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Are Labor Market Institutions Really at the Root of Unemployment? A Critical Perspective on the Statistical Evidence

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Theories have lives of their own, quite defiantly of the phenomenal world that can be actually observed.” Amartya Sen (2006)

The last three decades has been marked by high unemployment across much of the developed world. As recently as 1979, among OECD-member countries only Ireland and Portugal reported unemployment rates above 8 percent (each with about 8.5 percent). But by 1983, just four years later, 11 of these 20 countries posted rates above 8 percent, and six reached double-digit levels, ranging from Belgium (10.7) to Ireland (14.9). This crisis in employment performance persisted throughout the 1980s and 1990s, but by the early 2000s the unemployment “crisis” had become concentrated in the four large continental European countries. The 2005 rates were 9.5 for France and Germany, 9.2 for Spain, and 7.5 for Italy.

Much like the response of economists to the Great Depression, the dominant explanation has centered on supply-side rigidities generated primarily by protective labor market institutions, and correspondingly, the solution is greater (downward) wage flexibility and stronger work incentives. If there is a single labor market institution at the heart of the rigidity account it is the unemployment benefit system. As Bertil Holmlund (1998, p. 114) has written, “The research devoted to UI in the past couple of decades is closely intertwined with the retreat of traditional Keynesian macroeconomics and the development of theories of the natural (or equilibrium) rate of unemployment... In fact, in many simple models, the wage replacement rate provided by UI is often the only explicit exogenous variable that determines unemployment.”

Unlike the earlier episode of high unemployment, in which professional work focused on theory – the “classical” orthodoxy and Keynes’ attack on it – the current focus is on the empirical confirmation of the role of labor market rigidities. The cross-country analyses have evolved from simple cross-country regression models with average levels of unemployment and institutions for several 5-6 year cross-sections (e.g., Layard, Nickell, and Jackman, 1991; Nickell, 1997; Nickell and Layard, 1999), to one in which the focus shifted to the *interaction* of 1970s-80s economic shocks with strong institutions (e.g., Blanchard and Wolfers, 2000), and finally to models in which the annual change in unemployment are explained by *changes* in institutional strength, with or without

measures of shocks, macroeconomic conditions, or other factors (e.g., Nickell et al., 2005).

By the end of the 1990s, the orthodox rigidity account ruled the field. As the IMF (2003, p. 129) put it, “leading international institutions – the IMF, OECD and the European Commission – have long argued that the causes of unemployment can be found in labor market institutions. Accordingly, countries with high unemployment have been repeatedly urged to undertake comprehensive structural reforms to reduce ‘labor market rigidities’.” By 2004, the account had become so widely accepted as “stylized fact” that a leading scholar could state in the *Journal of Economic Perspectives* that “evidence supports the traditional view that rigidities that reduce competition in labor markets are typically responsible for high unemployment” without actually citing *any* published evidence (Gilles St. Paul, 2004, p. 53).

This paper critically assesses this evidence. In his survey on the labor market effects of the unemployment benefit system, Holmlund (1998, p. 114) writes that “a hallmark of modern labor economics is the close interplay between the development of theory, data sources and econometric testing.” There can be little doubt that this interplay has advanced our knowledge in many areas. At the same time, in his Comment on this paper, Manning (1998) contends that theory has played a “disturbingly large part in informing” (p. 145) the way this interplay has developed in the research and discussion on the causes of high unemployment: “the strength of the evidence linking the generosity of the benefit system is not as strong as we would like and our belief in such a link derives more from the theory than from the evidence” (p. 143). In the spirit of Mark Blaug’s (1992) plea that mainstream researchers do far too much “confirming” and far too little “confronting” of theory with evidence, in this survey we take an explicitly skeptical stance on the ability of the available evidence to support the conventional wisdom that the rigidities generated by labor market institutions explain the pattern of unemployment across countries. The importance of putting the empirical work and its interpretation to a rather strict critical test seems particularly important in this subject matter, since the research been largely motivated – and certainly used – as the basis for reforming public policies and institutions that have enormous consequences for both the functioning of labor markets and for individual and social well-being.

After describing the broad pattern of unemployment trends in the OECD since the 1960s, Section 2 considers the simple correlation evidence, both between standard measures of labor market institutions and unemployment (levels and changes over time). Section 3 surveys the macroeconomic evidence by addressing three dimensions of robustness: the consistency of results across studies, the consistency of published tests within studies, and the sensitivity of the results in several studies to minor changes in data and specification. We find that the labor market institution that shows the strongest, most robust effect on the pattern of unemployment in this literature is the generosity of unemployment benefit entitlement, and Section 4 addresses two main points: the interpretation and substantive importance of unemployment benefit replacement rate effects (the key benefits variable); and the degree to which the microeconomic evidence supports the macro (replacement rate) effects. Section 5 returns to correlation evidence with a critical assessment of two recent influential efforts to develop an index of labor market reforms and use it to explain the cross-country pattern of unemployment. Section 6 concludes with a summary and brief discussion of implications for research and the relationship between research and policy recommendations.

1. Unemployment and Institutions in the OECD: The Basic Facts

Figure 1 shows the levels and dispersion of unemployment rates for 19 OECD-member countries for each 5-year period between 1960 and 2004, and includes 2005 at the far right. As a reference, the line that runs from left to right marks the U.S. rate. The table at the bottom presents the U.S. rate, the median, and a measure of the dispersion of rates (the standard deviation).

This figure highlights some key facts about the changing nature of the unemployment problem in the developed world. First, nearly all countries experienced escalating unemployment through at least the late 1980s. The median unemployment rate (see the table below the Figure) rose from 1.9% in the late 1960s to 8.8% in 1990-94. Second, the dispersion of rates has moved upward with the median. The standard deviation for these 19 countries increased sharply from the 1.2 – 2.2 range in the 1960s-70s to 3.3 – 4.5 in the 1980s-90s. Third, unemployment rates have declined and converged substantially since the late 1990s: the median fell from 7.9% in 1995-99 to 5.3% in 2000-04 and 5.2% for 2005; the

standard deviation fell from 3.9 to just below 2, which is about where it was in the 1970s. The figure shows that the distribution of unemployment rates in 2005 covers about six percentage points (from four to ten percent), about the same as the range in 1960-74 (from about zero to 6 percent). And fourth, the unemployment performance of the U.S. varies dramatically over this period, from among the very highest rates through the first two decades (1960-79) to among the lowest in the second half of the 1990s, to close to the median since 2000 (2000-04 and 2005). It is also worth noting that New Zealand has regained its position as the country with the lowest unemployment rate, Ireland has dropped to the second lowest from the second highest in the 1985-94 period, and Spain's rate has fallen sharply to just below that of Germany and France.

Table 1 provides demographic detail for 2003 for countries in three groupings: the market-oriented Anglo-Saxon countries; the continental European countries, of which two have had very low unemployment (Austria and the Netherlands) and five have been plagued by high unemployment (though it should be noted that Germany's rate rose above the U.S. rate only after 1993); and the Scandinavian countries with strong universalistic welfare states (Denmark, Norway and Sweden). This table shows that for all four demographic groups – male and female young and prime age workers - Austria and Holland were by far the best performers, followed by the Scandinavian and Anglo-Saxon groups, which had nearly identical rates for each demographic group. The five high-unemployment Continental countries show substantially higher unemployment for each age-gender group, and in all countries but Germany, youth unemployment is extremely high. Female youth show rates of 17.5% in Belgium, 22.8% in France, 27.2% in Spain, and 30.9% in Italy; male youth rates range from 18-23%. Clearly, young people in these four countries account for an important part of the European unemployment problem.

In the popular press and in a surprising number of professional papers, "Europe" is often portrayed as a single entity characterized by high unemployment and strong social protections, and this is contrasted with the much better performing and relatively unregulated labor markets of the U.S. and other Anglo-Saxon economies. This turns out to be only partly true. Table 2 allocates 18 OECD countries to three groups (see the table for the countries in each group): six English-speaking countries; six high unemployment

continental European countries; and six mainly northern European low unemployment countries. These roughly correspond to the traditional groupings used in the “varieties of capitalism” literature – liberal/market-oriented economies; conservative/coordinated economies; and social-democratic countries with strong, universalistic welfare states. While both of the European groups show much greater levels of social protection and regulation (rows 4-9) and much higher tax revenue shares (row 10), only the conservative/corporatist economies show worse employment performance than the liberal economies. Indeed, on both unemployment and employment rates, the northern European welfare states show, on average, superior labor market performance to the liberal ones (rows 1-3), and they do so with much lower wage inequality (row 11). As Nickell (1997; 2003) has pointed out, many Europeans live in regions with lower unemployment rates than the U.S. and most of the unemployed of Europe live in four large countries (France, Spain, Italy and Germany).

2. Correlation Evidence

2.1 Implicit Correlations

Perhaps because the deleterious effects of social regulation on labor market performance is so widely accepted, establishing the link has often been done simply by establishing that a country’s score on a particular institutional measure is relatively high. For example, Heckman (2003, p. 373) suggests that part of the German employment performance problem is due to “substantial” unemployment benefit replacement rates, because “Germans, like all people, respond to these incentives (not to work).” The OECD’s Economic Survey of France (OECD 2005x) establishes that employment protection is relatively strict in France and calls for a series of reforms, but no evidence or references are offered to establish that EPL strictness helps explain French unemployment, or that the recommended reforms would reduce it. Similarly, the OECD’s Economic Survey of the Netherlands (OECD 2005x, p. 25) recommends “increasing the responsiveness of employment to economic conditions by easing strict EPL or regular contracts, (and) making real wages even more responsive to unemployment by phasing down unemployment benefit replacement rates as unemployment spells lengthen” without reference to any evidence on the links between either EPL strictness and

employment responsiveness or benefit replacement rates and the responsiveness of real wages to unemployment.

Between these examples of assumed relationships based, presumably, on theoretical common-sense, and the macroeconometric exercises that we review below, lie simple bivariate correlations, most commonly represented by scatter plots. One would expect that if the expected effects of protective labor market institutions on employment performance are as direct and strong as commonly believed, we should observe some evidence of it with simple correlations. But it turns out that this sort of evidence is generally not very supportive of the orthodox account. Thus, using five-year averages for the 1980s and 1990s for 20 OECD countries, Baker et al. (2005) found no statistical association between unemployment and standard OECD measures of employment protection laws, unemployment benefit replacement rates, the duration of unemployment benefits, union density or union coverage.

In the remainder of this section we focus on simple correlations between unemployment and various measures of unemployment benefit entitlement generosity, both for reasons of space and because our Section 3 survey of the regression literature suggests that it is the labor market institution with the strongest and most robust unemployment-increasing effects.

2.2 The OECD Jobs Study Correlations

Thanks to the efforts of the OECD, the quality and comparability of unemployment benefits data for OECD countries – particularly for measures of unemployment duration – improved dramatically after the mid-1990s. The OECD has produced an average gross replacement rate (across family types, income levels, and for different durations of unemployment) for every second year since 1961 that has become the measure of choice for empirical work in this area. More recently, the OECD had constructed net replacement rates, which take into account unemployment compensation after taxes and various related benefits. Net replacement rates have been constructed for the mid-1990s (see Martin, 1996), but the OECD warns against comparisons over time because of changes in methodology.

The Jobs Study (OECD 1994) made use of these improved measures to explore the association between unemployment benefits and unemployment across OECD-member countries. Perhaps because there was no evidence of correlation at any particular point in time, the OECD stated confidently that “a more comprehensive measure of unemployment compensation has typically been followed by an increase in unemployment but usually with a considerable lag” (p. 44). Chapter 8 of the OECD Jobs Study presents evidence of two sorts on the lagged effect of benefit entitlements. The first is reproduced here in full (OECD 1994, p. 178):

In some countries, there have been major reforms in benefit entitlements which give some more specific idea of how long lags may be. In Canada, entitlements rose in 1972 and unemployment rose unusually in 1978 and more strongly around 1983. In Finland, entitlements rose in 1972 and unemployment rose sharply (in contrast to its Scandinavian neighbours) through to 1978; in Ireland, changes increasing entitlements occurred over 1971 to 1985, and its rise in unemployment was particularly large (as compared to other European countries) from 1980 to 1985. In Norway, major increases in entitlements occurred in 1975 and 1984 (although also before and after these dates), and unemployment rose exceptionally around 1989. Entitlements rose in Sweden in 1974 and in Switzerland in 1977, with major rises in unemployment in 1991 in both cases. These experiences suggest lags between rises in entitlements and later sharp rises in unemployment of 5-10 years for Canada, Ireland and Finland but perhaps 10 to 20 years in Norway, Sweden and Switzerland.

The quality of this empirical demonstration is such that it can only reflect a powerful prior belief that high unemployment must be a direct effect of protective labor market institutions. As Manning (1998, p. 144) puts it, “I think that we would all agree that this is absurd. In fact, one could write a very similar paragraph relating performance in the Eurovision Song Contest to unemployment.”

In their second analysis, the authors “examine correlations more systematically” (p. 178) by showing scatter plots of “cycle-to-cycle changes in unemployment rates and the summary measure of benefit entitlements” (p. 180). For three periods (1973-77, 1979-85, and 1987-93) the OECD presents plots for the change in unemployment against the 6-year average (“summary”) benefits *level* as well as against the *change* in the benefits measure over the previous cycle. They do this for a full set of 21 countries and a reduced set of 14 countries (on the grounds that standardized unemployment data were unavailable for 7 of the 21 countries before the 1980s). They find that “In data for 21

countries, none of the individual correlations are statistically significant at the 5 percent level.” For the 14-country data, two of the six tests produce the expected positive correlation: 1973-77 using the level of benefit entitlements measure, and 1987-93 using the change in benefits measure. But it is not clear why this second test (for just 14 countries) was even run, except for the sake of symmetry, since the unemployment data were available for this time period (1980s-early 1990s). But it is this 14-country result that is stressed in the text and provides support (with regression results from “some plausible specifications” on the same 14 countries (p. 178)) for the unequivocal conclusion regarding the lagged effects of benefits quoted above from Chapter 5 of *The OECD Jobs Study*.

The OECD’s view of the length of the lag between program changes and observed effects seems implausibly long and no subsequent panel data regression study we are aware of has modeled the relationship between unemployment and benefits with lags, much less lags of this length. On the other hand, it is noteworthy that Chapter 8 of the Jobs Study is frequently cited as showing support for the strong effects of unemployment benefit generosity on the pattern of unemployment.

2.3 Correlations Using Recent OECD Data

If unemployment benefit entitlements are one of the labor market institutions at the root of unemployment, the strongest evidence should appear with the use of the OECD’s net replacement measure of unemployment benefits. Net benefit is measured as the after-tax value of unemployment assistance and other social assistance, such as housing and child support. The net replacement rate takes this after-tax measure of benefits as a share of after-tax household earnings. If workers are calculating the tradeoff between the dole and work, such an after-tax measure is clearly the most appropriate. Figure 2 shows there is, indeed, a relationship, but it is perverse: in 2002, more generous after-tax benefits (measured as the overall average over 60 months for two earnings levels and three family types) is associated with *lower* unemployment across these 20 countries. As the figure shows, Italy is an outlier (it offers no benefits after the first phase of unemployment), but even without Italy there is a negative relationship. The figure shows eleven countries (in two groups) with unemployment below the US in 2002 but with net

replacement rates more than twice as high (60-80% vs 30%). With data from Martin (1996), we also found a negative relationship for 1994/5 (not shown).

Long duration of benefits is also expected to help explain high unemployment. Figure 3 shows benefit duration against unemployment, with duration measured as the ratio of the net replacement rate in the 60th month of benefit receipts to the “initial phase” on the entitlement (effectively the 1st month). This duration measure can be greater than one because for the generally smaller group still eligible for assistance in the 60th month, more kinds of social assistance may be available than in the initial phase. The data are shown for 2001, the most recent data available for short and long term net replacement rates. Figure 3 shows that this measure of benefit duration is associated with *lower* unemployment. Spain and Italy offered relatively ungenerous long term benefits but have high unemployment. The figure shows that Ireland, Denmark, the UK and Austria had similar or lower unemployment than the U.S., but much more generous long-term unemployment-linked net benefits.

It has been argued that extended duration of generous benefits will have particularly strong effects on long-term unemployment. Layard et al. (1994) put particular emphasis on this source of the unemployment crisis: “The *unconditional* payment of benefits *for an indefinite period* is clearly a major cause of high European unemployment” (p. 92, italics in the original). The only direct evidence they offer for this conclusion is a plot of “the maximum duration of benefit” in years against the long-term share of unemployment for the mid-1980s. The authors interpret this diagram to show that “all the countries where long-term unemployment has escalated have unemployment benefits of some kind that are available for a very long period, rather than running out after 6 months (as in the USA) or 14 months (as in Sweden)” (p. 59).

But there is another way to interpret this evidence. First, there appears to be little or no statistical correlation (no summary statistic is provided). Second, of the 15 countries that appear in the figure, 9 had the highest duration scores (3.5 to 4 years), and these 9 countries had long-term unemployment that ranged fairly evenly from just 20% to 70%. And third, the poor quality of the duration data raises questions about the meaningfulness of whatever correlation there might be. In their survey of the benefit entitlement literature, Atkinson and Micklewright (1991) single out these data for criticism, pointing

out that the institutional design of each of the countries with “indefinite” duration scores are quite different, and these differences have substantial effects on how generous the systems are for which parts of the unemployed population.¹ For example, the reality behind the “indefinite” score for the Netherlands in the mid-1980s is “UI at 70% of last earned wage for between 6 months and 5 years depending on contribution record, plus one year of benefit at 70% of minimum wage... On expiry of UI, (there is a) possibility of means-tested assistance” (Atkinson and Micklewright, 1991, Table 3, p. 1696).

Figure 4 plots long term unemployment against the same duration measure used in Figure 3 for 2001. The data again fail to show the predicted positive association between benefit generosity and unemployment. Germany and Belgium show high net benefit duration and high long term unemployment, but Ireland, the UK, New Zealand, Denmark and Austria are at least as generous with much lower shares of long-term unemployment. Italy has no long term benefits, but has the highest level of long-term unemployment.

A number of the most influential panel data studies that we survey below have focused on the extent to which *changes* in labor market institutions can account in a substantial way for *changes* in the pattern of unemployment across countries (e.g., Nickell et al., 2005). Along these lines, changes in benefit duration generosity might be expected to be associated with changes in long-term unemployment. Figure 5 explores this possibility for the 1991-2001 period (the longest period the available data permit), again using net replacement rates at different points in time to measure duration. The data show no correlation. The Netherlands and Norway experienced large declines in both duration and long term unemployment, but Ireland had the largest decline in unemployment at the same time that it had the largest *increase* in benefit duration. Although Canada had the largest decline in duration of benefits, its share of long-term unemployment showed little change.

The benefits measure used in nearly all of the recent time series regression tests is the average gross replacement rate, for which there are now measures from 1961 to 2003. The change in this measure has typically been found to be significantly associated with the change in unemployment (see below). Figure 6 shows that the association between the 1982-2002 percentage point change in unemployment and the gross replacement rate has the expected positive sign. It should be noted that this positive association primarily

reflects the effects of two countries which, though showing increasing unemployment, have both remained at very low levels (Switzerland and Norway), while another (Italy) adopted a benefits system for the first in the early 1990s (which explains the big change in Italy's gross replacement rate). Many countries show small changes in the benefits replacement rate (both up and down) and large changes in unemployment (from -7 percentage points for Ireland to +3 points for Japan). In Section 4 we will return to the question of how much weight ought to be assigned to these changes in the gross replacement rate.

3. Macroeconometric Evidence

3.1 The Consensus

As employment performance across much of Europe worsened, economists turned their attention to the links between institutions, rigidities, and unemployment (Blanchard and Summers, 1986; Lindbeck and Snower, 1988). A statistical demonstration of the orthodox rigidity account required appropriate measures of key labor market institutions, the development of which was pioneered by Stephen Nickell, Richard Layard and various colleagues. With these new measures, the results of cross-sectional regression tests were published in a series of papers and books in the early- and mid-1990s (Layard, Nickell, and Jackman, 1991 and 1994; Nickell and Bell, 1994; Layard and Nickell, 1996). At the same time, research within the OECD for the Jobs Study, generated new and improved measures of labor market institutions, which helped produce more sophisticated analyses by OECD researchers (Scarpetta, 1996; Elmeskov et al., 1998).

This early- to mid-1990s research spawned a rapidly growing literature aimed at explaining cross-country differences in unemployment or the evolution of these differences over time. The most influential of these share the same broad conclusion – that in one way or another, the regression analysis lends support to the orthodox theoretical expectation that labor market institutions have played a key role in these cross-country unemployment differences, with clear implications for policy. For example:

- “Thus, with six institutional variables plus the change in inflation, we can explain over 90 per cent of the differences in unemployment between countries” (Layard, Nickell and Jackman, 1994, p. 82).

- “The broad empirical conclusions suggest that policy variables (labor market institutions) and the institutional mechanisms of wage determination do matter for the level of structural unemployment as well as for the speed of labour market adjustment in the OECD countries” (Scarpetta, 1996, p. 45).
- “This paper has identified a number of policy settings and institutional features of the labour market which are associated with high structural unemployment... (we) assign significant roles to unemployment benefits, collective bargaining structures, active labour market policies ... and the tax wedge.... It requires strong political will and leadership to convince electorates that it is necessary to swallow all of the (deregulation) medicine and that it will take time before this treatment leads to improved labour market performance and falling unemployment. But the success stories show that it can be done!” (Elmeskov et al., 1998, pp)
- “To sum up, reductions in replacement rates, lower tax wedges, liberalized employment protection regulations, and improved active labor market policies remain essential ingredients of a comprehensive labor market strategy geared to reducing Europe’s high structural unemployment rate” (IMF, 2003, p. 141).
- “Our results indicate ... (that) broad movements in unemployment across the OECD can be explained by shifts in labour market institutions” (Nickell et al., 2005, p. 22).

Applying a critical perspective to these studies suggests that the statistical evidence may be substantially less compelling than these conclusions would imply. In the remainder of this section we explore the robustness (and economic importance) of the results of some of the most influential studies that have been employed in support of the conventional labor market rigidity view (for much more discussion of the methodology employed in each study, see Baker et al., 2005). We begin by comparing the main findings on the effects of labor market institutions across studies. We then turn to the robustness of findings across different tests in the same study. And finally, we briefly summarize the effects of our own attempts to replicate two studies (Nickell, 1997 and IMF, 2003) with minor changes in data and specification.

3.2 Robustness across Studies

The case for treating protective labor market institutions as the primary culprits responsible for high unemployment will be stronger the more consistent the findings are across studies. Table 3 provides a summary of the implied effects of changes in eight of

the most commonly employed institutional measures from eleven panel data regression studies published since 1996. For reasons of space, we have limited our survey to cross country studies.²

Again, while broadly representative, this is not meant to be a comprehensive list. The labor market institutions designed to provide social protection for workers – and therefore expected to be most responsible for reducing work incentives and raising wages – are employment protection laws (column 1); the unemployment benefit entitlement system, measured by the level of benefits (the replacement rate) and the duration of benefits (columns 2 and 3); collective bargaining, measured by the share of workers who are union members (union density), the share covered by union bargaining (union coverage), and bargaining coordination between groups of employers and unions (columns 5-7). Active labor market policies refer to education, training, job preparation, and job search programs. These are interventions aimed less at protecting workers than at promoting employment, and are often closely linked to the receipt of unemployment benefits. The final measure, labor taxes (column 8) is not really a “labor market institution,” despite being labeled as such in nearly every study.

3.2.1 Early OECD Studies

In “Key Lessons for Labor Market Reforms,” Elmeskov, Martin and Scarpetta (1998) (hereafter, EMS) aim to “distill the main lessons for labour market reforms from the (country) “successes” and “failures” revealed by recent OECD research” (p. TK). The authors, three OECD economists, note that their econometric work is “essentially an update and extension” of Scarpetta’s (1996) earlier work. As Table 3 shows, both Scarpetta and EMS find a significant effect of EPL and unemployment benefit replacement rates, but differ on union density (significant for Scarpetta but not for EMS), the tax wedge (significant for EMS but not Scarpetta) and bargaining coordination (same direction, but the implied effect is twice as large in Scarpetta).

These rather substantial differences are not addressed in the EMS paper, which is notable since the results of these exercises have been highly influential for the way other researchers and policy makers understand the sources of poor employment performance. Elmeskov et al. (1998, p. TK) point to the key role played by Scarpetta’s regression

results: “The OECD work since 1994 has produced a series of additional publications.... This work has enabled the Organization to identify a number of country “success stories” as well as “failures” in terms of implementing the OECD recommendations and the resulting labour market outcomes. In assessing the needs for reform, the work has relied heavily on the econometric work of Scarpetta (1996)...”. If this influential work is found to be inconsistent in a substantial way with the same author’s work a few years later (in EMS, 1998), it would do readers, particularly researchers and policy makers, a considerable service to highlight and explain the differences. EMS compare their findings only to those of Nickell and Layard (1997), noting that while generally similar, the findings for EPL are inconsistent (Nickell and Layard find no significant effect).

3.2.2 From Nickell to Blanchard-Wolfers and Belot-van Ours

In many papers published in the 1990s alone and with various co-authors, Nickell presented results from relatively simple cross-country regressions based on the same grouped data (1984-89, 1989-94). The results shown in row 3 (Nickell 1997) are representative. Although all the variables are strongly significant with the expected sign except EPL, his conclusion in this paper is his most cautious: “It is clear that the broad-brush analysis that says that European unemployment is high because European labor markets are “rigid” is too vague and probably misleading.” Two important studies that also used grouped data extended Nickell’s tests by lengthening the time period covered and by focusing on the interactions between economic shocks and institutions (Blanchard and Wolfers, 2000) and between institutions (Belot and van Ours, 2002; 2004).

These new approaches produce consistent results in some cases and diametrically opposed results in others. Nickell (1997) and Blanchard-Wolfers (2000) (BW) show broadly similar results for the two unemployment benefit measures, union density, coordination, and taxes, but not for EPL (BW gets an unemployment-increasing effect), ALMP and bargaining coordination (both insignificant for BW). Similarly mixed results appear when the Belot-van Ours (BVO) results are compared to Nickell’s: BVO get insignificant effects on both coordination and taxes, and most strikingly, a large *negative* (unemployment-reducing) effect for unemployment benefit replacement rates. (BVO do find statistically significant effects for the interactions of taxes and the replacement rate,

employment protection and centralization of collective bargaining, and union density and centralization, but, by construction, these interaction terms are zero at the means for their sample.) While it should be noted that other tests reported by BVO in the same table produce the expected unemployment-increasing effects of the benefit replacement rate, the test we refer to in Table 3 (#5 in their table) is clearly their preferred one (along with test #6, which has a slightly *larger* unemployment-reducing effect).

3.2.3 From Nickell et al. (2005) to the OECD (2006)

Following the OECD researchers (Scarpetta, 1996; Elmeskov, 1998), Nickell and colleagues (Nickell et al., 2001; 2003; 2005) shift to the use of annual data to explain the *change* in the pattern of unemployment with time-varying measures of institutions, extending the analysis back to 1961. Their tests are distinguished by the use of a lagged dependent variable and country specific time trends, arguing that their inclusion “is to ensure that the estimated coefficients on the institution variables are not distorted by omitted trended variables in each country or common shocks” (2005, p. 15). The use of country trends is of interest since it implies that important secular movements in the unemployment rate have occurred *in the absence of any changes in labor-market institutions*. While the paper notes that most of the estimated coefficients for these trends are not close to being statistically significant, because of the presence of a lagged dependent variable, the trends actually account for much of the change in unemployment in many countries.³ If the magnitudes of their estimates are taken at face value, the question becomes why the mainstream research program has failed to focus on the explanation for these underlying country trends.

Because Nickell et al. (2005) includes a lagged dependent variable (the unemployment rate), their estimates of the unemployment impact of various labor market institutions have a long-run multiplier effect and the implied effects shown in Table 3 reflect this by assuming a five-year horizon (the implied effects after 5 years). Nickell et al. finds that the replacement ratio, benefit duration, and employment tax variables have positive significant effects. Unlike previous studies, Nickell et al replace the standard union density measure (for which they fail to get a significant effect in the 2001 version) with the change in union density, which gets the expected positive and significant effect

(a 10% increase in union density produces a .3 percentage point increase in unemployment after 5 years). Unlike the Scarpetta and Elmeskov et al. studies, however, they find no effect for EPL. Consistent with much of this literature, Nickell et al. do find that higher levels of bargaining coordination significantly reduce unemployment in both of the unemployment regressions.

Relying heavily on the Nickell et al. (2005) data and methodology, the IMF (2003) finds a strong unemployment-increasing effect for EPL (like Scarpetta and Elmeskov et al., but unlike Nickell, Belot and van Ours, and Nickell et al.). The IMF also appears to identify a large effect for union density: seven times larger than Nickell et al. (2005). Interestingly, they also find that labor taxes tend to *reduce* unemployment.

As of this writing, the latest entry is the OECD's effort to reassess the Jobs Study, shortly after its 10th anniversary. The relevant empirical analysis appears in Chapter 1 of Background Paper #1 (OECD, 2005): "This chapter looks at the impact of structural policies and institutions on aggregate unemployment, by means of cross-country / time-series macroeconometric estimation of unemployment models" (p. 7). This work uses the latest OECD data (in annual form), and takes into account interactions among institutions and between them and various "economic shocks." Perhaps most important, it pays particular attention to the robustness of the results. As Table 3 shows, among labor market institutions typically blamed for poor labor market performance, only the unemployment benefits replacement rate is found to have a significant effect. As the authors put it, a key policy conclusion is that "high unemployment benefits are found to amplify the unemployment effects of adverse shocks" (p. 36).

3.2.4 Assessment

In sum, the recent OECD (2006) findings appear to be a good representation of what can reliably be concluded about the effects of labor market institutions from panel data regression tests. As Table 3 illustrates, among institutions designed to provide social protection to workers, only the unemployment benefits replacement rate shows a fairly consistent positive and statistically significant association with cross-country unemployment rates. Although even here, Belot and van Ours (2002; 2004) find a large significant unemployment-decreasing effect of benefit replacement rates (at the sample

mean), and our tests (Baker et al. 2004; 2005) and Baccaro and Rei (2005) show insignificant effects. We will address the likely substantive importance of benefit generosity for the pattern of unemployment below in Section 4.

Despite the popular view that employment protection regulations are central to any explanation of persistent high unemployment in Europe, the panel data offer little supporting evidence. The evidence that appears in Table 3 is quite mixed. With the exception of the IMF (2003), the studies covered in this Table that find the predicted positive and significant effect on unemployment were all published before 2001. The implied effects of a one unit increase in EPL strictness on unemployment in these four studies range from .24 to 1.43 percentage points. On the other hand, Nickell (1997), Nickell et al. (2005), Belot and Van Ours (2004), Baker et al. (2004; 2005), Baccaro and Rei (2005) and the OECD (2005) all find no effect, or even unemployment-reducing effects. In its 1999 *Employment Outlook*, OECD researchers reached a similar conclusion: “The basic finding appears robust: overall unemployment is not significantly related to EPL strictness” (OECD, 1999, p. TK). According to the OECD, negative employment-related effects of current employment protection laws are likely to be found mainly among subgroups of workers – particularly youth and the least skilled (OECD, 2001, p. 63).

Similarly, the evidence on the effects of collective bargaining institutions is quite mixed. The most recent evidence from the OECD (2005) shows no effect. According to the OECD’s recent literature survey (2006, table 3.9), only five of the sixteen studies they cover show unequivocal positive (unemployment-increasing) effects. For two of these, Nickell (1977) and the IMF (2003), the positive union effect disappeared in our variations using the same or similar data and specifications (see below). As the OECD’s *Employment Outlook* chapter on collective bargaining (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.” And coordinated bargaining, a labor market institution designed to take competition out of labor markets, is among the most robust explanatory variables in this literature – in almost every case, all else equal, bargaining coordination among employers and groups of workers (unions) *lowers* unemployment. Nevertheless, the recent OECD study survey

on the effects of wage-setting institutions (2004, p. 133) cautions that “The overall fragility of the evidence linking collective bargaining to macroeconomic performance suggest that great caution should be exercised when attempting to draw guidance for making policy choices from this research...”.

In sum, the findings across panel data regression studies produces mixed results for each of the standard measures of labor market institutions. The strongest candidate is the unemployment benefits system, which we will return to below.

3.3 Within-Study Robustness

With the late 1990s, a number of empirical studies appeared that improved upon the Nickell/Layard institutional measures, added others, changed the time period covered, and experimented with the specification and econometric method (see Baker et al., 2005, for a detailed review of this literature). Among the most influential, Blanchard and Wolfers (2000) shifted the focus of the empirical tests from simple institution effects in panel data cross-sections to the interaction of institutions with macroeconomic shocks. Blanchard and Wolfers argue that labor market institutions may produce higher unemployment by limiting the ability of labor markets to respond to adverse shocks. This helps explain why the same institutions were not employment-unfriendly in previous decades. Their study is also distinguished by a much longer time period (8 five-year periods from 1960 to 1996; the last two years are treated as a full period), and while it relies heavily on Nickell’s institutional measures, it also employs alternative, OECD-generated measures of benefit replacement rates and employment protection laws that vary over time.

Using Nickell’s (1997) time invariant measures of institutions (the average for 1983-88 and 1989-94) and accounting for time and country effects, Blanchard and Wolfers obtained results for the entire 1960-96 period that were similar to Nickell’s for the late 1980s and early 1990s. But the authors point out that the results are quite sensitive to the specification. Indeed, it appears that the use of alternative, arguably much superior OECD-generated measures of unemployment benefit replacement rates and employment protection laws actually worsens the results. According to the authors, the table showing these results “suggests two conclusions, both worrisome: replacing the Nickell measures

by alternative, but time invariant measures, substantially decreases the R^2 . Going from the time invariant to the time varying measures further decreases the fit.”

For example, in a regression that uses alternative measures of benefit replacement rates from the OECD (the average 1985-89 value), 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 the time series version of the same OECD replacement rate measure is used (as it clearly should be), all three of these variables become insignificant, as do the two replacement rate variables themselves (table 6, column 2). But in regressions that use an alternative or time-varying measures of employment protection, the replacement rate, benefit duration, tax wedge, and union density variables all become 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.

Extending Blanchard and Wolfer’s work on the role of interactions among variables, Belot and van Ours (2002; 2005) report the results of seven regressions, four of which test just the direct impact of institutions on unemployment, while the other two include various interacted measures. Their four direct impact tests differ only in that they include different combinations of fixed time and country effects (column 1 shows results with neither time nor country effects; column 4 shows results when both are included), but the results are dramatically different. While their objective is to use the differences across tests to show the importance of interactions among institutions (shown in the last three tests in their Table 7), these differences across tests 1-4 serve to highlight how sensitive the results for each institutional measure is to the other variables included in the test. For example, the benefits replacement rate has the expected sign and is strongly significant in the first test, the right sign but not significant in their second and third tests, and has the wrong sign in their fourth test. When the three interacted variables are introduced, the direct effect of the replacement rate is large and significant with the wrong sign (unemployment-reducing). The coefficients on employment protection have the “wrong” sign (*reduces* unemployment) in all six tests in which it appears, with substantial variation in the size of both the coefficient and the level of statistical significance.

Nickell et al.'s study (2005) offers an example of the impact a few additional years can make. The main difference between the published versions (2003; 2005) and the original working paper (2001) appears to be the replacement of the level of union density with its change and the addition of three years to the time series (ending with 1995 instead of 1992). The consequences are substantial: the effect of EPL on unemployment changes from highly significant to small and insignificant; the level of union density was insignificant but its change becomes highly significant. In addition, the new version reports much smaller effects of taxes, coordination, and benefit duration, but a larger effect for the unemployment benefit replacement rate. It is also notable that these seemingly minor changes in specification change the coefficient of the interest rate variable from insignificant to significant.

Using early versions of Nickell et al. as their starting point, researchers at the International Monetary Fund (IMF, 2003) addressed the same question – the effects of changes in institutions on changes in unemployment - with much the same data but with some notable differences in specification.⁴ Like the early OECD studies (Scarpetta and Elmeskov et al.), but unlike Nickell et al., the IMF finds the EPL has a significant positive effect on unemployment, at least in three of their four published tests. Indeed, by reporting four alternative specifications, the IMF study underscores the difficulty of making generalizations about the effects of labor market institutions, and highlights the lack of robustness even within a particular study. The implied impact of given changes in labor market institutions for each of their four alternative specifications are shown in Table 4.⁵ There are several differences in results across these tests worth noting:

- The results in column 1 imply that the impact of employment protection is almost three times as large as the results shown in column 3, whereas the results shown in the fourth column actually indicate that increased employment protection leads to *lower* unemployment.
- In column 1, increases in bargaining coordination at levels below the cross-country mean are found to substantially reduce unemployment; more modest reductions are implied by the results shown in second and third columns; and the fourth test (the one used for the simulations in the paper) implies that higher levels of bargaining coordination has essentially no effect on the unemployment rate.
- The implied impact of changes in union density has the same sign in all four sets of regression results, but the magnitude of the implied impact varies substantially. The

results from the regression shown in column 2 imply an impact of changes in union density that is nearly *twenty* times as large as the results from the regression in column 4.

- Only the coefficients for the unemployment benefit replacement rate variable are roughly similar across the four regressions, but only in variant 4 is the coefficient statistically significant (at just the 10% level).

With the notable exceptions of Blanchard and Wolfers (2000) and the OECD's recent Background Paper #1 (OECD, 2006), there has been little evidence of concern in the panel data regression literature over the sensitivity of the results that are published to minor changes in years covered, the quality of the measures used (see below), or the mix of variables tested.

3.4 Robustness in Replication

One of the problems with the studies done prior to the availability of OECD measures was the questionable quality of the institutional measures. Atkinson and Mickelwright (1991) criticized the measure of unemployment benefit duration used by Layard (see below). Blanchard and Blanchard and Wolfers warn of the inadvertent bias that may have crept into the construction of the first generation of these variables:

One must worry however that these results are in part the result of research 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 (Blanchard and Wolfers, 2000, p. c22).

Baker et al. (2005) explored the sensitivity of the main results in Nickell's influential (1997) paper to newer versions of the institutional variables. For six of the eight institutional variables used in the 1997 tests we substituted data used by the same author in more recent work (Nickell et al. 2005). We also used alternative measures of union coverage (from Blanchard and Wolfers, 2000) and active labor market policies (OECD).⁶ With these newer versions of the same institutional measures, the regression produces markedly different results. In Nickell (1997), seven of the eight institutional variables had the expected 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. With the new data, only union coverage is significant (at the 10 percent level), and three of the remaining seven institutional variables have the wrong sign.⁷

Similarly, Baker et al. (2004) explored the robustness of the IMF's results. Table 5 summarizes the results of tests we ran that used the IMF's (2003) dataset and framework as the starting point. As a benchmark for the subsequent analysis, the first three columns of the table show the results of our attempts to replicate the IMF's three most important specifications of the relationship between unemployment and labor market institutions. We present all three specifications here because the IMF defends each as a reasonably good representation of reality. The first column, based on the IMF's "variant one," is essentially identical to the IMF's published results. Columns 2 and 3 (based on IMF variants 3 and 4) are very similar to the IMF's published results.⁸

The fourth column of Table 5 presents the results from our alternative test, based on a composite of specifications used in earlier research in this area. It is important to note that this is our original specification – we tested no alternatives. This alternative specification differs from the IMF variants in several important respects. First, the new regression includes common time dummies, rather than country-specific time trends.⁹ A second important difference is that the new regression in column four uses a different set of institutional variables and interactions: slight differences (improvements) in the union density, the benefit replacement ratio, and the tax wedge measures (typically involving changes to a few countries in a few years); a variable for the duration of benefits; and two new interaction variables – one interaction between benefit duration and the benefit replacement rate, and another between the tax wedge and bargaining coordination.

With the new specification, column 4 of Table 5 shows that only one of the direct institutional variables is statistically significant (the tax wedge, which is significant only at the 10% level). Minor changes to a few variables and reasonable changes in specification produce results that show no meaningful relationship between labor-market institutions and unemployment.

The fifth column of Table 5 presents an alternative test of the robustness of the IMF specifications. To the extent that the relationships posited in the original IMF model are true in the levels of unemployment and labor-market institutions, these relationships

should also hold in changes in unemployment and labor-market institutions.¹⁰ Column five shows the results of estimating the IMF's variant four (in column three of this table) in first differences. In this specification, only union density and bargaining coordination have a statistically significant effect on unemployment. Employment protection, benefit replacement, the tax wedge, and the interactions terms have no statistically discernible impact on the cross-country pattern of unemployment.

3.5 Assessment

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4. How Important is Unemployment Benefit Compensation?

4.1 Some Initial Considerations

It would seem perfectly natural that a system designed to provide insurance against unemployment would become more generous as the unemployment rate increased: with a decline in employment or the rate of employment creation, taxpayers could reasonably want to give unemployed workers additional time to search for the right job, thereby improving the eventual quality of the match between workers and jobs. More generous benefits would also reduce the harmful effects of lost income on workers and their families (and, of course, this additional income would help sustain aggregate demand during economic slowdowns). The social, psychological, and health-related costs of unemployment are well established (Korpi, 2002) and there can be little doubt that many workers would choose employment over the dole, no matter how generous (Gallie and Alm, 1997).

On the other hand, it is also obvious that as benefit entitlements become more generous, the costs of unemployment to workers decline and the incentive to search or take a job if unemployed is reduced. This dynamic could help increase the bargaining power of both job applicants and employees, and higher wages may reduce labor demand and thus raise unemployment or joblessness. In the end, the extent to which any association between the unemployment rate and unemployment benefit entitlements reflects mainly the response of policy to the state of the labor market or the response of

workers to compensation that reduces incentives to work and raises bargaining power, is an empirical question.

Spurred by the development of job search theory, the availability of unemployment benefits has become the cornerstone of the orthodox theory of unemployment. As Holmlund (1998, p. 115) puts it, “In almost all natural rate models, there is a positive relationship between the (equilibrium) unemployment rate and the “generosity” of the UI system. In fact, in many simple models, the wage replacement rate provided by UI is often the only explicit exogenous variable that determines unemployment; see, for example, the survey of natural rate models by Johnson and Layard (1986) in the *Handbook of Labor Economics*.” This centrality of the role of benefit entitlements has withstood recognition that all OECD-member country unemployment benefit systems include eligibility restrictions (Atkinson and Mickelwright, 1991) and the fact most offer compensation (the “replacement rate”) at substantially lower levels than previous pay for limited durations.

A good example of the application of this theory to the real world of high European unemployment is Lungquist and Sargent’s (1998, p. 547) influential paper. At the heart of their model is the proposition that high unemployment in European welfare states can be explained by the “adverse effects of generous unemployment compensation” when their economies face “turbulent times.” Without reference to policy endogeneity, and with little reference to direct empirical evidence, they conclude with a recommendation that welfare states redesign their safety nets “to incorporate incentives to work” (548). This echoes, as Lundquist and Sargent point out, Layard, Nickell and Jackman’s assertion (which also appears in their 1994 book, quoted above) that “unconditional payment of benefits for an indefinite period is clearly a major cause of high European unemployment.”¹¹

Indeed, as Table 3 illustrates, the preponderance of panel data evidence shows at least a statistically significant (if not economically important) effect of unemployment benefit generosity on unemployment. For example, comparing their results with those of Nickell and Layard (1997), Elmeskov et al. (1998) write that “Both studies assign significant roles to unemployment benefits, collective bargaining structures, active labor market policies... even if the variables in question are defined somewhat differently

between the two studies. Both Nickell et al. (2005) and Belot and van Ours (2005) point to the benefits system as the protective labor market institution most responsible for poor labor market performance. Supportive evidence is often cited from a rapidly expanding microeconomic literature on the effects of unemployment benefit compensation on the labor market transitions of individuals - and particularly on unemployment durations. Country case studies are also enlisted to make the case for the centrality of the benefits system.

There are some reasons to think that the major role played by unemployment benefits generosity in the orthodox account is exaggerated. We consider each in some detail below.

First, the strongest cross-country statistical evidence comes from the use of the OECD's gross replacement rate in regression tests, and this measure is sufficiently inadequate to wonder about the plausibility of the large implied effects shown in Table 6 (1-2 percentage points for a 10 percentage point change in the rate). These include the "gross" (rather than the "net") nature of the measure; the evidence that there is a long lag in response by potential beneficiaries to changes in program rules and benefits; the share of the unemployed who do not get benefits, which is substantial in most high unemployment countries (especially Spain and Italy); closely related, the small impact the replacement rate should have on youth, who account for a large share of the unemployed but are not eligible for benefits in many high unemployment countries; the failure of the country specific replacement rate and unemployment trends to suggest a close relationship for either the "success" stories or the "failure" countries; and the Granger-type causality estimates, which suggest substantial policy endogeneity - any relationship between benefits and unemployment may run at least as strongly from unemployment to benefits as from benefit generosity to unemployment.

Second, we review the microeconomic evidence, which has frequently been enlisted in support of the findings summarized in table 3.

4.2 Interpreting Gross Replacement Rate Effects

The consensus finding that a 10 percentage point change in the OECD's gross replacement rate will change the unemployment rate by 1-2 points seems implausibly

large for a variety of reasons. To begin with, researchers have paid little attention to the actual mechanics of how changes in benefit entitlement generosity are translated into transitions in and out of unemployment. We will address the micro evidence on this below, but it is worth noting here that it is widely recognized that workers operate under conditions of imperfect information, so it is not surprising that they tend to respond with a lag to changes in incentives produced by new public policies. As the OECD points out, this lag can be quite substantial: “The lags between changes in programmes rules and in the number of beneficiaries appear to be very long: between 7 to 10 years in the case of unemployment benefits...” (OECD, (Benefit Coverage Rates) 2004, p. 58). This extremely long lag, however, does not seem consistent with the large implied unemployment-to-benefit elasticities found in regressions using annual data without lagged unemployment benefit variables.

A second reason not to entirely suspend disbelief over large unemployment responses to relatively small changes in the gross replacement rate is that substantial numbers of unemployed workers do not actually receive benefits in the first place. Atkinson and Mickelwright (1991, p. 1692) flagged this long ago: “nearly one in five of those registered as claimants in Britain in November 1988 were in receipt of neither UA nor UI. In West Germany in December 1988 over a third of registered unemployed received neither Arbeitslosengeld (UI) nor Arbeitslosenhilfe (UA).” According to the Eurostat Labour Force Survey, in 1995 the percentage of unemployed receiving any welfare benefit or assistance was: Italy, 7%; Greece, 9%; Spain, 24%; Portugal, 27%; France, 45%; the Netherlands, 50%; Germany, 70%; Austria and Denmark, 66%; Ireland, 67%; Sweden, 70%; and Belgium, 81% (Manning, 1999, p. 144). According to the most recent evidence, the share of “ILO unemployed” that actually receives unemployment benefits in OECD-member countries ranges from around 20 to 80 percent (OECD, Benefit Coverage Rates 2004, fig 3.6).

These rather low coverage rates reflect the effects of two factors. The first is the “take-up” rate – the number of eligible workers who, for whatever reason, do not collect their benefits – which is estimated by the OECD to range from 60-80 percent (Hernanz et al. 2004, p. 4 – “Take-up or Welfare Benefits in OECD Countries: A Review of the

Evidence,” OECD Social, Employment and Migration Working Papers No. 17, March 8). The second is eligibility for benefits, which is particularly important for youth.

Younger workers, who in many countries account for a large part of the unemployment problem and who may be most sensitive to work incentives, may be either ineligible for benefits in the first place or they are eligible only for lower levels of benefits. The OECD’s Jobs Study (1994, p. 184) explored the association across countries between the maximum duration of wage-related insurance benefits (for a worker with a good employment record at age 20) with the share of long-term unemployment for ages 14-24: “For young people there is no correlation, perhaps because of the limited relevance for them of insurance benefits.”

More recent evidence supports this conclusion. Table 7 shows youth unemployment rates and net replacement rates for 20 year old single workers. Among the four largest continental European countries, France (26.5%), Spain (28.3%) and Italy (31.1%) have by far the highest youth unemployment rates, but youth in these three countries were ineligible for unemployment insurance benefits (see column 2). As the OECD’s Benefits and Wages (2004, p. 34) report puts it “In France, Italy and Spain, the incomes of unemployed 20-year-olds without employment record are likely to be strongly dependent on informal family support as they qualify for none of these social benefits.” With reference to incentives, it is interesting to note that a number of countries with very low youth unemployment (at or below 10%) are, according to the net replacement measure, extremely generous to their youth (Germany, the Netherlands, Ireland, and Denmark).

For these reasons, the OECD’s gross replacement rate might not be expected to be closely related to the overall unemployment rate over time, and this is just what the country-specific time series appear to indicate. Figure 9 presents within-country unemployment and replacement rate trends for four “success story” countries and four “failure” countries. The “GRR” is the explanatory variable that so powerfully accounts for “UR” in the regression tests reported in Table 3.

Panel A of Figure 9 shows the GRR and UR trends for each of the success stories. While Denmark’s success has been the sharp decline in unemployment since 1993, GRR was stable at a high level over the previous decade, spiked upward between 1993 and 1995, and remained over 60% until 1999 – the highest of any OECD country. The

Netherlands shows a modest hike in its GRR in the mid and late 1980s to above 50 percent, where it has stayed, but beginning in the mid-1980s, unemployment fell dramatically and fairly steadily through the end of the 1990s. This change in GRR measured by the OECD appears to contradict a national measure that shows a sharp decline between 1980 and 2000 (from 71% to 56%) that is heavily relied upon in the explanations given for the “Dutch Miracle” by Broersma et al. (2000) and van Ours (2003).¹² The Irish unemployment rate rose drastically in the early 1980s, stayed very high between the mid-1980s and mid-1990s, and then has fallen equally dramatically since, while the Irish GRR has remained fairly stable. The British unemployment rate has increased sharply twice and fallen sharply twice since the late 1970s while its GRR has edged gradually downward.

It is notable that if there is any relationship between GRR and UR trends for these four success stories, Granger-causality tests indicate that it is the unemployment rate that predicts the benefits level in each case – just the reverse of the orthodox prediction. The first four columns of Table 8 show the F-statistics of the Granger tests that the benefits “cause” unemployment --in Granger's sense that past values of the generosity of unemployment benefits are useful in predicting future values of the unemployment rate. To allow for the broadest test of the relationship, we present results separately for Granger tests that include one, two, three, and four lags of the gross replacement rate. Changes in unemployment “predict” changes in the benefits measure (GRR) for Denmark with significance at the 5-10 percent level for all four lags; for the UK at the 5-10 percent level for each of the first 3 lags; for the Netherlands at the 1 percent level for the first 2 lags and at the 5-10 percent level over the 3rd and 4th year; and for Ireland with significance at the 1 percent level in the first year. Interestingly, another success story – the U.S. – shows this “reverse causality” as well for lags 3-4.

Returning to Figure 9, Panel B shows the GRR and UR trends for four “failure” countries. At least until the late 1990s, the trends for France appear to move roughly together, but it is clear that unemployment took off in the 1970s well before GRR began to edge up. Despite the pro-market policy shift in the early 1980s, French unemployment rates continued to rise, and in response to political protests, “the authorities expanded social spending to help protect workers from dislocation and to undercut resistance to measures of

economic liberalization” (Levy 309). This is consistent with the Granger results shown in Table 8 for France: all four lags show a significant relationship from unemployment to GRR, and this is particularly strong for the first two years.

Nor do the trends in Panel B of Figure 9 suggest the mainstream account for the other three large high unemployment countries. Germany shows a steadily rising UR but a stable GRR. The trends graph for Spain shows that since the mid-1980s GRR remained stable at a moderate rate of just above 30%, while unemployment shows a huge increase from the late 1970s through the 1980s, peaked at over 20% in 1994, and then has fallen sharply and steadily to 10.9% 2004. And finally, Italy’s unemployment rate rises steadily until 1995, remains stable and then falls steadily after 1998 despite rapid increases in a GRR that was effectively introduced in 1992. Like France, the four “success stories,” and the U.S., the Granger results in Table 8 indicate that the benefits measure *follows* rather than precedes changes in the unemployment rate.

Evidence that benefit generosity reflects the state of the labor market (“policy endogeneity”) has also been found by the OECD. Elmeskov et al. (1998, Table A.3) report results broadly consistent with ours - Granger-causality running 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. More recently, in the “Political Economy of Structural Reform” (OECD 2006, p.18), the OECD concludes that “there is evidence that unusually high increases in unemployment rates are associated with increased employment protection ... and relatively more generous unemployment benefits for the long-term unemployed (the latter is also triggered by higher long-term unemployment).”

Finally, much has been made of the role that stricter rules and stricter enforcement of them have played in accounting for sharp declines in unemployment in countries like the Netherlands and Denmark. For example, van Ours (2003, p. 11) argues that “the introduction of a system of benefit sanctions may be one of the main policy measures responsible for the ‘Dutch Miracle’.” Similarly, Nickell et al. (2005) point out that despite continuing to provide “very generous unemployment benefits,” the Danish benefit system was “totally reformed” in the 1990s by “tightening of the criteria for benefit

receipt and the enforcement of these criteria via a comprehensive system of sanctions.” For evidence of the importance of these reforms, they point out that the “The Danish Ministry of Labour is convinced that this process has played a major role in allowing Danish unemployment to fall dramatically since the early 1990s without generating inflationary pressure” (p. 4-5).¹³

While tightening eligibility rules and their enforcement will tend to reduce participation in the unemployment compensation system, we are not entirely convinced that these reforms played a major role in the dramatic decline in unemployment in either the Netherlands or Denmark. Referring to a new law toughening sanction policy that went into effect in August 1996, van Ours (2003) presents a figure that shows the evolution of the number of unemployment benefit recipients and the number of sanctions. Sanctions increase sharply from 1996 to 1997, stay at that level in 1998, and then fall back in 1999 almost to 1996 levels. But his figure also shows that benefit recipients fell noticeably two years *before* the law went into effect, and continued to fall after the number of sanctions began to fall in 1999. This 1997-8 surge in sanctions took place in the midst of a collapse in unemployment rates – from 6.8% in 1994 to 3.2% in 1998 to 2.5% in 2001 – which suggests that the tightening of sanctions may have been more a reflection of a strengthening labor market (in which it is easier to be tougher) than a principal cause of this employment performance “miracle” in the first place. Indeed, van Ours (2003, figure 3) reports a spectacular takeoff in total number of working hours between 1995 and 2000.

If effective tightening of eligibility for participation in the unemployment benefit system played a leading role in reducing unemployment rates (despite keeping highly generous *levels* of benefits), we might expect to see low benefit recipiency rates, particularly relative to the country’s unemployment rate. Figure 10 shows unemployment rates and unemployment benefit recipiency rates (recipients as a share of the working age population), ranked by unemployment rate in 1999. The Netherlands achieved an unemployment rate of 3.2 percent by the end of the decade, the best among these 16 countries, with a benefit recipiency rate of 4 percent. With a lower unemployment rate than the U.S., Japan, or the U.K., the Netherlands showed a higher benefit recipiency rate: the Netherlands was able to outperform these countries on unemployment despite a

substantially larger share of the population receiving unemployment compensation. At the same time, Sweden and Spain had the same benefit reciprocity rate as the Netherlands, and the French rate was only slightly higher, despite much higher unemployment in all three countries: a similarly generous benefits system in terms of access did not yield Dutch levels of unemployment.

Figure 10 produces a similar story for Denmark. After all reforms of the 1990s, Denmark's benefit reciprocity rate was still higher than seven of the 15 other countries in the figure, including Sweden and Spain, and was almost as high as the French rate. While both the Netherlands and Denmark reduced their benefit reciprocity rates between 1990 and 1999 (from 5.01 to 4.1 in the Netherlands and from 7.6 to 4.35 in Denmark), we would expect a decline in benefit recipients as labor markets improve. Nevertheless, both countries show quite high recipient rates relative to their unemployment rates, suggesting, after the spate of 1990s reforms, that each has achieved its successful employment performance while maintaining a relatively large and generous unemployment benefit entitlements system.

In sum, while the labor market institution that shows the strongest, most robust relationship to changes in the pattern of unemployment in the cross-country regression studies is the OECD's measure of unemployment benefit generosity - the gross replacement rate (GRR) - there are a number of reasons to maintain a healthy skepticism. First, as the OECD would be the first to acknowledge, the GRR is at best a rough measure of the incentives confronting workers (because it is a gross rather than net measure, and because it does not capture other key dimensions, such as duration, eligibility and enforcement). Second, there is typically a long lag between program changes and potential recipient responses (7-10 years according to the OECD, though, we believe any lag is likely to be shorter than this), so the typical regression (which is almost never run with lagged replacement rates) cannot effectively reflect changes in program generosity. Third, only a portion of the unemployed receive benefits, and youth, who account for a large share of unemployment in the high unemployment countries, are generally ineligible for benefits. Fourth, as Figure 9 indicates, the time trends between gross replacement rates and unemployment show little correspondence for either the "success" or the "failure" countries. Fifth, Granger tests indicate that in most cases it is

unemployment that predicts benefits, not the reverse (the case in all four success stories and two of the four failure countries). And sixth, as Figure 10 shows, the absence of any relationship between benefit reciprocity rates and unemployment rates suggests that benefit generosity, strictness of eligibility rules, and the tough enforcement of those rules do not distinguish the “success stories.”

4.3 The Microeconomic Evidence

4.3.1 Evidence from Literature Surveys

Evidence of the effects of unemployment benefit entitlements on individual behavior has frequently been cited in support of the orthodox interpretation of the macro evidence. A good example is Elmeskov et al. (1998, p.):

Turning to the role of labour market policies, there is strong evidence that more generous unemployment benefits (UB) lead to higher structural unemployment. The implicit average elasticity of unemployment with respect to the OECD summary measure of benefit entitlements is around 0.4, a value which is close to those often found in the microeconomic literature (Holmlund, 1998). These findings suggest that the effects of generous benefits on the reservation wage of unemployed job-seekers and/or on wage bargaining dominate any positive impact of benefits on search effectiveness.

Nickell et al. (2005, p. 4) also cites Holmlund (1998), along with chapter 8 of the OECD Jobs Study (1994), for microeconomic evidence that the level of unemployment benefits will, as they put it, “influence equilibrium unemployment.”

It is notable that a close look at what Holmlund actually writes produces a strikingly different picture. The only reference to an elasticity estimate of “.4” comes in a sharply critical discussion of a paper by Mortensen (1996). Holmlund refers to Mortensen’s “simulation results of UI policies in a parameterized version of the Mortensen and Pissarides (1994) model... A rise in the replacement rate from 30 to 40 percent would increase unemployment by at least 4 percentage points, and possibly by more than 10 percentage points, according to these simulations” (Holmlund, p. 124). Holmlund suggests that these estimates are implausibly large:

“The most likely reason why benefit hikes apparently have a much stronger impact in Mortensen’s experiments than in those reported in Table 1 is because Mortensen imputes a non-trivial value to leisure... Unfortunately, economists know virtually nothing about a reasonable estimate of the leisure value of

unemployment. A liberal interpretation of some empirical evidence on unemployment and psychological well-being suggests that the value may well be *negative* (emphasis in the original); see e.g., Blanchflower and Oswald (1997). Policy simulation that hinge crucially on assumptions concerning unobservables should therefore be used with more than the usual caution as prediction so what is likely to happen if a particular policy is implemented” (p. 124-5).

So, far from being a consensus estimate of the microeconomic literature by Holmlund, this .4 estimate is actually an example of an implausibly large estimate from what Holmlund suggests are thoroughly fanciful policy simulations. Indeed, the Elmeskov reference to Holmlund appears to directly contradict Holmlund’s own assessment: “Do the estimates from micro data give reliable answers to general-equilibrium questions about the effects UI on unemployment? In general, the answer is no” (p. 125). Holmlund goes on to conclude that “The weight of the evidence suggests that increased benefit generosity causes longer spells of unemployment and probably higher overall unemployment as well. But there remains a considerable degree of uncertainty regarding the magnitudes of these effects” (p. 137). Holmlund presents no direct evidence in support of effects on “overall unemployment” and makes clear that his conclusion reflects, not the balance of the statistical evidence, but “my own judgment” (p. 126).

Two responses to Holmlund’s survey were published in the same issue of the *Scandinavian Journal of Economics*. Manning (1998, p. 143) suggests that Holmlund’s judgment call in the absence of evidence reflects the dominance of orthodox renditions of mainstream theory: “the strength of the evidence linking the generosity of the benefit system and unemployment is not as strong as we would like and our belief in such a link derives more from the theory than from the evidence.” In a second comment, Strom (1998, p. 151) makes it perfectly clear that the evidence that Holmlund does reference is not at all compelling: “Neither microeconomic nor macroeconomic results give strong and/or convincing support to the predictions of a strong positive relationship between unemployment and unemployment benefits as indicated in microeconomic and macroeconomic theory.”

This is probably a stronger negative position than Holmlund would accept, but there is little microeconomic support in his survey for “major effects” of unemployment

benefit generosity on the unemployment rate, as suggested by Layard et al. (1991; 1994) and Elmeskov et al. (1998), among others. As Holmlund (p. 138) puts it, “We are a long way from a situation where economists can with any confidence provide policymakers with reliable menus for choice among key UI parameters.” This caution reflects an assessment similar to that of Atkinson and Mickelwright (1991, p. 1712) some 7 years earlier: “As with the U.S. and the U.K., the evidence (from the rest of the OECD) does not suggest that the effects of benefits on transitions out of unemployment (however defined) are large or measured with precision.”

4.3.2 Unemployment Duration Evidence

Most of the micro level research has focused on program effects on the duration and exit rate out of unemployment. While this has often been conflated with effects on the overall unemployment rate, there is no direct correspondence. Atkinson and Mickelwright (1991, p. 1710) point out that greater benefit generosity may affect only the composition, not the level of unemployment. “Suppose for example that *ceteris paribus* we observe that persons with higher benefits exit unemployment more slowly. This does not necessarily mean that aggregate unemployment is higher since the refusal of jobs by one group may lead to the work being offered to others. In other words it is the composition of unemployment which is altered.” Another reason longer duration induced by benefit generosity may not produce higher unemployment levels has been labeled the “entitlement effect.” As Holmlund (1998, p. 116) explains: “for some workers, in particular those who do not qualify (or have ceased to qualify), higher benefits will make work *more* attractive relative to unemployment. The effect of higher benefits on the duration of unemployment is therefore, in general, ambiguous.” And if work becomes more attractive because it qualifies a worker for more generous benefits should they become unemployed, this lower duration might also tend to *reduce* the unemployment rate.

Nevertheless, early research suggested that benefit generosity had a strong positive effect on unemployment duration and that this, in the words of Lancaster and Nickell (1980) “is now a rather firmly established parameter.” This assessment was repeated in Layard et al. (1991). At about the same time, Barr (1992, cited by Hammer, 1999, p.

132), disagrees: “Despite continuing controversy, the general conclusion is that though the duration of unemployment is likely to be slightly longer at higher replacement rates, the magnitude of the effect is not large.” With the benefit of research done in the 1990s on this question, Holmlund (1998, p. 118) contends that the Lancaster and Nickell conclusion “was surely premature. The effect of benefits on unemployment duration is far from a firmly established parameter that is comparable in robustness to, say, estimates of the returns to schooling.”

More recent studies have found stronger evidence for a benefits effect on unemployment duration. For Norway, Roed and Zhangs (2003, p. 204) find that a 10 percent decline in benefits “may cut a 10-month duration by approximately one month for men and 1-2 weeks for women.” For Austria, Lalive and Zweimuller (2004), examine a massive policy change provided an ideal “natural experiment.” Anticipating deteriorating labor market conditions in regions with heavy steel industry employment, the Austrian government “dramatically increased benefit generosity” for some workers in certain regions from 30 to 209 weeks. They found a reduction in the transition rate to jobs of 17 percent, which meant “increasing unemployment duration by about 9 weeks, leading to an increase in unemployment duration per week of additional benefits of .055” (p. 2610). The distinctiveness of this study was that they were able to take into account policy endogeneity – had they not been able to account for actual changes in the labor market, the decline in the transition rate to jobs would have appeared to have been much larger - 40 percent.

Jan van Ours and Vodopivec (2005, p. 3) investigate a 1998 reform of the benefits system in Slovenia that “drastically reduced the potential duration of unemployment benefits” and find “important and sizeable disincentive effects” (p. 17). With the drop in the maximum duration of entitlements from 12 to 6 months, they found that the share of unemployed who found a job within 6 months rose from 44 percent to 52.4 percent, an 8.4 percentage point gain. But interestingly, those who exited unemployment but not into employment (out of the labor force) increased from 6 percent to 15.1 percent, an increase of 9.1 percent. As the authors point out, the benefits of the rise in exits to employment “have to be weighted against possible additional hardship created by the curtailment of benefit entitlement, as well as worse quality of post-unemployment jobs in terms of their

stability, type of appointment, and precariousness” (p. 17). In research on West Germany, Pollmann-Schult and Buchel (2005) explore the effects of the duration of benefits on the quality of post-unemployment jobs. They conclude that “although receipt of benefits delays exits from unemployment, it raises aspiration levels and hence improves the quality of the eventual job match” (p. 35).

Evidence from micro data suggests that unemployment benefit generosity has quite limited effects on youth, consistent with the evidence presented above. In their study of cross country differences in the transition from unemployment to employment for youth, Russell and O’Connell (2001) find that successful transitions are inversely related to unemployment generosity – controlling for differences in individual characteristics, Denmark and France show the highest exit rates to jobs while Spain, Italy and Greece show the lowest rates. “Of the institutional factors considered, only unemployment compensation payments did not operate in the expected manner: contrary to economic orthodoxy, lower levels of benefit coverage were associated with lower rates of exit to employment.” Part of this finding may be explained by the more extensive use of active labor market policies (ALMP) in the northern European countries, which facilitate job preparation and search. This is certainly the case for Denmark, which has perhaps the most generous benefits system, but whose effective ALMP combined with strict enforcement of participation in these programs have helped produce a very low unemployment rate (Danish Ministry of Finance, 1998).

4.4 An Assessment

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5. Labor Market Reforms and Unemployment

5.1 The OECD’s Policy Reforms and NAIRU Evidence

A central pillar of OECD labor market policy has been that reforms that reduce labor market rigidities are the answer to persistent high unemployment. An enumeration of such reforms was carried out by the OECD (1999) as part of its follow-up to *The OECD Jobs Study* (OECD 1994) and provides a comprehensive listing of changes in the generosity of unemployment benefits, the strictness of employment protection laws, the

level of minimum wages and the like, focused on the period from 1995 but also with summary data from the early 1990s. The OECD also listed all the reforms recommended for each country in its labor market reviews, developed a weighting system for assessing their significance, and then analyzed whether the recommended reform had been fully implemented, partially implemented, ignored, or even flouted (in the sense that policy had moved in the “wrong” direction). The OECD’s “follow-through” measure was defined as the share of recommendations actually adopted, wholly or partially by each country.

The OECD found a significant positive relation between this measure of “follow-through” and the extent to which the unemployment (the NAIRU) fell in the 1990s (OECD 1999b: figure 2.7). This is presented as evidence that there was a payoff to the implementation of Jobs Strategy recommendations. Apart from any issues of the definition or weighting of reforms, the problem with this approach is that it ignores the very different number of recommendations for labor market reforms that each country received from the OECD. For example, recommendations varied from 4 in the case of US and Australia to 21 for Finland and 23 for Germany. The effect of reforms on unemployment should presumably depend on how many were actually implemented, not simply the *proportion* of recommendations implemented. One would think that the implementation of 11-12 recommendations by Germany (50%) would have a greater payoff to employment performance than the implementation of just 2 by Australia (50%).

Accordingly, we constructed an alternative index showing the “volume” of labor market deregulation recommendations that were actually carried out, which depends on both the number of measures advocated by the OECD and their “follow-through” by the countries (for details, see Baker et al., 2005). We limited our index to the OECD’s list of reforms related to unemployment benefits, employment protection, and wage bargaining systems, as these constitute the key labor market institutions typically regarded as employment-unfriendly.

Figure 5 compares this alternative index of labor market deregulation in the 1990s with the OECD’s estimate of the change in structural unemployment over the same period for 21 OECD member countries. The figure shows no significant relationship between this narrower and more appropriately defined measure of deregulation and the

change in unemployment across OECD countries. Ireland is an extreme case, with the most dramatic fall in unemployment accompanied by rather little labor market reform. But even if Ireland is excluded (and this would be hard to justify), the relationship between deregulation efforts and structural unemployment across countries still appears very weak (only about one tenth of the variance in the change in unemployment is “explained”). By this measure of labor market reforms, the changes in structural unemployment across the major OECD member countries in the 1990s are not systematically associated with the extent of labor market reform.

The OECD is currently undertaking a major reassessment of its Jobs Strategy and has updated its survey of labor market reforms since 1994, the year of its Jobs Study (Brandt, Burniaux and Duval 2005). This new study adopted our “volume” approach to measuring reforms and found a significant correlation between the amount of labor market reform over the period 1994 to 1999 and changes in unemployment after 1998. This appears to confirm the earlier OECD finding that there is a statistical association between adoption of labor market reforms and lower unemployment.

But this latest confirmation of the payoff to reforms by the OECD depends on assuming a rather long lag and is driven by two of the “reforms” – increased spending on Active Labor Market Policies (ALMP) and reductions in the aggregate “tax wedge” between gross labor cost and the wage received by workers. The OECD’s Jobs Strategy calls for greater public investment in ALMP (such as retraining and matching of workers with vacancies). While it is viewed as a way to promote flexibility and efficiency in the labor market, it is an intervention into the labor market by the state and as such is surely not part of the orthodox deregulation agenda. Similarly, the overall tax burden in the economy, while it may have labor market effects, reflects a myriad of influences and is certainly not adjusted primarily with labor market effects in mind. Accordingly, we again developed an indicator of labor market reforms focused on the core deregulation agenda of employment protection, unemployment benefits and wage-setting. Again, we adopted the basic OECD data, methodology and judgment calls on the identification and weighting of reforms and the lag period. Repeating the recent OECD’s exercise with our more targeted labor market reforms indicator again shows no significant relationship with

subsequent unemployment declines (not shown), confirming the patterns shown in Figure 5.

In sum, appropriately defined measures of labor market reforms do not provide strong evidence that labor market deregulation has yielded significant payoffs in the form of reduced unemployment.

5.2 Nickell's Index of Reforms and Unemployment

Another example of the use of simple correlations with an aggregate indicator of labor market reforms to show that labor market deregulation helps reduce unemployment appears in a recent paper by Stephen Nickell (2003). Based on his judgments of what changes in labor market institutions qualify as employment-enhancing and what are employment-unfriendly, Nickell develops a labor market reforms scorecard and relates it to the change in unemployment between the early 1980s and the late 1990s.

Nickell makes two related empirical claims. The first is that the problem of European unemployment today is concentrated in “the big four”: France, Germany, Italy and Spain. This is an important point since a good explanation of the “European” unemployment problem must explain the persistence of high unemployment in these four big European countries. But nearly all of the analysis in this recent paper is dedicated to the much bolder second claim, which is that this pattern of high unemployment, and indeed the general cross-country pattern of changes in unemployment, can be well-explained by his reforms scorecard. The objective of the scorecard exercise is to “see how these institutional variables have changed over time and what these changes can tell us about why the European Big four countries have performed less well than most other countries on the unemployment front in the 1990s.”

In a simple OLS regression, Nickell accounts for 51% of the variation in unemployment over these two decades for 20 OECD countries with the sums of “ticks” (good changes) and “crosses” (bad changes) on nine institutional measures. It is this finding alone that Gilles St. Paul (2004: 53) cites for his conclusion that “evidence supports the traditional view that rigidities that reduce competition in labor markets are typically responsible for high unemployment.”

We do not take issue here with the reforms scorecard per se. This means we set aside questions regarding 1) which institutions to include (e.g., taxes and ALMP might not be considered, for reasons mentioned above); 2) what threshold should determine a cross or a tick for each of the 9 measures; the poor quality of some of the measures (e.g., there is in fact no good cross-country measure of the strictness of eligibility rules for unemployment insurance, much less how this measure may have changed over time); and 3) the fragility of the simple regression results (see Glyn et al. 2006). We do address only the apparent inconsistency between the results that are presented and the conclusion that is drawn from them.

The stated objective is to explain why the big four high unemployment countries have continued to perform so poorly. Based on the ticks and crosses analysis, “We may reasonably conclude that the countries which had very high unemployment in the early 1980s and still have high unemployment today simply have too few ticks and/or too many crosses.” But the policy reforms scorecard – the net sum of ticks and crosses – fails to identify three of the four big high-unemployment countries (Spain, Germany and Italy). Figure 6 reports Nickell’s net total of ticks/crosses. It turns out that, among the big four high unemployment countries, only France fits the prediction, and there is some question about the appropriateness of its score.¹⁴ Based on the scorecard, both Austria and Switzerland should have shown about the same poor performance as France. The three other high unemployment countries (Germany, Spain and Italy) get the same scores in the middle of the distribution (0 to 1) as Norway, the U.S. and Belgium. This figure shows that, despite the impressive sounding explanatory power of his unemployment equation (51%), for at least three of Nickell’s four persistent high unemployment countries, it is *not* reasonable to conclude that the problem has been simply too few ticks and too many crosses.

In sum, it has been common to use simple correlation evidence – and even simply relatively high values of protective labor market institutions alone – in support of the mainstream view that these institutions play a central role in the determination of the pattern of unemployment across the OECD in recent decades. But a close look at this evidence reveals, in fact, little or no compelling support. The standard for supportive evidence appears strikingly weak in a number of the influential studies reviewed above,

which focused on unemployment benefits. Our own correlations, using the OECD's gross and net replacement rates, indicate that in almost every case, neither replacement rates nor the duration of benefits is correlated with either standardized unemployment rates or long-term unemployment rates. The one exception is the modest relationship found between change in gross replacement rates and change in unemployment from 1982 to 2002. We also found that in several prominent cases the evidence of correlations between labor market reforms and unemployment has been weak or nonexistent. In general, a common theme is that the evidence presented often fails to warrant the conclusions regarding the key role played by labor market institutions in generating high and persistent unemployment.

6. Conclusion

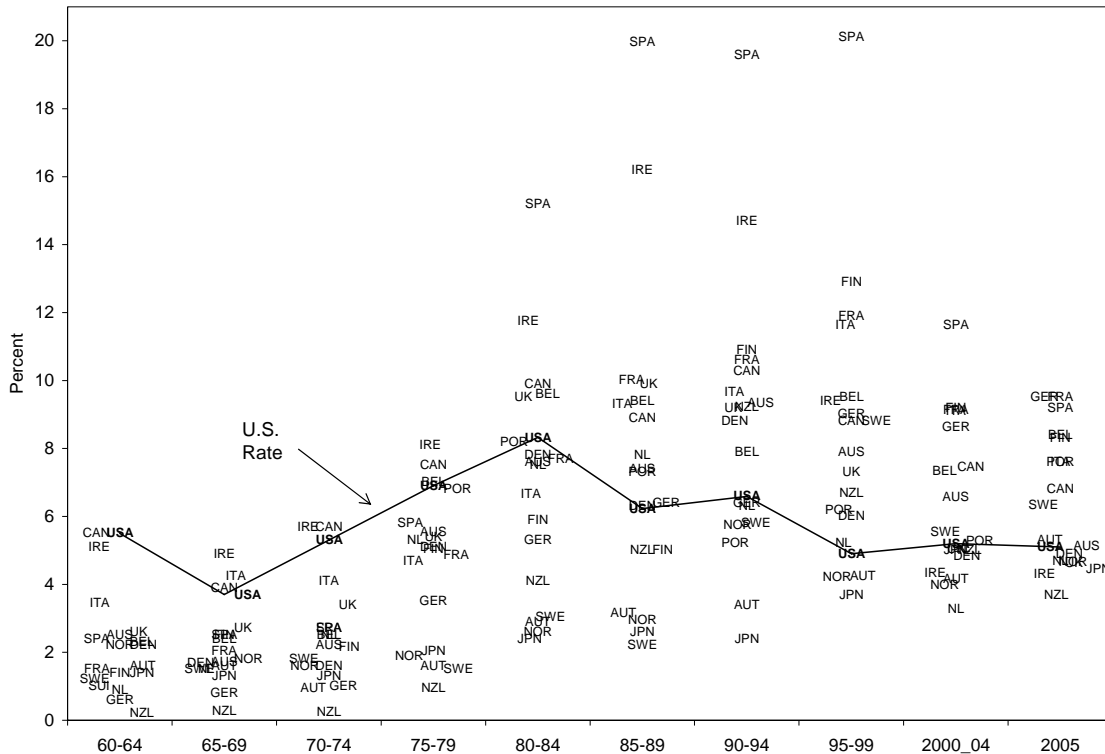
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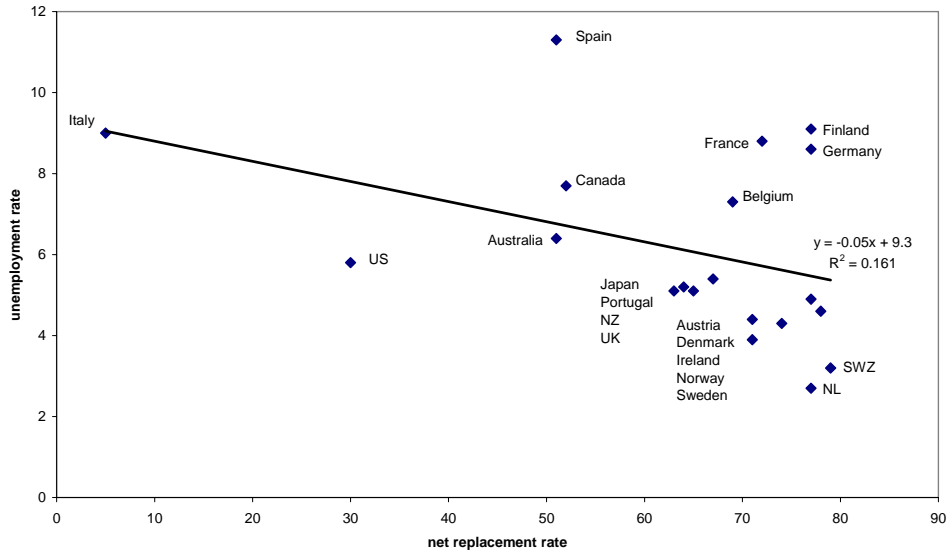
Figure 1: Standardized Unemployment Rates for 19 OECD Countries, 1960-2005



US (%)	5.5	3.7	5.3	6.9	8.3	6.2	6.6	4.9	5.2	5.1
Median	2.2	1.9	2.4	5.1	7.6	7.3	8.8	7.9	5.3	5.2
Std. dev.	1.60	1.22	1.62	2.20	3.35	4.47	3.94	3.93	2.27	1.96

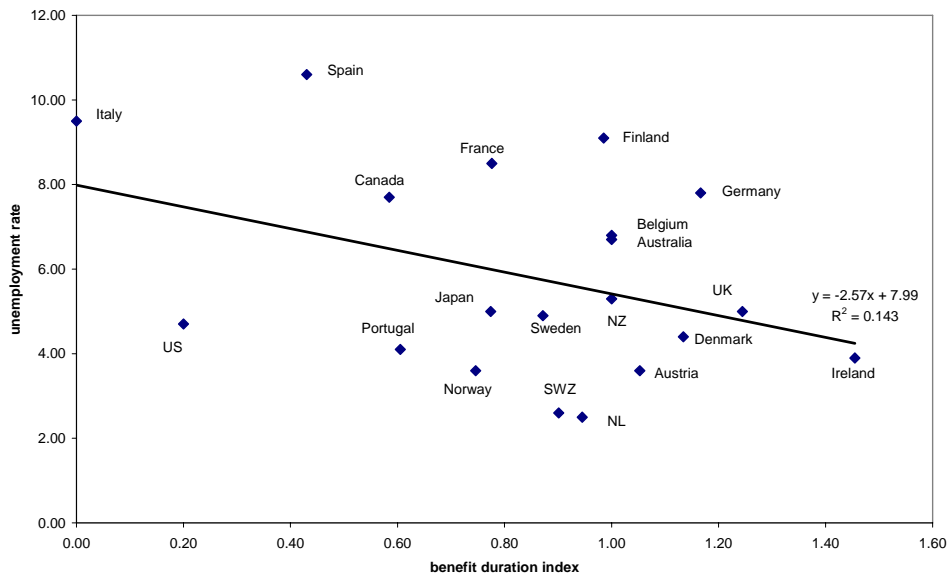
Sources: 5-year unemployment rates, 1960-99: Baker et. al., Appendix 2.
 Unemployment 2000-04: OECD Employment Outlook, July 2005, Statistical Annex.
 Unemployment 2005: OECD online (www.oecd.org).
 Medians/standard deviations: author's calculations.

Figure 2: Net Unemployment Benefit Replacement Rates and Unemployment for 20 OECD Countries, 2002



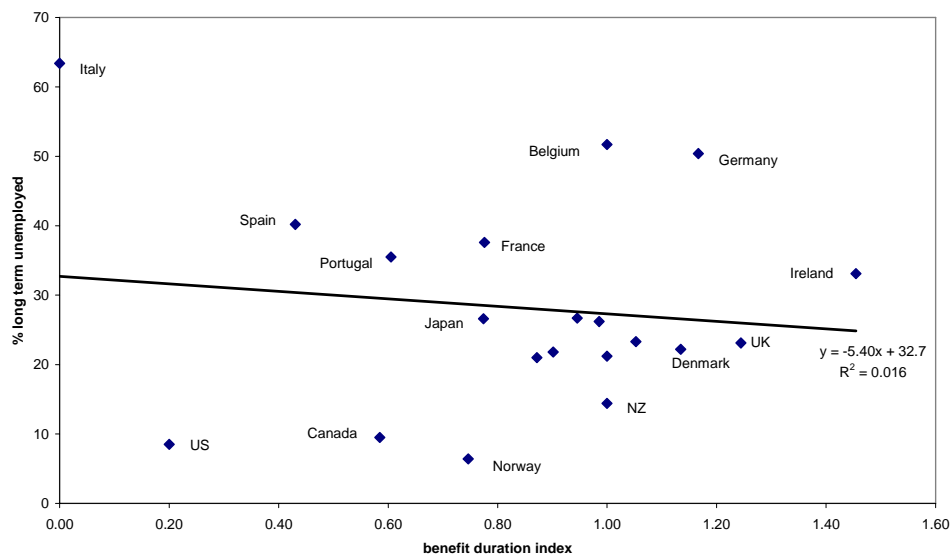
Sources: replacement rates: OECD Benefits and Wages 2004, table 3.3b (overall average net replacement rates over 60 months of unemployment); unemployment rates: OECD standardized rates.

Figure 3: Unemployment Benefit Duration and Unemployment for 20 OECD Countries, 2001



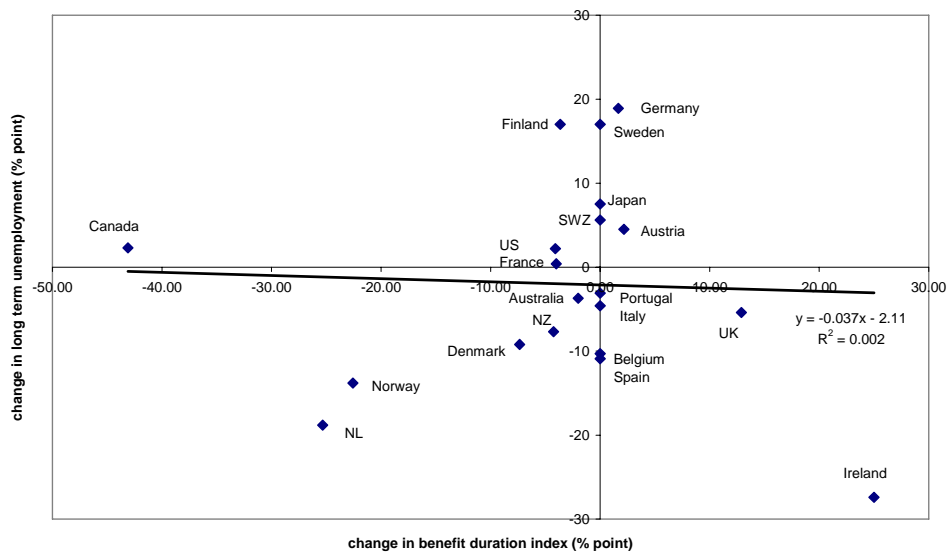
Duration is measured as the ratio of the net replacement rate for the 60th to the rate for the “initial period” (1st month), for single earner married couple without children, at 100% of the average production worker wage (OECD 2004, table 3.1a and table 3.2a).

Figure 4: Unemployment Benefit Duration and Long Term Unemployment for 20 OECD Countries, 2001



Duration is measured as the ratio of the net replacement rate for the 60th to the rate for the “initial period” (1st month), for single earner married couple without children, at 100% of the average production worker wage (OECD 2004, table 3.1a and table 3.2a). Long term unemployment is the share of the unemployed out of work at least 12 months (OECD Employment Outlook, 2002, Table G).

Figure 5: Change in Unemployment Benefit Duration and the Change in Long Term Unemployment for 20 OECD Countries, 1991-2001



Duration and long term unemployment for 1991 are defined as they are for 2001 (see Figure 4 for definitions and sources for 2001). Sources for 1991: benefit duration: OECD Jobs Study, 1994, table 8.1; long term unemployment: OECD Employment Outlook, 1996, Statistical Annex Table Q.

Figure 6: Change in Gross Replacement Rates and Unemployment Rates for 20 OECD Countries, 1982-2002

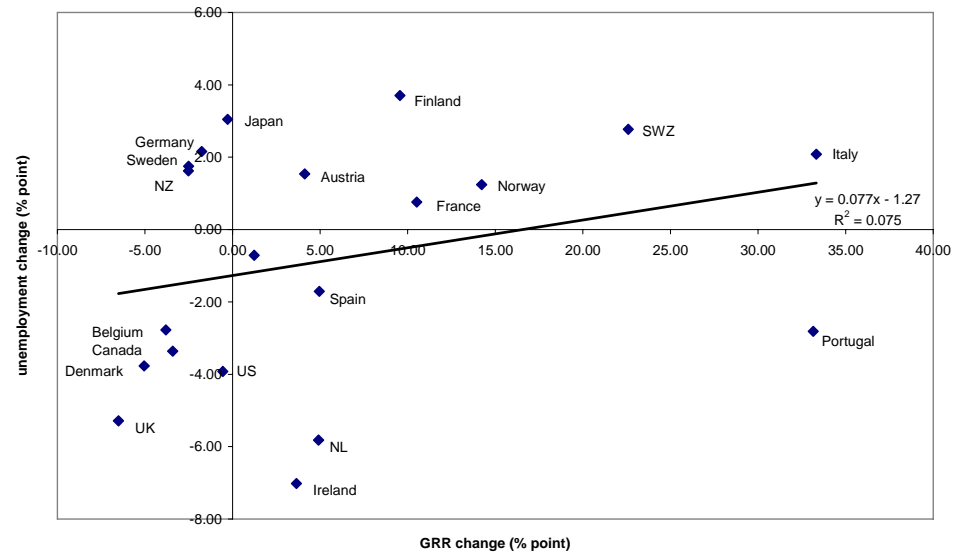


Figure 7: Labor Market Deregulation and Changes in the NAIUR for 21 OECD Countries in the 1990s

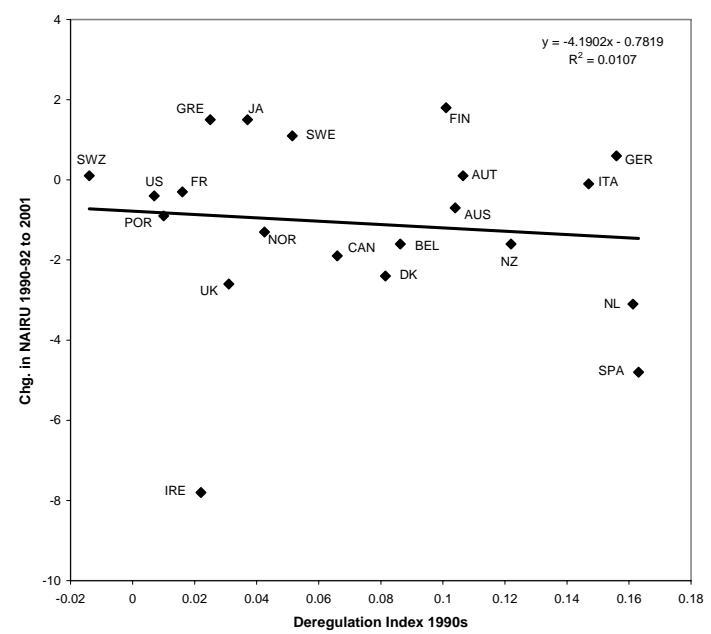
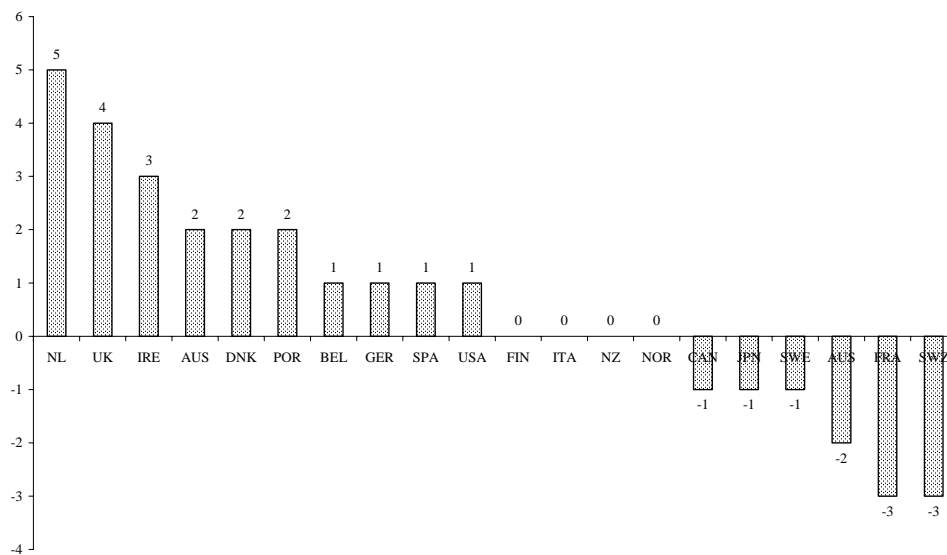


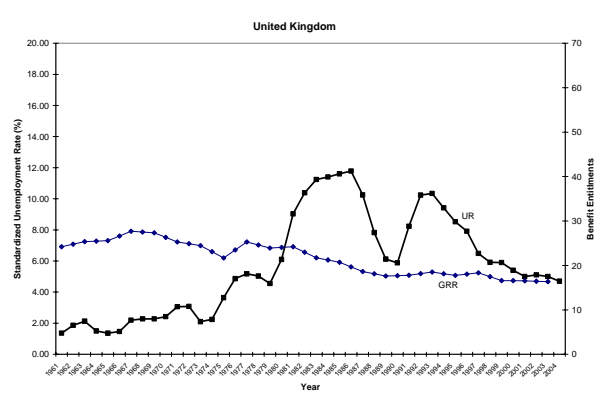
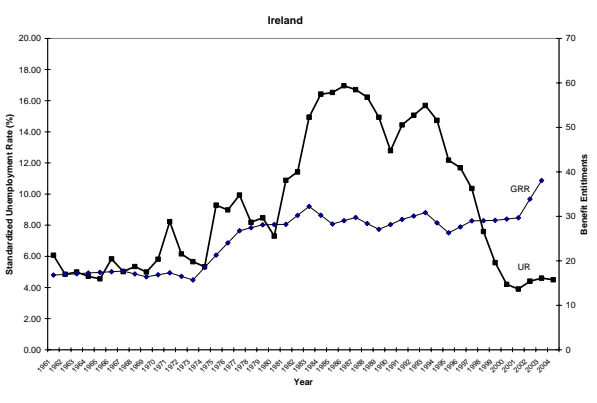
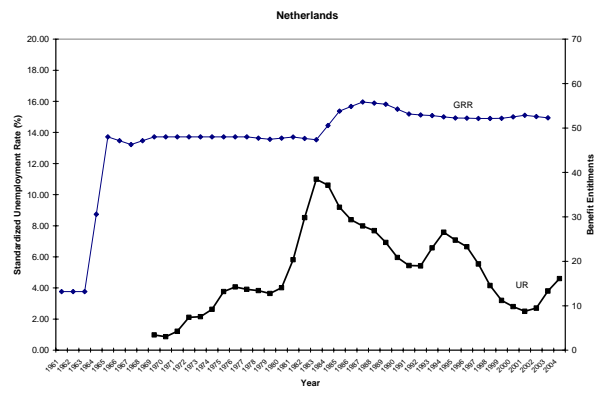
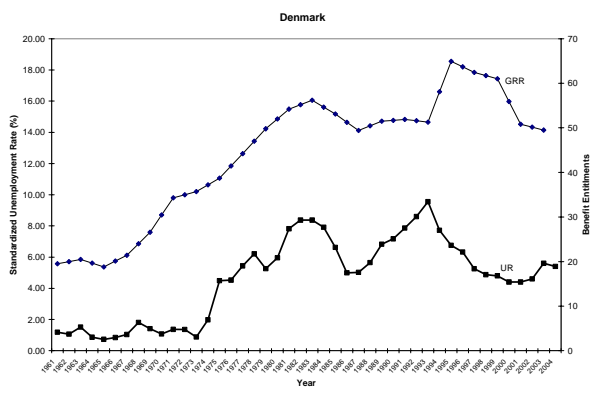
Figure 8: Nickell's Net "Good" and "Bad"
Labor Market Policy Changes, Early 1980s to Late 1990s



source: author's calculation from Nickell 2003, table 13.

Figure 9: Gross Replacement Rates and Standardized Unemployment Rates for 16 OECD Countries, 1961-2004

Panel A: Four “Success Stories”



Panel B: Four High Unemployment Countries

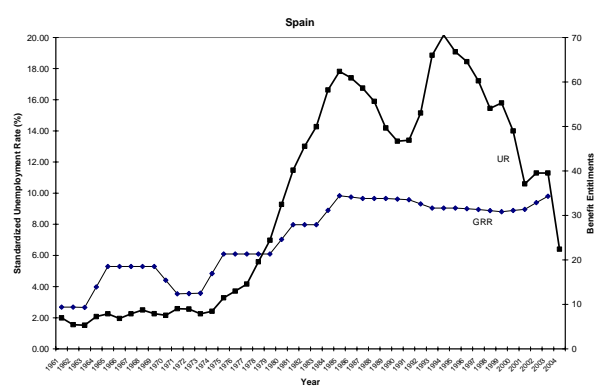
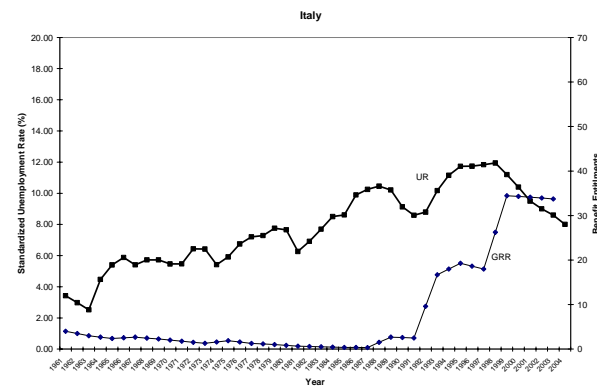
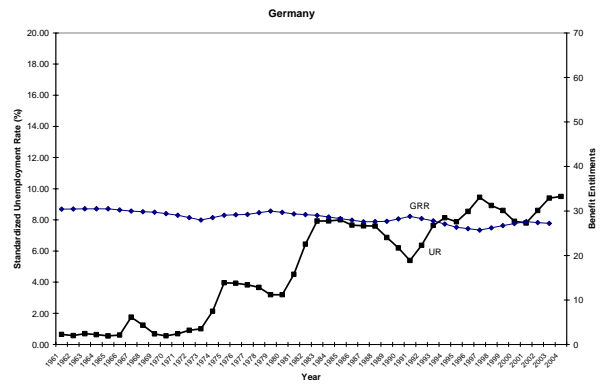
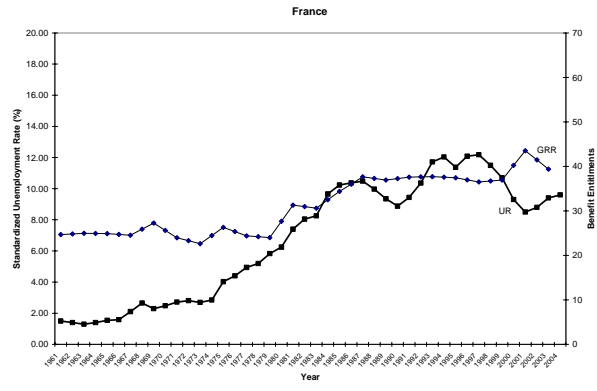
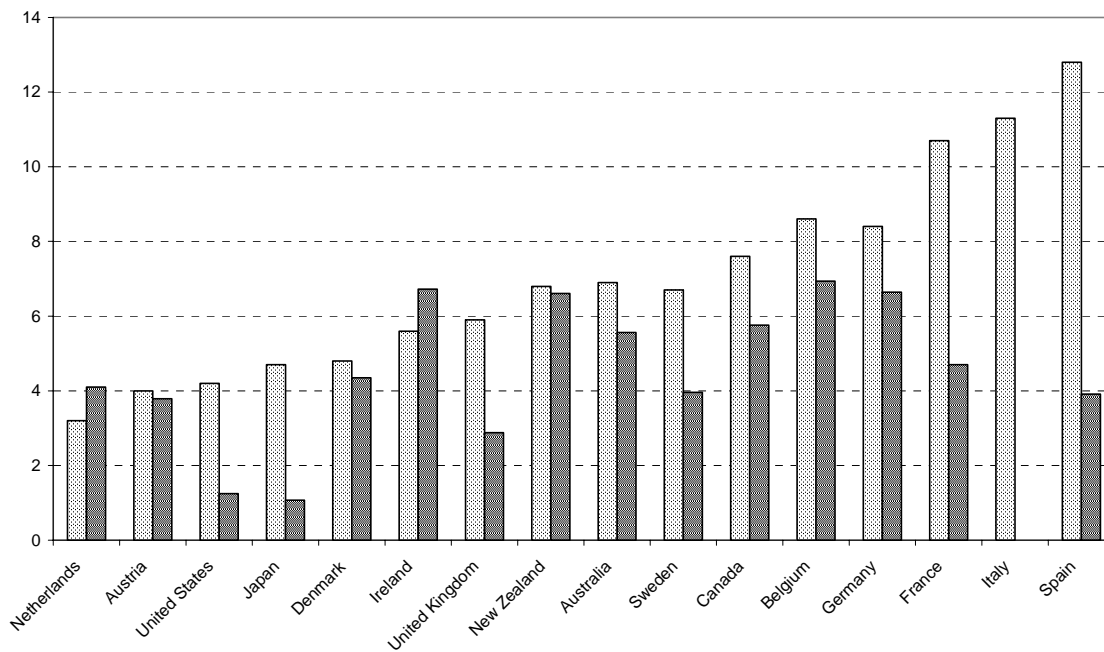


Figure 10: Unemployment Rates and Unemployment Benefit Reciprocity Rates for Selected OECD Countries, 1999



Source: OECD standardized unemployment rates, measured as a share of the labor force; OECD unemployment benefit reciprocity rates, measured as a share of the working age population (OECD Employment Outlook 2003, Chapter 4, Table 4.A1.1)

**Table 1: Standardized Unemployment Rates
by Gender and Age, 2003**

	MALE		FEMALE	
	15-24	25-54	15-24	25-54
<i>Liberal OECD Countries</i>				
US	12.9	4.6	11	4.6
Australia	12.2	3.9	11.1	4.3
Canada	14.9	6.1	11.8	5.9
Ireland	8.7	4.5	7.4	3.1
New Zealand	8.7	2.5	10.1	3.3
UK	11.8	3.8	9.9	3.4
Average	11.5	4.2	10.2	4.1
<i>High Unemployment European Countries</i>				
Belgium	15.8	6	19.5	7.4
Finland	22.2	7	19.4	7.6
France	20.8	7.4	22	9.8
Germany	13.3	9.8	9.7	9
Italy	20.7	5.2	27.2	9.2
Spain	18.7	6.9	26.4	13.8
Average	18.6	7.1	20.7	9.5
<i>Lower Unemployment European Countries</i>				
Austria	11.3	4.3	10.7	4.4
Denmark	8.5	4.4	7.1	5.1
Netherlands	7.9	3.7	8.1	4.4
Norway	12.6	4.3	10.7	3.3
Sweden	17.8	5.7	16.1	5.2
Switzerland	8	3.5	7.3	4.6
Average	11.0	4.3	10	4.5

Source: OECD, 2005: Statistical Appendix, Table C.

Table 2: Measures of Employment Performance, Social Protection and Collective Bargaining for 18 OECD Member Countries

	Six Liberal OECD Countries	Six High Unemployment European Countries	Six Low Unemployment European Countries
<i>Employment Outcomes:</i>			
1. Unemployment Rate 2004 (%)	5.2	9.1	5.0
2. Employment Rate 2004 (%)	70.8	62.6	73.7
3. Employment Rate <HS 2004 (%)	59.4	56.0	63.9
<i>Labor Market Institutions:</i>			
4. Employment Protection Legislation 2003 (Index)	1.2	2.6	2.2
5. Unemployment Benefits – Net Replacement Rate 2002 (%)	52	72	77
6. Trade Union Density 2000 (% of employees)	26	36	47
7. Collective Bargaining Coverage 2000 (% of employees)	36	83	76
8. Co-ordination of Bargaining 2000 (index)	1.7	3.8	3.9
9. Active Labor Market Policy Spending YEAR? (% of GDP)	0.5	1.0	1.2
<i>Other:</i>			
10. Tax Revenue Share 2004 (% GDP)	32	41	43
11. Wage Inequality 2000 (50/10 ratio)	1.9	1.6	1.5
12. < HS in population 2004 (%)	27	38	23

Data are mean values for country groups.

Liberal OECD Economies: Australia, Canada, Ireland, New Zealand, UK, USA

High unemployment Europe: Belgium, Finland, France, Germany, Italy, Spain

Low unemployment Europe: Austria, Denmark, Netherlands, Norway, Switzerland, Sweden.

Rows 1-3, 12: OECD Employment Outlook 2005 tables A and B

Row 4: OECD Benefits and Wages 2004 Table 3.1.b

Row 5: OECD Employment Outlook 2004 Table 2 A2.4 (calculated as average of benefits

when unemployed after-tax to after-tax earnings and benefits in work for the average of 4 family types and 2 wage levels)

Rows 6-8: OECD Employment Outlook 2004 Tables 3.3, 3.5

Row 9: OECD Earnings Dispersion Database

Row 10: OECD Revenue Statistics 1965-2004 Table 8

Row 11: OECD Employment Outlook 2005 Table 8

Row 12: OECD Employment Outlook 2005 table D

Table 3: Summary of Implied Impacts of Labor Market institutions on Unemployment: Selected Studies, 1997-2005

	EPL (1 unit increase)	UB RR + 10 PP	UB Dur + 1 yr	ALMP + 10 PP	Union Den +10 PP	Union Coverage +10 PP	Co-ord. + 1 unit	Taxes + 10 PP
1. Scarpetta 1996	0.37	1.3	--	insig	1.1	--	-3.07	insig
2. Elmeskov et al 1998 ^b	1.43	1.29	--	-1.47	insig	--	-1.48 ^g	0.94
3. Nickell 1997 ^a	insig	0.88	0.70	-1.92	0.96	3.60 ^f	-3.68	+2.08
4. BGHS 2005 (variation on Nickell 1997 ^j)	insig	insig	--	insig	insig	insig	insig	insig
5. Blanchard / Wolfers 2000	0.24	0.70	1.27	insig	0.84	insig	-1.13	0.91
6. Belot & Van Ours 2005 ^c	insig	-2.20	--	--	1.5	--	insig	insig
7. Nickell et al. 2003/2005	insig	.96	.17	--	.30	--	-3.92	.70
8. IMF 2003	0.52	0.51	--	--	2.37	--	-0.27 ^h	-0.51 ⁱ
9. BGHS 2004 (variation on IMF 2003 ^k)	insig	insig	insig	--	?	--	?	?
10. Baccaro & Rei (ILO) 2005	insig	insig	--	--	1.02	--	insig	insig
11. OECD 2006	insig	2.10	--	--	insig	--	-4.97	3.00

Sources: Nickell 1997: Table 6: column 1; Elmeskov et.al 1998: table 2 column 1; Belot and Van Ours 2002: Table 7, column 5; Nickell et al 2001, Table 13, column 1; Blanchard and Wolfers, 1999, Table 5, column 1; Bertola et al 2001, Table 9, column 1; Nicoletti/ Scarpetta 2002: Table 5.1 column 6; IMF (Debrun et. Al) 2003: Table 4.3, column 3; BGHS 2005: table 3.6 column 2; BGHS (2004) Table 4 column 4??; Baccaro and Rei (2005): table 4 column 3; OECD 2005: Table 1.2, column 2.

"Insig effect" means not statistically significant at 5% level; -- means variable not included in regression

For footnotes, see Appendix 1

Table 4: Implied Effects of Labor Market Institutions on the Unemployment Rate Using Coefficients from the IMF (2003) Study

	Regression Number			
	I	II	III	IV
Employment Protection index (+1 unit)	1.47	0.30	0.52	-0.44
Replacement Rate (+10 PP)	0.68	0.53	0.51	0.57
Union Density (+10 PP)	1.57	3.90	2.37	0.21
Bargaining Coordination Index (+1 unit)	-2.46	-0.48	-0.27	0.01
Tax Wedge (+10 PP)	2.66	0.67	-0.51	1.12

Source: IMF 2003 and authors' calculations. These calculations use the published regression results in IMF 2003, Table 4-3. The estimated impact for interacted variables assumes that the interacted variable has the mean value for the OECD nations for 1998. The calculation for the impact of a one unit increase in bargaining coordination assumes that the index rises from 0.5 units below the mean to 0.5 units above the mean.

Table 5: Variations on the IMF's "Why Reforms Payoff"

	IMF Var. 1	IMF Var. 3	IMF Var. 4	Alternative #1: General	Alternative #2: Differences
Institutions					
Employ. prot.	6.957** (1.215)	1.361* (0.571)	1.202* (0.579)	0.024 (0.131)	-1.241 (1.864)
Union density	0.474** (0.043)	0.180** (0.024)	0.172** (0.025)	-0.010 (0.011)	0.145** (0.054)
Barg.coordination	-15.233** (1.372)	-2.212* (0.975)	-0.928 (0.912)	-0.042 (0.546)	-3.943* (1.901)
Barg. Coord. ²	2.674** (0.357)	0.606** (0.185)	0.464* (0.184)	0.056 (0.129)	1.075** (0.402)
Benefit repl. rate	-0.019 (0.020)	0.003 (0.006)	0.012* (0.005)	0.003 (0.003)	-0.004 (0.009)
Benefit duration	--	--	--	-0.482 (0.353)	--
Tax wedge	0.452** (0.039)	0.076** (0.027)	0.068* (0.027)	0.033# (0.017)	0.020 (0.046)
Interactions					
EP-UD	-0.061** (0.012)	-0.031** (0.010)	-0.032** (0.010)	--	0.013 (0.031)
EP-BR	-0.023* (0.011)	--	--	--	--
EP-TW	-0.048* (0.021)	--	--	--	--
UD-BC	0.033* (0.013)	--	--	0.012* (0.005)	--
UD-BR	-0.001** (0.000)	--	--	--	--
UD-TW	-0.006** (0.001)	-0.002** (0.001)	-0.002** (0.001)	--	-0.001 (0.001)
BR-TW	0.003** (0.001)	--	--	--	--
CBI-BC	--	-1.850** (0.628)	-2.098** (0.638)	--	-1.346 (0.876)
BR-BD	--	--	--	0.007 (0.008)	--
TW-BC	--	--	--	-0.017** (0.006)	--
Central Bank indep.	--	3.386** (1.257)	3.432** (1.285)	--	3.017# (1.834)
Country effects	Yes	Yes	Yes	Yes	Yes
Country-specific CPI	No	Yes	Yes	No	No
Time dummies	No	No	No	Yes	Yes
Country-specific trend	No	Yes	Yes	No	No

Table 6: Youth Unemployment and Net Replacement Rates (NRR) for a 20 Year Old Single Person, 1999 (percent)

	Unemployment Ages 15-24	Initial NRR (only UI or UA)	Long-term NRR (includes SA)
<i>Liberal OECD Countries</i>			
US	9.9	0	10
Australia	13.5	39	0
Canada	14	0	35
Ireland	8.5	10	68
New Zealand	13.8	50	0
UK	12.3	60	60
<i>High Unemployment European Countries</i>			
Belgium	22.6	33	51
Finland			
France	26.5	0	0
Germany	8.2	26	52
Italy	31.1	0	0
Spain	28.3	0	32
<i>Low Unemployment European Countries</i>			
Austria	5.9	0	41
Denmark	10	69	85
Netherlands	7.4	20	33
Norway	9.6	0	53
Sweden	14.2	44	79
Switzerland			

Sources: *Unemployment*: OECD Employment Outlook 2002 (statistical annex table c)

Net replacement rates: OECD Benefits and Wages, 2002 (Table 3.8)

TABLETK
Granger-Causality Tests, Gross Replacement Rate and Unemployment Rate, 1962-2004
(F-statistics)

Lags	Gross Replacement Rate to Unemployment Rate				Unemployment Rate to Gross Replacement Rate			
	1	2	3	4	1	2	3	4
Australia	0.78	1.39	3.03 *	2.72 *	1.32	3.62 *	2.54 #	1.48
Austria	1.78	2.06	1.30	1.37	1.48	2.37	0.62	0.87
Belgium	4.40 *	2.00	1.92	1.70	0.12	0.88	0.59	0.57
Canada	4.02 #	3.22 #	2.03	1.82	0.04	1.51	1.06	0.66
Denmark	0.18	1.35	1.81	1.50	6.67 *	3.10 #	2.14	2.17 #
Finland	3.50 #	3.49 *	1.52	1.32	1.62	0.08	0.76	0.66
France	0.60	0.57	0.23	0.65	6.48 *	7.86 **	2.91 *	2.56 #
Germany	0.05	0.37	0.29	1.11	1.53	2.24	1.10	1.01
Ireland	0.05	0.95	2.91 *	1.80	10.06 **	2.39	2.05	1.88
Italy	1.48	0.52	0.39	0.64	5.22 *	1.69	2.31 #	1.72
Japan	0.76	0.16	0.57	0.48	5.46 *	3.49 *	2.56 #	4.14
New Zealand	0.06	0.91	0.56	0.36	0.01	0.10	0.09	0.14
Netherlands	5.98 *	0.79	1.04	0.63	18.67 **	5.82 **	3.57 *	2.26 #
Norway	5.12 *	6.09 **	2.76 #	4.37	0.59	0.38	1.32	0.76
Portugal	0.24	0.34	0.35	0.27	6.06 *	1.63	1.50	1.86
Spain	0.66	1.97	1.18	2.11	0.87	2.13	1.46	0.79
Sweden	1.74	1.54	0.95	0.74	1.82	0.04	0.28	0.76
Switzerland	1.74	4.76 *	3.52 *	4.08 **	1.48	0.32	0.45	0.49
UK	0.94	0.98	0.31	0.09	5.79 *	3.71 *	2.52 #	1.65
US	0.88	0.35	0.75	0.94	1.97	1.17	5.35 **	3.87 *

Notes: Authors' analysis of OECD data. In the first four columns, F-tests are distributed under the null hypothesis that the unemployment rate does not Granger-cause the gross replacement rate; in the last four columns, under the null hypothesis that the gross replacement rate does not Granger-cause the unemployment rate. Results marked ** are statistically significant at the 1% level; *, at the 5% level; and # at the 10% level. Full sample for Netherlands is 1970-2004.

¹ Atkinson and Mickelwright (1991) actually refer to the “duration of benefit” scores from Layard (1989), which if not the same, are quite similar: all four countries that get an “indefinite” score in the Layard data that are reproduced in Atkinson and Micklewright get the same score in Table 5 of Layard et al. (1994), which is the source for the benefits variable in the scatterplot.

² It may be noted, however, that a recent Bank of England study of the NAIRU in the UK concludes that “A wide range of equations with different combinations of structural variables was examined. Overall, the estimation work has shown that it is extremely difficult to link movements in the natural rate to structural economic variables. It is generally difficult to derive robust coefficient estimates for structural variables that have the expected sign and are statistically significant” (Cassino and Thornton 2002 p 34). It is also worth noting that this assessment sharply contrasts with Nickell’s results and interpretation, also for the UK (Nickell, 1998, p.).

³ For example, the -0.078 coefficient of the country specific trend estimated for Sweden would imply a drop of almost 3.0 percentage points in Sweden’s unemployment rate after ten years. The logic of this is that the time trend variable directly decreases the unemployment rate by 0.078 percentage points each year. The lagged dependent variable means that in addition to this direct effect, the current year’s unemployment rate U_t is equal to the 0.87 times the increment added directly or indirectly due to the trend in U_{t-1} . The implied effect of Sweden’s time trend after twenty years would be a drop in the unemployment rate of more than 7.0 percentage points. While the absolute value of the coefficient on Sweden’s time trend is considerably larger than the average, the coefficients for most of the country time-trends are large enough to imply an increase or decrease of at least 2 percentage points in the unemployment rate after two decades.

⁴ In addition to using country-specific time trends, the IMF also uses country-specific terms for the inflation-unemployment trade-off. In other words, unlike prior studies, the IMF does not impose the restriction that the trade-off between inflation and unemployment is identical for all countries. The IMF also includes somewhat novel specifications for the standard set of institutional variables. Specifically, the regressions include a quadratic term for bargaining coordination. This allows for the possibility that the effect of bargaining coordination on unemployment may not be linear. It also includes (like Nickell et al.) a lagged dependent variable. However, the IMF also separately includes interaction terms for the lagged unemployment rate multiplied by the benefit replacement rate and the lagged unemployment rate multiplied with the bargaining coordination level. In principle, these additional variables allow for the possibility that these institutions affect the persistence of unemployment through time. The other noteworthy departure of the specifications used by the IMF is the inclusion of a variable for central bank independence. This allows for the possibility that independent monetary policy may either lead to higher unemployment – possibly as a result of shielding central

bankers from political pressures to try to reduce unemployment – or alternatively, to lower unemployment as a result of consistent well-planned monetary policy.

⁵ The IMF states that all four specifications are reasonable representations of reality. For example, the IMF writes that "the very simple model [in variant one] does a good job in explaining unemployment variation across countries (although not across time)" (p. 148). The IMF's preferred specification is arguably variant three, but this equation includes complicated interactions between institutions and the lagged dependent variable that make it difficult to use in various simulation exercises. Variant four is based on variant three, but without these interaction terms.

⁶ We 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.

⁷ One 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.

⁸ The coefficients on the institutional variables across the three IMF regressions are not directly comparable for two main reasons. First, the second and third columns include a lagged dependent variable, which is not the case in the first column. To put the institutional coefficients in the second and third column on a roughly comparable basis, divide each coefficient in columns two and three by one minus the lagged dependent variable. Second, the second column includes separate interaction terms between the lagged dependent variable and bargaining coordination, and the lagged dependent variable and the benefit replacement rate. Third, other interaction terms also differ across specifications.

⁹ We include a common set of time dummies in order to remove global shocks (good and bad) and common business-cycle effects over the 1960-98 period. We see little theoretical justification for imposing a common time trend, and even less justification for including a separate time trend for each country. To the extent that unemployment in OECD economies is trended over time, the role of this kind of modeling ought to be to explain such a trend, not to control for it. In the same spirit, this kind of modeling should seek to use institutions (and other economic variables) to explain differences in trends across countries. A common set of time dummies allows us to control for common global shocks, leaving national institutions to explain deviations from unemployment from the average pattern implied by these shocks. A common time trend or country-specific time trends leaves institutions (and other variables) only the task of explaining deviations from the typically rising trend in unemployment.

We also use a common term for the change in the consumer price index (CPI), rather than country-specific CPI terms. Much of the interest in this kind of research is

precisely on the way in which national institutions change the nature of the tradeoff between inflation and unemployment in particular countries. The argument that the data reject the common CPI (or time trend term) is not particularly persuasive since the data almost certainly reject common coefficients for the institutional variables (except in the case where the institutional results themselves are poorly defined).

¹⁰ The first-difference specification may also act as a crude guard against problems arising from the possibility that the series regressed here are not stationary and not cointegrated. If the variables follow a random walk with drift, differencing would yield stationary series. The IMF does not present any tests of stationarity or cointegration, but standard theory suggests that the coefficient estimates presented in the WEO and here would be invalid if the series are not stationary or cointegrated (see, for example, Davidson and MacKinnon (1993), Chapter 10). Junakar and Madsden (2004) and Baccaro and Rei (2004) discuss this issue in the context of OECD unemployment.

¹¹ As we noted above, whatever the empirical strength of this relationship, Atkinson and Mickelwright (1991) definitively put to rest the notion that there has ever been any European country with unconditional and indefinite unemployment benefit entitlements.

¹² According to van Ours (2003), “The decline in replacement rate had a clear effect on unemployment.” But before we can be sure of how clear this effect is, we would need an explanation for the stability of the OECD measure shown in Figure 9.

¹³ The Survey by the Danish Ministry of Finance (1999) does state that “The level of structural unemployment has evidently fallen over the last years, due to the extensive reforms of the labour market, *cf. chapter 2*” (p. 9). But turning to chapter 2 we find Box 2.1, titled “Labour market initiatives since 1993.” A close look at these initiatives reveals that what is being tightened are the links between benefit receipt and participation in active labor market programs, and the improvement and expansion of these programs. While it is not unreasonable to assume that these initiatives played an important role, Chapter 2 offers no direct statistical evidence on the relationship between the decline in the Danish unemployment rate and the tightening of the linkages between the benefit entitlement system and participation in active labor market programs.

¹⁴ It could easily be argued that Nickell’s allocation of ticks and crosses for France is among the most problematic. For example, in Nickell’s table 5, France gets a cross on the basis of changes that took place in the 1980s, not the 1990s, and gets no credit (a tick) for reducing union density from 16% to 10%, a level below that of the U.S.. It gets another for increasing strictness of employment protection, which turns out to have been entirely due to changes in regulations that apply to temporary workers, who comprise just 15% of the workforce. Finally, Nickell’s criterion for a cross on EPL is a rise of *more than .1*; France’s score increases from 1.3 to 1.4 (exactly .1). France gets a cross.