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EMPLOYMENT PATTERNS IN OECD COUNTRIES: REASSESSING THE ROLE OF POLICIES AND INSTITUTIONS

By Andrea Bassanini and Romain Duval

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Abstract

This paper explores the impact of policies and institutions on employment and unemployment of OECD countries in the past decades. Reduced-form unemployment equations, consistent with standard wage setting/price-setting models, are estimated using cross-country/time-series data from 21 OECD countries over the period 1982-2003. In the "average" OECD country, high and long-lasting unemployment benefits, high tax wedges and stringent anti-competitive product market regulation are found to increase aggregate unemployment. By contrast, highly centralised and/or coordinated wage bargaining systems are estimated to reduce unemployment. These findings are robust across specifications, datasets and econometric methods. As policies and institutions affect employment not only *via* their impact on aggregate unemployment but also through their effects on labour market participation - particularly for those groups "at the margin" of the labour market, group-specific employment rate equations are also estimated. In the "average" OECD country, high unemployment benefits and high tax wedges are found to be associated with lower employment prospects for all groups studied, namely prime-age males, females, older workers and youths. There is also evidence that group-specific policy determinants matter, such as targeted fiscal incentives. The paper also finds significant evidence of interactions across policies and institutions, as well as between institutions and macroeconomic conditions. Consistent with theory, structural reforms appear to have mutually reinforcing effects: the impact of a given policy reform is greater the more employment-friendly the overall policy and institutional framework. Certain more specific interactions across policies and institutions are found to be particularly robust, notably between unemployment benefits and public spending on active labour market programmes as well as between statutory minimum wages and the tax wedge. Finally, it is shown that macroeconomic conditions also matter for unemployment patterns, with their impact being shaped by policies.

JEL: J38, J58, J68
Keywords: employment; unemployment; institutions; shocks; reform complementarities.

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Résumé

Cet article explore l'impact des politiques et des institutions sur l'emploi et le chômage dans les pays de l'OCDE au cours des dernières décennies. Des équations réduites de taux de chômage, telles que dérivées par exemple d'un modèle de négociations salariales, sont estimées sur un panel de 21 pays de l'OCDE sur la période 1982-2003. Il ressort que, dans le pays « moyen » de l'OCDE, le taux moyen de remplacement des indemnités chômage, le coin fiscalo-social et le degré de réglementation des marchés de produits augmentent le taux de chômage structurel. A contrario, il apparaît qu'un haut degré de centralisation/co-ordination des négociations salariales réduit le chômage structurel. Ces résultats sont robustes à des changements de spécification, d'échantillon et de méthode d'estimation économétrique. Étant donné que les politiques et les institutions affectent l'emploi non seulement *via* leur impact sur le chômage mais aussi au travers de leurs effets sur la participation au marché du travail –en particulier pour les groupes « à la marge » du marché du travail, des équations d'emploi par groupes sont également estimées. Il ressort que dans le pays « moyen » de l'OCDE, le taux de remplacement des indemnités chômage et le coin fiscalo-social réduisent les perspectives d'emplois de chacun des groupes étudiés, à savoir les hommes de 25 à 55 ans, les femmes, les travailleurs âgés et les jeunes. Certains déterminants spécifiques à chaque groupe jouent également un rôle, en particulier les incitations fiscales ciblées. Le document conclut également à l'existence d'interactions significatives entre politiques et institutions, de même qu'entre institutions et chocs. En accord avec la théorie, il ressort également que les effets des réformes structurelles se renforcent mutuellement : l'impact d'une réforme donnée est d'autant plus fort que l'ensemble des autres politiques et institutions sont déjà favorables à l'emploi. Certaines interactions plus spécifiques entre politiques apparaissent particulièrement robustes, notamment entre le taux de remplacement des indemnités chômage et les dépenses en politiques actives en faveur de l'emploi, ou encore entre le salaire minimum et le coin fiscalo-social. Enfin, l'article montre que les conditions macro-économiques affectent le chômage et que cet impact dépend des politiques.

Classification JEL: J38, J58, J68

Mot-clefs: emploi; chômage; institutions; chocs; complémentarité entre réformes.

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EMPLOYMENT PATTERNS IN OECD COUNTRIES: REASSESSING THE ROLE OF POLICIES AND INSTITUTIONS

by ANDREA BASSANINI and ROMAIN DUVAL[*]

Summary and main findings

1. The 1994 OECD *Jobs Strategy* stressed the need for fundamental labour market reforms to deal with high and persistent unemployment that affected many member countries (OECD, 1994a, 1997). The case for policy reforms made in the 1994 *Jobs Strategy* was based on a careful scrutiny of the evidence (both qualitative and quantitative) available at the time. Since then, empirical research on the topic has improved on two fronts. First, microeconometric techniques have evolved and new evidence has become available, as reflected in the 2006 edition of the *OECD Employment Outlook* (OECD, 2006a). Second, the OECD Secretariat has constructed several indicators of policies and institutions that are comparable both across countries and over time. These indicators have been used in a wide range of macroeconometric studies to explore the labour market effects of policies and institutions. While the main policy conclusions from these studies have generally been consistent with the main thrust of the 1994 *Jobs Strategy*, some of the recommendations have also been challenged in some cases (see Section 1 and Annex 1 for references). The purpose of this paper is to reassess and expand the recent macroeconometric evidence, taking into account recent advances in both theoretical and empirical analysis, in order to provide further support for the evaluation presented in OECD (2006a) and the restated OECD Jobs Strategy – see OECD(2006b).[1]

2. The paper studies the impact of structural policies and institutions on aggregate unemployment and employment rates, the latter disaggregated by main labour market groups. To this end, heavy reliance is made of cross-country / time-series econometric techniques, which have been used extensively in the empirical literature over recent years. Yet, one distinguishing feature of this paper with respect to most of

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[1] A summary version of the paper is presented in Chapter 7 of OECD (2006a).

the existing literature is that particular care is taken throughout to assess and document what findings are robust and what are not.

3. Although the main focus of the paper is on labour market policies and institutions, issues that have emerged more recently in the literature are also covered, including the role of product market regulation, interactions among structural policies, and the effects of policies and institutions on economic resilience to macroeconomic shocks.

4. The paper is divided into two main sections. Section 1 looks at the determinants of structural unemployment in OECD countries. Stressing that sound labour market performance ultimately hinges on high employment prospects for all rather than on low unemployment *per se*, Section 2 analyses policy and institutional drivers of employment rates, paying particular attention to institutional determinants of labour market participation of youth, women and older workers.

5. The main findings are as follows:

- On average, changes in policies and institutions appear to explain almost two thirds of non-cyclical unemployment changes over the past two decades. In particular, high and long-lasting unemployment benefits, high tax wedges and stringent anti-competitive product market regulation (PMR) are found to increase aggregate unemployment. On average, it is estimated that a 10 percentage point reduction in the tax wedge, a 10 percentage point reduction of unemployment benefits and/or a decline in product market regulation by two standard deviations would be associated with a drop in the unemployment rate by about 2.8, 1.2 and 0.7 percentage points, respectively. By contrast, highly centralised and/or coordinated wage bargaining systems as well as some categories of public spending on active labour market programmes (ALMPs), such as labour market training, are estimated to be associated with lower unemployment. Extensive sensitivity analysis shows that these findings are robust across specifications, datasets and econometric methods.

- Policies and institutions affect employment not only *via* their impact on aggregate unemployment but also through their effects on labour market participation, particularly for those groups "at the margin" of the labour market. High unemployment benefits and high tax wedges are found to be associated with lower employment prospects for all groups. There is also evidence that group-specific policy determinants matter, such as targeted fiscal incentives. Yet, some caution is necessary when interpreting these latter findings, insofar as the empirical analysis of employment rates is not always as robust as that of unemployment.

- The precise impact of a given policy reform appears to vary depending on the institutional context, tending to be greater the more employment-friendly the overall policy and institutional framework. Indeed, *any* reform that lowers unemployment is likely to be complementary with *all* reforms that go in the same direction. This suggests that well-designed reform packages would yield greater employment gains than separate, "piece-meal" reforms, although the magnitude of such reform complementarities is found to be moderate for the average OECD country.

- Besides systemic interactions, a few specific interactions seem to be particularly robust: in particular, the impact of generous unemployment benefits on unemployment appears to be mitigated by high public spending on ALMPs, perhaps because high spending on ALMPs is often accompanied with strong emphasis on "activation". Also, the unemployment effects of high tax wedges are found to be largest in those countries where binding minimum wage floors prevent tax shifting to workers.

- In line with a number of previous studies, no significant impact of employment protection legislation (EPL) on aggregate unemployment is found. This finding appears to be consistent across different specifications and econometric techniques. However, effects appear to vary across labour market groups. Stringent EPL has negative effects on youth entry into the labour market, while it may benefit older workers, at least where retirement incentives are high (see below). Likewise, strict EPL is associated with a substitution of part-time for full-time female work.

- Among the policies and institutions that affect the job prospects of those groups "at the margin" of the labour market, tax incentives appear to play an important role. High implicit taxes on continued work embedded in old-age pension schemes and other social transfer programmes deter older workers from continuing to work beyond certain ages. Likewise, low tax incentives for part-time work are associated with lower female employment rates. Family-friendly policies also matter, with some evidence that childcare subsidies are preferable to child benefits from the point of view of raising female labour market participation.

- Finally, while policies and institutions appear to play a major role in shaping employment patterns, macroeconomic conditions also matter. Negative total factor productivity shocks, deteriorations in the terms of trade, increases in long-term real interest rates or negative labour demand shocks are all found to increase aggregate unemployment. Furthermore, there is clear evidence that their impact is shaped by existing policies and institutions. In particular, the effects of macroeconomic shocks appear to be amplified by high unemployment benefits and dampened by highly centralised and/or coordinated wage bargaining systems. More tentatively, high rates of home ownership – which are often associated with low degrees of labour mobility across regions – increase the unemployment impact of shocks, while public spending on ALMPs reduce it. By contrast, the effects of strict EPL or stringent PMR appear to be ambiguous. They seem to dampen the unemployment effects of shocks in the short run, while lengthening the adjustment period needed for unemployment to return to its initial level.

1. THE DETERMINANTS OF STRUCTURAL UNEMPLOYMENT

Introduction

6. Economic theory and previous empirical studies have identified a number of policy and institutional determinants of unemployment. These include *inter alia* unemployment benefits, taxes, trade union bargaining power and the structure of collective bargaining, employment protection legislation (EPL), anti-competitive product market regulation (PMR), active labour market policies (ALMPs), minimum wages and housing policies.

7. Overall, there is fairly robust evidence that the level and duration of unemployment benefits have a significantly positive impact on unemployment (Scarpetta, 1996; Nickell, 1998; Elmeskov *et al.*, 1998; Nunziata, 2002). Likewise, a number of empirical studies have found that high labour taxes tend to increase unemployment rates (Belot and van Ours, 2004; Nickell, 1997), although other studies are less conclusive (Scarpetta, 1996; Nunziata, 2002; Macculloch and DiTella, 2002). Some macroeconometric studies also identify a favourable effect of ALMP spending and an adverse impact of home ownership on

aggregate unemployment but fail to agree on their magnitudes (*e.g.* Scarpetta, 1996; Nickell, 1997, 1998; Green and Hendershott, 2001, Boone and van Ours, 2004, Nickell *et al.,* 2005).

8. There is less consensus in the literature on the unemployment effects of EPL, trade union bargaining power, the structure of collective bargaining. Finally, there is only scant macroeconometric evidence on the employment effects of product market regulation. Among the few studies on this issue, Nicoletti *et al*. (2001) and Nicoletti and Scarpetta (2005) both find that product market reforms improve labour market performance. Annex 1 provides a more comprehensive theoretical and empirical survey of the impact of policies and institutions on employment.

9. This section looks at the impact of structural policies and institutions on aggregate unemployment, by means of cross-country / time-series macroeconometric estimation of unemployment models. It is divided into four subsections. Section 1.1 reassesses existing evidence by means of new panel data econometric estimates spanning the past two decades. Particular emphasis is put on the unemployment effects of tax wedges, unemployment benefit systems, EPL, PMR, and wage-bargaining systems. Section 1.2 then explores the extent to which *interactions* across these policies and institutions matter. Section 1.3 brings into the picture additional institutions which for various reasons could not be incorporated earlier in the analysis, including public spending on ALMPs and statutory minimum wages. Section 1.4 then undertakes an analysis of how policies and institutions contribute to shape unemployment patterns not only directly but also indirectly *via* their interaction with macroeconomic shocks.

1.1. Panel data econometric analysis of the role of policies and institutions

10. In this section, pooled cross-country / time-series econometric analysis is used to explore the direct effects of policies and institutions on unemployment – *i.e.* omitting at this stage possible interactions between institutions as well as interactions between institutions and shocks. The main policy and institutional determinants of unemployment are introduced into a reduced-form unemployment equation that is consistent with a variety of theoretical models of labour market equilibrium, including standard job-search (Pissarides, 2000) and wage-setting/price-setting (*e.g.* Layard *et al.* 1991; Nickell and Layard, 1999) models. More specifically, the following static model is estimated for a sample of 20 OECD countries[2] over the period 1982-2003:

$$U_{it} = \sum_j \beta_j X_{it}^j + \chi G_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \qquad [1.1]$$

where i and t are country and time suffices, α_i and λ_t are country and time fixed effects,[3] U_{it} is the standardised rate of unemployment, and 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. Finally, the X^j's

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

3 Except in part of the sensitivity analysis, country effects are always included and modelled through deterministic dummy variables. The inclusion of country effects is necessary to control for country-specific averages of omitted policies and institutions. Since the policy and institutional indicators included in the analysis tend to be much more correlated across countries than within a given country and over time (see section 1.4 below), one can expect that the inclusion of country effects is sufficient to control for most of the relevant omitted variables. The choice of fixed rather than random country effects reflects the view that country effects are unlikely to be independent from other explanatory variables included in the estimated equation – in which case random-effects FGLS estimators would yield inconsistent estimates.

are OECD measures of the policies and institutions considered as explanatory variables, namely: [4] 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 (an average of replacement rates across various earnings levels, family situations and durations of unemployment); the degree of stringency of EPL; the average degree of stringency of PMR across seven non-manufacturing industries;[5] union membership rates; the degree of centralisation/co-ordination of wage bargaining, a proxy for the concept of "corporatism" which has received widespread attention in the comparative political economy literature. The rationale for including these explanatory variables in the equation is explained in Annex 1.

11. One important adjustment made to the data sample and specifications should be mentioned at the outset. In both the descriptive and panel data analyses below, observations for Finland, Germany and Sweden in 1990 and 1991 are removed from the sample, and different country fixed effects are used for each of these three countries over the two sub-periods 1982-1989 and 1992-2003. In practice, this approach is equivalent to splitting Finland, Germany and Sweden into two sub-countries, pre- and post-1990/1991. This reflects the view that for these three countries, neither the institutions considered in this section, nor the set of macroeconomic shocks which will be included at a later stage, are able to capture the highly country-specific factors – including *inter alia*, the collapse of the Soviet Union, the unification and the banking crises, respectively – which were behind the upward shift in unemployment over this two-year period. Therefore, keeping these six observations within the sample could increase the risk of estimate bias. Still, as will be shown below, the main conclusions from the analysis are not dependent on whether these observations are excluded from the sample.

Preliminary descriptive analysis

12. Table 1.1, Panel A, presents a series of simple pooled cross-country / time-series correlations between unemployment and each of the individual policies and institutions considered, excluding corporatism – which shows too little variance over time in most countries – but including an indicator of the "spending" effort on active labour market policies (total expenditures per unemployed worker as a percentage of GDP per capita). The panel shows significant positive correlations for the tax wedge, EPL and PMR, negative correlations for union density and ALMP, and almost no correlation in the case of unemployment benefits.

13. Such correlations are hard to interpret, however, as they are often dependent on few observations (for instance, significant correlations for EPL and union density are entirely due to the presence of Spain in the sample) and may be obscured by the presence of other, omitted determinants of unemployment. One possible way to mitigate this problem is to look at correlations between unemployment and policy or institutions purged from country and time fixed effects (see *e.g.* Daveri and Tabellini, 2000). This is done here by, first, estimating separate regressions of the type $U_{it} = \alpha_i + \lambda_t + \varepsilon_{it}$ and $X_{it}^j = \alpha_i + \lambda_t + \nu_{it}$, and, second, computing simple correlations between ε_{it} and ν_{it} Using this approach, unemployment is found to be positively and significantly correlated with the tax wedge, the average benefit replacement rate, the

4 Full details on data sources and methods are provided in Annex 2.

5 This PMR indicator is used here because it is available over the whole period 1975-2003 for most OECD countries, unlike the economy-wide indicator which covers only the period 1998-2003. One drawback is that changes in the PMR indicator for non-manufacturing industries do not incorporate all aspects of regulatory reforms that have been undertaken by a number of OECD countries in the past decades, such as administrative reforms affecting all sectors. As a result, the unemployment effects of regulatory reforms may not be fully captured by the econometric estimates presented in this paper.

OECD index of product market regulation and expenditures on active labour market policies (Table 1.1, Panel B).[6] However, no significant correlation is found between unemployment and EPL or union density.

[Table 1.1. A. Simple correlations between unemployment and selected institutions and policies B. same correlations purged from both country and time fixed effects, 1982-2003]

Baseline equation

14. Multivariate analysis yields results that are broadly similar to the second set of correlations (Table 1.2, column 1). Tax wedges, average benefit replacement rates and PMR are estimated to raise aggregate unemployment, while EPL and union density are statistically insignificant at conventional confidence levels.[7] The significant impact of the OECD summary measure of benefit replacement rates reflects the combined effect of the replacement rate during the first year of unemployment, the duration of benefit receipt and the interaction between these variables -- all of which are statistically significant (column 2). Furthermore, the degree of corporatism –a dummy variable whose value takes 1 when wage bargaining is highly centralised or co-ordinated and 0 otherwise[8]– is found to significantly reduce unemployment, lending some support to the view that, in centralised/co-ordinated bargaining systems, unions and employers are able to internalise the adverse employment consequences of excessive wage claims.

[Table 1.2. Baseline unemployment rate equation, 1982-2003]

15. The finding that the unemployment effects of both union density and EPL are statistically insignificant is not necessarily inconsistent with either theory[9] or empirical studies. However, this needs to be qualified. As discussed in OECD (2006a) and Annex 1, union density might poorly capture the actual bargaining power of workers. Indeed, in some countries, the coverage of collective agreements largely exceeds the number of trade union members –this reflects, *inter alia*, legal procedures and practices to extend collective contracts to unaffiliated workers, including those employed in non-signatory firms. Likewise, the absence of a significant impact of EPL on aggregate unemployment is in line with a number

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6 Not too much should be made of the significant and negatively signed correlation with expenditures on active labour market policies, since no allowance is made at this stage for the possibility of a reverse causal relationship. In fact, insofar as the measure of expenditures on active labour market policies has the number of unemployed in the denominator, it might tend to rise mechanically when unemployment declines, and *vice versa*.

7 The comparison with the correlations presented in Table 1.1, Panel B (that can be viewed as estimates of regression models identical to equation [1.1] but including just one policy or institution at a time) suggests that, contrary to what is sometimes argued in the literature (see *e.g.* Baccaro and Rei, 2005), results presented in Table 1.2 are not due to multicollinearity.

8 As already done in previous OECD work (*e.g.* Scarpetta, 1996, Elmeskov *et al*., 1998), dummies for different levels of corporatism are used here to capture non-linearities in the effect of corporatism. In order to do so, the quantitative indicator of "coordinated wage-bargaining" developed in OECD (2004) has been aggregated into three classes (low, intermediate and high, see Annex 2). In principle, the baseline specification includes a dummy for intermediate corporatism. However, being time-invariant within the sample, the effect of this variable is not identified (even if controlled for) and therefore is not reported in the following tables and charts.

9 In a standard wage-setting/price-setting model, both the effects of firing costs and union bargaining power depend on the parameterisation of the model. For instance, if an efficient bargaining model or a sufficiently concave utility function for unions are used, the short-term impact on unemployment of an increase in union bargaining power is zero or negative, respectively, while the long-run effects crucially depend on the assumptions on firm entry and exit (see *e.g.* Blanchard and Giavazzi, 2003).

of previous studies (see in particular OECD, 2004). But the insignificant coefficient may mask two opposite effects, with EPL on regular contracts exerting upward pressure on unemployment and EPL on temporary contracts pushing in the opposite direction (see column 3 of Table 1.2).[10] While supporting certain recent theoretical developments mentioned earlier, this latter finding should be viewed as highly fragile, as it hinges only on the presence of Spain – the country which undertook the deepest reforms of EPL for permanent workers over the period considered – in the sample.[11]

16. The measure of the tax wedge used in the baseline equation (column 1) is derived from OECD tax models and therefore only captures *labour* taxes (social security contributions and income taxes), but not consumption taxes.[12] A broader measure of the tax wedge, which covers both labour and consumption taxes, has been derived from National Accounts -- following the approach of Carey and Rabesona (2002) -- in order to check the robustness of the results. Such a National Accounts measure of the tax wedge is more likely to suffer from endogeneity problems and provides a cruder picture of the tax incentives effectively faced by individuals than the tax model measure of the tax wedge –which is why the latter is used throughout the present paper (with the exception of Section 1.4 below), despite being only a measure of the labour tax wedge. Re-estimating the baseline equation using the National Accounts measure yields similar coefficients for all the explanatory variables including the tax wedge itself (Table 1.2, column 4). Moreover, consistent with theoretical priors, no significant difference is found between the impact of labour and consumption taxes (Table 1.2, column 5).

17. The estimated coefficient of the output gap is highly significant in all specifications. This result points to the importance of cyclical unemployment patterns that can be explained by macroeconomic shocks.[13] In order to shed further light on this issue, the baseline equation is re-estimated by substituting a number of observable macroeconomic variables, or "shocks", for the output gap (Table 1.2, columns 6 and 7). In line with recent empirical literature, four types of "shocks" are considered for analysis: [14]

- *Total factor productivity* (TFP) shocks, defined here as the deviation of the logarithm of TFP from its trend calculated by means of a Hodrick-Prescott filter. In the presence of lagged wage adjustment to productivity growth, positive (negative) productivity surprises –as measured here by a positive gap between actual and trend TFP– should induce a temporary decline (increase) in structural unemployment (see *e.g.* Ball and Moffitt, 2002; Meyer, 2000). [15]

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10 The OECD indicator of EPL comprises two main components, namely EPL on temporary contracts and EPL on permanent contracts. It has been suggested that it is the latter that exerts adverse effects on unemployment (see Annex 1). However, when only EPL on permanent contracts is included in the specification, its estimated coefficient remains insignificant.

11 The result obtained for Spain is consistent with previous analysis by Bentolila and Dolado (1994).

12 The source is the OECD Taxing Wages Database (see Annex 2), which defines it as the wedge between the labour cost to the employer and the corresponding net take-home pay of the employee for a single-earner couple with two children earning 100% of APW earnings. The tax wedge expresses the sum of personal income tax and all social security contributions as a percentage of total labour cost.

13 However, this result must be interpreted with caution due to the endogeneity of the output gap.

14 Data sources and methods used to construct these shocks are discussed in detail in Annex 2.

15 Also, in job-search models of the labour market, if technological progress is not embodied in new jobs – *i.e.* if it can materialise without existing jobs being destroyed and replaced by new ones, faster productivity growth increases the value of jobs through "capitalisation effects" (Pissarides, 2000). The latter reflect the fact that firms pay the cost of job creation upfront and recover it later from the revenues generated over the lifetime of the job. These "capitalisation effects" increase labour demand and reduce unemployment. Conversely, if technological progress is embodied in new jobs, faster productivity growth

- *Terms of trade* shocks, defined as the ratio of imports to output multiplied by the logarithm of their relative prices ($(M/Y) \log (P_M / P_Y)$). *i.e.* in such a way that its growth rate is the change in the relative price of imports weighted by the share of imports in GDP. By widening the wedge between consumer and producer prices, a rise in the relative price of imports should increase wage pressure and, ultimately, unemployment (see e.g. Layard *et al.*, 1991).

- *Real interest rate* shocks, defined as the difference between the 10-year nominal government bond yield and the annual GDP price inflation. A rise in real interest rates affects negatively capital accumulation and labour productivity, thereby reducing labour demand (at a given wage level) and increasing unemployment (see *e.g.* Blanchard, 1999, 2000).

- In some specifications, *labour demand* shocks, defined as the logarithm of the labour share in business-sector GDP purged from the short-run influence of factor prices.[16] As discussed in Blanchard (1998), this variable could rise for two possible reasons: i) a decline in the gap between the wage rate and the marginal product of labour, e.g. due to labour shedding by firms following a weakening of union power and/or rising pressures from capital markets to increase the rate of return on capital; or ii) a shift in production techniques away from labour and towards capital. In both cases, this can be interpreted as an adverse labour demand shock that is set to raise unemployment. The former explanation has been for instance put forward to account for the concomitance of a continued decline in the labour share and a rise in unemployment in certain European countries in the 1980s and part of the 1990s.

18. Re-estimating the baseline regression with a set of observed shocks instead of the output gap term yields highly significant and correctly signed coefficients for TFP, terms of trade and interest rate shocks (column 6), as well as for the labour demand one (column 7). In terms of these estimates, whether the effect of shocks is temporary or permanent depends on whether shocks are stationary or not. However, this cannot be tested for in a reliable way given the small size of the estimation sample and the high degree of persistence of unemployment. In any event, one important and reassuring feature of the estimates in Table 1.2 is that no estimated parameter of policies and institutions – with the partial exception of union density – depends on whether the equation controls for business cycle effects *via* the output gap or the set or macroeconomic shocks used in this paper.

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may lead to more “creative destruction”, *i.e.* higher turnover and higher unemployment (Aghion and Howitt, 1994). Within this context, existing empirical literature on the negative relationship between productivity growth and unemployment may be interpreted as evidence of predominantly disembodied technological progress (Pissarides and Vallanti, 2005).

16 See Annex 2 for further details. The logic behind the use of this variable as a proxy for labour demand shocks is the following. If the aggregate production function characterising the economy is Cobb-Douglas ($Y = A\, L^{\alpha} K^{1-\alpha}$), then in long-run equilibrium $\alpha = [Y'(L)\, L] / [P_Y Y]$, where $Y'(L)$ is the marginal product of labour, L is total employment and $P_Y Y$ is total nominal value added. If one further assumes that imperfections in goods and/or labour markets drive a wedge between the wage rate and the marginal product of labour, denoted $\mu = w / Y'(L)$, then the equilibrium labour share is: $[w\, L] / [P_Y Y] = \alpha\, \mu$. Therefore, the equilibrium labour share may decline as a result of declines in either α or μ. In the short-run, however, the observed labour share may also vary as a result of changes in factor prices that are not immediately and fully offset by corresponding changes in factor proportions –as is the case in the long-run if the production function is Cobb-Douglas. Therefore, the observed labour share is purged here of these short-run effects, following for simplicity and comparative purposes the same methodology as Blanchard (1998). This yields an “adjusted” labour share, which should a priori vary only as a result of changes in either α or μ.

19. In general, therefore, these results suggest that labour- and product-market reforms can have sizeable effects on unemployment. Taking the baseline estimates (Table 1.2, column 1) at face value, a 10 percentage point cut in the tax wedge, a 10 percentage point reduction of unemployment benefits and/or a decline in product market regulation by two standard deviations[17] would on average be associated with a drop in the unemployment rate by 2.8, 1.2 and 0.7 percentage points, respectively. High corporatism is found to lower unemployment by 1.4 percentage points.

20. Overall, the baseline equation appears to explain a significant share of past unemployment trends for most countries (Figure 1.1, Panel A). This is true even when considering the impact of policies and institutions alone, *i.e.* excluding the effects of the output gap (Figure 1.1, Panel B). Changes in policies and institutions between 1982 and 2003 are estimated to explain 47% of the cross-country variance of observed unemployment changes over the same period. This figure rises to 74% when changes in both policies and the output gap are taken into account (the difference between the two figures reflecting the fact that certain countries were in different phases of the business cycle at the beginning and at the end of the observation window). As a consequence, 64% of the cross-country variation in non-cyclical unemployment changes[18] between 1982 and 2003 can be attributed to changes in policies and institutions.

[Figure 1.1. The baseline equation: explaining past unemployment trends]

21. Many of the countries that, as shown in Figure 1.1, succeeded in lowering unemployment reduced tax wedges and/or unemployment benefits (*e.g.* Denmark, Ireland, United Kingdom, see Figure 1.2), while policy changes were typically not employment-friendly in those countries where unemployment stagnated or rose (*e.g.* France, Japan, Switzerland). In addition, the general move towards less regulated product markets has contributed to improve the unemployment record. Yet, for certain countries (*e.g.* Canada, Finland, Spain and Sweden), labour market performance between 1982 and 2003 is essentially explained by the output gap, pointing to the fact that these countries were in different phases of the business cycle at the beginning and at the end of the period under analysis (compare Figure 1.1, Panel A with Figure 1.1, Panel B). Finally, there are some countries for which past unemployment trends are harder to explain. In particular, the gradual pick up in unemployment in Germany since unification is not properly explained by either policy or control variables included in the analysis. By contrast, the drop in unemployment in the Netherlands since the early 1980s has been larger than predicted by the model.

[Figure 1.2. Simulating the impact of changes in policies, baseline specification, 1982-2003]

Sensitivity analysis

22. While panel data econometric approaches have been used extensively to explain the cross-country and time-series patterns of unemployment, it has sometimes been argued that their findings are not sufficiently robust across samples, model specifications or estimation techniques (Baker *et al.*, 2004; Baccaro and Rei, 2005). In any event, any inference from models estimated on a small panel data set and including qualitative variables should be made with care. In order to check the robustness of the above results, a thorough sensitivity analysis of the baseline equation estimated in column 1 has been carried out

17 By construction, the value of the indicator of product market regulation for seven non-manufacturing industries ranges from 0 to 6. In 2003, its average value and its standard deviation across the 20 countries included in the sample were equal to 2.1 and 0.55, respectively. For the "average" OECD country, a decline by two standard deviations would be equivalent to bringing product market regulation down to the stance observed in the most liberal OECD country (the United Kingdom).

18 Calculated as the difference between the actual change in unemployment between 1982 and 2003 and the change in unemployment that can be assigned to cyclical factors – *i.e.* to the change in the output gap – over the same period.

(see Annex 3 for details). The key finding is that the significant unemployment effects of tax wedges, average benefit replacement rates, product market regulation and high corporatism are robust to:

- *The choice of the estimation sample.* The results do not hinge on whether the six observations that correspond to the 1990 and 1991 idiosyncratic shocks in Finland, Germany and Sweden are removed from the sample and different fixed effects are used over the two sub-periods 1982-1989 and 1992-2003. Likewise, excluding from the sample either influential observations (statistical outliers), or any particular country, or any random draw of 10% of observations, has no noticeable impact.[19] Furthermore, the estimated equation seems to fulfil its objective to capture some long-run relationship between unemployment and policies and institutions. Indeed, the set of coefficients obtained when re-estimating the model using 5-year averages[20] is not found to be significantly different from that obtained when using annual data.

- *Model specifications.* Dropping time effects and/or the output gap variable, or considering country-specific coefficients for the latter variable, does not affect the main findings.

- *Estimation techniques.* Feasible Generalised Least Squares (FGLS) with random effects yield comparable results, as do FGLS with fixed effects and country-wise heteroskedasticity.[21]

23. In addition, estimates obtained from the baseline model do not appear to be affected by heterogeneity bias. The latter may arise if the impact of a given policy or institution varies across countries, due for instance to the existence of policy interactions. In such a situation, pooled regressions assuming common coefficients for all countries can yield inconsistent estimates of the average impact of certain explanatory variables (see *e.g.* Pesaran and Smith, 1995). Here, Hausman tests on the absence of heterogeneity bias (see Annex 3) suggest that the baseline regression yields consistent estimates of the average impact of each policy or institution.[22]

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19 Going further, excluding 50% of observations randomly drawn from the sample, almost never affects the sign of coefficients, although statistical significance can be weakened dramatically in a number of cases. However, there are strong arguments against going this far in the sensitivity analysis. In particular, the full sample is of small size, and policies and institutions can remain unchanged for long periods in many countries. As a result, dropping 50% of observations can in some cases remove all the time variance contained in the explanatory variables, thereby making the identification of coefficients virtually impossible in a fixed-effects panel data framework.

20 In this case, the estimation sample consists of the four 5-year periods 1982-1986, 1987-1992, 1993-1998 and 1998-2003.

21 All regressions in Table 1.2 –including the baseline– attempt to correct for individual heteroskedasticity using the Huber/White/sandwich estimator of variance. This conservative approach is systematically adopted even though the null assumption of homoskedasticity is not rejected by a White test at conventional confidence levels. However, when testing instead for the more specific group-wise source of heteroskedasticity, the null assumption is rejected. This suggests, as part of a sensitivity analysis, to estimate Feasible Generalised Least Squares regressions under the assumption of country-wise heteroskedasticity. By contrast, the White test statistic is significant in certain specifications if the logarithm of the unemployment rate is used as dependent variable, as suggested by Nickell (1998). For this reason, linear specifications are preferred to log-linear ones in this paper, although it has been checked that the main qualitative results do not depend on this choice (see also below).

22 The fact that the average impact of policies and institutions is estimated in a consistent manner does not imply that they have the same effect for *all* countries. Therefore, this finding is not inconsistent with the existence of interactions between policies and institutions.

24. Another potential concern is the risk of reverse causality, reflecting some degree of endogeneity of policies and institutions with respect to unemployment patterns. For instance, the observed relationship between benefit replacement rates and unemployment may reflect governments' propensity to raise (cut) benefits when unemployment is high (low) – *i.e.* there may be cases where causality runs from unemployment changes to policy changes. While there is no straightforward way to address this issue, it is still possible to attempt to control for policy endogeneity by means of instrumental variable (IV) techniques. Here, the baseline equation is found to be reasonably robust to the use of a Generalised Method of Moments (GMM) estimator, in which all policies and institutions –with the exception of the degree of corporatism– are assumed to be endogenous.[23]

1.2. Policy interactions

25. The OECD Jobs Strategy argued that comprehensive policy packages are likely to be more effective at reducing unemployment than "piece-meal" labour market reforms and this has been echoed by a number of researchers (Belot and Van Ours, 2004; Coe and Snower, 1997; Elmeskov *et al.,* 1998; Fitoussi *et al.,* 1998; Orszag and Snower, 1998). Recent descriptive evidence on labour market reforms in OECD countries does not provide straightforward evidence for the existence of successful policy packages (Brandt *et al.* 2005): while several countries that succeeded in lowering unemployment have applied comprehensive reforms programmes (Denmark, Netherlands), in other successful countries reforms have been more narrowly targeted on specific fields (Ireland, United Kingdom). To shed further light on this question, this section undertakes an econometric analysis of interactions among policies and institutions (the way theory has treated policy interactions in the context of unemployment analysis is briefly discussed in Box 1). To this end, the baseline model of the previous section –which estimates only "average" effects of policies and institutions irrespective of possible interactions among them– is extended in various ways to allow for interaction effects.

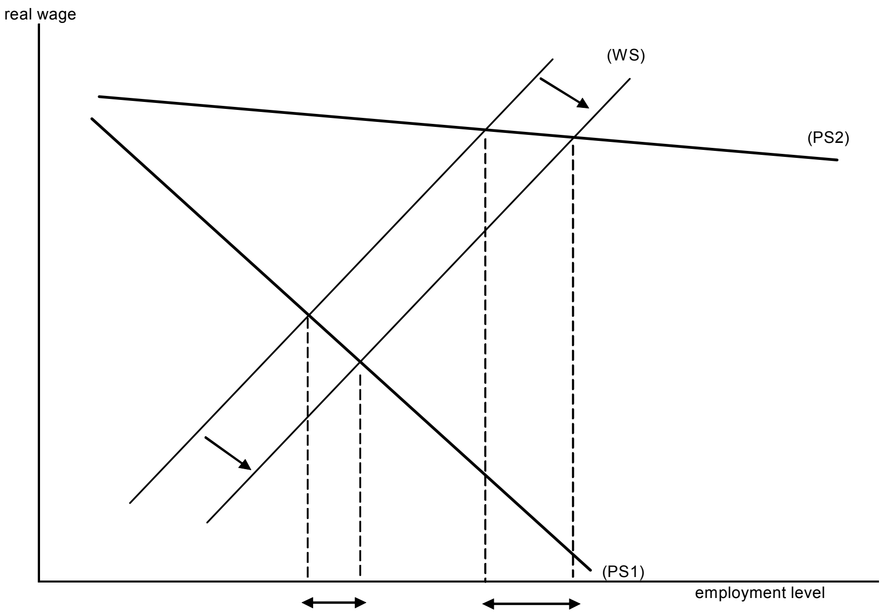
23 The GMM estimator uses (appropriately) lagged levels of an explanatory variable as instruments for its contemporaneous variation. In theory, alternative instruments could also be chosen on the basis of recent literature on the determinants of economic policies and institutions, which stresses the influence of certain political, legal, ideological or cultural factors such as legal origins (*e.g.* Botero *et al.,* 2004; La Porta *et al.,* 1999) or religion (*e.g.* Algan and Cahuc, 2004). However, this approach is not followed here for at least three reasons: i) empirical evidence supporting this recent literature remains limited thus far; ii) appropriate indicators of political institutions, ideologies or cultural values are not straightforward; and iii) in any event, several of the existing indicators (*e.g.* the legal system) are time-invariant and are therefore unlikely to explain past *changes* –as would be needed here– in policies and institutions.

Box 1. Interactions among policies and institutions in the standard wage setting / price-setting model

In a standard wage-setting/price-setting (WS-PS) model (*e.g.* Layard *et al.,* 1991; Nickell and Layard, 1999), it can be shown that institutions interact with each other in their impact on aggregate employment and unemployment. Such interactions reflect two groups of mechanisms (Belot and van Ours, 2004):

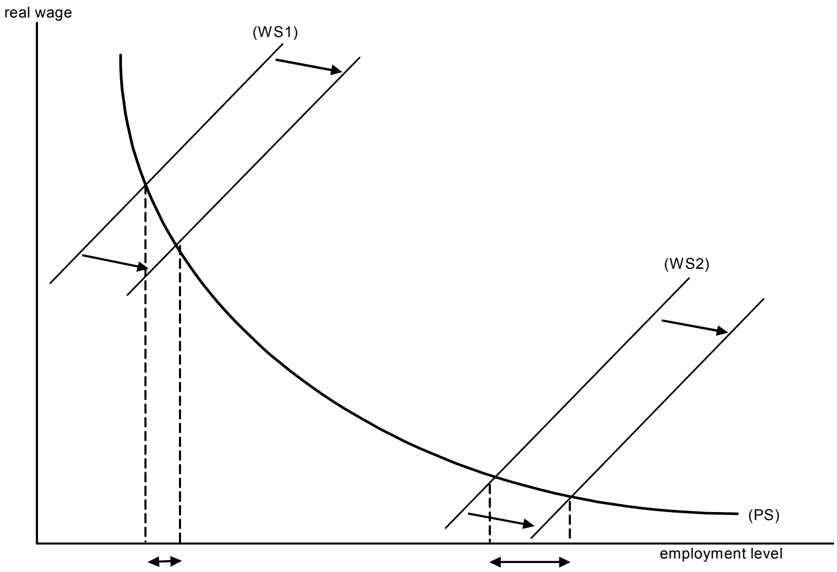
- First, policies and institutions that affect the *elasticity* of wage claims to employment (*e.g*. unemployment benefits, union bargaining power, product market regulation) and/or the elasticity of labour demand to the bargained wage (*e.g.* product market regulation, EPL, the tax wedge) interact with policies and institutions that shift the *level* of wage claims (e.g. unemployment benefits) and/or labour demand (*e.g.* product market regulation). More formally, any factor that affects the slope of the WS and/or PS curves interacts with any factor that affects the level (*i.e*. the vertical position) 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 (see Figure below for a graphical illustration); 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.

The more elastic the labour demand, the greater the impact of a labour market reform



- Second, the marginal impact on labour demand of a given change in real wages is likely to be larger when employment is already high than when it is low. More formally, the labour demand or PS curve is likely to be approximately iso-elastic, *i.e.* convex in the real wage / employment space. As a result, labour market reform that shifts the WS curve downwards (*e.g.* a cut in unemployment benefits) will be greater the higher the initial level of employment, *i.e*. the more employment-friendly the initial institutional framework (see Figure below). This observation has lead a number of researchers to argue that structural reforms are 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 (Coe and Snower, 1997; Fitoussi *et al.,* 1998; Orszag and Snower, 1998).

The higher the initial employment level, the greater the impact of a labour market reform



Generally speaking, both of these groups of mechanisms provide theoretical support for the view that reforms are complementary. This may not always be the case, however, because unlike the second type of mechanism, the first one does not unambiguously lead to reform complementarities. For example, a cut in unemployment benefits not only shifts the WS curve downwards but also makes it steeper, reflecting the greater sensitivity of wage claims to overall labour market conditions when the income loss incurred in case of job loss is high. While the downward shift of the WS curve associated with lower unemployment benefits leads to higher employment, thereby amplifying the impact of other reforms, the steepening of the WS curve has the opposite effect. Therefore, whether the benefit cut and any other reform that shifts the wage curve downwards are complementary in this case depends on the net outcome of these two opposite effects. The answer typically hinges on the parameters of the WS and PS curves, including in particular the degree of convexity of labour demand.

The overall lesson that emerges from these theoretical considerations is two-fold: i) virtually all possible interactions across policies and institutions can affect employment outcomes; and ii) whether such interactions imply reform complementarities should ultimately be assessed on the basis of the empirical evidence.

Specific interactions: is there any evidence of robust policy interactions?

26. 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:

$$U_{it} = \sum_j \beta_j X_{it}^j + \gamma_{kh}\left(X_{it}^k - \overline{X}^k\right)\left(X_{it}^h - \overline{X}^h\right) + \chi G_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \qquad [1.2]$$

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 [1.1]. 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 –*e.g.*

unemployment benefits and the tax wedge in the baseline equation of Table 1.2, a negative and significant sign for the interaction coefficient γ_{kh} would provide evidence of reform complementarity.[24]

27. Undertaking a systematic analysis of policy interactions within the above framework is not straightforward, however. This is because any extension of equation [1.2] 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.[25] For this reason, the analysis of this paper starts by searching for interactions that appear to be robust in simple specifications with only one or two interactions (plus additional "implicit" interactions when appropriate).

28. For instance, one of the most recurrent interactions in the empirical literature on unemployment is between policies and bargaining regimes.[26] In particular, some earlier studies find that the effects of the tax wedge and EPL are most detrimental for unemployment in intermediate bargaining regimes, where "insiders" have stronger bargaining power and can more easily resist attempts by employers to transfer the burden of payroll taxes and/or turnover costs onto wages. Here, this issue is reassessed by allowing the slope of either the tax wedge or EPL (or both) to vary across three levels of corporatism in the baseline specification. The evidence presented, in Table 1.3 (Columns 1-3), seems to confirm a "hump-shaped" relationship between the impact of the tax wedge and the degree of corporatism. In intermediate bargaining

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24 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 [1.2] 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 (in algebraic terms) 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.

25 The additional interactions that are included in an empirical model to meet the above conditions are called "implicit interactions" in the literature. More generally, in the absence of specific parameter restrictions suggested by the theory, for a reduced-form model with multiplicative interactions to be correctly specified, there should exist a partition of the set of institutions to be interacted with one another such that: i) all pairs of institutions identified by one included interaction are subsets of the sets belonging to the partition; and, ii) for each set belonging to the partition all interactions (with two or more terms) that can be generated with its elements are included into the model (see Braumoeller, 2004).

26 Economic theory provides no clear-cut view on the mechanisms through which the bargaining regime shapes the employment impact of a given reform. For example, it has been argued that union bargaining power is lower in decentralised regimes, thereby lowering wage claims and making them less sensitive to employment conditions. Under this assumption, structural reforms are most effective in decentralised regimes (see *e.g.* Belot and van Ours, 2004). This is because the employment effects of any reform that shifts the wage-setting and/or the price-setting curve downwards are greater the flatter is the initial wage-setting curve (see Box 1 for a graphical exposition). By contrast, other authors (*e.g.* Calmfors and Driffill, 1988) argue that labour market insiders are most powerful in intermediate bargaining regimes. When insiders negotiate the wage, they tend to set the highest possible wage conditional on their own employment (see *e.g.* Bertola, 1999). As a consequence, except for high unemployment levels, wage claims are relatively independent from aggregate unemployment. The resulting flatness of the wage-setting curve implies that any structural reform that stimulates labour demand has greater employment effects than in other bargaining regimes.

regimes, a 10 percentage point cut in the tax wedge is estimated to reduce unemployment by 2.8 percentage points more than is the case in decentralised regimes.[27] By contrast, while a hump-shaped pattern is also found in the case of EPL, it is statistically significant only at the 10% level.[28]

[Table 1.3. Simple interactions between institutions and bargaining regimes, 1982-2003]

29. Care must be exerted in interpreting these results, however. Testing for coefficient "poolability" through the Hausman test discussed earlier (see Section 1.1 and Annex 3) indicates that the specification where the impact of the tax wedge is allowed to vary across bargaining regimes yields inconsistent estimates of average parameters, reflecting a heterogeneity bias.[29] More detailed analysis indeed confirms that countries with intermediate bargaining systems form a highly heterogeneous group as far as the unemployment impact of the tax wedge is concerned, with the large detrimental effect being attributable essentially to two countries, namely France and Spain (Table 1.3, Column 4). A similar problem occurs in the case of EPL (Table 1.3, Column 5).[30]

30. Other interactions have been analysed in the literature, often with contrasting results (see *e.g.* Elmeskov *et al.,* 1998; IMF, 2003; Belot and van Ours, 2001; Nicoletti and Scarpetta, 2005). Here, a systematic approach is followed. For all possible interactions among pairs of explanatory variables, an augmented version of the baseline specification including only that interaction is estimated. The explanatory variables for which these specifications are tested include not only the policies and institutions used in the baseline specification but also two dummy variables, one for low degrees of corporatism[31] and another for high levels of collective bargaining coverage.[32] Only few interactions turn out to be significant

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27 Notice for comparison that the estimated effect of the tax wedge at the sample average is such that a 10 percentage point reduction of the tax wedge is estimated to reduce unemployment by 3.1 percentage points and this effect is reduced to only 2.1 percentage points in decentralised countries.

28 Hump-shaped patterns have been searched also for other institutions that affect the labour demand, such as product market regulation. Yet, no significant variation across regimes has been found.

29 The Hausman test statistic rejects the poolability hypothesis even if all variables are allowed to vary across bargaining regimes (with a p-value of .001).

30 For this reason, the remainder of this section will always consider interactions with only one –rather than two– wage-bargaining variable, namely a dummy for either high or low corporatism. When considering a dummy for high (low) corporatism, countries with intermediate bargaining regimes will be grouped into the low (high) corporatism group. This turns out to be sufficient to remove the heterogeneity bias. In fact, in the case of baseline models augmented by one single interaction between the tax wedge and a dichotomous variable for, alternatively, high, low or intermediate corporatism, the p-values associated with the Hausman test statistics are .086, .058 and .001, respectively.

31 Although a high-corporatism dummy is already included in the institutional set, there is a case for also considering a low-corporatism dummy. In the baseline unemployment regression with country fixed effects, the estimated coefficient of the high-corporatism dummy can be readily interpreted as the effect of switching from low to high corporatism, because intermediate corporatist countries do not change regime within the estimation sample. This no longer holds when one incorporates interactions that include dichotomous variables. In such a case, the interpretation of the high- and low-corporatism dummy variables in simple interactions differs, because they imply different groupings of intermediate corporatist countries. Therefore, given that the correct grouping is unknown, interactions with low corporatism are not ruled out.

32 The latter variable is included in the institutional set because it is likely to capture union power more accurately than union density does. It takes value 1 when collective bargaining coverage exceeds 50% and zero otherwise. It is also time-invariant, which explains why a high collective bargaining coverage dummy variable is only informative in interactions and cannot be included in the baseline unemployment equation. While a purely quantitative measure of collective bargaining coverage would have done an even better job

in this estimation exercise and, unsurprisingly, the majority of them concern the variables with the strongest direct effects in the baseline equation, namely unemployment benefits and the tax wedge (Table 1.4, Column 1).[33]

31. Again, however, these results should be interpreted with caution. Omitted interactions might bias coefficient estimates. More precisely, suppose that no interaction exist between an institution X^k and another institution X^h. If X^k is correlated with an omitted third variable X^s and if X^h interacts with X^s, then the interaction between X^k and X^h might still appear significant. In the baseline equation, the presence of country fixed effects essentially aims to control for the effect of omitted, approximately time-invariant, institutions that are correlated with both unemployment and the explanatory variables. Consistently, at least some of the estimates of Table 1.4 are likely to be biased due to the omission of additional interactions with time-invariant variables.[34] For example, one cannot rule out *a priori* that the significant positive interaction between unemployment benefits and the tax wedge in fact reflects the positive interaction between each of these policy indicators and a third, omitted determinant of unemployment –e.g. eligibility rules for access to unemployment or other welfare benefits.

32. To address this issue, two alternative strategies are implemented. First, an instrumental variable (IV) approach is followed, where any interaction $\left(X_{it}^k - \overline{X}^k\right)\left(X_{it}^h - \overline{X}^h\right)$ is instrumented with $\left(X_{it}^k - \overline{X}_i^k\right)\left(X_{it}^h - \overline{X}_i^h\right)$, which is the product of the deviations of X^k and X^h from their respective country-specific means.[35] Second, augmented versions of each OLS specification are estimated, including all interactions of X^k and X^h with country-specific fixed effects variables.[36] Results from both

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at capturing union power, no annual time series are available for such a variable in most countries (see OECD, 2004).

33 Interactions among dichotomous variables, as well as between union density and high collective bargaining coverage, are not shown here, as they are clearly difficult to interpret from either a statistic or economic point of view.

34 The reason for including fixed effects rather than random effects relies on the fact that institutions are assumed to be correlated with country effects. This conjecture is also confirmed by Hausman tests on the baseline specification (see Annex 3). However, if an institution X^k is correlated with a third time-invariant variable X^s, then the interaction between X^k and any other variable X^h (that is $X^k X^h$) will be correlated with $X^s X^h$. This implies that for at least one of the interactions of Table 1.4, OLS estimates are biased.

35 This can be viewed as a "quasi Hausman-Taylor" IV approach. Hausman and Taylor (1981) have noted that the deviation of a variable from its country-specific mean is a valid instrument for that variable in the absence of fixed effects, insofar as it is uncorrelated with any time-invariant unobservable factor. In the approach followed here, the necessary orthogonality conditions for the validity of the instrument are of the type $E\left(\left(X_{it}^k - \overline{X}_i^k\right)\left(X_{it}^h - \overline{X}_i^h\right)X_i^s X_{it}^h\right) = 0$, where $\overline{X}_i^j$ stands for the country-specific mean of X^j and X_i^s for the time-invariant unobservable variable. These conditions are met if $E\left(\left(X_{it}^k - \overline{X}_i^k\right)X_i^s \middle| \left(X_{it}^h - \overline{X}_i^h\right)\right) = 0$ and $E\left(\left(X_{it}^k - \overline{X}_i^k\right)X_i^s \overline{X}_i^h \middle| \left(X_{it}^h - \overline{X}_i^h\right)\right) = 0$, which does not appear too stringent if one takes into account that the unconditional moments $E\left(\left(X_{it}^k - \overline{X}_i^k\right)X_i^s\right)$ and $E\left(\left(X_{it}^k - \overline{X}_i^k\right)X_i^s \overline{X}_i^h\right)$ are equal to zero by construction.

36 While consistent, this approach is likely to be more inefficient than the IV approach described above, due to excessive reduction of degrees of freedom.

approaches are reported in Table 1.4, Columns 2 and 3, with IV estimates being presented only when the corresponding instrument is found to be acceptable using standard criteria.[37] Only the negative interaction between the average unemployment benefit replacement rate and union density appears to be robust across all estimation methods.[38] To the extent that union density can be considered a proxy of union bargaining power, this finding might reflect the fact that, in most theoretical models, lower bargaining power makes wage claims less responsive to unemployment conditions (see Box 1). Given that union density has no significant direct unemployment effect at the sample mean,[39] the estimates presented in Table 1.4 imply that, for a country with average values of all institutions, a 10 percentage point reduction in union density can increase the elasticity of unemployment to a benefit cut by 15% to 75%. For instance, a simultaneous reduction of the average benefit replacement rate and union density by 10 percentage points would lower unemployment by between 1.4 and 2.1 percentage points in the average country (depending on the estimates), against 1.2 percentage points only if union density remains stable.

[Table 1.4. Simple interactions, 1982-2003]

"Systemic" interactions: do policies interact with the overall institutional framework?

33. Taken at face value, the evidence provided in Table 1.4 is not strongly supportive of the hypothesis that reforms reinforce each other – in the form of 2-by-2 interactions. However, other explanations exist for the lack of robustness of most interactions. First, small sample size might prevent the emergence of significant patterns. Second, and most importantly, the above approach may be too narrowly focused on specific policy interactions, while the main theoretical prediction is that interactions should take place between individual policies and the overall policy and institutional framework, *i.e.* "systemic interactions" (see Box 1).

34. As already noted, however, systemic interactions cannot be analysed by means of a standard general model – including all possible multiplicative interactions in equation [1.2] above, since the latter would easily be overfitted.[40] As a way to overcome this problem, the alternative approach followed here is to estimate a more compact specification that can be derived from mainstream wage-setting/price-setting models of structural unemployment under fairly general conditions.[41, 42] The main starting point is that

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37 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.

38 It might be argued that, if the convexity of the labour demand curve is one of the main sources of reform complementarity, negative interactions might disappear in a log-linear specification and/or quadratic terms of institutions should be included, too. Yet, repeating the exercise presented in Table 1.4 with log-linear specifications yields the same results (the interaction between the average replacement rate and union density is the only robust one), while no robustly significant quadratic term is found. As an additional sensitivity analysis, the baseline model has been estimated by augmenting it by all possible combinations of two interactions (including all implicit interactions, when applicable). Again, the interaction between the average replacement rate and union density turns out to be the only one significant in all specifications.

39 None of the specifications presented in Table 1.4 yields coefficients of the average effects substantially different from those obtained with the baseline model.

40 With 6 policies or institutions, a general unrestricted model allowing all interactions among observables only 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*).

41 Namely that: i) labour demand is iso-elastic, or more broadly such that the employment effects of a given reform are greater the more employment-friendly the overall institutional framework; and, ii) policies and institutions have a greater impact on the *level* of wage claims than on their *elasticity*. Under these conditions, equation [1.3] below is more suited to capture reform complementarity patterns than the standard linear model with unrestricted multiplicative interactions.

labour demand is plausibly close to be iso-elastic, which implies that a greater fall in real wages is required to raise labour demand by a given magnitude when employment is low rather than when employment is high. 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 (see Box 1).[43] This suggests that systemic reform complementarity patterns can be explored by estimating a non-linear equation where each institution is interacted with the overall institutional framework, defined as the sum of the direct unemployment effects of institutions:[44]

$$U_{it} = \sum_j \beta_j X_{it}^j + \sum_k \left(\gamma_k \left(X_{it}^k - \overline{X}^k \right) \left(\sum_j \beta_j \left(X_{it}^j - \overline{X}^j \right) \right) \right) + \chi G_{it} + \alpha_i + \varepsilon_{it} \qquad [1.3]$$

where the parameters β_j and γ_k are simultaneously estimated.[45] β_j denotes the direct effect of institution X^j at the sample average, *i.e.* for a country with an average mix of policies and 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 ($\sum_j \beta_j \left(X_{it}^j - \overline{X}^j \right)$, expressed in deviation form in the interaction). Again, a negative and significant sign for γ_k would provide evidence for reform complementarity. To the extent that, as discussed above, estimates of interaction coefficients may be biased because of the correlation of certain institutions with unobserved time-invariant unemployment determinants, additional interactions involving country-fixed effects are also included in the specification.[46]

42 An alternative approach is that followed by Nicoletti and Scarpetta (2005), who study complementarities between product and labour market reforms through an interaction between the OECD PMR indicator and a linear combination of a subset of labour market institutions, whose weights are obtained by factor analysis. Yet, such an approach is potentially subject to the "omitted interaction problem" discussed in the previous section as regards simple one-interaction models.

43 Provided that shifts in the slope of the wage-setting or labour-demand curves do not thoroughly counteract this effect.

44 Following this line of reasoning, it would seem tempting to explore direct interactions between institutions and the unemployment rate. In this case, however, interaction terms would be endogenous, with no possible instrument by construction.

45 By Non-linear Least Squares or Maximum Likelihood.

46 This implies that the specification actually estimated is slightly more complex than [1.3]: $U_{it} = \sum_j \beta_j X_{it}^j + \sum_k \left(\gamma_k \left(X_{it}^k - \overline{X}^k \right) \left(\sum_j \beta_j \left(X_{it}^j - \overline{X}^j \right) \right) \right) + \sum_h \left(\mu_h \left(I_i^h - \bar{I}^h \right) \left(\sum_j \beta_j \left(X_{it}^j - \overline{X}^j \right) \right) \right) + \chi G_{it} + \alpha_i + \varepsilon_{it}$ 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 Table 1.4, Column 3. IV approaches such as those implemented in Column 2 of Table 1.4 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, due to 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 $\sum_h \left(\mu_h \left(I_i^h - \bar{I}^h \right) \left(\sum_j \beta_j \left(X_{it}^j - \overline{X}^j \right) \right) \right)$ is

35. Table 1.5 shows the estimation results obtained by allowing for systemic interactions. Column 1 presents the general model, while Column 2 presents the final specification obtained by sequential elimination of insignificant interactions. Three main results stand out:

- First, compared with the baseline unemployment equation (Table 1.2), 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.[47] 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.[48]

- Second, all significant interactions are negative, lending some support to the reform complementarity hypothesis.[49] Furthermore, compared with the baseline equation of Table 1.2, the model appears to do a better job at explaining unemployment trends over the sample period, except in the case of Ireland (Figure 1.3, compare Panel A with Panel B). [50] In fact, the model with systemic interactions is estimated to explain 92% of the cross-country variance of unemployment changes between 1982 and 2003, against 74% only for the baseline model, despite the fact the latter also includes time dummies while the former does not.

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dropped, yield qualitatively similar –albeit less significant– results. Similarly, time dummies are not included in the estimated equation for lack of convergence of the algorithm.

47 The estimated elasticities of unemployment to EPL and union density are not large, however. According to the estimates, a fall in EPL by two standard deviations from the sample mean would reduce unemployment by about 0.9 percentage points. By construction, the value of the EPL indicator ranges from 0 to 6. In 2003, its average value and its standard deviation across the 20 countries included in the sample were equal to 1.84 and 0.87 respectively. For the "average" OECD country, a decline by two standard deviations would be equivalent to bringing EPL down to the stance observed in the some of the most liberal OECD countries (Canada and the United Kingdom, where EPL is estimated to be slightly more stringent than in the most liberal country, namely the United States). A fall in union density by 10 percentage points –which roughly corresponds to the average variation actually observed over the sample period, excluding New Zealand where the decline was larger – from the sample mean is estimated to reduce unemployment by about 0.7 percentage points.

48 As a robustness check, the specification of Column 2 has been re-estimated excluding the high corporatism dummy variable in the sum of direct effects of institutions that is included in the interaction. In this case, the estimated equation is:

$$U_{it} = \sum_{j,X^j \neq C} \beta_j X_{it}^j + \sum_k \left(\gamma_k \left(X_{it}^k - \bar{X}^k \right) \left(\sum_{j,X^j \neq C} \beta_j \left(X_{it}^j - \bar{X}^j \right) \right) \right) + \sum_h \left(\mu_h \left(I_i^h - \bar{I}^h \right) \left(\sum_{j,X^j \neq C} \beta_j \left(X_{it}^j - \bar{X}^j \right) \right) \right) + \delta C_{it} + \chi G_{it} + \alpha_i + \varepsilon_{it},$$

where C_{it} is the high corporatism dummy. 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 1.5, except that the direct impact of high corporatism becomes insignificant.

49 As an additional sensitivity analysis, the specifications presented in Table 1.5 have been re-estimated with the logarithm of unemployment as dependent variable, yielding qualitatively similar results – although with slightly less significant interaction terms.

50 The slight differences between Panel B of Figure 1.3 and Panel A of Figure 1.1 are due to the fact that, in the latter, the X-axis considers the predicted unemployment change including the contribution of time dummies while, in the former, the X-axis considers only the change in unemployment explained by policies, institutions and the output gap.

- From a quantitative viewpoint, however, the gains from implementing reform packages are found to be moderate for the "average" OECD country. Table 1.6 uses the specification in column 2 of Table 1.5 to simulate the additional gain from undertaking jointly two reforms that would each reduce unemployment by 1 percentage point if implemented separately.[51] All possible combinations of two such reforms yield a total reduction of unemployment between 2.25 and 2.37 percentage points for the "average" OECD country, instead of 2 percentage points when interaction effects are not taken into account. In other words, in this simulation exercise, reform complementarities would amplify the unemployment effects of separate reforms by between 12% and 19%.[52]

[Table 1.5. Systemic Interactions, 1982-2003]

[Table 1.6 Simulated effect of reform complementarities]

[Figure 1.3. Systemic interactions: how better do they explain past unemployment trends?]

Summing up

36. The conclusions from the analysis of policy interactions carried out in this section is that reform packages seem to yield greater employment gains than separate, "piece-meal" reforms. Indeed, the impact of a given policy reform is usually greater the more employment-friendly the overall policy and 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.

37. While there is some evidence of "systemic" interactions, no firm conclusions can be drawn as regards the particular impact of more specific interactions across policies and institutions which have been singled out by previous literature. Such lack of robustness reflects three main factors. First, while theory clearly suggests that all interactions are possible and should therefore be studied simultaneously, this is not feasible in practice using a general model due to the small sample size. Second, certain interactions –e.g. between the tax wedge and EPL on the one hand and wage bargaining regimes on the other– appear to reflect particular country experiences and cannot easily be generalised. Finally, the majority of apparently significant interactions become insignificant or even change sign when allowance is made for possible correlations between institutions and other, omitted time-invariant determinants of unemployment. This latter result points to the importance of interactions with a number of policies and institutions which are not considered in this section for lack of available data at an annual frequency and/or endogeneity problems. To a limited extent, these additional policy issues are addressed in the next section.

1.3. Additional determinants of unemployment patterns: housing policy, minimum wages and active labour market policies

38. Sections 1.1 and 1.2 above follow the standard approach of excluding from the baseline specification those policies (such as housing policy, minimum wage and active labour market policies – ALMPs) that require specific econometric treatments and are, therefore, usually not included in general

51 That is, simulations consider reductions by 6.7, 5.6 and 12.6 percentage points for tax wedge, average replacement rate and union density, respectively, as well as by 3.3 standard deviations for product market regulation. Such reductions are large in historical perspective, insofar as they exceed in each case the average change observed in OECD countries over the sample period.

52 The greatest effect is obtained by combining reforms of the average replacement rate with reductions in union density, consistent with the estimation of single interaction models presented in Table 1.4.

macroeconomic studies of institutional determinants of unemployment (see Annex 1). In particular, i) data on housing policy and home-ownership are scattered and available essentially in cross-section; ii) reliable minimum wage time series exist only for countries where minimum wages are statutory; and iii) measures of ALMP intensity are available only since 1985 and are likely to be endogenous to unemployment by construction. The strategy followed above does not intend to downplay the importance of these factors. Their impact on unemployment is therefore analysed in this section, through case-by-case adaptation of the general approach set forth in Sections 1.1 and 1.2.

Housing policy and home ownership

39. A growing body of literature has been focussing recently on the relationship between housing policy, home ownership and job mobility. However, cross-country comparable data on transaction costs and housing policies are scattered, while home ownership data are available only for period-averages.[53] Still, one way to shed some light on the relationship between home ownership and unemployment, while controlling simultaneously for the effects of other time-varying institutional factors, is to look at the simple cross-country correlation between country fixed effects from the baseline specification and the rate of home ownership (defined here as owner-occupied housing as a percentage of total occupied housing stock). As shown in Figure 1.4, this correlation is indeed high: more than one-third of the variance among fixed effects appears to be explained by the average rate of home ownership during the 1990s. This finding is in line with most existing macroeconomic studies (*e.g*. Oswald, 1997, Cameron and Muellbauer, 1998, Green and Hendershott, 2001, Nickell *et al*., 2005).

[Figure 1.4. Country fixed effects and home ownership]

40. Care must be exerted, however, in drawing policy conclusions from this result. In fact, home ownership might be endogenous insofar as, *ceteris paribus,* societies with lower degrees of internal and external migration are likely to have higher rates of home-ownership. From a policy perspective, it is indeed crucial to determine whether high rates of home ownership induce high unemployment by preventing mobility or whether more mobile labour forces bring about simultaneously low home ownership rates and better labour market outcomes. Disentangling these different links is, however, impossible with the data at hand.

Minimum wages

41. The natural approach to study the impact of the minimum wage on unemployment is to augment the set of explanatory variables in the baseline specification (equation [1.1]) with a measure of the minimum wage that is comparable across countries. While the most frequent approach is to use the ratio of gross statutory minimum wages to median or average wages (see *e.g.* OECD, 1998; Elmeskov *et al.,* 1998), a few papers combine information on both statutory and contractual minimum wages (*e.g.* Neumark and Wascher, 1999). However, the latter can vary substantially across sectors and often depend on workers' age, experience and qualifications. Such detailed information is rarely available and, in any event, is inherently hard to summarise in a single, cross-country comparable indicator. In addition, the employment effects of negotiated minima are likely to be quite different from those of a uniform national minimum wage. For these reasons, following the main thrust of the literature, collectively-bargained minima are excluded from the scope of this analysis. However, the main drawback of focussing on

53 A few papers (*e.g.* Nickell *et al.,* 2005) use annual time series for home ownership. However, they are obtained by interpolation of ten-year data. This choice is clearly not suited for the relatively short time span of the sample used for this study. For this reason, a more cautious empirical approach is adopted here (see below).

statutory minima – measured here as a percentage of median wages – is that the estimation sample is halved.[54]

42. Table 1.7 presents the outcome of this regression exercise. Consistent with previous OECD work (OECD, 1998, Elmeskov *et al.,* 1998), no significant direct impact of the minimum wage on the unemployment rate is found (Column 1),[55] except when controls for the output gap are omitted (Column 2). This latter result can be explained by the pro-cyclicality of median wages, which makes the ratio of minimum to median wages highly endogenous in the absence of a good control for the business cycle. Consistent with this interpretation, no significant impact of the minimum wage is estimated if measures of macroeconomic shocks such as those used in Table 1.2 are substituted for the output gap in the specification (not reported in Table 1.7). Conversely, and in line with theoretical priors, additional estimates suggest that a high tax wedge has more adverse effects on unemployment when the minimum wage is high (Columns 3 and 4).[56] The estimated impact of the minimum wage on the elasticity of unemployment to the tax wedge is large. Taken at face value, the estimates reported in Columns 3 and 4 suggest that an increase in the ratio of minimum to median wages by ten percentage points[57] would increase the impact of the tax wedge on unemployment by about 50% in the "average" OECD country.

[Table 1.7. Minimum Wages, 1982-2003]

Active labour market policies

43. Most macroeconometric studies use ALMP expenditures per unemployed person as an indicator of countries' spending efforts in pursuing active policies. This indicator is expressed as a percentage of GDP per capita to ensure cross-country comparability (*e.g.* Scarpetta, 1996; Nickell, 1997, 1998; Nickell and Layard, 1999; Boone and van Ours, 2004). Since ALMP expenditures are unlikely to vary in proportion to changes in unemployment, such a synthetic indicator of ALMP spending is likely to be pro-cyclical, *i.e.* it declines (rises) when unemployment goes up (down). The resulting endogeneity bias has typically been addressed in the literature by instrumenting the ALMP spending indicator by its country average and by estimating the unemployment equation using random country effects.[58] However, such an

54 Countries with statutory minima during the whole sample period are Australia, Belgium, Canada, France, Japan, the Netherlands, New Zealand, Portugal, Spain and the United States. Ireland and the United Kingdom introduced a national minimum wage in 1999. They are not included in the sample, however, since their corresponding time series is too short. Results presented below, anyway, are robust to the inclusion of these two countries (even if the minimum wage is set to zero prior to the introduction of the statutory minimum).

55 It might be argued that this result is due to the inclusion of country fixed effects insofar as most of the variation of the minimum wage ratio is cross-sectional, while its time-series variation is essentially due to movements of the median wage. Yet, this conjecture does not appear to be grounded in the data, insofar as the result presented in Column 1 of Table 1.7 is robust to the exclusion of country and time effects from the specification.

56 Following the same approach as in Section 1.2 (cf. Table 1.4), Column 4 reports estimates obtained by two stage least squares where the interaction between the tax wedge and the minimum wage is instrumented with the product of the deviations of both variables from their respective country-specific means.

57 This corresponds to one standard deviation of the cross-country distribution of the ratio of minimum to median wages in 2003.

58 Fixed-effects regressions cannot be estimated in this context since the country fixed effects would be collinear with the ALMP variable. Alternatively, one could introduce the time-varying ALMP indicator in a fixed-effects framework and control for cyclical effects through the inclusion of the output gap (see for instance Table 1.8, Column 1 below). However, controlling for the output gap is likely to be insufficient to purge the ALMP indicator from the influence of changes in unemployment, for at least two reasons. First,

approach is likely to yield inconsistent estimates, as it rests on the implausible assumption that country effects are independent from other co-variates. One alternative approach is to look at the simple correlation between country averages of ALMP spending and country fixed effects from panel data unemployment regressions. Consistent with most available studies, average ALMP expenditures per unemployed as a percentage of GDP per capita are found to be significantly correlated with country fixed effects obtained from the baseline specification (Figure 1.5).[59] However, as in the case of home ownership discussed above, this finding cannot be readily interpreted as a causal relationship between ALMP spending and aggregate unemployment.

[Figure 1.5. Country fixed effects and ALMPs, 1985-2003]

44. A more ambitious approach is to use instrumental variable (IV) techniques to re-estimate the baseline fixed-effect specification (Table 1.2, Column 1) augmented with the synthetic indicator of ALMP. Here, the retained instrument is the lagged first difference of the residual of the regression of the ALMP indicator on current and lagged values of the output gap.[60] While the non-instrumented regression yields a significant effect of ALMP spending (Table 1.8, Column 1), the coefficient becomes insignificant in the IV approach (Column 2).[61] While this finding could be interpreted as evidence against any significant effects of total ALMP spending on unemployment, it could also reflect the low efficiency of two-stage least squares estimators or, as explored in detail below, the heterogeneity of ALMP programmes that are included in the aggregate spending indicator.

[Table 1.8. Active Labour Market Policies, 1985-2003]

45. A related issue is whether ALMP expenditures can mitigate the detrimental unemployment effects of high unemployment benefits. This could be the case mainly for two reasons (Boone and Van Ours, 2004). First, long-term oriented programmes, such as training programmes, are designed to reduce the risk of future unemployment spells by improving workers' competencies and reducing mismatch. Thus,

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cyclical fluctuations in unemployment may affect ALMP spending with fairly long lags. Second, ALMP spending is unlikely to change in proportion to changes in structural unemployment, for instance if there are increasing returns to ALMP spending arising from fixed administrative costs of public employment services. As a result, any factor that lowers (increases) structural unemployment might permanently increase (reduce) ALMP spending per unemployed as a share of GDP per capita, *ceteris paribus.*

59 Country averages of ALMP spending are calculated over the period 1985-2001 or 1985-2002 for all sample countries with the exception of Denmark (1986-2002), Japan (1987-2001) and Portugal (1986-2000).

60 The motivation for this instrument is two-fold. First, regressing the ALMP indicator on a certain number of lags –three in the specifications reported in Table 1.8– of the output gap plausibly eliminates the effect of contemporaneous and lagged cyclical fluctuations. Second, using lagged and differenced residuals is expected to remove the effect of once-off shifts in structural unemployment due to unobserved reforms. All the results discussed in this section are robust to different choices of instruments, such as reducing the number of lags of the output gap included in the preliminary regression, increasing the order at which the first-differenced residual is lagged and/or not differencing the residuals. The results are also robust to the exclusion of high ALMP-spending countries (Denmark, Sweden, Norway and the Netherlands) from the sample.

61 As a sensitivity analysis, this exercise has been replicated substituting ALMP spending as a percentage of GDP for ALMP expenditures per unemployed person as a percentage of GDP per capita, with similar conclusions. While the non-instrumented regression yields a *positive* and significant effect –consistent with the view that the ratio of ALMP spending to GDP is counter-cyclical, which should bias the estimates upwards, the coefficient becomes negative and insignificant in the instrumented regression. Moreover, in the latter case, the point estimate, once standardised by the average ratio of unemployment to population, is close to that reported in Table 1.8, Column 2, thereby providing indirect evidence supporting the IV strategy adopted here.

high benefit replacement rates might be complementary to these programmes insofar as they reduce trainees' incentives to accept offers for unstable jobs before programme completion. Second, besides facilitating job-search, ALMPs can be used for the purpose of "activation" and are thus likely to motivate job-search as some benefit recipients seek to avoid complying with unpleasant programme requirements. This latter effect is likely to be greater, the higher the level of unemployment benefits with respect to the wage level of potential job offers. Indeed, the literature on programme evaluation has shown that careful integration of active and passive policies can be effective in reducing disincentives effects brought about by generous unemployment benefits (OECD, 2005).

46. The last two columns of Table 1.8 shed some light on this interaction by using two different methods. In Column 3, following Boone and van Ours (2004), the baseline unemployment regression is augmented with the interaction between the average benefit replacement rate and country-specific averages of the synthetic measure of ALMP spending. These estimates, however, potentially suffer from the omitted interaction bias discussed in Section 1.2. Therefore, following the approach developed earlier, IV estimates of this interaction are obtained by exploiting the time-series variation of the ALMP indicator (Table 1.8, Column 4).[62] The estimated coefficient of the interaction is robust across methods and suggests that the adverse impact of unemployment benefits is lower in countries that spend more on ALMPs. Taken at face value, these estimates suggest that the unemployment effect of the generosity of unemployment insurance becomes statistically insignificant in high ALMP countries, such as Denmark or the Netherlands.[63]

47. Table 1.9 proceeds with a disaggregated analysis of the unemployment effects of ALMP expenditures. Concretely, the synthetic indicator of ALMP spending is decomposed into the five main categories available in the OECD Labour Market Policies database: public employment services (PES) and administration; training programmes; youth measures; subsidized employment; and measures for the disabled (see Annex 2).

[Table 1.9. Categories of Active Labour Market Policies, 1985-2003]

48. Columns 1-3 of Table 1.9 report estimates of specifications including all five categories. Column 1 reports fixed-effects estimates and Column 2 IV estimates. In addition, Column 3 reports fixed-effects estimates obtained by substituting ALMP spending as a percentage of GDP to ALMP expenditure per unemployed person as a percentage of GDP per capita, as suggested by Estevao (2003). Given that, *ceteris paribus,* ALMP spending rises and GDP declines with an increase in unemployment, the estimated coefficient of ALMP spending as a percentage of GDP is unambiguously upward biased. If negative and significant, however, its estimate would then provide a lower bound (in absolute terms) for the true effect of ALMPs. Consistent with Boone and van Ours (2004), labour market training is the only ALMP category whose negative coefficient appears to be robust across all three estimation methods.[64] The statistical

62 Following the same approach as in Section 1.2 (cf. Table 1.4), Column 4 reports estimates obtained by two stage least squares where the interaction is instrumented with the product of the deviation of the average replacement rate from its country-specific means and the lagged first difference of the residual of a regression of ALMP spending per unemployed as a percentage of GDP per capita on up to three lags of the output gap. Note that by definition the second term of this product is uncorrelated with country fixed effects.

63 Increasing ALMP expenditures per unemployed worker as a percentage of GDP per capita from the OECD average (27.9% in 2000) by 10 percentage points would reduce the coefficient of unemployment benefits by about one-fifth. Therefore if ALMP spending per unemployed were increased further to the level of Denmark (64.5% of GDP per capita in 2000), the estimated impact of unemployment benefits on unemployment would be reduced by about two-thirds, making it insignificant.

64 IV estimates yield similar results whether expenditures per unemployed worker as a percentage of GDP per capita or ALMP expenditures as a percentage of GDP are used as dependent variables and instruments adjusted accordingly.

significance of ALMP spending on training programmes is also found to be robust to the exclusion of other ALMP categories from the estimated equation (Columns 4-6), as well as to system GMMs estimation, where ALMP and the output gap are assumed to be endogenous (Column 7).[65] Since lower bound estimates (Columns 3 and 6) are significant in the case of training, it is possible to conclude that, for the average OECD country, increasing ALMP spending on training programmes per unemployed as a percentage of GDP per capita by 4 percentage points[66] would reduce unemployment by *at least* 0.2 percentage points. This value increases to 0.6 percentage points if simulations are based on IV or GMM estimates.[67]

49. These results provide some additional insight on the role of ALMPs in curbing unemployment[68] and can be seen as complementary to microeconometric studies. In fact, general equilibrium and long-run effects can hardly be taken into account in practice in micro-evaluation studies. The consequence is that those studies might tend to be overly optimistic as regards programmes involving large potential substitution effects (*e.g.* subsidised employment) as well as overly pessimistic on programmes that are likely to pay off only in the long-run (*e.g.* training programmes).

1.4. Interactions between institutions and shocks

50. It has recently been argued that the current degree of heterogeneity in policies and institutions across OECD countries largely pre-dates –and is therefore unable to account for– cross-country trends in unemployment performance since the early 1970s (Blanchard and Wolfers, 2000). The evidence presented in the previous sections yields no support for this view, insofar as a sizeable share of the cross-country variation of unemployment changes between 1982 and 2003 can be explained by a model considering only institutions and the output gap. Still, besides permanent effects, policies and institutions may also have had a temporary but persistent impact on unemployment during the past three decades *via* their interaction with

65 System GMMs allow handling in more general terms the possible presence of an AR(1) component in the error term. Yet, GMMs are very sensitive to the specification and are inconsistent if the number of lags of the autoregressive component is not correctly specified. Additionally, given that the number of periods is as large as the number of countries, the same instruments are used at all lags in this case, which potentially increases the sensitivity of the estimator to specification errors. For this reason, GMM estimates are presented here only as a sensitivity exercise. Since Arellano-Bond specification tests reject a standard ARMA (1,0) model for the error term (not shown in the table), instruments are lagged at least three and two periods in the difference and level equations, respectively, consistent with an ARMA (1,1) specification of the error term, which does not appear to be rejected by Arellano-Bond tests at the 5% level (cf. Table 1.9, Column 7).

66 This is about one standard deviation of the country average of historical changes in the sample, excluding high-spending countries.

67 It is also reassuring that all the estimates based on instrumental variable approaches – either two-stage least squares (IV) or GMM – yield very close coefficients no matter what specification is used.

68 Care must be exerted, however, in drawing conclusions from the findings of Table 1.9. First, a potential source of bias in the estimates is that no account is made for the effectiveness of ALMP spending. For instance, a decline in the ratio of PES expenditures to GDP could well reflect an increase in efficiency through cuts in administrative costs rather than a decline in the "quality" of services provided to the unemployed. As a result, the lack of significance of ALMP categories other than training programmes does not necessarily imply that these are ineffective. Second, the results could be partly affected by the degree of consistency of the expenditure classification across countries. Third, programmes involving individual case management and mixed strategies might be classified as "training programmes", even though training constitutes only one of the components of the programme. Finally, workers on a training programme are often classified as inactive in labour force surveys and therefore cease being recorded as unemployed. As far as the latter caveat is concerned, it is important to check that the same results as in Table 1.9 hold when the employment rate is used as dependent variable, which will be done in section 2.5 below.

the series of adverse macro-economic shocks which have hit OECD countries, including oil price shocks, real interest rate shocks and the slowdown in the pace of technological progress.[69] The purpose of this section is to investigate these interactions between institutions and shocks.

Theoretical underpinnings and previous empirical evidence

51. There are a number of potential channels through which cross-country differences in policy settings may lead to divergent employment outcomes in the face of common shocks. In particular, many of the policies and institutions that have been put forward as explanations for high structural unemployment may also increase unemployment persistence. For instance, by protecting labour market "insiders" from the risk of income loss, high unemployment benefits and/or strict EPL can reduce the sensitivity of wages to general economic conditions, thereby preventing a swift adjustment of unemployment back to its initial level in the aftermath of a shock (see *e.g.* Blanchard, 1999). Increased "economic turbulence" – e.g. greater skill losses of laid-off workers due to skilled-biased technological progress, or more frequent reallocation of production factors across industries – may even lead to a permanent increase in unemployment in the presence of high unemployment benefits, as wages of laid-off workers fail to adjust to less favourable market conditions (Ljungqvist and Sargent, 1998).[70, 71] Moreover, strict PMR can further increase unemployment persistence by making labour demand less sensitive to wages. By contrast, certain categories of ALMPs such as job-search assistance can increase the influence of labour market "outsiders" –including the long-term unemployed, youth and/or certain groups of female workers– in wage determination and thus reduce wage and unemployment persistence. A high degree of centralisation and/or co-ordination of wage bargaining may also speed up wage adjustment to adverse shocks at the aggregate level. Other relevant structural settings, which are not covered below but have been studied in previous OECD work on resilience to economic shocks, include notably competition in financial markets.[72]

52. Recent empirical evidence[73] points to cross-country differences in the resilience of output and employment to shocks –most prominently between the United States and Continental European countries, and previous OECD work suggests that structural policy settings seem to matter in this respect.[74] These findings are consistent with the empirical literature indicating that interactions between institutions and

69 While shocks may have persistent effects on unemployment, not least via their interactions with policies and institutions, mainstream economic theory nevertheless suggests that the long-run unemployment effects of a permanent change in TFP, terms of trade, real interest rates and/or labour demand are likely to be either zero or small (see *e.g.* Blanchard and Katz, 1999).

70 Karanassou and Snower (1998) offer another theoretical model in which the presence of institutions that increase unemployment persistence also implies permanent unemployment effects of increased economic turbulence. In their model, labour demand and labour supply curves are continually drifting under the influence of new exogenous shocks, so that lagged adjustment processes towards a natural unemployment rate never have a chance to work themselves out entirely. As a result, the long-run equilibrium unemployment rate differs from the natural one, which can never be reached in a dynamic framework. The more persistent the employment level –due *inter alia* to existing institutions, the greater the gap between the long-run and the natural unemployment rate, and the greater its sensitivity to larger and/or more frequent shocks.

71 In practice, however, there is no robust empirical evidence that more frequent reallocation of production factors across industries has been taking place.

72 See, in particular, Drew *et al.* (2004) and Catte *et al.* (2004).

73 Amisano and Serrati (2003), Balakrishnan and Michelacci (2001), Balmaseda *et al.* (2000).

74 See *e.g.* Drew *et al.* (2004).

shocks have contributed to shape employment patterns over the past decades.[75] In their seminal paper, Blanchard and Wolfers (2000) find that interactions between time-invariant labour market policies and institutions – as measured by country averages of the corresponding indicators over the period 1960-1995 – and macroeconomic shocks have played a crucial role in explaining the rising dispersion of unemployment rates in OECD countries over time. However, no allowance is made in their analysis for the direct impact of changes in institutions, which casts some doubts on the magnitude of the estimated effects. Addressing this issue, two other recent studies which attempt to disentangle the roles played by institutions and interactions between institutions and shocks conclude that direct effects clearly dominate (Nickell *et al.*, 2005; Nunziata, 2002). Also, as already noted, the fact that the unemployment regressions presented in the previous sections account for a significant share of unemployment trends over the period 1982-2003[76] is consistent with the view that interactions between institutions and shocks are likely to provide *only* a complementary explanation for the evolution of unemployment.

Econometric analysis: unobserved shocks

53. This section reports the results of a comprehensive panel data econometric study of the unemployment effects of interactions between institutions and shocks. Ideally, the analysis should cover a lengthy time span encompassing in particular the major oil and real interest rate shocks of the 1970s and early 1980s. However, some of the OECD policy and institutional indicators are available only over shorter periods, *e.g.* EPL and the tax wedge since 1982 and 1979, respectively. In order to get around this difficulty, and in line with other recent studies (*e.g.* Blanchard and Wolfers, 2000), country averages of the indicators are computed over the longest available periods and are then interacted with time-varying measures of macroeconomic shocks.[77]

54. The analysis proceeds with the estimation by non-linear least squares of a simple unemployment equation with interactions between time-invariant institutions and unobserved shocks over 1970-2003, in the spirit of Blanchard-Wolfers (2000):

$$U_{it} = \lambda_t \left(1 + \sum_j \gamma_j (\overline{X}_i^j - \overline{X}^j) \right) + \alpha_i + \varepsilon_{it} \qquad [1.4]$$

75 At a more basic level, the baseline unemployment regression in Section 1.1 also hints at the presence of such interactions. As already discussed, there is statistical evidence that estimates of the average unemployment effects of policies and institutions are consistent. However, this is not the case for the output gap variable, for which coefficient heterogeneity across countries is found to be very large. A possible explanation for this finding could be the existence of interactions between temporary macroeconomic shocks –which drive output and employment away from their equilibrium levels, as reflected in a negative output gap– and heterogeneous institutions –which explain cross-country differences in the reaction of unemployment to these shocks.

76 See Figure 1.1.

77 This approach is in line with most of the recent empirical work in the field and has been shown to yield more stable results than when time-varying policy indicators are used (Blanchard and Wolfers, 2000). Another side benefit of the approach is to allow the use of the OECD estimate of collective bargaining coverage – which is available only for three years, 1980, 1990 and 2000 – instead of union density in order to capture unions' strength in wage bargaining (Ireland is an exception, for collective bargaining coverage data come from Belot and Van Ours, 2004). Also, an analysis of variance (ANOVA) carried out by the Secretariat (not reported here) shows that the variation in policies and institutions across countries by far outweighs their variation over time, at least over the sample considered. This suggests that only a limited amount of information is lost by omitting the time-series dimension of policy indicators. The only exception is the OECD measure of PMR in seven non-manufacturing industries, for which a time-varying indicator is therefore used over the period 1975-2003.

where $\overline{X}_i^j$ is the country average of policy indicator X^j for country i over the longest available period,[78] $\overline{X}^j$ is the sample average of policy indicator X^j [79] and λ_t is a time dummy variable which is assumed to capture an undefined set of shocks that are common to all countries.[80] However, one cannot safely estimate equation 1.4 with the full set of time-invariant institutions as explanatory variables, due to multicollinearity problems. Multicollinearity arises from the high (cross-country) correlation which exists between several of the policy indicators used as explanatory variables (Box 2).

Box 2. Cross-country correlation of policies and institutions: evidence and consequences for econometric analysis

Cross-country correlations between the various policy and institutional indicators used throughout this paper are usually high. For instance, as shown in the Table below, those countries that have strict (lax) EPL also tend to have high (low) tax wedges and a high (low) share of workers covered by collective agreements.

Cross-country correlation between country means of institutional indicators

Correlation coefficients

	Replacement rate	Tax wedge	Collective bargaining coverage	EPL	PMR (time varying)	High corporatism
Replacement rate	1					
Tax wedge	0.39	1				
Collective bargaining coverage	0.47	0.61	1			
EPL	0.16	0.67	0.55	1		
PMR (time-varying)	0.12	0.28	0.38	0.44	1	
High corporatism	0.30	-0.04	0.25	0.65	0.16	1

As a result, estimates of equation 1.4 including the entire set of time-invariant policies and institutions suffer from multicollinearity problems. The Table below illustrates this issue. In Column 1, equation 1.4 is estimated using the same set of institutions as in the baseline regression of Section 1.1 of the main text –except that union density is replaced by collective bargaining coverage. The results are in line with those of Blanchard-Wolfers (2000), *i.e.* all the coefficients have the expected signs and are statistically significant – albeit only at the 10% level in the case of PMR, with the exception of the tax wedge which is negatively signed. While most of the policy indicators keep the same sign and the same level of statistical significance when studied in separate equations (column 2-7), the coefficient of the tax wedge becomes positive and significant (column 3). One cannot draw robust policy conclusions from these equations either, as the positive coefficient of the tax wedge could simply capture the positive impact of another policy variable to which the tax wedge is strongly correlated.

78 The periods considered are 1970-2003 for unemployment benefit average replacement rates and the index of corporatism, 1979-2003 for the tax wedge, 1980-2003 for collective bargaining coverage, 1982-2003 for EPL, and 1985-2003 for ALMPs where applicable. As already noted, an exception is made for PMR, for which a time-varying indicator for seven non-manufacturing industries is used. However, this indicator is not available before 1975. Therefore, the PMR indicator used here mixes a time-invariant component – equal to its 1975 value – over 1970-1975 with a time-varying one over 1975-2003.

79 As done in the previous section, interaction terms are specified in deviation form.

80 Throughout this section, the output gap is never included in the specifications insofar as the focus is on shocks.

Interactions between institutions and shocks: various models with unobserved shocks, 1970-2003

	1	2	3	4	5	6	7
Interactions shocks/institutions:							
Replacement rate	0.03	0.03					
	[7.68]***	[6.80]***					
Tax wedge	-0.03		0.02				
	[4.05]***		[3.17]***				
Collective bargaining coverage	0.01			0.01			
	[3.90]***			[5.16]***			
EPL	0.11				0.11		
	[2.28]**				[2.92]***		
PMR	0.06					0.17	
	[1.73]*					[5.59]***	
High corporatism	-0.98						-0.54
	[10.21]***						[5.54]***
Observations	669	669	669	669	669	669	669
R-squared	0.81	0.77	0.76	0.77	0.76	0.77	0.77

Non-linear least squares. Absolute value of t statistics in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%

55. There is no straightforward way to address the multicollinearity issue. Here, in order to identify the relevant policy variables with a higher degree of confidence, the following strategy is implemented. Equation 1.4 is first estimated with two variables for all (the 15) possible pairs of policies and institutions. Those policies and/or institutions that are found to be insignificant in *at least* one of the 15 regressions are then discarded, and the remaining ones are built upon to estimate equations with three variables. The selection procedure continues until a final model is selected. It can be safely inferred from this "statistical tournament" that "surviving" policies and/or institutions significantly affect unemployment *via* their interaction with shocks. The final model selected through this procedure contains two variables (Table 1.10, column 1): consistent with previous results, high unemployment benefits are found to amplify the unemployment effects of a given shock, while a high degree of corporatism has the opposite effect.[81] Taken at face value, the estimates suggest that a 10 percentage point increase in the average benefit replacement rate raises the impact of a 1 percentage point *ex-ante* unemployment shock by 0.4 percentage points *ex-post*, while switching to a highly centralised/co-ordinated bargaining system would reduce it by 0.8 percentage points. No firm conclusions can be drawn as regards discarded policies and/or institutions – given that any possible significant impact may have been obscured by the even more significant effect of other variables, even though their impact seems to fit with theoretical priors.

[Table 1.10. Interactions between institutions and unobserved shocks: final models with unobserved shocks, 1970-2003]

81 An alternative selection procedure would be to consider all possible specifications with 2 interactions and: *i*) select those interactions that are *always* significant; *ii*) consider only specifications including the interactions selected at step (*i*) and consider all specifications with one additional interaction; *iii*) select those interactions that are significant at step (*ii*) and stop if no more than one is significant; otherwise iv) consider only specifications including the interactions selected at step (*iii*) and consider all specifications with one additional interaction; and select those interactions that are significant at step (*iv*) and stop if no more than one is significant; otherwise proceed in the same way until the final model is selected. With unobserved shocks, this selection procedure ends up with the same specification discussed above augmented by one interaction between collective bargaining coverage and shocks. However, when the selected specification is estimated with observed shocks or including the direct effect of institutions, the interaction between collective bargaining coverage and shocks becomes insignificant.

56. Column 2 of Table 1.10 augments equation 1.4 with the direct effect of policies and institutions on unemployment, which comes over and above their impact *via* interactions with shocks:

$$U_{it} = \sum_j \beta_j X_{it}^j + \lambda_t \left(1 + \sum_j \gamma_j (\overline{X}_i^j - \overline{X}^j) \right) + \alpha_i + \varepsilon_{it} \qquad [1.5]$$

where $\sum_j \beta_j X_{it}^j$ is the usual set of time-varying policies and institutions. In order to expand as much as possible the sample period, EPL is omitted, which should come at a minimal cost given the lack of significance of this policy indicator in the baseline unemployment regression of Section 1.1. Also, the tax wedge considered here is not derived from OECD tax models but rather comes from National Accounts.[82] As a result of these changes, equation 1.3 can be estimated over the period 1975-2003.

57. Two main results stand out. First, the impact of interactions between shocks and policies and/or institutions is found to be robust to controls for the direct effects of institutions, as illustrated by the statistically significant coefficients of unemployment benefits and the high corporatism dummy variable in $\overline{X}_i^j$. Second, the direct effects of policies and institutions appear to be consistent with those estimated in Section 1.1, *i.e.* unemployment benefits, the tax wedge and PMR –albeit only at the 10% level– tend to increase unemployment, while a high degree of corporatism tends to reduce it. Overall, these findings can be interpreted as evidence that direct and indirect effects of policies and institutions complement each other in explaining unemployment trends. There is also tentative evidence that expenditures on ALMPs lessen the unemployment effects of shocks (Table 1.10, column 3) while high rates of home ownership amplify them (column 4), even though –as is the case for most other explanatory variables– not too much should be made of these findings due to potential multicollinearity problems.

Econometric analysis: observed shocks

58. In order get a better grasp of the nature of shocks affecting unemployment, the set of time dummies used previously can be replaced by the set of observable shocks considered in Section 1.1:

$$U_{it} = \sum_j \beta_j X_{it}^j + \left(\sum_l \varphi_l Z_{it}^l \right) \left(1 + \sum_j \gamma_j (\overline{X}_i^j - \overline{X}^j) \right) + \alpha_i + \varepsilon_{it} \qquad [1.6]$$

where $\sum_l \varphi_l Z_{it}^l$ is the set of observed macroeconomic variables – labelled here "shocks" – to be interacted with policies and institutions.[83]

59. Table 1.11 indicates that these shocks indeed affect unemployment not only directly but also indirectly *via* their interactions with certain policies and institutions. The conclusions drawn previously from equations with unobserved shocks are largely confirmed when observed shocks are used instead. Overall, these and previous estimates strongly suggest that policies and institutions, macroeconomic shocks and interactions between them all contribute to shape unemployment patterns.

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82 Previous analysis (Table 1.2) shows that even though these tax wedges are conceptually different, they can be used alternatively in panel data unemployment regressions without affecting the results.

83 To allow the usual interpretation of the direct effect of institutions, each shock variable is defined here as the deviation of that variable from its sample mean.

[Table 1.11. Interactions between institutions and shocks: models with observed shocks]

Disentangling amplification from persistence effects

60. By construction, the static framework used up to here (Tables 1.10 and 1.11) provides no clue as to whether policies and institutions amplify (mitigate) the initial unemployment effect of a shock and/or make it more (less) persistent. Both of these aspects contribute to determine the degree of resilience of labour markets to adverse macroeconomic shocks. In this respect, those policies and institutions that amplify the initial impact of a shock may not necessarily increase its persistence, and *vice versa*. For instance, high firing costs may deter firms from laying off workers in the short run, but they might slow down the wage adjustment process as well as workers' reallocation towards more productive jobs, thereby delaying the return of unemployment to its lower initial level. Likewise, stringent PMR creates rents that allow firms to minimise lay-offs in the short run, but it may slow down resource reallocation and lengthen the period of excess unemployment by making labour demand less responsive to the likely decline in wages. To explore this issue further, various versions of the following dynamic model with unobserved shocks are estimated:

$$\Delta U_{it} = -\left(\phi - \sum_j \gamma_j (\overline{X}_i^j - \overline{X}^j)\right) U_{it-1} + \lambda_t \left(1 + \sum_k \gamma_k (\overline{X}_i^k - \overline{X}^k)\right) + \alpha_i + \varepsilon_{it} \qquad [1.7]$$

61. This equation clearly disentangles amplification from persistence mechanisms. The lagged unemployment term on the right-hand side captures unemployment persistence, while the second one describes short-term interactions between institutions and shocks, *i.e.* the amplification mechanism. Both the persistence and amplification of unobserved shocks are supposed to be a function of policies and institutions. A positive (negative) and significant γ_j implies that the policy or institution $\overline{X}_i^j$ considered increases (reduces) the persistence of unemployment. Likewise, a positive (negative) and significant γ_k implies that the policy or institution $\overline{X}_i^k$ considered amplifies (mitigates) the initial unemployment effect of a shock.

62. The starting point of the analysis is the final, static model selected previously, either with unobserved or observed shocks (Tables 1.10, column 2, and Table 1.11, column 3, respectively). Dynamic versions of this model[84] suggest that unemployment benefits and a high degree of corporatism affect the initial impact of a shock but not adjustment mechanisms (Tables 1.12 and 1.13, column 1).[85] Consequently, both of these institutions are dropped from the lagged unemployment coefficient (Tables 1.12 and 1.13, column 2), and the equation obtained serves as a basis for the reassessment of those policies and institutions which were not found to be robust to the selection procedure followed earlier. Each of these is studied in separate regressions in order to keep parsimonious specifications and minimise multicollinearity problems.

84 As before, the model is estimated by non-linear least squares. Since the model is dynamic, estimates suffer from the standard downward dependent variable bias (Nickell, 1981). However, the downward lagged dependent variable bias falls as the time span of the sample grows and it is less of a concern when the time span is large and of the same order of magnitude as the number of countries (Judson and Owen, 1999).

85 In the case of unemployment, the results may seem counterintuitive. *A priori,* high unemployment benefits would be expected to buffer the shock, while long-lasting benefits should slow down the adjustment *via* lower job search intensity. However, unemployment benefits may also impair wage flexibility and amplify the short-run effect of shocks *via* this channel,

63. There is evidence that a high tax wedge mitigates the initial impact of a shock, possibly reflecting the stronger role played by fiscal stabilisers in high-tax countries, while no significant effect is found on persistence (Tables 1.12 and 1.13, column 3). The strength of unions in wage bargaining –as captured by collective bargaining coverage– seems to increase persistence, although its impact is significant only at the 10% level and when unobserved shocks are considered (Table 1.12, column 4). As expected, stringent EPL and PMR appear to reduce the short-run impact of an adverse shock but lengthen the adjustment process, although the negative short-run effect of PMR is significant only when observed shocks are considered (Tables 1.12 and 1.13, column 5). This finding suggests that EPL and PMR were eliminated by the selection procedure of the static model because they have opposite effects on amplification and persistence.[86] High expenditures on ALMPs are found to reduce the persistence of shocks, and possibly their initial impact (Tables 1.12 and 1.13, column 7), providing some support for the view that certain categories of ALMPs improve the job-matching process. Finally, high rates of home ownership seem to amplify the initial impact of shocks while also lengthening the adjustment process, although the persistence effect is insignificant when observed shocks are considered (Tables 1.12 and 1.13, column 8).

[Table 1.12. Disentangling persistence from amplification effects: unobserved shocks, 1970-2003]

[Table 1.13. Disentangling persistence from amplification effects: observed shocks, 1970-2003]

Summing up

64. The main conclusion of this Section is that policies and institutions not only affect unemployment patterns *via* their direct effects but also *via* their interactions with economic shocks. There is clear empirical evidence that the unemployment effects of TFP, terms of trade, real interest rate and labour demand shocks are at least partly shaped by the policy and institutional framework. Identifying precisely which policies and institutions matter in this respect is not straightforward, not least because most countries tend to have similar policy stances –either lax or stringent– in all areas. Still, the econometric analysis points to a number of policy conclusions. In particular, high unemployment benefits are found to amplify the unemployment effects of adverse shocks. By contrast, a high degree of corporatism appears to improve the resilience of the labour market. There is more tentative evidence that high expenditures on ALMPs have similar effects, while by contrast high rates of home ownership appear to slow down labour market adjustment. Also, stringent EPL and/or PMR, while mitigating the initial impact of adverse shocks, seem to make it more persistent.

2. THE DETERMINANTS OF EMPLOYMENT RATES

Introduction

65. Good labour market performance depends not only on low rates of unemployment but also on high levels of employment, especially in the context of population ageing. To some extent, labour force participation decisions certainly reflect households' optimal decisions as regards the trade-off between

86 However, the separate impact of EPL and PMR is difficult to disentangle insofar as they are highly correlated and interact with shocks through similar channels. In fact, the effect of these variables becomes somewhat less significant when they are simultaneously included.

work and alternative activities –including *inter alia* leisure, education or child rearing. In practice, however, previous OECD analysis[87] has shown that current policy distortions result in sub-optimal participation rates in many OECD countries, in particular for certain groups that are often "at the margin" of labour market such as youth, females and older workers. Another related issue, which is not addressed in this paper, is the impact of policy distortions on hours worked.

66. This Section aims to explore the policy and institutional determinants of employment rates. The empirical analysis proceeds by estimating pooled cross-country / time-series regressions, all of which incorporate within the same framework two types of variables: i) the determinants of unemployment studied in the baseline regression of the previous Section, namely average unemployment benefit replacement rates, tax wedges, union density, EPL, PMR and a dummy variable for high corporatism; and, ii) driving factors of labour force participation. Insofar as the latter typically vary across population groups, the analysis is undertaken separately for prime-age males, prime-age females, older workers and youth.

2.1 Prime-age males

67. The employment equation for prime-age males follows the same specification as the baseline unemployment equation of Section 1:

$$E_{it} = \sum_j \beta_j X_{it}^j + \chi G_{it} + \alpha_i + \lambda_t + \varepsilon_{it} \qquad [2.1]$$

where i and t are country and time suffixes, α_i and λ_t are country and time fixed effects, E_{it} is the total employment rate[88] of males aged 25-54, G_{it} is the OECD measure of the output gap and the X^js are the policy and institutional variables studied in section 1.1. In theory, additional determinants could be incorporated into this equation to better capture labour supply behaviour. In practice, however, the very high and relatively stable level of prime-age males' participation rates in the vast majority of OECD countries implies that unemployment is the main driver of employment rates for this age-group.

68. Econometric estimates of equation 2.1 indicate that high unemployment benefits and high tax wedges reduce prime-age males' employment rates (Table 2.1, column 1), while EPL has an insignificant effect. Taken at face value, coefficient estimates imply that a 10 percentage point rise in the average benefit replacement rate and the tax wedge reduce the employment rate of prime-age males by 1.7 and 3 percentage points, respectively. Such effects are consistent with those obtained in Section 1 from aggregate unemployment rate equations. However, certain results differ from previous analysis. In particular, union density is found to increase the employment rate of prime-age males –while no impact was found in general on aggregate unemployment, and strict PMR is insignificant. These findings appear consistent with an insider-outsider characterisation of the labour market, in which the jobs of prime-age males are better protected than those of other population groups by high unionisation rates and sheltered from any detrimental effects of strict PMR.

[Table 2.1. Employment rate equations: prime-age males and prime-age females, 1982-2003]

69. Overall, the estimated equation does a fairly good job at explaining past trends in prime-age male employment rates for most OECD countries (Figure 2.1), even though its explanatory power is not as high as that of the baseline unemployment regression estimated in Section 1. Policy changes alone do not

87 See *e.g.* Burniaux, Duval and Jaumotte (2004) and OECD (2002, 2003).

88 The main source for this variable is the OECD *Database on Labour Force Statistics*. See Annex 2 for details.

account for the performance of certain countries –such as Finland after the macroeconomic shocks of the early 1990's (Figure 2.1, Panel A), but changes in policies and cyclical conditions together well explain past employment trends (Figure 2.1, Panel B). Still, the gradual decline in prime-age employment in Germany cannot be accounted for while, by contrast, the pick up in prime-age male employment in the Netherlands since the early 1980s has exceeded model forecasts. This is consistent with the results obtained for aggregate unemployment (see Figure 1.1). Overall, however, policies, time dummies and the output gap are found to account for only 57% of the cross-country variation of changes in the employment rate of prime-age men between 1982 and 2003.

[Figure 2.1. Prime-age male employment rate equation: explaining past employment trends, 1982-2003]

2.2 Prime-age females

Econometric framework

70. In order to study prime-age females' employment rates, equation 2.1 is augmented with a number of policy variables which have been found to be important determinants of females' labour force participation in previous studies, including OECD work (Jaumotte, 2004). These include: [89]

- The relative "marginal" tax rate on second earners, defined as the ratio of the marginal tax rate on second earners – *i.e.* the share of additional income that goes into paying increased household taxes when a previously inactive spouse takes up a job – to the labour tax wedge, for a single-earner couple with two children in which the husband earns 100% of APW earnings. This variable aims to capture the progressiveness of the tax system for a married woman. Two relative "marginal" tax rates are considered in practice, corresponding to two alternative income levels of the second earner: 66% of APW earnings, corresponding approximately to a full-time job for an average female if account is made for existing wage gaps between sexes; and 33% of APW earnings, corresponding to a part-time job. There is ample evidence that high marginal tax rates reduce labour supply in general,[90] and these effects are likely to be greater for females, whose labour supply is more elastic.

- The tax incentives for females to work part-time, defined as the gain (loss if negative) in household disposable income from sharing market work between spouses. This is calculated as the difference in household net income between a situation in which the husband earns 133% of APW while his wife stays out of the labour force and a situation in which the husband earns 100% of APW and his wife earns 33% of APW, possibly by taking up a part-time job. This difference is expressed as a percentage of household net income in the second situation.

- Child benefits, defined as the impact of having two children – as opposed to having none – on household net income. While the existence of child benefits is typically justified on equity grounds rather than for labour market purposes, they generate an income effect which, if anything, is likely to deter – typically lower-educated – women from participating in the labour market.

- The total number of leave weeks, defined as the maximum number of leave weeks that can be taken by a mother for the birth of a first child as maternity leave, parental leave and childcare

89 For further details on data sources and methodology, see Annex 2 and Jaumotte (2004).

90 See *e.g.* Journal of Human Resources (1990).

leave. The effects of these leaves on female labour supply are *a priori* ambiguous. They should boost female participation by helping women to reconcile working and family life. The job security dimension also strengthens the continuity of their attachment to the labour market, though negative effects on hiring cannot be excluded. However, taking parental leave for an extended period may also weaken labour market skills and convey a negative signal to current and future employers, thereby damaging future career paths and earnings (Edin and Gustavsson, 2004).[91]

- Public expenditures on childcare, defined as public spending on formal day care and pre-primary school per child (in 1995 PPP-US$). Childcare subsidies contribute to remove existing distortions to female labour supply arising from tax and benefit systems (See *e.g.* Krashinsky, 1981), and may also help lower-educated females to overcome the credit market imperfections that prevent them from borrowing against future earnings to break away from welfare dependence (Walker, 1992).

71. The specification also controls for a number of other influences on female participation, including female education (average years of education of women aged 25 years and over), the proportion of married women and the number of children per woman (measured by the ratio of children aged 0-14 years to women aged 15-64 years).[92, 93]

72. Furthermore, the analysis is undertaken separately for full-time and part-time female workers in order to allow for possible differences in the magnitudes of policy effects across both groups. For example, tax incentives to part-time should boost the take-up of part-time jobs but may at the same time discourage women from working full-time. Likewise, while the income effect associated with child benefits may be small for women who occupy full-time jobs, it could be significantly greater in the case of part-time, often lower-educated females.

73. Finally, an account needs to be made for the fact that full-time and part-time female employment rates are not independent from one another. First, substitution effects exist between both types of jobs. Second unobserved variables and common exogenous shocks tend to affect them simultaneously. In addition, while certain shocks are idiosyncratic to female employment, others (such as macroeconomic shocks) affect both male and female workers, and can be better identified if the information provided by prime-age men employment patterns is used. In order to account for such links, the following three equations are estimated simultaneously within a Seemingly Unrelated Regression (SURE) framework:[94]

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91 There might also be a direct, statistical effect of parental leave on female employment, if women on leave end up being classified as inactive in labour force surveys.

92 However, it should be acknowledged that this latter variable is not fully exogenous and, to some extent, is likely to be influenced by the same policy and institutional factors that affect female participation and employment.

93 The set of control variables also includes the interaction between the proportion of married women and the number of children per woman. The logic behind this interaction term is the following. While having children may reduce the labour force participation of married women, this effect should be smaller and possibly reversed in the case of lone mothers. Here, the proportion of married women is used as a crude way to capture the share of women living in couple.

94 Ideally, the estimated SUREs would also feature two equations for older workers and youth, whose employment rates are also likely to be linked to those of females. This would allow for a joint estimation of employment equations for all the groups studied in this Chapter. However, in practice, a major drawback of this approach is that the joint estimation would be made on a much shorter dataset than if separate SURE estimates were carried out for females, older workers and youth. This is because the estimation sample

$$E_{it}^{f-FT} = \sum_j \beta_j^{f-FT} X_{it}^j + \sum_k \gamma_k^{f-FT} Z_{it}^k + \chi^{f-FT} G_{it} + \alpha_i^{f-FT} + \lambda_t^{f-FT} + \varepsilon_{it}$$

$$E_{it}^{f-PT} = \sum_j \beta_j^{f-PT} X_{it}^j + \sum_k \gamma_j^{f-PT} Z_{it}^k + \chi^{f-PT} G_{it} + \alpha_i^{f-PT} + \lambda_t^{f-PT} + \nu_{it} \qquad [2.2]$$

$$E_{it}^{m} = \sum_j \beta_j^{m} X_{it}^j + \chi^{m} G_{it} + \alpha_i^{m} + \lambda_t^{m} + \tau_{it}$$

where E_{it}^{f-FT} and E_{it}^{f-PT} are the full-time and part-time prime-age female employment rates, respectively, E_{it}^{m} is the prime-age male employment rate and the Z^k's are the specific policy influences on female employment discussed above (including control variables). These three equations have distinct coefficients for all variables including the country and time fixed effects. They are linked only by their disturbances ε_{it}, ν_{it} and τ_{it}.[95]

Econometric results

74. The results from the prime-age female employment rate equations estimated within this SURE framework are presented in Table 2.1 (column groups 2-5).[96] The baseline estimates[97] (column group 2) indicate that high unemployment benefits, high tax wedges and stringent PMR all reduce full-time, part-time and therefore aggregate prime-age female employment rates. Aggregate elasticities are typically larger than for males, consistent with the view that female participation and employment is more sensitive to economic conditions. While high unemployment benefits could have been expected to discourage part-time employment more than full-time employment, no significant difference is found here in practice. The negative effect of PMR – a decline in the PMR indicator by two standard deviations is estimated to increase the prime-age female employment rate by 1.8 percentage point – did not show up for males, and could reflect several factors. First, excessive regulation tends to restrict the supply and drive up the prices of services such as childcare and household services. In addition, restricted opening hours of shops also make it difficult for women to reconcile work and family life. Also, by hindering the development of the service sector, excessive regulations of the service market may limit the creation of employment opportunities for women, who tend to be predominantly employed in the service sector.

75. Certain policies and institutions have no impact on aggregate female employment rates but are found to exert contradictory effects on full-time and part-time employment. In particular, strict EPL is

would be the (smaller) set of observations for which each explanatory variable of each equation is available.

95 Expected contemporaneous correlations across disturbances are as follows: $E(\varepsilon,\nu) < 0$, due to substitution between full-time and part-time work and $E(\varepsilon,\tau) > 0$, due to common aggregate shocks.

96 In each of these columns and in accordance with [2.2], prime-age male employment equations have also been estimated but for convenience they are not reported here.

97 Childcare subsidies are excluded from these baseline estimates for two main reasons. First and foremost, insofar as this explanatory variable is available over a much smaller sample than others, introducing it into the estimated equations implies a substantial loss of information. Second, childcare subsidies are highly endogenous –they typically rise with female employment– and as such should be given a specific statistical treatment (see below).

associated with a substitution of part-time for full-time work, although it has no (net) effect overall.[98] One possible reason for this finding is that strict EPL induces firms to resort to part-time contracts in order to achieve greater flexibility. By the same token, the positive and significant effect of union density on full-time employment contrasts with its negative impact on part-time work, and might signal unions' propensity to promote full-time as opposed to part-time job opportunities. Finally, while there would seem to be evidence that a high degree of corporatism is detrimental to full-time female employment, this result is not robust across specifications and hinges entirely on the presence of Australia in the estimation sample.

76. As regards those policies and institutions that affect female employment more specifically, one important conclusion is that taxation matters. The net impact of tax incentives to part-time on female employment rates is positive and significant, with the positive coefficient on part-time work more than offsetting the negative coefficient on full-time work. This suggests again that expanding part-time work opportunities can attract new female workers into the labour market.[99] Column group 2 shows no significant employment effects of relative marginal tax rates on second earners, over and above the impact of tax incentives to part-time and the overall tax wedge. However, the significant (negative) relationship which exists *de facto* between relative marginal tax rates and tax incentives for part-time work may partly account for the lack of significance of the former.

77. To explore this issue further, in column group 3 tax incentives to part-time are dropped from the set of explanatory variables and the two marginal tax rates on second earners mentioned earlier are considered, namely one on "full-time work" and another on "part-time work". Two main results stand out from this exercise. First, high marginal taxes on "part-time work" reduce (at the 10% level) part-time female employment rates, while they increase full-time rates. Second, high marginal taxes on full-time work lower full-time employment rates (while no significant effect is found on part-time employment). Interestingly, the coefficients of both marginal tax rates have similar magnitude but opposite signs in the full-time employment rate equation. This tentatively indicates that it is the difference between these marginal tax rates, *i.e.* the marginal tax on moving from a part-time to a full-time job, which matters for females' decision to work full-time. Taken as a whole, the estimates in column groups 2 and 3 strongly suggest that tax incentives influence both females' decision to enter the labour force and the number of hours they are willing to work.

78. Parental leaves appear to be detrimental to part-time work, but they have a positive impact on full-time employment (the overall effect remains negative but is significant only at the 10% level). However, there is tentative evidence that the estimated coefficients change signs when the number of leave weeks becomes large (column 4), suggesting that extended parental leaves actually foster the development of part-time work to the detriment of full-time jobs. Child benefits are found to reduce aggregate female employment rates through their significant negative impact on part-time work. It is indeed only for part-time female workers that the income effect from child benefits is likely to be large enough to induce a reduction in participation and employment. Overall, a 10 percentage point rise in the child benefit indicator (child benefits for two children as a share of APW earnings, in per cent) is estimated to reduce the prime-

98 See Table 2.1. The null assumption that the sum of the EPL coefficients in full-time and part-time employment equations is zero cannot be rejected at the 5% level (F-statistics = 0.61, p-value = 0.43). Similar results are obtained for union density and high corporatism.

99 Taken at face value, the estimates imply that a 10% gain in net household income by sharing market work between spouses would raise the prime-age female employment rate by over 7 percentage points, compared with a situation in which the tax system provides no such incentive to split total income. An effect of such magnitude looks implausibly large, and indeed hinges on the presence of the Netherlands, the country which has experienced the largest increase in both tax incentives to part-time and female employment rates over the period 1982-2003. When the Netherlands are dropped from the sample, the impact of tax incentives to part-time remains statistically significant but becomes three times smaller.

age female employment rate by 2.4 percentage points. By contrast, in line with previous studies (Jaumotte, 2004; Powell, 1998), childcare subsidies are found to stimulate full-time and aggregate female employment (column 5).[100] This result, along with the negative employment effects of child benefits, confirms that from the point of view of raising female participation and employment, childcare subsidies are preferable to child benefits, as only the former increase the return from market work for mothers.

79. Finally, the estimates confirm that education stimulates female participation and employment *via* an increase in full-time employment rates. Taken at face value, the estimates imply that a one-year rise in female education length increases the aggregate female employment rate by 2.3 percentage points. Another noticeable result is the lack of statistical significance of the output gap variable in the part-time employment equation, which indicates that unlike full-time employment, part-time employment is not pro-cyclical. A possible interpretation is that the direct negative (positive) impact on part-time employment of reduced (increased) labour demand during economic downturns (upturns) tends to be offset by the downward (upward) adjustment of working hours. Overall, in terms of signs and statistical significance, the results in Table 2.1 are broadly in line with previous OECD work (Jaumotte, 2004).[101]

80. In several OECD countries, including the Netherlands and, to a lesser extent, Denmark and the United Kingdom, changes in policies and institutions appear to have made a significant contribution to the rise in female employment rates that took place over the past two decades (Figure 2.2, Panel A). For instance, the large increase in female employment experienced by the Netherlands seems to some extent to be explained by a combination of general (tax cuts, product market deregulation) and specific (increase in tax incentives for part-time work, decline in child benefits) policy reforms. Still, unsurprisingly, changes in policies and institutions alone typically under-estimate the trend rise in female participation and employment observed in most OECD countries. Other factors – the control variables, such as increased female education – also need to be taken into account in order to explain female employment trends across the OECD (Figure 2.2, Panel B). Overall, policies and control variables are estimated to explain 81% of the cross-country variation of the changes in the employment rate of prime-age women between 1982 and 2003.

[Figure 2.2. Prime-age female employment rate equation: explaining past employment trends, 1982-2003]

2.3 Older workers

81. A similar econometric approach is followed to evaluate policy and institutional influences on the employment rates of older workers, except that part-time and full-time work are no longer treated separately, given the prevalence of full-time work among the 55-64 age group (among males in particular) in the vast majority of OECD countries. Concretely, the baseline employment equation [2.1] is augmented with specific determinants of older workers' participation and employment rates and is estimated jointly with a prime-age male employment equation within the following SURE framework:

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100 Two issues need to be stressed regarding the regression in column 5. First, due to limited availability of data on public childcare expenditures, the equation is estimated on a much smaller sample than those in columns 2-4. Second, instrumental-variable techniques were used in order to address the endogeneity of childcare spending. Public childcare expenditures per child were instrumented by their lagged change (which was found to be an acceptable instrument on the basis of a Fisher test).

101 The size of coefficients cannot be readily compared, due to wide differences in both the specification and the econometric approach used.

$$E_{it}^{55-64} = \sum_j \beta_j^{55-64} X_{it}^j + \sum_k \gamma_k^{55-64} Z_{it}^k + \chi^{55-64} G_{it} + \alpha_i^{55-64} + \lambda_t^{55-64} + \varepsilon_{it} \qquad [2.3]$$

$$E_{it}^m = \sum_j \beta_j^m X_{it}^j + \chi^m G_{it} + \alpha_i^m + \lambda_t^m + \tau_{it}$$

where E_{it}^{55-64} is the employment rate of the 55-64 age group and the Z^k's now represent a set of policies and institutions that affect the participation and/or employment rates of older workers but not those of prime-age males.

82. The specific policies considered here capture early retirement incentives embedded in pension systems and other social transfer programmes, which have been found to play a significant role in the empirical literature on retirement behaviour,[102] including OECD work (Duval, 2004): More specifically, the following two policy variables are incorporated into the older workers' employment equation:

- The implicit tax (subsidy) on continued work, defined as the loss (gain) in net pension wealth from continuing to work, where net pension wealth is defined as the present value of the future stream of pension payments to which a person is entitled over his or her remaining life-time minus the future stream of contributions. Provided that an individual is already eligible for a pension, and that the receipt of a pension cannot be combined with earnings from work, remaining in the labour market for an additional year implies foregoing one year of benefits. If the cost in terms of foregone pensions and contributions paid is not exactly offset by an increase in future pension benefits, there is an implicit tax on continued work which favours early labour market withdrawal, *ceteris paribus*.[103] High implicit taxes may arise not only from old-age pension systems but also from the various social transfer programmes that enable certain categories of older workers to withdraw before the earliest age of eligibility for old-age pension,[104] mainly because these schemes typically provide high replacement rates and old-age pension rights continue to accrue (even if, in some cases, at a reduced rate). Therefore, for the econometric analysis below, a summary measure of implicit tax rates over the age span 55-64 is constructed as a weighted average of implicit taxes on continued work over the age spans 55-59 and 60-64 in both old-age pension systems and early retirement schemes.[105]

- The standard age of entitlement to old-age pension benefits. In theory, the age of eligibility to a pension should not necessarily affect the actual age of retirement and thus the participation and

102 See *e.g.* Gruber and Wise (1999, 2002).

103 In theory, the labour supply effects of implicit taxes on continued work are more complex (Mitchell and Fields, 1984), as – similar to changes in wages – they produce opposite income and substitution effects. In practice, however, there is overwhelming empirical evidence that substitution effects are dominant (Lazear, 1986; Lumsdaine and Mitchell, 1999), so that implicit taxes on continued work tend to bias the retirement decision towards early labour market withdrawal. In addition, any estimate of the impact of implicit taxes may also capture some of the retirement effects of the generosity of (early retirement and pension) benefits. This is because implicit taxes and benefit replacement rates often tend to be correlated in practice: *ceteris paribus,* the higher the replacement rate, the higher is the "opportunity cost" of, and therefore the implicit tax on, continued work.

104 These schemes include special early retirement provisions as well as unemployment-related and disability benefits. For detailed analysis, see Casey *et al.* (2003) and OECD (2006c).

105 See Annex 2 for methodological details. For a more disaggregated analysis *–i.e.* by 5-year instead of 10-year age groups– of the retirement effects of implicit taxes on continued work, see Duval (2004).

employment rates of older workers. The reason is that rational forward-looking individuals could always set their retirement age at the optimal level chosen to maximise their welfare, by trading off between consumption and leisure (including retirement) over their life cycle through lending and borrowing in capital markets. However, there is clear evidence in practice that many workers choose to retire at early and standard retirement ages. Several factors have been put forward in order to explain such a "bunching" of retirement ages, including: *i)* liquidity constraints, which prevent some individuals from borrowing in order to retire before pension benefits are available; *ii)* custom or accepted practice (Lumsdaine *et al.,* 1996); and *iii)* myopia or information constraints, implying that workers do not assess accurately actuarial incentives/disincentives to continued work embedded in pension systems and thus tend to retire at the earliest age at which benefits become available.

83. Results from such older worker employment rate equations are presented in Table 2.2.[106] As expected, and in line with previous OECD work,[107] the baseline equation indicates that high implicit taxes on continued work deter older workers from remaining in the labour market, while high statutory retirement ages have the opposite effect (Table 2.2, column 1). A 10 percentage points cut in the implicit tax and a one-year increase in the standard retirement age are estimated to raise the employment rate of older workers by 1 and 0.6 percentage points, respectively. Consistent with the existing literature (see e.g. Coile, 2003), these effects are somewhat larger in the case of older males, even though the difference in coefficients across the two equations is not statistically significant at the 5% level (Table 2.2, column 2).

[Table 2.2. Employment rate equations: older workers (55-64 age group), 1982-1999]

84. The estimates also point to significant negative effects of high unemployment benefits and high tax wedges on the employment rate of older workers, with estimated coefficients that do not statistically differ from those obtained earlier for prime-age males. By contrast, union density is now found to reduce older workers' employment. This finding would be consistent with the view that strong unions may compress the wage structure (see *e.g.* Bertola *et al.* 2002b) and drive a wedge between the labour cost and the marginal productivity of least productive workers, thereby pricing them out of the job market. Alternatively, unions could push for early retirement options for older workers. In the case of older workers, the wage-productivity gap could be further enlarged by the presence of rigid, seniority-based pay scales. In a similar vein, high corporatism is negatively signed and significant, even though one needs to stress that excluding Italy from the estimation sample would result in an insignificant coefficient.

85. Another noticeable difference with prime-age males is the significant positive impact of EPL and PMR – albeit significant only at the 10% level in the latter case. A rise in the EPL and PMR indicators by two standard deviations is found to increase older worker employment rates by 2.7 and 0.6 percentage points, respectively. Therefore, while EPL and/or PMR are likely to reduce the job opportunities of older job seekers through their negative impact on hiring rates, this seems to be more than offset by a reduction in the probability that incumbent older workers are laid off.

86. However, there is evidence that the estimated impact of EPL is conditional on existing incentives for older workers to withdraw from the labour market. Indeed, the positive and significant interaction between EPL and the implicit tax on continued work – as well as the smaller direct positive effect of EPL when such interaction is introduced – implies that any positive effects of EPL on the employment rate of

[106] As for female employment rate equations, each of the 55-64 age group employment rate equations has been estimated jointly with a prime-age male equation which, for convenience, is not reported here.

[107] See Duval (2004). While both studies are consistent in terms of signs and statistical significance, the size of coefficients cannot be readily compared, due to wide differences in both the specification and the econometric approach used.

older workers are larger where retirement incentives are high (Table 2.3, column 3).[108] As a result, pension and EPL reforms appear to be complementary in this case, *i.e.* the laxer the stance of EPL, the greater the employment effects of a reduction in implicit taxes on continued work. One possible interpretation is that lax EPL facilitates the needed adjustments on the labour demand side in order to accommodate the increase in labour force participation rates brought about by pension reforms, thereby maximising employment gains. The estimated coefficient of EPL might then partly reflect the fact that strict EPL coincided with the existence of significant retirement incentives in a number of OECD countries over the sample period. These retirement incentives have typically enabled certain categories of older workers to withdraw from the labour market upon being laid off, without having to go through a job-search process whose chances of success would be impaired by stringent EPL. An alternative interpretation of the positive interaction between EPL and the implicit tax on continued work is that, when protected from lay offs by strict EPL, older workers may be less inclined to use early retirement schemes, because of the lower probability that they will be forced to quit their job at an inopportune time.

87. Changes in policies and institutions are found to account for a significant share of the changes in older workers' employment that took place in OECD countries during the 1980s and the 1990s (Figure 2.3, Panel A). The contribution of changes in cyclical conditions is comparatively small (Figure 2.3, Panel B). Policy reforms appear to have contributed to stimulate the participation and employment of older workers in the New Zealand and, to a lesser extent, in Australia, the United Kingdom and the United States. By contrast, they have discouraged participation and employment of older workers in France, Italy, Portugal and Spain over the sample period 1982-1999.[109]

[Figure 2.3. Older worker employment rate equation: explaining past employment trends, 1982-1999]

2.4 Younger workers

88. Cross-country / time-series econometric studies of employment outcomes are inherently more difficult to undertake for youth than for the other population groups considered above, for both conceptual and empirical reasons. On the theoretical side, the main problem is that enrolment in education is one of the main reasons behind low youth participation. To a significant extent, the downward trend in youth employment rates observed in a number of OECD countries over the recent decades reflects an increase in the duration of education, which in turn has a potential pay-off in terms of future standards of living, all the more so as past OECD work has found that private and social rates of return on higher education typically exceed those on other productive assets.[110] On the empirical side, several important drivers of youth education and labour force participation decisions cannot be studied due to lack of data, including returns on education,[111] the effectiveness of the education system – as measured by its ability to provide students

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108 The interaction between the implicit tax and EPL is robust to the omitted interaction bias discussed in Chapter 1, *i.e.* it remains statistically significant even when interactions with country fixed effects are taken into account. By contrast, interactions between the implicit tax on continued work and policies and institutions other than EPL –including a positive interaction with PMR– were not found to be robust to such exercise. Therefore, they are not reported here.

109 This does not take into account recent reforms in a number of OECD countries. For instance, in the case of Italy and France, the participation effects of recent pension reforms had not yet materialised in 1999, which is the end date for the simulations undertaken in Figure 2.3 (see Burniaux, Duval and Jaumotte, 2003).

110 See Blöndal *et al.* (2002). In some OECD countries, the trend increase in the duration of education may also reflect to some extent poor labour market prospects for youth.

111 While OECD data on returns from education have been assembled in the past (Blöndal *et al.,* 2002), they only cover a limited range of countries and most importantly are not available in a time-series dimension.

with a given educational level within a minimal number of years – or certain features – such as the level of tuition fees – that influence the extent to which students choose to combine education with part-time work. As a result, it is not possible to rule out the existence of an omitted variable bias in the regressions below.

89. Bearing these caveats in mind, the baseline employment equation [2.1] is augmented with specific determinants of youths' participation and employment rates and is estimated jointly with a prime-age male employment equation within the following SURE framework:

$$E_{it}^{Y} = \sum_{j} \beta_j^Y X_{it}^j + \sum_{k} \gamma_k^Y Z_{it}^k + \chi^Y G_{it} + \alpha_i^Y + \lambda_t^Y + \varepsilon_{it} \qquad [2.4]$$

$$E_{it}^{m} = \sum_{j} \beta_j^m X_{it}^j + \chi^m G_{it} + \alpha_i^m + \lambda_t^m + \tau_{it}$$

where E_{it}^{Y} is the youth employment rate and the Z^k's now represent specific policy and control variables that have been found to significantly affect youth employment rates in some previous studies, namely:

- Minimum wage legislation, captured as previously by the ratio of the youth minimum wage to APW earnings.[112] The impact of minimum wage legislation on youth employment is theoretically ambiguous. While a high minimum wage may increase school dropouts and therefore labour force participation, it can also drive a wedge between youth labour costs and their expected productivity, thereby raising unemployment and discouraging some of them to enter the labour market. Several cross-country or cross-region empirical studies have identified negative effects of minimum wages on youth employment (*e.g.* OECD, 1998; Neumark and Wascher, 1999), but other have failed to do so (*e.g.* Card and Krueger, 1995).

- Cohort effects, measured as the ratio of youth population to the total working-age population. Existing empirical literature is generally supportive of the "cohort crowding" hypothesis, according to which larger youth cohorts face reduced job opportunities in the presence of imperfect substitutability between workers of different ages and wage rigidities (OECD, 1996; Korenman and Neumark, 1997; Jimeno and Rodriguez-Palenzuela, 2002). However, while such effects are reasonably well-established as regards youth unemployment,[113] it is far less clear whether they also apply to participation and thus employment.

- Relative youth education, captured by the difference between the number of years of education of the over 15 and the over 25 age groups. The presence of this variable in the estimated equation aims to control for the trend rise in education observed in most OECD countries,[114] which is expected to have reduced youth participation and employment rates, *ceteris paribus*. However, as already discussed, it has to be acknowledged that youth education and participation are jointly

112 The youth minimum wage is computed here as a simple average of the minimum wages for individuals aged 20, 21, 22, 23 and 24. This variable differs from the adult minimum wage only in those four countries where subminimum rates apply for younger workers (Belgium, Ireland, Netherlands, United Kingdom).

113 A discordant view is provided by Shimer (2001). Based on cross-state / time-series analysis on US data, the author finds that larger youth cohorts are associated with *lower* youth unemployment. Nordstrom Skans (2005) finds similar results on Swedish data. One possible interpretation for these findings is the following: in a labour market with a greater (smaller) share of older workers in good job matches, firms are more (less) reluctant to create jobs designed for mismatched workers, including youth.

114 More precisely, given the presence of time dummies in the estimated equation, this variable controls for the improvement in educational attainment between new cohorts and previous ones.

determined in practice, depending at least partly on a number of factors which cannot be explored here for lack of data.

90. Regression results are presented in Table 2.3.[115] In order to minimise potential biases arising from omitted interactions between employment and schooling, the youth population is restricted to the 20-24 age group.[116] The baseline equation (Table 2.3, column 1) suggests that high unemployment benefits and high tax wedges reduce youth employment rates, with coefficients that are comparable in size to those found earlier for other population groups. At first glance, insofar as young workers' employment history is often too short to be entitled to (full) benefits, the sizeable and significant coefficient of the average benefit replacement rate may look rather surprising. However, it may reflect not only direct effects on youth employment *via* higher reservation wages and reduced job-search intensity, but also indirect effects *via* higher aggregate unemployment which in turn discourages youths from entering the labour market (see below). EPL is also negatively signed and significant, in line with previous OECD work (OECD, 2004). A decline in the OECD EPL indicator by two standard deviations is estimated to be associated on average with a 4 percentage point rise in youth employment. This finding lends support to the view that stringent EPL essentially undermines the job prospects of "marginal" groups in the labour market, e.g. those groups that enter the labour force lacking labour market credentials. While high corporatism is also found to be detrimental to youth employment, this result is driven by a single country (Australia) and cannot therefore be considered as robust. Finally, PMR and union density are both insignificant.

[Table 2.3. Employment rate equations: youth (20-24 age group), 1982-2003]

91. The estimates also suggest that large youth cohorts tend to have poorer job prospects, even though the effect is quantitatively small – a 1 percentage point increase in the share of youth population is associated with .2 percentage point decline in the youth employment rate – and not robust across all specifications in Table 2.3. Youth education is found to have a much larger impact, with one additional year of education reducing the youth employment rate by 3.3 percentage points. Finally, in line with the extensive literature on the youth labour market (see *e.g.* Clark and Summers, 1982; OECD, 1986, 1994b, 1996, 1998), youth employment is found to be highly sensitive to aggregate economic fluctuations, with the estimated coefficient of the output gap being twice as large as for prime-age males.

92. When estimated on a reduced sample limited to those countries where a statutory minimum wage exists,[117] the equation suggests that minimum wage hikes significantly increase youth employment rates (Table 2.3, column 2). This finding is at odds with past studies, which as already noted typically find either significantly negative or insignificant effects. Yet, the positive effect of minimum wages in column 2 does not hold in the cross-country dimension. This is apparent from the following econometric analysis (not reported in Table 2.3). In a first step, the specification in column 1 – *i.e.* excluding the minimum wage from the set of explanatory variables – is estimated over the sub-sample of countries where a minimum wage exists. In a second step, the country fixed effects for youth (α_i^Y) estimated in the first step are

115 As previously, each of the 20-24 age group employment rate equations has been estimated jointly with a prime-age male equation which, for convenience, is not reported here.

116 Estimating the baseline equation for the 15-24 instead of the 20-24 age group yields comparable results. There are two exceptions, however. The coefficient of EPL is no longer significant, and union density has a significant positive effect on youth employment. Also, when re-estimating the equation in Table 2.3, column 2, the size and significance of the minimum wage coefficient is unaffected.

117 Australia, Belgium, Canada, France, Japan, Netherlands, New Zealand, Portugal, Spain, United States. Ireland and the United Kingdom are excluded from the sample because minimum wages have been introduced only recently in these two countries. However, incorporating them would leave the results in Table 2.3 (qualitatively) unaffected.

regressed over the minimum wage variable. In the latter equation, the coefficient of the minimum wage becomes insignificant.

93. In order to investigate possible sources for such discrepancy, columns 3-6 replicate the specification used in the detailed study on the youth employment effects of minimum wages published by Neumark and Wascher (1999).[118,119] With the exception of the minimum wage, all policy and institutional variables as well as the output gap are dropped from the equation,[120] and replaced by the unemployment rate of the 25-54 age group. The latter variable is found to have a large and significant effect on youth employment in such specification (Table 2.3, column 3). When introduced into this equation, the minimum wage variable is again positive and significant (column 4), but it gets insignificant when country fixed effects are dropped and standard errors are adjusted for cluster level effects using the procedure suggested by Moulton (1986) (column 5).[121] If, as in Neumark and Wascher (1999), no such adjustment of the variance-covariance matrix of the estimators is made, standard errors are lower and the negative coefficient of the minimum wage variable now becomes significant (column 6). Overall, these results are fairly consistent with those in Neumark and Wascher (1999). Based on a panel of 17 OECD countries for the period 1975-2001, the authors find a significant negative impact of the minimum wage on youth employment rates when country fixed effects are omitted, but an insignificant effect when they are included. The most straightforward interpretation is that while significant negative effects may be found within a cross-country dimension, this conclusion does not hold in the time-series dimension. As a result, no strong policy conclusions can be drawn from this and the present analysis as regards the impact of minimum wages on youth employment.

2.5 Other issues

94. By focusing on overall employment rates, the analysis above does not make any distinction between public and private employment. Yet, public and private employment are likely to be driven by different factors. While the level of public employment primarily reflects (exogenous) government decisions, private employment is likely to depend both on the policies and institutions studied previously and the level of public employment, *via* general equilibrium effects. The latter mainly reflect the fact that public employment has to be financed by taxes, with possible negative effects on private employment. In this context, an important policy issue is whether increases in public employment bring about net employment gains, once negative general equilibrium effects are taken into account.

95. In order to explore this issue, the baseline employment rate equations estimated previously for prime-age males, prime-age females, older workers and youth are re-estimated with public employment rates as an additional explanatory variable (Table 2.4). Importantly, and despite some differences across

118 The only difference is that the estimates in columns 3-5 of Table 2.3 control for the youth employment effects of education, unlike the specification used in Neumark and Wascher (1999). However, sensitivity analysis (not reported here) shows that none of the results in columns 3-5 hinges on the presence of the education variable in the estimated equation.

119 While the specifications used do not differ, the estimation samples do. While Neumark and Wascher's analysis covers 16 countries over the period 1975-1997, the equations estimated in columns 4-6 cover a sample of 10 countries over the period 1982-2003.

120 Sensitivity analysis (not reported here) shows that including policies and institutions in the equation would affect neither the sign nor the significance of the minimum wage in columns 4-6 of Table 2.3.

121 Moulton (1986) suggests to adjust the standard errors of OLS estimates, which otherwise would be inconsistent insofar as the errors are correlated within groups –here, countries. A possible alternative to the Moulton procedure is a random-effects regression, which was also run (but not reported here) as a robustness check. The conclusion was similar, *i.e.* the minimum wage variable was found to be negatively signed and insignificant.

population groups, on aggregate increases (declines) in public employment rates are found to be fully offset by declines (increases) in private employment, without any significant positive effect on overall employment. Furthermore, the policy results obtained earlier (Tables 2.1, 2.2 and 2.3) appear to be robust, both in terms of magnitude and statistical significance, to the presence of public employment in the estimated equation.[122]

[Table 2.4. Employment rate equations: re-estimation of group-specific regressions with public employment as an additional explanatory variable]

96. In Section 1.3, evidence has been found that public expenditures on training lower aggregate unemployment, while no robust effect could be identified for other categories – and expenditures on ALMPs as a whole. These results are mirrored by those obtained by augmenting the group-specific SURE employment models with ALMP indicators.[123] Total expenditures per unemployed as a percentage of GDP per capita are never found to be significant. By contrast, training expenditures appear to significantly increase the employment rates of all groups, with the exception of women (Table 2.5). The insignificant coefficient found for prime-age females does not statistically differ from those obtained for other groups, however. Caution is thus needed in interpreting this result, insofar as it could simply reflect estimation inefficiency in small samples, measurement error and/or heterogeneity in training programmes.[124] In the case of prime-age men, the estimates suggest that increasing ALMP spending in training programmes per unemployed as a percentage of GDP per capita by 4 percentage points would raise employment by about 0.6 percentage points, an effect similar in size to that obtained earlier for aggregate unemployment (see Table 1.9, column 6).[125] Finally, ALMP measures for youth are found to exert a specific impact on youth employment rates, over and above that of training programmes. This finding contrasts with the insignificant effect of this policy variable on aggregate unemployment (see Section 1.3), and may essentially reflect substitution effects across groups.

[Table 2.5. Employment rate equations: re-estimation of group-specific regressions with expenditures on ALMPs as an explanatory variable]

2.6 Summing up policy influences on employment rates

97. Table 2.6 summarises the main policy results of the group-specific (baseline) employment rate regressions estimated above. An important conclusion that can be drawn from the Table is the significant negative impact of high unemployment benefits and high tax wedges on the employment rates of all groups, consistent with the results obtained in Section 1 as regards the determinants of aggregate unemployment.

122 It has been argued that agricultural employment should also receive specific treatment in such employment rate equations (*e.g.* OECD, 2002, and Nicoletti and Scarpetta, 2005). Additional empirical analysis (not reported here) shows that the policy conclusions drawn from Tables 2.1-2.3 are robust to the presence of the agricultural employment rate among the explanatory variables of the estimated equation.

123 Estimating a SURE model by a two-stage procedure would be questionable insofar as the correlation across errors is already taken into account in SURE estimation. For this reason, these models are estimated by three stage least squares.

124 Indeed, when estimated on the same sub-sample over which the SURE model for prime-age women can be estimated, the training coefficient for prime-age men becomes lower than in the full sample – although it is still significant, and is actually identical to that estimated for full-time female workers.

125 Overall, the comparison of these employment rate estimates with the IV unemployment regressions estimated in Chapter 1 suggests that the latter are not affected by the fact that trainees are usually recorded as inactive, with a spurious statistical effect on unemployment statistics.

98. Using population-weighted averages of group-specific coefficients, baseline estimates imply that a 10 percentage point reduction in the average gross replacement rate or in the tax wedge would increase the employment rate by 2.3 and 3.7 percentage points, respectively. By contrast, other general policies and institutions – including PMR and high corporatism which have been found earlier to be significant influences on aggregate unemployment – appear to have contrasted effects on the employment rates of various groups, with no clear aggregate impact.

[Table 2.6. Main estimated employment rate effects of policies and institutions: summary table]

99. Policies that affect only the employment rate of specific groups also presumably impact on the overall employment rate, even though general equilibrium analysis would ideally need to be undertaken in order to support this view.[126] In particular, lower implicit taxes on continued work, higher statutory retirement ages, higher tax incentives to part-time work – to which lower marginal tax rates on second earners can contribute – and family-friendly policies that increase the return to market work for mothers – such as a substitution of public childcare subsidies for child benefits – all contribute to increase the overall employment rate.

100. The estimated impact of lower implicit taxes on continued work and higher statutory retirement ages on overall employment is, however, moderate, reflecting both the small share of the 55-64 age group in the working-age population and the small magnitude of estimated elasticities compared with other existing studies.[127] A 10 percentage point cut in the implicit tax and a one-year increase in the standard retirement age are estimated to raise the employment rate of older workers by 0.2 and 0.1 percentage points, respectively. However, these effects grow by a factor of 1.5 if predicted population shares for the year 2025 (see *e.g.* Burniaux *et al.,* 2004) are used instead of actual population shares, and would be even greater if effects on the population aged 65 and over were taken into account.

126 For instance, any positive employment impact of a rise in public expenditures on childcare is likely to be offset (at least partly) by negative effects arising from the increase in taxes required to finance it.

127 In particular, microeconometric studies as well as simple cross-country analysis point to larger employment effects of implicit taxes on continued work. See Duval (2004) for a discussion.

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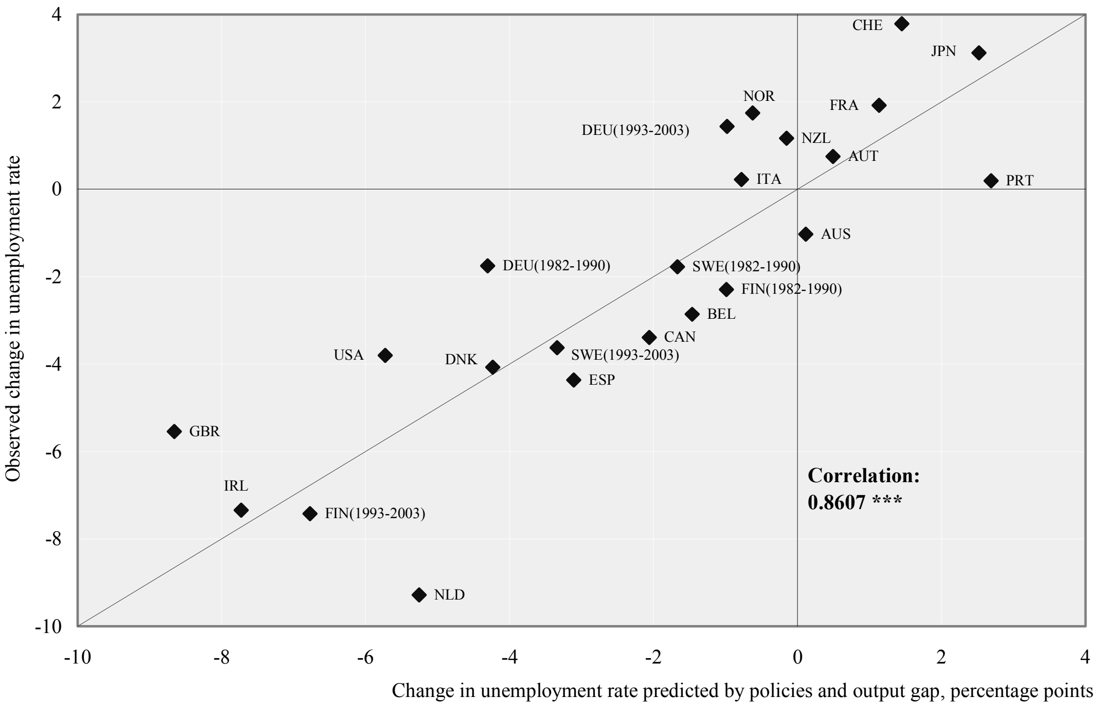
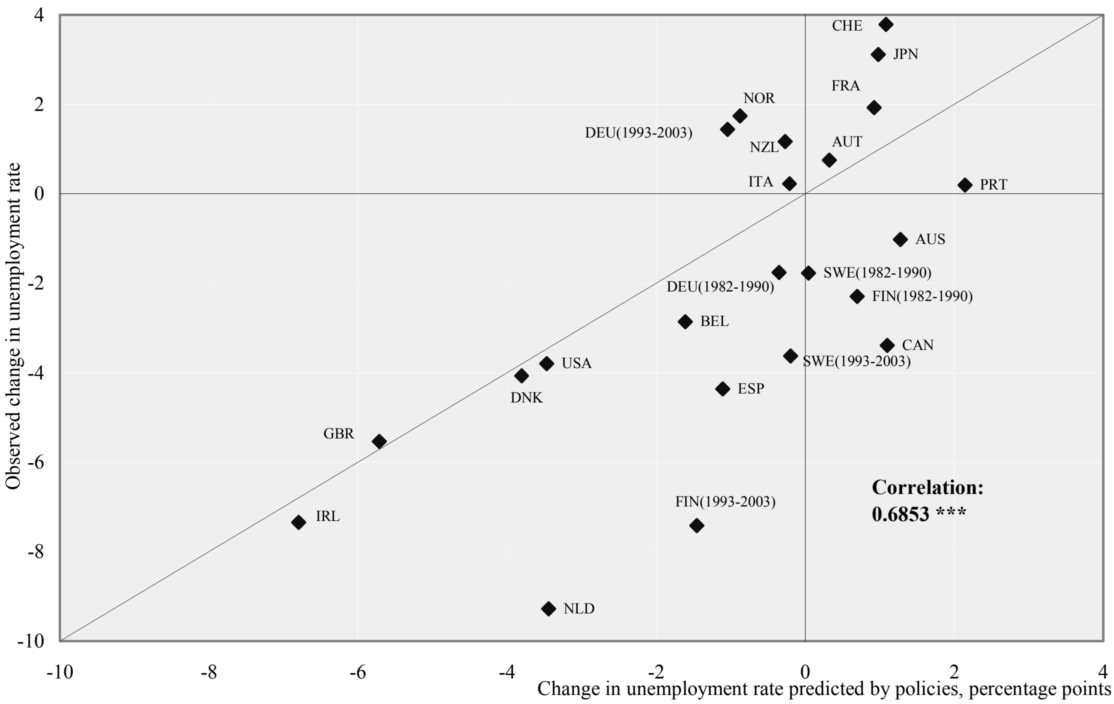
FIGURES AND TABLES

Figure 1.1. **The baseline unemployment rate equation: explaining past unemployment trends**

Panel A. Change in unemployment explained by policies and output gap, 1982-2003



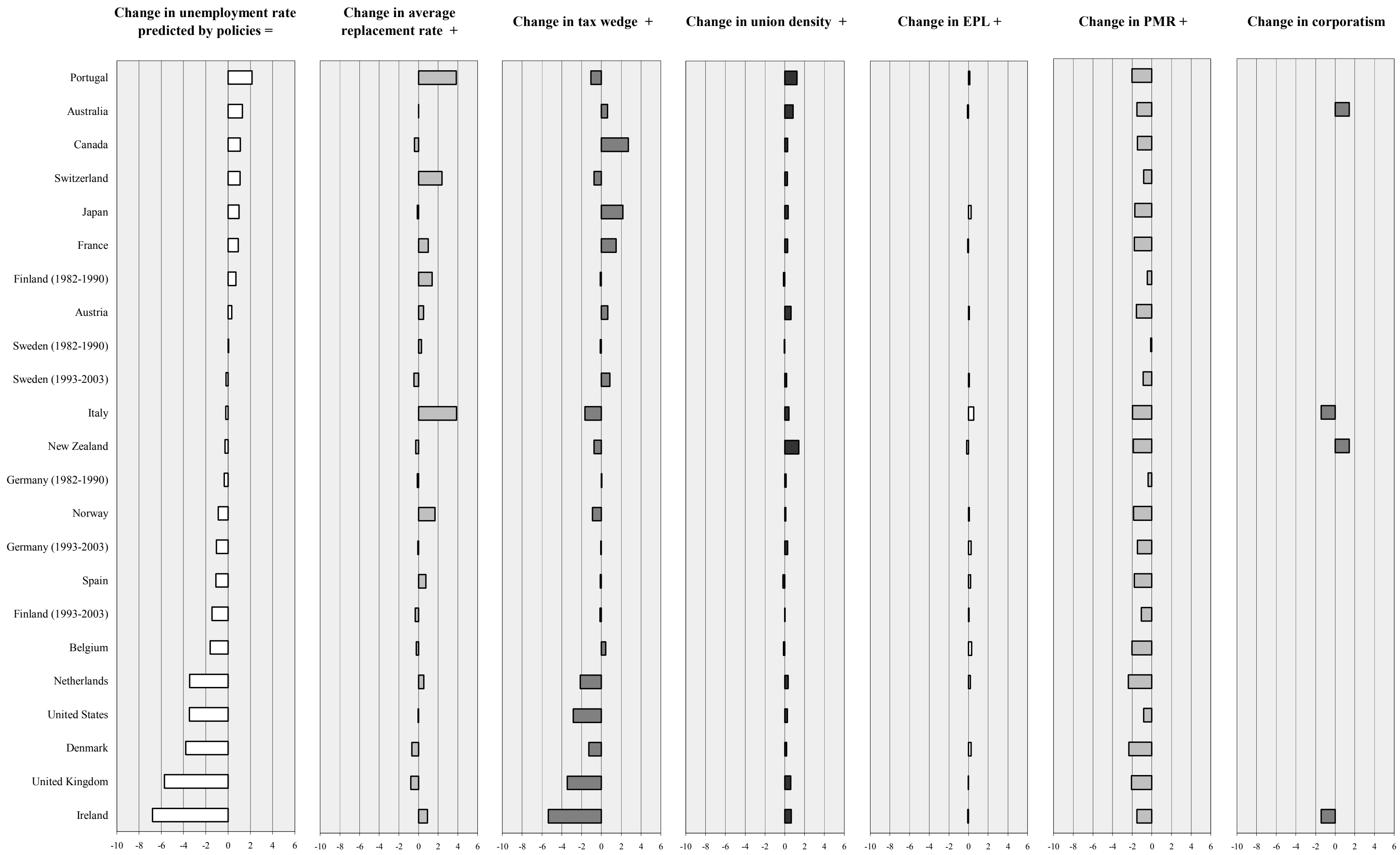
Panel B. Change in unemployment and policies, 1982-2003



Estimates on the basis of the unemployment rate equation in Table 1.2, column 1.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Figure 1.2. **Decomposing the impact of policies on the change in unemployment**

Unemployment effects of changes in policies over 1982-2003, on the baseline equation, in percentage points

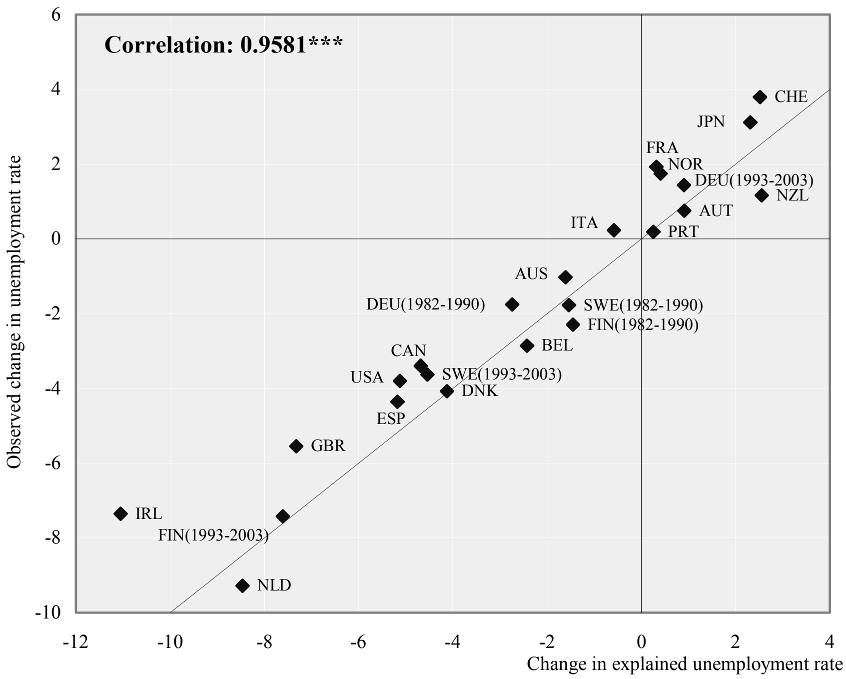


Estimates on the basis of the unemployment rate equation in Table 1.2, column 1.
Source : Authors' estimates on the basis of data sources described in Annex 2.

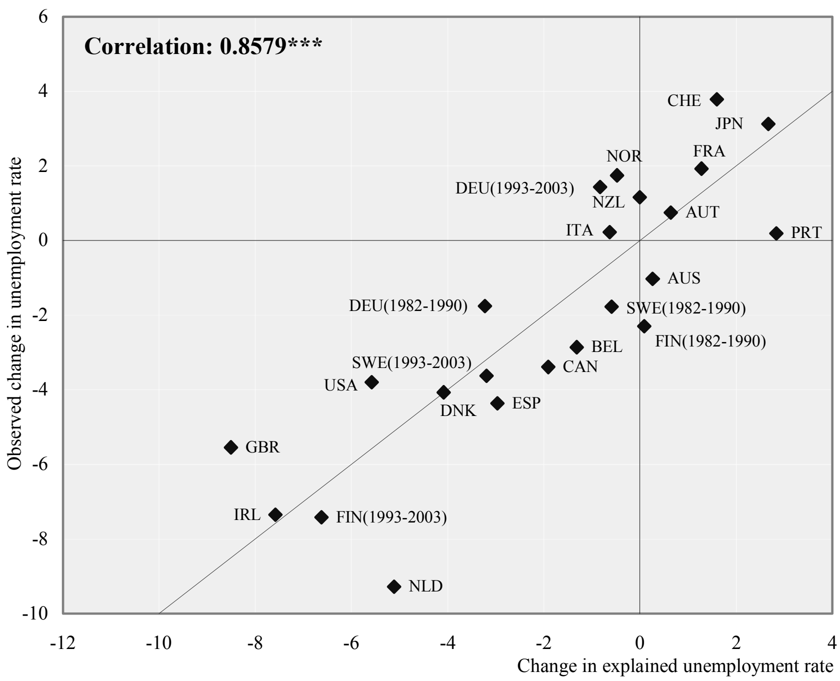
Figure 1.3. **Systemic interactions: how better do they explain past unemployment trends?**

Observed and explained unemployment rate in OECD countries over1982-2003, in percentage points

Panel A. **Non-linear model with systemic complementarities**



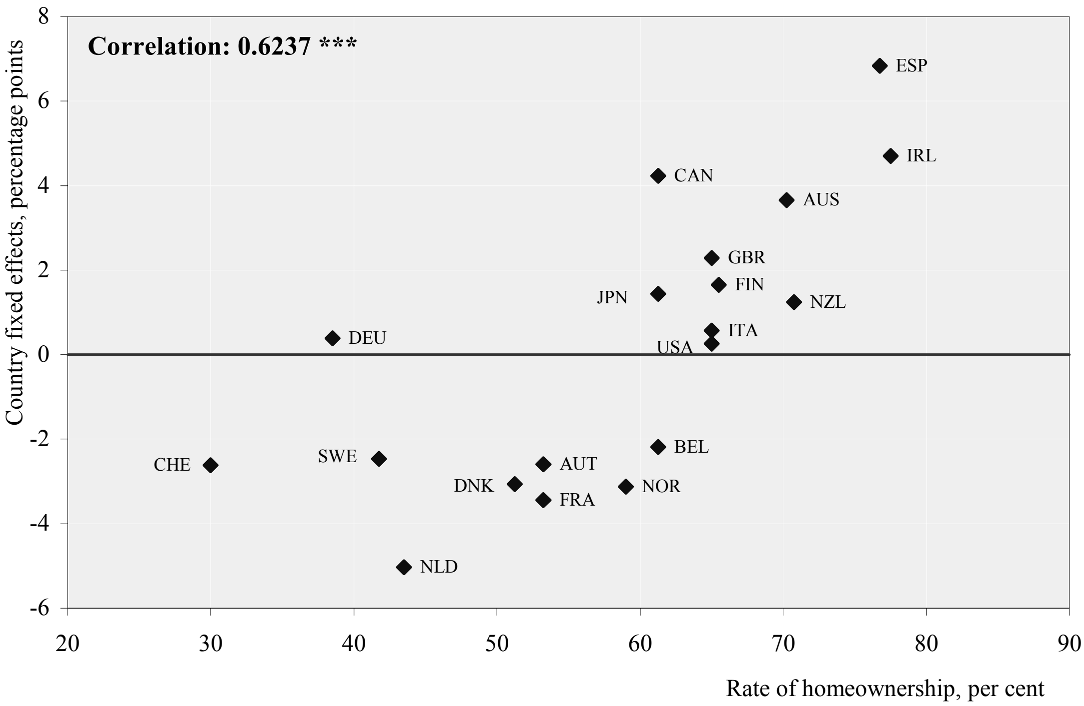
Panel B. **Baseline linear specification without interactions**



Estimates on the basis of the unemployment rate equations in Table 1.5, column 2 (Panel A), and Table 1.2, column 1 (Panel B).

Source : Authors' estimates on the basis of data sources described in Annex 2.

Figure 1.4. **Country fixed effects from the baseline unemployment regression and rates of home ownership**[1]

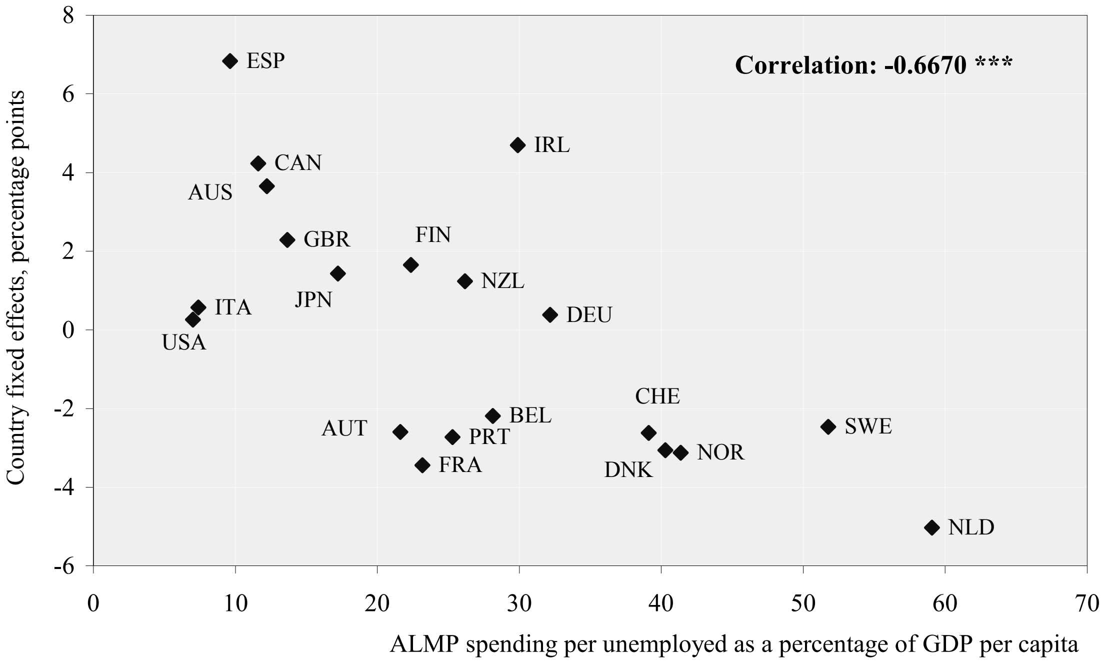


*** statistically significant at 1% level.

1. Country fixed effects are taken from the unemployment rate equation in Table 1.2, column 1. In the case of Finland, Germany and Sweden, they refer to the period 1993-2003 (as opposed to 1982-2003 for other countries). Rates of home ownership are averages over the period 1990-2000 (see Annex 2 for sources and methodological details).

Source : Authors' estimates on the basis of data sources described in Annex 2.

Figure 1.5. **Country fixed effects from the baseline unemployment regression and ALMP spending**[1]



*** statistically significant at 1% level.

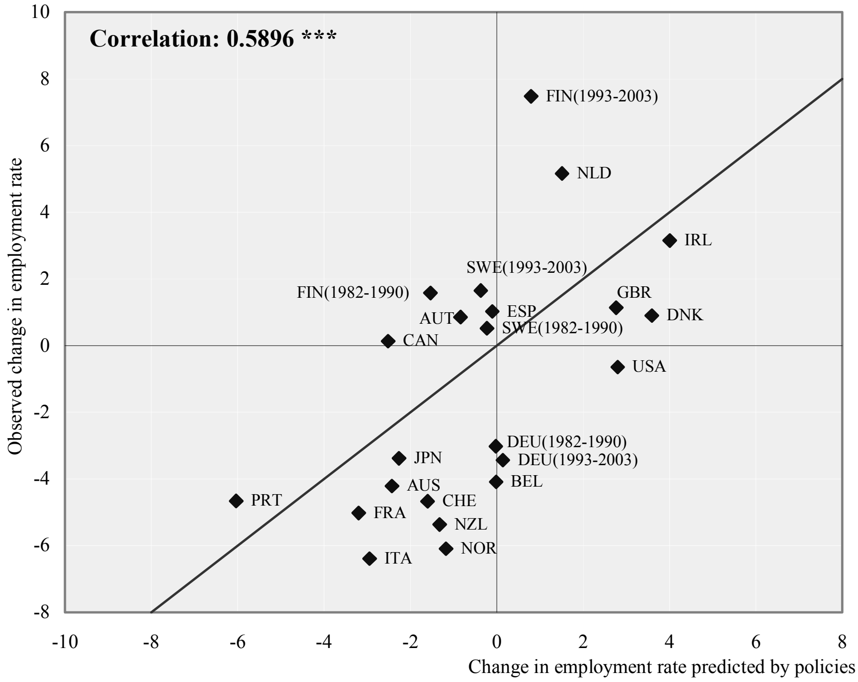
1. Country fixed effects are taken from the unemployment rate equation in table 1.2, column 1. In the case of Finland, Germany and Sweden, they refer to the period 1993-2003 (as opposed to 1982-2003 for other countries). ALMP spending is ALMP spending for unemployed as a percentage of GDP per capita (see Annex 2 for details). Country averages of ALMP spending are calculated over the period 1985-2001 or 1985-2002 for all sample countries with the exception of Denmark (1986-2002), Japan (1987-2001) and Portugal (1986-2000).

Source : Authors' estimates on the basis of data sources described in Annex 2.

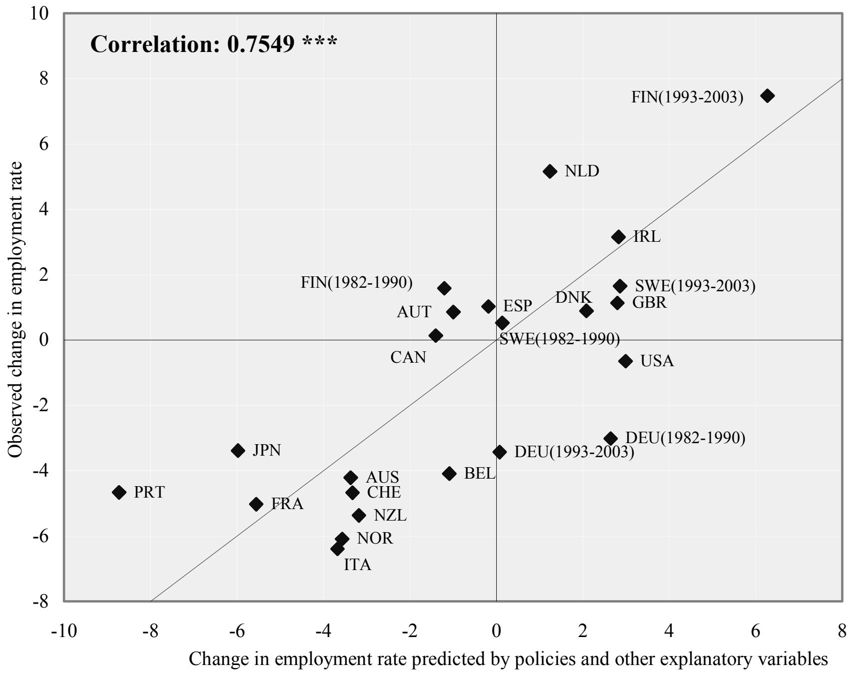
Figure 2.1. **The baseline equation for prime-age males: explaining past employment trends**

Percentage points

Panel A. **Change in employment rate of prime-age males explained by**



Panel B. **Change in employment rate of prime-age males explained by policies and other explanatory variables, 1982-2003**

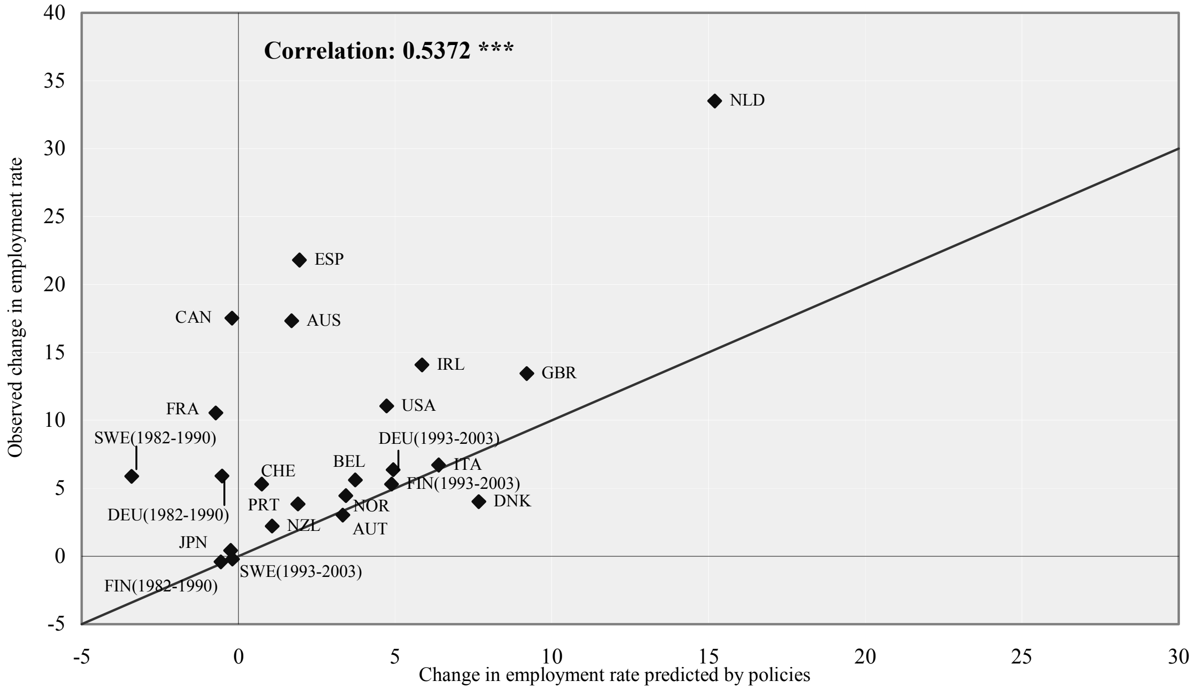


*** statistically significant at 1% level.

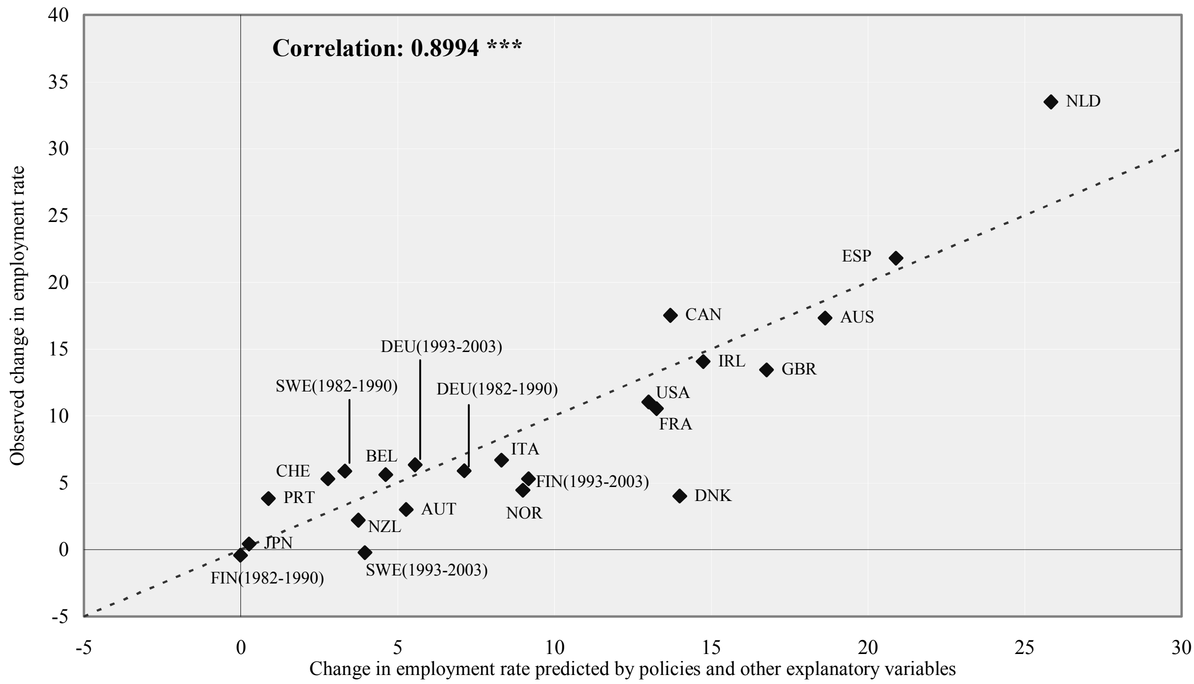
Source : Authors' estimates on the basis of data sources described in Annex 2.

Figure 2.2. **The baseline equation for prime-age females: explaining past employment trends**

Panel A. **Change in employment rate of prime-age females explained by policies, 1982-2003**



Panel B. **Change in employment rate of prime-age females explained by policies and other explanatory variables, 1982-2003**



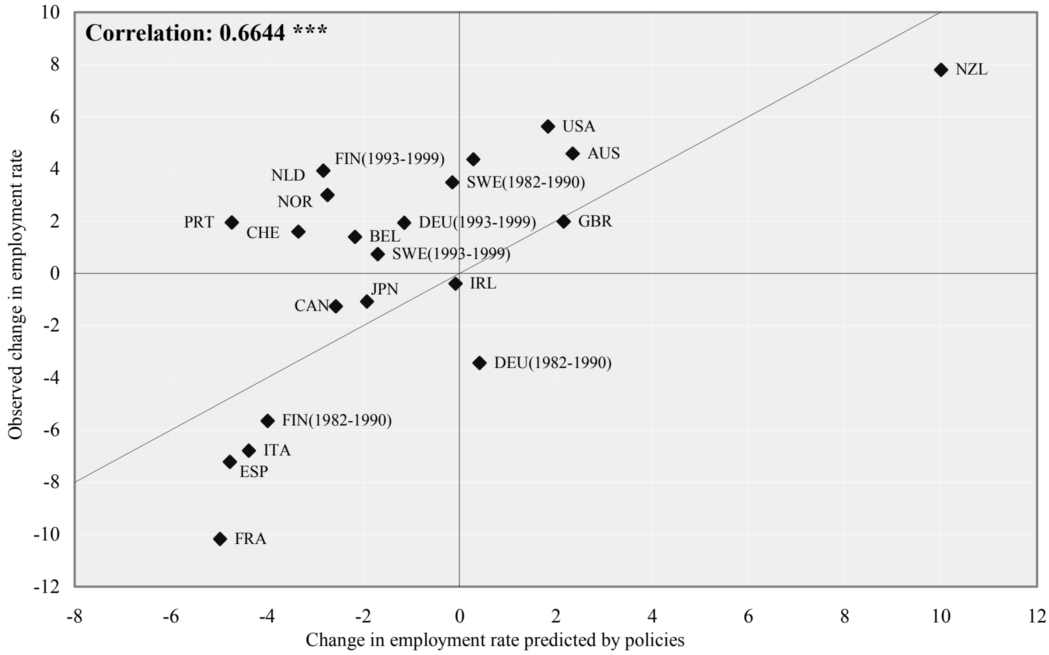
*** statistically significant at 1% level.

Source : Authors' estimates on the basis of data sources described in Annex 2.

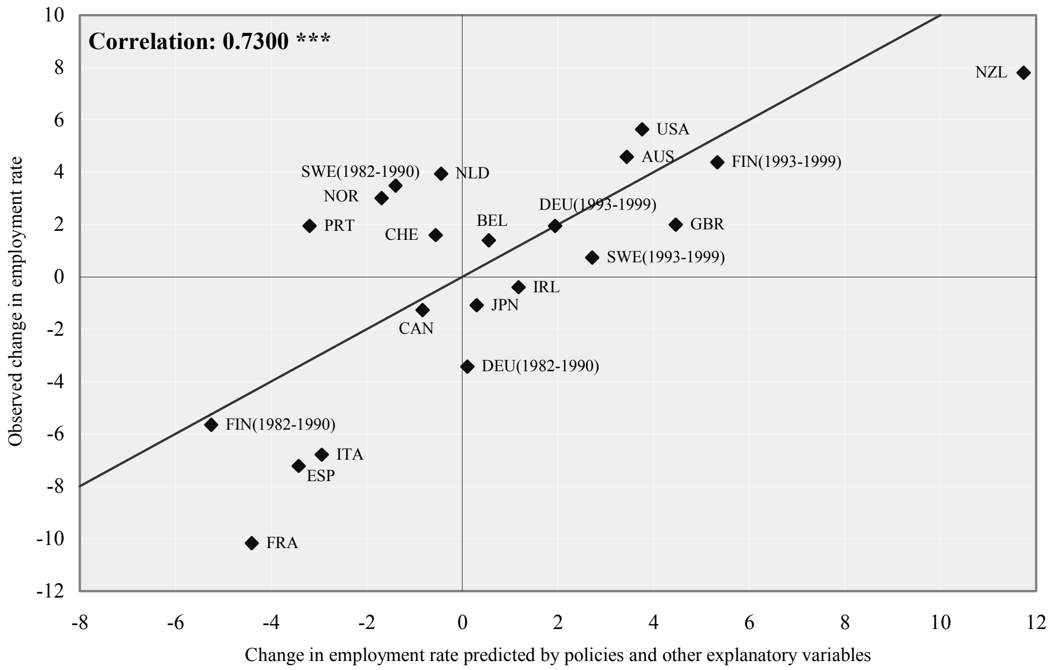
Figure 2.3. **The baseline equation for older workers: explaining past employment trends**

Percentage points

Panel A. **Change in employment rate of older worker explained by policies, 1982-1999**



Panel B. **Change in employment rate of older workers explained by policies and other explanatory variables, 1982-1999**



*** statistically significant at 1% level.

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.1. **Simple correlations between unemployment and selected institutions and policies, 1982-2003**

Panel A. **Observed values of variables**

	Average replacement rate	Tax wedge	EPL	Union density	PMR	ALMP
Unemployment rate	0.0869 *	0.3609 ***	0.1546 ***	-0.1097 **	0.2297 ***	-0.4055 ***

Panel B. **Variables purged from both country and time fixed effects**

	Average replacement rate	Tax wedge	EPL	Union density	PMR	ALMP
Unemployment rate	0.1181 **	0.4823 ***	-0.0177	0.0407	0.186 ***	-0.4631 ***

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

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.2. **Baseline unemployment rate equation, 1982-2003**

	1	2	3	4	5	6	7
	Excluding Germany, Finland and Sweden 1990-1991, common OG	= 1 with RR split into 2 components	= 1 with EPL split into 2 components	= 1 with tax wedge derived from National Accounts	= 4 with separate labour and consumption tax rates	= 1 with standard macroeconomic shocks	= 6 with labour demand shock
Average replacement rate (RR)	0.12 [6.28]***		0.12 [6.79]***	0.08 [4.22]***	0.09 [4.16]***	0.10 [4.14] ***	0.09 [3.35] ***
Tax wedge	0.28 [9.75]***	0.27 [10.96]***	0.27 [11.14]***	0.24 [4.49]***		0.24 [7.73] ***	0.22 [6.40] ***
Union density	-0.03 [1.57]	-0.03 [1.89]*	-0.03 [1.64]	-0.02 [0.56]	-0.01 [0.49]	0.04 [1.48]	0.06 [2.33] **
EPL	-0.31 [0.98]	-0.20 [0.55]		0.03 [0.08]	0.01 [0.02]	-0.61 [-1.52]	-0.51 [-1.22]
PMR	0.60 [2.98]***	0.67 [3.29]***	0.73 [3.52]***	0.50 [2.17]**	0.50 [2.17]**	0.54 [2.25] **	0.79 [3.28] ***
High corporatism	-1.42 [3.57]***	-1.09 [2.88]***	-1.39 [3.94]***	-2.06 [4.80]***	-2.09 [4.89]***	-1.42 [-2.90] **	-1.58 [-3.26] ***
Output gap	-0.48 [14.00]***	-0.48 [14.21]***	-0.47 [13.99]***	-0.54 [11.89]***	-0.54 [11.60]***		
RR 1st year		0.09 [7.37]***					
Benefit duration		2.64 [2.03]**					
(RR 1st)*(duration)		0.09 [2.69]***					
EPL regular			1.28 [2.49]**				
EPL temporary			-0.45 [2.16]**				
(EPL reg)*(EPL temp)			-0.28 [1.21]				
Labour tax rate					0.25 [4.82]***		
Consumption tax rate					0.21 [1.92]*		
Macroeconomic shocks:							
TFP shock						-12.81 [-3.34] ***	-8.87 [-2.33] **
Terms of trade shock						19.40 [6.45] ***	19.09 [6.09] ***
Interest rate shock						0.22 [2.72] ***	0.19 [2.44] **
Labour demand shock							11.79 [3.91] ***
Country dummies	yes	yes	yes	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes	yes	yes	yes
Observations	434	434	434	398	398	419	397
R-squared	0.98	0.92	0.92	0.98	0.98	0.98	0.98

OG = output gap. Absolute value of robust t statistics in brackets.
*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.3. **Simple interactions between institutions and bargaining regimes, 1982-2003**

	1	2	3	4	5
				=1	=2
	interaction between tax wedge and corporatism	interaction between EPL and corporatism	interaction between both tax wedge and EPL and corporatism	with intermediate corporatism broken down by country	with intermediate corporatism broken down by country
Direct effect of institutions:					
Average replacement rate	0.107 [5.59]***	0.116 [5.45]***	0.126 [5.90]***	0.116 [6.82]***	0.103 [4.65]***
Tax wedge	0.307 [10.83]***	0.273 [9.48]***	0.304 [10.82]***	0.277 [9.66]***	0.260 [8.97]***
Union density	-0.036 [1.57]	-0.035 [1.61]	-0.032 [1.42]	-0.035 [1.60]	-0.025 [1.09]
EPL	-0.538 [1.59]	-0.360 [1.11]	-0.402 [1.17]	-0.303 [0.93]	-0.814 [2.30]**
PMR	0.644 [3.20]***	0.704 [3.30]***	0.646 [3.17]***	0.617 [2.98]***	0.723 [3.44]***
High corporatism	-1.280 [3.03]***	-1.522 [3.70]***	-1.714 [4.12]***	-1.653 [3.38]***	-1.506 [3.62]***
Interactions between institutions:					
Tax wedge * high corporatism	0.061 [2.00]**		0.132 [2.98]***	0.086 [2.85]***	
Tax wedge * intermediate corporatism	0.278 [2.61]***		0.287 [2.60]***		
EPL * high corporatism		0.030 [0.09]	-1.092 [2.13]**		0.199 [0.59]
EPL * intermediate corporatism		1.698 [1.70]*	0.284 [0.26]		
Tax wedge Sweden				0.194 [2.03]**	
Tax wedge Spain				1.919 [4.21]***	
Tax wedge France				0.671 [4.44]***	
Tax wedge Portugal				-0.321 [2.62]***	
EPL Sweden					0.126 [0.07]
EPL Spain					2.597 [1.60]
EPL France					7.076 [3.54]***
EPL Portugal					-1.205 [0.75]
Output gap, Time and country dummies	yes	yes	yes	yes	yes
Observations	434	434	434	434	434
R-squared	0.98	0.98	0.98	0.98	0.98
Hausman heterogeneity test (P-value)	0.001	0.047	0.001	0.330	0.075

Absolute value of robust t-statistics in brackets.

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

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.4. **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 collective bargaining coverag	-0.093 ***	. .	*0.2*	-0.605
Average replacement rate * High corporatism	-0.009	0.042	*32.7*	-0.042
Average replacement rate * Low corporatism	0.039	. .	*5.2*	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 collective bargaining coverage	0.234 ***	-0.093	*58.6*	-0.236 *
Tax wedge * High corporatism	0.050 *	0.037	*30.0*	-0.335 ***
Tax wedge * Low corporatism	-0.072 **	-0.042	*23.0*	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
Union density * low corporatism	-0.025	-0.215 ***	*56.7*	-0.115
EPL * PMR	-0.111	-1.076 **	*17.3*	-0.272
EPL * High collective bargaining coverage	1.142	-0.211	*502.5*	4.632
EPL * High corporatism	-0.150	. .	*9.6*	-1.365
EPL * Low corporatism	-0.176	. .	*5.5*	2.401 **
PMR * High collective bargaining coverage	0.168	0.212	*43.4*	-0.623
PMR * High corporatism	-0.410 **	. .	*3.2*	0.301
PMR * Low corporatism	0.143	. .	*0.0*	-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).

Estimates of interactions among dichotomic variables as well as between union density and high collective bargaining coverage are not shown.

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 the fixed effects and estimated by OLS.

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

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.5. **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.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.6 **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 1.5 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.

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.7. **Unemployment effects of statutory minimum wage, 1982-2003**

	1	2	3	4
		=1	=1	=3
	baseline equation augmented with minimum wage	without output gap	with tax wedge interacted with minimum wage	with instrumented interaction[2]
Direct effect of policies/institutions:				
Average replacement rate	0.095	0.101	0.091	0.090
	[2.52]**	[2.82]***	[2.43]**	[2.39]**
Tax wedge	0.236	0.250	0.237	0.237
	[5.66]***	[4.40]***	[6.63]***	[6.62]***
Union density	-0.047	0.011	-0.048	-0.048
	[1.65]	[0.37]	[1.66]*	[1.65]*
EPL	0.013	-0.892	-0.566	-0.746
	[0.03]	[1.56]	[1.14]	[1.32]
PMR	0.847	1.158	0.562	0.473
	[3.13]***	[3.40]***	[2.16]**	[1.52]
High corporatism	-1.764	-2.826	-1.645	-1.608
	[2.66]***	[3.32]***	[2.49]**	[2.40]**
Minimum wage[1]	-0.048	0.113	0.029	0.052
	[1.30]	[2.31]**	[0.72]	[0.94]
Interactions between policies/institutions:				
Tax wedge * Minimum wage			0.011	0.015
			[4.12]***	[2.35]**
F test on instrument[3]				23.16
Country dummies	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Output gap	yes	no	yes	yes
Observations	217	217	217	217
R-squared	0.98	0.98	0.98	0.92

Absolute value of robust t-statistics in brackets.

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

1. The minimum wage is measured as the ratio of the legal minimum wage to the median wage (see Annex 2 for details).

2. The equation is estimated by two-stage least squares where the interaction between the tax wedge and the minimum wage is instrumented with the product of the deviations of the tax wedge and the minimum wage from their respective country-specific means.

3. F test statistic on the significance of the instrument in the first-stage regression.

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.8. **Active Labour Market Policies, 1985-2002**

	1	2	3	4
	baseline equation + ALMP spending per unemployed as percentage of GDP per capita (ALMPU)	= 1 with instrumented ALMPU[1]	baseline equation + interaction between replacement rate and country average of ALMPU[2]	= 3 with instrumented interaction between replacement rate and ALMPU[3]
Direct effect of policies/institutions:				
Average replacement rate	0.129	0.110	0.107	0.084
	[7.00]***	[5.27]***	[5.59]***	[3.12]***
Tax wedge	0.233	0.157	0.272	0.148
	[5.35]***	[4.07]***	[9.83]***	[3.62]***
Union density	-0.029	-0.012	-0.025	0.017
	[1.31]	[0.40]	[1.15]	[0.49]
EPL	-0.177	0.011	-0.117	-0.064
	[0.50]	[0.03]	[0.36]	[0.16]
PMR	0.357	0.681	0.643	0.431
	[1.50]	[2.25]**	[3.16]***	[1.29]
High corporatism	-1.917	-2.243	-1.698	-2.680
	[5.26]***	[5.15]***	[3.94]***	[5.31]***
ALMPU	-0.028	-0.015		-0.003
	[4.23]***	[1.39]		[0.27]
Interactions between policies/institutions:				
ALMPU*Average replacement Rate			-0.002	-0.002
			[2.42]**	[1.90]*
F test on instruments[4]		15.42		23.37
Country dummies	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Output gap	yes	yes	yes	yes
Observations	332	272	434	272
R-squared	0.99	0.94	0.98	0.94

Absolute value of t statistics in brackets.

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

1. 2SLS; ALMPU is instrumented with the lagged first difference of the residual of a regression of ALMPU on up to three lags of the output gap.

2. The baseline equation is augmented with the interaction of the average replacement rate with the country-specific average of ALMPU, both expressed as deviations from their sample averages.

3. 2SLS; ALMPU is instrumented with the lagged first difference of the residual of a regression of ALMPU on up to three lags of the output gap; the interaction Replacement rate*ALMPU is instrumented with the product of the deviation of the replacement rate from its country-specific means and the lagged first difference of the residual of a regression of ALMPU on up to three lags of the output gap.

4. F test statistic on the significance of the instruments in the first-stage regression.

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.9. **Categories of active labour market policies, 1985-2002**

	1	2	3	4	6	5	7
	baseline equation + ALMP spending per unemployed by category as percentage of GDP per capita	= 1 with instrumented ALMP variables[1]	baseline equation + ALMP spending by category as percentage of GDP[2]	baseline equation + training spending per unemployed as percentage of GDP per capita	= 4 with instrumented training[1]	baseline equation + training spending as percentage of GDP[2]	= 4 estimated by System GMMs[3]
Direct effect of ALMP categories:							
Training	-0.078	-0.189	-0.058	-0.086	-0.158	-0.053	-0.150
	[4.92]***	[3.36]***	[2.17]**	[5.58]***	[4.32]***	[1.70]*	[1.94]*
PES	-0.149	-0.044	0.184				
	[3.17]***	[0.48]	[2.63]***				
Youth measures	0.056	0.095	0.287				
	[1.44]	[0.83]	[5.61]***				
Subsidised employment	-0.025	0.023	-0.034				
	[1.31]	[0.28]	[1.40]				
Measures for disabled	0.054	0.064	0.200				
	[3.16]***	[1.43]	[4.98]***				
F test on instrument[4]		10.32			22.19		
Hansen test (P-value)							1.00
Arellano-Bond AR1 test							-3.27***
Arellano-Bond AR2 test							-1.78*
Baseline controls[5]	yes	yes	yes	yes	yes	yes	yes
Country dummies	yes	yes	yes	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes	yes	yes	yes
Output gap	yes	yes	yes	yes	yes	yes	yes
Observations	324	266	324	324	266	324	298
R-squared	0.99	0.94	0.99	0.99	0.94	0.99	

Absolute value of t statistics in brackets.

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

1. 2SLS estimates. ALMP variables are instrumented with the lagged first difference of the residual of their regression on up to three lags of the output gap.
2. To facilitate the comparison with other columns, ALMP spending / GDP is devided by the sample average of the unemployment / population ratio.
3. One-step GMM-SYS robust estimates. The error term is modeled as an ARMA process with an AR(1) component. Training and output gap are treated as endogenous variables. The common factor restriction is not imposed. Only long-run effects are presented. Training and output gap dated t-3, t-4 and t-5 are used as instruments in the difference equation. First differences of training and output gap dated t-2, t-3 are used as instruments in the level equation. The Hansen-Sargan statistic provides a test of overidentifying restrictions. The model is rejected if the statistic is significant. Arellano-Bond statistics test the autocorrelation of the first difference of the residuals at order 1 and 2 and are normally distributed under the null. The model is rejected if evidence of autocorrelation is found at order 2.
4. F test statistic on the significance of the instruments in the first-stage regression.
5. Baseline controls are: average benefit replacement rate, tax wedge, union density, EPL, PMR, and a dummy for high corporatism.

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.10. **Interactions between institutions and shocks: final models with unobserved shocks**

	1	2	3	4
	Final model selected 1970-2003	= 1 with control for direct effect of institutions 1975-2003	= 1 with ALMPs and control for direct effect of institutions, 1975-2003	= 1 with home ownership and control for direct effect of institutions, 1975-2003
Direct effect of institutions:				
Average replacement rate		0.06	0.07	0.07
		[3.73]***	[4.41]***	[4.51]***
Tax wedge		0.18	0.17	0.19
		[5.08]***	[4.85]***	[5.14]***
Union density		-0.03	-0.01	-0.01
		[1.38]	[0.65]	[0.65]
PMR		0.34	0.41	0.15
		[1.73]*	[2.15]**	[0.77]
High corporatism		-1.38	-1.75	-1.79
		[4.40]***	[5.37]***	[5.26]***
Interactions shocks/institutions:				
Average replacement rate	0.04	0.04	0.05	0.04
	[9.50]***	[6.21]***	[6.64]***	[6.44]***
High corporatism	-0.85	-0.95	-0.89	-0.65
	[8.65]***	[5.43]***	[5.16]***	[3.88]***
ALMPs			-0.01	
			[3.58]***	
Home ownership				0.03
				[4.92]***
Country dummies	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes
Observations	669	516	516	468
R-squared	0.80	0.87	0.87	0.88

Non-linear least squares. Absolute value of t statistics in brackets.
*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.11. **Interactions between shocks and institutions: models with observed shocks**

	1	2	3
	Final model selected with interactions 1975-2003	= 1 with control for direct effect of institutions 1975-2003	= 2 with labour demand shock, 1975-2003
Direct effect of institutions:			
Average replacement rate		0.06 [3.54]***	0.04 [2.46]**
Tax wedge		0.21 [6.08]***	0.22 [6.30]***
Union density		-0.01 [0.37]	-0.02 [0.88]
PMR		0.29 [1.96]*	0.33 [2.30]**
High corporatism		-0.96 [2.97]***	-1.02 [3.35]***
Interactions shocks/institutions:			
Average replacement rate	0.05 [5.90]***	0.04 [4.65]***	0.06 [6.44]***
High corporatism	-0.89 [4.83]***	-0.80 [3.91]***	-1.30 [5.41]***
Direct effect of shocks:			
TFP shock	-10.81 [3.35]***	-12.65 [3.99]***	-5.92 [2.18]**
Terms of trade shock	4.95 [3.94]***	5.97 [3.26]***	7.09 [4.46]***
Interest rate shock	0.42 [15.36]***	0.34 [11.87]***	0.29 [10.77]***
Labour demand shock			11.45 [4.36]***
Country dummies	yes	yes	yes
Time dummies	yes	yes	yes
Observations	548	505	487
R-squared	0.79	0.86	0.88

Non-linear least squares. Absolute value of t statistics in brackets.
*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.12. **Disentangling persistence from amplification effects: unobserved shocks, 1970-2003**

	1	2	3	4	5	6	7	8
	Final model selected in the static regressions	= 1 with significant terms only	= 2 with tax wedge	= 2 with union coverage	= 2 with EPL	= 2 with PMR	= 2 with ALMPs	= 2 with home ownership
Persistence coefficient: $1-\phi$	0.911 [4.89]***	0.915 [4.98]***	0.914 [5.02]***	0.920 [4.60]***	0.906 [5.39]***	0.905 [5.69]***	0.903 [5.37]***	0.889 [5.33]***
Effect of institutions on persistence of shocks: γ_j								
Average replacement rate	-0.012 [0.79]							
Tax wedge			0.009 [0.52]					
Collective bargaining coverage				0.018 [1.71]*				
EPL					0.190 [1.90]*			
PMR						0.697 [4.55]***		
High corporatism	-0.354 [1.17]							
ALMPs							-0.015 [2.21]**	
Home ownership								0.020 [2.61]***
Effect of institutions on amplification of shocks: γ_k								
Average replacement rate	0.035 [5.59]***	0.034 [5.45]***	0.039 [5.61]***	0.031 [4.48]***	0.035 [5.63]***	0.032 [5.50]***	0.036 [5.78]***	0.041 [5.64]***
Tax wedge			-0.015 [1.93]*					
Collective bargaining coverage				0.003 [0.90]				
EPL					-0.137 [2.46]**			
PMR						-0.057 [0.78]		
High corporatism	-0.729 [4.66]***	-0.725 [4.65]***	-0.768 [4.82]***	-0.778 [5.06]***	-0.717 [4.66]***	-0.519 [3.57]***	-0.688 [4.37]***	-0.624 [3.49]***
ALMPs							-0.003 [1.19]	
Home ownership								0.024 [3.61]***
Country dummies	yes	yes	yes	yes	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes	yes	yes	yes	yes
Observations	646	646	646	646	646	646	646	586
R-squared	0.42	0.41	0.42	0.42	0.42	0.45	0.42	0.44

Non-linear least squares. Absolute value of t statistics in brackets.

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

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 1.13. **Disentangling persistence from amplification effects: observed shocks, 1970-2003**

	1	2	3	4	5	6	7	8
	Final model selected in the static regressions	= 1 with significant terms only	= 2 with tax wedge	= 2 with union coverage	= 2 with EPL3	= 2 with PMR	= 2 with ALMPs	= 2 with home ownership
Persistence coefficient: $1-\phi$	0.901 [5.88]***	0.901 [6.76]***	0.895 [6.80]***	0.901 [6.72]***	0.888 [7.24]***	0.889 [7.56]***	0.893 [7.18]***	0.892 [6.42]***
Effect of institutions on persistence of shocks: γ_j								
Average replacement rate	-0.015 [1.00]							
Tax wedge			0.013 [0.85]					
Collective bargaining coverage				0.005 [0.58]				
EPL					0.168 [1.87]*			
PMR						0.248 [3.65]***		
High corporatism	-0.479 [1.74]*							
ALMPs							-0.014 [2.15]**	
Home ownership								0.010 [1.16]
Effect of institutions on amplification of shocks: γ_k								
Average replacement rate	0.020 [3.14]***	0.019 [2.96]***	0.028 [3.82]***	0.021 [2.85]***	0.021 [3.31]***	0.015 [2.20]**	0.021 [3.42]***	0.026 [3.27]***
Tax wedge			-0.026 [2.80]***					
Collective bargaining coverage				-0.002 [0.64]				
EPL					-0.218 [3.78]***			
PMR						-0.191 [3.22]***		
High corporatism	-0.592 [3.44]***	-0.596 [3.41]***	-0.703 [3.91]***	-0.607 [3.38]***	-0.660 [3.86]***	-0.531 [2.95]***	-0.525 [3.07]***	-0.658 [3.21]***
ALMPs							-0.005 [2.29]**	
Home ownership								0.016 [2.28]**
Country dummies	yes	yes	yes	yes	yes	yes	yes	yes
Time dummies	yes	yes	yes	yes	yes	yes	yes	yes
Observations	604	604	604	604	604	604	604	548
R-squared	0.38	0.37	0.38	0.37	0.39	0.39	0.38	0.38

Non-linear least squares. Absolute value of t statistics in brackets.
*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 2.1. **Employment rate equations: prime-age males and prime-age females, 1982-2003**

	1	2			3		4		5	
	Prime-age males	Prime-age females								
		Baseline equation			= 2 with different marginal tax rates on 2nd earner		= 2 with non-linear impact of leave weeks		= 2 with public expenditures on childcare, IV method[2]	
		full-time	part-time	aggregate[1]	full-time	part-time	full-time	part-time	full-time	part-time
General policies and institutions:										
Average replacement rate	-0.17	-0.14	-0.17	-0.32	-0.17	0.0003	-0.11	-0.21	0.00	-0.27
	[7.42]***	[3.71]***	[3.00]***	***	[5.43]***	[0.01]	[2.84]***	[3.52]***	[0.01]	[3.78]***
Tax wedge	-0.30	-0.12	-0.38	-0.50	-0.20	-0.22	-0.09	-0.42	-0.05	-0.14
	[8.34]***	[2.34]**	[4.45]***	***	[3.60]***	[2.14]**	[1.70]*	[4.77]***	[0.63]	[1.09]
Union density	0.06	0.16	-0.21	-0.05	0.12	-0.17	0.17	-0.22	-0.18	-0.01
	[2.30]**	[3.47]***	[3.00]***		[2.70]***	[2.16]**	[3.86]***	[3.14]***	[1.54]	[0.03]
EPL	-0.23	-1.54	0.99	-0.55	-1.52	1.72	-1.20	0.67	0.26	-0.59
	[0.66]	[3.06]***	[1.32]		[2.97]***	[1.93]*	[2.50]**	[0.88]	[0.56]	[0.85]
PMR	-0.12	-0.75	-0.86	-1.60	-0.27	-1.95	-0.81	-0.80	0.18	-0.41
	[0.47]	[2.67]***	[1.99]**	***	[0.94]	[3.93]***	[3.02]***	[1.87]*	[0.55]	[0.79]
High corporatism	0.48	-1.63	0.57	-1.06	-1.70	2.06	-0.29	-0.72	0.18	0.01
	[1.14]	[2.06]**	[0.47]		[2.54]**	[1.71]*	[0.35]	[0.55]	[0.33]	[0.01]
Group-specific policies and institutions:										
Tax incentives to part-time		-0.58	1.35	0.76			-0.57	1.34	-0.21	0.27
		[8.91]***	[11.34]***	***			[9.09]***	[11.38]***	[2.60]***	[1.95]*
Relative marginal tax rate on 2nd earner		0.38	-1.23	-0.85	-4.32	3.82	0.45	-1.30	2.36	-0.83
(transition 100/0 - 100/66)		[0.55]	[0.98]		[2.82]***	[1.27]	[0.67]	[1.03]	[1.81]*	[0.37]
Relative marginal tax rate on 2nd earner					4.10	-4.72				
(transition 100/0 - 100/33)					[3.08]***	[1.81]*				
Family cash benefits		0.06	-0.30	-0.24	0.03	-0.26	0.04	-0.28	-0.34	-0.35
		[1.47]	[3.83]***	***	[0.56]	[2.60]***	[0.81]	[3.49]***	[3.27]***	[1.93]*
Leave weeks		0.02	-0.04	-0.02	0.02	-0.05	0.06	-0.08	-0.02	0.01
		[2.60]***	[2.83]***	*	[2.74]***	[2.99]***	[5.22]***	[3.55]***	[1.35]	[0.30]
Leave weeks squared							-0.0003	0.0003		
							[4.61]***	[2.39]**		
Public childcare spending									0.002	-0.002
									[2.32]**	[1.52]
Control variables:										
Female education		2.90	-0.63	2.27	3.38	-2.24	3.10	-0.82	2.64	-0.44
		[4.72]***	[0.57]	**	[5.06]***	[1.71]*	[5.21]***	[0.74]	[3.57]***	[0.34]
Output gap	0.49	0.17	0.10	0.26	0.17	0.12	0.15	0.12	0.19	-0.17
	[11.60]***	[3.17]***	[1.25]	***	[3.15]***	[1.25]	[2.95]***	[1.48]	[3.12]***	[1.80]*
Other controls[3]	no	yes	yes		yes	yes	yes	yes	yes	yes
Country dummies	yes	yes	yes		yes	yes	yes	yes	yes	yes
Time dummies	yes	yes	yes		yes	yes	yes	yes	yes	yes
Observations	404	277	277		284	284	277	277	120	120
R-squared	0.99	0.99	0.96		0.99	0.94	0.99	0.96	0.99	0.99

Seemingly Unrelated Regression Estimators (SURE). Absolute value of t statistics in brackets. Equations for prime-age men for models corresponding to Columns 2 to 5 are not reported.

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

1: The impact of each explanatory variable on the aggregate prime-age female employment rate is derived as the sum of its effects on full-time and part-time employment rates.
Its degree of significance is provided by a Fisher test of the null assumption that the sum of the coefficients on full-time and part-time employment rates is zero.

2: In the Instrumental variables (IV) approach of column 5, public childcare spending is instrumented by its lagged change.

3: "Other controls" include the proportion of married women, the number of children per woman as well as the interaction between the latter two variables (see Annex 2 for details on data sources and methodology).

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 2.2. **Employment rate equations: older workers (55-64 age group), 1982-1999**

	1	2	3
	Baseline equation, total population aged 55-64	Baseline equation, male population aged 55-64	=2 with interaction between retirement incentives and EPL
General policies and institutions:			
Average replacement rate	-0.19	-0.23	-0.25
	[7.12]***	[7.04]***	[7.61]***
Tax wedge	-0.31	-0.33	-0.33
	[6.74]***	[5.91]***	[6.00]***
Union density	-0.13	-0.13	-0.11
	[5.34]***	[4.40]***	[3.87]***
EPL	1.59	1.90	1.44
	[2.62]***	[2.57]**	[1.97]**
PMR	0.56	0.75	0.74
	[1.74]*	[1.93]*	[1.94]*
High corporatism	-1.35	-1.14	-0.76
	[3.09]***	[2.15]**	[1.44]
Group-specific policies and institutions:			
Implicit tax on continued work	-0.10	-0.15	-0.14
	[2.82]***	[3.37]***	[3.38]***
Standard age of eligibility to pension benefits	0.57	0.65	1.01
	[2.28]**	[2.17]**	[3.33]***
Interactions between implicit tax rates and other policies and institutions:			
Implicit tax * EPL			0.07
			[4.05]***
Control variables:			
Output gap	0.20	0.34	0.33
	[4.39]***	[6.12]***	[6.03]***
Country dummies	yes	yes	yes
Time dummies	yes	yes	yes
Observations	279	279	279
R-squared	0.99	0.98	0.98

Seemingly Unrelated Regression Estimators (SURE). Absolute value of t statistics in brackets. Equations for prime-age men are not reported.
*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 2.3. **Employment rate equations: youth (20-24 age group), 1982-2003**

	1	2	3	4	5	6
	Baseline equation	= 1 with minimum wage	= 1 with aggregate unemployment rate instead of policies/institutions	= 3 with minimum wage	= 4 without country fixed effects	= 5 without correction for "cluster" effects
General policies and institutions:						
Average replacement rate	-0.24 [5.61]***	-0.29 [4.40]***				
Tax wedge	-0.34 [5.86]***	-0.67 [7.55]***				
Union density	0.06 [1.39]	0.04 [0.67]				
EPL	-2.35 [2.97]***	-5.44 [5.49]***				
PMR	0.51 [1.04]	-0.93 [1.55]				
High corporatism	-1.66 [2.13]**	0.93 [0.69]				
Group-specific policies and institutions:						
Minimum wage		0.67 [7.50]***		0.13 [1.82]*	-0.18 [0.73]	-0.18 [3.14]***
Control variables:						
Youth education	-3.18 [3.77]***	-0.65 [0.47]	-5.03 [5.67]***	-5.30 [3.62]***	1.51 [0.20]	1.51 [0.95]
Youth cohort size	-0.22 [3.44]***	-0.08 [1.01]	0.13 [1.91]*	0.16 [1.60]	0.12 [0.20]	0.12 [0.69]
Prime-age unemployment rate			-1.53 [18.71]***	-1.85 [13.70]***	-2.36 [6.12]***	-2.36 [15.36]***
Output gap	0.82 [10.72]***	1.02 [8.93]***				
Country dummies	yes	yes	yes	yes	no	no
Time dummies	yes	yes	yes	yes	yes	yes
Observations	367	199	367	199	199	199
R-squared	0.94	0.95	0.96	0.96	0.60	0.60

Columns 1 and 2, estimated by SURE (equations for prime-age men are not reported). Columns 3 to 6 estimated by OLS. Absolute value of t statistics in brackets.
*, **, *** statistically significant at the 10%, 5% and 1% levels, respectively.
Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 2.4. **Employment rate equations: re-estimation of group-specific regressions with public employment as an explanatory variable**

	1	2			3	4
	Prime-age males	Prime-age females			Older workers	Youth
		full-time	part-time	aggregate[1]		
General policies and institutions:						
Public employment rate	0.02	0.46	-0.30	0.15	0.13	-0.45
	[0.19]	[2.85]***	[1.20]		[1.10]	[2.41]**
Average replacement rate	-0.17	-0.16	-0.17	-0.33	-0.22	-0.19
	[6.68]***	[4.07]***	[2.85]***	***	[7.00]***	[4.29]***
Tax wedge	-0.29	-0.14	-0.37	-0.51	-0.31	-0.34
	[8.33]***	[2.79]***	[4.17]***	***	[6.58]***	[5.79]***
Union density	0.06	0.07	-0.16	-0.09	-0.14	0.09
	[2.20]**	[1.20]	[1.77]*		[5.46]***	[1.95]*
EPL	-0.18	-1.53	0.90	-0.63	1.56	-2.49
	[0.50]	[3.08]***	[1.18]		[2.47]**	[3.15]***
PMR	-0.13	-0.90	-0.73	-1.63	0.44	0.75
	[0.47]	[3.17]***	[1.64]	***	[1.30]	[1.51]
High corporatism	0.49	-1.41	0.46	-0.95	-1.18	-1.91
	[1.14]	[1.78]*	[0.37]		[2.60]***	[2.45]**
Group-specific policies and institutions:						
Tax incentives to part-time		-0.60	1.37	0.77		
		[9.18]***	[11.32]***	***		
Relative marginal tax rate on 2nd earner		0.02	-0.85	-0.83		
(transition 100/0 - 100/66)		[0.02]	[0.66]			
Family cash benefits		0.07	-0.31	-0.24		
		[1.54]	[3.84]***	***		
Leave weeks		0.01	-0.03	-0.02		
		[1.90]*	[2.39]**	*		
Implicit tax on continued work					-0.11	
					[3.11]***	
Standard age of eligibility to pension benefits					0.68	
					[2.65]***	
Control variables:						
Female education		2.43	-0.12	2.32		
		[3.84]***	[0.10]	**		
Youth education						-3.48
						[4.15]***
Youth cohort size						-0.22
						[3.43]***
Output gap	0.49	0.13	0.12	0.24	0.20	0.86
	[11.05]***	[2.42]**	[1.40]	***	[4.13]***	[11.04]***
Other controls[2]	no	yes	yes		no	no
Country dummies	yes	yes	yes		yes	yes
Time dummies	yes	yes	yes		yes	yes
Observations	391	270	270		270	366
R-squared	0.99	0.99	0.95		0.98	0.84

Seemingly Unrelated Regression Estimators (SURE). Absolute value of t statistics in brackets. Equations for prime-age men for models corresponding to Columns 2 to 4 are not reported.

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

1: The impact of each explanatory variable on the aggregate prime-age female employment rate is derived as the sum of its effects on full-time and part-time employment rates. Its degree of significance is provided by a Fisher test of the null assumption that the sum of the coefficients on full-time and part-time employment rates is zero.

2: "Other controls" include the proportion of married women, the number of children per woman as well as the interaction between the latter two variables.

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 2.5. **Employment rate equations: re-estimation of group-specific regressions with expenditures on ALMPs as an explanatory variable**

	1	2			3	4
	Prime-age males	Prime-age females			Older workers	Youth
		full-time	part-time	aggregate[1]		
ALMPs:						
Training	0.161	0.096	-0.040	0.056	0.348	0.268
	[3.14]***	[0.91]	[0.21]		[3.33]***	[2.31]**
Youth measures						0.404
						[2.15]**
Other controls and output gap[2]	yes	yes	yes		yes	yes
Country dummies	yes	yes	yes		yes	yes
Time dummies	yes	yes	yes		yes	yes
F test on the difference with prime-age men[3]		0.04	0.67	0.13	0.01	1.53
F test on instruments[4]	42.16	28.53			25.72	27.89
Observations	250	203	203		178	241
R-squared	0.93	0.99	0.98		0.99	0.96

3SLS estimates (ALMP variables are instrumented as in Table 1.9). Absolute value of t statistics in brackets. Instrumental variable equations and equations for prime-age men for models corresponding to Columns 2 to 4 are not reported.

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

1: The impact of each explanatory variable on the aggregate prime-age female employment rate is derived as the sum of its effects on full-time and part-time employment rates. Its degree of significance is provided by a Fisher test of the null assumption that the sum of the coefficients on full-time and part-time employment rates is zero.

2: "Other controls" mean institutions and controls included in the baseline specification of each group.

3: Fisher F statistic on the difference between the estimated coefficients of Training of each specific group and those of prime-age men.

4: Fisher F test statistic on the joint significance of the instruments.

Source : Authors' estimates on the basis of data sources described in Annex 2.

Table 2.6. **Main estimated employment rate effects of policies and institutions: summary table**

	Prime-age males	Older workers	Youth	Prime-age females			Total groups
				total	full-time	part-time	
General policies and institutions:							
Average replacement rate	---	---	---	---	---	---	NEGATIVE
Tax wedge	---	---	---	---	--	---	NEGATIVE
Union density	+++	---	(+)	(-)	+++	---	?
EPL	(-)	+++	---	(-)	---	(+)	?
PMR	(-)	+	(+)	---	---	--	?
High corporatism	(+)	---*	--*	(-)	--*	(+)	?
Group-specific policies and institutions:							
Implicit tax on continued work	NA	---	NA	NA	NA	NA	NEGATIVE
Standard retirement age	NA	+++	NA	NA	NA	NA	POSITIVE
Tax incentives for part-time	NA	NA	NA	+++	---	+++	POSITIVE
Number of weeks of maternity leave	NA	NA	NA	-	+++	---	?
Family cash benefits	NA	NA	NA	---	(+)	---	NEGATIVE

(+) positive but insignificant at 10%; + positive and significant at 10%; ++ positive and significant at 5%; +++ positive and significant at 1%.
(-) negative but insignificant at 10%; - negative and significant at 10%; -- negative and significant at 5%; --- negative and significant at 1%.
*: statistically significant but not robust as it depends on the presence of a single country in the estimation sample.
NA: not applicable.
?: unknown direction.
Source : Authors' estimates on the basis of data sources described in Annex 2.

ANNEX 1

THEORETICAL FOUNDATIONS AND PREVIOUS CROSS-COUNTRY EMPIRICAL EVIDENCE

101. This annex reviews the main policies and institutions which have been identified as possible influences on structural unemployment according to both labour economic theory and past empirical studies (see Table A1.1 for a survey of recent empirical results based on cross-country time-series evidence). These include unemployment benefits, taxes, the role of trade unions and the structure of collective bargaining, employment protection legislation (EPL), product market regulation (PMR), active labour market policies (ALMPs) and minimum wages and determinants of home ownership.

[Table A1.1. Reduced-form panel data estimates of the determinants of structural unemployment: findings from selected studies]

Unemployment benefits

102. Relatively high unemployment benefits that are available for a relatively long duration can have adverse effects on labour market performance. In particular, such benefits may raise unemployment *via* two mechanisms: *i*) by reducing the job-search intensity of the unemployed and their willingness to accept job offers, benefits can weaken the job-matching process; and, *ii*) by lowering the economic cost of unemployment, they may put upward pressure on workers' wage claims and ultimately reduce vacancies and increase separations.[128] Benefit eligibility conditions, monitoring of job search and sanctions also influence unemployment and wage outcomes.

103. On the other hand, unemployment benefits may allow jobseekers more time to find better matches, thereby lowering the likelihood of subsequent job separations and enhancing productivity. In addition, adequate income support for the unemployed is widely seen as being necessary for social reasons, cushioning the impact of job losses on living standards of the unemployed, and is also in some countries regarded as being a precondition for relatively light EPL. If the effects of unemployment benefits on job-search intensity and wage claims dominate the potentially favourable impact on the quality of job matches, a trade-off between equity and efficiency may arise. In empirical work, there is much evidence that various indicators of the level and duration of unemployment benefits have a significantly positive impact on unemployment (Scarpetta, 1996; Nickell, 1998; Elmeskov *et al*., 1998; Nunziata, 2002).

Taxes

104. It is generally accepted that payroll, personal income and consumption taxes have broadly similar effects on the wage-formation process and unemployment levels.[129] What ultimately matters is the sum of these tax rates, *i.e.* the overall tax wedge between the cost of labour to employers and the consumption wage of workers. Whether this tax wedge permanently affects labour market outcomes is *a priori* unclear.

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128 Furthermore, benefits need to be financed by taxes which in turn may bear a negative impact on employment (see below).

129 See for instance the summary in Nickell (1997).

Assuming a high degree of international mobility of capital in the long run, real labour costs depend only on exogenous financial (the real cost of capital) and technological (the level of technological progress) factors (the so-called *"factor-price frontier"*). In a perfectly competitive labour market, workers would be expected to bear the entire tax burden through lower net wages, leaving equilibrium unemployment unchanged. However, this may not be the case in practice, provided that the following three conditions are met:

- The labour market must be imperfect, in the sense that wages can be set above market-clearing levels.

- Workers must, at least to some extent, bargain over net –as opposed to gross– wages, which in turn assumes that they prefer higher direct wages over indirect benefits financed by taxes. This assumption is most likely to be true for taxes and charges that are not paid in exchange for direct government provision of goods, services and transfers to the individual taxpayer. Personal income taxes clearly fall into that category, together with indirect taxes levied on goods and services. However, it is less clear how contributions to public social security schemes should be treated. To the extent that they are perceived by employees as giving rights to future transfers, they would resemble compulsory savings or insurance contributions more than a tax, with a potentially very different impact on employees' incentives to supply labour. As a result, it is difficult in practice to divide social security contributions into taxes, compulsory saving and insurance premia.[130]

- Along the same lines, no institutional constraint must prevent shifting the tax burden onto wages. For instance, in the presence of a binding legal minimum wage, an increase in employers' contribution rates to social security raise the minimum cost of labour, with possible adverse effects on labour demand (see below).

- Finally, the reservation wage – defined as the minimum wage level required in order to prefer work to unemployment – must be affected proportionally less than the wage by an increase in the tax wedge. As underlined for instance by Blanchard (1999), components of the reservation wage include not only unemployment benefits but also other sources of economic welfare that are much less likely to be influenced by the tax wedge, such as leisure, household production, black market activities or non-labour income.

However, theory provides clearer guidance for minimum wage workers: insofar as a rise in payroll taxes can not be shifted onto them, it puts pressure on labour costs and reduces employment.

105. A number of empirical studies have found that high labour taxes tend to increase unemployment rates (Belot and van Ours, 2004; Nickell, 1997), although other studies are less conclusive (Scarpetta, 1996; Nunziata, 2002; Macculloch, and DiTella (2002)). Daveri and Tabellini (2000) find a particularly strong impact in countries with powerful trade unions and a low or intermediate degree of centralisation/co-ordination of the wage bargaining process, and Elmeskov *et al.* (1998) also find notably large effects in countries with intermediate centralisation/co-ordination. These studies conclude that such an environment allows trade unions to compensate for higher taxes by successfully pushing for higher wages.[131]

130 Disney (2004) attempts to divide contributions into tax and premia contributions.

131 The likely implications of different wage bargaining systems for wage claims are discussed in more detail below.

Trade unions and the structure of collective bargaining

106. Strong trade unions have the ability to push wages above market-clearing levels, at the cost of lower employment. Furthermore, according to Bertola, Blau and Kahn (2002), the job losses incurred may fall primarily on those groups whose labour supply is most elastic, *e.g.* youth, women and older workers who often have more alternatives to paid employment compared with prime-age males.[132] However, it has long been argued that, in practice, union influence on wage formation varies depending on the structure of collective bargaining (see *e.g.* Traxler, 2000):

- Decentralised wage bargaining at the firm level has often been regarded as employment-friendly, preventing excessive wage claims since this would lead to a loss of market shares to competitors with detrimental effects on employment. Also, a major effect of collective bargaining is to harmonise working conditions across workers, with possible negative consequences on employment (Siebert, 1997). For instance, the compressed wage structure (across qualification levels) that is usually associated with centralisation/coordination may price a number of low-skilled workers out of work (Calmfors, 1993). Similarly, insufficient wage flexibility at the regional level can maintain pockets of high unemployment in the presence of limited geographical mobility.[133]

- On the other hand, very centralised or coordinated bargaining systems are more likely to lead to wage moderation, because they induce unions to internalise the detrimental effects – *e.g.* on employment – that excessive wage pressure may have at the macroeconomic level. Centralisation/coordination also facilitates implicit or explicit "social pacts" under which unions agree to restrain wage demands in exchange for policy concessions from the government. Under this approach, there is a positive relationship between the degree of centralisation/coordination, or "corporatism",[134] and overall employment performance (Bruno and Sachs, 1985; Soskice, 1990).

- These considerations suggest that the relationship between employment and the degree of corporatism may not be monotonic but rather "hump-shaped" (Calmfors and Driffill, 1988): intermediate systems based on branch-level bargaining without higher-level coordination yield the worst labour market outcomes, as they benefit neither from the internalisation of negative externalities associated with centralised/coordinated systems nor from the market discipline that prevails at the decentralised level. Indeed, when bargaining takes place at the firm level (without coordination), the high elasticity of demand in the product market implies that any price increase resulting from higher wages would result in severe output and job losses. Enterprise-based unions are thus unlikely to exploit their market power to secure higher wages. By contrast, unions which bargain at the industry level have more room to secure higher wages because product demand elasticity is generally lower–as there are unlikely to be as many close substitutes as at the firm

132 These alternative activities are respectively education, household production and retirement.

133 McHugh (2002) also suggests that the greater the degree of centralisation, the more unions are able to pressurize authorities into granting a favourable legal environment for their activities, *i.e.* the more powerful they are in wage bargaining.

134 The degree of corporatism combines two dimensions: *i*) the level of bargaining: centralised, intermediate (at the branch or regional level) or decentralised (at the firm level); and, *ii*) the degree of coordination among, on the one hand, trade unions and, on the other hand, employer associations. This latter dimension of corporatism allows considering cases where coordination at the industry level is functionally equivalent to centralised systems, thereby mimicking their outcomes.

level.[135] Intermediate level bargaining may particularly emerge, with detrimental effects on employment, in the presence of legal extensions of sectoral collective agreements which reduce the scope for domestic competition to moderate wage demands (Box 1).

While certain studies have found support for the "hump-shaped" hypothesis (Elmeskov *et al.*, 1998; Scarpetta, 1996), the empirical literature remains inconclusive overall (for a survey, see Flanagan, 1999).

Box 3. The labour market effects of legal extension mechanisms

Legal extension procedures, by which collective agreements become partially (*e.g.* minimal provisions) or fully binding on parties which were originally non-signatories, were identified as potentially damaging for labour market performance within the context of the original 1994 *Jobs Study*. Concerns about extension mechanisms of collective agreements at the branch level are three-fold:

- They purposefully harmonise working conditions across firms within the sector concerned. Doing so, they prevent wages from reflecting local –firm-level and/or geographical– conditions, notably local productivity levels. As a result, least productive firms and workers are likely to be priced out of product and labour markets, while most productive firms enjoy rents as they do not have to pay higher wages.
- They remove an important restraint on unions' wage demands, namely the need to avoid pricing their members out of work. Knowing that their wages will be imposed on non-members through statutory extension, they are likely to raise their claims, at the cost of lower employment.
- The very existence of statutory extension provisions stimulates membership in employer federations, as individual employers are likely to prefer to influence the terms of negotiated agreements rather than having them dictated by competing firms. Therefore, multi-employer agreements are likely to be more frequent when extension mechanisms exist, *i.e.* the degree of centralisation in wage bargaining is likely to be higher. To the extent that there is some validity in the "hump-shaped" hypothesis, sectoral bargaining (without coordination) may be detrimental to labour market performance (and to economic performance more broadly) compared with decentralised wage-setting.

The Table below, based on a quantitative indicator of extension procedures recently constructed by the OECD (Brandt *et al*., 2004), sums up the importance of extension mechanisms in a number of OECD countries. These appear to be mainly available in continental European countries, with the exception of Nordic countries (but including Finland). Some functional equivalent for statutory extension still exists in Australia through the role played by the "arbitration system" in setting minimal provisions at the sectoral level (Hawke and Wooden, 1998; OECD, 2004). However, its influence has been significantly reduced over the past decade following the 1993 Industrial Relations Reform Act. Another OECD country which has undergone major changes in legal extension procedures during the 1990s is New-Zealand, where the 1991 Employment Contract Act terminated a long-standing tradition of extensions (see for instance Pencavel, 1999).

Legal extension of sectoral collective agreements in selected OECD countries

	Inexistent	**Limited**	**Important**
Importance of extension mechanisms	Canada Denmark Japan New Zealand Norway Sweden United Kingdom United States	Australia Germany Greece Ireland Switzerland	Austria Belgium Finland France Italy Luxembourg Netherlands Portugal Spain

Source: OECD. See Brandt *et al.* (2004) for details.

Almost by definition, the importance of extension mechanisms is a powerful determinant of variations in the level of bargaining coverage across countries. Here, the cross-country correlation between the quantitative index of extension procedures presented in Brandt *et al.* and excess union coverage (the difference between the rate of collective bargaining coverage and trade union density) is found to be significant at the 1% level.[1] This suggest that for given union density,[2] legal extension mechanisms tend to increase coverage, thereby providing more bargaining power to negotiating unions.

There is also some preliminary indication that statutory extension mechanisms contribute to shape the structure of collective

135 For instance, there are arguably fewer substitutes for clothes as a whole than for a particular brand of clothes. However this argument does not take into account the impact of foreign competition, which in practice also limits the ability of industry-level unions to exploit their market power.

bargaining. To investigate this issue, two dummy variables are constructed in a cross-country dimension. One dummy variable takes the value 1 if legal extension procedures exist in the country and 0 otherwise, and the other takes the value 1 if bargaining at the intermediate level plays an important role[3] and 0 otherwise. The correlation between these two dummy variables (over a sample of 22 countries) is found to be significant at the 1% level, suggesting that statutory extension devices indeed stimulate bargaining at the industry level.

1. For other, recent evidence along these lines, see Traxler et al. (2001).

2. However, legal extension procedures also provide lower incentives for employees to join unions, as the application of the terms of negotiated agreements to workers does no longer hinge on union membership.

3. This corresponds to countries where the OECD centralisation index takes the values 2 or 3 for the year 2000 (see OECD, 2004), *i.e.* to countries where industry-level bargains are either concomitant with company/plant level agreements or predominant.

Employment protection legislation

107. In the presence of EPL, firms incur costs when dismissing workers. As a result, they may offer lower wages in order to compensate for these firing costs. At the same time, because they are better protected against dismissals, incumbent workers have more bargaining power and demand higher wages. Resulting wage tensions lead firms to reduce hiring rates, which in turn mechanically increases the average time job seekers have to spend in unemployment before finding a new job. As unemployment becomes more costly to workers, they are more willing to accept lower wages to maintain their jobs, and labour market equilibrium is restored (Blanchard, 1999).[136] Therefore, theory clearly predicts that EPL lowers labour turnover (both hiring and layoff) on the one hand, but increases the length of unemployment spells on the other hand, with ambiguous net effects on aggregate employment and unemployment rates. Econometric estimates of the impact of EPL on the unemployment rate do not clearly settle the matter.[137]

108. However, by reducing turnover, the job prospects for those with relatively weak attachment to the labour market, such as young workers and women, have been found to be compromised (OECD 2004). As well, given that the probability to find a job increases with turnover, the incidence of long-term unemployment is likely to be boosted by restrictive EPL. The low turnover associated with strict EPL may also hamper the effectiveness of the Public Employment Service and other active labour market policies (ALMPs) aiming at re-integrating unemployed workers in the labour market. Moreover, if wages are not sufficiently flexible to allow high dismissal costs to be reflected in lower wages, *e.g.* due to wage floors, the job prospects of low-wage workers may be adversely affected. On the other hand, some degree of EPL may counteract the emergence of implicit agreements between workers and firms in industries subject to greater demand volatility to intensively use intermittent job-spells and unemployment insurance as part of the remuneration in what is in fact a continuous employment relationship.

109. It has also been argued that in a setting where extensive employment protection for workers with permanent contracts coexists with lighter regulation for temporary contracts, wage pressure and therefore unemployment may increase to the extent that unions pursue mainly the interests of permanent workers (Bentolila and Dolado, 1994). The argument behind this is that “insiders” on permanent contracts can raise their wage claims without much risk of job losses as any resulting negative effects on employment will be borne mainly by the “outsiders” who work on temporary contracts. Moreover, deregulated temporary contracts may merely increase the turnover in this segment of the labour market – potentially implying disincentives to train the concerned workers – without constituting a stepping stone to more permanent

136 For more comprehensive analysis of EPL effects on labour market outcomes, see OECD (2004).

137 Scarpetta (1996) and Elmeskov *et al.* (1998) find a positive effect in some of their estimated equations, while Nickell (1997) and Nunziata (2002) find no significant effect.

work relationships, as long as these remain costly to dissolve (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002). These arguments have raised concerns that a deregulation of EPL focused on temporary contracts may not improve labour market performance (Dolado *et al*., 2002).

Product market regulation

110. A growing body of recent literature[138] supports the view that competition in product markets can have an impact on labour market performance. For instance, lower barriers to entry curb market power and rents of incumbents and make entry of new firms possible. Both these effects tend to expand activity levels and labour demand. Moreover, lower product market rents usually translate into lower wage claims, tending to close the gap between productivity and real wages that generates unemployment. Increased competition as a result of product market reforms will also boost real wages *via* lower prices, even if this effect may be attenuated somewhat if wages prior to reforms contain a large rental element that is reduced as intensified competition lowers product market rents and thereby the scope for rent sharing. The increase in real wages will stimulate labour supply and employment. Furthermore, an increase in product market competition is likely to result in changes in the functioning of the labour market with repercussions for employment and unemployment. Stronger competition may harden the bargaining position of employers and increase the employment costs of pushing for higher wages, thereby leading to lower unemployment. Reduced incidence and extent of rent sharing would also tend to reduce "wait unemployment" as it would become less attractive to prolong and limit search for employment opportunities in "high-wage" sectors.

111. While these effects will tend to reduce unemployment and boost employment in the long run, there could be significant short-term adjustment problems. This is because weak competition forces in product markets not only spill over into wages but also to productivity levels as rents are taken out as "quiet life" and "x-inefficiency". Under these circumstances, increased competition may result in a labour shake-out. Whether the shake-out is absorbed quickly or leads to a protracted increase in joblessness and a drop in employment rates is not clear-cut, however. On the one hand, by enhancing resource reallocation through a process of creative destruction, greater competition accelerates any reshuffling process in the labour market, thereby raising equilibrium employment levels. On the other hand, certain institutions that defer the necessary adjustments may lengthen the transition process. This may occur, for instance, when unemployment and related benefits are linked to past wages, including any rent components, thus making unemployment and other forms of inactivity financially attractive compared with wages that are not artificially boosted by rents.

Active labour market policies

112. Properly designed ALMPs – which typically consist of job placement services and labour market programmes such as job-search, vocational training or hiring subsidies – can reduce unemployment by improving the efficiency of the job matching process and by enhancing the work experience and skills of the unemployed. Linking eligibility criteria for unemployment insurance to participation in ALMP programmes can also increase pre-programme job-search effort (OECD, 2003). In practice, however, the effectiveness of ALMPs has been found to differ significantly between different types of programmes. In particular, the outcomes of public job creation and wage subsidy programmes, which often entail large dead-weight losses (subsidised jobs are created that would have been created even without the subsidy) and substitution effects (workers who qualify for a subsidy replace others who do not), have often been disappointing in terms of bringing the unemployed back into to unsubsidised work (Martin and Grubb 2001). Furthermore, any beneficial effects of ALMPs need to be weighted against the costs of taxes required to finance them, which may in turn increase unemployment. Moreover, certain programmes may

138 See Krueger and Pischke (1997), Amable and Gatti (2001), Pissarides (2001), Spector (2002), Blanchard and Giavazzi (2003); Messina (2005); Ebell and Haefke (2003).

reduce search efforts, if not properly designed, and in the case where participation in ALMPs represents a more attractive alternative than open unemployment (financially or possibly with respect to the stigma associated with alternatives), they can augment wage demands (Calmfors and Forslund, 1991).

113. Empirical macroeconomic studies generally find a negative effect of ALMP spending on aggregate unemployment but fail to agree on its magnitude (*e.g.* Scarpetta, 1996; Nickell, 1997, 1998; Nickell and Layard, 1999; Boone and van Ours, 2004). Microeconomic studies find a positive impact for some types of programmes, but not for others (see Heckman *et al.*, 1999, Martin and Grubb, 2001, Kluve and Schmidt, 2001, Betchermann *et al.*, 2004). When proper account is made for the long-run impact of ALMPs on job attachment,[139] intensive employment services, individual case management and mixed strategies with selective referrals to long-term programmes are found to have the largest impact. In any event, apparently similar programmes can yield widely different outcomes, so that detailed programme design is key (OECD, 2005).

Minimum wages

114. In a perfectly competitive labour market, a minimum wage set above the market-clearing level reduces labour demand and thus raises unemployment. In particular, least productive categories of workers are priced out of employment, and possibly of the labour force if, as in the case of youth or low-skilled women, their attachment to the labour market is weaker. However, there is no longer such a clear cut answer once account is made for firms' monopsony power. In such a case, employers have some discretion in wage-setting because workers cannot easily find a job elsewhere (for such reasons as imperfect information on job vacancies or low geographical mobility). Under such circumstances, a rise in the minimum wage reduces unemployment up to a certain point and starts to increase it thereafter.[140] The main lesson from alternative representations of the functioning of labour markets[141] is that minimum wages need to be set at sufficiently low levels in order to minimise their potential negative impact on employment.

Incentives to mobility and housing policy

115. Labour mobility within and across firms may play an important role in reducing regional employment imbalances and assuring a rapid allocation of labour to most productive jobs, thereby increasing the overall efficiency of the job matching process. The literature on this issue has particularly focussed on the potential obstacles to mobility induced by existing housing policies (*e.g.* Oswald, 1997). Since housing costs (mortgage payments or rents) are typically the largest component of households' budgets, decisions to change residence in order to take up a new job are likely to be influenced by housing market conditions and housing policies. In general, homeowners are less likely to migrate than renters. Therefore, *ceteris paribus,* the larger the tax incentives and subsidies to home ownership, the higher the

[139] Micro-evaluation studies often have short post-programme observation windows and fail to capture the effect of ALMPs on post-programme job attachment. This is especially an issue for programmes that aim at increasing job-match quality, such as those with some training content. In fact, these programmes tend to fare better the longer the observation window (OECD, 2005). Similarly, general equilibrium effects are often not accounted for in microeconometric studies (Boone and van Ours, 2004).

[140] More precisely, starting from the equilibrium wage rate in a monopsonistic labour market, a rise in the minimum wage reduces the "monopsonistic rent" of employers and increases employment levels until it is set at the level that would prevail in a perfectly competitive labour market. A rise in the minimum wage beyond this competitive level will then start to reduce employment. In practice, however, an appropriate minimum wage level is hard to determine, because it depends on a wide range of factors including the actual functioning of labour markets and labour demand and supply elasticities.

[141] For a survey of other theoretical models –such as efficiency wage models– in which a rise in the minimum wage may reduce unemployment under certain conditions and up to a certain level, see OECD (1998).

share of home owners relative to renters and the lower the geographical mobility, all the more so if transaction costs (legal procedures, taxes, real-estate fees) are high (Catte *et al.,* 2004; OECD, 2005).

Table A1.1. **Reduced-form panel data estimates of the determinants of structural unemployment: findings from selected studies**

Study	Period, data frequency and countries	Main determinants	Interactions considered	Findings
Baker, Glyn, Howell and Schmitt (2004)	**independent variable :** Aggregate unemployment rate **data:** 1960-1999, 5-year observations, 20 OECD countries **method:** FGLS random effects, allowing for heteroskedastic errors	- Time-varying measures of UB RR and duration, union density and coverage, coordination, wedge, and EPL. - Time-invariant measure of ALMP (average over sample period). - Change in inflation (proxy for short-term macroeconomic situation).	- (UB RR) * (UB duration) - (Union density) * coordination - Wedge * coordination	- When estimating Nickell's (1997, 1998) equation over the same period (1985-1994) but using slightly different (improved) data for institutional variables, none of these are found to be significant. - When a similar regression is run over the whole 1960-1999 period and also incorporates interactions, a counter-intuitive result is that UB RR and duration significantly reduce unemployment provided their level is not particularly high. EPL, union density and wedge are found to have no effect. Conversely, coordination and interaction between union density and coordination have the expected signs and are highly significant. - When the period 1960-1999 is split into two sub-periods, there is no consistency between the results, e.g. UB RR and duration are not significant over 1980-1999, coordination and interaction between union density and coordination are not significant over 1960-1984, EPL reduces unemployment over 1980-1999. - These findings are interpreted as an illustration of the lack of robustness of panel data estimates found in the literature.
Belot and Van Ours (2001)	**independent variable:** Aggregate unemployment rate **data:** 1960-1995, annual, 18 OECD countries **method**: OLS fixed effects	-Time-varying measures of UB RR, union density and coverage, coordination, wedge, and EPL. - Change in inflation (proxy for short-term macroeconomic situation).	All possible interactions between the six institutional determinants of unemployment (institutions) considered	- The tax wedge is highly significant. UB RR, union density and union coverage are significant in most (but not all) regressions. The unusual negative sign found for union coverage is interpreted as a proxy for the effect of the degree of centralisation in wage bargaining. EPL reduces unemployment in some regressions, but is insignificant in the majority of them. Coordination is almost never significant. - Interactions matter: most (but not all) of them are significant and consistent with the underlying theoretical model presented in the paper (although not always with the rest of the literature). In particular, a "restrictive" set of labour market policies (e.g. high UB RR, tax wedge, union density) reduces the effectiveness of a given, isolated reform (e.g. high union density, which leads to higher unemployment, reduces the favourable impact of reducing unemployment benefits on the unemployment rate).
Bertola, Blau and Kahn (2002)	**independent variable :** Aggregate unemployment rate, youth unemployment rate **data**: 1960-1995/96, 5-year observations, 20 OECD countries **method**: OLS fixed effects for specifications without interactions - Non-linear least squares when interactions are considered	- Common, unobservable, macroeconomic shocks: time fixed effects. - Country-specific, observable, macroeconomic shocks: TFP growth, real interest rate, labour demand shift, change in inflation, demographic shock (proxied by youth share in total population). - Institutions: time-invariant measures of UB RR and duration, ALMP, EPL, wedge, union density and coverage, coordination; time-varying measures of UB RR, EPL, union density and coverage, coordination.	- (Common shocks) * (time invariant institutions) - (Common shocks) * (time-varying institutions) - (Country-specific shocks) * (time invariant institutions) - (Country-specific shocks) * (time-varying institutions)	- All observable shocks are significant with expected signs in the baseline specification, except the youth share. Less defavourable shocks in the United States than in Europe have contributed to some extent to diverging unemployment trends across both sides of the Atlantic. However, interactions matter more than shocks *per se* : a model with interactions between shocks and time-invariant institutions explains a significantly larger share of the divergence than a model with shocks only (50% vs 11%). - Similarly, changes in institutions have contributed to some extent to diverging unemployment trends, but interactions matter more than institutions *per se* : a model with interactions between shocks and time-varying institutions explains a larger share of the divergence than a model with shocks and institutions entered separately (63% vs 30%). - However, most interactions (except those including unemployment benefit duration and, to a lesser extent, EPL) are insignificant in regressions with time-invariant institutions, and even more so with time-varying ones. This questions the robustness of simulations using these models.

Table A1.1. **Reduced-form panel data estimates of the determinants of structural unemployment: findings from selected studies** *(Cont.)*

Study	Period, data frequency and countries	Main determinants	Interactions considered	Findings
Blanchard and Wolfers (2000)	**independent variable:** Aggregate unemployment rate, structural unemployment rate **data:** 1960-1995/96, 5-year observations, 20 OECD countries **method**: OLS fixed effects for specifications without interactions - Non-linear least squares when interactions are considered	- Common, unobservable, macroeconomic shocks: time fixed effects - Country-specific, observable, macroeconomic shocks: TFP growth, real interest rate, labour demand shift - Institutions: time-invariant measures of UB RR and duration, ALMP, EPL, wedge, union density and coverage, coordination; time-varying measures of UB RR and EPL	- (Common shocks) * (time invariant institutions) - (Common shocks) * (time-varying institutions) - (Country-specific shocks) * (time invariant institutions) - (Country-specific shocks) * (time-varying institutions)	- Shocks account for most of the general evolution of unemployment over the last decades. - Interactions between shocks and institutions account for most of the heterogeneity of unemployment evolutions over the sample period. - Most interacted terms are significant (except those including union coverage), i.e. institutions contribute to determining the final impact of a given shock on unemployment. - When time-varying (as opposed to time-invariant) measures of EPL and UB RR are used, the results are significantly weaker.
Boone and Van Ours (2004)	**independent variable:** Aggregate unemployment and non-employment rates **data:** 1985-1999, annual and 5-year averages, 19 OECD countries **method**: OLS fixed effects and FGLS Random effects	- Time-varying measures of UB generosity, union density as well as shares of each ALMP category in total ALMP (fixed effects specifications) or time-invariant measures of ALMPs (average over sample period) by category in random effect specifications - Other institutions mentioned but not shown (authors argue they are not significant) - Change in inflation (proxy for short-term macroeconomic situation)	UB generosity * ALMP training	- UB and union density are negative and significant. - Labour market training is found to be the most effective programme to bring down unemployment, public employment services have some impact while subsidized jobs are not effective at all. - UB generosity and labour market training are found to be complementary.
Daveri and Tabellini (2000)	**independent variable**: Aggregate unemployment rate, first difference in aggregate unemployment rate **data:** 1965-1995, 5-year observations, 14 OECD countries **method**: - OLS fixed effects in basic specification with unemployment level as dependent variable - OLS or GLS in alternative specification with change in unemployment as dependent variable	- Time-varying measures of labour tax rate (defined as the tax wedge excluding consumption taxes), UB generosity and EPL	- (Labour tax rate) * (strength of unions in wage bargaining and degree of centralisation/coordination) NB: in practice countries are classified into 3 groups: 1) weak unions (whatever the degree of centralisation/coordination); 2) strong unions with highly centralised or coordinated bargaining; 3) strong unions with low or intermediate degree of centralisation/coordination	- In all specifications, the impact of the labour tax rate is consistent with theory: very large (compared with other studies) and significant in countries with high union power and low/intermediate degree of centralisation/coordination in bargaining, small (and even insignificant in regressions in first differences) where union power is low, and insignificant where unions are powerful but the degree of centralisation/coordination is high. - When the dependent variable considered is the level of unemployment, the coefficient on UB generosity is positive and significant at 5% level, and that of EPL is negative and significant at 10% level. Both become insignificant in regressions in first differences. - The consumption tax rate has no effect on unemployment when entered separately in any specification. This suggests that only the labour-related component of the tax wedge matters for unemployment.

Table A1.1. **Reduced-form panel data estimates of the determinants of structural unemployment: findings from selected studies** *(Cont.)*

Study	Period, data frequency and countries	Main determinants	Interactions considered	Findings
Elmeskov, Martin and Scarpetta (1998)	**independent variable** : Aggregate unemployment rate **data:** 1983-1995, annual, 19 OECD countries **method**: FGLS random effects	- Time-varying measures of UB generosity, union density, coordination, centralisation, wedge (and EPL over 1989-1995) - Time-invariant measure of ALMP (average over sample period) - Other variables : minimum wage - Control variables: output gap	- (UB generosity) * ALMP - (UB generosity) * EPL - EPL * corporatism - Wedge * corporatism	- UB generosity, high corporatism, coordination and wedge are highly significant, centralisation (consistent with hump-shaped hypothesis) and EPL to a lesser extent; mixed evidence regarding ALMP. - No significant effect of union density and minimum wage. - Detrimental effects of EPL and wedge are larger in countries with intermediate degree of corporatism (consistent with hump-shaped hypothesis); detrimental impact of UB generosity is not affected by tightness of EPL, and is larger when ALMP is low (counter-intuitive results).
International Monetary Fund (2003)	**independent variable** : Aggregate unemployment rate **data:** 1960-1998, annual, 20 OECD countries **method**: fixed effects FGLS, dynamic panel	- Country-specific, observable macroeconomic shocks: labour productivity growth, real interest rate, terms of trade - Institutions: time-varying measures of UB generosity, union density, coordination, wedge, EPL - Other variables: degree of central bank independence	All possible interactions between the five institutional determinants of unemployment (institutions) considered	- Labour productivity growth and real interest rate shocks are highly significant. Significance of terms of trade shock varies across specifications. - EPL, union density and tax wedge are highly significant, as well as coordination (evidence in favour of the "hump-shaped" hypothesis). UB generosity is significant only when no country-specific time-trends are included in the regression. - Several interactions between institutions have a significant impact on aggregate unemployment. The most robust ones across different specifications of the estimated equation are (EPL) * (union density) and (wedge) * (union density), both of which are negatively signed. - Central bank independence tends to raise unemployment. This effect is mitigated when bargaining coordination is high. - Adjustment speed towards equilibrium unemployment is reduced by UB generosity and bargaining coordination. - Institutions account for cross-country differences in unemployment but less so for the trend rise in European unemployment is more limited. In this respect, shocks also appear to have played a major rol
Nickell (1997, 1998)	**independent variable** : Log(aggregate unemployment rate), log(short-term unemployment rate), log (long-term unemployment rate) **data**: 1983-1994, two 6-year observations (1983-88 and 1989-94), 20 OECD countries **method**: FGLS random effects	Time-varying measures of UB RR and duration, union density and coverage, coordination, wedge, EPL, ALMP (instrumented) - Change in inflation (proxy for short-term macroeconomic situation) - Owner occupation rate (proxy for barriers to regional mobility, used only in Nickell, 1998)	-	- UB RR, union density and (more so) coverage, coordination, ALMP (more so for the long-term unemployed only in Nickell, 1997) and wedge all have a highly significant impact on aggregate unemployment. UB duration is significant for total and long-term unemployment in Nickell (1998), but only for the latter in Nickell (1997) - No significant effect of EPL on aggregate unemployment, but negative impact on aggregate employment (however, zero impact when females are excluded)

Table A1.1. **Reduced-form panel data estimates of the determinants of structural unemployment: findings from selected studies** *(Cont.)*

Study	Period, data frequency and countries	Main determinants	Interactions considered	Findings
Nickell, Nunziata and Ochel (2005)	**independent variable** : Aggregate unemployment rate 1961-1992 or 1961-1995 depending on specifications, **data:** annual, 20 OECD countries **method**: FGLS random effects, dynamic panel allowing for heteroskedastic errors and country-specific first order serial correlation	- Common, unobservable, macroeconomic shocks: time fixed effects - Country-specific, observable shocks: change in TFP growth, change in real import prices, real interest rate, change in money supply growth (proxy for aggregate demand shock), labour demand shift - Institutions: time-varying measures of UB RR and duration, change in union density, coordination, wedge, EPL - Owner occupation rate (proxy for barriers to regional mobility) - Country-specific time trends	- (UB RR) * (UB duration) - (Union density) * coordination - Wedge * coordination - (Common shocks) * (time invariant institutions) in some specifications	- Labour demand, TFP and real import price shocks are highly significant, unlike money supply shock. Statistical significance of real interest rate shock varies across specifications - UB RR and coordination are highly significant. UB duration and wedge are significant in most (but not all) regressions, while EPL is not - All interactions between institutions have a highly significant impact on aggregate unemployment, consistent with theory: (UB RR) * (UB duration), (union density) * coordination, wedge * coordination. - Changes in institutions explain a sizeable share of the rise in European unemployment over the sample period: 55%, two thirds of which stem from increases in UB RR, UB duration and tax wedges - The high significance of interactions between common shocks and time-invariant institutions found by Blanchard and Wolfers (2000) vanishes when one controls for changes in institutions
Nicoletti and Scarpetta (2004)	**independent variable** : Aggregate employment rate in the non-agricultural business sector **data**: 1980-2002, annual, 20 OECD countries **method**: fixed effects	Time-varying measures of public employment rate, UB generosity, union density, coordination, centralisation; EPL, PMR - In certain specifications: LMR (principal component of UB, EPL and tax wedge) and Inst (principal component of union density, centralisation and coordination) - Control variables: output gap, country-time trends	- Lmr * PMR - EPL * corporatism	- UB generosity, union density, PMR, EPL have significantly negative coefficients in all regressions, - Evidence of an hump-shaped effect of corporatism. - The estimated effect of EPL is entirely due to countries with an intermediate level of corporatism. - The effect of PMR is found to be more negative in countries with stricter LMR.
Nunziata (2002)	**independent variable** : Aggregate unemployment rate **data:** 1960-1995, annual, 20 OECD countries **method**: fixed effects, FGLS, dynamic panel allowing for heteroskedastic errors and country-specific first order serial correlation	- Common, unobservable, macroeconomic shocks: time fixed effects - Country-specific, observable, macroeconomic shocks: change in TFP growth, change in real import prices, real interest rate, change in money supply growth (proxy for aggregate demand shock), labour demand shift - Institutions: time-invariant and time-varying measures of UB RR and duration, EPL, wedge, union density and coordination - Other variables: fixed term contracts regulation, owner occupation rate	- (UB RR) * (UB duration) - (Union density) * coordination - Wedge * coordination - (Common shocks) * (time invariant institutions) - (Common shocks) * (time-varying institutions) - (Country-specific shocks) * (time invariant institutions) - (Country-specific shocks) * (time-varying institutions)	- Labour demand, TFP and real import price shocks are highly significant, unlike money supply shock. Statistical significance of real interest rate shock varies across specifications - UB RR is highly significant; UB duration, coordination and change in (but not level of) union density are significant in most estimates, wedge in some of them. Fixed-term contract regulations raise unemployment - EPL does not affect unemployment levels but increases unemployment persistence - Interactions between institutions are significant with expected signs: (UB RR) * (UB duration), (union density) * coordination and, to a lesser extent, wedge * coordination - Changes in institutions explain a sizeable share of the rise in European unemployment over the sample period: 55%, much of the remainder being due to the direct effect of shocks - Some interactions between shocks and institutions (including UB RR and duration) have a significant effect in a number of specifications. However, their contribution to past unemployment trends is generally found to be low compared with the direct effect of changes in institutions

Table A1.1. **Reduced-form panel data estimates of the determinants of structural unemployment: findings from selected studies** *(Cont.)*

Study	Period, data frequency and countries	Main determinants	Interactions considered	Findings
Scarpetta (1996)	**independent variable** : Aggregate, youth and LTU unemployment rates, as well as aggregate non-employment rate **data**: 1983-1993, annual, 15 to 17 OECD countries depending on specifications **method**: FGLS random effects	Time-varying measures of UB generosity, union density, coordination, centralisation; time-invariant measures of EPL (1989 value) and ALMP (average over sample period) - Other variables : time-varying measures of wedge, real interest rate, terms of trade, pervasiveness of trade restrictions (proxy for product market competition) - Control variables: output gap	- (Union density) * coordination - (Union density) * centralisation	- UB generosity, union density and coordination are highly significant in all regressions, as well as EPL (especially for youth and LTU) when no centralisation variable is included; significant impact of centralisation, consistent with hump-shaped hypothesis; mixed evidence regarding ALMP. - No significant effect of other individual variables (except wedge for LTU, and trade restrictions for LTU and non-employment) - (Union density) * coordination is significant with the expected negative sign. - Dynamic equation, estimated using non-linear SUR estimators, shows that adjustment speed towards equilibrium unemployment is reduced by UB generosity, strict EPL and union density. Impact of centralisation on speed of adjustment is consistent with hump-shaped hypothesis.

Notations:

UB RR : unemployment benefits' replacement rate.

UB generosity: a combination of UB RR and duration.

Wedge: tax wedge, i.e. the gap between the cost of labour to the employer and the employee's take-home pay.

LTU: long-term unemployed.

SUR: seemingly unrelated regression.

OLS: ordinary least squares.

FGLS: feasible generalised least squares.

EPL: index for employment protection legislation.

ALMP: an indicator of active labour market policies.

EPL: employment protection legislation.

UB duration: duration of unemployment benefits.

PMR: product market regulation.

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ANNEX 2

DATA SOURCES AND METHODOLOGY

A2.1 Unemployment and employment rates

Aggregate unemployment (employment) rate:

Definition: unemployed (employed) workers as share of the labour force (working-age population), in %. Aggregate rates refer to the 15-64 age group.

Source: OECD, Database on Labour Force Statistics; OECD, Annual Labour Force Statistics.

Data adjustments: while the primary source is the OECD Database on Labour Force Statistics, Annual Labour Force Statistics –which are usually available over longer time periods– were also used in some cases to extrapolate (un)employment rates backwards (under the assumption of similar percentage *changes* in unemployment and employment rates in both sources). Missing observations are obtained by linear interpolation when possible.

Group-specific employment rates:

Definition: employed workers as share of the corresponding population group, in %.

Source: OECD, Database on Labour Force Statistics.

Data adjustments: missing observations are obtained by linear interpolation when possible.

Public employment rate:

Definition: public employment as a share of the working-age population (15-64 age group), in %.

Source: OECD, Database on Labour Force Statistics; OECD, Annual Labour Force Statistics.

Data adjustments: missing observations are obtained by linear interpolation when possible.

A2.2 Policy and institutional indicators used in unemployment rate regressions

Average unemployment benefit replacement rate:

Definition: average unemployment benefit replacement rate across two income situations (100% and 67% of APW earnings), three family situations (single, with dependent spouse, with spouse in work) and three different unemployment durations (1st year, 2nd and 3rd years, and 4th and 5th years of unemployment).

Source: OECD, Benefits and Wages Database.

Data adjustments: original data are available only for odd years. Data for even years are obtained by linear interpolation.

Initial (first year) unemployment benefit replacement rate:

Definition: average unemployment benefit replacement rate during the first year of unemployment across two income situations (100% and 67% of APW earnings) and three family situations (single, with dependent spouse, with spouse in work).

Source: OECD, Benefits and Wages Database.

Data adjustments: original data are available only for odd years. Data for even years are obtained by linear interpolation.

Unemployment benefit duration:

Definition: ratio of average to initial unemployment benefit replacement rate (see above).

Labour tax wedge:

Definition: tax wedge between the labour cost to the employer and the corresponding net take-home pay of the employee for a single-earner couple with two children earning 100% of APW earnings. The tax wedge expresses the sum of personal income tax and all social security contributions as a percentage of total labour cost.

Source: OECD, *Taxing Wages*.

Data adjustments: Austria: original data include employers' social security contributions starting from 1997 only, thereby inducing an upward shift in tax wedge from this year; the tax wedge starting from 1997 is therefore recalculated based on the fact that employers' contribution rates to social security remained unchanged between 1996 and 1997. Netherlands: unlike other years, in 2002 and 2003 APW earnings are just above the threshold beyond which employers and employees do no longer have to contribute to the national health insurance plan (private medical insurance is typically provided instead), thereby inducing a temporary decline in the tax wedge; this issue is addressed by replacing the 2002 and 2003 observations by data obtained from linear interpolations between the 2001 and 2004 observations.

Tax wedge derived from National Accounts:

Definition: combined labour and consumption tax rate derived from National Accounts. Compared with labour tax wedges simulated by tax models –such as those published in *Taxing Wages*, the main advantage of tax wedges derived from National Accounts is to incorporate consumption taxes. A number of other differences exist between the *Taxing Wages* calculations and those based on National Accounts.[142] In particular:

- While differences in population structure (*e.g.* income distribution or demographics) affect implicit average effective tax wedges derived from National Accounts, they have no impact on the *Taxing Wages* calculations. From this standpoint, the latter are thus more comparable across countries as they refer to similar family situations. On the other hand, the *Taxing Wages*

142 For further details, see Heady C. (2002), "The 'Taxing Wages' Approach to Measuring the Tax Burden on Labour", *CESifo Working Paper* (Category 1: Public Finance) No. 967, June.

calculations may not accurately reflect the situation of very low and very high income individuals.

- Implicit average effective tax wedges are based on "real" data and thus reflect all the factors that influence the amount of taxes actually paid. By contrast, the *Taxing Wages* calculation takes no account of observed data but instead is based on calculations for a small number of different "typical" families. The extent to which these "typical families" are representative of the population of actual taxpayers will usually differ across countries.

- Unlike tax wedges derived from National Accounts, data published in *Taxing Wages* incorporate family benefits.

- The timing of changes in tax wedges may differ across sources. This is because revenue statistics are computed on a cash basis while tax wedges published in *Taxing Wedges* reflect the tax rules that prevail for the year considered.

Source: Carey D. and J. Rabesona (2002), "Tax Ratios on Labour and Capital Income and on Consumption", *OECD Economic Studies* No35, 2002/2; Carey D. (2003), "Tax Reform in Belgium", *OECD Economics Department Working Paper* No. 354, May. The calculations made by these authors are based in turn on *OECD Revenue Statistics* and *OECD National Accounts*.

Labour and consumption tax rates:

Definition: components of the overall tax wedge derived from National Accounts (see above).

Employment Protection Legislation (EPL):

Definition: OECD summary indicator of the stringency of Employment Protection Legislation.

Source: OECD, *Employment Outlook* 2004.

Product Market Regulation (PMR):

Definition: OECD summary indicator of regulatory impediments to product market competition in seven non-manufacturing industries. The data used in this paper[143] cover regulations and market conditions in seven energy and service industries: gas, electricity, post, telecoms (mobile and fixed services), passenger air transport, railways (passenger and freight services) and road freight.

Source: Conway, P., D. De Rosa, G. Nicoletti, and F. Steiner (2006), "Regulation, competition, and productivity convergence", *OECD Economics Department Working Paper*, forthcoming.

Degree of corporatism:

Definition: indicator of the degree of centralisation/co-ordination of the wage bargaining processes, which takes values 1 for decentralised and uncoordinated processes, and 2 and 3 for intermediate and high

[143] Details on the broader PMR indicator for the whole economy –which is available only for 1998 and 2003 and therefore is not used in this paper– can be found in Conway, P., V. Janod and G. Nicoletti (2005) "Product Market Regulation in OECD Countries: 1998 to 2003", *Economics Department Working Paper* No. 419, OECD, Paris.

degrees of centralisation/co-ordination, respectively. The "low corporatism" dummy variable frequently used in this paper takes value 1 when bargaining is decentralised and uncoordinated and zero otherwise.

Source: OECD, *Employment Outlook* 2004.

Data adjustments: original data are five-year averages and classify countries in each period along a 0-5 scale from least to most "corporatist" countries. In the present paper, annual data have been reconstructed based on various sources on the timing of past changes in centralisation and/or co-ordination of wage bargaining. Furthermore, the indicator has been rescaled along a 1-3 scale. In this process, it has been assumed that wage bargaining in France predominantly occurs at the intermediate level, while original data describe it as a mix of firm-level and industry-level bargaining. For other countries, values 1, 2 and 3 correspond to values 1-2, 3 and 4-5 in the original dataset, respectively.

Union density:

Definition: trade union density rate, *i.e.* the share of workers affiliated to a trade union, in %.

Source: OECD, *Employment Outlook* 2004.

Data adjustments: data for missing years are obtained by linear interpolation. Furthermore, original data are typically available until 2001 for most OECD countries. Extrapolations have therefore been made in order expand data availability up to 2003. These are mainly based on national sources but, in some cases, an assumption of unchanged union densities over the period 2001-2003 had to be made due to lack of data.

Union coverage:

Definition: collective bargaining coverage rate, *i.e.* the share of workers covered by a collective agreement, in %.

Source: OECD, *Employment Outlook* 2004. In the case of Ireland, the average bargaining coverage rate is taken from Belot M. and J. Van Ours (2004), "Does the Recent Success of Some OECD Countries in Lowering their Unemployment Rates Lie in the Clever Design of their Labor Market Reforms?", *Oxford Economic Papers*, Vol. 56.

Public expenditures on active labour market policies (ALMPs):

Definition: public expenditures on active labour market programmes per unemployed worker as a share of GDP per capita (or public expenditures on active labour market programmes as a share of GDP, depending on econometric specifications), in %. The five main categories use in the disaggregated analysis are defined as follows:

1. Public employment services (PES) and administration: placement, counselling and vocational guidance, job-search courses, assistance with displacement costs, administration of unemployment benefits, all other administration costs of labour market agencies including running labour market programmes.

2. Labour market training: training for unemployed adults and those at risk, training for employed adults (special training programmes for youth and disabled are excluded).

3. Youth measures: special programmes concerning measures for unemployed and disadvantaged youth, support of apprenticeship and related forms of general youth training.

4. Subsidized employment: targeted measures to promote or provide employment for the unemployed and other priority groups (but not youth and the disabled).

5. Measures for the disabled: special programmes concerning vocational rehabilitation and work for the disabled.

Source: OECD calculations based on OECD, *Employment Outlook*.

Data adjustments: data for Italy are not available before 1996 in the OECD database; for the purpose of this study, these data have been extrapolated backwards based on the dataset used by Elmeskov J., J. Martin and S. Scarpetta (1998), "Key Lessons for Labour Market Reforms: Evidence from OECD Countries' Experiences", *Swedish Economic Policy Review*, Vol. 5.

Minimum wage:

Definition: ratio of statutory minimum wage to median wage, in %.

Source: OECD Minimum Wages Database.

A2.3 Policy and institutional indicators used in group-specific employment rate regressions

Tax incentives for part-time work:

Definition: increase in household disposable income between a situation where the husband earns the entire household income (133% of APW), and a situation where husband and wife share earnings (100% and 33% of APW respectively), for a couple with two children. Denoting the first scenario by A, and the second by B, the calculation is simply:

$$Tax\ incentives\ to\ part-time = \frac{(Household\ Net\ Income)_B - (Household\ Net\ Income)_A}{(Household\ Net\ Income)_A}.$$

Source: OECD calculations based on OECD "tax models".

Relative marginal tax rates on second earners:

Definition: ratio of the marginal tax rate on the second earner to the tax wedge for a single-earner couple with two children earning 100% of APW earnings (see definition of the "labour tax wedge" above). The marginal tax rate on the second earner is in turn defined as the share of the wife's earnings which goes into paying additional household taxes:

$$Tax\ second\ earner = 1 - \frac{(Household\ Net\ Income)_B - (Household\ Net\ Income)_A}{(Household\ Gross\ Income)_B - (Household\ Gross\ Income)_A},$$

where A denotes the situation in which the wife does not earn any income and B denotes the situation in which the wife's gross earnings are X%of APW. Two different tax rates are calculated, depending on whether the wife is assumed to work full-time (X = 67%) or part-time (X = 33%). In all cases it is assumed that the husband earns 100% of APW and that the couple has two children. The difference between gross and net income includes income taxes, employee's social security contribution, and universal cash benefits. Means-tested benefits based on household income are not included (apart from some child benefits that vary with income) due to lack of time-series information. However, such benefits are usually less relevant at levels of household income above 100% of APW.

Source: OECD calculations based on OECD "tax models".

Family cash benefits:

Definition: increase in household disposable income from child benefits (including tax allowances) for a single-earner couple earning 100% of APW earnings. It is calculated as follows:

$$Child\ Benefits = \frac{(Household\ Net\ Income)_B - (Household\ Net\ Income)_A}{(Household\ Net\ Income)_A},$$

where A denotes a household earning 100 % of APW without children, and B denotes a household earning 100% of APW with two children.

Source: OECD calculations based on OECD "tax models".

Number of leave weeks:

Definition: maximum number of leave weeks that can be taken by a mother for the birth of a first child as maternity leave, parental leave, and childcare leave.

Source: The main data source is Gauthier A.H. and A. Bortnik (2001), "Comparative maternity, parental, and childcare database", preliminary version (February), University of Calgary. Additional calculations, in particular to improve data comparability across countries, come from Jaumotte (2004).

Data adjustments: the focus is on the most generous provisions that can be obtained, even though these may not apply to all women depending on their employment history or income. Only the national legislation is used (variations in schemes by region, province, länder, or canton are not included). For further country-specific details, see Jaumotte (2004), "Labour force Participation of Women: Empirical Evidence on the Role of Policy and Other Determinants in OECD Countries", *OECD Economic Studies*, No. 37.

Public expenditures on childcare:

Definition: public spending on formal day care and pre-primary school per child, in 1995 PPP-US$. Data on formal day care do not include tax expenditures (*i.e.,* tax allowances and tax credits for childcare expenses) unless they are refundable. Spending on pre-primary school includes both direct and indirect – i.e. transfers and payments to private entities– expenditures.

Source: The main data sources for formal day care and pre-primary school spending are the OECD "Social Expenditures Database" and the OECD "Education Database", respectively. The target population of children for formal day care and pre-primary school is calculated using data on age of entry to primary school from the UNESCO Statistical Yearbook (various years) and data on the number of children by age category. The latter is obtained from national sources for EU countries and from the United Nations World Population Prospects 1950-2050 (The 2000 revision, February 2001) for other countries.

Data adjustments: country-specific details are provided in Jaumotte (2004), "Labour force Participation of Women: Empirical Evidence on the Role of Policy and Other Determinants in OECD Countries", *OECD Economic Studies*, No. 37**.**

Implicit tax on continued work:

Definition: weighted average of implicit tax rates on continued work between age 55 and 60 in early retirement pathways (50%) and between age 60 and 65 in both early retirement pathways (25%) and old-age pension schemes (25%). This variable can be interpreted as a summary measure of retirement incentives facing the age group 55-64**.**

Source: Original data on implicit taxes on continued work for various age groups in both early retirement pathways and old-age pension schemes are described in detail in Duval R. (2004), "Retirement Behaviour in OECD Countries: Impact of Old-age Pension Schemes and Other Social Transfer Programmes", *OECD Economic Studies*, No. 37.

Standard retirement age:

Definition: standard age of eligibility to old-age pension benefits

Source: Duval R. (2004), "Retirement Behaviour in OECD Countries: Impact of Old-age Pension Schemes and Other Social Transfer Programmes", *OECD Economic Studies*, No. 37.

Youth minimum wage:

Definition: ratio of statutory minimum wage for workers age 20-24 to median wage, in %.

Source: OECD Minimum Wages Database.

Data adjustments: the statutory minimum wage for younger workers is calculated as a simple average of statutory minimum wages for workers aged 20, 21, 22, 23 and 24, respectively.

A2.4 Macroeconomic shocks

Total factor productivity shock:

Definition: deviation of the logarithm of Total Factor Productivity (TFP) from its trend calculated by means of a Hodrick-Prescott (HP) filter (smoothing parameter $\lambda = 100$).

Source: OECD calculations based on OECD, *Economic Outlook* 76. Gross capital stock data for Canada and the United States are drawn from OECD (2003), *The Sources of Economic Growth in OECD Countries*.

Data adjustments: The calculation proceeds in three steps. First, growth in the Solow residual in the business sector is calculated as: $\Delta \log(TFP) = [\Delta \log(Y) - \alpha \Delta \log(N) + (1-\alpha)\Delta \log(K)] / \alpha$, where Y refers to real business sector GDP, N to total employment, K to the gross capital stock and α to labour income as a share of business sector income. Second, an index log(TFP) of the logarithm of TFP is obtained by cumulating the annual values of Δlog(TFP). Finally, the TFP shock variable is computed as the difference between log(TFP) and its HP filtered trend.

Terms of trade shock:

Definition: logarithm of the relative price of imports weighted by the share of imports in GDP, i.e. terms of trade shock = $(M/Y) * \log(P_M / P_Y)$, where M and Y denote total imports and GDP in nominal terms, respectively, and (P_M / P_Y) is the ratio of the deflator of total imports to the GDP deflator.

Source: OECD calculations based on OECD, *Economic Outlook* 76.

Real interest shock:

Definition: difference between the 10-year nominal government bond yield (in %) and the annual change in the GDP deflator (in %).

Source: OECD calculations based on OECD, *Economic Outlook* 76 and IMF, *International Financial Statistics*.

Labour demand shocks:

Definition: logarithm of the labour share in business sector GDP purged from the short-run influence of factor prices.

Source: OECD calculations based on OECD, *Economic Outlook* 76.

Data adjustments: The methodology follows Blanchard O. and J. Wolfers (2000), "The Role of Shocks and Institutions in the Rise of European Unemployment: The Aggregate Evidence", *The Economic Journal,* Vol. 110, No. 462, March. First, measures of real wages and employment in efficiency units are computed as $W_{efficiency\ units} = (W / P_Y) / TFP$ and $N_{efficiency\ units} = N * TFP$, respectively (for details on notations and sources, see the construction of total factor productivity shocks above). The simplest possible measure of labour demand shocks would be the negative of the sum of the logarithm of the ratio of labour input in efficiency units to real output in the business sector, on the one hand, and the logarithm of real wages in efficiency units, on the other hand: $-[\log(N_{efficiency\ units} / Y) + \log(W_{efficiency\ units})] = -\log[(N * TFP) / Y] - \log[(W / P_Y) / TFP] = -\log[(W * N) / (P_Y * Y)] = -$ (labour's share of business sector income). However, this simple measure of labour demand shocks is accurate only to the extent that the production function is Cobb-Douglas and factor proportions adjust instantaneously to changes in factor prices. Insofar as the latter assumption is unlikely to be verified in the short-run, changes in the labour share reflect both genuine labour demand shocks and the lagged adjustment of factor proportions to changes in factor prices.

Therefore, it is necessary to purge the labour share from the short-run influence of factor prices. For simplicity and comparative purposes, this is done here by following the same methodology as Blanchard (1998), "Revisiting European Unemployment: Unemployment, Capital Accumulation and Factor Prices", *NBER Working Paper* No. 6566, May. Concretely, a wage measure which takes into account the gradual adjustment of factor proportions is computed as: $\log(W_{adjusted}) = \lambda * \log(W_{adjusted}) + (1-\lambda) * \log(W_{efficiency\ units})$, where the value of parameter λ is set equal to 0.8 in line with estimates on annual data provided by Blanchard. The labour demand shock is then constructed as $-[\log(N_{efficiency\ units} / Y) + \log(W_{eadjusted})]$. The negative sign implies that an increase in this variable should be interpreted as an adverse labour demand shock. Finally, this variable is set equal to zero in 1970 (or in the first year of data availability for those countries where long time series are unavailable).

A2.5 Other variables

Output gap:

Definition: OECD measure of the gap between actual and potential output as a percentage of potential output.

Source: OECD, *Economic Outlook* 76, December 2004.

Rate of home ownership:

Definition: share of owner-occupied housing as a percentage of total occupied housing stock (average over the 1990-2000 period, 2000 observation only for Portugal).

Source: Oswald, A.J. (1999), "The Housing Market and Europe's Unemployment: A Non-Technical Paper", University of Warwick, United Kingdom, May, (www2.warwick.ac.uk/fac/soc/economics/staff/faculty/oswald/homesnt.pdf). Data for Portugal are taken from OECD, *Employment Outlook* 2005.

Female education:

Definition: number of years of education of female population aged 25 and over.

Source: Barro, R. J. and J-W. Lee (2000), "International data on educational attainment updates and implications", *NBER Working Paper* No. 7911, September.

Data adjustments: for the years 2000-2003, data are extended using average years of education of the population aged between 25 and 64 years, taken from Conway, P., D. De Rosa, G. Nicoletti, and F. Steiner (2006), "Regulation, competition, and productivity convergence", *OECD Economics Department Working Paper*, forthcoming, using the following two-step procedure: first, over the period 1990-1999 the growth rate of group-specific years of education is regressed on the growth rate of average years of education in the whole population aged between 25 and 64 years; and, second, coefficient estimates obtained thereby are used to extrapolate group-specific years of education.

Proportion of married women:

Definition: share of married women among the female population aged 25-54.

Source: United Nations, *Demographic Yearbook.*

Data adjustments: data extrapolation to years 2002 and 2003 based on country-specific trends in the proportion of married women over the period 1998-2001.

Number of children per woman:

Definition: ratio of total population aged 0-14 to female population aged 15-64.

Source: OECD, Annual Labour Force Statistics.

Youth cohort size:

Definition: ratio of total population aged 15-24 to total population aged 25-54.

Source: OECD, Annual Labour Force Statistics.

Relative youth education:

Definition: difference between the number of education years of total population aged 15 and over and the number of education years of total population aged 25 and over.

Source: Barro, R. J. and J-W. Lee (2000), "International data on educational attainment updates and implications", *NBER Working Paper* No. 7911, September.

Data adjustments: for the years 2000-2003, data are extended using average years of education in the population aged between 25 and 64 years, taken from Conway, P., D. De Rosa, G. Nicoletti, and F. Steiner (2006), "Regulation, competition, and productivity convergence", *OECD Economics Department Working Paper*, forthcoming, using the same two-step procedure described above for female education.

Descriptive statistics of all variables are reported in Table A2.1-A2.3.

Table A2.1. **Descriptive statistics of variables used in the unemployment regressions, 1982-2003**

Variable	Mean	Standard deviation	Maximum	Minimum	Number of observations
Unemployment rate (%)	7.76	4.19	24.04	0.40	434
Average benefit replacement rate (%)	29.68	12.63	64.94	0.35	434
Initial benefit replacement rate (%)	48.03	19.80	88.80	1.04	434
Benefit duration (years)	0.65	0.23	1.64	0.32	434
Tax wedge (%)	28.66	8.97	45.50	6.40	434
Tax wedge National Accounts (%)	44.00	9.86	63.56	26.19	398
Labour tax wedge National Accounts (%)	27.09	6.05	41.72	16.86	398
Consumption taxes (%)	16.87	5.58	28.62	6.09	411
EPL	2.07	1.09	4.19	0.20	434
EPL regular contracts	2.08	1.00	5.00	0.17	434
EPL temporary contracts	2.07	1.48	5.38	0.25	434
PMR	3.81	1.29	6.00	1.05	434
Union density (%)	39.55	20.52	83.86	8.20	434
High corporatism	0.55	0.50	1.00	0.00	434
Intermediate corporatism	0.20	0.40	1.00	0.00	434
Low corporatism	0.25	0.43	1.00	0.00	434
High union coverage	0.72	0.45	1.00	0.00	434
Minimum wage (%)	45.79	10.32	64.21	28.97	217
ALMP (%)	27.94	25.15	179.13	3.28	332
Training (%)	7.44	7.85	54.57	0.28	324
Youth measures (%)	2.72	2.97	21.03	0.00	324
Subsidised employment (%)	6.19	6.11	33.93	0.07	324
PES (%)	5.57	4.50	25.46	0.00	324
Measures for disabled (%)	6.45	11.85	83.58	0.00	332
Output gap (%)	-0.89	2.53	6.30	-12.21	434
TFP shock	0.00	0.02	0.05	-0.10	419
Terms of trade shock	-0.04	0.07	0.19	-0.23	434
Interest rate shock (%)	4.61	2.25	14.12	-9.28	434
Labour demand shock	0.03	0.06	0.24	-0.14	397

Note: See text for definitions and sources.

Table A2.2. **Descriptive statistics of variables used in the unemployment regressions, 1970-2003**

Variable	Mean	Standard deviation	Maximum	Minimum	Number of observations
Unemployment rate (%)	6.45	4.14	24.04	0.09	669
Average benefit replacement rate (%)	26.85	13.34	64.94	0.00	674
Tax wedge (National Accounts, %)	43.07	9.96	63.56	21.28	522
Union density (%)	42.06	19.37	83.86	7.38	654
Collective bargaining coverage (average by country, %)	66.99	22.50	95.00	19.33	674
High corporatism	0.57	0.49	1.00	0.00	674
EPL (average by country)	2.09	1.07	3.90	0.20	674
PMR	4.27	1.27	6.00	1.05	674
ALMP (average by country, %)	29.82	26.78	157.48	7.02	674
Home ownership (average by country)	0.59	0.13	0.78	0.30	20
Output gap (%)	-0.52	2.60	16.02	-12.21	616
TFP shock	0.00	0.02	0.07	-0.10	622
Terms of trade shock	-0.02	0.07	0.19	-0.23	654
Interest rate shock (%)	2.94	3.83	14.12	-14.08	674
Labour demand shock	0.02	0.06	0.24	-0.14	597

Note: See text for definitions and sources.

Table A2.3. **Descriptive statistics of additional variables used in the employment regressions, 1982-2003**

Variable	Mean	Standard deviation	Maximum	Minimum	Number of observations
Prime-age male employment rate (%)	87.60	3.94	97.02	75.87	404
Prime-age female employment rate (%)	64.72	13.01	89.69	28.45	404
Employment rate, age 55-64 (%)	45.40	12.43	69.44	21.38	404
Male employment rate, age 55-64 (%)	57.99	12.49	85.23	32.13	404
Employment rate, age 20-24 (%)	62.77	10.77	80.44	35.46	388
Public employment rate (%)	12.74	5.50	26.72	5.20	391
Relative marginal tax rate on second earner (100/0 - 100/66)	1.14	0.41	3.48	0.44	342
Relative marginal tax rate on second earner (100/0 - 100/33)	1.05	0.44	3.07	0.30	349
Tax incentives to work part-time (%)	3.76	3.71	13.49	-3.67	322
Leave weeks	63.40	52.70	164.00	0.00	434
Family cash benefits (%)	10.04	4.82	25.46	0.00	336
Public expenditures on childcare (1995 PPP - US$)	1705.29	1642.29	8008.72	64.11	205
Implicit tax on continued work (%)	0.33	0.26	0.87	0.01	288
Standard age of retirement (years)	64.37	1.93	67.00	60.00	288
Youth minimum wage (%)	45.33	10.03	64.21	28.97	217
Female education (years)	8.68	1.95	12.29	2.96	414
Number of children per woman	0.58	0.10	1.04	0.42	434
Proportion of married women	0.71	0.08	0.89	0.46	391
Youth cohort size	0.34	0.06	0.52	0.25	403
Youth education	0.09	0.36	0.97	-1.01	414
Prime-age unemployment rate (%)	6.65	3.52	20.89	1.15	404

Note: See text for definitions and sources.

ANNEX 3

SENSITIVITY ANALYSIS

116. In this annex, several robustness tests are presented. For brevity, the sensitivity exercises are reported only for the baseline specification of the unemployment model (cf. Table 1.2, Column 1 and Table A3.1, Column 1 below). Additional robustness checks are reported in the main text of this paper and its footnotes.

117. Columns 2 and 3 of Table A3.1 show the results from estimating the baseline specification (reported in Column 1 for comparison) using Feasible Generalised Least Squares with random country effects (Column 2) and fixed effects but allowing for country-specific variances for the error term (Column 3). Random effect FGLS would be consistent and more efficient than fixed effect OLS under the assumption that country effects are independent from other co-variates, which can be tested using a battery of Hausman tests comparing the two estimators. If the difference between the parameter variance-covariance matrices of fixed and random effect specifications is positive definite, a joint Hausman test would suffice. Here, however, this matrix turns out not to be positive definite, and single parameter Hausman tests must be used to validate the hypothesis. Since test statistics are significant in the case of EPL, PMR and output gap, the hypothesis is rejected, thereby suggesting that random effects estimates are inconsistent and fixed effects models should be preferred. Although no evidence of heteroskedasticity is revealed by a White test on the baseline specification, a specific Breusch-Pagan test reveals some evidence of country-wise heteroskedasticity. Still, no important difference emerges between OLS and FGLS estimates, except that the FGLS estimate of the effect of PMR is smaller (about two-thirds of the OLS estimate) and the FGLS estimate of union density is significant (although not significantly different from the OLS one).

[Table A3.1. Sensitivity analysis: annual data 1982-2003]

118. Columns 4 to 6 present the results of varying the specification by excluding the output gap and/or the time dummies, while Column 7 present the results obtained by allowing the effect of the output gap to vary across countries. The sensitivity to the exclusion of time dummies is checked essentially for comparison with previous OECD work (*e.g.* Scarpetta, 1996, Elmeskov *et al.* 1998, OECD, 2002). The effect of the exclusion of the output gap and/or the inclusion of country-specific output gaps is shown because poolability tests (see below) perform better –*i.e.* they have a greater propensity to accept the null hypothesis of coefficient poolability– on these specifications than on the baseline equation. Still, once again, no noticeable difference emerges for the parameters of interest. In addition, parameter estimates appear to be robust to the substitution of observed shocks for the output gap (see Table 1.14, Columns 1 and 2).

119. Columns 8 to 10 consider the robustness of the baseline specification to sample variations, namely: *i*) inclusion of the omitted observations for Sweden, Finland and Germany in 1990 and 1991 — excluding the pre-shock additional fixed effect from the specification; *ii*) outright exclusion of these countries; and *iii*) exclusion of observations that appear to be particularly influential, on the basis of the

DFITS and *COVRATIO* cut-offs.[144] Two observations are in order: *i*) with the exception of EPL, whose coefficient estimate is significantly negative when the larger sample is used, estimates are robust to these exercises, despite the fact that 11 observations are singled out as outliers (Ireland in 1988 and 1998, Netherlands in 1984, New Zealand in 1987, Portugal in 1982, Spain in 1993-1995 and 2001-2003); and, *ii*) when Sweden, Finland and Germany are excluded, the resulting estimates are very similar to those of the baseline specification, which supports the choices made therein for the treatment of these countries.

120. It might also be argued that in small country samples, one single country could significantly affect the estimated parameters. An additional sensitivity analysis was thus performed on the baseline specification in order to assess the robustness of results to changes in country coverage, by eliminating one country at a time and re-estimating the baseline equation. As shown in Figure A3.1, point estimates of the impact of the tax wedge and PMR are reduced by about one-third upon elimination of Ireland and Spain, respectively. Yet, point-estimates of the four institutions that are significant in the baseline specification – benefit replacement rate, tax wedge, PMR and the high corporatism dummy variable – never become insignificant upon elimination of any country from the sample.

[Figure A3.1. Sensitivity analysis by country]

121. Another way to study the sensitivity of baseline estimates to sample variation is to re-estimate the baseline specification on random sub-samples of the main sample (see *e.g.* Baccaro and Rei, 2005). This exercise was repeated on 1 000 random draws of 90% of the original estimation sample and 1000 random draws of 50% of the sample (Table A3.2). The four institutions that are significant in the baseline specification – benefit replacement rate, tax wedge, PMR and the high corporatism dummy variable – never become insignificant upon random elimination of 10% of the sample (Panel A). Additionally, they never change sign upon random elimination of 50% of the sample (Panel B), except in very few instances in the case of PMR (in 0.4% of the draws) and corporatism (in 0.1% of the draws).

[Table A3.2. Re-estimation of the baseline specification on random subsamples]

122. One important concern to be taken into account when estimating pooled cross-country / time-series regression models imposing common cross-country coefficients is that the estimates might be affected by heterogeneity bias. The latter may arise if the impact of a given policy or institution varies across countries, due for instance to the existence of policy interactions. In such a situation, pooled regressions assuming common coefficients for all countries – i.e. making the *poolability hypothesis* – can yield inconsistent estimates of the average impact of certain explanatory variables (see *e.g.* Pesaran and Smith, 1995). Conversely, an always consistent but inefficient approach would be to estimate the baseline equation with country-specific coefficients and derive average effects from cross-country averages of estimated parameters. This suggests that a Hausman test can be implemented in order to select the appropriate estimator. The procedure is repeated for: *i*) the baseline specification; *ii*) the baseline specification without output gap; and *iii*) the baseline specification with country-specific output gaps instead of a common one. At conventional significance levels, test results support the poolability hypothesis for both the specification without output gap and that including country-specific output gaps, but not for the baseline model (Table A3.3). Yet, closer scrutiny shows that, if separate Hausman tests are implemented for institutions and output gap, the poolability hypothesis is rejected only for the latter, suggesting that the baseline specification yield consistent estimates of the average impact of each policy or institution. Moreover, the rejection of the poolability hypothesis in the case of the output gap suggests that

144 The *DFITS* statistics approximates empirically the asymptotic marginal effect on the coefficient estimates of adding one specific observation to the population. The *COVRATIO* or *covariance ratio* assesses the marginal effect of adding one specific observation on the estimated confidence ellipsoids (see Chatterjee and Hadi, 1988).

the sensitivity of unemployment to the business cycle differs considerably across countries. This provides an additional motivation for Section 1.4, which explores the policy and institutional determinants of these cross-country differences.

[Table A3.3. Hausman tests of the poolability hypothesis]

123. Reforms might yield their full employment effects only with long lags; therefore, one cannot exclude *a priori* that their impact may be difficult to be captured with annual data and a relatively short sample. This is particularly true if the evolution of institutions is not exogenous and/or exhibits some autocorrelated pattern which cannot be entirely controlled for by the output gap. For these reasons, it is important to check the robustness of parameter estimates to the estimation of the same specification on observations spanning longer time periods where residual autocorrelation does not appear to be significant. Column 1 of Table A3.4 shows parameter estimates obtained by re-estimating the baseline specification on 5-year averaged data, for which the Arellano-Bond test cannot reject the null hypothesis of no autocorrelation at conventional levels. In this case, the estimation sample consists of four periods (1982-1986, 1987-1992, 1993-1998 and 1998-2003). Although parameter estimates tend to be insignificant – which comes as no surprise given the very small sample size, point-estimates are quite similar to those of the baseline specification with annual data. Moreover their absolute value is always greater than in a specification with annual data that excludes outliers (Table A3.1, Column 10), which provides the most appropriate comparison insofar as 5-year averages smooth away the impact of annual influential observations.

[Table A3.4. Estimates on 5-year averaged data, 1982-2003]

124. Finally, the potential endogeneity of reforms is a matter of concern. In the absence of straightforward third factors that affect reforms with no direct impact on labour market performance, suitable instruments might be constructed by exploiting time-series properties through GMM estimators (see *e.g.* Arellano and Bond, 1991, Arellano and Bover, 1995, and Blundell and Bond, 1998). Insofar as the stance in any of the areas (explanatory variables) covered in the baseline specification may be influenced by current unemployment levels *via* political economy mechanisms — with perhaps the exception of the degree of corporatism, a correctly specified equation should allow at least five endogenous policy variables (benefit replacement rate, tax wedge, PMR, EPL and union density). However, GMMs tend to perform reasonably well only when the number of instruments is not too large with respect to the number of observations. For this reason, when the number of periods is large with respect to the number of countries, the same instruments are usually employed at all lags (as done in Table 1.9, Column 7, where however only ALMP expenditures and the output gap are assumed to be endogenous). However, this means that the system is almost exactly identified, which is unwelcome when the number of endogenous variables is large. For this reason, here, GMMs are applied only to 5-year averaged data. Given the lack of autocorrelation on 5-year averaged data, difference GMMs are preferred to system estimation, following Blundell and Bond (1998).

125. Baseline estimates of the parameters of interest are found to be reasonably robust to the use of GMM estimators (Table A3.4, Columns 2 and 3). The only limited exception concerns the impact of PMR, whose point estimates is however much larger when obtained through GMMs than through OLS. Nevertheless, since GMMs relies on large N for consistency and the number of countries is only 23, one cannot take these results as definitive evidence of exogeneity of policy variables.

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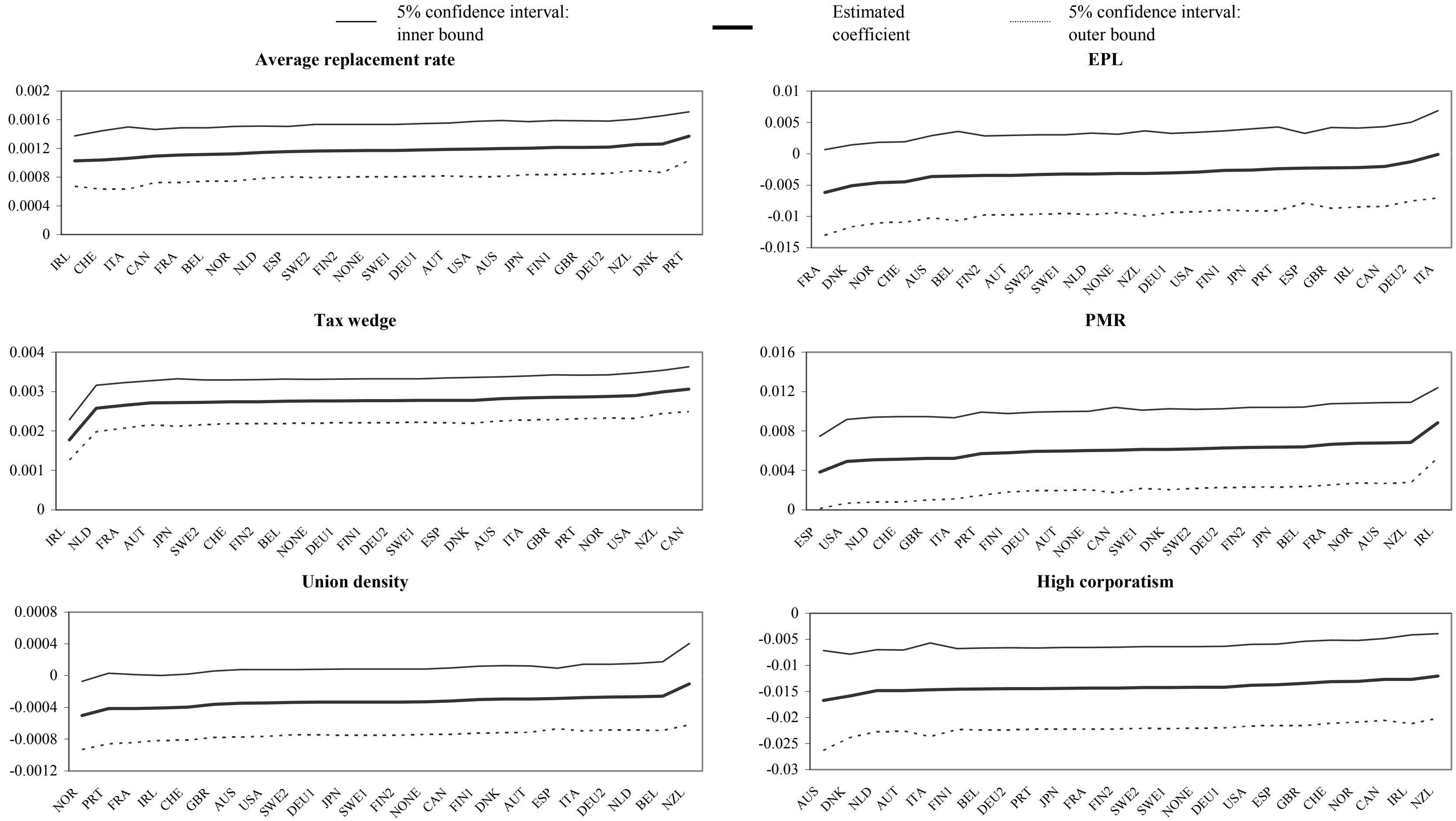
Figure A3.1. **Sensitivity analysis by country**

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. NONE identifies the baseline for the purpose of comparison.

Table A3.1. **Sensitivity analysis: annual data 1982-2003**

	1 Baseline (Excluding DEU-FIN-SWE 90-91, common OG, including fixed effects, estimated by OLS)	**2** = 1 estimated by FGLS random effects[1]	**3** = 1 estimated by FGLS fixed effects with country-wise hetero-skedasticity	**4** = 1 without OG	**5** = 1 without time dummies	**6** = 1 without time dummies and without OG	**7** = 1 with country specific OG	**8** = 1 including DEU-FIN-SWE 90-91	**9** = 1 excluding DEU-FIN-SWE	**10** = 1 excluding outliers[2]
Replacement rate	0.12	0.10	0.12	0.13	0.13	0.14	0.09	0.08	0.13	0.13
	[6.28]***	[6.24]***	[8.86]***	[5.78]***	[6.93]***	[6.34]***	[4.58]***	[4.33]***	[6.91]***	[7.49]***
Tax wedge	0.28	0.26	0.24	0.33	0.29	0.33	0.26	0.30	0.29	0.28
	[9.75]***	[10.98]***	[11.18]***	[9.20]***	[10.14]***	[9.17]***	[7.94]***	[10.69]***	[9.56]***	[9.60]***
Union density	-0.03	-0.05	-0.03	0.01	-0.04	0.02	-0.01	-0.01	-0.03	-0.03
	[1.57]	[3.10]***	[2.01]**	[0.22]	[1.77]*	[0.76]	[0.59]	[0.51]	[1.50]	[1.68]*
EPL	-0.31	-0.66	0.15	-0.38	-0.23	-0.11	0.02	-1.41	-0.04	-0.16
	[0.98]	[2.07]**	[0.51]	[0.95]	[0.71]	[0.27]	[0.05]	[4.09]***	[0.11]	[0.59]
PMR	0.60	0.69	0.41	0.69	0.74	0.88	0.59	0.51	0.73	0.44
	[2.98]***	[3.45]***	[2.61]***	[2.65]***	[6.04]***	[5.78]***	[2.74]***	[2.36]**	[5.81]***	[2.49]**
High corporatism	-1.42	-1.43	-1.25	-2.00	-1.44	-2.27	-1.42	-1.53	-1.47	-1.37
	[3.57]***	[4.13]***	[3.71]***	[4.22]***	[3.54]***	[4.69]***	[3.79]***	[3.86]***	[3.54]***	[3.58]***
Intermediate corporatism		-1.23								
		[0.72]								
Output gap	-0.48	-0.49	-0.47		-0.46			-0.54	-0.47	-0.47
	[14.00]***	[14.37]***	[18.86]***		[16.55]***			[14.00]***	[13.54]***	[14.51]***
Country effects	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Country effects*output gap	no	no	no	no	no	no	yes	no	no	no
Time dummies	yes	yes	yes	yes	no	no	yes	yes	yes	yes
Observations	434	434	434	434	434	434	434	440	374	423
R-squared	0.98			0.97	0.98	0.97	0.98	0.97	0.98	0.98

Absolute value of t statistics in brackets. Robust t statistics except for FGLS estimates.

* significant at 10%; ** significant at 5%; *** significant at 1%.

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. The sample is adjusted by excluding influential observations identified by the DFITS cut-off combined with the COVRATIO cut-off.

Table A3.2. **Re-estimation of the baseline specification on random sub samples**

Panel A. **1000 draws of 90% of the sample: value of t-statistics**

	Number of observations	Mean	Standard deviation	Minimum	Maximum
Replacement rate	1000	5.95	0.44	4.93	9.21
Tax wedge	1000	9.18	0.58	6.74	11.37
Union density	1000	-1.49	0.36	-3.04	-0.56
EPL	1000	-0.89	0.32	-2.09	0.3
PMR	1000	2.79	0.32	1.74	3.87
High corporatism	1000	-3.36	0.34	-4.49	-2.2

Panel B. **1000 draws of 50% of the sample: value of estimated coefficients**

	Number of observations	Mean	Standard deviation	Minimum	Maximum
Replacement rate	1000	0.119	0.022	0.045	0.19
Tax wedge	1000	0.277	0.035	0.156	0.379
Union density	1000	0.033	0.024	-0.102	0.037
EPL	1000	-0.296	0.393	-1.39	0.994
PMR[1]	1000	0.618	0.234	-0.134	1.334
High corporatism[2]	1000	-1.435	0.448	-2.886	0.02

1. A negative coefficient estimate for PMR is estimated 4 times out of 1000 draws.
2. A positive coefficient estimate for High corporatism is estimated once out of 1000 draws.

Table A3.3. **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. A significant test statistic implies rejection of the poolability hypothesis.

Table A3.4. **Estimates on 5-year averaged data, 1982-2003**

	1	2	3
	Baseline[1]	= 1 estimated by difference GMMs with endogenous variables lagged two periods as instruments[2,3]	= 1 estimated by difference GMMs with all possible instruments[3,4]
Replacement rate	0.13 [1.65]*	0.15 [1.81]*	0.21 [2.15]**
Tax wedge	0.33 [2.81]***	0.35 [4.89]***	0.30 [3.72]***
Union density	-0.04 [0.70]	-0.05 [0.60]	0.04 [0.43]
EPL	-0.49 [0.33]	-1.90 [0.76]	-1.11 [0.58]
PMR	0.46 [0.52]	1.79 [1.42]	1.08 [0.91]
High corporatism	-1.42 [1.14]	-2.99 [2.21]**	-4.97 [2.53]**
Output gap	-0.36 [2.44]**		
Hansen test (P-value)		1.00	1.00
Arellano-Bond AR1 tes	-1.85*	-1.80*	-3.11***
Arellano-Bond AR2 test		-1.54	-0.93
Country dummies	yes		
Time dummies	yes	yes	yes
Observations	80	80	80
R-squared	0.97		

Absolute value of t statistics in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

1. OLS estimates. Arellano-Bond AR1 statistic is computed on the residual in levels and tests the presence of autocorrelation at order 1. It is normally distributed under the null.
2. One-step difference GMM robust estimates. The error term is modeled as an ARMA process with an AR(1) component. All institutions except High corporatism are treated as endogenous variables. The common factor restriction is not imposed. Only long-run effects are presented. Endogenous variables dated t-2 are used as instruments in the difference equation.
3. The Hansen-Sargan statistic provides a test of overidentifying restrictions. The model is rejected if the statistic is significant. Arellano-Bond statistics are computed on the first difference of the residuals at order 1 and 2 and are normally distributed under the null. The model is rejected if evidence of autocorrelation is found at order 2.
4. One-step difference GMM robust estimates. The error term is modeled as an ARMA process with an AR(1) component. All institutions except High corporatism are treated as endogenous variables. The common factor restriction is not imposed. Only long-run effects are presented. Endogenous variables dated t-2 and earlier (when possible) are used as instruments in the difference equation.

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