as dependent variables, in addition to the regressions with the unemployment rate as the dependent variable.

Nickell et al. are quite explicit about their goal: “our aim is to see how far it is possible to defend the proposition that the dramatic long term shifts in unemployment seen in the OECD countries over the period from the 1960s to the 1990s can be explained by changes in labor market institutions in the same period” (p. 1). This is clearly a far less agnostic starting point on the importance of labor-market institutions than the position taken in Nickell’s earlier paper (1997).

Before directly assessing the study’s results, it is worth noting several unusual features of the Nickell et al. analysis. First, the regressions in this paper all include country specific time trends. While none of these time trends are close to being statistically significant, they do explain a large portion of the changes in unemployment over this period. This point may not be readily apparent because the estimated coefficients are generally small (the largest in absolute value is Portugal’s -0.107). But, because the regressions include a lagged dependent variable that is estimated at between 0.86 and 0.87, the full effect of the time trend is more than seven times what it would be in the absence of the lagged dependent variable. This means, in the case of Portugal, that its country specific time trend implies a decline in its unemployment rate of 9.6 percentage points over a twenty-year period. While the estimated time trends for the other countries are smaller, eleven of the twenty countries have time trends that imply a change in the unemployment rate of at least 3.5 percentage points over a twenty-year period. One implication of these estimated time trends is that, if institutions had remained unchanged, the unemployment rate would have been *negative* in several countries by the end of the period. By definition, the changes in the unemployment rate attributable to the time trends are independent of the labor market institutions included in the regression.

A second notable feature is that, while most of the cross-country studies have used data grouped in five or six-year periods, Nickell et al. make use of annual data. There must be some question about how much extra precision is really bought with the apparent increase in

degrees of freedom gained by using annual data, especially when most of the relevant institutions change slowly and several of the institutional measures are essentially interpolated from a few benchmark observations (especially EPL and coordination).

Nickell et al find that most of the institutional variables and the macro shock variables are statistically significant with the expected sign. The replacement ratio, benefit duration, and employment tax variables all have positive significant coefficients in the two unemployment regressions that appear in the paper (see column 4 of table 4). However, both the employment protection and union density variables have insignificant coefficients. The size of coefficient of the tax variable is comparable to the estimates in the other models, however, the estimated impact of the benefit duration is much larger than in the other studies, and the impact of the replacement rate is considerably smaller.

Consistent with much of this literature, Nickell et al find that bargaining coordination is negative and significant in both of the unemployment regressions. But the effects implied by their coefficients are far too large to be plausible, implying that the direct effect of an increase of one unit in the coordination index is associated with an 6.4 – 7.2 percentage point decline in the unemployment rate (table 13, columns 1 and 2). Taking interactions into effect, the total decline in the unemployment rate resulting from an increase in bargaining coordination implied by these regressions would be even larger (since interaction terms with employment protection and union density both had highly significant negative coefficients). The coefficients of the interacted variables also show that coordination offsets much of the impact of taxation in raising the unemployment rate, and combined with higher union density leads to a lower unemployment rate. The interaction terms also show that higher replacement rates amplify the impact of benefit duration in raising the unemployment rate (or vice versa), an effect also found in Belot and van Ours (2002).

The impact of the macroeconomic variables is largely consistent with standard theory. The labor demand shock, total factor productivity shock, and real import price shock variables were all highly significant with the expected signs. However, the money supply shock term was not significant. The real interest rate variable was significant with the expected sign, but the estimated impact is small.⁹

Nickell et al. find that only the coefficients of the replacement rate and employment tax variables are significant in regressions that have the employment to population [EPOP]

rate as the dependent variable. The coordination variable is again highly significant with a positive coefficient, suggesting that greater coordination increases the EPOP. While the study correctly notes that the determinants of the EPOP and unemployment rates are likely to be different, the inclusion of country fixed effects should account for much of these differences. Insofar as the institutional variables included in this analysis affect the unemployment rate in a way that has no effect on the EPOP, the impact of changes in these institutions is substantially different than is generally recognized. Specifically, the implication is that a change in a labor-market institution that lowers the unemployment rate (e.g. weakening employment protection legislation) will not increase employment levels. Instead, it will simply encourage people to leave the labor market altogether. While Nickell et al. make little of this result, it can be interpreted as challenging the conventional rigidity view, since there seems little reason to weaken labor-market protections if the main outcome is to drive people out of the labor force, rather than increase the percentage of the work force who hold jobs.

We should also point out that this paper has been revised, and that there are some notable differences between the results presented in the 2002 version and those that appear in the earlier (2001) draft. The main difference seems to be that the more recent one extends the data from 1992 to 1995. In the 2001 version, the employment protection legislation variable was highly significant in all three of the published unemployment regressions (table 13) and quite large in its economic impact. In contrast, the coefficient of this variable in the regressions in the more recent version is not close to being significant. The additional three years also seems to have a substantial affect on the impact of other variables. In the 2002 version, the effect of higher taxes is more than 30 percent lower, the effect of coordination is nearly 40 percent lower, and the effect of benefit duration is cut by more than 50 percent. The additional three years of data also now make the coefficient of the interest rate variable significant. It had been very close to zero and not close to significant in the earlier regressions. In the EPOP regressions in the earlier version, only the replacement rate and benefit duration variables were found to have significant negative effects and the employment tax variable was not close to being significant.¹⁰ The fact that the inclusion of three additional years leads to substantial changes in the regression results raises serious questions about the robustness of the conclusions.

Nevertheless, Nickell et al. conclude that their results show that “broad movements in unemployment across the OECD can be explained by shifts in labor market institutions.” Indeed, they contend that “with better data, e.g. on union coverage or the administration of the benefit system, we could probably generate a more complete explanation. To be more precise, changes in labor market institutions explain around 55 percent of the rise in European unemployment from the 1960s to the first half of the 1990s” (p. 19). Much of the rest of the increase is attributed to the recession of the early nineties.

Blanchard and Wolfers

An innovation of Blanchard and Wolfers (2000) is their emphasis on the interaction of institutions with macroeconomic shocks, represented by the slowdown in total factor productivity growth, trends in long-term real interest rates, and shifts in labor demand. These macro-institution interactions are central to the study, since Blanchard and Wolfers, in direct contrast to Nickell et al. 2001, explicitly rule out the possibility that institutions alone, or the change in institutions over time, can explain the evolution of OECD unemployment. The authors point out that the same supposedly employment-unfriendly institutions were present in the seventies when the unemployment rate was low. As Blanchard and Wolfers put it, “while labor market institutions can potentially explain cross country differences today, they do not appear able to explain the general evolution of unemployment over time” (p. 2). The authors instead pursue the hypothesis that certain labor market institutions inhibit the ability of economies to respond to adverse shocks, thereby leading to higher unemployment.

The Blanchard-Wolfers study uses eight five-year periods from 1960 to 1996 (the last two years are treated as a full period). In some regressions, some of the institutions vary over the period, but in most cases labor market institutions are held fixed. The regressions use non-linear least squares to estimate the coefficients of the interaction terms, allowing for the simultaneous estimate of coefficients for the macro shock terms and the institutional variables.¹¹

The results provide some evidence for the proposition that labor market institutions, in the presence of adverse shocks, lead to higher unemployment. Column 5 of their Table 5 shows the impact of differences in labor-market institutions on unemployment assuming that the values for the macroeconomic shocks variables were at their levels for the period 1991-1995. Most of the coefficients are significant and have the expected sign. The sizes of the im-

plied effects are generally comparable to those in Nickell (1997), EMS, and Nickell et al. (2001), although the impact of differences in employment protection legislation variable and taxes are somewhat smaller than in the other studies. The results imply that even a ten-percentage-point rise in the tax rate would lead to just a 0.9 percentage point rise in the unemployment rate.

Blanchard and Wolfers hold that their results provide support for the view that the combination of macroeconomic shocks over the last three decades with the rigidity in the labor markets in some countries helps to explain both the general increase in the unemployment from the 1960s to the 1990s and the variation across countries. However, the study also notes that their findings are sensitive to changes in specification. For example, in a regression that uses alternative measures of the replacement rate, the employment protection and tax wedge variables become insignificant, while union density is only significant at a 10 percent confidence level (Blanchard and Wolfers, 2000, table 6, column 1). Further, when a time-varying measure of the replacement rate is used (as it clearly should be), all three of these variables become insignificant, as do the replacement rate variables (table 6, column 2). In regressions that use an alternative or time-varying measures of employment protection, this variable is insignificant, although the replacement rate, benefit duration, tax wedge, and union density variables are all highly significant (table 6, columns 3 and 4). It is worth emphasizing that only bargaining coordination (a “good” labor market institution) has a significant coefficient in every regression, regardless of the specification.

The fact that the inclusion of time-varying institutions weakens the results leads Blanchard and Wolfers to be cautious in assessing their evidence about the links between institutions and unemployment. They conclude by noting that institutions are becoming more “employment-friendly,” and that “further improvements should help reduce unemployment – although the poor results obtained using time-varying institutions make us reluctant to push this position strongly, at least based on the evidence in this paper” (p 19).

Fitoussi, Jestaz, Phelps, and Zoega

Like Blanchard and Wolfers (2000), Fitoussi et al. (2000) try to explain unemployment with a model that emphasizes the interaction of macroeconomic shocks and institutions. More specifically, they run tests with country fixed effects, a country-specific persistence pa-

parameter (which measures the persistence of unemployment levels through time), a country-specific sensitivity parameter (which measures the extent to which the unemployment rate responds to contemporaneous shocks), and a series of macroeconomic shocks over the last three decades. Although Fitoussi et al. view their results as confirming the theory that the interaction of shocks and institutions explains much of the variation in unemployment rates over the last three decades, there are problems with this conclusion, some of which are noted in the study. The biggest problem is that the estimates of the sensitivity parameter, which is supposed to measure the extent to which shocks lead to higher unemployment, are highest in the countries which are thought to have relatively few labor-market rigidities. For example, the United States, United Kingdom, Canada, Ireland, and the Netherlands are five of the seven highest ranking countries by the estimated sensitivity parameter (Fitoussi, et al, table 2). This implies that macroeconomic shocks lead to more unemployment in these five countries than they do in most other OECD countries. This result appears to be directly at odds with the view that the interaction of macroeconomic shocks with the labor-market rigidities characteristic of continental Europe is the source of high European unemployment.

The paper examines the extent to which the country fixed effect and the country-specific sensitivity parameter can be explained by differences in labor-market institutions. Fitoussi et al find that the replacement ratio (albeit very small), union density, and union coverage have positive and statistically significant effects on the size of country-specific fixed effect, which should imply that they lead to higher unemployment, whereas coordination has a negative and statistically significant effect. Benefit duration and union density have a positive and statistically significant effect on the size of the country-specific sensitivity parameter, while coordination and active labor-market policies have a statistically significant negative effect. The other labor-market variables are insignificant in these regressions. Given the weakness of these results, these somewhat unusual regressions can be seen as, at best, providing only modest support to the labor-market rigidities view.

The authors then present regressions that test more directly the extent to which changes in labor-market policies, monetary policy, and national differences in asset prices explain trends in unemployment in the 1980s and 1990s. A regression that essentially replicates Nickell (1997) for six years in the eighties finds that labor-market institutions (including country fixed effects) can explain nearly 80 percent of the variance in national unemployment

rates over the years from 1983 to 1988 (Fitoussi et al., table 6). However, when changes in the unemployment rate from the 1980s to the 1990s are regressed against changes in institutions, most of the coefficients become insignificant (Fitoussi et al., table 7).

Fitoussi et al. test the monetary policy hypothesis by using a set of variables intended to capture the effect of monetary policy in a simple cross-section regression, with the difference between the country unemployment rates in the 1990s and the 1980s as the dependent variable. These regressions provide some support for the view that monetary policy is at least partly responsible for higher unemployment in the 1990s. A test that includes only real interest rate variables and the country average unemployment rate in the 1990s explains nearly 40 percent of the variance in unemployment among the nations tested (Fitoussi et al., table 10, column 1). The coefficient on the real interest rate variable implies that a one-percentage-point rise in the real interest rate is associated with a 0.84 percentage point increase in the unemployment rate.¹²

Fitoussi et al. accept that labor-market institutions can at least explain the persistence of high unemployment in some nations, but conclude that “institutional reforms in the OECD” can only explain a small portion of the divergent trends in unemployment (p 257). The study then points to the success of many countries, most notably Ireland, which have seen large reductions in their unemployment rates with little or no reform of their labor-market institutions (see Chapter x below). This study examines the extent to which monetary policy and asset price fluctuations can explain recent patterns in unemployment rates, precisely because it views the explanatory power of the labor-market institutions view to be limited. Fitoussi et al. concludes that “the labor market reforms advocated by the OECD Secretariat, although helpful in some cases, leave us far short of explaining why the countries that recovered in the 1990s did so, and by the amounts they did” (p. 276).

Bertola, Blau, and Kahn

Bertola, Blau, and Kahn (BBK) also attempt to explain trends in unemployment rates by the interaction of macroeconomic shocks and labor-market institutions. One notable difference in the BBK analysis is its inclusion of demographic variables, specifically variables intended to measure the percentage of young workers in the labor force, in regressions examin-

ing differences in unemployment rates across countries and through time. In most other ways, the core analysis follows closely the methodology used by BW.

The additional hypothesis that BBK examines is that differences in the youth share of the population both partly explain differences in national unemployment rates, and that the rigidities created by various labor-market institutions will make some countries less able to employ young workers. The evidence reported in the study on this issue is ambiguous, with the youth variables statistically insignificant in several specifications and, in one case, statistically significant with the wrong sign (e.g. table 9 columns 1 and 5).

The study also presents rather ambiguous evidence on the larger hypothesis that labor-market institutions explain national differences in unemployment rates. Column 6 of table 4 shows the impact on the unemployment rate of differences in each of the institutional variables, implied by the estimated coefficients in the regression whose results are shown in BBK (table 9, column 1). This calculation uses the size of the macroeconomic shocks in the period 1991-95.¹³ The tax variable is significant and consistent with the size of the estimates produced in other studies, implying that a ten-percentage-point decline in the tax rate is associated with a decline of 1.0 percentage points in the unemployment rate. Benefit duration is positive and significant, although the implied effect is somewhat larger than in other studies, with the regression result implying that a one-year increase in benefit duration is associated with a 1.4 percentage-point rise in the unemployment rate. The employment protection legislation variable has a positive and significant coefficient, although it is worth noting that this variable is a straight ranking of the OECD countries. While Nickell (1997) used the same variable for employment protection legislation, subsequent research has relied on indexes that assigned values for different types of employment protection. The study does not provide a rationale for returning to this cruder method of measuring the strength of employment protection legislation.¹⁴

The coordination variable has a negative and significant coefficient similar in size to the results found in other studies. The replacement rate, union density, union coverage and active labor-market variables are all insignificant in this regression, as is also the case in most of the other regression results shown in the study.

In our view, this regression result, coupled with the others shown in the same table, provides little basis for accepting the labor-market institutions explanation. In these regres-

sions, none of the institutional variables consistently have significant coefficients, with the results very sensitive to the specification used in the specific regression. (It should be noted that BBK assess their results quite differently, by emphasizing that the institutional variables are jointly significant using an F-test.)

In spite of the mixed nature of their regression results, BBK are quite unambiguous in assessing their findings, which they take as confirmation of the “Unified Theory,” commenting that, “we find the superior overall performance in the United States since the 1970s is largely due to the interaction between macro shocks and our laissez-faire labor-market institutions” (p. 52). Summarizing its findings, the study asserts that, “high wage inequality and low wage levels are associated with low unemployment” and “that ‘globalization’ and ‘new technologies’ make it increasingly difficult for OECD countries to deliver favorable employment and wage opportunities to some of their workers” (.p 53).

Assessment

While this literature is widely viewed to provide strong evidence for the labor-market rigidity view, a close reading of the leading papers suggests that the evidence is actually quite mixed, as several of the studies explicitly acknowledge.

Even when we focus only on the most supportive results from each study, we see a disconcerting range of estimates of the impact of institutions. Only the tax and unemployment benefit duration variables are significant in all the regressions in which they appear, although two of the regressions did not include a duration measure.¹⁵ Even with these variables, the range of the estimated coefficients is sufficiently large to raise questions about the structure of the tests. The implied impact of a ten-percentage-point increase in the tax-rate variable ranges from an increase in the unemployment rate of 0.9 percentage points (EMS, 1998) to an increase of 2.1 percentage points (Nickell, 1997). The implied impact of a one year increase in benefit duration in the five regressions in which it appeared range from an increase in the unemployment rate of 0.7 percentage points (Nickell, 1997) to 1.4 percentage points (Bertola et al. 2001).¹⁶

The employment protection legislation (EPL) variable is positive and significant in five of the six regressions in which it appears, although the impact of an increase of one unit in the EPL index ranges from a 0.2 percentage point increase in the unemployment rate (Ber-

tola et al. 2001) to a 4.45 percentage point increase in the unemployment rate (Nickell et al. 2001). While some of this difference can be explained by the different indexes used in the regressions, there would still be a substantial range of estimates even after making adjustments for these differences.

The unemployment replacement rate is positive and significant in five of the six sets of regression results shown in the table. But here also the range of the estimates is striking. The implied impact of a ten percentage-point increase in the size of the replacement rate variable ranges from a 0.1 percentage-point rise in unemployment (Belot and van Ours, 2002) to a 1.3 percentage point increase (Elmeskov et al. 1998). The range of the estimated coefficient for the variables that were generally found to have a significant relationship with the unemployment rate is sufficiently large to both raise questions about the robustness of this result and also to obscure the potential tradeoffs for policy makers.

A second point is that some of the explanatory power of the regressions comes from “good” institutions – those that lower unemployment. The coefficient of the coordination variable is negative and significant in five of the six sets of regression results shown in the table, although the size of the effect implied by two of the estimates is too large to be plausible. Also, the active labor market policy variable is negative and significant in two of the four regressions results shown in table 4, suggesting that a greater commitment to retraining unemployed workers and matching them to jobs may be an effective method of lowering the unemployment rate. While the OECD has actively promoted ALMP as one solution to high unemployment, the organization has been almost silent about the one labor-market policy that consistently shows the largest promise in reducing unemployment: bargaining coordination. Indeed, the OECD has consistently advocated decentralisation of wage bargaining

Third, it is worth repeating that there are features of many of these studies that raise serious doubts about the labor market institutions explanation of unemployment. The sizes of several of the coefficient estimates in Nickell (1997) are clearly implausible. For example, the implied result that an increase of one unit in the bargaining coordination variable is associated with a 3.7 percentage point decline in the unemployment rate, while an increase of one unit in the union coverage index is associated with a 3.6 percentage point increase in the unemployment rate. The EMS study (1998) finds that most of the changes in the unemployment rate in the eighties to mid-nineties are explained by country specific effects, rather than by the insti-

tutional variables used in the regressions. It also found significant evidence of reverse causality in the case of the replacement rate and the unemployment rate, suggesting that the strength of the correlation found in these regressions may be at least partly explained by the fact that countries tend to increase benefits when they have high rates of unemployment.

The Nickell et al. (2001) study also reports implausible coefficient estimates. As is noted in the study itself, the structure of the regressions is highly unusual, including a lagged dependent variable. In addition, the fact that labor market institutions show almost no effect on the employment-to-population ratio raises serious questions over how these institutions can be responsible for unemployment. The Fitoussi et al. (2000) effort to explain unemployment through the interaction of shocks and institutions had the peculiar finding that most of the “success” stories appeared among the list of nations most vulnerable to macroeconomic shocks. The Bertola et al. 2001 study mostly finds weak results, although their discussion implies otherwise.

The Blanchard and Wolfers (2000) study also shows mixed results, as they note. The results are highly sensitive to specification, and regressions that use time-varying measures of institutions produce weaker results than regressions that assume these institutions never change. Assessing the research on institutions and unemployment, the authors note that:

“One must worry however that these results are in part the result of economic Darwinism. The measures used by Nickell have all been constructed ex-post facto, by researchers who were not unaware of unemployment developments. When constructing a measure of employment protection for Spain, it is hard to forget that unemployment in Spain is very high ... Also, given the complexity in measuring institutions, measures which do well in explaining unemployment have survived better than those that did not.” (p18)

Blanchard and Wolfers rightly stress the importance of ensuring that results are robust to variations in variable specification, time period, and estimation method. Our interpretation of this literature is that the results are decidedly not robust to such variations. Our own analysis of the cross-country data is presented in the next section.

Summary of the Implied Impacts of Differences in Labor Market Institutions on Unemployment

	Nickell 1997 ¹⁷	Elmeskov 1998 ¹⁸	Belot & VO 2002 ¹⁹	Nickell et al 2001	Blanchard / Wolfers 2000	Bertola et al 2001
<i>Institutions</i>						
EPL (1 unit increase)	No effect	1.43	0.87	4.45	0.24	0.20
UB Repl. Ratio (+ 10 PP)	0.88	1.29	0.10	1.24	0.70	No effect
UB Duration + 1 yr	0.70	--	--	0.88 ²⁰	1.27	1.43
ALMP + 10 PP	-1.92	-1.47	--	--	No effect	No effect
Union Density +10 PP	0.96	No effect	-1.06	No effect	0.84	No effect
Union Coverage +10 PP	3.60 ²¹	--	--	--	No effect	No effect
Co-ordination + 1 unit	-3.68	-1.48 ²²	-0.70	-11.64	-1.13	-1.11
Taxes + 10 PP	+2.08	0.94	1.79	1.69	0.91	0.97
<i>Macroeconomic shocks</i>						
Δ inflation + 1 PP	-1.36	--	-0.48	--	--	-3.06
GDP Gap		-1.25	--	--	--	
TFP slowdown – 1PP	--	--	--	0.86	0.73	No effect
Real interest hike +1 PP	--	--	--	--	0.47	0.63
Labor demand fall -1 PP	--	--	--	2.54	0.18	0.12
Money supply	--	--	--	No effect	--	--
Real Import/Oil Price	--	--	--	0.52	--	--
Rise +1 PP						
Youth share	--	--	--	--	--	No effect
<i>Interactions</i>						
Institutions w/ macro	No	No	No	No	Yes	Yes
Institutions w/ institutions	No	Yes	Yes	Yes	No	No
<i>Fixed effects</i>						
Country	No	Yes	Yes	Yes	Yes	Yes
Time	Yes	No	Yes	Yes	No	No
Country trend	No	No	No	Yes	No	No
<i>Sample</i>						
Period	1983-94	1983-95	1960-95	1961-92	1960-95	1970-96
Periodicity	6-year	Annual	5-year	Annual	5-year	5-year
Countries	20	19	18	20	20	20
Data Set	Nickell	OECD	Belot & VO	Nickell et al	Nickell/BW	Nickell/BW

Notes: Column (1) is based on Nickell 1998, Table 2: column 1; Column (2): Elmeskov et.al 1998, table 2 column 1; Column (3): Belot and Van Ours 2002, Table 7, column 5; Column (4): Nickell et al 2001, Table 13, column 1; Column (5): Blanchard and Wolfers, 1999, Table 5, column 1; Column (6): Bertola et al 2001, Table 9, column 1. “No effect” means not statistically significant; -- means variable not included

5. Regression Results

In this section, we produce our own empirical estimates of the effects of labor-market institutions on unemployment rates across OECD member countries using a data set that spans the full 1960-99 period. Our data – which build primarily on those constructed by Nickell et al (2002), but includes variables from Blanchard and Wolfers (1999), Belot and Van Ours (2001), and other sources – have several advantages over what has been analyzed to date. Our data are augmented, mainly from OECD sources, to cover the late 1990s, when unemployment rates fell sharply through many of the OECD countries. At the same time, we have filled some gaps for the 1960s that are present in other data sets. We have also been able to combine what appear to be the most appropriate variables from different, previously published sources. To preview the results, our analysis reinforces the conclusions we drew from our review of earlier research. Using simple and transparent models, our results provide little support for the widely accepted labor market rigidity view.

Table 6 presents our main results. Columns one and two conduct a simple test of the robustness of the main results in Nickell's influential (1997) paper. Our basic approach is to test the sensitivity of the initial Nickell results by using new versions of the institutional variables produced for Nickell et al (2001). As in Nickell (1997), the regression in column one attempts to explain the standardized unemployment rate in twenty OECD countries using data on each country's level of employment protection, replacement rate, benefit duration, union density, bargaining coordination, and tax wedge.²³ Nickell's original regression spanned two six-year periods (1983-88 and 1989-94), while our regressions cover two five-year periods (1985-89 and 1990-94). Following Nickell (1997), we have estimated the relationship using generalized least squares random effects. Since the Nickell et al (2001) data set does not include information on union coverage or active-labor-market policies because of lack of data for the 1960s and 1970s, the regression in column one excludes these variables, which did appear in the original Nickell specification (we will add these variables, from other sources, below in column two).

Using the Nickell et al (2001) data in the Nickell (1997) regression produces results that differ markedly from those obtained in the original study. In Nickell (1997), seven of the eight institutional variables had the correct sign and were statistically significant at standard levels. The only exception was the employment protection variable, which was close to zero

and not statistically significant. Using the Nickell et al (2001) data, however, three of the six institutional variables have the wrong sign (employment protection, union density, and the tax wedge) and none are statistically significant.²⁴ These initial results reinforce the conclusions we drew from our literature review: the strong policy recommendations often associated with the rigidity view appear to flow from empirical analyses that are not particularly robust.

Of course, it may be that the exclusion of two important variables that appeared in the original Nickell specification – union coverage and active-labor-market policies – explains the poor results in column one. To explore this possibility, the regression in column two reintroduces the two variables into the analysis, using data on union coverage from Blanchard and Wolfers (1999)²⁵ and data on active-labor-market policies from the OECD.²⁶ The inclusion of the two missing variables does little to rescue the rigidity story. The union coverage variable is significant at the 10 percent level, but ALMP is not significant and the introduction of these variables does not alter the signs or statistical significance of the original institutional variables.²⁷ This second set of results, then, further confirms the sensitivity of the empirical support for the rigidity view to reasonable alterations in the definitions of the institutional variables.

As our earlier review of the literature indicated, after Nickell (1997), research in this area has generally headed in the direction of greater complexity. Researchers have expanded the time period analyzed, allowed for interactions between institutions (Bertola, Blau, and Kahn, 2001 and Nickell et al, 2001, for example) and between institutions and macroeconomic shocks over time (Blanchard and Wolfers, 1999), and deployed increasingly sophisticated econometric techniques (see Section 3). Policy discussions based on these analyses, however, have been much less conditional in their thrust than would be justified by the findings of most of this new underlying research. The suggestion that countries cut replacement ratios, for example, has generally not been conditional on the existence of a negative productivity shock or adverse turns in the terms-of-trade. Rather, the general economic remedies proposed by the rigidity view have been packaged as a set of recommendations that seek to improve labor-market performance unconditionally, through a "one-size-fits-all" series of "reforms" to the labor market.

In this context, it is worth using the available data over the last four decades to see to what extent labor-market institutions, in and of themselves, can account for the evolution of

national unemployment rates. We do this by extending the simple model in column one to data for twenty OECD countries over eight five-year periods spanning the years 1960 to 1999.²⁸ We also include the interactions between institutions which have entered into the mainstream of the discussion (allowing that unemployment replacement ratios may have a bigger effect when the duration of benefits is long, for example), and country effects (which implies that we are examining the extent to which changes in institutions over time affect the evolution of unemployment over time) and time effects common to all countries (which means that our results explain deviations from the evolution over time of the average OECD unemployment rate). The macroeconomic situation is represented by the change in inflation. One interpretation of the results of this procedure is that it estimates the "average" effect of institutions on unemployment, independent of particular macroeconomic shocks.²⁹

The results of estimating this model for the whole period are reported in column (3) of Table 6. They provide little support for those advocating comprehensive deregulation of OECD labor markets. Employment Protection Legislation has no systematic effect at all. A higher replacement ratio is associated with lower unemployment unless benefit duration is extremely high and conversely longer duration of benefits reduces unemployment unless benefits are at very high levels.³⁰ Co-ordination has a very large effect in reducing unemployment (lessened if union density is very high). Taxation has no effect. The time dummies are very large and significant (with 5.5 percent more unexplained unemployment on average in 1995-99 than in 1960-64) and some of the country effects are enormous (to take the extreme cases, Spain has unemployment on average 15 percent higher than Austria, unexplained by the institutional variables).

There are further revealing results if the forty years are split into the period up to the early 1980s, which includes most of the overall rise in unemployment (column 4), and the period from the early 1980s during which unemployment rates continued to diverge but without a strong average trend (column 5). The effects of benefits appear weaker in the second period, and EPL now reduces unemployment. The impact of coordination in reducing unemployment is much stronger in the second period, though the effect is lessened at higher levels of either unionization or taxation. Whereas taxation increases unemployment in the first period, it had no systematic impact in the second (column 5). If anything the results for the more recent period offer even weaker support for the deregulationist position than does the 1960-84 period.

The results reported here serve to underline the lack of robustness in the estimates of the impact of labor market institutions; these seem dependent on the particular measures of the institutions used and on the time period covered. Certainly there is little evidence here of the consistency of results which could convincingly underpin sweeping recommendations for labor-market reform.

Table 6: Determinants of the standardized unemployment rate

	(1)	(2)	(3)	(4)	(5)
Period	1985-94	1985-94	1960-99	1960-84	1980-99
EPL	-0.117	-0.737	-0.009	0.199	-0.317*
	(2.157)	(2.715)	(0.506)	(0.389)	(1.444)
Replacement rate	0.064	0.052	-0.610**	-0.058**	0.012
	(0.050)	(0.065)	(0.009)	(0.015)	(0.018)
Duration	3.955	-0.138	-5.174**	-6.685**	-5.100
	(2.950)	(3.495)	(1.024)	(0.814)	(0.144)
Union density	-0.009	-0.027	-0.599	0.014	0.021
	(0.056)	(0.065)	(0.428)	(0.052)	(0.055)
Coordination	-1.587	-2.795	-4.793**	1.663	-7.043**
	(1.623)	(1.764)	(1.091)	(1.674)	(1.327)
Tax wedge	-0.039	-0.147	-0.023	0.185**	-0.097
	(0.101)	(0.107)	(0.065)	(0.069)	(0.072)
Union coverage	--	5.540#			
		(2.963)			
ALMP (inst'd)	--	-0.013			--
		(0.080)			
Rep Ratio* Duration			0.126**	0.167**	0.096#
			(0.027)	(0.027)	(0.059)
Union Den* Co-ord			0.076**	0.011	0.071**
			(0.020)	(0.026)	(0.025)
Tax* Coord			0.020	-0.067**	0.058*
			(0.024)	(0.023)	(0.048)
Change inflation	-1.841*	-1.830#	-0.451**	-0.315**	-0.277
	(0.769)	(0.997)	(0.151)	(0.083)	(0.220)
Time effects	Yes	Yes	Yes	Yes	Yes
Country effects	No	No	Yes	Yes	Yes
Obs	40	37	156	96	80
Countries	20	19	20	20	20
Notes: Columns (1) and (2) estimated using random effects ("xtreg, re" in Stata 6.0). ALMP in column (2), following Nickell (1997), is instrumented using countries' average ALMP value over the 1985-99 period. Column (3) estimated using feasible generalized least squares, correcting for panel heteroscedasticity ("xtgls, p(h)" in Stata 6.0). Standard errors in parentheses; #, significant at 10%; *, significant at 5%; **, significant at 1%.					
Data set: see Appendix					

Summary and Conclusions

This paper has examined the evidence for the widespread belief that labor market rigidities are largely responsible for high unemployment and that labor market deregulation is

therefore the best route to raising employment rates. Section 1 summarized the evolution of the labor market institutions held to account for these rigidities in the recent empirical literature, as well as some that may tend to reduce unemployment. We noted that measures of union strength, the severity of employment protection legislation, and unemployment benefit replacement ratios tended to reach maximum values in the 1980s, with some tendency to decline subsequently, while benefit duration continued to grow and bargaining coordination tended to decline.

Simple cross section plots presented in section 2, however, show no correlation whatever between the six most commonly employed institutional variables and levels of unemployment. Nor is there any obvious link between the pattern of deregulation in the 1990s and trends in unemployment rates. In support of its case for labor market deregulation, the OECD has attempted to link the degree to which countries have followed their prescriptions for labor-market deregulation with the extent to which structural unemployment (the NAIRU) has declined (see for example, OECD 1999). We constructed from OECD sources an index of the extent of labor-market deregulation in the 1990s and showed that there is no meaningful relationship between labor-market deregulation and shifts in the NAIRU. It is also worth noting that a leading paper by OECD economists found that nearly all of the change in structural unemployment rates between 1990 and 1995 was accounted for by country-specific effects, not by “institutional factors” (Elmeskov et al, 1998, Table 3 and page 11). This conclusion is strongly supported by the detailed case study chapters that follow later in this volume.

The increasingly sophisticated empirical literature that has attempted statistically to link these institutions with the pattern of unemployment across the OECD was reviewed in section 3. We noted that these studies are far from unanimous in their estimates of the impact of the standard institutional variables on unemployment and that a number of the prominent papers explicitly refer to this lack of robustness of their own results across specifications and variable definitions. We then presented some econometric results of our own using an expanded data set including more observations for the 1960s and including the later 1990s. We showed that the strong cross sectional relation between unemployment and institutions found by Nickell (1997) in his influential paper is not robust to alternative definitions of the variables. We then used our extended data set to further examine the relations between unemployment and labor-market institutions that are generally presumed to be valid in mainstream

policy prescriptions. The strongest result was for bargaining coordination, particularly for the period since the early 1980s - a “good” institutional variable, since it tends to reduce unemployment, but one that rarely features in the OECD’s policy advice. High taxation seems to have been associated with high unemployment up to the early 1980s, but the relationship appears much weaker subsequently. Two leading “bad” institutions, Employment Protection and Unemployment Benefits, have perverse or weak effects.

Our results suggest a yawning gap between the confidence with which the case for labor-market deregulation has been asserted and the evidence that the regulating institutions are the culprits. It is even less evident that further weakening of social and collective protections for workers will have significant positive impacts on employment prospects. The effects of various kinds of deregulation on unemployment are very hard to determine and may be quite negligible. Moreover such effects as there are may influence labor-force participation rather than employment (e.g., lower wages and greater employment insecurity may lead workers to opt out of the labor force altogether, which could contribute to lowering the unemployment rate).

Furthermore, it must be recognized that the generosity of unemployment insurance, the level of the minimum wage, and the extent of employee rights in case of dismissal have direct impacts on large numbers of people, whether at work or not and reflect a long process of struggle by citizens and the labor movement. This, of course, may help to explain the continued overwhelming popularity of these sheltering institutions throughout much of Europe. Too often such benefits are not incorporated into the policy discussion to be set against potential costs, and are simply dismissed as the unjustified gains of “insiders.” Deregulation is promoted as though the employment costs of protective labor market institutions are self-evidently greater than the benefits in terms of security and incomes for the recipients. We conclude that the empirical case has not been made that could justify the sweeping and unconditional prescriptions for labor market deregulation which pervade much of the policy discussion.

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Data Appendix 1: Definition of Labor Market Institution Variables Shown in Table 4

	Nickell 1997	Elmeskov 1998 #4	Belot & Van Ours 2002	Nickell et al 2001	Blanchard & Wolfers 2000	Fitoussi et al 2000	Bertola et al 2001
Dependent Variable	ln (unemploy- ment rate)	Unem- ployment rate PP	Unem- ployment rate PP	Unem- ployment rate PP	Unem-- ployment rate PP	Unem- ployment rate PP	Unem- ployment rate PP
Institutions							
EPL	Ranking 1-19	Index 0-16	Index 0-1	Index 0-2	Ranking 1- 19	Index 0-2	Ranking 1-19
Unemployment Benefits							
Replacement ratio	RR in first year	Index (RR*Dur- ation)	RR in first year	RR in first year	RR in first year	RR in first year	RR in first year
Duration	Years	--	Index 0-2	Index Based on RR in years 2-5	Years	Index Based on RR in years 2-5	Index 0-4
ALMP	Ratio of government expenditures per unemployed worker to output per worker	--	--	--	Same as Nickell' 97	--	Same as Nickell' 97
Unionization							
Density	PP	PP	PP	PP	PP	PP	PP
Coverage	Index 1-3	--	--	--	Index 1-3	--	PP
Co-ordination	Index 2-6	Index 1-3	Index 1-3	Index 1-3	Index 2-6	Index 1-3	Ranking 1-19
Taxes	PP	PP	PP	PP	PP	PP	PP

Notes: Column (1) is based on Nickell 1998, Table 2; column 1: Column (2) Elmeskov, Martin & Scarpetta 1998 table 2 column 1; Column (3) Belot and Van Ours 2002, Table 7, column 5; Column (4) Nickell et al 2001, Table 13, column 1; Column (5) Blanchard and Wolfers 1999, Table 5, column 1; Column (6) Fitoussi et al 2000, table 6; Column (7) Bertola et al 2001, Table 9, column 1.

Data Appendix 2

Our data set is based on the data covering 1960-95 assembled by Nickell and Nunziata (NN) prepared as an update to the data set used in Nickell et al (2001). For the regression in column (1) of Table 6, we used the NN data without alteration; for regression (2) we added ALMP and Union Coverage as described below. For regression (3) and for the other tables and charts in sections (1) and (2), we amended or added to the NN data as described below.

Unemployment Rate. For 1980-99 we assembled series for the standardized unemployment rate from *OECD Economic Outlook* for December 2001, which we linked back to 1980 using earlier issues and *OECD Labour Force Statistics* data on unemployment rate, following national definitions for countries where earlier series on the standardized rate was not available. This was combined with NN data for 1960-79.

Inflation. Private Consumption Deflator from *OECD Historical Statistics* database on *OECD Statistical Compendium* 2000 No 2 (missing data filled in using *OECD National Accounts*).

Total Factor Productivity. NN series updated for 1995-99 using data from OECD Working Paper No. 248, 2000 and updates from OECD). For Austria, New Zealand, and Switzerland, five-yearly average growth for 1995-99 assumed to be same as in previous decade.

Impact of Terms of Trade. NN series updated for 1995-99 using data on import prices, GDP prices and import share from *OECD National Accounts*.

Tax Wedge. NN series updated for 1995-99 using series from the *OECD Revenue Statistics* 1965-1999 CD (2000) for the sum of individual (income) tax, social security contributions (employer and employee), payroll taxes, VAT, sales taxes, excise taxes, and customs duties, all as a share of GDP. Gaps in data filled in using OECD series for share of government receipts as a percent of GDP (from *OECD Historical Statistics*).

Real Interest Rates. NN series updated for 1995-99 from *OECD Economic Outlook* series for long-term interest rates and consumer price deflator. Gaps in data for earlier years filled from *IMF International Financial Statistics*.

Structural Budget Deficit. Series from *OECD Economic Outlook* December 2001 linked to earlier data from Economic Outlook Database in Statistical Compendium and to actual deficits, if structural deficit not available.

Union Density. NN series updated for 1995-99 from Ebbinghaus and Weber 2000 (European countries), *UK Labour Market Trends*, *Japan Statistical Yearbook*, Australian Bureau of Sta-

tistics website (1995 Nickell figure adjusted) and New Zealand Statistics website. Ireland figure kindly supplied by H.Perry, UCD.

Union Coverage (of collective agreements). From Belot and Van Ours (2001).

Bargaining Co-ordination. NN provide two series, one created for Nickell et al and one from Belot and Van Ours. We followed them in using the latter for regressions (1) and (2) and the former, which we prefer because it incorporates more variation over time and which is updated in Nickell et al 2002, for regression (3)-(5).

Employment Protection Legislation. We used NN's series, which was derived from Blanchard and Wolfers and updated for Nickell et al 2002..

Benefit Replacement Ratio. We followed NN, using the updated OECD database on replacement ratios (kindly provided by the OECD); very minor modifications were made to their procedure for three Scandinavian countries in the 1970s.

Benefit Duration. As for the benefit replacement ratio, above; this measure is a weighted-average of benefits in force in the second to fifth years of benefit as a percent of the first-year benefits.

Active Labor Market Policies. Authors' calculations from database on ALMP spending kindly supplied by the OECD.

Labor Market Deregulation Authors' calculations from Data in OECD 1999 Appendices. For each measure, we applied two weights: one to assess its significance (as shown in van Poeck and Borghijs (2001) derived from OECD 1999) and the other directly from the OECD (1999) to assess whether it had been carried out. Coverage of measures is not so complete for period from 1990 prior to *OECD Jobs Report* in 1995.

Endnotes

¹Box 2.3 is titled “Recent cross-country evidence on the determinants of structural unemployment.” The OECD’s use of the term “structural reform” refers to liberalization of labor-market institutions and policies: improving the effectiveness of collective bargaining arrangements to maintain wage moderation, and scaling back unemployment benefit systems, employment protection legislation, and “taxes on labor” (p. 55).

²For simplicity, we will use “labor market institutions” to refer to both institutions (like union density) and policies (like employment protection laws).

³As might happen, for example, when those made long-term unemployed become decreasingly effective as part of the reserve army in holding down wages – an example of “hysteresis.”

⁴The growth of TFP shows how far real wages can grow, allowing for an equal proportionate change in the rate of profit; the more intuitive measure of the growth of labor productivity shows how far real wages can grow whilst maintaining the share of profits in national income. In both cases any faster growth of consumer prices compared to the GDP deflator reduces the "space" for real wage increases.

⁵We have not included regression results from a seventh study discussed in this section, Fitoussi et al. (2000), in table 5 because the main findings cannot be directly compared to the other studies, using the framework in the table.

⁶The study also includes a set of employment measures, reflecting labor force participation, which appear as dependent variables in another set of regressions. Measures of employment are generally not included in subsequent research within this framework, although they are of considerable interest, since institutions that may affect unemployment are usually thought of as doing so by affecting numbers of people in work rather than by causing people to drop out of the labor force.

⁷It is important to note that active labor market policy is measured as spending per unemployed worker. This means that a 10 percentage point increase in this variable incorporates the fact that higher active labor market policy is associated with lower rates of unemployment. The regressions all use instrumental variables to control for this problem of endogeneity.

⁸The labor demand variable seems problematic in a regression with unemployment as the dependent variable, since it can be seen as being equivalent to regressing unemployment on employment. Nickell et al. justify the use of this variable by defining it as the residual of a labor demand model, where a positive residual can be seen as evidence of a shift in technology towards one that uses relatively more labor. The obvious danger in this method is that if the labor-demand model is misspecified, then this term is effectively just a measure of employment. In the regressions in the paper the labor-demand variable always appears with a very large and extremely significant coefficient (t-statistics over 19), which suggests that this term is in fact simply measuring employment.

⁹The real interest rate variable uses a long-term interest rate, so it is not directly testing the effect of monetary policy on unemployment.

¹⁰The new version also includes a change in union density variable, which is found to be highly significant. Since these regressions all include country dummies, this term should be thought of as a measure of the change in the change (the second derivative), since the regression would be picking up differences from the mean rate of change. In other words, if a country consistently experienced a decline of 0.5 percentage points in its unionization rate, it would have no effect on the unemployment rate. The regression results imply that the unem-

ployment rate would rise, if the rate of decline in the unionization fell to zero, and that the unemployment rate would fall if the rate of decline in the unionization rate rose to 1.0 percentage point annually. There is no obvious theoretical explanation for this pattern, and it is not obvious why the union density variable was run included in this form.

¹¹In contrast to earlier studies, this study has interacted variables in which the coefficient is estimated separately. In other words, the NLS method will combine productivity growth and employment protection legislation, producing coefficients for each that minimizes the error in the regression. In the other papers with models that included interactions, the interacted terms were entered in exactly the same way as any other variable, and only had a single coefficient. For example, the benefit duration and replacement rate variable in Nickell et al. 2001 entered the regression in exactly the same way as either the benefit duration or replacement rate variable. Only a single coefficient is estimated for this interacted variable.

¹²The study then examines the extent to which higher stock prices may explain a reduction in unemployment rates in a series of regressions using annual data from 1960-98, which the authors regard as the main contribution of this paper. These regressions provide some evidence for this view, with the stock market variables having significant negative coefficients. Fitoussi et al. interpret this result as suggesting a supply-side phenomenon: firms are willing to invest in hiring more workers when they anticipate larger profits in the future, as evidenced by rising share prices. It is worth noting that the study's findings are also consistent with a demand-driven reduction in unemployment rates, as higher stock prices lead to more consumption through the wealth effect.

¹³BBK also include a novel test of the underlying hypothesis of the labor-market institution explanation for unemployment – that compression of wage inequality is responsible for high unemployment. They construct predicted unemployment rates for each time and country, using a regression with unemployment as a dependent variable and time and country variables as the independent variables. They similarly construct predicted levels of wage inequality. Finally, they regress the residuals against each other. While the study treats the results as confirming the labor-market institution explanation, it is worth noting that only these tests – which regress residuals against each other -- produce significant results. The study does not find a statistically significant relationship between inequality and unemployment when a direct test is used.

¹⁴This sort of ranking is problematic both because it can easily result in an inaccurate ordering, due to errors in judgment. More importantly, it misrepresents differences in the strength of protection. For example, if five countries have almost identical levels of employment protection, they will be separated by 5 units with a ranking measure (e.g. the lowest ranked 9, the highest ranked 14). By contrast, they would have almost the same number if an index were used.

¹⁵When assessing the coefficients shown in table 4, it is important to keep in mind that we have attempted to focus on the preferred regression in each study. In each of the studies, there were results shown from other regressions that provided less support for the institutions theory.

¹⁶The benefit duration variable is measured somewhat differently across the studies, see Appendix 1 for a more precise description of each of the variables.

¹⁷Shows the impact of differences in the independent variable on a country with the mean unemployment rate for the sample.

¹⁸Shows impact of a change of one standard deviation in the independent variables.

¹⁹The calculation of the change in EPL assumes a 10 unit increase in the index. Effects shown include the effect of the interaction terms, under the assumption that that the interacted institutional variable is set at the sample mean for the last period.

²⁰Assumes an increase of 0.12 in the duration index, which is equivalent to adding an additional year of benefits at a replacement rate of 40 percent.

²¹Assumes a rise of one unit in an index that ranges from 1 to 3.

²²The effect of being a country with either a low or high degree of coordination and centralization, compared to a country with intermediate levels for these measures.

²³All variables are five-year averages from the 1960-95 version of the Nickell et al (2001) data set. The only exception is the tax wedge variable, which we have modified slightly, relying on OECD sources. First, we have filled in gaps for New Zealand and Australia for the 1990 period; and, second, we have altered what we believe may be minor data errors for Japan and the Netherlands in the 1990s. Neither set of data changes has any effect on the qualitative results in Table 6.

²⁴One additional difference between Nickell (1997) and the regressions in columns one and two is that Nickell (1997) uses the log of unemployment, while we use the level (in the line with most other studies). Using the log of the unemployment rate does not change qualitatively the results in Table 6.

²⁵The Blanchard and Wolfers union-coverage variable is the same in both periods, as we believe was the also case in the original Nickell (1997) analysis. It takes the values one, two, or three, based on whether coverage was low (less than 25 percent), medium (25-70 percent), or high (more than 70 percent).

²⁶We use the data set on expenditures on ALMP as a share of GDP per unemployed person, provided to us by the OECD. In the regression analysis, following Nickell (1997), we instrument the potentially endogenous ALMP variable using the average level of expenditures over the full 1985-99 period for which we have data.

²⁷The regression in column two has three fewer observations than column one because the OECD does not have data on ALMP for Portugal, or for Italy for the 1985-89 period. Running the regression in column one on the sample in column two, does not alter qualitatively the results in column one.

²⁸Following Nickell et al (2001), we incorporate country effects; a full set of time dummies, and estimate the model using feasible generalized least squares, allowing for panel heteroscedasticity.

²⁹Some experiments including some of the macroeconomic shocks deployed in the literature suggest that their inclusion has rather limited impact on the results for institutions.

³⁰The interacted variables are introduced as deviations from the sample mean so that the coefficients of the “uninteracted” variables in the top rows of the table show the impact of the variable given average values of the of those variables with which it is interacted.