Unemployment, institutions and reform complementarities: Re-assessing the aggregate evidence for OECD countries

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UNEMPLOYMENT, INSTITUTIONS AND REFORM COMPLEMENTARITIES: RE-ASSESSING THE AGGREGATE EVIDENCE FOR OECD COUNTRIES*

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Abstract

There is no or limited consensus on the quantitative impact of institutions on unemployment, which has led some to question the case for structural reforms. Recent studies suggest also that institutions interact with each other and cannot be analysed in isolation. In this paper, we estimate a standard reduced-form model to explore the institutional determinants of unemployment and assess its robustness using a large battery of robustness checks. We show that, although the impact of each individual policy varies across countries due to policy interactions, the simple linear model can be used to draw inferences for countries with an average mix of institutions. The model is then extended to encompass systemic interactions, in which individual policies interact with the overall institutional framework. We find relatively robust evidence of broad reform complementarities.

I. INTRODUCTION

There is a rich literature on the aggregate unemployment effects of policies and institutions (see, among others, Scarpetta, 1996, Nickell, 1997, Blanchard and Wolfers, 2000, and Nickell *et al.*, 2005). However, many macroeconometric studies, relying on limited short-time series for few OECD countries, or larger samples based on *ad hoc* extensions of existing data, have failed to provide convincing evidence of the robustness of their results. As a consequence, there is no or limited consensus on the quantitative impact of institutions on unemployment, which has led some to question the case for structural reforms (*e.g.* Glyn et al., 2006, Baccaro and Rei, 2007, Howell et al., 2007). In addition, recent studies suggest that institutions interact with each other (*e.g.* Belot and Van Ours, 2004). Therefore, the effect of one given policy cannot be analysed in isolation, as it depends on the characteristics of other institutions prevailing in each country. Not only do institutions interact, but they may do it in a systematic manner. There is in fact some theoretical support for reform complementarities and, as a result, for broad reform packages (*e.g.* Coe and Snower, 1997), but so far no comprehensive empirical evidence has been provided to back this view.

Against this background, the objective of this paper is threefold. First, we estimate a standard reduced-form model of institutional determinants of unemployment on homogeneous data, which

* Comments from Sven Blöndal, Wendy Carlin, Dave Coe, Jean-Philippe Cotis, Martine Durand, Jorgen Elmeskov, Mike Feiner, David Howell, Etienne Lehmann, Edmond Malinvaud, John Martin, Giuseppe Nicoletti, Stefano Scarpetta, Paul Swaim, Raymond Torres and audiences in Paris, Toronto and Amsterdam are gratefully acknowledged. Catherine Chapuis, Sebastien Martin and Rebecca Oyomopito provided excellent research assistance. Any errors are the responsibilities of the authors alone. The views expressed in this paper are those of the authors, and do not necessarily reflect those of the OECD or of its member countries.

come from the same source and cover more than 20 years. We assess the robustness of our estimates using a large battery of sensitivity exercises. Second, we highlight the weaknesses of past work on interactions across policies and institutions. In practice, we show that following the standard procedure of augmenting baseline specifications by a number of selected multiplicative interactions – common to all papers in the literature – leads to fragile estimated interaction effects that do not survive to simple robustness checks. Third, we briefly discuss the theoretical case for systemic interactions, in which individual policies interact with the overall institutional framework, and explore them through the estimation of a non-linear model. We find fairly robust evidence of broad reform complementarities.

The paper can be divided into five sections. In section 2, reduced-form unemployment equations consistent with standard job-search and wage setting/price-setting (WS-PS hereafter) models (*e.g.* Mortensen and Pissarides, 1994, Layard *et al.*, 1991, Nickell and Layard, 1999) are estimated on cross-country/time-series data covering 20 OECD countries. Given that policies and institutions can have heterogeneous unemployment effects across countries, we test for data poolability and stress that our estimated relationship only prevails at the sample average. In the average OECD country, we find that high and long-lasting unemployment benefits, high tax wedges and stringent anti-competitive product market regulation increase aggregate unemployment. By contrast, highly centralised and/or coordinated wage bargaining systems are estimated to be associated with lower unemployment. We show that these findings are robust across different specifications, choices of the estimation sample, data and econometric methods, including treatment of possible reverse causality. Section 3 takes another look at the policy interactions which have been typically considered in previous literature. We argue that any interaction between two institutions is *a priori* endogenous, due to the potential correlation between each of these institutions and others that are omitted from the analysis due to lack of data and/or the fact that many institutions are difficult to quantify. We then show that, once methods that correct for potential endogeneity bias are used, virtually none of the standard interactions which have been highlighted in the past appears to be robust. In section 4, we note that such a lack of robustness is not inconsistent with theory, because interactions should in fact take place between each individual policy and the *overall* institutional framework. This suggests a way to search for systemic interactions and broad reform complementarities. Defining the unemployment effect of the overall institutional framework (at the sample average) as the sum of the linear unemployment effects of individual institutions, we estimate a non-linear model where the effect of the overall institutional framework is interacted with each individual institution, with all parameters simultaneously estimated. We find that structural policy reforms appear to have mutually reinforcing effects, *i.e.* the impact of a given reform is greater the more "employment-friendly" the overall institutional framework. Section 5 provides a few concluding remarks.

II. ASSESSING THE ROBUSTNESS OF INSTITUTIONAL DETERMINANTS OF UNEMPLOYMENT

In this section, we analyse the policy and institutional determinants of unemployment through a standard reduced-form unemployment equation, which we estimate for a sample of 20 OECD countries[1] over the period 1982-2003. More specifically, the following static model is estimated:

$$U_{it} = \sum_j \beta_j X_{it}^j + \chi G_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \qquad [1]$$

where i and t are country and time suffices, U_{it} is the unemployment rate, G_{it} is the OECD measure of the output gap – and aims to control for the unemployment effects of aggregate demand fluctuations over the business cycle, while α_i and λ_t are country and time effects, that we generally capture by including country and time dummies. Following a recent trend in this literature (see Biagi and Lucifora, 2008, and references cited therein), in order to capture large idiosyncratic shocks such as the collapse of the Soviet Union, the German reunification and the Swedish banking and real estate crises, in most specifications observations for Finland, Germany and Sweden in 1991 and 1992 are removed

[1] Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Netherlands, Norway, New Zealand, Portugal, Spain, Sweden, Switzerland, United Kingdom, United States.

from the sample, and different country fixed effects are used for each of these three countries over the two sub-periods 1982-1990 and 1993-2003. The X^js are measures of the usual labour market policies and institutions featured as explanatory variables in previous papers (for a survey, see Bassanini and Duval, 2006a), namely: the tax-wedge between labour cost and take-home pay (for a single-earner couple with two children, at average earnings levels); a summary measure of unemployment benefit generosity, capturing both level and duration of benefits (an average of gross replacement rates across various earnings levels, family situations and durations of unemployment); the degree of stringency of employment protection legislation (EPL hereafter); union membership rates, proxying trade-union bargaining power; the degree of centralisation/co-ordination of the wage bargaining, a proxy for the concept of "corporatism" which has received large attention in the comparative political economy literature (*e.g.* Flanagan, 1999). As already done in a number of previous papers (*e.g.* Scarpetta, 1996, Elmeskov *et al.*, 1998), dummies for different levels of corporatism are used here to capture non-linearities in the unemployment effect of corporatism. Finally, we capture the effect of product market institutions, which has received growing attention in recent literature (*e.g.* Blanchard and Giavazzi, 2003, Fiori *et al.*, 2007), through an indicator of the average degree of stringency of anti-competitive product market regulation across seven non-manufacturing industries (PMR hereafter).[2] All our data, except when differently specified are drawn from available OECD datasets.[3]

When we estimate such a model, we find that the tax wedge and average benefit replacement rate appear to be associated with higher unemployment (Table 1, Column 1), in line with a majority of empirical papers (see *e.g.* Nickell et al., 2005, and Bassanini and Duval, 2006a for a survey). We also find that economies with limited product market competition tend to be associated with high unemployment. Finally, high corporatism appears to dampen unemployment,[4] while EPL and union density are statistically insignificant at conventional confidence levels.

Table 1 here

However, insofar as institutions differ across countries and interact with each other (as discussed in the next section), their unemployment impact is likely to be country-specific. And indeed, if we estimate equation [1] assuming that all parameters are country-specific, specification tests reject the null hypothesis of parameter homogeneity. Despite this, the baseline equation of Column 1 may still capture adequately the *average* unemployment effects of institutions, i.e. the effects that prevail at the sample average for a hypothetical OECD country with an average mix of institutions. In order to check for this possibility, one needs to check that imposing identical coefficients across countries does not lead to biased and inconsistent estimates of average coefficients. This is done here through a battery of Hausman tests that compare an always consistent specification (with heterogeneous country-specific coefficients) with other possibly inconsistent but potentially more efficient ones, where a number of coefficients (or all of them, as in the baseline equation) are restricted to be homogeneous across countries. As shown in Table 2, a heterogeneity bias is found to emerge only for the output gap coefficient, while coefficients of institutions appear to be unbiased. This suggests that our baseline estimates can indeed be used to infer the average unemployment effects of institutions. This is further confirmed by the robustness of our main results to re-estimation of the baseline specification either without the output gap variable or with heterogeneous output gap coefficients (Columns 2 and 3 in

[2] In a companion paper (Bassanini and Duval, 2006a), we explore the effect of other policies often considered in aggregate analyses, such as active programmes and minimum wages. However, due to smaller samples and endogeneity issues, their analysis requires specific treatment, and they are therefore excluded from the analysis in the present paper. What matters is that controlling for these variables would not affect the estimated effects of the other institutions studied here. The companion paper also considers a number of specifications in which the output gap is replaced by a set of macroeconomic variables that capture more directly the unemployment impact of aggregate shocks and are less subject to potential endogeneity concerns. These specifications are not reported here, as they have no impact on the estimated coefficients of institutional variables.

[3] Available at http://www.oecd.org/dataoecd/25/25/37431112.zip. Descriptive statistics and details on variable construction and sources are reported in Bassanini and Duval (2006b).

[4] In principle, the baseline specification includes dummy variables for both high and intermediate corporatism. However, given that no country moved in or out of the intermediate level of corporatism over the sample period, the effect of this variable cannot be identified – even if controlled for – in most of the specifications. For this reason, it does not appear in Table 1 except in Column 7 (random effects estimates).

Table 1), *i.e.* to specifications that are free from heterogeneity problems (cf. Table 2, Columns 2 and 3).

Table 2 here

Now what is the relative importance of each of these institutions in determining unemployment in the average OECD country? In order to answer this question, we consider "typical" historical reforms, corresponding to one standard deviation of each institutional variable with respect to each country's average – so that in practice the standard deviation is netted out of cross-country variation. On the basis of the estimates of Column 1 in Table 1, it can be concluded that a "typical" historical reform of the average benefit replacement rate (that is 4.7 percentage points), the tax wedge (2.8 percentage points) and PMR (1 indicator unit) would lower the unemployment rate in the average OECD country by about 0.5, 0.7 and 0.5 percentage points, respectively. These effects are both fairly similar and sizeable.

These results appear to be robust to several sensitivity exercises. First, although the average gross benefit replacement rate indicator used here has the advantage of capturing both the effect of unemployment benefit levels and duration, it does not adequately reflect take-home benefit levels.[5] In order to check the robustness of this particular coefficient, we replace the gross benefit replacement rate variable with an average of the two net replacement rate measures reported by Scruggs (2005). The point estimate of the replacement rate remains highly significant but is more than halved by this measurement change (Column 4 in Table 1). However, due to the greater variability of the Scruggs indicator, the "typical" historical reform of unemployment benefits (6.8 percentage points if measured with this indicator) is still found to reduce unemployment by 0.35 percentage points on average, an effect only 30% smaller than the baseline estimate.

Second, we check the robustness of our baseline estimates to sample variations. The sample and specification adjustments made in the baseline equation for Germany, Finland and Sweden are not found to be influential, as the main findings are virtually unaffected when the excluded observations are re-incorporated in the estimation sample, and no data adjustment is made for these countries, or when these countries are excluded altogether (Columns 5 and 6 in Table 1). Also, in order to investigate the more general possibility that one single country might significantly affect the estimated parameters in our small country sample, we eliminate each country after the other and re-estimate the baseline specification at each step (Figure 1). While some countries are found to be influential – point estimates of the impact of the tax wedge and PMR are reduced by about one-third upon elimination of Ireland and Spain, respectively –, the four main significant institutions (benefit replacement rate, tax wedge, PMR and the high corporatism dummy variable) never become insignificant upon elimination of any country from the sample. Finally, we check whether regression results might be driven by specific data points by re-estimating the baseline specification on random sub-samples of the main sample (see *e.g.* Baccaro and Rei, 2007). Concretely, two re-estimation exercises are performed, one on 1000 random draws of 90% of the original estimation sample, and another on 1000 random draws of 50% of the sample. Again the four significant institutions in the baseline specification never become insignificant upon random elimination of 10% of the sample. Additionally, they never change sign upon random elimination of 50% of the sample, except in very few instances in the case of PMR (in 0.4% of the draws) and corporatism (in 0.1% of the draws).

Figure 1 here

Third, we investigate the robustness of our results to alternative estimation methods. As many of the papers in the literature have tended to use random effect estimators to capture omitted time-invariant institutions, we present random effects estimates in Column 7 of Table 1. Hausman tests reject estimate consistency in this case, suggesting that more reliable estimates are obtained by using fixed effects (that is, by including country dummies), as we do in most other specifications of Table 1. This comes as no surprise since institutional variables are unlikely to be uncorrelated with country effects – as these include other unobserved time-invariant institutions –, a condition required for the consistency of the random effect estimator. Another potential issue is serial correlation in the residual.

[5] Indeed, once separate indicators for duration and levels are included, both appear to be significant (see Bassanini and Duval, 2006a).

We address this issue in two ways. On the one hand, we re-estimate our specification by Feasible Generalised Least Squares by allowing errors to be serially correlated and heteroskedastic across panels (Column 8). On the other hand, we re-estimate our baseline specification using 5-year-averaged data (Column 9), for which we find no evidence of serial correlation, although at the price of inflated standard errors, which are at least four times larger. Looking at point estimates, both exercises suggest that baseline coefficients are not biased by serial correlation. In this context, the low significance of estimates in Column 9 can be explained on the basis of the inefficiency of the estimators due to the very small number of observations.

Finally, the potential endogeneity of reforms is a matter for concern. For example, policy makers might be expected to increase the generosity of benefits in response to the perceived need for a safety net, which in turn tends to be greater in periods of gloomy employment prospects. Therefore, causation may run from unemployment to benefit generosity, rather than the opposite (see for example Howell *et al.*, 2007). A similar argument can be made for the tax wedge. In order to check that our results do not reflect reverse causality, we carry out Granger-causality tests for both variables. Somewhat surprisingly, they show no evidence of reverse causality, while the long-run impact of unemployment benefits and the tax wedge on unemployment is only marginally affected (Table 3).[6] However, endogeneity concerns may also arise if omitted variables simultaneously drive both institutions and unemployment. As a partial check for this problem, we re-estimate our baseline model using a difference GMM estimator (Arellano and Bond, 1991) on five-year averaged data, in which each policy change is instrumented with lagged values of policy levels. Only minor differences with our baseline point estimates are found (Column 10 of Table 1).[7]

Table 3 here

Overall, robustness checks suggest that our main findings are reasonably robust.

III. POLICY INTERACTIONS

In a standard WS-PS model, it can be shown that institutions interact with each other in their impact on aggregate employment and unemployment (*e.g.* Belot and van Ours, 2004). In fact, policies and institutions that affect the *elasticity* of wage claims to employment (*e.g.* unemployment benefits, union bargaining power, product market regulation) and/or the elasticity of labour demand to the bargained wage (*e.g.* product market regulation, EPL, the tax wedge) interact with policies and institutions that shift the *level* of wage claims (*e.g.* unemployment benefits) and/or labour demand (*e.g.* product market regulation). More generally, any factor that affects the slope of the WS and/or PS curves interacts with any factor that affects the level of the WS and/or PS curves. For example, the employment effects of a labour market reform that shifts the WS curve downwards (*e.g.* a cut in unemployment benefits) will be greater: i) the flatter the PS curve (*e.g.* the lower the degree of product market regulation), because the decline in real wages induced by the reform has larger effects on labour demand in this case (Figure 2); and, ii) the flatter the WS curve (*e.g.* the lower the bargaining power of unions and/or the lower the degree of product market regulation), because the increase in employment induced by the reform has smaller feedback effects in terms of higher wage claims – and thus lower employment gains. As virtually all institutions considered here can affect the slope and the position of at least one of both curves, all possible interactions should in principle be considered, and assessing the most relevant is essentially an empirical issue.

[6] These tests are obtained by estimating models with two lags of the unemployment rate and labour market institutions whose baseline coefficients are potentially affected by reverse causality (unemployment benefits and tax wedge). Although insignificant in the baseline model, we also include two lags for EPL and union density, as reverse causality arguments can be made for them as well. Nevertheless, their estimated long-run impact – not shown in Table 3 – remains insignificant.

[7] Serial correlation tests suggest that autocorrelation is low in five-year averaged data, pointing to difference GMM estimators, with up to only one lag for the autoregressive component in the error term, as an appropriate choice. We do not implement a GMM estimator using annual data because GMM estimators are sensitive to the choice of the order of the autoregressive component in the error term, and this choice can hardly be made in a parsimonious way using persistent annual data and short time series.

Figure 2 here

Interactions among institutions in macroeconometric equations are usually specified as multiplicative terms, which take the form of products of deviations of institutions from their sample mean. In the case of one single interaction between institutions X^k and X^h, this implies augmenting the baseline model as follows:

$$U_{it} = \sum_j \beta_j X_{it}^j + \gamma_{kh}\left(X_{it}^k - \overline{X}^k\right)\left(X_{it}^h - \overline{X}^h\right) + \chi G_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \qquad [2]$$

where $\overline{X}^k$ and $\overline{X}^h$ are the sample means – across countries and over time – of X^k and X^h, respectively, and other variables are denoted as in equation [1]. With this formulation, coefficient β_k can be readily interpreted as the marginal unemployment effect of X^k at its sample mean $\overline{X}^k$, when all other co-variates are kept constant at their sample means. For two institutions X^k and X^h that increase unemployment, a negative and significant sign for the interaction coefficient γ_{kh} would provide evidence of reform complementarity.[8]

Undertaking a systematic analysis of policy interactions consistent with Figure 2 within the above framework is not straightforward, however. This is because any extension of equation [2] to more than one type of interaction should also include all "implicit" interactions in order to minimise the risk of coefficient bias – unless there are strong *a priori* reasons to proceed otherwise, see *e.g.* Braumoeller, 2004. For example, estimating a model with four couples of multiplicative institutions (X^k, X^h), (X^k, X^m), (X^k, X^n) and (X^k, X^p) would in fact imply incorporating a total of 26 interaction terms in the equation – the total number of combinations of two and more variables within a set of five institutions, thereby inducing a substantial loss of degrees of freedom. In addition, given the likely correlation among these interaction terms, such an overspecified model would raise legitimate multicollinearity concerns. For this reason, virtually all existing studies consider only a small, *ad hoc* number of interactions, *i.e.* they implicitly restrict all other interaction coefficients to 0 (see *e.g.* Scarpetta, 1996, Nickell *et al*., 2005, Baccaro and Rei, 2007). However, in order to obtain robust findings through this approach, one needs at least to show that the chosen interaction(s) maintain(s) sign and significance regardless of the specification and, notably, of the inclusion of additional interaction terms.

In particular, interaction terms including omitted institutions might bias coefficient estimates. Let us suppose for instance that no interaction exists between an institution X^k and another institution X^h. If X^k is correlated with an omitted third variable X^s, and X^h interacts with X^s, then a significant interaction between X^k and X^h might in fact merely reflect the omitted (correlated) interaction between X^h and X^s. This seems especially relevant in the present context, as it is easy to think about institutions (*e.g.* eligibility criteria for unemployment benefits, cultural attitudes, etc…) that are difficult to measure and are therefore omitted from the analysis, but at the same time could be correlated both with unemployment and some explanatory variables. Insofar as such omitted variables are approximately time-invariant, country dummies (fixed effects) would be expected to control for them in linear specifications such as our baseline. Unfortunately, however, such dummies do not control for the correlation between included and omitted interactions, because the latter are not time-invariant, except when they involve only time-invariant variables.

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8 A negative sign implies that the detrimental effect of each policy indicator on unemployment is smaller the higher the other policy indicator, so that reforms diminishing the levels of these institutions should be undertaken together to maximise their impact. More formally, in equation [2] the partial derivative of unemployment with respect to the institutional indicator X^k is: $\partial U / \partial X^k = \beta_k + \gamma_{kh}\left(X_{it}^h - \overline{X}^h\right)$. If γ_{kh} is negative, the marginal unemployment effect of institution X^k will be larger, the lower the value of X^h, *i.e.* the more employment-friendly is the other institution X^h. In other words, the lower X^h, the greater the potential employment gain from reforms reducing the level of X^k.

Against this background, we first explore the robustness of individual interactions by augmenting our baseline model (equation [1]) with any possible interaction among the pairs of policies considered in the baseline, taken one by one (equation [2], and Column 1 of Table 4 for the results). We then consider two alternative strategies: i) an instrumental variable (IV) approach, where any interaction between institutions X^k and X^h is instrumented with the product of the deviations of X^k and X^h from their respective country-specific means;[9] and ii) an augmented version of each OLS specification, including all interactions of X^k and X^h with country dummies – equivalent to assuming that coefficients of X^k and X^h are country-specific. Results from both approaches are reported in Table 4, Columns 2 and 3, with IV estimates being presented only when the corresponding instrument is found to be acceptable using standard criteria.[10] Only the negative interaction between the average unemployment benefit replacement rate and union density appears to be robust across all estimation methods. Finally, as an additional robustness check, we re-estimate the baseline model by augmenting it with all possible combinations of *two* interactions among pairs of institutions included in our baseline model (including all implicit interactions, where applicable). Again, the interaction between the average replacement rate and union density turns out to be the only one significant in all specifications where it is included (irrespective of the estimation method).

Table 4 here

Taken at face value, the evidence provided in Table 4 is not strongly supportive of the hypothesis that reforms reinforce each other – at least in the form of multiplicative interactions among pairs of institutions. No interaction among observable policies appears to be robust across our sensitivity exercises, except for the interaction between the average replacement rate and union density, whose negative sign is hard to explain. However, lack of robustness does not necessarily imply that institutions do not interact. One issue is that small sample size might prevent the emergence of significant patterns. Most importantly, the above approach may be too narrowly focused on specific policy interactions, while certain theoretical studies (*e.g.* Coe and Snower, 1997) suggest in fact that each policy interacts with the *overall* policy and institutional framework and most structural reforms are complementary. We explore this possibility in the next section.

IV. REFORM COMPLEMENTARITIES

Let us go back to our simple graphical representation of the WS-PS model to illustrate through one important example the case for broad policy interactions and reform complementarities. Insofar as the PS curve is approximately iso-elastic, *i.e.* convex in the real-wage employment space, the marginal impact on labour demand of a given change in real wages declines with the employment level. As a result, the employment impact of any labour market reform that shifts the WS curve downwards (*e.g.* a cut in unemployment benefits) is greater the higher the initial level of employment (Figure 3). In other words, the more (less) employment-friendly the overall policy and institutional framework, the greater (smaller) the impact of a given reform is likely to be. Therefore, interactions are "systemic" and structural reforms complementary, in the sense that the combined effect of several employment-friendly reforms is greater than the sum of the effects of each of them undertaken in isolation.

Figure 3 here

As already noted, however, interactions among many variables cannot be analysed by means of a standard general model, since the latter would easily be overfitted.[11] As a way to overcome this

[9] See the appendix for a discussion of the validity of this instrument.
[10] Following the "rule of thumb" of Staiger and Stock (1997), the instrument is considered to be acceptable when the F test on the significance of the instrument is greater than 10.
[11] For instance, a general unrestricted model allowing all interactions among observables included in equation [1] above would already result in the inclusion of 57 additional variables (since $C_{6,2}+C_{6,3}+C_{6,4}+C_{6,5}+C_{6,6} = 57$, where $C_{n,k}$ stands for the combinatorial of *k* out of *n*).

problem, we follow here an alternative approach that is consistent with Figure 3:[12] we estimate a more compact, non-linear specification where each institution is interacted with the overall institutional framework, defined as the sum of the direct unemployment effects of institutions:

$$U_{it} = \sum_j \beta_j X_{it}^j + \sum_k \left(\gamma_k \left(X_{it}^k - \bar{X}^k \right) \left(\sum_j \beta_j \left(X_{it}^j - \bar{X}^j \right) \right) \right) + \chi G_{it} + \alpha_i + \varepsilon_{it} \qquad [3]$$

where we simultaneously estimate the parameters β_j and γ_k by Non-linear Least Squares. β_j denotes the direct effect of institution X^j at the sample average, *i.e.* for a country with an average mix of institutions, while γ_k indicates the strength of the interaction between X^k and the overall institutional framework. The latter is captured by the sum of direct effects of policies and institutions ($\sum_j \beta_j \left(X_{it}^j - \bar{X}^j \right)$, expressed in deviation form in the interaction). Following the above discussion, additional interactions involving country-fixed effects are also included in the specification in order to avoid potential estimation bias resulting from the correlation of certain institutions with unobserved time-invariant unemployment determinants.[13]

As in the analysis of individual interactions undertaken above (equation [2]), for any policy X^k that increases unemployment, a negative and significant coefficient γ_k would provide evidence of reform complementarity, in the sense that any reform that reduces X^k would have a larger impact the more employment-friendly the overall policy stance.

Table 5 shows the estimation results obtained when allowing for such systemic interactions. Column 1 presents the general model, while Column 2 provides the final specification obtained after sequential elimination of insignificant interactions. Three main results stand out. First, compared with the baseline unemployment equation (Table 1), taking systemic interactions into account affects some of the direct effects of policies and institutions estimated for the average country. The coefficients of unemployment benefits and product market regulation are virtually unchanged, but the impact of the tax wedge is reduced by half, and both EPL and union density are now positive and significant, although the estimated effect of EPL is small if the size of the "historically typical" reform is taken into account (0.25 points). In addition, a high degree of corporatism is now found to raise unemployment when evaluated at the sample mean, even though this result is not robust across specifications.[14]

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[12] More formally, equation [3] below is based upon the assumption that that: i) labour demand is close to be iso-elastic; and, ii) policy reforms are such that do not excessively modify the slope of the WS curve but rather entail a parallel shift of it.

[13] This implies that the specification actually estimated is slightly more complex than [3], and is: $U_{it} = \sum_j \beta_j X_{it}^j + \sum_k \left(\gamma_k \left(X_{it}^k - \bar{X}^k \right) \left(\sum_j \beta_j \left(X_{it}^j - \bar{X}^j \right) \right) \right) + \sum_h \left(\mu_h \left(I_i^h - \bar{I}^h \right) \left(\sum_j \beta_j \left(X_{it}^j - \bar{X}^j \right) \right) \right) + \chi G_{it} + \alpha_i + \varepsilon_{it}$ where I_i^h is a country dummy variable – which takes value 1 in country *h* and 0 otherwise – and μ_h is a parameter to be estimated. This approach mirrors that one considered in Column 3 of Table 4. IV approaches such as those implemented in Column 2 of Table 4 have not been attempted here for computational problems associated with the maximisation of the joint likelihood function. It might also be argued that country fixed effects contribute to the determination of structural unemployment and should therefore be added to the sum of direct effects in the interaction term. Yet, this route is not followed here, both for parsimony and because of lack of convergence of the related algorithm. However, specifications where fixed effects are added to the sum of direct effects in the interaction term, while the term $\sum_h \left(\mu_h \left(I_i^h - \bar{I}^h \right) \left(\sum_j \beta_j \left(X_{it}^j - \bar{X}^j \right) \right) \right)$ is dropped, yield qualitatively similar – albeit somewhat less significant – results. Likewise, time dummies are not included in the estimated equation.

[14] As a robustness check, the specification of Column 2 was re-estimated excluding the high corporatism dummy variable in the sum of direct effects of institutions that is included in the interaction. This exercise aims at checking that the results do not hinge on the statistical treatment of corporatism which, as a dummy variable with little variation over time, has a somewhat particular status in the regressions. The results obtained are similar to those in Column 2 of Table 5, except that the direct impact of high corporatism becomes insignificant.

Table 5 here

Second, all significant interactions are negative, lending some support to the reform complementarity hypothesis. From a quantitative viewpoint, however, the gains from simultaneously implementing more than one reform are found to be moderate for the average OECD country. As an illustration, in Table 6 we use the specification in column 2 of Table 5 to simulate the additional gain from undertaking jointly two large reforms that would each reduce the unemployment rate by 1 percentage point if implemented separately.[15] Concretely, we simulate the impact of all possible combinations of reductions in the tax wedge, the average benefit replacement rate, union density and product market regulation by 6.7 percentage points, 5.6 percentage points, 12.6 percentage points and 3.3 standard deviations, respectively. These are large reforms by historical standards. All combinations are found to reduce the unemployment rate by between 2.25 and 2.37 percentage points for the average OECD country, instead of the 2 percentage points that would prevail in the absence of reform complementarity. In other words, according to this simulation exercise, reform complementarities would amplify the unemployment effects of separate reforms by between 12% and 19%. Interestingly, the largest effect is obtained by combining reforms of the average replacement rate with reductions in union density, consistent with results from the previous section.

Table 6 here

Third and finally, in contrast with what usually occurs with linear models (see Bassanini and Duval, 2006b, and Carlin and Soskice, 2008, for a discussion), the model with systemic interactions appears to account well for unemployment trends over the sample period 1982-2003 for virtually all countries (Figure 4). In fact, this model is estimated to explain 92% of the cross-country variance of unemployment changes between 1982 and 2003, against 74% only for the baseline model.

Figure 4 here

Overall, the results of this section suggest that reform packages are likely to yield greater employment gains than separate, "piece-meal" reforms. Indeed, the impact of a given policy reform appears to be greater the more employment-friendly the overall institutional framework, so that *any* reform that lowers unemployment is likely to be complementary with *all* reforms that go in the same direction. However, the magnitude of such systemic reform complementarities is found to be moderate for the average OECD country.

V. CONCLUSIONS

In this paper we estimate a standard model of institutional determinants of unemployment. We find that, for the average OECD country, high and long-lasting unemployment benefits, high tax wedges and stringent anti-competitive product market regulation (PMR) increase aggregate unemployment. Conversely, highly centralised and/or coordinated wage bargaining systems appear to dampen it. We present an extensive sensitivity analysis showing that our results are robust to model specification, choice of estimation sample and estimation techniques.

We warn, however, that our inferences are to be viewed only as referring to an average OECD country. For example, in a companion paper (Bassanini and Duval, 2006a), we show that the positive impact of unemployment benefits on unemployment diminishes and can even collapse in countries that offset their detrimental effects through extensive active labour market policies. More broadly, the impact of a given policy reform appears to vary depending on the institutional context, tending to be greater the more employment-friendly the overall institutional framework. The fact that employment-enhancing structural reforms reinforce each other suggests that well-designed reform packages yield greater employment gains than separate, "piece-meal" reforms. The magnitude of such reform complementarities appears to be moderate for the average OECD country, however.

[15] As the gain is larger the larger the extent of the reforms, we simulate the complementarity effect for large reforms in historical perspective, in order to show that its magnitude is small.

Despite this evidence supporting broad reform complementarities, no firm conclusions can be drawn as regards the impact of specific, individual interactions across institutions which have been singled out by previous empirical literature. Such lack of robustness reflects two main factors which so far have received only little attention in the literature. First, while theory clearly suggests that all interactions are possible and should therefore be studied simultaneously, this is not feasible in practice due to small sample size. Second, many apparently significant interactions become insignificant or even change sign when their potential endogeneity is taken into account. This suggests that one should avoid drawing firm conclusions from simple models featuring only a few *ad hoc* interactions. From this perspective, more comprehensive analysis of interactions through the estimation of non-linear models such as those presented in this paper might be more informative, at least of relationships prevailing at the sample average.

APPENDIX

In section 3, we instrument each interaction of the type $\left(X_{it}^{k}-\bar{X}^{k}\right)\left(X_{it}^{h}-\bar{X}^{h}\right)$ with $\left(X_{it}^{k}-\bar{X}_{i}^{k}\right)\left(X_{it}^{h}-\bar{X}_{i}^{h}\right)$, where $\bar{X}_{i}^{k}$ and $\bar{X}_{i}^{h}$ stand for the country-specific means of X^{k} and X^{h}. This can be viewed as a "quasi Hausman-Taylor" IV approach. Hausman and Taylor (1981) note that the deviation of a variable from its country-specific mean is a valid instrument for that variable when correlation with time-invariant factors is the main source of endogeneity. In fact, this deviation is uncorrelated with any time-invariant unobservable variable by construction. In the approach followed here, the necessary orthogonality conditions for the validity of the instrument are of the type

$$E\left(\left(X_{it}^{k}-\bar{X}_{i}^{k}\right)\left(X_{it}^{h}-\bar{X}_{i}^{h}\right)X_{i}^{s}X_{it}^{h}\right)=0,$$

where $\bar{X}_{i}^{j}$ stands for the country-specific mean of X^{j}, X_{i}^{s} for the time-invariant unobservable variable and E for the mathematical expectation. These conditions are met if

$$E\left(\left(X_{it}^{k}-\bar{X}_{i}^{k}\right)X_{i}^{s}\middle|X_{it}^{h}\right)=0$$

and

$$E\left(\left(X_{it}^{k}-\bar{X}_{i}^{k}\right)X_{i}^{s}\bar{X}_{i}^{h}\middle|X_{it}^{h}\right)=0,$$

which does not appear too stringent if one takes into account that the unconditional moments $E\left(\left(X_{it}^{k}-\bar{X}_{i}^{k}\right)X_{i}^{s}\right)$ and $E\left(\left(X_{it}^{k}-\bar{X}_{i}^{k}\right)X_{i}^{s}\bar{X}_{i}^{h}\right)$ are equal to zero by construction.

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Table 1. Institutional determinants of unemployment: annual data 1982-2003

	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
	Baseline (Excluding DEU-FIN-SWE 91-92, common OG, including fixed effects, estimated by OLS)	= 1 without OG	= 1 with country specific OG	= 1 but substituting net for gross replacement rates	= 1 including DEU-FIN-SWE 91-92 without adjustments	= 1 excluding all observations for DEU-FIN-SWE	= 1 estimated by FGLS random effects[1]	= 1 estimated by FGLS fixed effects with country-wise hetero-skedasticity and AR1 serial correlation	= 1 using 5-year averaged data	= 9 estimated by difference GMMs with endogenous variables lagged two periods as instruments[2]
Replacement rate	0.12	0.13	0.09	0.05	0.08	0.13	0.10	0.06	0.13	0.21
	[6.28]***	[5.78]***	[4.58]***	[3.73]***	[4.33]***	[6.91]***	[6.24]***	[2.87]***	[1.65]*	[2.15]**
Tax wedge	0.28	0.33	0.26	0.23	0.30	0.29	0.26	0.13	0.33	0.30
	[9.75]***	[9.20]***	[7.94]***	[7.45]***	[10.69]***	[9.56]***	[10.98]***	[5.25]***	[2.81]***	[3.72]***
Union density	-0.03	0.01	-0.01	-0.05	-0.01	-0.03	-0.05	-0.01	-0.04	0.03
	[1.57]	[0.22]	[0.59]	[2.45]**	[0.51]	[1.50]	[3.10]***	[0.04]	[0.70]	[0.42]
EPL	-0.31	-0.38	0.02	-0.81	-1.41	-0.04	-0.66	-0.42	-0.49	-1.11
	[0.98]	[0.95]	[0.05]	[2.50]**	[4.09]***	[0.11]	[2.07]**	[1.17]	[0.33]	[0.58]
PMR	0.60	0.69	0.59	0.51	0.51	0.73	0.69	0.41	0.46	1.08
	[2.98]***	[2.65]***	[2.74]***	[2.42]**	[2.36]**	[5.81]***	[3.45]***	[1.99]**	[0.52]	[0.91]
High corporatism	-1.42	-2.00	-1.42	-0.92	-1.53	-1.47	-1.43	-1.51	-1.42	-4.96
	[3.57]***	[4.22]***	[3.79]***	[2.35]**	[3.86]***	[3.54]***	[4.13]***	[3.56]***	[1.14]	[2.53]**
Int. corporatism							-1.23			
							[0.72]			
Output gap	-0.48			-0.50	-0.54	-0.47	-0.49	-0.39	-0.36	
	[14.00]***			[13.06]***	[14.00]***	[13.54]***	[14.37]***	[13.37]***	[2.44]**	
Country effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	n.a.
Country effects*OG	no	no	yes	no	no	no	no	no	no	no
Time dummies	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	434	434	434	367	440	374	434	434	80	80
R-squared	0.98	0.97	0.98	0.98	0.97	0.98			0.97	

Absolute value of t statistics in brackets. Robust t statistics except for FGLS estimates.
* significant at 10%; ** significant at 5%; *** significant at 1%. n.a.: not applicable. OG: output gap.

1: The joint Hausman test is not reported since the difference between the parameter variance-covariance matrices of fixed and random effects specifications is not positive definite. Yet, single parameter Hausman tests are significant in the case of EPL, PMR and output gap, thereby suggesting that random effects estimates are not consistent.
2: One-step difference GMM robust estimates. The error term is modeled as an ARMA process with an AR(1) component. All institutions except corporatism are treated as endogenous variables. The common factor restriction is not imposed. Only long-run effects are presented. Levels of endogenous variables dated t-2 and earlier are used as instruments in the difference equation. The P-value of the Hansen test of overidentification is 0.99. Arellano-Bond tests for AR serial correlation in the differenced residual are -3.11 (first-order) and -0.93 (second-order).

Table 2. **Hausman tests of the poolability hypothesis**

P-values

	Baseline	Baseline, without OG	Baseline, country-specific OG
Institutions	0.0555	0.1274	0.1101
Output gap	0.0011		
Total	0.0004	0.1274	0.1101

Hausman tests comparing mean-group estimates with fixed effect estimates. Common time dummies are included in all specifications. "Total" indicate the joint Hausman test for the hypothesis that all parameters are homogeneous across countries. A significant test statistic implies rejection of the poolability hypothesis.

Table 3. **Granger causality tests**

	Effect of institutions on unemployment		Effect of unemployment on institutions	
	Long-run effect	Causality test (robust F-statistics)	Long-run effect	Causality test (robust F-statistics)
Replacement rate	0.13 [4.35]***	14.37***	0.24 [0.19]	0.04
Tax wedge	0.26 [4.95]***	20.01***	0.55 [0.53]***	0.29

***: significant at the 1% level. Robust t-statistics in brackets. Based on estimated models with 2 lags of the unemployment rate, replacement rate, tax wedge, EPL and union density. Models include also PMR, one high corporatism dummy, country dummies and time dummies. The long-run effect is the derived long-run coefficient of the model. Only long-run effects of selected variables are reported. The causality test is the F-statistics on the joint significant of the two lagged term of an explanatory variable. A significant F-statistic for the causality tests indicates evidence supporting a causal impact.

Interpretation : The table shows that a 1 percentage point increase in the average replacement rate raises the unemployment rate by 0.13 percentage points in the long-run in the average country, and this effect is significant and causal. By contrast, a 1 percentage-point increase in the unemployment rate is estimated to raise the average replacement rate by 0.24 percentage points, but this estimate does not appear to reflect any significant long-run effect.

Table 4. **Simple interactions across institutions, 1982-2003**

	OLS	IV [1]	F-Test on instrument[2]	OLS with country-specific variables [3]
Average replacement rate * Tax wedge	0.003 ***	. .	*0.6*	-0.023 ***
Average replacement rate * Union density	-0.002 ***	-0.009 ***	*65.1*	-0.006 ***
Average replacement rate * EPL	0.023 *	. .	*2.2*	0.081
Average replacement rate * PMR	0.008	. .	*3.4*	0.040
Average replacement rate * High corporatism	-0.009	0.042	*32.7*	-0.042
Tax wedge * Union density	-0.001	-0.006	*27.4*	0.001
Tax wedge * EPL	0.009	. .	*0.2*	-0.512 ***
Tax wedge * PMR	0.033 ***	-0.045	*34.1*	0.022
Tax wedge * High corporatism	0.050 *	0.037	*30.0*	-0.335 ***
Union density * EPL	-0.004	-0.004	*16.2*	-0.362 **
Union density * PMR	-0.004	0.023	*13.0*	-0.040 **
Union density * High corporatism	-0.013	0.164 ***	*159.8*	0.115
EPL * PMR	-0.111	-1.076 **	*17.3*	-0.272
EPL * High corporatism	-0.150	. .	*9.6*	-1.365
PMR * High corporatism	-0.410 **	. .	*3.2*	0.301

Notes:

The table reports the interaction cofficients of baseline specifications augmented by one interaction at a time.

. .: IV estimates are not reported when the instrument is weak according to the Stock-Staiger rule (F < 10).

1. 2SLS estimates. Any interaction X*Y is instrumented with the product of the deviations of X and Y from their country-specific means.
2. F test statistic on the significance of the instrument in the first-stage regression.
3. For any interaction X*Y, the specification is augmented by the interactions of both X and Y with country dummies and estimated by OLS.

*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.

Table 5. **Systemic Interactions across institutions, 1982-2003**

	1	2
	including all possible interactions	= 1 after sequential elimination of insignificant interactions
β : Direct effect of institutions :		
Average replacement rate	0.11	0.12
	[6.71]***	[7.58]***
Tax wedge	0.15	0.16
	[6.45]***	[7.01]***
EPL	0.38	0.47
	[2.43]**	[3.43]***
Union density	0.06	0.07
	[4.39]***	[4.90]***
PMR	0.46	0.47
	[6.29]***	[6.54]***
High corporatism	0.46	0.70
	[1.55]	[3.07]***
γ : Interactions between institutions and the sum of direct effects Σj βjXj :		
Average replacement rate	-3.29	-3.67
	[3.64]***	[4.33]***
Tax wedge	-1.24	-1.56
	[1.62]	[2.20]**
EPL	13.41	
	[1.13]	
Union density	-1.61	-1.59
	[2.22]**	[2.44]**
PMR	-11.72	-10.40
	[2.71]***	[2.86]***
High corporatism	29.15	
	[1.15]	
Country dummies	yes	yes
Country dummies interacted with Σj βjXj	yes	yes
Time dummies	no	no
Output gap	yes	yes
Observations	434	434
R-squared	0.96	0.96

Non-linear least squares. Absolute value of t statistics in brackets.
*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.

Table 6 **Simulated effect of reform complementarities**

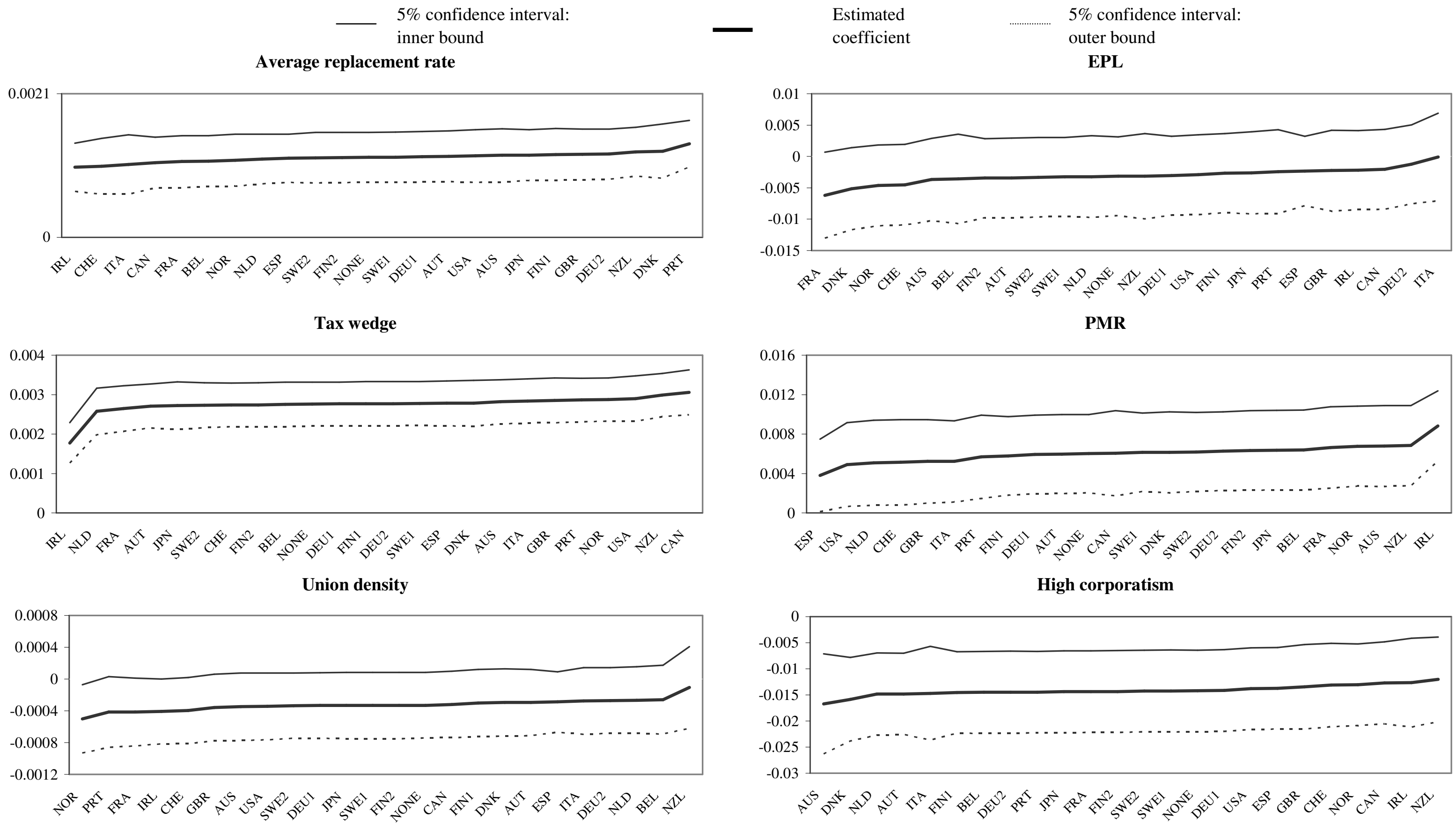
	Av. repl. rate	tax wedge	union density	PMR
Av. repl. rate				
tax wedge	-0.30			
union density	-0.37	-0.26		
PMR	-0.36	-0.25	-0.33	

Note: The table shows the reduction in unemployment (in percentage points) that would be obtained from the combined reform of each pair of institutions, in excess of the sum of the unemployment reductions implied by each reform taken in isolation. As a standardisation, reforms are set in such a way that each of them, taken in isolation, would bring about a 1 percentage point drop in the unemployment rate for the average country. Column 2 of Table 5 is used as the basis for the simulation.

Interpretation: a *combined* decline in the tax wedge and the unemployment benefit replacement rate brings about an additional 0.3 percentage point decline in the unemployment rate, over and above the 2 percentage point reduction associated with the *direct* effects- i.e. omitting reform complementarities of these reforms.

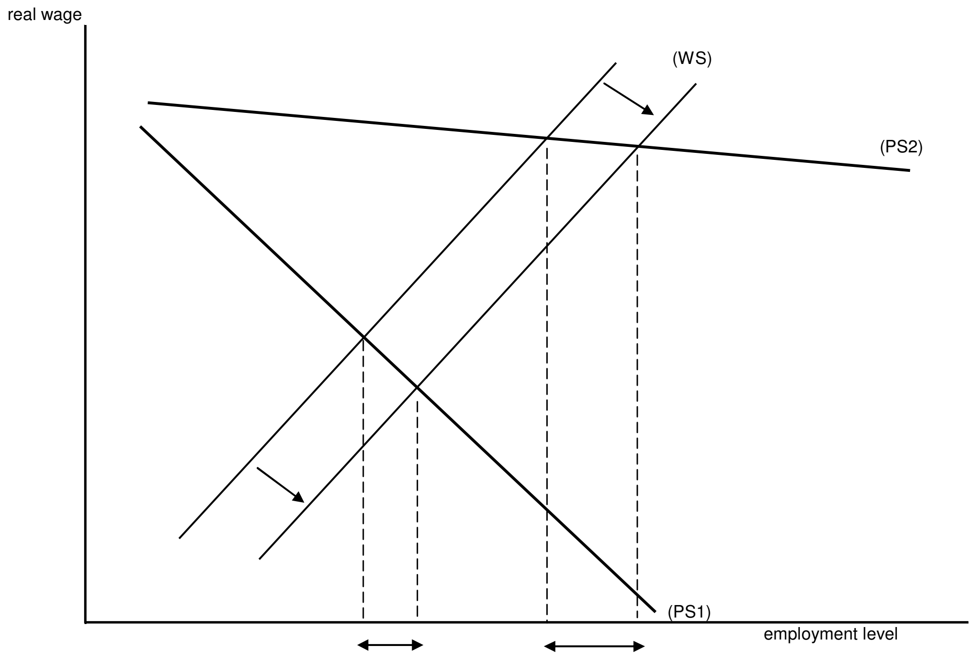
Figure 1. **Sensitivity analysis by country**

Coefficients and confidence intervals at 5% level



The figure shows central estimates and confidence intervals obtained by re-estimating the baseline specification after excluding one country at a time from the sample. 1 and 2 indicates pre- and post- shock periods for Germany, Finland and Sweden. NONE identifies the baseline for the purpose of comparison.

Figure 2. **Simple interactions in a WS-PS model**



Note: The figure shows the employment effect of a policy reform shifting the WS curve for two different elasticities of the PS curve

Figure 3**. The effect of shifting the WS curve when the PS curve is iso-elastic**

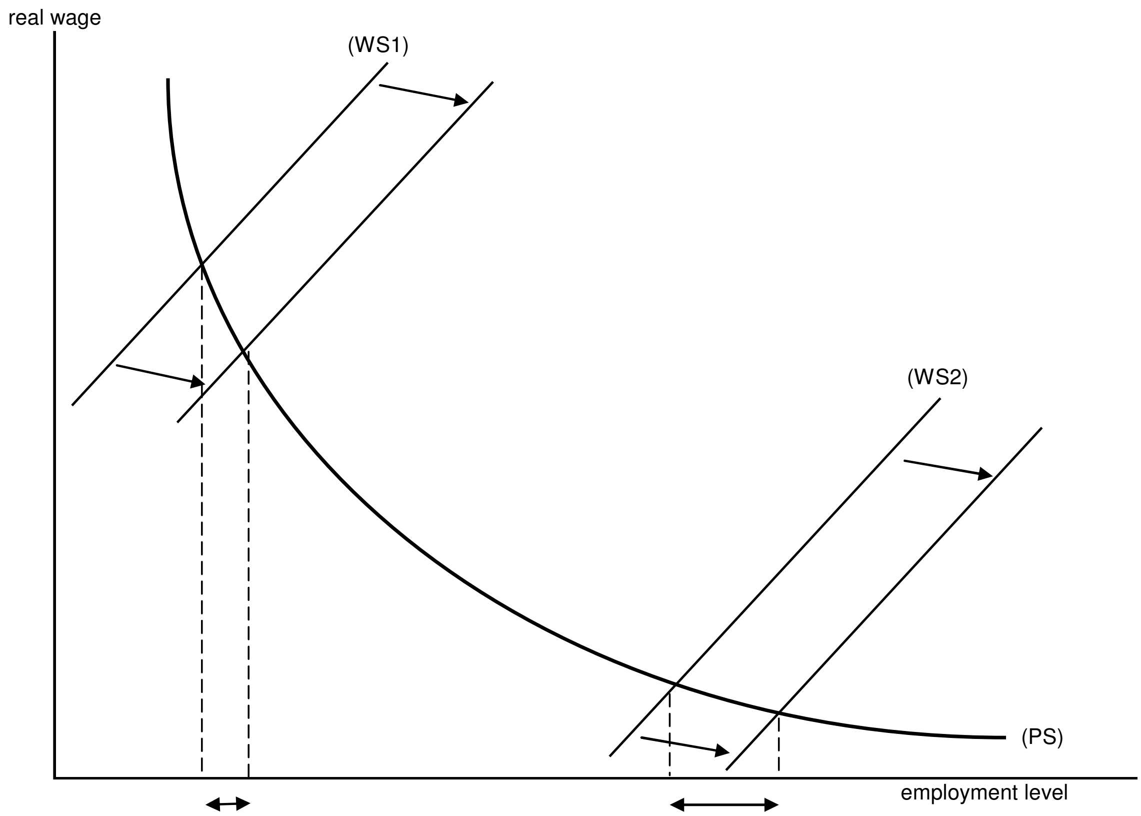
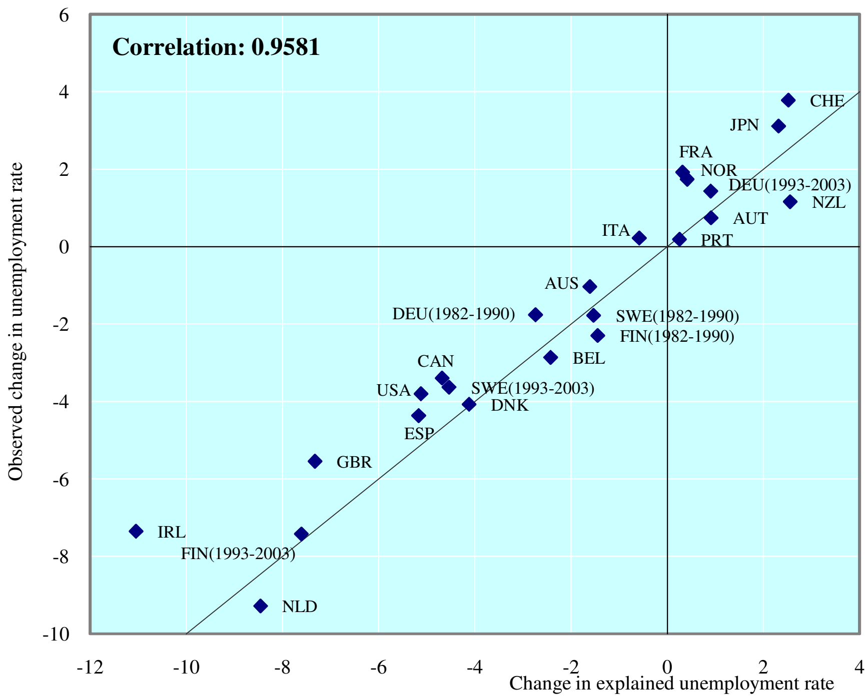


Figure 4. **Observed and explained unemployment changes, 1982-2003**

Systemic interactions, percentage points



Note: Estimates on the basis of estimates in Table 5, column 2.