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Are Protective Labor Market Institutions at the Root of Unemployment? A Critical Review of the Evidence

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Are Protective Labor Market Institutions at the Root of Unemployment? A Critical Review of the Evidence*

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Abstract

A rapidly expanding empirical literature has addressed the widely accepted claim that employment-unfriendly labor market institutions explain the pattern of unemployment across countries. The main culprits are held to be *protective* institutions, namely unemployment benefit entitlements, employment protection laws, and trade unions. Our assessment of the evidence offers little support for this orthodox view. The most compelling finding of the cross-country regression literature is the generally significant and robust effect of the standard measure of unemployment benefit generosity, but there are reasons to doubt both the economic importance of this relationship and the direction of causation. The micro evidence on the effects of major changes in benefit generosity on the exit rate out of unemployment has been frequently cited as supportive evidence, but these individual level effects vary widely across studies and, in any case, have no direct implication for changes in the *aggregate* unemployment rate (due to “composition” and “entitlement” effects). Finally, we find little evidence to suggest that 1990s reforms of core protective labor market institutions can explain much of either the success of the “success stories” or the continued high unemployment of the large continental European countries. We conclude that the evidence is consistent with a more complex reality in which a variety of labor market models can be consistent with good employment performance.

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As recently as 1979, among the 20 most developed (OECD-member) countries only Ireland and Portugal reported unemployment rates above 8 percent (each at about 8.5 percent). Just four years later, 11 of these 20 countries posted higher rates and six reached double-digit levels, ranging from Belgium (10.7 percent) to Ireland (14.9 percent). This collapse in employment performance persisted throughout the 1980s and 1990s. Between 1995 and 1997, as the U.S. was showing rates between 5.6 to 4.9 percent, OECD-Europe ranged from 10.1 to 9.7 percent. By 2005, the OECD-Europe rate had dropped to 8.6 percent, but the two largest economies of continental Europe, France and Germany, had rates of 9.5 percent.¹

Much like the response of economists to the Great Depression, the dominant explanation for persistent high unemployment has focused on supply-side rigidities generated by protective labor market institutions, and similarly, the proposed solution has been greater (downward) wage flexibility and stronger work incentives. As Fitoussi (2006) has recently put it, “The reference model, in the plea for structural reforms, is centered on an economy with perfect competition and rational expectations. In such a model full employment is always assured absent rigidities...”. Spurred in particular by the influence of the Layard, Nickell and Jackman (1991) and the OECD’s Jobs Study (1994), by the late 1990s this orthodox rigidity account thoroughly ruled the field. The title of a prominent paper in the *Journal of Economic Perspectives* aptly summed up the conventional wisdom: “Labor Market Rigidities: At the Root of Unemployment in Europe” (Siebert, 1997).

The policy implications of the rigidities view are straightforward and profound. 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.’” Their own empirical tests led IMF researchers to conclude that European adoption of “well-designed reforms (the U.S. model) could produce output gains of about 5 percent and a fall in the unemployment rate of about 3 percentage points (IMF, p. 129).” At the same time, the orthodox labor market rigidity view has become so widely accepted that a leading scholar could recently claim in a recent issue of 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 citing *any* peer-reviewed research (St. Paul, 2004, p. 53).

The dominance of the orthodox labor market rigidities explanation of unemployment and the recent focus on macroeconomic testing reflects a striking evolution in mainstream economics. As recently as 1994, Charles Bean’s influential survey of European unemployment allocated little space to evidence on

the effects of labor market institutions on employment performance, finding little compelling empirical support in the literature for any of them.² Bean concluded with three recommendations for future research, the first of which was to discourage further macroeconomic testing: “There is simply not enough information in the data to give clear signals on the relative merits of the competing hypotheses (p. 615).” Far from heeding this advice, the cross-country macroeconomic literature has grown dramatically (for recent surveys, see OECD, 2006; Blanchard, 2006).

Certainly an important part of the explanation for this explosion in cross-country studies of unemployment was the growing gap in employment performance between the U.S. and large European countries after the early 1990s, which lent at least anecdotal support to the orthodox rigidities explanation. It is worth noting that the dominance of the labor market rigidities account for developed country employment performance closely paralleled that of the “Washington Consensus” in trade and development studies. Both reflected what has become known as “free market fundamentalism.” The growing interest in verifying the rigidities account through macroeconomic testing required much “more information in the data” as Bean had put it, and due largely to the efforts of the OECD, one of the distinctive features of this literature over the last decade has been the steady improvement in country-level institutional measures.

This paper critically assesses the empirical evidence produced by this recent literature. Three labor market institutions have been held to play leading roles in the promotion of employment-unfriendly rigidities: unemployment benefit entitlements, employment protection laws, and trade unions.³ These are the key institutional mechanisms that most developed countries have relied upon to shelter less-skilled workers from the most harmful effects of competitive labor markets. We will refer to them as “protective labor market institutions” and distinguish them from other key institutions that also have important labor market effects: active labor market policies (ALMP), which are concerned with matching and preparing workers for jobs; tax policy, which influences behavior and affects labor costs, but is principally designed to raise revenue, not protect workers; and housing policies, which affect ownership rates and could affect worker mobility, but are not designed to protect workers in the labor market.

After outlining the basic facts on the cross-country pattern of unemployment and labor market institutions, section 2 considers some critical (and generally underappreciated) issues of measurement. Section 3 then evaluates the simple correlation evidence between standard measures of labor market institutions and unemployment. Section 4 addresses the macroeconomic evidence. Since the most robust evidence in favor of the orthodox rigidity view concerns the role played by unemployment benefit generosity, in Section 5 we take a closer look at the interpretation of the effects of benefit generosity in recent macroeconomic research. This section also reviews the microeconomic evidence on benefit

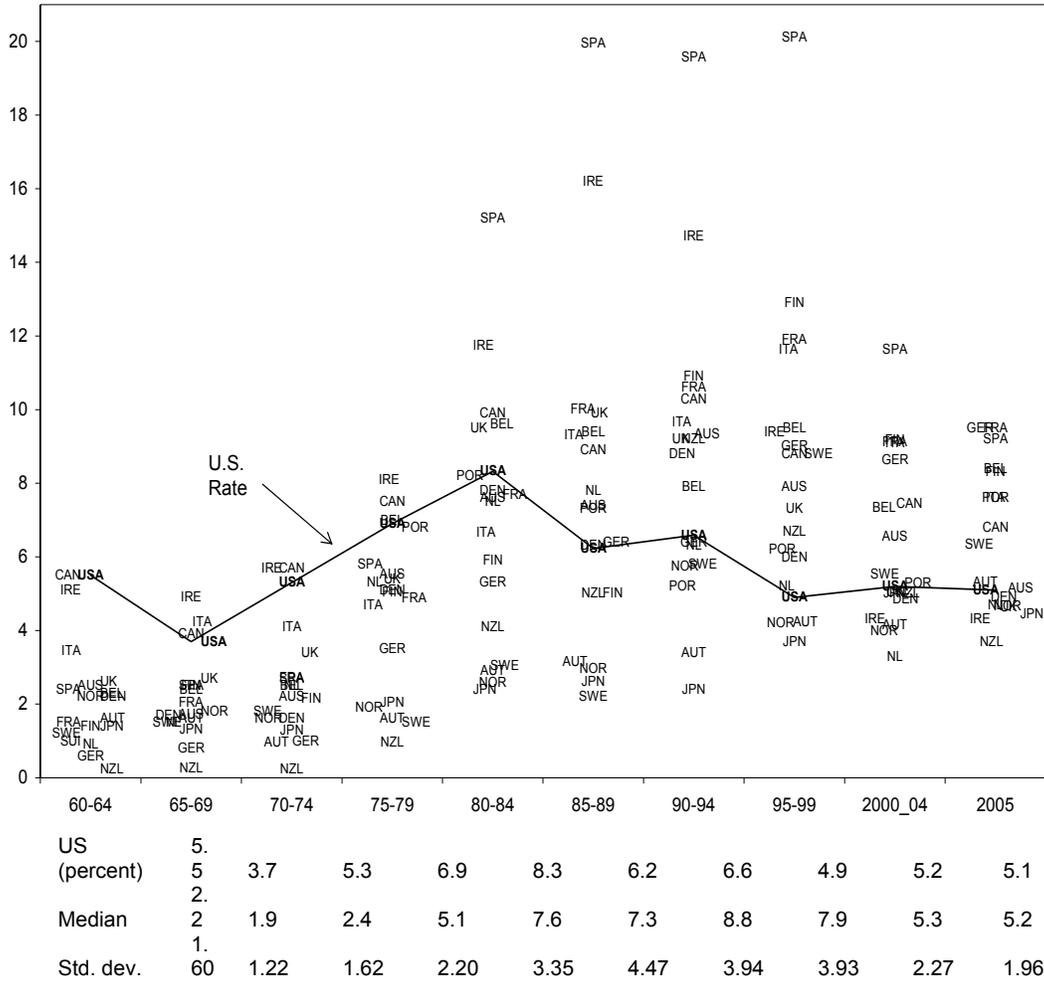
generosity and worker behavior, which has often been cited as supporting evidence. Section 6 then turns to recent efforts to develop aggregate indicators of labor market reform designed to show the payoff of comprehensive labor market reform for employment performance. We conclude in Section 7 with a summary and brief discussion of the interplay between theory, evidence, and policy recommendations.

1. UNEMPLOYMENT AND INSTITUTIONS: 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 the most recent figures for 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 percent in the late 1960s to 8.8 percent in 1990-94 (unemployment rates prior to the 1980s should be viewed with considerable caution – see section 2.1). 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 percent in 1995–99 to 5.3 percent in 2000–04 and 5.2 percent for 2005; the standard deviation fell from 3.9 to just below 2, which is about where it was on average in the 1970s. The figure shows that the distribution of unemployment rates in 2005 falls in a range of 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 countries with the very highest rates through the first two decades (1960–79) to among those with the lowest rates in the second half of the 1990s, and back again to close to the median since 2000 (see 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 rate from the second highest in 1985–94; and Spain as experienced a remarkable decline, to a level that is now just below that of Germany and France.

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



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.

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, in contrast to the much better performing and relatively unregulated labor markets of the U.S. and other Anglo-Saxon economies. This conventional view greatly misrepresents the facts, at least based on standard OECD measures of labor market institutions and policies.

Table 1 reports unemployment rates for 2003 by demographic group for countries in three groupings: six English-speaking countries with generally low unemployment (Canada remains at higher levels); six high unemployment continental European countries; and six European low unemployment countries. This table shows that the six liberal, English speaking countries had average unemployment rates nearly identical to those of the six low-unemployment European countries for all four

**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.

demographic groups – male and female young and prime age workers. The five high-unemployment Continental countries show substantially higher unemployment for each age-gender group. With the exception of Germany, each has experienced extremely high youth unemployment. Female youth show rates of 17.5 percent in Belgium, 22.8 percent in France, 27.2 percent in Spain, and 30.9 percent in Italy; male youth rates range from 18–23 percent. Clearly, young people in these four countries account for an important part of the European unemployment problem. It should be recognized, however, that using an alternative measure of unemployment— as a share of the youth population rather than as a share of the youth labor force—the picture looks quite a bit different. With this alternative measure, for example, France and the U.S. have similar youth unemployment rates (Howell, 2005, chapter 1).

Table 2: Employment and Institutional Patterns across country groups

	Six Liberal Economies	Six High Unemployment Euro Countries	Six Low Unemployment Euro Countries
1. Unemployment Rate 2004 (%)	5.2	9.1	5.0
2. Employment Rate Rate 2004 (%)	70.8	62.6	73.7
3. Employment Rate – less than High School (%) (% <HS in pop)	59.4 (27)	56.0 (38)	63.9 (23)
4. Employment Protection Legislation 2003 (Index)	1.2	2.6	2.2
5. Unemployment Benefits – Net Replacement Ratio 2002 (%)	52	72	77
6. Trade Union Membership 2000 (% 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. Wage Differentials: ratio of median wages to 10 th decile c2000	1.9	1.6	1.5
10. Tax Revenue as % GDP 2004	32	41	43
11. Active Labour Market Policies 2004 (% of GDP)	0.5	1.0	1.2

Data are mean values for country groups. Liberal 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.
Sources: Rows 1-3: *OECD Employment Outlook 2005* (Tables A, B, D).
Rows 4: *OECD Employment Outlook 2004* (Table 2 A2).

Row 5: *OECD Benefits and Wages 2004* (Table 3.1.b4), calculated as the ratio of after-tax benefits to after-tax earnings and benefits in work (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.

The similarity between unemployment rates for the liberal English-speaking countries and low-unemployment Europe is notable because the latter remain characterized by strong welfare states and highly protective labor market institutions. Table 2 shows that 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 of “high-unemployment Europe” 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. MEASUREMENT ISSUES

Before examining the statistical evidence on the relationships between unemployment and labor market institutions, it is worth taking a brief look at the construction and quality of the measures. Despite a vast increase in the sophistication of econometric techniques used in this literature since the early 1990s, much less attention has been paid to the data. The confidence with which many authors draw strong policy conclusions from their published regression coefficients implies a high level of confidence in the quality of the underlying data. Although there have been impressive improvements in data quality over this period, mainly by the OECD, the unfortunate reality is that the variables on both sides of the equation remain remarkably poorly measured. In addition, since there is no consistency across studies in the measures used, it is difficult to know whether the effects in a particular study differ from those published in another because of the data, the specification, or the estimation methods.

2.1 UNEMPLOYMENT

For the most part, empirical work on unemployment has proceeded under the assumption that the dependent variable—the unemployment rate—is well-

measured and comparable (“harmonized” or “standardized”) over time and across countries. Indeed, rarely do authors offer more than simply a citation for the source of this measure. But a closer look at the data shows that comparability is often limited and that the many different series in use can produce quite different results.

The source data are collected at the national level, both as “registered” unemployment (collected by the National Employment Service) and from household surveys (similar to the U.S. Current Population Survey). Over time, OECD countries have adopted international standards that establish the criteria for who is “unemployed” based on household surveys, but some series refer to those between 15–64 (for the U.S. it is age 16) and others to those over age 15. While comparability has increased as data collection and processing methods and criteria have converged across countries, both the OECD and the U.S. Bureau of Labor Statistics have developed additional series designed to be broadly comparable across countries (Sorrentino, 2000).

But full comparability remains elusive. Since the unemployed cannot be employed, how a respondent replies to a question asking whether or not he/she was employed for pay in the reference week will reflect to some degree local social norms and levels of economic development (Howell, 2005). What is considered “real” employment may differ across regions and countries, and this may help explain, for example, how Spain could have an unemployment rate above 20 percent in the 1990s, more than four times that of Mexico, whose rate, in turn, was about the same as that of the U.S. (Howell, 2005; Martin, 2000).

In the early unemployment-institutions tests by Layard, Nickell, and Jackman (1991) a simple cross section of unemployment rates averaged over the 1984–89 period was used. As the literature has progressed, analysts have demanded annual series covering the longest possible period. The latest generation of studies has aimed at explaining *changes* in unemployment over time with *changes* in institutional and policy measures, with fixed country effects (see section 4). This empirical strategy relies heavily on the use of historically consistent unemployment rate series for each country.⁴ For many countries, this criterion is clearly not satisfied. National methods have changed substantially over this period, and there is no standardized or historically consistent series that comes close to dating back that far for the 19 OECD countries that usually appear in the cross-country tests. For example, the OECD’s standardized data extend back to 1980 for just nine OECD countries. The OECD’s longest historical unemployment series (not standardized) is available for just nine countries for 1970 and four countries for 1965.⁵ With data as limited as this, one has to wonder about the meaningfulness of regression results presented for the typical 19 or 20 countries that extend back to the early 1960s.

Our effort to understand how the Dutch and Swiss unemployment rates have changed over the last two decades using OECD data offers an illustration of the

problem with the historical statistics. The Bassanini-Duval (2006) macroeconomic tests provide the empirical basis for Chapter 7 of the new *OECD Employment Outlook* (OECD, 2006a). This research focuses on the 1982–2003 period, and in both the Bassanini-Duval report and the *Employment Outlook* Chapter, a cross-country scatter plot of predicted against “observed” changes in unemployment is presented for 1982–2003 (OECD 2006, Figure 7.3). The conclusion drawn from this correlation is that a small number of labor market policies and institutions can largely account for cross-country differences in how unemployment has evolved since the early 1980s.

The Bassanini-Duval figure shows a decline in unemployment over the 1982–2003 period for Holland of over nine percentage points, based on a 1982 rate of 13.2 percent. But according to the OECD’s Labour Force Statistics publication (OECD, 2005a), the change is just 7.4 percentage points (11.6 percent in 1982 to 4.2 percent in 2003). Another OECD series—the standardized rates that appear in the OECD’s *Employment Outlook*—shows a still smaller change of four points (7.68 to 3.68). This range of 4–9 points in the various OECD unemployment rate series compares to a 4.8 point change (8.5 to 3.7) that appears in the series used by the IMF (2003) and Nickell et al. (2001).⁶

At the other end of the country points in the figure are the poor performers. The worst among these is Switzerland, which shows an increase in observed unemployment for 1982–2003 of about four percentage points. This extremely poor performance contributes substantially to the figure’s overall correlation. The problem is that it is computed on the basis of an implausibly low 1982 unemployment rate of 0.2 percent. Prior to 1991, the Swiss data referred only to registered unemployment, and for this reason the OECD publishes no standardized figures for Switzerland prior to that date. In short, the four point increase in Swiss unemployment reflects a comparison that has little meaning, either for changes in employment performance within the country, or for cross-country comparisons. Switzerland is by no means unique in relying on administrative data for historical time series.⁷

These Dutch and Swiss examples illustrate the weaknesses in the historical time series on unemployment for many countries. The comparability of the unemployment rate numbers declines substantially the further back the time series runs—it is not until the early 1990s that nearly all major OECD member countries generally adopted the ILO standard (the broad definition of unemployment based on household surveys). But even for recent years, differences remain both across countries and over time within countries in exactly how the ILO unemployment rate is calculated (such as what qualifies as “active” job search), which may be important. At a minimum, studies should pay much closer attention to the historical and cross-country consistency of the measure of unemployment that is being explained.

2.2 LABOR MARKET INSTITUTIONS

Unlike most macroeconomic research, statistical tests of the effects of labor market institutions on the pattern of unemployment required the creation of the institutional measures. This effort was pioneered by Stephen Nickell, Richard Layard and various colleagues, who used newly created measures for cross-sectional regression tests published in a series of papers and books in the early- and mid-1990s (Layard, Nickell, and Jackman, 1991 and 1994; Nickell, and Layard, 1997; Nickell, 1997).

For example, the measure of unemployment benefits duration that was employed extensively in the 1990s was an estimate of the number of years a representative unemployed worker was eligible for benefits. Thus, Layard et al. (1994, p. 74) gave the U.S. a score of .5, Denmark 2.5, and France 3.75. Like eight other countries, The Netherlands received a four, indicating “indefinite” duration of benefits. In their survey of the benefit entitlement literature, Atkinson and Micklewright (1991) single out these data for criticism, noting that the institutional design of each of the countries with “indefinite” duration scores is quite different, and these differences have substantial effects on how generous the systems really are for which parts of the unemployed population.⁸ Atkinson and Micklewright (Table 3, p. 1696) explain the reality behind the “indefinite” score for the Netherlands in the mid-1980s: “UI at 70 percent of last earned wage for between 6 months and 5 years depending on contribution record, plus one year of benefit at 70 percent of minimum wage... On expiry of UI, (there is a) possibility of means-tested assistance.”

The creation of measures of institutions and policies like benefit duration, employment protection, and bargaining coordination requires considerable subjective judgment, and this has raised additional concerns. If the empirical tests are designed to confirm strongly held theoretical priors (institution-caused rigidities explain unemployment), and the same researchers generate the measures of the key explanatory variables (the institutions), it might be expected that the measures that do the best job of confirming the guiding hypotheses will be preferred. Blanchard and Wolfers warn of this “Darwinian” effect:

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).

The 1994 *Jobs Study* triggered a major OECD effort to produce better quality institutional measures. The objective was, like the first generation efforts, to facilitate tests of the orthodox cornerstone of the *Jobs Study*, that strong labor market institutions could explain employment performance across countries. But the creation of these improved measures took time, and meanwhile researchers demanded long time series.

In the case of a measure of the strictness of employment protection laws, Blanchard and Wolfers took the recently developed OECD country estimates (OECD, 1999a) and merged them with an entirely different series produced by Lazear (1990) to create an EPL score for each 5-year period from the early 1960s to the mid-1990s. The extraordinarily detailed and carefully constructed OECD EPL measures were available for just two data points: “the late 1980s” and “the late 1990s”. From these two estimates for each country, Blanchard and Wolfers created another for 1990–94 by interpolating between the OECD’s late 1980s and late 1990s scores, and still another for the early 1980s simply by using the late 1980s figures, on the grounds that they did not have information to suggest that there were any changes between the early and late 1980s. Thus, from two multi-year averages for the late 1980s and late 1990s (OECD), four 5-year averages were created.

Blanchard and Wolfers (2000) then used severance pay data from a paper by Edward Lazear (1990) to do the same for the first two decades (1960s and 1970s). But the Lazear and OECD measures are quite different, a problem not revealed to readers by either Blanchard-Wolfers or the many subsequent studies that relied on this EPL series. Thus, for the late 1980s and late 1990s, the OECD (1999a) took into account three dimensions of employment protection: procedural inconveniences which the employer faces when trying to dismiss; notice and severance pay provisions; and prevailing standards of and penalties for unfair dismissal. Further, their estimates were designed to cover both white and blue collar workers. In contrast, Lazear's index is narrowly confined to just one of the three OECD dimensions, severance pay and notice, and was further limited to “the number of months of severance pay or notice *a blue collar worker with ten years of service received upon termination without ‘cause’*” (emphasis added, p. 707-8).

By merging the OECD and Lazear series, Blanchard and Wolfers produced an EPL measure for 5-year periods from the early 1960s to the late 1990s. To make matters worse, Nickell et al. (2001, 2003, 2005) then annualized these data by simple interpolation. If only for comparative purposes, this mongrel EPL measure has been used (either in its annual or 5-year format) by a number of recent studies, including the IMF (2003), Baker et al., and Baccaro and Rei (2005). In contrast, the Bassanini-Duval (2006) employs what must be a far superior annual EPL series that has recently been generated by the OECD for the post-1982 period.

Again, due to the efforts of the OECD, the quality and comparability of unemployment benefits data for OECD countries improved dramatically after the mid-1990s. The OECD produces an average gross replacement rate (across family types, income levels, and different durations of unemployment) for every second year since 1961, and this has become the measure of choice for empirical work in this area. This measure allows researchers to capture both replacement rate and duration in a single measure of benefit generosity.

More recently, the OECD constructed net replacement rates, which take into account unemployment compensation after taxes and various related benefits. These are far more appropriate than the gross replacement rates for measuring the incentives facing workers. Net replacement rates have been constructed for selected dates between the early 1990s and 2003, but the OECD is only now (2006) coming out with a historical time series. These new net benefit figures are particularly attractive because they will measure generosity relative to average wages, not the average production worker wage (which, with the shift to services, is increasingly misleading). The question is whether the new, much improved measures of benefit generosity will perform as well (i.e., supporting the orthodox rigidity thesis) as the average gross replacement rate in regression tests (section 4).

There is some reason for doubt. Currently, these new net replacement rates can be compared over time for just eight of the countries typically included in cross-country tests, and only for 1995–2004. If changes in the standard gross replacement rate is a good measure of the change in benefit generosity likely to have major effects on labor supply decisions and wage pressure (and therefore on employment and unemployment rates), they should be correlated with the superior new net replacement rates. It turns out that there is little correspondence between the two. Three countries show changes roughly similar in magnitudes, France (NRR: +4 pts; GRR: +2), Austria (NRR: -2; GRR: -1), and the UK (NRR: -1; GRR: -2). But three other countries show changes in opposite directions: the U.S. (NRR: -6; GRR: +2); Japan (NRR: +2; GRR: -2); and Germany (NRR: +1; GRR: -3). The two remaining countries show huge differences in the size of the change: Italy (NRR: +2; GRR: +15) and Finland (NRR: -9; GRR: 0).⁹ These are not results that give us much confidence that when the improved net rates become available for the full set of 19 OECD countries, the change in the superior benefit generosity measure (net) will show a close correspondence to changes in the measure (gross) that many studies have found to have a statistically significant association with changes in unemployment.

But even a net replacement rate measure that reflects both levels and the duration of benefit relative to the average wage, other critical features of unemployment benefit systems are unmeasured. There is no cross-country measure of eligibility, but we do know that the share of the unemployed who are receiving unemployment-related benefits varies dramatically across countries. For

example, 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 (e.g., Spain and Italy) or are eligible only for much lower levels of benefit. Closely related, enforcement of eligibility rules is a critical dimension of benefit generosity, but there is no reliable historical series that can be used for cross-country comparisons.

Finally, the bargaining power workers gain from trade union led collective action is poorly measured. The most commonly used measure is union density—the share of employees who are union members. But union density is not closely correlated with collective bargaining coverage—the share of employees whose wages and employment conditions are set through collective bargaining. The most extreme example is France, which had a union density rate of just 10 percent in 2000, below that of even the U.S. (13 percent). But collective bargaining coverage stood at over 90 percent. Countries with less than 40 percent union density and more than 80 percent collective coverage included Austria, Australia, Portugal Spain and The Netherlands. The coverage measure is much harder to generate, which helps explain why it is available for fewer countries and much fewer years than the union density measure (with a fairly wide margin of error). In any case, it is not clear that either union density or coverage is a particularly good measure of power of unions to affect market outcomes.

Given these considerable inadequacies in measurement on both sides of the relationship—for the unemployment rate and these key labor market institutions—it might be viewed as surprising if any statistical fit was uncovered. Interestingly, as Section 4 (Table 3) will show, rather inexplicably, as the quality of the labor market institution measures has improved (and attention to robustness has increased), the strength of the reported statistical association between these measures and the pattern of unemployment has fallen. We begin our review of this evidence in the next section.

3. CORRELATION EVIDENCE

3.1 CASUAL ASSOCIATIONS

It seems likely that when the conventional wisdom overwhelmingly frames the discourse, less attention will be paid to alternative accounts and standards of evidence will suffer. This may explain the frequent resort to casual association in making the case for the orthodox rigidity explanation.

The OECD's *Jobs Study* offers an example. Although no evidence of a statistically significant relationship between unemployment benefit generosity and the unemployment rate is presented for any particular point in time, Chapter 5 of the *Jobs Study* confidently declares that “increases in a more comprehensive

measure of unemployment compensation has typically been followed by an increase in unemployment but usually with a considerable lag” (p. 44). For empirical support for this long-term lagged effects claim the reader is referred to Chapter 8 where two kinds of evidence are presented. The first is described in the following passage (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 neighbors) 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.

To say the least, such breathtaking leaps in association require extremely strong theoretical priors. 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.”

To take another example, Heckman (2003, p. 373) suggests that an important part of the German employment performance problem can be traced to what he terms “substantial” unemployment net benefit replacement rates (79 percent), because “Germans, like all people, respond to these incentives (not to work).” But direct evidence of a causal relationship running from benefits to unemployment for Germany is not offered. It is worth noting that the figure in which this 79 percent German rate appears also that Denmark (80 percent), the Netherlands (82 percent), Switzerland (84 percent) and Sweden (85 percent) all show even higher replacement rate generosity (for 1995). While this pattern of generosity has remained stable, unemployment rates for these four high generosity countries have consistently been lower than Germany’s since 1995. Three of the four (Sweden is the exception, but just barely) have shown lower unemployment rates than the U.S. since the late 1990s, despite a much lower U.S. net replacement rate.

Similar reliance on casual association can be found in discussions of the labor market effects of employment protection laws. The OECD’s *Economic Survey of*

France (OECD 2005c) notes that employment protection is relatively strict in France and, for this reason, calls for a series of reforms. But no evidence or references are offered to establish that EPL strictness actually helps explain French unemployment, or that the recommended reforms would reduce it. Similarly, the OECD's *Economic Survey of the Netherlands* (OECD 2005d, 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." As in the French country survey, this policy recommendation for the Netherlands (a very low unemployment country) is made without reference to any evidence on either 1) EPL strictness and employment responsiveness to economic conditions, or 2) benefit replacement rates and the responsiveness of real wages to unemployment.

3.2 SIMPLE CORRELATIONS

In terms of statistical rigor, between these examples of *assumed* relationships and the macroeconomic exercises that we review below lie simple bivariate correlation evidence, 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. Indeed, scatter plots have often been employed to support the orthodox case that employment performance is directly harmed by the rigidities produced by various protective labor market institutions. Examples range from Layard et al. (1991) and the OECD (1994) to Heckman (2003) and Blanchard (2004).

To take one example, it has frequently 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 authors present a plot of a measure of the maximum duration of benefit in years against the long-term share of unemployment for the mid-1980s (1991, Figure 13; 1994, Figure 13) and remark 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) (1994, p. 59)." Based on this evidence, they conclude that "In countries in which benefits are indefinitely available, employment is much less likely to rebound after a major downwards shock (1991, p. 40; 1994, p. 62)." This seems quite a thin reed on which to support such a pronouncement – a simple correlation for a handful of countries with an "indefinite duration" measure of benefits that is highly suspect (Atkinson and

Micklewright 1991).

The OECD's *Jobs Study* made frequent use of scatter plot and simple correlation evidence. After the passage linking increasing unemployment to increases in benefit generosity quoted above, Chapter 8 proposes to "examine correlations more systematically (p. 178)." This is their second piece of evidence for the lagged effects of benefits. Here they show 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 scatter plots for the change in unemployment against the six-year average ("summary") benefits *level* as well as against the *change* in the benefits measure over the previous cycle. This produces six correlation tests. 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 seven of the 21 countries before the 1980s) resulting in a total of 12 tests. 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.¹⁰ In sum, this is the correlation evidence (two of 12 tests showing some association) that is claimed to support the OECD's Chapter 5 contention that increases in unemployment tends to follow with a long lag increases in unemployment compensation.

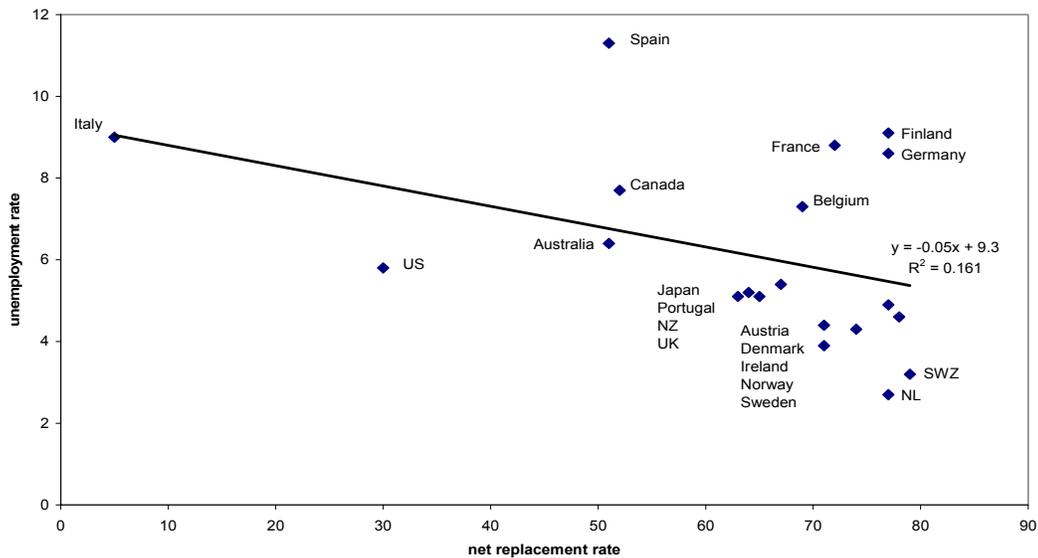
It turns out that as a general rule, simple cross-country correlations between unemployment and the best measures of the key labor market institutions offers little support for the orthodox account. Using five-year averages for the 1980s and 1990s for 20 OECD countries, Baker et al. (2005) found no statistical 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.

To further illustrate what appears to be a rather selective use of correlation evidence in this literature, we present simple correlations between unemployment and various measures of unemployment benefit entitlement generosity. We limit these to benefit entitlements both for reasons of space and because our survey of the regression literature (below) indicates that the benefit system is the labor market institution with the strongest and most robust unemployment-increasing effects.

If unemployment benefit entitlement generosity is 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 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 that 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 11 countries (in two groups) with unemployment below the US in 2002 but with net replacement rates more than twice as high (60-80 percent vs. 30 percent). With data from an OECD paper by John Martin (1996), we also found a negative relationship for 1994/5 (not shown).

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



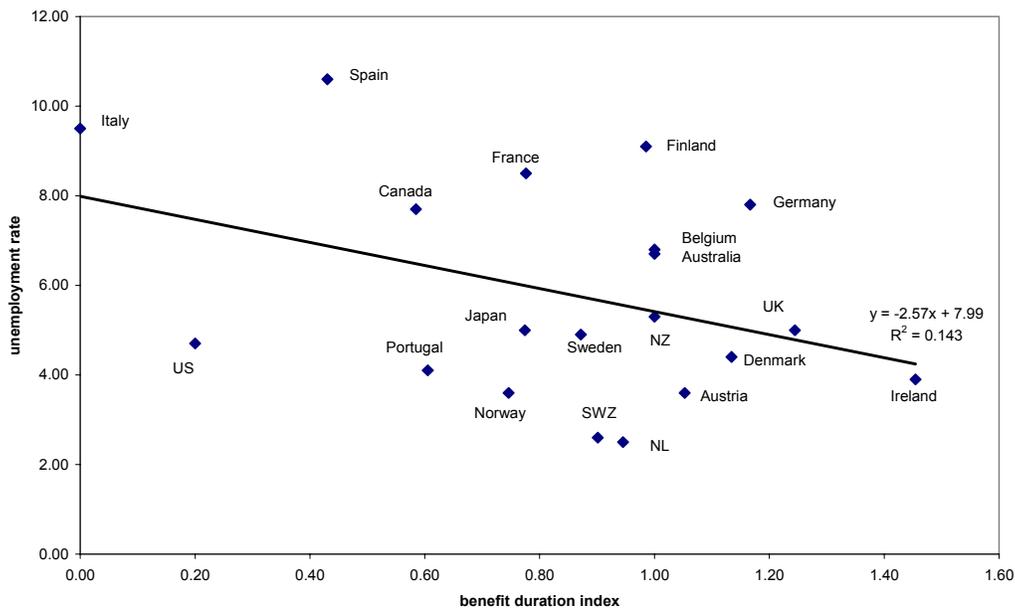
Sources: replacement rates: OECD Benefits and Wages 2004, table 3.3b (average net replacement rates over 60 months of unemployment); unemployment rates: OECD standardized rates.

Long duration of benefits is also expected to help explain high unemployment. Figure 3 shows benefit duration plotted 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 higher levels of benefit duration are associated with *lower* unemployment. Spain and Italy offered relatively ungenerous long term benefits

but have high unemployment; 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.

As noted above, Layard et al. (1991; 1994) argue that there is a close fit between benefit entitlement duration and long-term unemployment. Figure 4 shows a plot of long-term unemployment against the same duration measure used in Figure 3. 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.

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

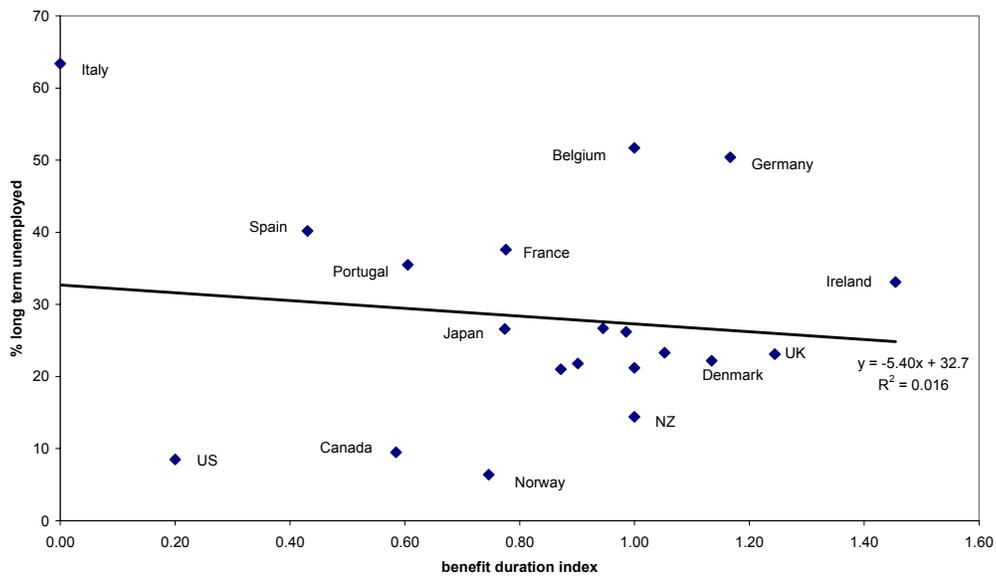


Duration is measured as the ratio of the net replacement rate for the 60th to the rate for the “initial period” (1st month), for single earner married couple without children, at 100 percent of the average production worker wages (OECD 2004, table 3.1a and table 3.2a).

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, using net replacement rates at

different points in time to measure duration, for 1991–2001 (the longest period the available data permit). 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.

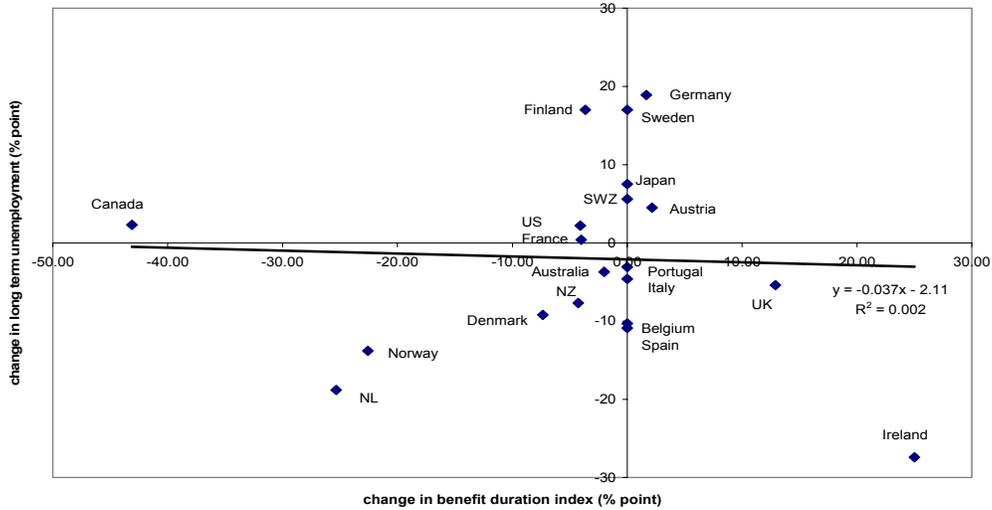
Figure 4: Unemployment Benefit Duration and Long Term Unemployment for 20 OECD Countries, 2001



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 percent of the average production worker wage (OECD 2004b, 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, Statistical Annex: Table G).

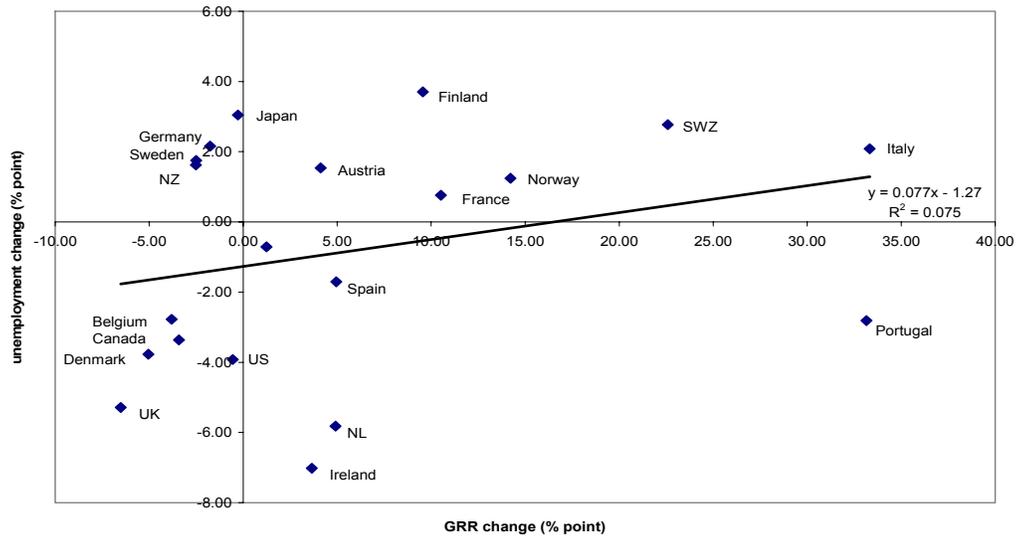
The benefits measure used in nearly all of the recent time series regression tests is the average gross replacement rate, for which there are now measures from 1961 to 2003. The change in this measure has typically been found to be significantly associated with the change in unemployment (see below). Figure 6 shows that the simple correlation between the 1982–2002 percentage point change in unemployment and the gross replacement rate has the expected positive sign. In Section 5 we will return to the question of how much weight ought to be assigned to these changes in the gross replacement rate.

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, Stat. Annex Table Q.

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



Sources: GRR (OECD, 2004, Benefits and Wages: OECD Indicators (www.oecd.org/els/social/workincentives); Unemployment: OECD Employment Outlook, Statistical Annex (Table A), various issues.

4. MACROECONOMETRIC EVIDENCE

4.1 THE CONSENSUS VIEW: CENTRALITY OF LABOR MARKET INSTITUTIONS

As employment performance across much of Europe worsened, economists turned their attention to the links between institutions, rigidities, and unemployment (Bruno and Sachs, 1985; Blanchard and Summers, 1986; Lindbeck and Snower, 1988). This early research, in turn, spawned a rapidly growing literature aimed at explaining both cross-country differences in unemployment and the evolution of these differences over time with regression tests. The most influential studies share the same broad conclusion, that in the final analysis, the evidence offers substantial support for the orthodox theoretical expectation that labor market institutions have played a key role in cross-country unemployment differences. 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. 29-30)
- “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).

In the next several sections (4.2–4.4), we survey a number of influential cross-country econometric studies for the purpose of assessing the consistency and robustness of the findings. The case for treating protective labor market institutions as the principal determinants of high unemployment will be stronger the more robust the results and 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. While broadly representative, this is by no means a comprehensive list (in some cases studies were not included because it was difficult to make comparable the implied effects). In section 4.5 we then briefly assess several recent studies that have explored the role played by labor market institutions in the striking decline in UK unemployment in the 1990s.

4.2 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. 1). 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).

The implications of these rather substantial differences are not addressed in the EMS paper, which is notable since the results of both studies have been highly influential for the way other researchers and policy makers understand the sources of poor employment performance. Indeed, Elmeskov et al. (1998, p. 2) 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).

4.3 OTHER EARLY CROSS-COUNTRY TESTS

In many papers and books published in the 1990s, Nickell (in many cases with Layard) reported 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 except EPL are strongly significant with the expected sign, his conclusion in this paper is 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." Nevertheless, the empirical results seem more in line with Siebert's (1997) much more unequivocally orthodox position in his paper in the same journal issue.

Baker et al. (2005) explored the sensitivity of the main results in Nickell's influential (1997) paper to newer versions of the institutional variables. We replaced six of the eight institutional variables used in the 1997 tests with improved measures that were employed by the same author in more recent work (Nickell et al., 2003; 2005). We also used alternative measures of union coverage (from Blanchard and Wolfers, 2000) and active labor market policies (OECD).¹¹ 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 ten percent level), and three of the remaining seven institutional variables have the wrong sign.¹²

In 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. The idea, which had in fact been around for some time, was that labor market institutions may produce higher unemployment by limiting the ability of labor markets to respond to adverse shocks, and that this can help explain why the same institutions were apparently not employment-unfriendly in previous decades. The Blanchard-Wolfers study was also distinguished by a much longer time period (eight 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 varied over time. While Nickell (1997) and Blanchard-Wolfers (2000) show broadly similar results for the two unemployment benefit measures, union density, coordination, and taxes, this is not the case for EPL, ALMP and bargaining coordination.

Table 3: Summary of Implied Impacts¹ of Labor Market institutions on Unemployment: Selected Studies, 1996-2006

	EPL 1 unit increase	UB RR +10 PP	UB Dur +1 yr	ALMP + 10 PP	Union Den +10 PP	Union Cov +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 ²	Insig	1.29	--	insig	insig	--	-1.48 ⁷	0.94
3. Nickell 1997	Insig	0.88 ⁵	0.70	-1.92	0.96	3.60 ⁶	-3.68	+2.08
4. BGHS 2005 (variation on Nickell 1997) ³	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	insig ⁴	-1.20	--	--	1.5	--	insig	insig
7. Nickell et al. 2003/2005	Insig	.96	.17	--	.30	--	-3.92	insig
8. IMF 2003	0.51	0.53	--	--	2.24	--	-0.34 ⁸	-0.69 ⁹
9. Baccaro & Rei (ILO) 2005	Insig	insig	--	--	1.02	--	insig	insig
10. OECD 2006	Insig	1.20	-- ¹⁰	--	insig	--	-1.42	2.80

¹The coefficients show the impact of differences in the independent variable on a country with the mean unemployment rate for the sample. "Insig effect" means not statistically significant at 5 percent level; (--) means the variable is not included in the regression.

²Shows impact of a change of one standard deviation in the independent variables.

³This is the result of estimating an equation similar to that in Nickell 1997 using the data from Nickell 2001 (see BGHS 2005 pp 104-4).

⁴The calculation of the change in EPL assumes a 10 unit increase in the index. Effects shown include the effect of the change in EPL assumes a 10 unit increase in the index. Effects shown include the effect of the interaction terms, under the assumption that that the interacted institutional variable is set at the sample mean for the last period.

⁵Assumes an increase of 0.12 in the duration index, which is equivalent to adding an additional year of benefits at a replacement rate of 40 percent.

⁶Assumes a rise of one unit in an index that ranges from 1 to 3.

⁷ The effect of being a country with either a low or high degree of coordination and centralization, compared to a country with intermediate levels for these measures.

⁸ Since the bargaining coordination variable is entered into this regression with a quadratic term, in order to best reflect the impact of these hypothetical changes on a typical country, it is assumed that an increase in the measure of bargaining coordination is from 0.5 units below the mean to 0.5 units above the mean.

⁹ This number is extremely sensitive to rounding. The implied effects based on the coefficients we found in our re-estimation of this equation, rather than the published results, is an increase of 0.29 percentage points.

¹⁰ This “baseline” regression does not include a duration variable, but their “column 2” regression replaces the broadly defined gross replacement rate with separate 1-year replacement, benefit duration, and the interaction measures, all of which are statistically significant. Note, however, that in this alternative specification, higher union density has a small, statistically significant, *negative* (downward) effect on unemployment.

Sources: Scarpetta 1996, table 1, column 3; Elmeskov et al. 1998 – Table 2, column 4; Nickell 1997: Table 6: column 1; BGHS 2005: table 3.6 column 2; Blanchard and Wolfers, 1999, Table 5, column 1; Belot and van Ours 2005: Table 7, column 5; Nickell et al. 2005, Table 5, column 3; IMF (2003): Table 4.3, column 3; Baccaro and Rei (2005): table 4 column 3; OECD 2006: Table 7.A1.1 column 1.

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 weaken the results. According to the authors, the table showing these results “suggests two conclusions, both worrisome: replacing the Nickell measures by alternative, but still 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 ten 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, time-varying measure 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 specification.

Belot and van Ours (2002; 2005) emphasize the importance of interactions among labor market institutions. They report the results of seven regressions, four of which test just the direct impact of institutions on unemployment, while the last three include various interacted measures. Their four direct impact tests differ only in that they include different combinations of fixed time and country effects (their 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 it should be recognized that 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 are 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 the fourth test. When the three interacted variables are introduced, the direct effect of the replacement rate is large and significant with the wrong sign (it is unemployment-reducing!). The coefficients on employment protection also have the wrong sign (EPL *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.

4.4 EXPLAINING CHANGES OVER TIME WITH ANNUAL DATA

Following 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. As noted above (Section 2), this use of unemployment data for the 1960s–70s raises serious concerns about the historical consistency of the time series and the meaningfulness of *annual* changes in the institutional measures (due to the quality of the measurements, the heavy reliance on interpolation, and the typically small or non-existent year-to-year changes in most of the institutions under investigation). What is clear is that increasing the number of observations will increase the degrees of freedom and thus reduce the apparent standard errors of the estimated coefficients. There has been virtually no justification in print for this use of long annual time series in the face of these challenges, and it is notable that the most recent work by the OECD has been much more conservative, limiting the span covered to 1982-2003.

The Nickell et al. 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 the authors believe that important secular movements in the unemployment rate have occurred *in the absence of changes in key 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.¹³ If the magnitudes of their estimates are taken at face value, the question becomes why the mainstream research program has failed to focus on explaining these large underlying country trends.

Because Nickell et al. (2005) include 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 five 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 their 2001 version of the paper) with the change in union density, which gets the expected positive and significant effect (a ten percent increase in union density produces a .3 percentage point increase in unemployment after five 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.

Nickell et al.’s more recent study (2005) offers another example of rather large effects of what appear to be fairly minor changes, this time across different versions of the same basic paper. 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. Particularly given the paper’s strong conclusions and significant implications for public policy (that shifts in labor market institutions explain movements in unemployment), it is notable that there is no discussion of robustness.

Using Nickell et al. (2001) 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,¹⁴ 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 identifies a large effect for union density, which is more than seven times larger than the Nickell et al. (2005) estimate. Interestingly, they also find, in contrast to Nickell et al., that labor taxes tend to significantly *reduce* unemployment.

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.¹⁵

By reporting four alternative specifications, the IMF report conveniently illustrates the difficulty of making generalizations about the effects of labor market institutions from cross-country regression. The implied impact of changes in labor market institutions for each of their four alternative specifications are shown in Table 4.¹⁶ 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 percent level).

With the same data set, Baker et al. (2004) further explored the robustness of the IMF's results. After replicating the results of the IMF's three most important specifications, we ran an alternative based on a composite of specifications used in earlier research in this area. The alternative specification differs from the IMF variants in using common time dummies rather than country-specific time trends.¹⁷ It also includes a somewhat 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); an additional 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. In this alternative test, we found only one of the direct institutional variables to be statistically significant (the tax wedge, at the ten percent level).

To the extent that the relationships posited in the original IMF model are true for the levels of unemployment and labor-market institutions, these relationships should also hold for changes in unemployment and labor-market institutions.¹⁸ In this alternative specification, we found that 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. In sum, minor changes to a few variables and reasonable changes in specification produce results that show no meaningful relationship between labor-market institutions and unemployment.

Baccaro and Rei (2005) build on the methods and data used by Nickell et al. (2001) and the IMF (2003). But Baccaro and Rei are distinctive in this literature for their extreme attention to robustness, exploring both static and dynamic models, yearly versus averaged data, numerous alternative estimation techniques, and a myriad of variable specifications. For example, they point out that “We started off with dynamic fixed effects models in levels using yearly data. We then shifted to dynamic models in first differences with yearly data. We then grouped our data in five-year averages and estimated fixed effects models in levels, random effects models in levels, as well as models in first differences” (p. 40). No fewer than 72 tests are presented in 12 tables.

Although many of these tests were quite similar to those published by Nickell et al. and the IMF, essentially the same data produced dramatically different results (see Table 3). Baccaro and Rei (2005, p. 44) conclude that

“Changes in employment protection, benefit replacement rates, and (the) tax wedge seem negatively associated with changes in Unemployment, even though the coefficients are (mostly but not always) insignificant. The one institutional variable we find to be positively associated with changes in unemployment is the union density change variable.... What transpires from these findings is that unemployment is mostly increased by policies and institutions that lead to restrictive macroeconomic policies... the claim that systematic deregulation of labour markets would solve the unemployment problem faced by several advanced countries appears unwarranted based on our results.”

As of this writing, the latest entry is the OECD’s effort to reassess the *Jobs Study*, which comes shortly after its 10th anniversary. The relevant empirical analysis appears in Bassanini and Duval (2006), which forms the basis for Chapter 7 of the OECD’s 2006 Employment Outlook (OECD 2006a). This research uses the latest OECD data (in annual form), which is particularly significant for two measures: EPL, which for the first time is a truly annual series (not just interpolated from a small number of years for which the OECD had estimates); and product market regulation, a measure that has been developed very recently (and covers just seven non-manufacturing industries). The authors control for cyclical effects with a measure of the output gap, and control for both time and country effects. Unlike Nickell et al. (2005), these OECD tests do not include lagged unemployment as an explanatory variable. Like Baccaro and Rei (2005), it is notable that the authors pay considerable attention to the robustness of the results.

We focus here only on the Bassanini-Duval (2006) “baseline” test. Four institution and policy variables are found to be highly significant in the predicted (orthodox) direction: the benefit replacement rate (a summary measure of benefit generosity), the tax wedge on labor incomes, high corporatism (highly centralized or coordinated bargaining), and the strictness of product market regulation. Thus, among protective 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).

This analysis, as careful and comprehensive as it is, leaves many key policy-relevant questions unanswered. While the “risk of reverse causality” is briefly

acknowledged, the authors simply note that “there is no straightforward way to address this issue” (Bassanini and Duval, 2006, p. 11). This is of particular concern for the interpretation of the estimated coefficients on unemployment benefit generosity. Like all the studies we have surveyed, the text or this report implies that significant coefficients reflect causal relationships that run from benefit generosity to the level of unemployment. Obviously, for policy purposes establishing that the effects run in this direction, and to what extent, is critically important.

4.5 TIME SERIES EVIDENCE FROM THE UK

A number of studies have attempted to explain long-run trends in unemployment with country-specific time series tests. Perhaps the most extensive work has been done on the UK, which has experienced large swings in unemployment, sharp declines in union membership and power, and fairly substantial labor market deregulation.¹⁹ It is worth a brief detour to see what the UK time series evidence says about the role played by protective labor market institutions.

Extending his earlier work on Britain (Nickell, 1988; Nickell and Bell, 1995), Nickell (1998) accounts for changes from 1964 to 1992 with measures of industrial turbulence, the replacement ratio, terms of trade, skill mismatch, the union mark-up and the real interest rate. It is notable that the benefit replacement rate is not significant and the union power measure accounts for just 19 percent of the overall rise in unemployment. But Nickell judges the equation as a whole a success, since it “comfortably explains the four-fold rise in unemployment... Despite the fact that it seems quite easy to explain long-run trends in unemployment in Britain using wage pressure variables, there remains a feeling of dissatisfaction. Seven variables is a lot, so it is hard to tell a simple story. And, because of this, many remain unpersuaded” (p. 815).

Several recent studies have had a considerably harder time explaining the trend in UK unemployment with institutional and policy measures. Henry and Nixon (2000) extend Nickell (1998) and find that UK unemployment can be well-explained by a small number of transitory shock variables: oil prices, terms of trade, and real interest rates. More recently, Henry (2004) finds that wage pressure variables do not explain the UK experience: “To summarise the overall findings reported in the tables above, these show that the labour supply variables (institutions) do not figure strongly in the behaviour of wages and employment.”²⁰

Another study by two Bank of England researchers (Cassino and Thornton, 2002, p. 34) focuses on the 1960–98 period. They conclude 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.”

In sum, the time series evidence suggests that the deregulation of the UK labor market does not account for much of the observed changes in unemployment since the 1970s.

4.6 ASSESSMENT

The two most recent studies we have considered, Baccaro and Rei (2005) and Bassanini and Duval (2006), took advantage of the availability of improved institutional measures and the methodological innovations of earlier studies. But they are also characterized by much greater attention to issues of robustness. It is notable, therefore, that these latest studies have found substantially less support for the orthodox rigidity view than earlier studies. For example, Scarpetta (1996) finds significant effects for EPL, the replacement rate, and union density, and Nickell (1997) finds the same for the replacement rate, benefit duration, union density and union coverage. In contrast, with better data and more apparent attention to robustness issues, Baccaro-Rei and Bassanini-Duval each find just one measure of protective labor market institutions to be significant (union density; the gross benefits replacement rate).

The results in Table 3 indicate that the Bassanini-Duval finding for benefit generosity is, with just a few exceptions, consistent with the results of earlier studies, and for this reason we will address its relationship to unemployment in more detail in the next section.

Despite the fairly consistent support in Baccaro and Rei, we are not convinced that there is a meaningful direct union density effect. First, while Table 3 shows that seven of the eleven studies considered have found a significant effect for union density, the OECD's recent literature survey (2006a, table 3.9) concludes that only five of the sixteen studies they cover show unequivocal positive (unemployment-increasing) effects. For two of these studies, Nickell (1977) and the IMF (2003), the positive union effect disappeared in our replication tests (Baker et al., 2004, 2005). And second, as noted in section 2, it is not clear what this variable actually measures. The effect of union density on employment performance is complicated by vast differences between density and coverage in some countries, and by the role of the centralization and coordination of bargaining, which in most tests are associated with *lower* unemployment. As the OECD (1999b, 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.”

It is widely accepted that strict employment protection regulations are central to any explanation of persistent high unemployment in Europe, but the panel data offer little supporting evidence. The evidence in Table 3 is quite mixed. With the exception of the IMF (2003), the results reported here that find the predicted positive and significant effect on unemployment were all published before 2001. In these four studies, the implied effects of a one unit increase in EPL strictness on unemployment are substantially different, ranging 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 Bassanini and Duval (2006) all find no effect, or even unemployment-reducing effects. In the 1999 *Employment Outlook*, OECD researchers reached a similar conclusion: “The basic finding appears robust: overall unemployment is not significantly related to EPL strictness” (OECD, 1999a, p. 77). However, negative employment-related effects of current employment protection laws are likely to be found among subgroups of workers—particularly youth and the least skilled (OECD, 2004a).

In sum, the regression evidence surveyed in this section does not, in our view, offer compelling statistical evidence that protective labor market institutions are at the root of persistent high unemployment in OECD member countries. Perhaps the most robust effect is shown by the gross replacement measure of unemployment benefit entitlements. The next section takes a closer look at the evidence on the role played by the unemployment benefit entitlement system.

5. UNEMPLOYMENT BENEFIT COMPENSATION AND UNEMPLOYMENT

5.1 SOME INITIAL CONSIDERATIONS

Spurred by the growing influence of job search theory, the availability of unemployment benefits has become the cornerstone of the orthodox theory of wages and unemployment (Holmlund, 1998, p. 115). As the costs of unemployment to workers decline, so does the incentive to search for work and take a job. This disincentive, in turn, could help increase the bargaining power of both new job applicants and current employees, and higher wages may in turn reduce labor demand and thus raise unemployment (and/or joblessness).

A good example of the application of this theory to the real world of high European unemployment is Lungquist and Sargent’s (1998) 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” (p.

547). Without considering the possibility of policy endogeneity and with little reference to direct empirical evidence, they conclude with the strong 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 (1991; 1994) belief that “unconditional payment of benefits for an indefinite period is clearly a major cause of high European unemployment.”²¹

It seems, however, that policy endogeneity ought to be considered. Policy makers are likely to make benefits more generous as the unemployment rate increased: in hard times, with a decline in employment or the rate of employment creation, taxpayers could reasonably want to give unemployed workers additional time and support to search for the right job, thereby improving the quality of the match between workers and jobs. More generous benefits would reduce the harmful effects of lost income on workers and their families and 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). It is, however, extremely hard to find any mention of even the possibility of this “policy endogeneity” in the unemployment-institutions literature.

This section begins with some reasons for caution in the interpretation of regression coefficients between benefit generosity and unemployment. We then turn to the microeconomic evidence, which has frequently been employed in support of the orthodox interpretation of the macro results.

5.2 INTERPRETING BENEFIT EFFECTS

Table 3 suggests a broad consensus finding that a ten percentage point change in the OECD’s overall measure of benefit generosity will generate a change in the unemployment rate of about one percentage point. If such substantial effects are to be believed, one might expect more political support for modest reduction, particularly in countries with replacement rates in the 60–80 percent range. But there are some reasons to believe that this may be an implausibly large effect.

5.2.1 ACCESS TO THE BENEFITS SYSTEM

First, a substantial number 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 percent; Greece, 9 percent; Spain, 24 percent; Portugal, 27 percent; France, 45 percent; the Netherlands, 50 percent; Germany, 70 percent; Austria and Denmark, 66 percent; Ireland, 67 percent; Sweden, 70 percent; and Belgium, 81 percent (Manning, 1999, p. 144). According to the most recent evidence, the share of “ILO unemployed” actually in receipt of unemployment benefits in OECD-member countries ranges from around 20 to 80 percent (OECD, 2004b, fig 3.6).

That benefit reciprocity rates tend to be much smaller than unemployment rates reflects 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).

Table 5: 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)

The second is eligibility for benefits. All OECD countries, for example, require prior employment experience. Eligibility 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 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) and 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 5 shows youth unemployment rates and net replacement rates for 20-year-old single workers. Among the four largest continental European countries, three have by far the highest youth unemployment rates (France, 26.5 percent; Spain, 28.3 percent; and Italy, 31.1 percent), but youth in these three countries were ineligible for unemployment insurance benefits (see column 2). As the OECD's *Benefits and Wages* (OECD 2004b, 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 worth noting that a number of countries with very low youth unemployment (at or below ten percent) are, according to the net replacement measure, extremely generous to their youth (Germany, the Netherlands, Ireland, and Denmark). If half the unemployed are not getting benefits, the measured effect must be twice as large for those who are. At a minimum, it seems fair to say that a policy reform that substantially reduced the replacement rate would have little effect on the youth unemployment rate.

Much has been made of the role that stricter eligibility rules (and stricter enforcement of them) have played in accounting for sharp declines in unemployment in countries like the Netherlands and Denmark. It is worth a brief digression to address this claim. 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).²²

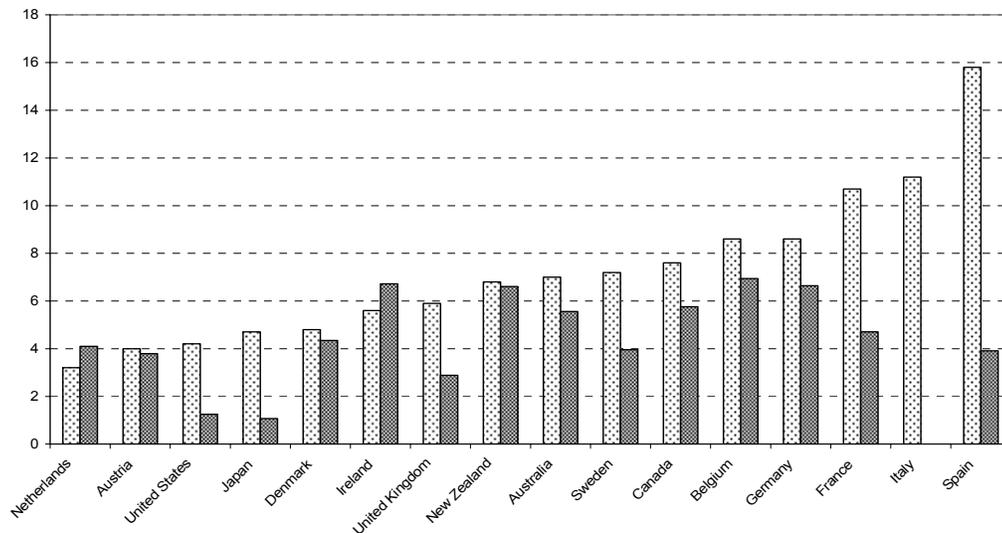
While tightening eligibility rules and their enforcement will tend to reduce participation in the unemployment compensation system, it is not entirely clear that such administrative reforms have played a major role in the employment success of either the Netherlands or Denmark. In his discussion of a new law toughening Dutch 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 a full 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 percent in 1994 to 3.2 percent in 1998 to 2.5 percent in 2001—which suggests that the tightening of sanctions may have been more a reflection of a strengthening labor market (an environment in which it is much easier to be tougher) than a principal *cause* of this employment performance “miracle” in the first place. In another figure, van Ours (2003, figure 3) shows a huge takeoff in the total number of working hours between 1995 and 2000. Again, this suggests that it may have been rapidly improving job opportunities rather than tougher enforcement policies that accounts for the sharp decline in unemployment.

If effective tightening of eligibility for participation in the unemployment benefit system has played a leading role in reducing unemployment rates, we might expect to see some association between benefit recipiency rates and unemployment rates. The reasoning is that easy access to generous benefits encourages workers to become unemployed. Figure 7 shows unemployment rates and unemployment benefit recipiency rates (recipients as a share of the working age population), ranked by the unemployment rate in 1999. The Netherlands achieved an unemployment rate of just 3.2 percent by the end of the decade, the best among these 16 countries. With a lower unemployment rate than the U.S., Japan, or the UK, the Netherlands showed a higher benefit recipiency rate (four percent). In short, 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 recipiency rate as the Netherlands, and the French rate was only slightly higher, despite much higher unemployment in all three countries.

Figure 7 suggests a similar story for Denmark. After all reforms of the 1990s, Denmark’s benefit recipiency 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 it is true that both the Netherlands and Denmark reduced their benefit recipiency 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 market opportunities 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. Benefit generosity as measured by reciprocity rates is uncorrelated with unemployment. Indeed, it is particularly striking how large the gap is between these two rates for the high unemployment countries (Spain, France and Italy).

Figure 7: Unemployment Rates and Unemployment Benefit Reciprocity Rates for Selected OECD Countries, 1999



Source: OECD²³

5.2.2 *Timing and Causality*

A second reason to question the consensus effects reported in Table 3 concerns the timing and causality of trends in unemployment and benefit generosity. To begin with, if there is a payoff in labor market performance from benefit reforms that increase work incentives, it is not likely to be observed right away. Indeed, as the OECD points out, this lag can be quite substantial: “The lags between changes in programme rules and in the number of beneficiaries appear to be very long: between 7 to 10 years in the case of unemployment benefits...” (OECD 2004b, p. 58). This may help explain why policy makers in some democratic countries are loath to make major changes—the costs are immediate but the payoffs stretch out well beyond their likely term of office.

But perhaps most importantly, to the extent that policy makers increase and decrease the generosity of benefits in response to the perceived need for a safety net, the statistical fit should not necessarily be interpreted as a measure of the rigidity effects of the benefits system because the causation may run from poor employment conditions to benefit generosity. Figure 8 presents within-country unemployment and gross replacement rate (GRR) trends for four “success story” countries and four “failure” countries. “GRR” is by far the most popular measure of benefit generosity in recent empirical tests and has frequently been found to be significantly related to the pattern of unemployment (see Table 3).

Figure 8 shows the GRR and UR trends for each of the success stories.

- While Denmark’s success is shown by 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 percent 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. Curiously, this change in the OECD’s GRR appears to contradict a national measure that shows a sharp decline between 1980 and 2000 (from 71 percent to 56 percent), which is heavily relied upon in the explanations given for the “Dutch Miracle” by Broersma et al. (2000) and van Ours (2003).²⁴
- The Irish unemployment rate rose drastically in the early 1980s, stayed very high between the mid-1980s and mid-1990s, and has fallen dramatically since, while the Irish GRR remained 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 drives the benefits level in each case—just the reverse of the orthodox prediction.

The first four columns of Table 6 show the F-statistics of Granger tests of whether unemployment rates predict the benefit replacement rate, while the four columns in Panel B test the reverse—that unemployment benefit generosity predicts unemployment. To allow for the broadest reasonable test of the relationships, we present results separately for Granger tests that include one, two, three, and four year lags of the gross replacement rate.

TABLE 6

(F-tests)

Granger-Causality Tests, Gross Replacement Rate and Unemployment Rate, 1962-2004

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

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

These results clearly indicate that the predicted effect runs from unemployment to benefits. Changes in unemployment predict changes in the benefits measure (GRR) for Denmark with significance at the five-ten percent level for all four lags; for the UK at the five-ten percent level for each of the first 3 lags; for the Netherlands at the one percent level for the first two lags and at the five-ten percent level over the 3rd and 4th year; and for Ireland with significance at the one percent level in the first year. Interestingly, another success story—the U.S.—shows this “reverse causality” as well for lags 3–4.

Figure 8: Gross Replacement Rates and Standardized Unemployment Rates For 8 OECD Countries, 1961-2004

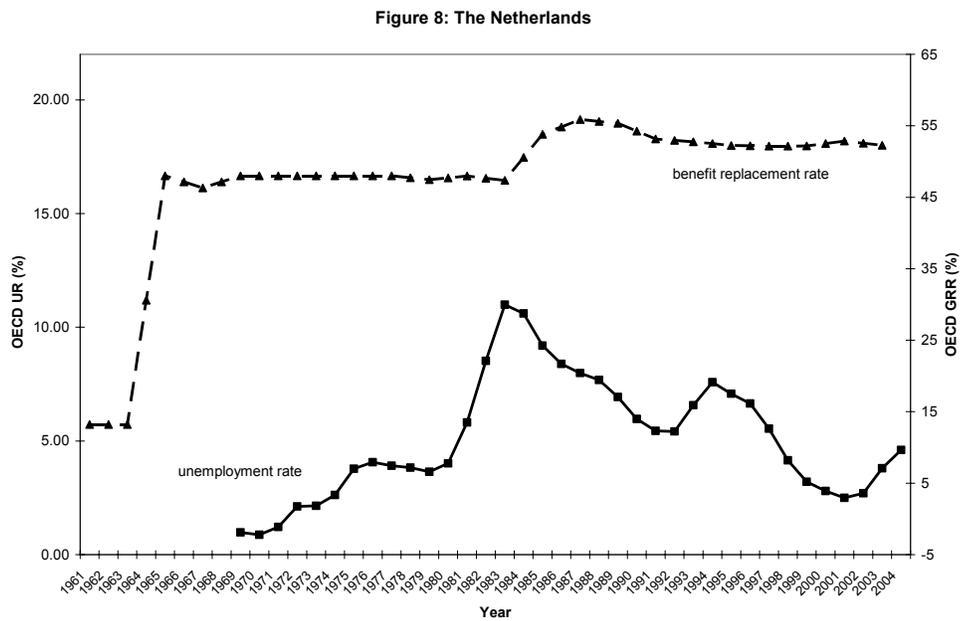
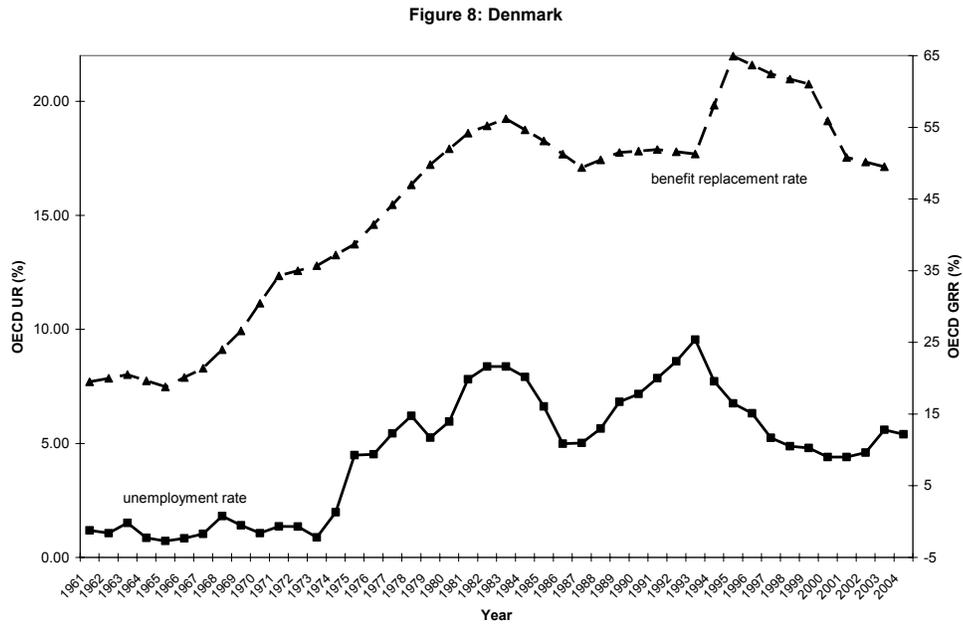


Figure 8: Ireland

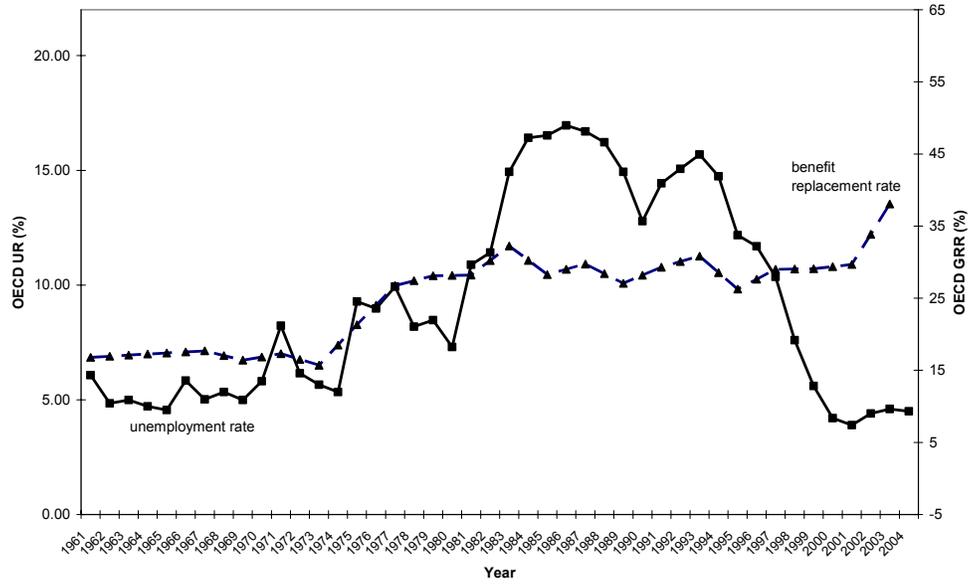


Figure 8: United Kingdom

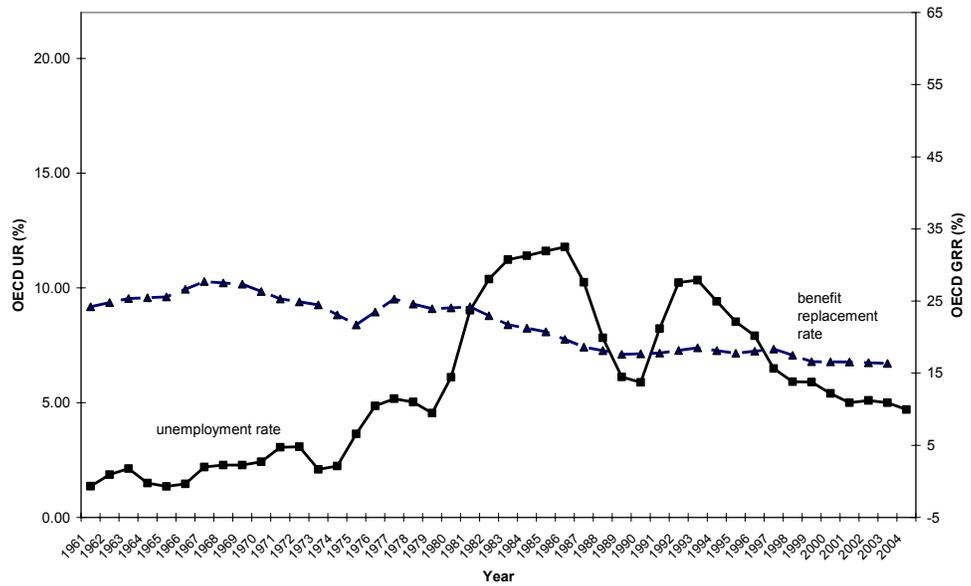


Figure 8: France

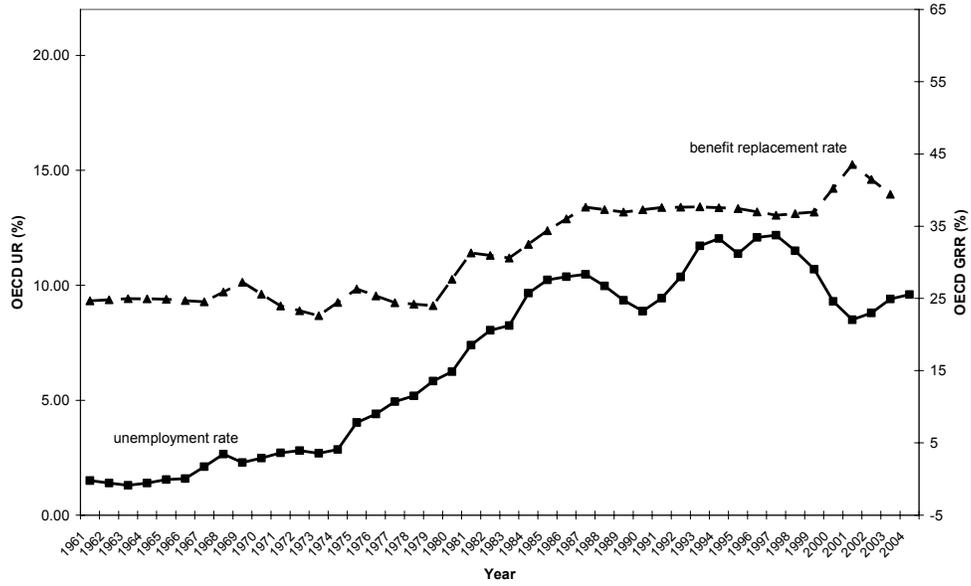


Figure 8: Germany

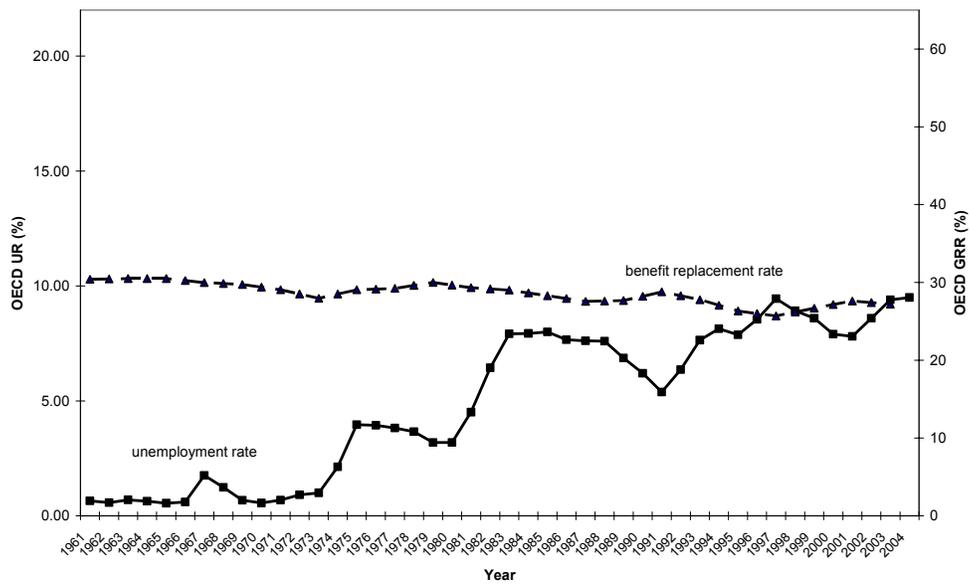


Figure 8: Spain

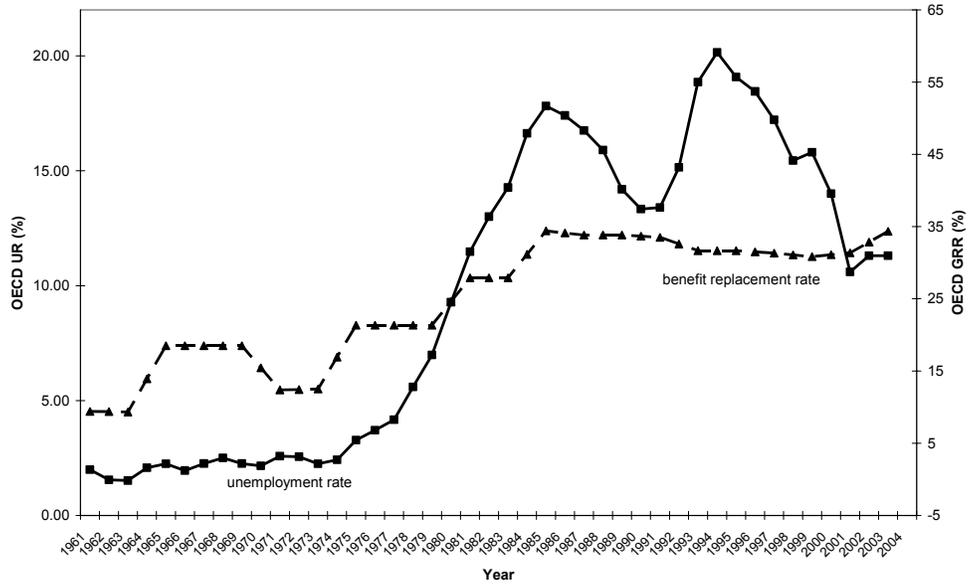
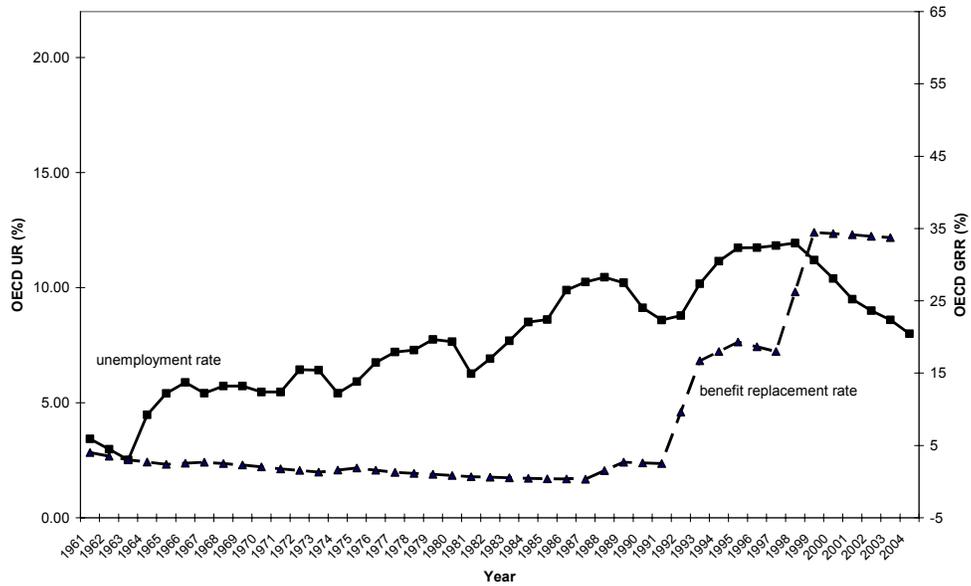


Figure 8: Italy



The remaining graphs in Figure 8 show UR and GRR trends for the four “failure” countries:

- At least until the late 1990s, the unemployment and benefit 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; see also Blanchard, 2006). Consistent with this account, the figure shows a sharp rise in unemployment from 1975 to 1986 and an increase in benefit generosity that begins in 1980. This story is also consistent with the Granger results shown in Table 6 for France: all four lags show a significant relationship from unemployment to GRR and this is particularly strong for the first two years.
- Germany shows a steadily rising UR but a fairly stable GRR.
- The trends for Spain shows that since the mid-1980s GRR remained stable at a moderate rate of just above 30 percent, while unemployment shows a huge increase from the late 1970s through the 1980s, a peak at over 20 percent in 1994, and then a sharp decline to 10.9 percent 2004.
- 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 6 indicate that the benefits measure *follows* rather than precedes changes in the unemployment rate.

Evidence that benefit generosity tends to reflect the state of the labor market (“policy endogeneity”) has also been found by several other studies. Although they make little of it, 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). DiTella and MacCulloch (2002, p. 413) conclude that “in terms of Granger causality, it is just as likely that causality runs from unemployment to benefits as it is that causality runs the opposite way.” Their regression results show a negative lagged effect of unemployment on the level of benefits, suggesting a tax burden effect, but the longer run effects appear to be positive: “The evidence tends to favor the hypothesis that long-run unemployment bolsters demands for more generous long-duration benefits” (p. 418). More recently, the OECD (2006b, p.18) has concluded 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).”

5.3 THE MICROECONOMETRIC EVIDENCE

5.3.1 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. 9):

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) and the OECD (2006a, Chapter 3, p. 59) also cite Holmlund (1998) for microeconomic evidence supportive of a strong benefit generosity-to-unemployment effect.

It is notable that a close look at what Holmlund actually writes produces – in our reading at least - a strikingly different picture. The only reference we found to an elasticity estimate of “.4” comes in a critical discussion of a paper by Mortensen. Holmlund ref to Mortensen’s “simulation results of UI policies in a parameterized version of the Mortensen and Pissarides (1996) 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 then 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, this .4 estimate is actually an example of results from policy simulations that require “more than the usual caution.” Indeed, the Elmeskov et al. 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 states that this conclusion simply reflects “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 compelling 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) stresses the weak character of the evidence: “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 he does conclude that “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” (Holmlund, p. 138).

5.3.2 MICRO EVIDENCE ON UNEMPLOYMENT DURATION

At least after some threshold, more generous benefits can be expected to increase unemployment duration, and all else equal, this should increase unemployment rates. But all else is not equal and too frequently higher (lower) unemployment duration is conflated with higher (lower) aggregate and unemployment rates. In

fact, higher levels and duration of benefits not only tend to increase the incentive to remain unemployed, but also tend to counter the effect of this increased incentive by influencing the inflows into both unemployment and employment.

Regarding inflows into unemployment, 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."

Higher unemployment benefit generosity can also affect inflows into employment, which could limit or offset the effect of longer unemployment duration on the aggregate unemployment rate. Often referred to as the "entitlement effect," Holmlund (1998, p. 116) explains that "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. The possibility of "composition" and "entitlement" effects means that there is no necessary direct relationship between average unemployment duration and the unemployment rate.

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 same assessment appears in Layard et al. (1991). But the evidence from the 1970s and 1980s was in fact quite mixed. According to Atkinson and Mickelwright (1991, p. 1712), "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." At about the same time, Barr (1992, cited by Hammer, 1999, p. 132), comes to a similar conclusion: "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."

Over the last decade, many studies have taken advantage of the natural experiment-like quality of major policy shifts, and these have tended to find more convincing evidence for a benefits effect on unemployment duration, though the

magnitudes of the effects can be surprisingly small. Among studies finding fairly large effects is Roed and Zhangs (2003, p. 204), whose study of Norwegian data suggested that a ten percent decline in benefits “may cut a 10-month duration by approximately one month for men and 1-2 weeks for women.”

Lalive and Zweimuller (2004) examine a massive Austrian policy change that offered, they argue, 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).²⁵ In another study of this dramatic Austrian policy shift, Lalive et al. (2004) found that extending benefits from 30 to 39 weeks for 40–49 year olds “tends to increase expected unemployment duration by .45 weeks... increasing replacement rates by 6 percentage points tends to increase unemployment duration by .38 weeks” (p. 18). The nature of the policy shift allowed the authors to assess the effects of the joint effect of raising the duration and level of benefits: a 33 percent increase in potential benefit duration and a 15 percent increase in the level of benefits combined to raise unemployment duration by 3–4 days. Whether this seemingly modest effect of a substantial increase in unemployment benefit generosity had any effect on aggregate unemployment rates would depend on many factors, including (as noted above) the extent of composition and entitlement effects.

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 percentage points. 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, as might be expected. 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 positively 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 the highly developed Danish ALMP system combined with strict enforcement of participation in these programs has helped produce a very low unemployment rate (Danish Ministry of Finance, 1998).

In sum, references to micro evidence for support of macro findings that show a positive relationship between unemployment benefit generosity and unemployment rates have, in our view, tended to greatly overstate the case. The evidence surveyed by Atkinson and Mickelwright (1991) and Holmlund (1998) on the relationship between benefit duration and unemployment duration was mixed, particularly for European countries. Although more recent “natural experiment” studies report fairly consistent effects in the expected direction, several of the examples cited above also show surprisingly modest effects from drastic shifts in benefit generosity. And finally, there is no reason to believe that there is any simple direct link between these micro findings on incentive effects and movements in the aggregate unemployment rate.

5.4 ASSESSMENT

This section has outlined some reasons for caution in concluding from the available regression evidence that benefit generosity is at the root of the pattern of unemployment and its change over time in OECD countries.

Concerning the interpretation of the macroeconometric evidence, it should be recognized that in many countries, especially the high unemployment countries of Southern Europe (Spain, France, Italy), only a modest share of the officially counted unemployed actually receive benefits. This means the presumed effects of benefit generosity on the supply (work incentives) and demand (wage pressure) sides apply to as little as 20–50 percent of the unemployed. Further caution is suggested by the timing and causality in the relationship between benefit generosity and unemployment rates. 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.” Time trends between gross replacement rates and unemployment show little correspondence for either the “success” or the “failure” countries, and 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).

The microeconomic evidence offers some support for the predicted effects of changes in benefit entitlement generosity, eligibility rules, and enforcement strictness on the duration of unemployment benefit and the exit rate out of unemployment. But the results are surprisingly mixed and often with quite modest effects. In any case, the micro evidence does not imply that reductions in generosity or tightening eligibility rules would necessarily have any effect on the overall unemployment rate since changes in benefit generosity may have offsetting effects via inflows into unemployment and employment (due to “composition” and “entitlement” effects).

The orthodox prediction rests on large effects of the benefit system on the reservation wage and worker search behavior, and consequently on aggregate unemployment rates. There is very little direct evidence that establishes these links. What *has* been demonstrated is that most studies have found a significant statistical relationship between measures of benefit generosity and cross country patterns of unemployment. But we know of no research that has established that the direction of causation runs primarily in the orthodox direction or that micro evidence linking benefit generosity and unemployment duration translates to aggregate unemployment rates. On the basis of the available evidence, it seems reasonable to remain skeptical that any conceivable change in typical European benefits systems could alter wage pressure and search behavior sufficiently to have meaningful effects on the aggregate unemployment rate.

6. COMPREHENSIVE LABOR MARKET REFORM INDICATORS

It has become increasingly recognized that the institutions that distinguish national labor markets tend to be complementary and that reforms are likely to be more effective if implemented in a comprehensive manner across labor market institutions. The regression approach is not particularly well-suited for testing alternative models defined by differently designed mixes of institutions, although recent studies have attempted to allow for complementarity by interacting selected sets of institutions (e.g., the replacement rate and EPL; EPL and union density). In recent work on “varieties of capitalism” (Freeman, 2000; Hall and Soskice, 2003; Amable, 2003), it has been argued that very different combinations of institutions can produce equally effective employment results, ranging from highly *laissez-faire* markets (the “American” model) to far more regulated markets and a large

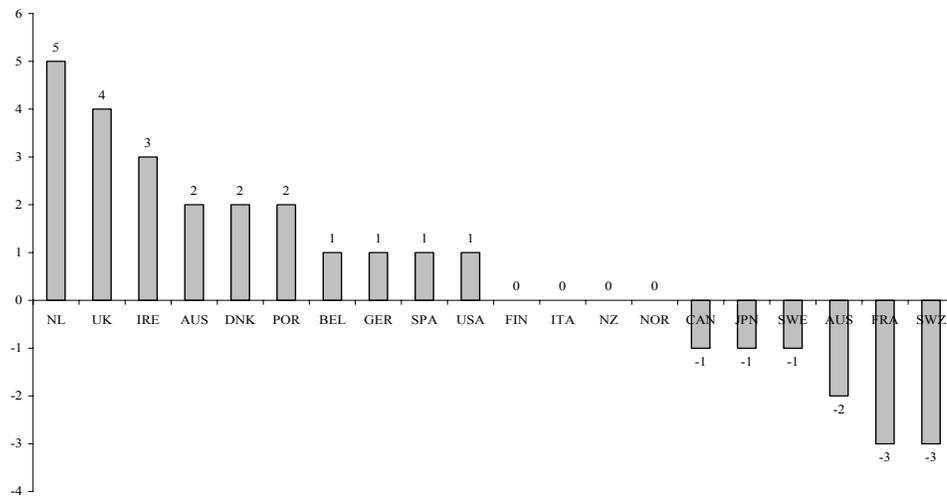
state role (the “Nordic” model).

This section critically assesses recent research that has explored the payoffs from the degree to which reforms are implemented in comprehensive packages. In the research considered here, “reform” generally means deregulation: less product market regulation, less unemployment benefit generosity, less employment protection, and lower trade union membership and coverage. Lower taxes are also viewed as a reform. The same goes for *increases* in active labor market policy spending and the degree of coordinated bargaining, despite the fact that these last two “reforms” are in fact interventions designed to improve outcomes by moving away from decentralized markets. Still, the guiding hypothesis is that more comprehensive deregulatory reform will produce lower levels of unemployment.

6.1 NICKELL’S REFORMS INDEX

European unemployment is concentrated in “The Big Four” countries (France, Germany, Italy, and Spain). In a recent paper, Nickell (2003) aims 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.” Based on his judgments of which change in labor market institutions qualify as employment-friendly and unfriendly, Nickell develops a labor market reforms scorecard and explores its relationship to changes in unemployment between the early 1980s and the late 1990s.

Figure 9: Nickell's Policy Reform Scorecard: Net "Good" and "Bad" Labor Market Policy Changes, Early 1980s to Late 1990s



Source: author’s calculation from Nickell 2003, table 13.

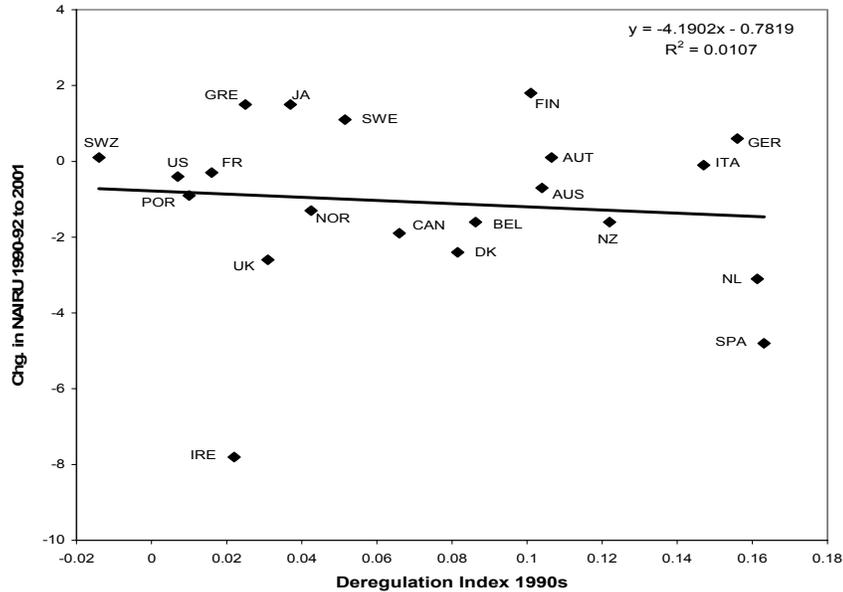
In a simple OLS regression, Nickell accounts for 51 percent of the variation in unemployment over these two decades for 20 OECD countries with the sums of “ticks” (good changes) and “crosses” (bad changes) on nine institutional measures. According to Nickell, based on this evidence “We may reasonably 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” (Nickell, 2003), and it is this evidence that Gilles St. Paul (2004: 53) cites for his conclusion that “rigidities that reduce competition in labor markets are typically responsible for high unemployment.”

But Nickell’s policy reforms scorecard—the net sum of ticks and crosses—actually fails to identify three of the four big high-unemployment countries.²⁶ As Figure 9 shows, according to his net totals of ticks/crosses, only France gets more “bad” than “good” marks, and there is some question about the appropriateness of the French score.²⁷ The scorecard indicates that 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 (zero to one) as the U.S. and Norway. This figure shows that, 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.

6.2 OECD REFORMS INDICES

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 (1999b) as part of its follow-up to the *Jobs Study* (OECD 1994). The OECD 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.

Figure 10: Labor Market Deregulation and Changes in the NAIRU for 21 OECD Countries in the 1990s

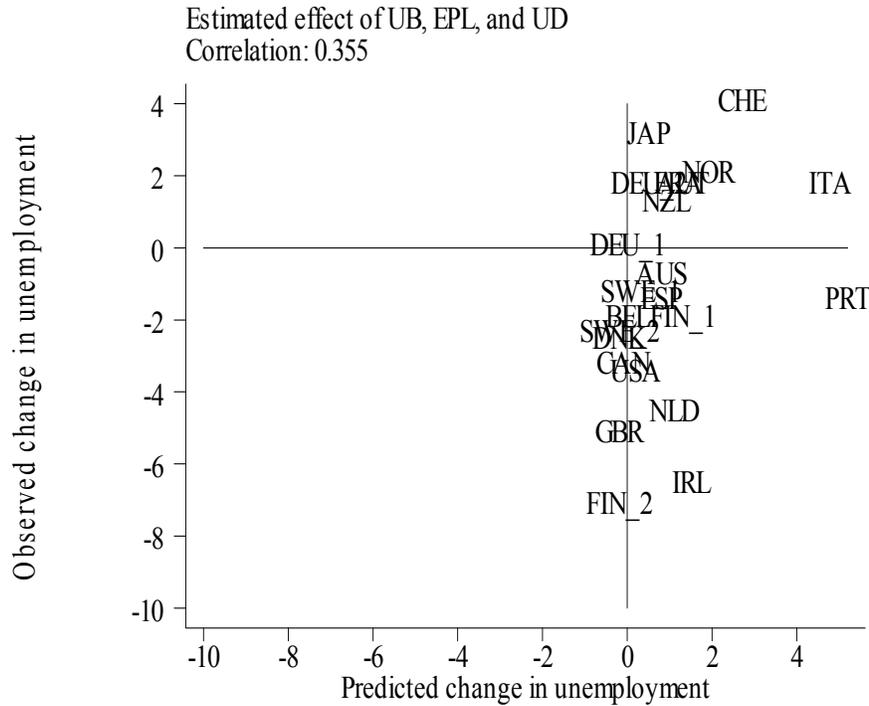


Source: authors' calculations from OECD (1999b).

The OECD found a significant positive relationship 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 has, indeed, been a payoff to countries that have implemented the *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.²⁸ 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. 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 (see Baker et al., 2005). Figure 10 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.

Figure 11: Predicted Change in Unemployment from Protective Labor Market Institutions and the Observed Change in Unemployment, 1982-2003



Source: authors' calculations from Bassanini and Duval (2006)

The most recent OECD effort of this sort replaces these rather ad hoc measures of “reforms” with the coefficients from the Bassanini-Duval (2006) baseline regression. According to the OECD’s Figure 7.3b in the latest *Employment Outlook* (OECD, 2006a), the unemployment predicted by changes in institutions and policies alone is highly correlated (.69) with observed changes in observed unemployment over the 1982–2003 period. As before, we have decomposed this overall effect to determine the role played by the three protective labor market institutions (PLMIs) by themselves. Figure 11 shows that the change in unemployment predicted by PLMI changes is far less strongly correlated to observed unemployment (less than .36), and this result is driven mostly by two very low unemployment countries (Norway and Switzerland). Adding the tax wedge improves the correlation dramatically, to .65 (not shown).

TABLE 7
Analysis of unemployment effects of "bad" institutions, 1982-2003

	Contribution from changes in			Actual chg in standardized unempl.	Standardized unempl., 2003
	"Bad" LMIs	"Bad" plus labor taxes	Product- market reg		
Finland (1993-03)	-0.2	-0.4	-1.1	-7.4	9.0
Ireland	1.5	-3.9	-1.5	-6.8	4.6
United Kingdom	-0.2	-3.7	-2.1	-5.4	5.0
Netherlands	1.1	-1.1	-2.4	-4.8	3.7
United States	0.2	-2.6	-0.8	-3.7	6.0
Canada	-0.1	2.6	-1.5	-3.5	7.6
Denmark	-0.2	-1.5	-2.3	-2.8	5.6
Sweden 1993-03)	-0.2	0.7	-0.9	-2.6	5.6
Finland (1982-90)	1.3	1.2	-0.5	-2.2	9.0
Belgium	0.0	0.4	-2.0	-2.2	7.9
Spain	0.8	0.7	-1.8	-1.7	11.3
Portugal	5.2	4.2	-2.0	-1.7	6.2
Sweden (1982-90)	0.3	0.2	-0.1	-1.5	5.6
Australia	0.8	1.4	-1.5	-1.0	6.1
Germany (1982-90)	0.0	0.0	-0.4	-0.2	9.1
New Zealand	0.9	0.2	-1.9	1.0	4.6
France	1.2	2.7	-1.8	1.5	9.5
Germany (1993-03)	0.5	0.4	-1.5	1.5	9.1
Italy	4.8	3.2	-2.0	1.5	8.4
Austria	1.3	1.9	-1.6	1.5	4.3
Norway	1.9	1.0	-1.9	1.8	4.5
Japan	0.5	2.7	-1.7	2.9	5.3
Switzerland	2.7	1.9	-0.8	3.8	4.2

Notes: Authors' analysis of data in Bassanini and Duval, 2006, Figure 1.2, and standardized unemployment rate data from OECD, OECD Employment Outlook 2005, Table A. "Bad" institutions are: average replacement rate, union density, and employment protection legislation.

The country level changes underlying these correlations results appear in Table 7. Based on the Bassanini-Duval coefficients, this table shows the contributions of PLMIs (benefit entitlement levels, employment protection strictness, and trade union density) to the 1982–2003 change in unemployment (column 1); this effect plus the effect of changes in the tax wedge (column 2); and the effect of changes product market regulation (column 3). These effects can be compared to changes in the observed standardized unemployment rate (column 4). The table is sorted by these column 4 changes.

The top of the table shows that reforms in PLMIs played virtually no role in the success of the “success stories.” In Ireland and The Netherlands—two of the

four countries with the largest declines in unemployment, changes in "bad" institutions actually increased unemployment (1.5 and 1.1 percentage points respectively). Among the top eight performers, where unemployment declined from 2.6 points to 7.4 points, the effect of these "bad" labor market institutions was negligible (.1–.2 points, or from 1/26th to 1/74th of the total change). And finally, it is notable that among the four big high-unemployment countries, only Italy shows a large contribution of PLMIs to unemployment (4.8 points), but this is mainly the effect of increasing the benefits replacement rate from zero to standard European levels in the mid-1990s, *after* most of the increase in unemployment had occurred (see figure 7b). France's 1.2 point contribution or PLMIs to unemployment were more than offset by the effects of reduced product market regulation. On balance, the new Bassanini-Duval results suggest that core protective labor market institutions have played little role in changes in OECD unemployment since the early 1980s.

7. CONCLUSION

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 greatly advanced our knowledge in many areas. At the same time, as Manning (1998) suggests in his comment on the Holmlund survey, theory seems the dominant partner in this interplay, playing a "disturbingly large part in informing the discussion" (p. 145).²⁹

Our survey is motivated by a concern that empirical research on the determinants of high unemployment in the developed world has been, to a disturbing degree, driven by efforts to verify, or confirm, orthodox theory, rather than by efforts to critically test it. Blaug (1992) has identified this as a wider affliction in the discipline. As he puts it (p. 241), "instead of attempting to refute testable predictions, modern economists all too frequently are satisfied to demonstrate that the real world conforms to their predictions, thus replacing falsification, which is difficult, with verification, which is easy."³⁰ This paper can be viewed, then, as a case study in which we take a deliberately skeptical stance on the ability of the available statistical evidence to support the conventional orthodox view that protective labor market institutions can largely account for differences in how unemployment has evolved across countries since the 1970s.

We began by noting that the cross-country pattern of unemployment has changed dramatically over the last three decades. For the major OECD countries, the overall picture is one of sharp increases in the level and dispersion of rates

through the mid-1990s, followed by a striking decline in level and dispersion. At the same time, we have seen large swings in unemployment in countries with very different institutional settings: the UK, Ireland, Canada, and New Zealand among the English-speaking market-oriented countries; Spain and Germany among the continental European countries; and Denmark, the Netherlands, Sweden, and Finland among the Northern European and Nordic countries. These are among the most extreme examples of the changes in employment performance that the presence of protective labor market institutions (PLMIs) is presumed to be able to explain.

Given the difficulty of developing consistent series unemployment rate series over time and across countries, and the even more daunting task of generating consistent time series of policies and institutions, it is surprising that so little attention has been paid to the historical consistency and quality of the data. There have been considerable efforts to improve the institutional variables, notably by the OECD, but gains from these improvements (and from the use of more sophisticated econometric methods) may have been compromised in studies that have extended the time series analysis back to the early 1960s with annual data, a research strategy that requires much better data than are currently available. For these reasons, the most recent entry into this field by the OECD (OECD, 2006a; Bassanini and Duval, 2006) examines just the period since 1982. It is notable that as the data and econometric methods have improved, the number of PLMI measures found to be significant in the expected direction has plummeted: compare, for example, Scarpetta (1996) and Nickell (1997) with Baccaro and Rei (2005) and Bassanini and Duval (2006).

The widely accepted centrality of PLMIs for labor market performance might lead the unwary to believe that there would be some strong simple cross-country relationships between the two. Indeed, bivariate plots have frequently been employed in this literature to establish the connection between, for example, benefit duration and unemployment rates. But a closer look shows that, especially with the shift from the more subjective early institutional measures to the more carefully constructed OECD data that replaced them, such significant simple correlations do not show up in the data.

A large and increasingly sophisticated literature has employed measures of PLMIs in panel data models designed to explain the cross-country pattern of unemployment over time. While significant impacts for employment protection, benefit generosity, and union strength have been reported, the clear conclusion from our review of these studies is that the effects for the PLMIs are distinctly unrobust, with widely divergent coefficients and levels of significance. The one possible exception to this conclusion is the role played by unemployment benefit generosity, but rather remarkably, little attention has been paid in this case to the direction of causation. Common sense political economy considerations

and Granger test results both suggest that much of any statistical association runs from changes in unemployment to changes in benefit generosity.

The microeconomic evidence has frequently been cited as confirmation for the dominant macroeconomic findings. It is certainly not controversial that greater benefit generosity will tend to have effects on labor market behavior. The question is whether politically realistic changes in benefit generosity are likely to have sufficiently large effects on worker labor supply decisions and wage pressure to produce measurable (much less major) impacts on the aggregate unemployment rate. Our review of this evidence indicates surprisingly small effects on the duration of unemployment, even when “drastic” changes in benefit generosity are examined in recent “natural experiment” studies. In any case, reductions in generosity or tightening eligibility rules have no necessary effect on the overall unemployment rate since changes in benefit generosity may have offsetting effects via inflows into unemployment and employment (“composition” and “entitlement” effects).

It is increasingly recognized that labor market institutions and policies are interdependent and successful employment performance is likely to reflect coordinated reforms. Our final section looked at recent attempts to measure the impact of overall labor market reform on unemployment rates over the past decade or so. Again, we find that the positive conclusion in these studies reflects more the initial orthodox presumption of a strong positive relationship than the actual statistical evidence that is presented, particularly concerning the impacts of the key PLMIs.

Such lack of robustness may very well be an inevitable result of attempts to estimate economic relationships with poorly measured institutions and policies, imperfectly measured macroeconomic shocks and shifting economic structures, and small numbers of (country) observations. But a striking feature of this literature is the contrast between the fragility of the findings (both within and across studies) and the confidence with which it is often concluded from them that labor market rigidities are indeed at the root of poor employment performance.

Our review suggests that the theory-data-testing interplay mentioned above has been too unbalanced in this literature — orthodox theoretical priors overly determine the variable construction, empirical testing and interpretation of the results. A healthier dose of skepticism is required to give the data a chance to challenge orthodox views. This is particularly important for this area of study - employment performance - since policy recommendations grounded in empirical research have had (and should have) a powerful influence on policy making. National institutions and policies that affect the functioning of labor markets need to be regularly re-examined and adjusted to reflect changes in the economy, the labor force, and social norms. At the same time, institutional reforms that

demonstrably reduce the well being of many workers should be undertaken only with compelling evidence about the magnitudes of offsetting benefits.

¹ The 1979 and 1983 figures come from an OECD-based series received from IMF researchers (IMF 2003). The recent figures are taken from the OECD Employment Outlook 2005, Statistical Annex, Table A. OECD country membership has expanded greatly since the early 1980s and our reference to “OECD-Europe” refers to 19 European countries that appear in Table A.

² For example, “an exogenous increase in the generosity of benefits does not seem to have been a prime factor causing the rise in unemployment” (p. 594); “As well as failing to find any link between unionization and persistence (of unemployment)...” (p. 610).

³ 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.”

⁴ We thank Paul Swaim for this point.

⁵ We thank Paul Swaim of the OECD for making these data available to us.

⁶ These differences largely reflect different adjustments made to account for both the shift from registered to household data between 1982-3 and another major break in the series in 1987, but the IMF data also show a significantly lower rate for 2003.

⁷ In the OECD’s LFS series (OECD 2004, Appendix: Notes by Country), unemployment rates are generated from administration-based sources prior to 1984 for Germany, prior to 1983 for The Netherlands, and prior to 1986 for New Zealand. For Belgium, the entire unemployment series is based on registration data.

⁸ 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 scatter plot.

⁹ The change in the net replacement rate comes from Chapter 3 of the OECD Employment Outlook (OECD, 2006, table 3.2). This NRR is measured as the average over 60 months of unemployment. The change in NRR is from 1995-2004; the change in GRR is from 1995-2003 (the most recent available).

¹⁰ It is not clear why this second test (for just 14 countries for 1987-93) was even run, except for the sake of symmetry, since the unemployment data were available for this more recent time period.

¹¹ 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.

¹² 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.

¹³ 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.

¹⁴ 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.

¹⁵ 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.

¹⁶ 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.

¹⁷ 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, models 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).

¹⁸ 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.

¹⁹ For example, Nickell (2003) allocates more “good” policy changes to the UK (6 ticks) than to any other country, getting credit for reducing the replacement rate (1 tick), increasing benefit system strictness (1 tick), reducing union coverage (2 ticks) and union density (1 tick), and for reducing labor taxes (1 tick).

²⁰ The only variable which is significant with the correct sign is the price and tax wedge in the wage equation. The interpretation we advance for this general finding on the supply-side variables is that when other variables which have figured in the debate on the determinants of medium term unemployment are also used, they regularly outperform the supply-side terms” (Henry, 2004, p. 22).

²¹ 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.

²² 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 1) what is being tightened are the links between benefit receipt and participation in active labor market programs, and 2) the key reform initiative is the improvement and expansion of ALMPs. 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.

²³ OECD standardized unemployment rates, measured as a share of the labor force (OECD, *Employment Outlook 2005*, statistical annex, table1); OECD unemployment benefit reciprocity rates, measured as a share of the working age population (OECD *Employment Outlook 2003*, Chapter 4, Table 4.A1.1)

²⁴ According to van Ours (2003), "The decline in replacement rate had a clear effect on unemployment." But before we can be confident about the real-world importance of this effect, we need an explanation for the stability of the much more widely used OECD measure (shown in Figure 9).

²⁵ 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).

²⁶ 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); or 2) what threshold should determine a cross or a tick for each of the 9 measures; or 3) the poor quality of some of the measures (e.g., there is in fact no usable cross-country measure of the strictness of eligibility rules for unemployment insurance, much less one that would show how this strictness has changed over time).

²⁷ 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 percent to 10 percent, 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 percent of the workforce.

Finally, Nickell's criterion for a cross on EPL is a rise of *more* than .1; it just happens that France's score increases by exactly .1 (from 1.3 to 1.4).

²⁸ 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 percent) would have a greater payoff to employment performance than the implementation of just 2 by Australia (50 percent).

²⁹ As Manning puts it, "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" (1998, p. 143).

³⁰ Similarly, Blaug quotes Robert Solow as saying that "Economists don't ask themselves – and I think this is the worst sin of them all – whether there doesn't exist a different model that would fit the data equally well, and what does that tell me? So I think that the problem with economists is that they do too much uncritical empirical work, and that they deceive themselves with the refinements of their methods" (p. 242).

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