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Time-Variant Institutions: Implications for European Unemployment

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Summary

The upward trend of European unemployment begs many questions, the most basic of which is why unemployment continues to climb after twenty-five years. Adverse shocks, rigid labor market institutions, and their interaction are used to explain this persistence and the differences in individual country experiences.

While these models do indeed answer both questions to some extent, they assume that institutions predate the rise in unemployment, often treating them as static. By compiling extant data series and constructing my own, I find that this assumption is weak, and that the evolution of institutions is far from static.

I create and estimate a new dynamic panel of 20 OECD countries with three empirical specifications, and uncover several significant results. First, the tax burden on labor appears to have a very strong and positive relationship with unemployment rates, implying that the tax structures of a country are an important indirect mechanism driving joblessness. Second, the unionization level shows a significant amplification effect; that is, high union density exacerbates adverse shocks to employment. Third, restrictions on firm hiring and firing seem to have the opposite effect – more protection reduces the impact of shocks. I also find evidence that both union/employer coordination and benefit duration affect unemployment rates significantly.

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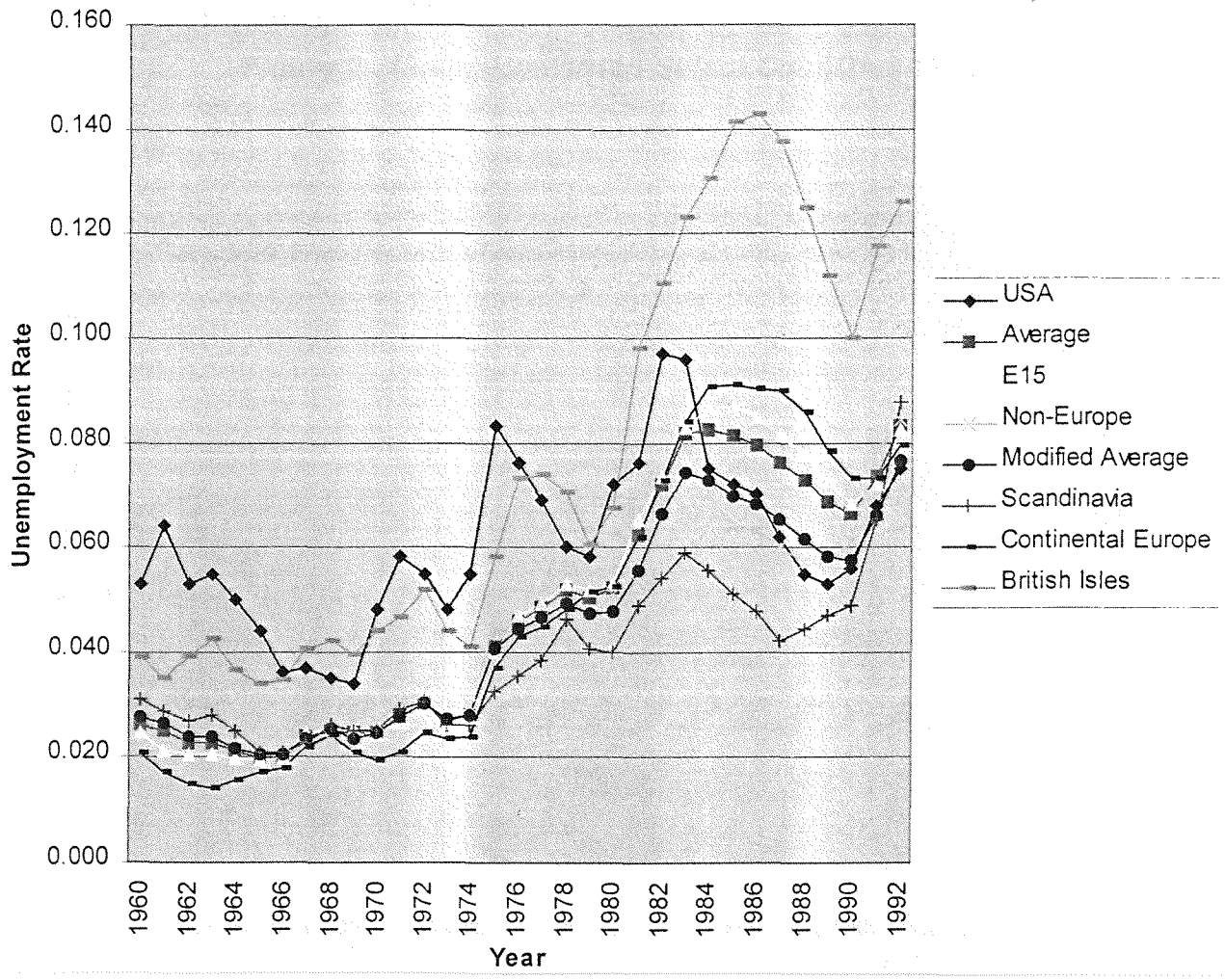
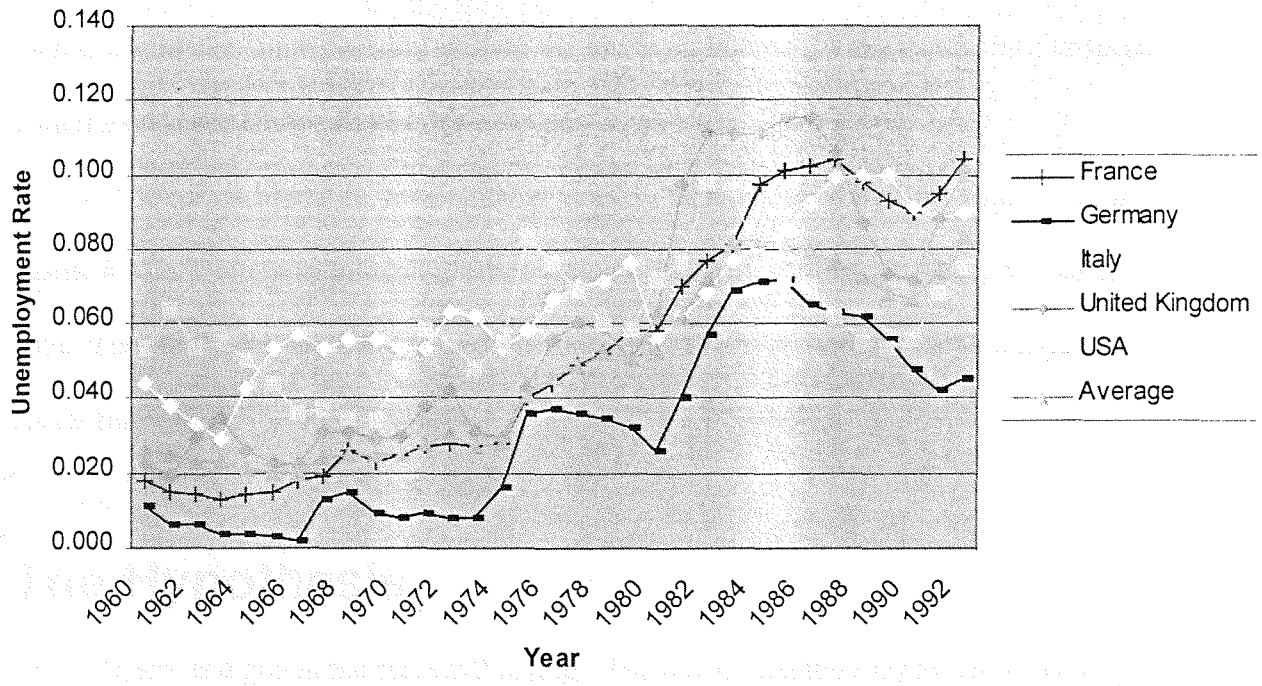
In the late 1960s, rates of unemployment in Europe lay below three percent. After the first oil shock of the 1970s, these rates rose dramatically, hitting 11 percent in 1985 before dipping back to eight percent by 1990. The United States likewise saw its unemployment rate rise in the wake of the 1960s productivity boom, but it recovered after 1992, and indeed has created concern that unemployment is too low. Meanwhile Europe's number of unemployed has continued to grow through the 1990s. ***Figure 1*** and ***Figure 2*** shows broad trends in unemployment rates before 1992.

Figure 1. The Evolution of Unemployment 1960-1992



Conventional wisdom ascribes this disparity to an idea relatively common to the macroeconomic analysis of labor markets. Disturbances in market equilibrium are caused by an exogenous *shock* that moves the unemployment level either up or down. Since the shock moves the market out of the long-run equilibrium, the effect will eventually die out. However, certain factors may either prolong or shorten the period in which the market is out of equilibrium, defining the *persistence* or *propagation* of the shock. Therefore, even if the initial shock is small, strong rigidities in the market may prevent a return to equilibrium for a long time. Likewise, a lubricated market will often return to normal quickly, even after an enormous shock.

Figure 2. Unemployment in Select Countries 1960-1992



Analysis of unemployment differences often takes this tact. Shocks are seen as moving the market out of equilibrium, and the inherent rigidities of labor market institutions are

blamed for the lengthy persistence. Institutions are then seen as the culprit in explaining cross-country differences.

Early explanations for the unemployment experienced by Europe and the United States focused on the two oil price shocks and subsequent disequilibria in labor markets. These were confounded when the turbulence resurfaced in Europe in the early 1990s, with the unemployment rate again moving above ten percent. To further complicate the picture, unemployment rates in comparably wealthy countries, most notably in the United States, fell to remarkable lows. Building off of the theory of the past 20 years, new frameworks arose to explain the unemployment problem. These avenues moved beyond obvious shocks, searching for subtler influences and, perhaps more importantly, cultivating the idea that unemployment was largely a persistence effect of rigid labor market institutions. If such was indeed the case, then it would also quite possibly explain the unemployment differences among European countries and the United States.

The recent literature, most notably work by Steve Nickell, Olivier Blanchard, and Justin Wolfers, attempts to explain cross-country disparities using cross-section institutional data. The results largely indicate that unemployment rates are strongly affected by a country's social framework.

The Hypothesis

There is a gap in the current thinking. The recent literature notes that knowledge of the nature of institutional effects in labor markets is limited because the major data for institutions occurs largely in cross section. Little effort has been put into the construction of a time-variant panel for institutional variables. Thus, econometric specifications try to explain the role of

institutions in cross-country variation given static variables. For example, recent work by Nickell (1997) and Blanchard and Wolfers (1999) use institutional data that comes from an averaged five-year period in of 1989-1994.

This is problematic because the cross-section can hide important dynamic information. The use of static institutions essentially relegates them to the country effect, making them only useful in explaining cross-country heterogeneity of unemployment rates. If unemployment institutions really do create a persistence effect, then the cross-section is insufficient information. The measure may very well lack the components that might be necessary to trace the dynamic adjustment path of unemployment. That is, cross-section data fails to capture the full nature of institutions, and thus the full nature of any persistence effect. Moreover, static institutions allow for no change in the legislative structure, expressly prohibiting the institution itself from acting as a shock (i.e. a drastic change in a short amount of time).

The hypothesis is then a simple one, originally proposed by Blanchard and Wolfers (1999) and fully implemented by this analysis. What if institutions are allowed to vary over time? That is, can time-variant effects in institutions help explain the differences in unemployment between Europe and the United States? Current explanations of the differences, based on static institutions, ignore three potential causes of difference.

- Institutional change itself could be a shock to the labor market. For example, a sudden reduction in benefit duration (i.e. the length of time for which it granted unemployment benefits) could kick long-term unemployed (who years ago exited the labor market) into the job market, thus increasing unemployment measures.
- The variance of institutions could better explain persistence. Timing is crucial in determining whether or not the variables are really the keystone. If Germany's benefit

duration was increased ten years after total factor productivity began to decline, the persistence effect should be smaller then if the increase occurred after only a year.

- Institutional change has a direct bearing on the initial impact of exogenous shocks. If the benefit duration is lengthened to compensate for unemployment caused by a total factor productivity decline, then the unemployment shock will likely be worsened.

The Contribution

My contribution exists on two levels: I create a new panel of time-variant institutional data and I find that the fresh data offer several surprising insights as to the relationship of institutions and the unemployment rate.

I compile a dynamic panel for 20 OECD countries over 33 years, constructing data series from primary sources and culling other series from complete and partial data sets collected by other economists, political scientists, and sociologists. An examination of the data and sample statistics indicates that institutions are not static, but rather show significant variation over time. This runs contrary to past analyses, which often simply assumed that recent institutional measures were a good proxy for past ones.

I use the new panel to estimate a series of specifications aimed at uncovering whether allowing institutional time variation yields new evidence for the nature of the relationship between labor institutions and unemployment rates. The first test is a simple regression to dissect the general nature of the association. Then I look at two separate specifications to examine the difference between institutions which directly affect unemployment (termed *additive* effects) and institutions which indirectly exacerbate adverse shocks to the

unemployment rate (termed *amplification* effects). Finally, I combine the two effects into a single equation.

Table 1 compares this methodology to recent analyses, Nickell's 1997 survey paper, which summed up the present thinking of institutional effects, and Blanchard and Wolfers' 1999 publication, which pioneered the empirical testing of institution/shock interaction.

I uncover strong evidence for several propositions regarding the implications of time-variant institutions.

- The tax burden on labor (often called the tax wedge) has a large and positive correlation with unemployment. This effect appears to be additive, meaning that the tax wedge does not act through a shock mechanism, but is rather associated with a direct influence.
- The level of union density in a country is important in determining the magnitude of an unemployment shock. Countries with a high density are inclined to also see shocks persist for much longer.
- How much a country legislates firm hiring and firing (termed employment protection) is likewise a significant factor in the persistence mechanism, albeit in the opposite direction. High protection seems to dampen the duration of a shock, leading a country to recover more quickly.

Other institutions show weaker evidence of a relationship with unemployment rates, with some ambiguity clouding the results.

- The coordination of unions and employers (referred to simply as coordination) evinces a positive correlation with unemployment rates. The greater the insularity of the bargaining process, the greater the unemployment rate.

However, it is unclear whether this effect occurs as essentially additive, or if it acts as a shock "megaphone".

- There is weak evidence that benefit duration moves positively with unemployment rates, but the correlation is also ambiguous as to whether the association is one of addition or amplification.

These results speak to the strength of this analysis compared to the others outlined in ***Table 1***. Previous work using static institutions fails to capture the time effects; that is, whether institutional variation is itself a shock, whether changes in institutions change the persistence effect, etc. Therefore, the best approximation of the unemployment/institution relationship is altered due to missing data. By incorporating most of the missing series, this paper shows that several institutional effects are quite larger than previous work by Nickell (1997) or Blanchard and Wolfers (1999) made apparent – namely the enormous effects of the tax wedge, unemployment protection, and labor union density. Moreover, introduction of the time-variant data downplays the importance of some prior results in the same papers – in this case, the power of benefit duration. Finally, earlier evidence of a negative correlation between coordination and unemployment is repudiated.

In another departure from previous analyses, this paper estimates each specification with a basic firewall against the endogeneity often argued in labor market institutions' relation with unemployment. The estimations are also unique to prior work in that they attempt to minimize issues of serial and spatial correlation.

The Literature

The extant literature can be dissected into three distinct episodes: initial explanations that relied on the commodity shocks of the 1970s, subsequent ideas about the effects of labor market institutions and other rigidities, and the present analysis that combines new conceptualizations of shocks with the persistence effects of institutions.

Initial Explanations – Shock Analysis

Sharp increases in unemployment impacted the European continent as a direct consequence of the recession induced by the 1972-73 commodity price explosion. Searching for the unexpected crisis' explanation, many economists seized on the relative oil price increase, gradually developing it into a broader decreased productivity analysis. In addition, research also turned to the role of aggregate demand, an aspect that has lost some significance in the overall story. Prodded by the frenetic work on institutions of the 1990s, some of the literature is returning to the shock concept, proposing alternative reasons for the initial unemployment episode.

Relative Price Shocks

The rise in commodity prices caused a general decline in terms of trade, which in turn precipitated a slowdown in the growth of real wages. This drop-off occurs because if firms are to maintain the same profit levels in the face of increased relative prices for raw materials (a decline in factor productivity), they must slow wage growth. If for some reason firms lag behind in assimilating this new information, wages increase too quickly, sparking

that the fall in productivity was responsible for the majority of the increase. Besides sharing the same mechanism for increasing unemployment as a rise in relative prices, a drop-off in productivity also ensures lower future wages. Work by Manning (1992) argues that if it takes time to find a job, then the lower rates in the future will likewise decrease the opportunity cost of being unemployed.[3] With this in mind, wage negotiations will demand a higher wage presently, resulting in increased unemployment.

Aggregate Demand Shocks

When inflation is low or middling, decreasing inflation will often imply an unemployment rate above equilibrium; increasing inflation indicates a rate below equilibrium. Blanchard (1999) notes that the change in inflation has been negative since the 1970s, reflecting an equilibrium above the actual unemployment rate. He argues that this is because macroeconomic policy delayed a portion of the unemployment increase from the 1970s to the 1980s. Modigliani, *et al* (1998) note that since evidence points to a short-run Phillips curve that is flatter at lower inflation, small declines in inflation (as, for example, in the 1990s) would result in large splits between actual and equilibrium unemployment. This then implies a sizable shortfall in aggregate demand.

A disflationary process, as in the first half of the 1980s, will also result in unemployment if there is nominal inertia in wages/prices or if the effects are unexpected. Bruno and Sachs (1985) find that there is substantial nominal wage inertia for North America and little to none for Europe. Grubb, Jackman, and Layard (1983) find a similar result. While

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[2] It is also interesting to note that the non-EC countries experienced a decline of only 2.2 percent, possibly indicating a reason for intra-Europe differences. Bean (1994) summarizes this analysis.

[3] The idea being that the present discounted value of the wages is lower than if the productivity decline has not occurred.

these results are oft disputed, no study has yet shown that, all things equal, Europe contains a relatively high level of nominal wage inertia. Therefore, most research tends to discount the role of the disflationary process in European unemployment.

Additional Shocks

Other shocks have been examined more recently. Blanchard (1999) argues that the lower real interest rates in the 1970s suppressed the full effects of the productivity shocks, leaving Europe open to a larger effect when interest rates rose in the late 1970s and early 1980s.

Blanchard (1997, 1998) also makes the controversial proposition that a decline in labor hoarding has increased the marginal product of labor relative to wages, meaning that the increase in capital share that Europe has seen is permanent. This implies that labor demand fell over the past decade. Cabellero and Hammour (1997) offer opposing analysis, claiming that the shares will return to their previous levels in the long run.

Further Explanations – Static Institutions and Rigidities

With the oil shocks of the 1970s long past, Europe again experienced a relentless rise in unemployment rates. Having already noted several problems with early attempts, economists began to focus on the relationship between institutions and unemployment. Initial analyses of institutions concentrated on how institutions affected the impact of shocks. For example, Bruno and Sachs (1985), concentrated heavily on the nature of collective bargaining and how it altered the response to shocks. Similar goals can be found in Calmfors (1994), a paper representative of a body that focused on the structure and coordination of bargaining. Taylor

(1980, 1998) initiated a wealth of debate as to whether staggered bargaining can make disinflation more difficult and prolong the effect on unemployment from unanticipated deflations.

As European unemployment continued unabated into the 1990s, research turned from the qualitative dimension to the time aspect. More attention was brought to how labor market rigidities could cause a shock to persist for far longer than it would have in the absence of such institutions. If those bodies changed in the past couple of decades, they could potentially have drastically altered the reaction of Europe to shocks. While a number of institutions in Europe are roughly the same as they were in 1970, the mere presence of a social safety net could potentially affect the duration of an unemployment spell. Lindbeck (1995) noted that it often takes a substantial amount of time for people to use the social institutions they're entitled to, meaning that even if an institution hasn't changed, it can still significantly affect the path of unemployment.

A storm of proposals followed in the wake of the unemployment resurgence of 1990s, with everything from the minimum wage to housing as an indication of labor mobility being considered as a potential persistence mechanism. Nickell (1997) distilled the debate down to eight institutions: the tax wedge, active labor market policy, employment protection, the benefit replacement rate, benefit duration, union density, union coverage, and employer/union coordination. I discuss each of these in turn.

The Tax Wedge

The concept of the tax wedge, or the tax burden on labor, straddles the distinction of shock and institution. The term "wedge" refers to the way in which taxes insert a difference

between the labor costs for firms (producer wages) and the net income for workers (consumption wages). A tax increase likewise broadens this gap, giving people less incentive to work as their consumption wage drops farther and farther below the producer wage. Indeed, firms also have a decreased incentive to hire workers, their wage being significantly higher than the worker wage they ultimately get. Wage pressures increase, causing a rise in unemployment.

Early work on tax rates predates the wave of institutional analysis, with much of the nomenclature treating taxes as a shock. That definition has since evolved, with tax rates now seen more as institutions. The OECD (1994) noted that the structure of the taxes (that is, the nature of the tax –consumption, payroll, etc.) is insignificant in determining the effect on unemployment. Many authors point to the tax wedge as a potential cause of unemployment in Europe, including Andersen (1988), Calmfors and Nymoen (1990), and Padoa Schioppa (1990). These earlier writings hypothesize that the increase of taxes in the 1970s and early 1980s resulted in a shock to employment via the wage mechanism. More recent work by Blanchard (1999) and Nickell (1997) includes the tax wedge as another institution, acting as a persistence factor.

Active Labor Market Policy[4]

Active labor market policy is usually thought of as the ratio of fiscal expenditures on unemployment programs, job skills development, job search assistance, etc. to the number of unemployed. This results in a measure of active labor market assets per unemployed person, and has been measured in recent years by the OECD's Employment Outlook 1995.

[4] The OECD defines a country's active labor market policy in five parts: entrepreneurship assistance, job search aid, wage subsidies for regular employment, job training, and the creation of temporary jobs.

Presumably, the more money a government is actively spending on its unemployed (that is, not just keeping meeting basic needs with benefits), the easier a job transition is and the smaller the persistence of any shock.

Studies of active labor market policies largely exist on a micro level, as seen in a survey by the OECD (1993). Relatively little work exists on the macroeconomic side, with most authors concluding that any policy effect is ambiguous at best (see for example Holmlund and Linden, 1993, or Jackman, 1994). Calmors and Forslund (1991) and Calmfors and Lang (1995) showed that a macro active labor policy is not linked to the results of micro studies; indeed, programs may be offset by rising wage pressures or substitution between groups of unemployed. Richardson (1997) attempted to eliminate this ambiguity, modeling the policy as a transition rather than a state. Surprisingly, he found that active labor market policy could potentially reduce the equilibrium unemployment level.

Employment Protection

Employment protection is an index of the constraints placed on individual firm's hiring and firing. As employment protection increases, turnover declines but unemployment duration increases. The ultimate effect on the unemployment rate is the subject of some debate.

Lazear (1990) compiled differences in regulations of advance notice and severance, producing panel estimations that revealed a positive correlation between such protections and unemployment. The OECD (1999) constructed a baseline measure of current protection in Europe and the United States. Building off of the OECD work, Blanchard and Wolfers (1999) propose a fusion of the OECD numbers and earlier work, developing a rough contiguous series for employment protection. Nickell (1997) also finds a positive relationship between

unemployment and employment protection. Hopenhayn and Rogerson (1993) and Mortenson and Pissardes (1997) evaluate other models of employment protection and unemployment.

However, work by Bertola (1990) indicates that employment protection provisions have no significant empirical correlation with long-run unemployment levels. Bertola and Rogerson (1996) postulate that this may mean that Lazear's findings were actually reverse causation.

Benefit Replacement Rate and Benefit Duration[5]

The benefit replacement rate is measured as the amount of pre-tax unemployment benefits and assistance divided by the pre-tax wage. Economic theory indicates that countries with higher replacement rates will see shocks persist longer. As workers are knocked out of jobs because of the shock, they have less incentive to seek new jobs if their compensation is close to what their wage was.

Benefit duration is simply the number of years for which unemployment benefits are available. Like the replacement rate, the duration of benefits will also have a negative persistence effect. The longer unemployed workers can draw benefits, the less their incentive to seek a new position quickly. This may then in turn exacerbate unemployment further, as workers who wait to get new jobs find their skills outdated and their positions usurped by newer laborers.

The OECD (1994) constructed a series for the average replacement rate dating back to 1960. Blanchard and Wolfers (1999) expand on this work, creating series both for the first year of benefits and for an average of the second to fifth years. They find a positive correlation

[5] Benefits are the direct remittance a worker receives while unemployed.

between benefits (both replacement rate and benefit duration), as do Nickell (1997) and Layard, *et al* (1991).

Mortenson (1986) gives a comprehensive overview of a vast microeconometric literature that positively links unemployment benefits and unemployment duration.[6] However, many studies (for example Kieffer and Neumann, 1989) offer evidence that this effect is rather small, to the point of being negligible. The macroeconomic evidence is likewise small – Adams and Coe (1990) find that a one percent increase in the replacement rate of the United States raises the Non-Accelerating Inflation Rate of Unemployment (NAIRU) by only .05 percent. Nickell (1997) and Blanchard and Wolfers (1999) find a significant, moderately sized positive effect of benefit duration on unemployment rates.

Union Coverage and Density[7]

Economic theory indicates that increased union coverage or membership will make the labor market more rigid, slowing response to shocks. Outside of the early work on union bargaining, research has focused on the extent to which a work force is given the power to bargain for wages. Again, Bruno and Sachs (1985) offer some of the original analysis, but subsequent studies by Nickell (1997) and Layard, *et al* (1991) reflect a positive relationship between coverage and unemployment. Additional evidence indicating the same result comes from OECD (1997).

[6] These models form the foundation for labor search theory, a robust literature in and of itself. Certain elements of search models are relevant to cross-country analysis, particularly in the case of labor-mobile regions. However, this is outside the bounds of this inspection.

[7] Union density refers to union membership divided by active labor force or some similar measure. Union coverage indices attempt to capture the largely European phenomena of union negotiations that cover an often sizable non-union proportion of the population.

Lindbeck and Snower (1988) propose that unions raise unemployment by creating a cadre of insiders. These insiders essentially form a non-competitive market in the economy, one in which the unemployed are not allowed to bid down wages. These outsiders stay unemployed for longer due to the higher wages. Consistent evidence (for example, Fuchs, Krueger, and Poterba,1988) confirms this result, implying that higher union density keeps wages artificially high and therefore keep unemployment above equilibrium.

Coordination[8]

On one hand, economic theory indicates that the more coordinated union/management negotiation is, the quicker and more responsive any contracted wage will be adjusted. If many firms are individually negotiating with many workers, there is an incentive to try to one-up the competition, thereby resulting in a slower actual response to shocks. However, coordination may exacerbate the problem of the insider/outsider disjunction outlined by Lindbeck and Snower (1988). The higher coordination may actually increase the grip of the insiders on wages, making it even more difficult for outsider unemployed to get jobs.

Early results by Calmfors and Driffill (1988) indicated that the adverse impact of the oil price shocks were offset more in countries with higher coordination. Layard, *et al* (1991) construct an index of coordination for both sides of the bargaining table. Coordination is found to have a negative relationship with unemployment, a result later confirmed by Nickell (1997), Layard and Nickell (1997), and Blanchard (1999). The ability of coordination to offset the negative impact of unions is also noted earlier, most notably in Calmfors (1994).

[8] Coordination refers to the ability of employers or unions to move together, thus avoiding overselling or undercutting that distorts the economy.

Other Institutions

Many other institutions are thought to affect unemployment in some way, but empirical analysis of most finds them to be insignificant. However, two of these factors are worth more explanation: labor mobility and the minimum wage.

Oswald (1996) finds a positive correlation between house ownership and unemployment. He conjectures that people are less willing to move when they own a house, thus ultimately making labor more immobile and increasing unemployment. While results are preliminary, empirical analysis of Oswald's proposition is tenuous at best, largely because of the unstable theoretical grounds on which it rests. Oswald essentially postulates that home ownership is a proxy for labor immobility, an ephemeral concept affected by numerous other factors. Notably, the United States continues to see a high incidence of home ownership and labor mobility, both relative to European countries. Not only are these two facts contradictory in a proxy relationship, but the high ownership also would indicate a slower response to shocks than more "mobile" European workers. While Oswald's conjecture is potentially useful, the results are considered too preliminary for the institutional analysis this paper is concerned with.

Minimum wage is likewise problematic. While the economic theory is solid, the tumultuous nature of such articles as Card and Krueger (1995) reveals little or no consensus as to the effects of the minimum wage on unemployment. Moreover, there is an enormous endogeneity problem with minimum wage – is it artificial, or is it merely the government setting the market-determined minimum as law?

Present Explanations – Shock and Persistence

The most recent analyses concentrate on the interaction between economic shocks and institutional rigidities. The earliest discussion of such interaction is found in Bruno and Sachs (1985). Layard, *et al* (1991) construct some preliminary methods for analyzing such a proposal, but Blanchard (1999) systematically works through the various proposals of shocks and institutions, finally concluding that the best way to explain European unemployment is to develop a synthesized model that describes the interactions between the two. Whereas there has been an enormous amount of research on both institutions and shocks, no one has yet addressed what happens when the two are combined in a single arena.

Blanchard and Wolfers (1999) take the first steps on this path, constructing some simple regressions to determine whether such analysis is really worthy of further inspection. Ultimately, they conclude that the exploration of interactions is potentially extremely lucrative for describing the European problem.

Blanchard and Wolfers are missing two keys to truly developing a way to explain unemployment via shock/institution interaction. First, their model of interactions is admittedly primitive, descriptive rather than structural. This is more a symptom of the fledgling nature of the field than anything else. Second, they construct time-variant series for only a couple of the institutional variables. It is the latter that I propose to build upon, as outlined in ***Table 1***.

To truly develop a model of interactions, the data must be as complete as possible. As the dynamics of the system increase (in this case by allowing shocks and institutions to affect each other in some capacity), all fluctuations can affect the ultimate outcome, and the failure to capture those changes is potentially grievous: the variance of institutions could better explain persistence; institutional change itself could be a shock; institutional change has a direct

bearing on the impact of exogenous shocks. If a change goes unseen, so do the effects of that shift on any shock or other institution. By improving the data, I hope to extend this relatively new trend in empirical unemployment analysis and possible shed new light on the importance of institutions in evaluating cross-country differences.

Institutional Evolution:
The Case for Time-Variant Data

I compiled or constructed time-series for six of the eight major institutions: benefit replacement rate, benefit duration, employment protection, coordination, union density, and the tax wedge. In most cases, data are for twenty countries, identified by a three-letter country code: Australia (AUS), Austria (AUT), Belgium (BEL), Canada (CAN), Denmark (DEN), Finland (FIN), France (FRA), Germany (GER), Ireland (IRE), Italy (ITA), Japan (JPN), the Netherlands (NLD), Norway (NOR), New Zealand (NZL), Portugal (PRT), Spain (SPN), Sweden (SWE), Switzerland (SWZ), the United Kingdom (UKI), and the United States (USA). That is, the E15 countries and five control OECD countries: Australia, Canada, Japan, New Zealand, and the United States.

Benefit Replacement Rate[9]

In lieu of the OECD average replacement rate series, I use Blanchard and Wolfers' (1999) more refined measure of the maximum replacement rate to trace the evolution of benefit generosity since 1960, shown in ***Figure 3***.[10] This rate reflects the maximum ratio of pre-tax

[9] My choice of series and measurement issues are explained in the data appendix. A more detailed discussion of the OECD construction is located in Appendix 8A of the OECD Jobs Study (1994). Justin Wolfers retains notes on the Blanchard and Wolfers (1999) alternative.

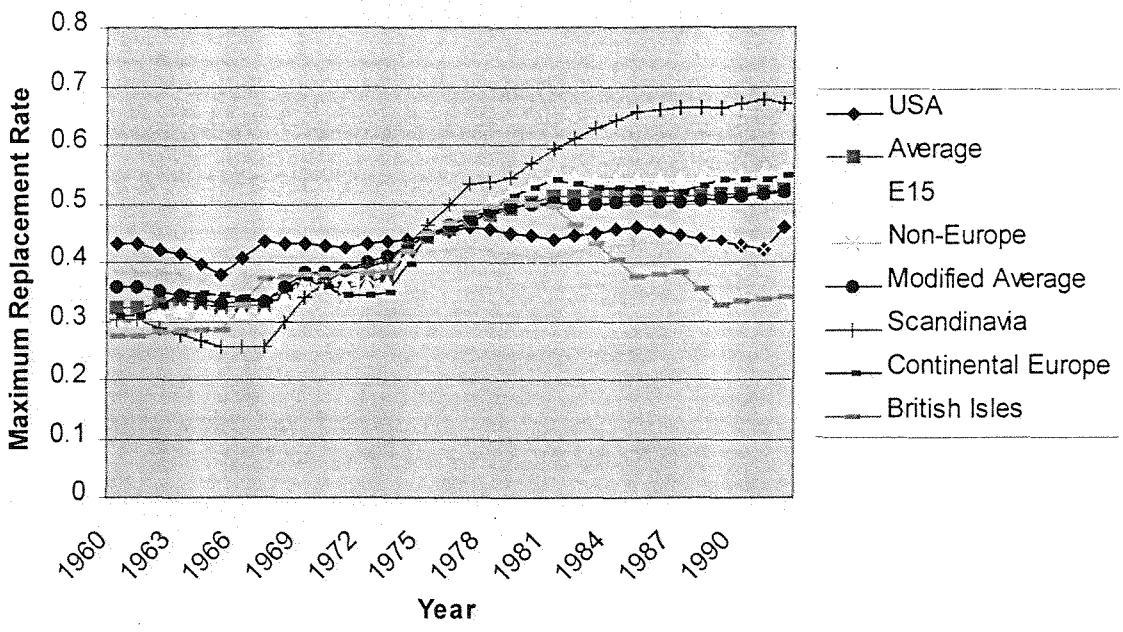
[10] Refer to the data appendix for notes on the constructions of these figures and averages.

unemployment benefits and assistance to pre-tax wage. This usually occurs in the first year of benefits, but sometimes appears in later years.

With the exception of the British Isles, most countries have seen an increase in generosity since 1960. Scandinavia jumped markedly in the 1970s and early 80s, while the rest of Europe saw a slower rise. The United Kingdom began a sudden and steady decline in 1980, eventually falling below even the fairly steady rates of the United States and non-European countries.

Perhaps surprisingly, the Netherlands reimburse their populace the most generously, follow by Spain and France. The rest of Europe hovers around a 50% reimbursement rate,

Figure 3. The Evolution of the Replacement Rate



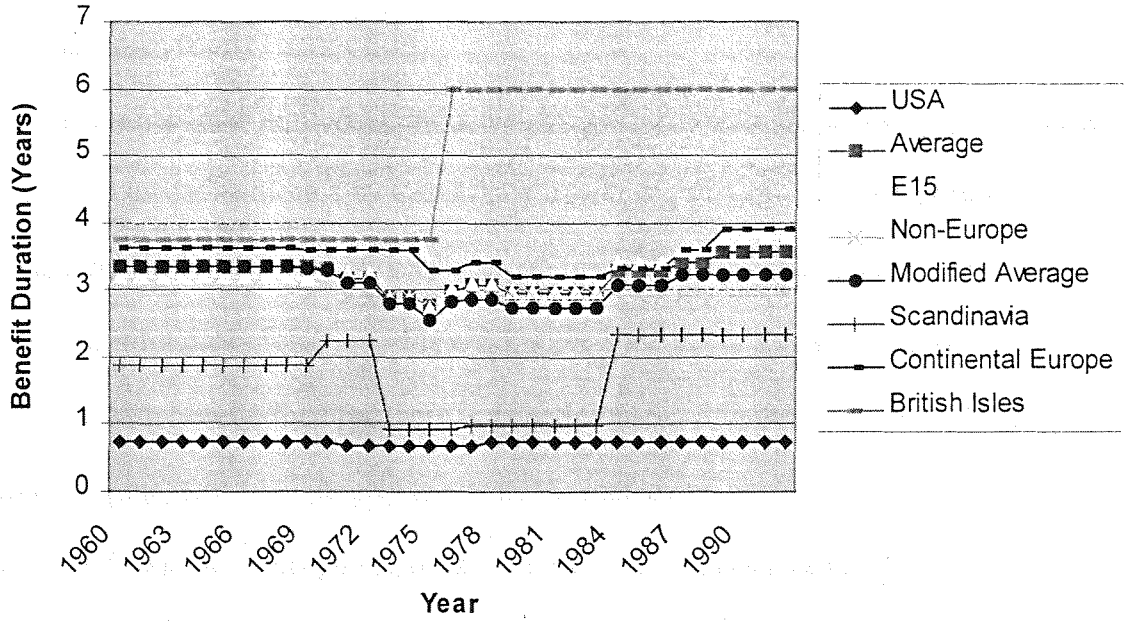
excepting Portugal and Italy. Most countries see about 10% variation, as seen in the standard deviations. High variation occurs in both Spain an Portugal, followed by many of the

Scandinavian countries. In contrast, the United States is relatively stable; out of Europe, only Germany's trend approximates this staidness. ***Table 2*** summarizes these sample statistics.

Benefit Duration

The duration series is constructed from data collected by the United States Social Security Administration and is entirely unique to this paper.[11] It captures the maximum duration of benefits in terms of years for an under-50 worker who is subject to means-tested assistance. A measurement of six years is the equivalent of unlimited assistance in the index scale. The evolution is captured in ***Figure 4***.[12]

Figure 4. The Evolution of Benefit Duration



The trend is much flatter than that for the replacement rate, with most countries remaining between three and four years. The United States is virtually constant, even more so

[11] The data appendix contains information on the construction and alternatives. Nunziata (2000) has developed a measure of duration base on OECD replacement rate series; my issues are addressed in the appendix.

than the replacement rate. The Scandinavian countries are below the average, an interesting juxtaposition to their higher-than-average replacement rates. Also diverging is the benefit duration in the British Isles. While the replacement rate is falling, the duration has stayed consistently high for about the last two decades.

The individual country data in ***table 3*** indicate that many European countries legislate unlimited benefit duration, largely as the result of means-tested assistance programs. The variance measures are likely inflated, given that most duration changes are subject to sudden changes in unemployment laws. This is exemplified by the high standard deviations of Sweden, Finland, and the UK, all of which saw major changes in benefit duration. The average variance remains fairly small, at about a quarter of a year.

Employment Protection

Blanchard and Wolfers (1999) construct a crude but usable employment protection series based on data collected by Lazear (1990) and the OECD.[13] The index is scaled {1, 4} and captures the level of limitations placed on firm hiring and firing; the higher the number, the more institutional protection. ***Figure 5*** reflects the index's general trends.[14]

As with benefit duration, there is a negligible trend in the data after 1975. A small drop-off begins after 1985, largely led by the Scandinavian countries, especially Sweden. Before 1975, with few exceptions, most countries experiences a gentle rise in employment protection. As with duration and the replacement rate, the United States experienced no trend

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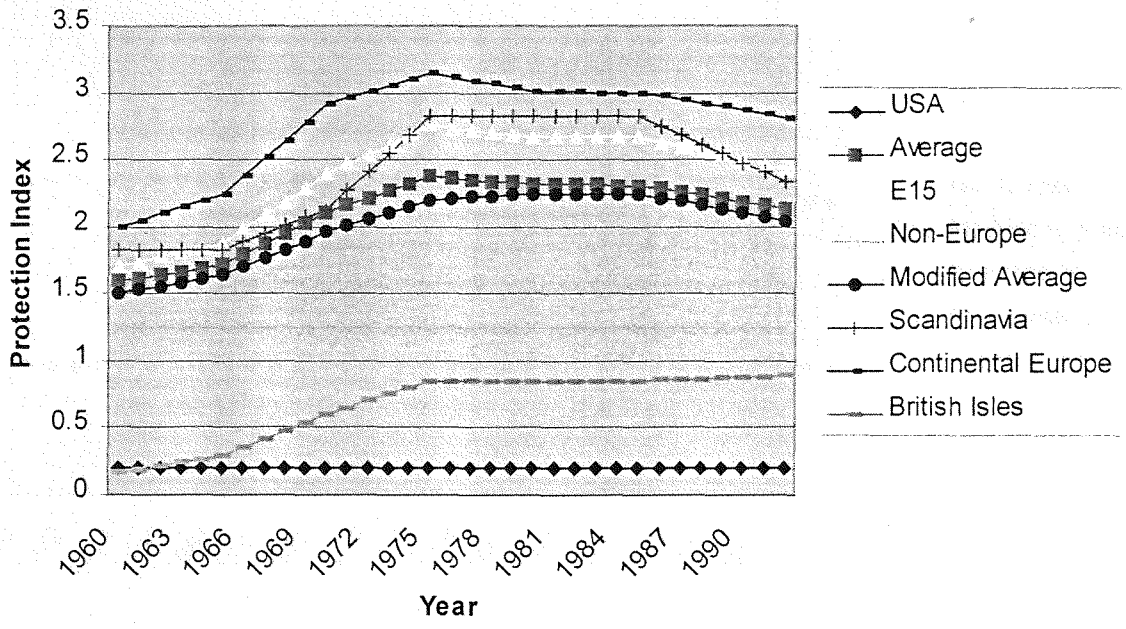
[12] Refer to the data appendix for notes on the constructions of these figures and averages.
[13] The OECD is working on a more comprehensive measure of employment protection, but the task is complicated by difficulties in comparing legislation across countries. Comments and changes in the data are noted in the appendix.
[14] Refer to the data appendix for notes on the constructions of these figures and averages.

in the amount of security granted to workers by government. Indeed, all of the sample countries outside of Europe have not significantly changed their protection.

Looking to ***Table 4***, the variation is mostly in Iberia and parts of Scandinavia, namely Sweden and Denmark. France and Germany also see a comparatively large deviation, although still significantly behind the swings experienced elsewhere. The Mediterranean countries – Portugal, Spain, and Italy – implement a much stricter protection scheme than their European counterparts.

Figure 5. The Evolution of Employment Protection



Coordination

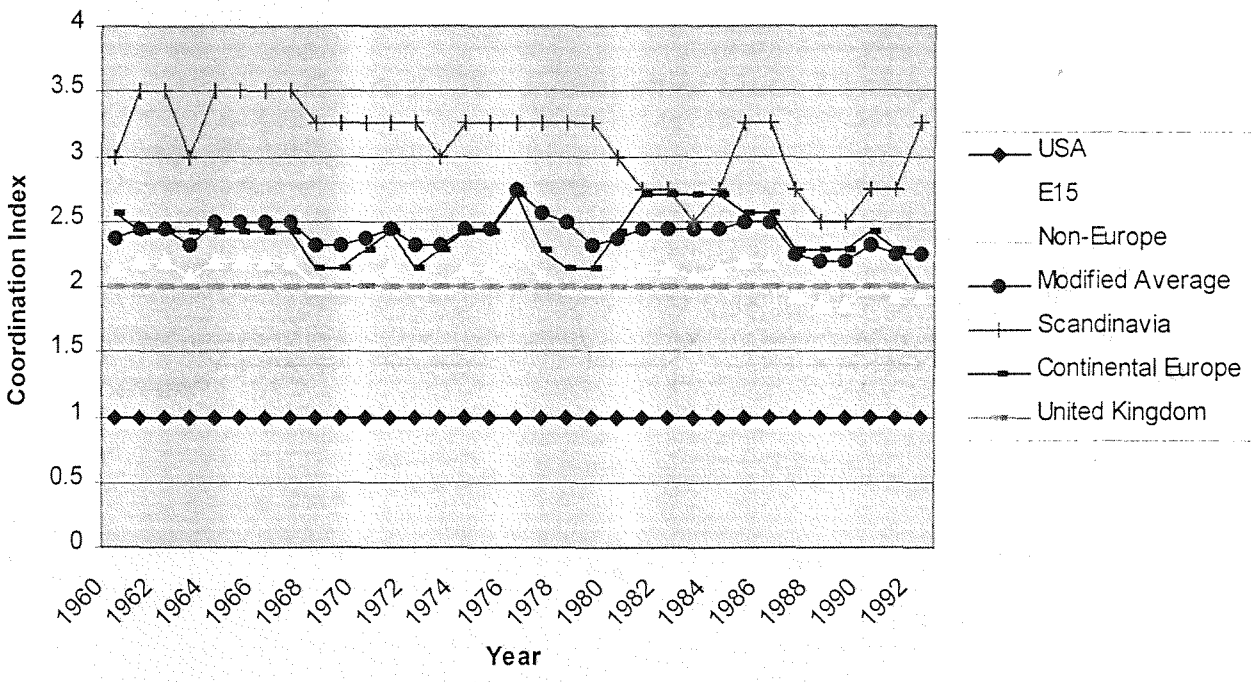
Coordination is measured by an index developed by Michael Wallerstein (1998).[15] It indexes the level of bargaining coordination on a {1, 4} scale, with the trends shown in ***Figure***

[15] The data are drawn from the set compiled by Golden, Wallerstein, and Lange (1998). See appendix.

6.[16] A larger number indicates a greater degree of coordination between employers and employees (usually in the form of unions).

The average and most countries are located between 2 and 2.5, indicating a moderate level of coordination. There is a sizably lower degree of coordination in the non-European countries, most notably in the United States. The United Kingdom also falls below the European average, with a constant coordination level of 2.

Figure 6. The Evolution of Coordination



Scandinavia again lies largely above the rest of Europe, with considerably more noise than the average. The exception is in the mid-1980s, when the path briefly drops.

In ***Table 5***, one can see that most of the variation is relegated to the continent, particularly Italy, Belgium, and the Netherlands. In contrast to other institutions, Sweden does

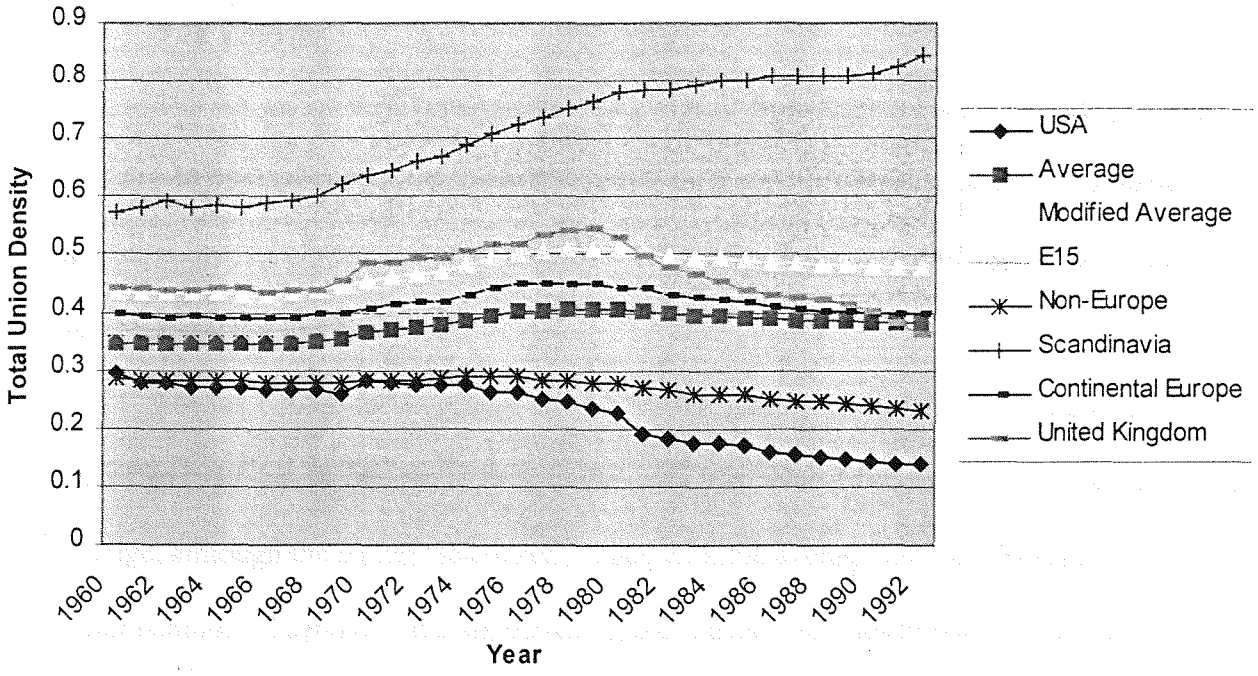
[16] Refer to the data appendix for notes on the constructions of these figures and averages.

not drive the Scandinavian variation, but rather Finland and Denmark do. Average variance remains small, with many countries experiencing no change in coordination over all 33 years.

Union Density

The measure of union density is derived from a database compiled by Jelle Visser (1996) to facilitate cross-country union comparison.[17] A total density measurement is used, which excludes self-employed but retains unemployed and retired members. ***Figure 7*** reveals the overall trend in union density.[18]

Figure 7. The Evolution of Union Density



The non-European countries and the United Kingdom have experienced a decline since the late 1970s, with a gentler decline realized in continental Europe. Despite the fall in

[17] The data are also drawn from the set compiled by Golden, Wallerstein, and Lange (1998). See appendix for a discussion of density measures.

density, European countries invariably retained a rate around 40%. Again Scandinavia is the exception, with unionization rates growing steeply from 1968, reaching almost 90% by the time the data end.

The Northern countries are also responsible for most of the variation in density. Sweden, Denmark, and Norway all see deviations from around 10%, matched only by Italy and the lowlands – Belgium and the Netherlands. Both Italy and Belgium experienced considerable upward unionization trends, while the Netherlands experienced a precipitous decrease. ***Table 6*** also shows the overall variation to be smaller, 2.8% across the entire sample.

Tax Wedge

The tax wedge series is constructed by myself from national accounts data from the OECD and derivative statistics from the Center for Economic Performance.[19] The series uses worker taxes as a percentage of market price GDP as an indicator of the tax burden. The time trends are shown in ***Figure 8***.[20]

The tax burden for all groups rise across the sample. Scandinavia increases at a faster rate after 1970, but then begins a drop in the early 90s. Non-European countries are below the average, although the United States keeps pace with the average until the Reagan administration. Thereafter, the tax burden in the States continues to rise, albeit at a slower rate than the average. The British Isles also rise more slowly and contain considerably more static than the continent.

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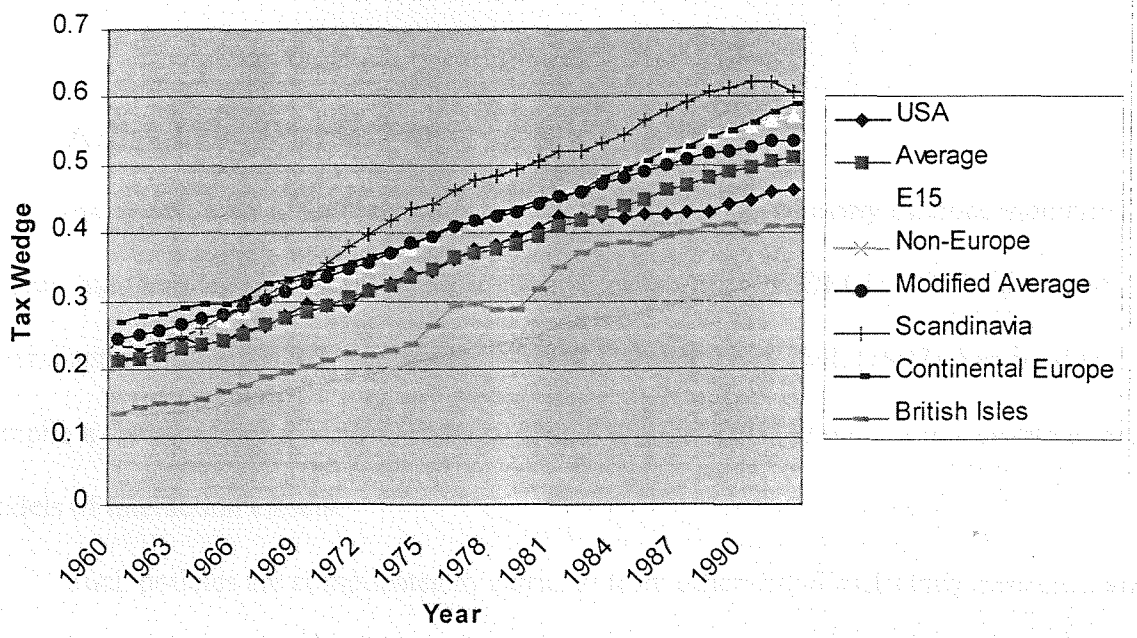
[18] Refer to the data appendix for notes on the constructions of these figures and averages.
[19] The construction of the series is outlined in the data appendix.
[20] Refer to the data appendix for notes on the constructions of these figures and averages.

Figure 8. The Evolution of the Tax Wedge



The variation is rather consistent, remaining around 10% for most countries. Finland, Sweden, and Iberia are slightly more chaotic, with around a 15% change for the entire period. There is also considerable difference in the means of Europe and the control countries, as evinced in ***Table 7.***

Union Coverage

Franz Traxler (1996) developed an index for union coverage in 1980 and 1990 by anecdotal conversations and data from separate country repositories. Richard Jackman, Steve Nickell, and Richard Layard (1991) construct a similar index for 1990 using fragments from a number of sources, including Bruno and Sachs (1985).

However, creating a time-variant series for union coverage remains difficult. Coverage by collectively bargained contracts is an ill-defined term, and although qualitative data are

useful in cross-section analysis, a rigorously constructed series for comparing across country and time is beyond the scope of this analysis.[21]

Active Labor Market Policy

Active policies to increase employment are the subject of many microeconometric studies, but there are few extant macroeconomic measures. The OECD (1995) measured several active initiatives as a percentage of GDP, data which Nickell (1997) then used to compile a cross-section measure. Blanchard and Wolfers (1999) then use this estimate in their models of interaction effects.

Such policies are complicated to extricate from other social assistance measures, and the definition of active labor market policy is ephemeral at best. Especially in European countries, pilot programs are started and eliminated after a few years, making small but significant variations that elude a complete definition. Creating a time-series across 33 years and 20 countries is indeed a massive task, and one outside the bounds of this paper.

Specification of the Model

The most troubling aspect of institutional analysis lies in the lack of empirical framework. Many theoretical models and their empirical implementations exist for a single institution, but there are no such couplings when multiple institutions are regressed on unemployment rates. Indeed, this gap in the literature makes any attempt to capture the effects of time-variant institutions a largely descriptive one. Careful evaluation of the data hints at

[21] Nunizata (2000) has built a data set back to 1960 that relies on single-year 1980 OECD estimates. I view this series as too much conjecture for robust analysis, but the estimates shed some light on the potential path of union coverage. A brief discussion is included in the data appendix.

interactions, as does narrative political analysis, but there is little in the way of technical and rigorous economic precedents. Therefore, the development of an empirical specification carries with it a heavier weight than a simple expansion of existing economic groundwork. Any analysis that moves beyond the descriptive is venturing into new theoretical groundwork as well as fresh empirical insights.

Past Models

The most simple model links unemployment institutions directly to unemployment rates in a linear regression. Nickell (1997) uses this technique to sketch the broad implications of institutions on unemployment rates.

$$u_i = \Sigma b_j X_{ij} + e_i$$

where i is a country index, j is an institution index, u is the unemployment rate, and X is the vector of institutions. Nickell actually regresses on log unemployment for ease of interpretation, using the econometric technique of random effects generalized least squares.

Another simple descriptive technique is used by Blanchard and Wolfers (1999) in their attempt to conjoin shocks and institutions in accounting for unemployment differences. They use two empirical specifications, one in which countries share common shocks and another in which shocks are allowed to vary across countries.

The first equation, in which shocks are held constant across countries, is defined in simple regression form as:

$$u_{it} = c_i + d_t (1 + \Sigma_j b_j X_{ij}) + e_{it}$$

where t is a period index (five years in Blanchard's work), i is a country index, j is an institution index, u is the unemployment rate, c is the country effect, and d is the time effect for

period t (that is, the common shock). Note that while the model does not incorporate time-variation in institutions, it can readily be added by making X (the vector of institutions) depend on time t.

A similar model can be specified for shocks that vary across countries.

$$u_{it} = c_i + (\Sigma_k a_k Y_{kit})\ (1 + \Sigma_j b_j X_{ij}) + e_{it}$$

where the notation is identical to the previous expression, except for the addition of Y shock vector (k is the shock index) for each country in place for the constant d. Again, the simple addition of the t index on X would make the model readily usable for time-variant institutional analysis.

In addition to these basic descriptive models, several general macroeconomic models adapt to some degree. One such example is based on the short-run supply curve, or Phillips curve. However these models tell us no more about the interaction of institutions and unemployment than the descriptive ones, and indeed may confuse the final interpretation. In the absence of a good theoretical framework, I opt for simplicity.

Specifications

I begin with a simple descriptive specification relating unemployment to a set of institutions. Clearly, this method does not result from a well-defined theoretical methodology, but rather provides a simple technique for evaluating whether time-variant institutions better explain unemployment trends. Time and country dummy variables are introduced to capture the panel effects. Lagged unemployment is included to account for long-term steady state adjustment and hedge against serial correlation.

$$u_{tj} = \Sigma c_j + \Sigma d_t + \Sigma \beta_i X_{itj} + {}_{l=1}\Sigma^4\ \gamma_l u_{t-l} + e_{tj}$$

where t is the time index, j is the country index, i is the institution index, u is the unemployment rate, c is the country effect, d is the time effect, X is the vector of institutions, and u_{t-l} is lagged unemployment by l years.

To simplify the estimation, I subtract out country means, effectively eliminating the individual effects. The equation becomes:

(1) $$u_t = \Sigma d_t + \Sigma \beta_i X_{it} + {}_{l=1}\Sigma^4 \gamma_l u_{t-l} + e_t$$

where the notation is identical to the former equation.[22]

The second specification is an emulation of the common-shock model proposed by Blanchard. While also not rooted in a tightly-specified theory of institutional interaction, it generally suggests the amplification effects of institutions when interacted with common shocks. That is, it can tell us something about whether an institution is associated with a reduced or enhanced shock.

(2) $$u_t = \Sigma d_t (1 + \Sigma \beta_i X_{it}) + {}_{l=1}\Sigma^4 \gamma_l u_{t-l} + e_t$$

where the notation again carries.

The final model used in this analysis is an attempt to combine both additive and amplification effects in a single descriptive relationship.

(3) $$u_t = \Sigma d_t (1 + \Sigma \beta_i X_{it}) + {}_{l=1}\Sigma^4 \gamma_l u_{t-l} + \Sigma \phi_i X_{it} + e_t$$

where the notation is consistent. Blanchard and Wolfers (1999) do not include such a model, but the dynamic effect could very well occur on both additive and amplification basis.

[22] Removing fixed effects from dynamic panel models can result in problems with underestimating the coefficients on the lagged dependent variables. However, this does not interfere with my primary results. Other estimation techniques (not reported), such as differencing the data, do not alter the major findings of the paper.

Estimation Procedure

I estimate these three equations using sixteen of the twenty countries outlined earlier. New Zealand, Ireland, Portugal, and Spain are removed.[23] As seen earlier, six time-variant series are placed in the institution vector: the maximum replacement rate (*maxrrate*), maximum benefit duration (*maxdur*), employment protection (*emppro*), coordination (*coord*), total union density (*untotden*), and the tax wedge (*taxwedge*). Union coverage and active labor market policy, the other two institutions outlined in Nickell (1997), are eliminated and their influence subsumed into the individual effect (as they are fixed effects).

Assigning an expected sign to some of these variables is problematic. The literature outlined above is often contradictory, and the evidence speaks to the problems of heterogeneity. For the purpose of this analysis, the expected sign of each of these institutions except coordination is positive – an increase in the institution is associated with a rise in unemployment.

Four institutions are expected to have a positive coefficient with relatively little debate in the literature. Larger or longer benefits (*maxdur* and *maxrrate*) increase the incentive to stay unemployed longer by making leisure relatively more attractive. The tax wedge increases the burden on workers and raises the firm's producer wage relative to the worker wage, also increasing unemployment by virtue of incentive. High union density creates rigid wage structures, preventing quick adjustment to shocks and subsequently creating more unemployment.

[23] Considering the size of the panel, my primary concern was consistency in data. The best measures for time-variance in union density and coordination did not include observations for these four countries. Given the potential strength of union effects, and the lack of a comparable source, I chose to eliminate them from the set. New Zealand's data were also problematic in the tax wedge calculation.

However, employment protection and coordination have ambiguous signs in the literature – many studies offer conflicting viewpoints. Employment protection increases the duration of unemployment, but decreases the flow in and out of unemployment. The net effect is a theoretical unknown. For the purpose of this analysis, the duration effect is assumed to be larger than the flow effect, effectively resulting in higher unemployment.

A similar problem exists with coordination – does more planning lubricate a unionized market? Or does the coordination merely exacerbate the disparity between insiders and outsiders? I choose the former, given that the extant empirical evidence seems to favor this approach. Coordination is expected negative; greater centralization theoretically results in a wage closer to equilibrium. I multiply coordination by –1 (effectively making it *un*coordination) so that the expected signs are all positive.

Several techniques are used to reach an accurate estimation:

- All three specifications are estimated using instrumental variables, specifically the institutional variable at time t lagged by one period and multiplied by the mean unemployment rate at time $(t-1)$.[24] Instrumental variables are used to hedge against endogeneity issues. The lack of a strong theoretical framework stymies efforts to endogenize variables in the specification, but instrumental variables provide a good alternative, especially with institutions. Unemployment in period t cannot drive an institution from period $(t-1)$ because the institution has been predetermined. For an example, look at the tax wedge. The tax wedge - by itself - has a good fit with unemployment. It is argued that as unemployment rises, a given configuration of the welfare state starts to cost more. Over the medium run governments tend to balance

[24] The mean unemployment rate at time $(t-1)$ could be thought of as the aggregate time effect of that particular period; that is, the fixed time effect.

their budgets, thus taxes probably rise in concert. But if unemployment rises, taxes the previous year cannot change to provide a budgetary buffer.

- The estimation potentially contains issues of serial correlation. Indeed, Blanchard and Wolfers (1999) note that their results would potentially be less significant if they compensated for the evidence apparent in their residuals. Preliminary tests on the time-variant data did indeed indicate that both serial correlation and heteroscedasticity might be a factor. Therefore, the reported estimations include four lags of unemployment to guard against serial correlation.[25]
- Finally, I also estimate each specification where the standard errors are robust to heteroscedasticity of an unknown form.

Equation *(1)* is estimated with simply the instrumental variables. I then utilize non-linear least squares and non-linear instrumental variables to reestimate *(1)* in terms of the additive time effects of institutions on unemployment rates. Finally, *(2)* (amplification effects) and *(3)* (both additive and amplification effects) are examined using the same two non-linear techniques for comparison.

Evidence

Estimation of *(1)* by instrumental variables produces results summarized in ***Table 8***. Four of the six institutional variables are significant, with the tax wedge and employment protection highly so. The strongest result indicates that the tax wedge has a strong, positive

[25] The question of serial correlation in dynamic panel studies is indeed a complicated one. In the discussion of evidence, a Durbin-Watson statistic is included to give an idea of how the estimations look under non-rigorous analysis. However, the DW is considered unreliable when a lagged dependent variable (indeed, in any case in which a regressor is correlated with the error term). The Durbin-h test may also be unwise, as pointed out in Inder

relationship with unemployment. Employment protection also reveals a tight and (perhaps surprisingly) negative relationship with unemployment. The replacement rate shows a weaker but still significant negative association, and (un)coordination is likewise related.

- The strength of the tax wedge is positive, with the expected result – an increased burden on labor implies a decreased incentive to work and to hire labor, making leisure or welfare marginally more attractive. The effect of the tax wedge is enormous, with the coefficient five times that of any other institution.

Employment protection turning up negative embodies the theoretical debate. The models imply a smaller flow in and out of jobs, but a longer duration. However, they are silent on the effect on the unemployment rate – which side of the net effect is larger, restrictions locking workers out or the same restrictions locking workers in? The results of *(1)* argue for the latter, that unemployment protection helps workers retain their jobs. The magnitude is much smaller than that of the tax wedge, and even falls below that of the less significant replacement rate coefficient. Both the tax wedge and employment protection retain their sign and significance when introduced alone against unemployment, pointing to a certain robustness of the results.

More confusing are the signs on the coefficients of the less significant variables. The literature provides generally weak results for the replacement rate, with the effect ambling between positive and negative depending on the specification or other regressors. However, theory seems to indicate that an increase in benefits would decrease the difference between wage income and leisure income. While this assuredly increases unemployment duration, it also tentatively points to a positive relationship with the unemployment rate. ***Table 8*** indicates

(1984). Breusch and Godfrey (1981) propose another test, which I also use. The results are less strong than the DW, as is to be expected, and there is some question as to whether even this test is applicable to a dynamic panel.

that the association is negative, that a rise in benefits is correlated with a decline in unemployment. However, the result is mitigated when the replacement rate is regressed alone on unemployment – the coefficient keeps a negative sign but becomes insignificant.

The predicted sign on (un)coordination is more strongly rooted in theory and prior results. Coordination will decrease unemployment as wage-bargaining becomes more centralized and thus closer to equilibrium wage. However, the analysis reveals a negative sign, pointing to a fit where higher coordination (i.e. less *un*coordination) results in lower unemployment. This supports an insider-outsider story, where the coordination of wage-bargaining actually locks out non-union workers, resulting in higher and sustained unemployment. The tighter the centralization, the greater the schism between insiders and outsiders. Like the replacement rate, the coordination figure is rendered insignificant when introduced alone on unemployment.

It is also notable that neither union density nor benefit duration are found to have significant effects, either in the combined regression or alone. Jackman, Layard, and Nickell (1991) correlate duration strongly with unemployment, and a sizable literature links large unions and unemployment. When viewed through the lens of a dynamic panel, however, neither variable appears to explain unemployment rates well.

The non-institutional variables are also important. The high statistical significance of the sum of lagged unemployment variables implies that unemployment is markedly explained by the rate in previous years. This points to a long-run adjustment effect, meaning that the duration of a worker's unemployment may perhaps be as important as original job loss in determining next period's chance of unemployment. In addition, the use of a dynamic panel as

opposed to a pooled cross-section is indeed important. A test of the time dummies reveals them to be quite consequential.

Finally, the regression fits quite well, with a adjusted R^2 of 0.935. The time-variant institutional variables and lagged unemployment describe unemployment well over time and across countries.

Robustness of the Estimation

In looking at robustness, I first divide the data into two periods, using 1977 as my break point. In addition, I run ***(1)*** on the group of 12 European Countries (The Europe 15, minus Portugal, Spain, and Ireland) and using step data for two interpolated series, employment protection and the replacement rate.[26] ***Table 9*** summarizes the results.

Overall the original estimation is robust, with alterations to the panel producing no sudden shifts in signs or magnitude. Several coefficients either lose or gain marginal significance, and the fit of the regression continues to be relatively good. The time dummies remain important, and the sum of lagged unemployment variables also retains its meaningfulness.

The tax wedge is several degrees of magnitude more important than any other institutional variable across all of the alternative tests. The coefficient also holds on to significance, dropping only in the later period of 1977-1992. This period also sees a drop in the degree of the coefficient, perhaps implying that the tax burden was less of a factor in explaining unemployment than it was pre-1977.

[26] The regression was also run on several other country groups, none of whose results differed significantly from those show in the original run of ***(1)***. Other tests were also used (dropping a single country, using a different break year) without distinction.

Employment protection follows an inverse trend, jumping in magnitude and significance in the early period, but losing both in the later one. The variable becomes less significant in the Europe-only sample, and loses meaning altogether in the estimation using step data. The latter is somewhat expected, given that the step data for employment protection is relatively jagged, averaged over five-year periods.

The other stepped variable, the replacement rate, also loses its importance in that estimation. The rate is a factor in the latter period but not in the earlier one. It gains significance in the Europe-only sample, and the coefficient becomes larger (in absolute terms) in both alternative estimations where it is starred.

Coordination becomes inconsequential when the data is split by year, but gains size and significance when the sample is restricted to Europe. It retains minor importance in the step analysis, with a negligible change in magnitude.

Both union density and benefit duration remain statistical nonentities. Union density become marginally explanatory in the first time period, and benefit duration follows suit in the step data analysis. Neither one shows up in the additional estimations.

It appears that the original estimation is capturing employment protection effects from early in the sample and replacement rate effects from later. Coordination seems significant only over all 33 years, particularly in Europe. The tax wedge remains large and strong throughout each estimation.

Interaction Effects

Taking my cue from Blanchard, I interact the time-variant institutional variables with a fixed time effect, emulating unobservable shocks across countries. ***Table 10*** summarizes the

results of all three interaction specifications. I look first at the additive effects alone, using non-linear analysis with both least squares and instrumental variables.

A few variables turn up notable results. The tax wedge is again large and highly significant, no matter the econometric technique used. An increased tax burden on labor is related to a large unemployment rate. Estimation by non-linear least squares produces no other interesting regressors, but the instrumental variable method shows benefit duration having a weakly significant positive correlation with unemployment and (un)coordination again edging negative.

These results strongly support the importance of the tax wedge noted in the original analysis of *(1)*. Less convincing is the consistency of coordination – the present value of coordination appears to have little relationship with unemployment, but the lagged coordination of the instrument does support the insider-outsider story. However, the surprising result on benefit duration is even weaker, though it does support earlier studies linking higher benefit duration with larger unemployment rates.

The additive effects alone also fit well. The adjusted R^2 is 0.936 and 0.935 for the least squares and instrumental variable estimation respectively. There is also a case to be made for amplification effects – institutions either reducing the impact of shocks or boosting the magnitude. I examine these amplification effects in isolation by estimating *(2)* using the same two techniques.

The coefficients are markedly different from the additive equation, indicating that different institutions affect the degree to which a shock alters unemployment. Union density and employment protection have a sizable impact, and the impressive nature of the tax wedge disappears.

Union density produces a very large and consequential coefficient, meaning that the variable makes shocks to unemployment much worse. This is compatible with the theoretical implications of union organization. When a relative price shock or decline in total factor productivity boosts unemployment, the rigid nature of unionized markets prevents a rapid adjustment of wages, keeping the market out of equilibrium for much longer than if the wage was allowed to fall accordingly.

Likewise, employment protection is also significant, albeit of a somewhat smaller magnitude. The effect is negative, implying that countries with more employment protection suffer less under an adverse shock. Given the ambiguous literature on employment protection, this is difficult to interpret. One reading is that as unemployment is negatively shocked, employment protection prevents employers from rapidly jettisoning workers, leaving workers in jobs at a lower wage.

Benefit duration and coordination also appear to be significant in altering the impact of a shock. Interestingly, the significance only turns up in the least squares estimation, as opposed to the additive importance granted by the instrumental variable technique. The signs are as in previous results, with benefit duration marginally significant and positive, and uncoordination statistically more important and negative.

The estimation of *(2)* is also a good fit to the data. Least squares creates an adjusted R^2 of 0.942, with the instrumental variable method being slightly worse at 0.936. From the results of the two single-effect equations, an intriguing story is emerging. The powerful effect of the tax wedge seems to be a mostly additive effect, with employment protection and union density being associated with the degree by which the unemployment rate is affected by a shock.

Coordination and benefit duration straddle the divide, showing significance depending on the econometric methodology. Finally, the replacement rate is consistently insignificant.

To shed light on the relationship between these additive and amplification effects, I estimate *(3)*, again utilizing non-linear least squares and non-linear instrumental variables. The results are consistent with the independent analysis described above.

The tax wedge remains the dominant additive effect, showing no indication of any impact on the magnitude of outside unemployment shocks. Union density also maintains an enormous positive amplification effect, whereas employment protection stays large and negative. Neither density nor protection turn up significant on the additive side.

Less apparent is the nature of benefit duration and coordination. The two variable maintain the trend revealed in the individual analysis, with instrumental variables deeming them both significant and additive. Conversely, least squares points to an important shock effect. The additive evidence is slightly more consequential.

This ambiguity might point to a dual effect. It is certainly possible that extended benefit duration creates an incentive to stay out of the job market; the instrumental variable is constructed around a lagged duration, and it is plausible that people base their allocation of work and leisure partly on recent (lagged) knowledge of how long benefits are available. However, when unemployment is adversely shocked, the present duration laws can act as a rigidity, prolonging the readjustment of workers to the new labor market.

A similar story exists for coordination. Higher coordination exacerbates the schism between insiders and outsiders, locking some workers out of labor markets. Therefore, as coordination increases, so does the unemployment rate as more workers find themselves in a disagreeable position to bargain for wages. Moreover, high coordination creates a coterie of

insiders who remain insulated when unemployment is increased via shock. The number of insiders is reduced under the shock effect in order to keep wages high, and the shock is thus worsened.

Ultimately, this dual effect may also point to a problem with the specification or econometric technique. The only difference between the two sets of results are the methodology. However, several strong results – employment protection, the tax wedge, union density – point to an internal consistency between the methods. Regardless, the effect of coordination and benefit duration is nebulous at best, and deserving of further investigation.

One anomaly of the combined analysis is the sudden significance of the replacement rate's amplification effect in the least squares analysis. While statistically important, the lack of robustness with respect to other regressions or methods sheds doubt on the validity of the result. Interestingly, the amplification effect is positive, as opposed to the estimation of *(1)* which yielded a negative coefficient. This sign dovetails well with theory – like duration, a higher rate of benefits will prolong the effect of any negative shock to unemployment rates.

Along with the strong results for the tax wedge, employment protection, and union density, the combined regressions also retain an excellent fit. The instrumental variable method's center R^2 is 0.920, and the least squares' is 0.945. Each of the three interaction estimations describe over 90% of the unemployment rate.

Conclusion

Of course, there is also much yet to be done. There still remains work to be done on a dynamic panel, namely the difficult tasks of constructing time-variant series for active labor market policy and union contract coverage. In addition, there may be other institutions which,

though perhaps unimportant when static, become significant when allowed to vary over time. The largest issue to come out of this analysis, however, is the development of a specified theory of institutional interaction. While there is a sizable theoretical literature with regard to individual institutions, little has been done to rigorously define how they interact in aggregate.

The introduction of time-variant institutions offers strong evidence for several ways in which unemployment rates can be altered. Foremost, the contemporary tax burden on labor appears to directly effect a country's unemployment rate; the more taxes on labor, the more unemployment experienced. Secondly, two institutions indirectly affect unemployment rates by either dampening or enhancing the power of adverse shocks. Employment protection evinces a strong negative amplification effect, drastically reducing the impact of a negative shock. Conversely, a high union density exacerbates the time effect, indicating that more unionization results in longer, more powerful shock.

I am also encouraged by the good fit of the regressions, despite correcting for serial and spatial correlation as well as endogeneity. While the descriptive of the nature may not come from a tightly specified theory of institutional interaction, the quality of the fit points to the importance of time-variant institutions in any summary analysis. Allowing time effects certainly captures something not seen before in the literature, institutions explaining heterogeneity among countries *and* time periods.

Data Appendix

The estimation analysis is based on a completely new dataset of time-variant institutions. Several sources exist for many of these variables, and issues of construction and measurement were as important as techniques of specification in developing the final evidence. Italics denote variables or titles appearing in tables and charts. Spreadsheets of the primary data are available upon request.

Chart/Table Averages and Groupings

The evolution charts trace the paths of seven group averages, all produced by a simple averaging technique. *Average* is taken over all 20 countries. *Modified Average* is taken over the 16 countries included in the estimations (20 minus Ireland, New Zealand, Portugal, and Spain). The *E15* refers to Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom. *Non-Europe* consists of Australia, Canada, Japan, New Zealand, and the United States. *Scandinavia* includes Denmark, Finland, Norway, and Sweden. The *British Isles* consist of Ireland and the United Kingdom. *Continental Europe* refers to the E15 minus Scandinavia and the British Isles.

In instances where data are missing for the omitted four countries, all computations are made without those countries. In this case, the *British Isles* becomes the *United Kingdom*. Note also that the E15 becomes effectively the E12.

The tables contain the following notation outside of the country codes: *AVG* is the *Average*; *MAVG* is the *Modified Average*; *EAVG* is the *E15* average; *CAVG* is the *Non-Europe* (control) average; *NAVG* is the North America Average (Canada and the United States).

Unemployment Rates[27]

The OECD compiles unemployment rates as measured by the originating countries. In addition, standardized rates are estimated using rules developed by the OECD and the ILO. I compiled both series from the 1999 OECD Economic Outlook and earlier editions. Standardized rates are used, except for Austria, Denmark, Ireland, New Zealand, and Portugal, and Switzerland. Common rates are used for the first five and for Sweden 1960-63. For Switzerland I uses the adjusted rates of Jackman, Layard, and Nickell (1991) before 1991, and the OECD standardized rates thereafter.

Replacement Rate[28]

OECD replacement rates are collected across three family types, two earnings levels, and seven unemployment durations, producing 42 separate gross replacement rates. Eighteen rates are usually culled from the data by reducing the duration samples to three – the first year, the second and third years, and the fourth and fifth years. The primary replacement rate series is then an average over these 18 series. The data are collected on odd years.

Data appendices to Blanchard and Wolfers (1999) point out that the current OECD measure confuses benefit generosity and benefit duration. A country with a relatively high benefit reimbursement but limited duration will see the average replacement rate drop, distorting the true caliber of unemployment benefits. Likewise, a country with unlimited but meager benefits will be also portrayed artificially.

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[27] For an assessment of cross-country comparability of unemployment rates, see ILO (1996). Several articles argue that the unemployment rate, while perhaps less than desirable for measurement within a country, is useful for cross-country comparison. See for example Murphy and Topel (1997).

[28] An excellent discussion of the evolution of benefit systems in the OECD can be found in Chapter 8 of the Jobs Study (1994). The Jobs Study also examines issues of the relationship among the replacement rate, duration, and employment protection.

The alternative measure compiled by Wolfers averages over family types and earnings levels, leaving seven series. The maximum replacement rate is then chosen from the seven and used as a measure of generosity. I use this measure, and derive two series: one that uses linear interpolation to fill in the even years and another that creates a step series, with the even year taking on the value of the odd year before it. In the latter case, 1960 takes on the value of 1961.

Benefit Duration

In *Social Security Programs Throughout the World*, the United States Social Security Administration reports on the state of welfare systems in a number of countries every two years, including the structure of unemployment benefits. Most countries legislate two levels of benefits: unemployment insurance (often limited and related to past earnings) and unemployment assistance (often unlimited and means-tested). Since the OECD makes no distinction in constructing replacement rate data, I evaluate the maximum duration as the point at which all benefits, including means-tested assistance, would be exhausted. In terms of years, this maps onto a {0, 6} index, with 6 interpreted as unlimited duration. The measure is for a worker under 50; many European countries maintain a higher duration for the aged which would skew the measure upwards.

In unreported years, I insert data according to the last implemented legislation. If there is a value of {6} for 1981 and 1979, then I insert {6} for 1980 if the 1981 report indicates that the legislation predated 1980. In the case that legislation occurs in an unreported year, I use linear interpolation to insert the value.

Nunziata (2000) proposes an index {0, 1} that estimates average duration as

$$BDAVG = \alpha\,(BRR_2 / BRR_1) + (1 - \alpha)\,(BRR_4 / BRR_1)$$

where BRR_1 is the replacement rate in the first year, BRR_2 is the rate for the second and third year, and BRR_4 is the rate for the fourth and fifth, with $\alpha = 0.6$. Therefore, if benefits stop after one year, the average is 0, and constant benefits are 1.

I do not use this measure because it 1.) fails to capture the nuances of durations in many European countries which are often fractions of years, 2.) blurs duration in the second/third and fourth/fifth years, 3.) reports an average, rather than maximum duration, and 4.) relies on a relatively arbitrary average to delegate the importance of early benefits. The measure is potentially useful, but does not fit the needs of my estimations.

Employment Protection

Blanchard and Wolfers (1990) link data from Lazear (1990) and an OECD dataset starting in 1985. The Lazear data only include information on severance pay and notice period for blue-collar workers with ten years seniority, making the set much narrower than the measures of the OECD. The combined database is observed every five years, and is reported on a {1, 4} scale, with a higher index number meaning stricter employment protection.

As with the replacement rate, I create two series, one interpolated and one stepped. The step series relies on the older year, meaning that the values for 1961, 1962, 1963, and 1964 will equal 1960 and not 1965.

Coordination

I use the internally consistent indexing created by Michael Wallerstein (1996) and reported in Golden, Wallerstein, and Lange (1998). The index number accounts for both

employer and union coordination, with a higher index meaning a greater degree of coordination. The index is reported $\{1, 4\}$ with

- 1 = plant-level wage-setting
- 2 = industry level wage-setting
- 3 = central wage setting without sanctions
- 4 = central wage setting with sanctions

Other measures of coordination exist, for example in Bruno and Sachs (1985). However, Wallerstein's index has the distinct advantage of using the same criteria for each year sampled. This facilitates better comparison across years.

Total Union Density

Union density is calculated from data provided by Visser (1992) and statistics from the OECD Labor Force Statistics. The total union membership series from Visser (which includes retired and unemployed but excludes self-employed) is divided by the total dependent labor force, or salary and wage earners plus the unemployed.

Because it includes the retired, the total union density yields a ratio biased upward by a small degree. This is an intentional and crude attempt to account for the higher union coverage experienced by most European countries. The use of net union density (that is, subtracting out retired from the union membership total) will not appreciably change the results of the estimations.[29] Due to missing or fragmented data, I interpolate the following observations: AUS: 1990-1992; SWZ: 1990; USA: 1990-1992.

Nunziata (2000) constructs a union coverage series based on a OECD cross-section from 1980. He interpolates from 1980 to 1995, and then constructs the series before 1980 by

[29] Arguably, coverage matters more than density precisely because many European countries have relatively low union densities by very high coverage ratios. This is especially true in France. When the difference is high, it makes density a rather poor measure of worker power in the bargaining process. However, the measurement issues associated with coverage leave us with density as a crude approximation.

1.) calculating the ratio of union coverage to union density in 1980 and 2.) using average values of that ratio, time-varying density data, and additional information to create the relevant projections. While useful for noting general trends, I view this construction as too tenuous for rigorous estimation.

Tax Wedge

My base measure comes from the Center for Economic Performance at the London School of Economics. The wedge is constructed as:

$$taxwedge = t1 + t2 + (tx - sb) / yq$$

where *t1* is the employment tax rate, *t2* is the direct tax rate, *tx* is indirect taxes, *sb* is subsidies, and *yq* is gross domestic product at market prices. I use the CEP's base estimates and then fill in gaps using the OECD's national accounts data. Because of missing or fragmented data, I interpolate the following observations: AUS: 1986-1992; BEL: 1988-1992; NLD: 1990-1992; NOR: 1960-1964; PRT: 1991-1992; SPN: 1987-1992.

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Table 1. The present analysis versus selected priors

	Nickell (1997)	*Blanchard and Wolfers (1999)*	*Present Analysis*
# Institutional Variables	8	7	6
# Time-Variant	0	2	6
Econometric Technique	Descriptive	Descriptive	Descriptive
Add. effects measured?	Yes	No	Yes
Amp. effects measured?	No	Yes	Yes
Both effects measured?	No	No	Yes

Table 2. Individual sample statistics for the maximum replacement rate

	AUS	AUT	BEL	CAN	DNK	FIN	FRA	GER	IRE	ITA	JPN	NLD	NOR
Mean	0.208	0.351	0.464	0.527	0.496	0.417	0.586	0.402	0.417	0.118	0.506	0.759	0.436
Median	0.223	0.352	0.497	0.580	0.518	0.406	0.600	0.395	0.438	0.112	0.505	0.785	0.386
Maximum	0.270	0.452	0.555	0.692	0.718	0.591	0.654	0.432	0.607	0.283	0.535	0.800	0.620
Minimum	0.137	0.195	0.297	0.358	0.257	0.246	0.500	0.373	0.275	0.022	0.475	0.700	0.235
Std. Dev.	0.041	0.070	0.078	0.106	0.165	0.098	0.050	0.020	0.122	0.069	0.020	0.047	0.154

	NZL	PRT	SPN	SWE	SWZ	UKI	USA	AVG	MAVG	EAVG	CAVG	NAVG
Mean	0.317	0.285	0.595	0.561	0.383	0.347	0.436	0.430	0.437	0.441	0.399	0.482
Median	0.318	0.323	0.640	0.680	0.323	0.335	0.435	0.457	0.462	0.469	0.413	0.510
Maximum	0.425	0.650	0.915	0.846	0.710	0.482	0.460	0.522	0.520	0.553	0.430	0.563
Minimum	0.267	0.000	0.000	0.216	0.050	0.217	0.380	0.322	0.331	0.301	0.342	0.380
Std. Dev.	0.042	0.270	0.216	0.251	0.286	0.098	0.018	0.082	0.070	0.100	0.030	0.060

AVG is the average over all 20 countries, MAVG is the average over the 16 countries used in the econometric estimations, EAVG is the average over the Europe 15, CAVG is the average over the five non-European countries, and NAVG is the average over North America. Refer to the data appendix for notes on the construction of these figures and averages.

Table 3. Individual sample statistics for the maximum benefit duration

	AUS	AUT	BEL	CAN	DNK	FIN	FRA	GER	IRE	ITA	JPN	NLD	NOR
Mean	6.000	6.000	6.000	2.584	0.905	1.964	5.083	6.000	6.000	0.626	0.793	4.495	1.161
Median	6.000	6.000	6.000	0.980	1.000	0.410	6.000	6.000	6.000	0.490	0.820	5.000	1.500
Maximum	6.000	6.000	6.000	6.000	1.000	6.000	6.000	6.000	6.000	0.990	0.820	6.000	1.500
Minimum	6.000	6.000	6.000	0.960	0.680	0.410	3.750	6.000	6.000	0.490	0.740	2.360	0.380
Std. Dev.	0.000	0.000	0.000	2.347	0.147	2.510	1.087	0.000	0.000	0.226	0.038	1.679	0.523

	NZL	PRT	SPN	SWE	SWZ	UKI	USA	AVG	MAVG	EAVG	CAVG	NAVG
Mean	6.000	0.875	2.258	2.861	0.401	3.818	0.737	3.228	3.089	3.230	3.223	1.661
Median	6.000	0.990	1.500	0.820	0.250	6.000	0.750	3.307	3.236	3.181	2.906	0.855
Maximum	6.000	3.750	3.500	6.000	0.680	6.000	0.750	3.551	3.356	3.766	3.898	3.375
Minimum	6.000	0.000	1.500	0.820	0.250	1.500	0.690	2.766	2.551	2.721	2.898	0.835
Std. Dev.	0.000	1.182	0.867	2.570	0.184	2.284	0.025	0.223	0.261	0.274	0.464	1.178

Refer to table 2 for notes on these figures.

Table 4. Individual sample statistics for employment protection

	AUS	AUT	BEL	CAN	DNK	FIN	FRA	GER	IRE	ITA	JPN	NLD	NOR
Mean	1.000	1.957	2.626	0.600	1.966	2.375	2.136	2.743	0.747	4.097	2.800	2.675	3.066
Median	1.000	1.907	2.763	0.600	1.930	2.400	2.600	3.240	1.000	4.200	2.800	2.700	3.100
Maximum	1.000	2.600	3.100	0.600	2.200	2.400	2.950	3.300	1.070	4.200	2.800	2.700	3.100
Minimum	1.000	1.300	1.329	0.600	1.570	2.190	0.709	0.825	0.000	3.640	2.800	2.490	2.820
Std. Dev.	0.000	0.595	0.461	0.000	0.200	0.057	0.735	0.844	0.360	0.165	0.000	0.057	0.076

	NZL	PRT	SPN	SWE	SWZ	UKI	USA	AVG	MAVG	EAVG	CAVG	NAVG
Mean	1.600	2.643	5.060	2.262	1.100	0.591	0.200	2.112	2.012	2.403	1.240	0.400
Median	1.600	3.580	5.735	3.000	1.100	0.700	0.200	2.213	2.109	2.538	1.240	0.400
Maximum	1.600	3.900	6.218	3.600	1.100	0.700	0.200	2.377	2.248	2.756	1.240	0.400
Minimum	1.600	0.000	3.310	0.000	1.100	0.329	0.200	1.591	1.501	1.709	1.240	0.400
Std. Dev.	0.000	1.558	1.236	1.475	0.000	0.151	0.000	0.260	0.256	0.347	0.000	0.000

Refer to table 2 for notes on these figures.

Table 5. Individual sample statistics for coordination

	AUS	AUT	BEL	CAN	DNK	FIN	FRA	GER	IRE	ITA	JPN	NLD	NOR
Mean	3.030	2.000	2.424	1.273	3.455	2.970	2.030	2.000	NA	3.242	1.879	3.091	3.000
Median	3.000	2.000	2.000	1.000	4.000	3.000	2.000	2.000	NA	3.000	2.000	3.000	3.000
Maximum	4.000	2.000	4.000	4.000	4.000	4.000	3.000	2.000	NA	4.000	2.000	4.000	3.000
Minimum	3.000	2.000	2.000	1.000	2.000	2.000	2.000	2.000	NA	2.000	1.000	2.000	3.000
Std. Dev.	0.174	0.000	0.830	0.876	0.905	0.637	0.174	0.000	NA	0.502	0.331	0.914	0.000

	NZL	PRT	SPN	SWE	SWZ	UKI	USA	AVG	MAVG	EAVG	CAVG	NAVG
Mean	NA	NA	NA	3.000	2.000	2.000	1.000	NA	2.400	2.481	1.436	1.136
Median	NA	NA	NA	3.000	2.000	2.000	1.000	NA	2.438	2.467	1.400	1.000
Maximum	NA	NA	NA	3.000	2.000	2.000	1.000	NA	2.750	2.667	2.000	2.500
Minimum	NA	NA	NA	3.000	2.000	2.000	1.000	NA	2.188	2.267	1.200	1.000
Std. Dev.	NA	NA	NA	0.000	0.000	0.000	0.000	NA	0.118	0.108	0.197	0.438

Refer to *table 2 for notes on these figures.*

Table 6. Individual sample statistics for total union density

	AUS	AUT	BEL	CAN	DNK	FIN	FRA	GER	IRE	ITA	JPN	NLD	NOR
Mean	0.513	0.592	0.609	0.304	0.715	0.676	0.178	0.386	NA	0.441	0.309	0.359	0.619
Median	0.512	0.585	0.672	0.311	0.726	0.769	0.196	0.385	NA	0.507	0.321	0.387	0.628
Maximum	0.545	0.635	0.704	0.336	0.839	0.945	0.230	0.416	NA	0.574	0.348	0.418	0.640
Minimum	0.473	0.510	0.465	0.251	0.602	0.342	0.082	0.365	NA	0.273	0.238	0.271	0.570
Std. Dev.	0.020	0.033	0.091	0.028	0.094	0.191	0.046	0.014	NA	0.112	0.035	0.053	0.022

	NZL	PRT	SPN	SWE	SWZ	UKI	USA	AVG	MAVG	EAVG	CAVG	NAVG
Mean	NA	NA	NA	0.819	0.334	0.462	0.228	NA	0.472	0.413	0.271	0.266
Median	NA	NA	NA	0.828	0.339	0.442	0.260	NA	0.480	0.430	0.278	0.269
Maximum	NA	NA	NA	0.974	0.386	0.544	0.294	NA	0.509	0.450	0.292	0.295
Minimum	NA	NA	NA	0.674	0.251	0.361	0.138	NA	0.431	0.366	0.232	0.223
Std. Dev.	NA	NA	NA	0.100	0.031	0.046	0.054	NA	0.028	0.032	0.018	0.022

Refer to table 2 for notes on these figures.

Table 7. Individual sample statistics for the tax wedge

	AUS	AUT	BEL	CAN	DNK	FIN	FRA	GER	IRE	ITA	JPN	NLD	NOR
Mean	0.227	0.449	0.391	0.298	0.349	0.472	0.495	0.456	0.211	0.510	0.248	0.503	0.405
Median	0.230	0.433	0.395	0.299	0.351	0.515	0.476	0.488	0.199	0.482	0.225	0.547	0.443
Maximum	0.354	0.567	0.577	0.479	0.517	0.712	0.672	0.562	0.377	0.702	0.374	0.607	0.527
Minimum	0.121	0.305	0.236	0.156	0.145	0.231	0.323	0.339	0.068	0.413	0.149	0.321	0.201
Std. Dev.	0.077	0.087	0.097	0.094	0.125	0.147	0.116	0.075	0.106	0.079	0.078	0.091	0.101

	NZL	PRT	SPN	SWE	SWZ	UKI	USA	AVG	MAVG	EAVG	CAVG	NAVG
Mean	NA	0.243	0.340	0.531	0.336	0.349	0.352	0.358	0.398	0.403	0.225	0.325
Median	NA	0.192	0.301	0.556	0.368	0.371	0.363	0.364	0.410	0.410	0.225	0.331
Maximum	NA	0.624	0.663	0.776	0.405	0.463	0.465	0.511	0.536	0.570	0.333	0.472
Minimum	NA	0.111	0.113	0.264	0.244	0.203	0.234	0.211	0.246	0.237	0.134	0.195
Std. Dev.	NA	0.147	0.173	0.166	0.059	0.092	0.080	0.096	0.095	0.107	0.065	0.086

Refer to table 2 for notes on these figures.

Table 8. Unemployment rates estimated by instrumental variables

Variable	*1*	*2*	*3*	*4*	*5*	*6*	*7*
Emp Protection (*x100*)	-0.156**	-	-	-0.099*		-	-
	{0.067}	-	-	{0.056}		-	-
Replacement Rate (*x100*)	-0.466*	-	-	-	-0.290	-	-
	{0.279}	-	-	-	{0.253}	-	-
Tax Wedge (*x100*)	5.030***	-	-	-	-	2.925**	-
	{1.405}	-	-	-	-	{1.150}	-
Union Density (*x100*)	-0.263	-	-	-	-	-	0.698
	{0.596}	-	-	-	-	-	{0.617}
(Un)Coordination (x100)	-0.140*	-	-	-	-	-0.010	-
	{0.072}	-	-	-	-	{0.065}	-
Benefit Duration (*x100*)	0.043	-	-	-	-	-	-
	{0.035}	-	-	-	-	-	-
Σ Lagged Variables (*x100*)	83.618***	85.348***	85.324***	85.012***	84.109***	86.071***	85.705***
	{2.356}	{2.137}	{2.136}	{2.173}	{2.362}	{2.058}	{2.105}
Time Dummies $\chi^2(29)$	163.208	221.611	219.386	211.195	209.974	165.240	228.232
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
R^2	0.935	0.934	0.933	0.934	0.933	0.935	0.934
Durbin-Watson	1.994	1.981	1.982	1.985	1.982	1.986	1.986

Standard errors in brackets. Number of observations: 464.

Table 9. Testing for robustness

Variable	*Complete*	*1960-1976*	*1977-1992*	*Europe*	*Step Data*
Benefit Duration (*x100*)	0.043 {0.035}	0.047 {0.031}	0.027 {0.053}	0.058 {0.042}	0.064* {0.034}
(Un)Coordination (*x100*)	-0.140* {0.072}	-0.125 {0.089}	-0.110 {0.091}	-0.205** {0.084}	-0.141* {0.074}
Emp Protection (*x100*)	-0.156** {0.067}	-0.213*** {0.082}	-0.089 {0.113}	-0.128* {0.073}	-0.005 {0.026}
Replacement Rate (*x100*)	-0.466* {0.279}	-0.054 {0.311}	-0.763* {0.401}	-0.654** {0.297}	-0.103 {0.153}
Tax Wedge (*x100*)	5.030*** {1.405}	5.509*** {1.673}	3.984** {2.036}	5.070*** {1.459}	4.375*** {1.330}
Union Density (*x100*)	-0.263 {0.596}	-0.965* {0.563}	0.443 {0.943}	-0.730 {0.589}	-0.308 {0.564}
Σ Lagged Variables (*x100*)	83.618*** {2.356}	87.254*** {3.021}	81.035*** {3.326}	81.042*** {3.056}	85.531*** {2.078}
Time Dummies $\chi^2(29)$	163.208 [0.000]	76.280 [0.000]	70.279 [0.000]	136.028 [0.000]	160.020 [0.000]
R^2	0.935	0.880	0.853	0.944	0.935
Durbin-Watson	1.994	1.853	1.893	2.006	1.986
# Observations	464	192	272	348	464

Standard errors in brackets

Table 10. Interaction effects

Variable	*Additive Effects*		*Amplification Effects*		*Both: Additive*		*Both: Amplification*	
	NLLS	*IV*	*NLLS*	*IV*	*NLLS*	*IV*	*LS*	*IV*
Benefit Duration (*x100*)	0.040	0.062*	13.007*	13.522	0.031	0.095**	16.827*	6.525
	{0.030}	{0.034}	{7.343}	{11.715}	{0.025)	{0.039}	{8.986}	{6.121}
(Un)Coordination (*x100*)	-0.042	-0.141*	-20.569**	-23.547	-0.044	-0.341**	-24.128*	3.997
	{0.045}	{0.074}	{10.589}	{21.673}	{0.048}	{0.130}	{13.206}	{18.475}
Emp Protection (*x100*)	-0.012	-0.012	-26.743***	-25.225***	0.035	0.019	-33.590***	-24.843***
	{0.027}	{0.027}	{4.741}	{6.811}	{0.024}	{0.026}	{6.263}	{7.640}
Replacement Rate (*x100*)	-0.169	-0.176	56.853	41.284	-0.224	0.005	90.607*	-31.902
	{0.152}	{0.152}	{39.096}	{58.891}	{0.199}	{0.167}	{51.915}	{47.139}
Tax Wedge (*x100*)	3.349***	4.441***	53.448	-58.303	2.984***	4.857***	36.541	-94.684
	{1.112}	{1.326}	{122.314}	{136.750}	{0.542}	{1.228}	{126.682}	{178.667}
Union Density (*x100*)	-0.038	-0.297	347.275***	539.211***	-0.366	-0.680	441.264***	384.484***
	{0.030}	{0.566}	{100.496}	{179.261}	{0.518}	{0.663}	{130.042)	{124.239}
Σ Lagged Variables (*x100*)	85.353	85.237	91.002	88.496	86.424	81.416	86.424	81.416
R^2	0.936	0.935	0.942	0.936	0.945	0.920	0.945	0.920
Durbin-Watson	2.000	1.986	1.933	1.776	1.955	1.591	1.955	1.591

Standard errors in brackets. Number of observations: 464.