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IS DEREGULATION NECESSARY? THE EFFECTS OF EMPLOYMENT PROTECTION ON UNEMPLOYMENT

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ABSTRACT

Is Deregulation Necessary? The Effects of Employment Protection on Unemployment

Using new data, the paper examines the effect of employment protection legislation (EPL) on aggregate and youth unemployment in advanced OECD economies and in Central and Eastern Europe during 1980-2009. The analysis assesses both the direct and indirect effects of EPL on levels of unemployment, as well as the short-term and long-term effects of changes in EPL on changes in unemployment. The results offer no clear support for the argument that EPL is a cause of either aggregate or youth unemployment. While EPL reaches statistical significance at conventional levels in some models, the results are sensitive to small changes in the sample or the use of alternative estimators. The only finding that appears robust concerns the interaction between EPL and the tax wedge, which suggests some scope for reform complementarity in tackling youth labour market problems. On the whole, the analysis suggests that government efforts to tackle unemployment by deregulating EPL alone may well be unwarranted.

Keywords: employment protection legislation, aggregate unemployment, youth unemployment

JEL Classification: J4, J48, P16, P52

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Introduction

Employment protection legislation (EPL) – a set of rules that govern hiring and firing – is frequently mentioned as a cause of unemployment. In this view EPL undermines market flexibility by limiting the freedom of employers to quickly adjust the size of their workforce to market fluctuations. Employers are therefore reluctant to hire new workers even during good economic times because they are concerned that they would not be able to easily dismiss them in bad times. Theoretically, however, the effect of EPL is ambiguous because strict hiring and firing rules may simultaneously reduce the rate of job creation and increase the rate of job retention. To complicate things further, strict EPL can foster investments in human capital and on the job training, thus enhancing productivity and overall performance.

Despite this theoretical ambiguity, much empirical research that has informed policy recommendations and government's choices draws a link between strict EPL and poor labour market performance. While the orthodox view of labour market regulations espoused by the OECD Jobs Study (1994) and related literature (Scarpetta, 1996; Siebert, 1997) has been subsequently toned down (OECD, 2004; Bassanini & Duval, 2006) and challenged by recent research (Baccaro & Rei, 2007; Baker et al. 2005), most of the literature agrees that even if EPL is not responsible for aggregate unemployment, it damages the prospects of particular market segments, especially the youth (Addison & Taxeira, 2003; Bertola et al., 2002; Botero et al, 2004; Esping-Andersen, 2000; European Commission, 2006; OECD, 2004). The fact that Mediterranean countries that generally have strict EPL also have higher rates of youth unemployment would seem to support this argument. However, this link is not always evident. For example, the United Kingdom has weak EPL, but substantially higher youth unemployment than Germany that has traditionally had stricter employment laws.

This paper re-examines the effects of EPL on aggregate and youth unemployment by relying on newly constructed data that includes the standard measure of EPL strictness, as developed by the OECD, but with actual annual figures (Allard, 2010; Avdagic, 2012), rather than the figures interpolated from a few data points as previously done. The aim is to reassess a number of standard models that have supported the deregulation argument, as well as to improve on this literature by investigating more thoroughly the effects of EPL when disaggregated on its core components that govern permanent and temporary contracts. The data include not only the usual group of advanced OECD economies, but also ten new EU member states from Central and Eastern Europe (CEE), covering together the period 1980-2009. To ensure the robustness of results, the analysis employs several different models and estimators, and examines if individual countries have disproportionate influence on coefficient estimates.

The findings of the paper can be summarized as follows: There is no clear link between EPL and aggregate unemployment. Various models and techniques used in this analysis do not support the argument that EPL is positively associated with unemployment, or that a reduction in the strictness of employment protection yields lower unemployment. Perhaps more surprising is the finding that there is no direct link between EPL and youth unemployment either. While the analysis detects a positive association between the regulations for permanent contracts and youth unemployment, as well as the contrasting effects of EPL in countries with different education systems, none of these results are sufficiently robust. The only finding that appears robust concerns the interaction between EPL and the tax wedge, which suggests some scope for reform complementarity. On the whole, the analysis suggests that EPL is not a key culprit of unemployment, and thus government efforts to tackle unemployment by liberalizing employment laws alone may well be futile.

The paper is organized in three sections. Section 1 discusses the main theoretical arguments and previous empirical findings about the effects of EPL on unemployment outcomes. Section 2 starts with a brief summary of EPL reforms during the last two decades and offers a first glance into their effectiveness. It then proceeds with a more thorough investigation of the role of EPL in unemployment. To this end, the paper examines both the direct and indirect impact of EPL on levels of unemployment, as well as the short-term and long-term effects of EPL reforms on changes in unemployment. Section 3 concludes by discussing the main findings.

1. Employment protection and labour market performance: theory and evidence

Employment protection may be described as restrictions on the ability of employers to hire and fire labour. In the standard competitive model any restriction on the freedom of contract is assumed to increase resource costs. It is therefore not surprising that much of the literature emphasizes the unemployment increasing effects of employment protection. In this line of reasoning, strict job security provisions increase overall employment costs, provide an incentive for 'insiders' to press for higher wages, reduce the speed of adjustment to exogenous shocks, inhibit the reallocation of labour from declining to expanding sectors, and generally dampen job creation (Addison & Teixeira, 2001). The core of this argument is related to the inter-temporal movement of labour. When employment protection is high, employers facing rising demand will be reluctant to hire new workers for fear of not being able to fire them cheaply and easily when the need arises. However, the opposite effect is also expected: high severance pay or procedural costs will influence employers to make fewer layoffs during downturns than they would have done in deregulated labour markets. Theory makes it clear that strict employment protection leads to a reduction of labour fluctuation over the cycle, and thus lengthening of employment and

unemployment spells, but it is unclear if this should lead to a reduction (increase) of overall employment (unemployment). The overall effect on unemployment, as Bertola (1992) has argued, is theoretically ambiguous and it may depend on issues such as the functional form of labour demand functions, the discount rate, labour turnover, and wage flexibility. Moreover, the effect of EPL is not necessarily adverse: strong job protection encourages investments in training and may enhance overall productivity performance (Estevez-Abe, et al., 2001).

What are the findings of the empirical literature about the effects of EPL on aggregate unemployment? Although the influential OECD account (OECD, 1994) about the need for deregulation has been toned down in recent years (see, e.g. OECD, 2004), a number of studies report an adverse effect of EPL on aggregate unemployment (e.g. Blanchard & Wolfers, 2000; Elmeskov et al., 1998, Nickel et al, 2005; Scarpetta, 1996; Siebert, 1997). Some recent research, however, challenges these findings and demonstrates that EPL loses its significance, or even changes its sign, with small changes in data, the sample, or the estimation procedures (Avdagic & Salardi, 2013; Baccaro & Rei, 2007; Baker et al. 2005; Howell et al., 2007; Vergeer and Kleinknecht, 2012).

Theory is equally ambiguous about the effect of EPL on youth unemployment (Noelke, 2011). Since strong dismissals protection increases the costs of a bad hire, employers are likely to be more selective and favour experienced workers with good track record. As a consequence, young people are facing higher unemployment risks. Strict employment protection is therefore associated with both longer transitions from school into first employment and increasing youth unemployment rates. However, EPL can also have beneficial consequences: strict employment protection should increase the duration of first employment and enhance the possibilities for human capital formation, which in turn should reduce youth unemployment risks.

Considering separately employment regulations for permanent and temporary employees offers further predictions about the impact of EPL on aggregate, and especially youth unemployment. When job security provisions for permanent workers are strict, more employers are expected to rely on the less costly temporary contracts. The increasing reliance on temporary jobs increases the rate of hiring, but also job loss rates due to the shorter length of employment spells generated by these contracts. The upshot of this is that deregulation of temporary contracts should affect labour market flows, but its impact on unemployment rates is again unclear and can be determined only empirically. Despite this theoretical ambiguity, two views have emerged in the literature: the 'integration view' emphasizes the beneficial effects of temporary contracts, while the 'segmentation view' stresses their adverse effects (see also Giesecke & Groß, 2003; Noelke, 2011). According to the 'integration view', deregulation of temporary contracts facilitates easier access to the labour market and the transition into first employment, thus lowering unemployment, and especially youth unemployment (OECD, 1994). Another mechanism through which temporary contracts may lower unemployment is related to wage flexibility. Temporary, particularly young, workers tend to be paid less than permanent workers. Such wage penalties may enhance the unemployment-reducing effects of lower hiring and firing costs that characterize these contracts. In contrast, the 'segmentation view' emphasizes the increasing divisions between permanent and temporary workers, with the former enjoying significantly more job stability than the latter. In this view, deregulation of temporary contracts offers an incentive for employers to increasingly use these contracts as a substitute for permanent employment. This should be especially pronounced in countries where the regulations for permanent contracts are particularly strict. Employers in this scenario are reluctant to transform temporary to permanent contracts even for those workers who seem to be a good match, because

the low costs of temporary contracts may make it worthwhile to take a chance with a new worker (Blanchard and Landier, 2002). Young people in this scenario, therefore, have a lower chance of securing a permanent job. Given the repeated episodes of temporary jobs and unemployment, turnover in entry-level positions may increase disproportionately, leading to higher unemployment (Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002). Furthermore, since temporary workers are less likely to receive training (Booth et al, 2002), their skills may deteriorate, thus increasing their unemployment risks.

While the empirical analyses of the effects of EPL on aggregate unemployment produce more ambiguous results, most of the empirical literature agrees that EPL has adverse effects on youth labour market performance (Addison & Texeira, 2003; Esping-Andersen, 2000; European Commission, 2006; OECD, 1994, 2004). Some authors qualify this position by showing that the impact of EPL on youth unemployment may be different in different countries, depending on other institutions, such as the nature of the education system (see below, Breen, 2005). A few recent analyses, however, challenge the conventional view and demonstrate that EPL does not cause high youth unemployment, and that deregulation of temporary employment does not improve labour market performance (Kahn, 2007; Noelke, 2011). The remainder of this paper reassesses this evidence based on new data that covers both advanced and new market economies.

2. Exploring the link between EPL and unemployment

Despite a common assumption that EPL reforms are rare, the last couple of decades have witnessed a large number of reforms. In several countries liberal reforms came onto the agenda already in the 1980s. During the 1990s and 2000s, EPL reforms picked up speed as many policy

makers adopted the discourse about the need for labour market deregulation. Systematic qualitative data on EPL reforms in Europe (FRdB-IZA, 2011; Avdagic, 2012) show that there were a total of 200 reforms during 1990-2007. A majority of the reforms were of liberal character, and around a quarter of these were structural reforms entailing considerable changes on key issues that affect all workers, such as severance payment, the notice period, or the obligation to consult unions prior to dismissals. While this data is too crude to allow a proper assessment of EPL effects, it provides a quick first glance into the relationship between EPL and unemployment. A fixed-effect logistic regression (not shown here) of a simple model in which a decline of unemployment depends on the occurrence of EPL reforms and GDP growth as the only control shows that neither structural nor marginal liberal reforms are associated with a decline in aggregate and youth unemployment.[1] Clearly, this model permits at best partial conclusions since the data on EPL reforms capture only the occurrence of a particular type of reform, but not more fine-grained differences between reforms, such as the difference in the extent of liberalization between reforms that belong to the same category. Moreover, the model does not consider the effects of other labour market institutions and institutional interactions, and it does not include non-EU OECD countries that are commonly included in analyses of unemployment. The analysis below takes these concerns into consideration and presents a more thorough empirical investigation of the impact of employment protection on aggregate and youth unemployment by using a new, annual measure of EPL strictness.

The analysis is divided in three parts. Following most of the literature that supports institutional deregulation, the first part focuses on the level of unemployment and estimates

[1] Since one could expect that the beneficial effects of liberalization take some time to materialize, three versions of this model were estimated where reforms were entered in 1-, 2- and 3-year lags. None of them suggest that EPL liberalization leads to a decline in unemployment.

dynamic fixed effects models with annual data. The focus is on the effects of EPL, its individual components that govern regular and temporary contracts, as well as interactions between EPL and other labour market institutions. The second part extends this analysis and employs a non-linear model to test if employment protection affects unemployment via interactions with macroeconomic shocks. Finally, the third part focuses on changes in unemployment and uses an error correction model to examine the short-term and long-term effects of changes in EPL.

Data

Data used in the analysis cover 31 countries including all current EU member states (apart from Cyprus, Malta and Luxembourg), Norway, Switzerland, the United States, Canada, Australia, New Zealand and Japan during 1980-2009. The series for CEE countries are somewhat shorter, starting roughly at the beginning of their democratic transitions.[2] This is a significantly larger sample than commonly used in the literature. While Feldmann (2009) and Bernal-Verdugo et al. (2012) include larger samples of 73 and 97 countries respectively, their time series are rather short. The former focuses only on three years, while the latter uses series that vary from three to twelve years. An important contribution of the analysis presented here is that it includes the longest and previously unavailable series that measure the strictness of EPL in CEE countries. A further contribution is that the EPL index used in the analysis captures the strictness of employment protection on a yearly basis. This is in contrast to the series provided by the OECD, which are interpolated from a few data points. As such, this data reflects more accurately the differences in the timing and the extent of EPL reforms. The EPL index for CEE countries was constructed by the author following the methodology used by the OECD (Avdagic, 2012a). This

[2] For reasons of comparability, the analysis excludes the first three years of post-communist economic transformation when these countries experienced particularly profound economic changes and macroeconimic shocks.

index measures the strictness of employment protection legislation on a scale ranging from 0 to 6, where higher scores imply stricter regulation. The calculations of the index are based on a combination of standardized questionnaires completed by teams of national experts, a review of national legislation, the ILO's Natlex database, and secondary literature. The analysis combines these data with Allard's (2010) EPL index for advanced economies, which also captures annual changes in legislation and is based on the same methodology.

The series on the unemployment benefit replacement rates for CEE countries is also newly constructed based on the scheme used by the OECD. These data capture the gross replacement rates in the first year of unemployment across two levels of earnings (67% and 100% of average wage). The calculations for CEE countries combine information on benefits provided by SSA/ISSA reports on 'Social Security Programs Throughout the World' with data on wages from the ILO's Travail database and the WIIW statistical handbooks on Eastern Europe. In addition, the analysis uses new data on the tax wedge provided by Labartino (2010). This database includes significantly longer and more complete series for this sample of countries than the OECD and Eurostat data. Data on union density and wage coordination are taken from Visser (2009). Data on macroeconomic controls come from the International Monetary Fund's International Financial Statistics (GDP), the World Bank's World Development Indicators (real interest rate), OECD National Accounts data files (inflation), and the European Commission's AMECO database (terms of trade).

Does EPL affect levels of unemployment?

This section estimates a dynamic model of unemployment that has been used widely in the literature (Nickel et al., 2001, 2005; see also Layard et al, 1991; IMF, 2003; Baccaro & Rei,

2007). In this model the unemployment rate depends on a set of labour market institutions and macroeconomic controls. The former determine the equilibrium level of unemployment, while the latter account for short-term deviations from the equilibrium level. The model has the following form:

$$u_{i,t} = \beta_0 + \beta_1 u_{i,t-1} + \sum_j \gamma_j x_{j,it} + \sum_k \eta_k z_{k,it} + \sum_n \delta_n v_{n,it} + \alpha_i + \lambda_t + \varepsilon_{i,t} \quad [1]$$

where $u_{i,t}$ represents the unemployment rate (aggregate or youth) in country *i* at time *t*, $u_{i,t-1}$ is the lagged unemployment rate, $x_{j,it}$ are *j* institutional variables, $z_{k,it}$ represent *k* macroeconomic controls, $v_{n,it}$ are *n* interactions between labour market institutions, and $\varepsilon_{i,t}$ is the stochastic residual. As indicated by F statistic of Wald test, the model also includes country dummies, α_i, which account for unmeasurable time-invariant country-specific characteristics, and year dummies, λ_t, which denote time-varying shocks affecting all countries. The lagged dependent variable is included among the predictors to capture the persistence of unemployment and hysteresis effects (Nickell et al., 2005).

Apart from employment protection (EP), the vector of institutional variables includes unemployment benefit replacement rate (BRR), tax wedge (TW), union density (UD) and wage bargaining coordination (BC). Generous *unemployment benefits* are commonly thought to increase aggregate and youth unemployment because they indicate a high reservation wage, which makes unemployed individuals both more reluctant to search for jobs and to accept available jobs. In addition, generous benefits may contribute to unemployment by making unions

more resolute in pushing for higher wages. The *tax wedge*, the difference between the labour cost to employers and the take-home wage for employees, is generally expected to influence labour market performance negatively by reducing the demand for labour, and in particular for young workers. However, theoretically the distribution of taxes between employers and labour determines the actual impact of the tax wedge. If employees carry most of the tax burden, this variable alone is not likely to reduce labour demand. At the same time, the impact on labour supply is indeterminate since a low take-home pay may either reduce workers' incentive to accept jobs and keep the existing ones, or it may motivate them to seek additional jobs. *Union density* indicates union bargaining power. In the orthodox view, unions tend to raise wages, and therefore a high share of workers belonging to unions is expected to increase unemployment. Strong unions are also associated with compressed wage structures, which may reduce the prospects for employment of youth and low-skill workers. Union density may also have an indirect impact and affect unemployment via job security regulations. Previous research has linked strong unions with the extension or maintenance of employment protection (Botero et al. 2004). More recently, however, unions have been associated with the deregulation of temporary employment. Giving the increasing pressures for labour market flexibilization, unions have an incentive to protect the interests of permanents workers as their core constituents, and shift the burden of flexibilization onto the shoulders of temporary workers. This strategy is at the root of labour market dualization, a trend marked by the growing divisions between the well-protected insiders and vulnerable outsiders, many of whom are young workers (King and Rueda 2008; Palier and Thelen 2010). Since dualization makes dismissals of temporary workers less costly, young workers are likely to be disproportionately affected by cyclical adjustments. Given this, one could expect union density to be positively related with youth unemployment, while its impact on aggregate

unemployment is more ambiguous. The effect of *wage bargaining coordination* is generally considered to be beneficial for labour market performance. Because unions in coordinated systems internalize the externalities of their wage policies, it is expected that real wages, and thus unemployment, will be lower than in systems characterized by uncoordinated bargaining (Soskice, 1990).

The macroeconomic controls include GDP growth, the change in inflation (CPI), the terms of trade, and the real interest rate. *GDP growth* and the *change in inflation* capture the influence of economic cycles. A fall in output is expected to be associated with higher unemployment. Following the logic of the Phillips curve, change in inflation should be negatively related to unemployment in the short-run. The *terms of trade* variable should have a negative relationship with unemployment. A deterioration of the terms of trade requires a downward adjustment of real wages. If wages do not respond accordingly, unemployment is likely to increase. Finally, the *real interest rate* affects capital accumulation and can cause shifts in labour demand. This variable is expected to be positively associated with unemployment, because an increase in real interest rates is likely to reduce aggregate demand, thereby generating higher unemployment rates (Baker et al., 2005). Models of youth unemployment also include the ratio of youth to adult (25-54 years) population as an additional control.

Estimates reported in this part are obtained through ordinary least squares with panel corrected standard errors (OLS-PCSE) (Beck and Katz, 1995). This is the preferred estimator because it controls for common properties of this type of data, including panel heteroskedasticity and contemporaneous correlation of the error terms. Nonetheless, the same models were tested by using a panel weighted least squares estimator (PWLS), which is the most commonly used procedure in the literature that supports the deregulatory view. This model assumes country-

specific heteroskedasticity and employs a Prais-Winsten transformation to address a first order (AR1) autoregressive structure in the errors (a common estimated rho). Compared to the Parks estimator, which produces severely underestimated standard errors in analyses where T is not significantly larger than N, this feasible generalized least squares (FGLS) estimator has better properties (Beck and Katz, 1996). However, this estimator is not designed to correct for contemporaneously correlated errors, which characterize this data. Given this, one has to keep in mind that PWLS may still suffer from overoptimistic errors. These drawbacks notwithstanding, PWLS provides a useful check of our results. If these models yield similar results to OLS-PCSE, this should increase our trust in the credibility of the results. For reasons of space, Tables 1 and 2 report only the OLS-PCSE estimates, while the text indicates if PWLS models offer considerably different conclusions. Given the dynamic nature of these models and thus the potential concerns about the Nickell bias,[3] OLS estimates were also checked against the least squares dummy variable model (LSDV) with the Kiviet correction (Kiviet, 1995). [4] The estimates were not substantially different, which confirms the conclusion from Beck and Katz (2011) whose simulations show that in relatively long panels (15 and above) the Nickell bias is negligible. In addition to these stability checks, the analysis relies on a Jackknife procedure to assess the influence of particular countries on the coefficient estimates.

Table 1 shows the OLS-PCSE estimates of Model 1. The first four columns report the results of the basic models that apart from EPL include the labour market institutions and macroeconomic controls discussed above. Columns 1 and 2 examine the effect of EPL on

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[3] The Nickell bias (1981) refers to situations in which the inclusion of the lagged dependent variable makes the fixed effect estimator biased due to the correlation between the demeaned lagged dependent variable and the error term.

[4] The LSDV models with the Kiviet correction were run using a bootstrap method with 50 and 500 observations. Estimates are obtainable upon request.

aggregate and youth unemployment respectively, while columns 3 and 4 disaggregate EPL into regulations for temporary and regular employment contracts. The coefficients of the lagged dependent variable are high, indicating considerable persistence of unemployment. While the null of a unit root cannot be rejected for some variables, the Augmented Dickey Fuller and Philips-Perron tests (Maddala and Wu, 1999) reject the null of no cointegration.[5] Although EPL coefficients are positively signed,[6] the results suggest that job security regulations as a whole have no statistically significant effect on either aggregate or youth unemployment. When EPL is disaggregated, however, strict regulations governing regular employment contracts seem to be associated with higher youth unemployment. However, this result is not robust: it does not hold in the PWLS model, and more importantly the significance of this coefficient hinges entirely on the presence of Spain in the sample. It should be noted that the results are not different in reduced-form models that have GDP growth as the only control, including the models that feature EPL as the only institutional variable.

Columns 5 and 6 examine the interaction between the employment regulations for standard and temporary contracts. This interaction term is included because the 'integration' and 'segregation' views (discussed above) suggest that the effects of deregulation of temporary contracts depend on the strictness of permanent contracts. According to the 'integration' scenario, the unemployment reducing effect of deregulation of temporary contracts should be enhanced when regulations for permanent contracts are strict. In contrast, the 'segmentation' scenario posits that strict regulations for permanent contracts in combination with liberal rules for temporary employment may even increase unemployment. The interaction coefficient is statistically

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[5] Unit root tests were run with one lag and two lags, with and without drift, with and without trend, and with and without demean option.

[6] This is not the case in PWLS models where the coefficients on EPL are negatively signed, albeit still insignificant.

significant (albeit only at the 10 percent level) only in the model with youth unemployment. The estimates suggest that when the regulations for standard contracts are strict, deregulating the rules for temporary contracts reduces youth unemployment. This seems to confirm the 'integration' scenario, however, the result is again not robust and survives neither the Jackknife procedure nor the use of the alternative estimators.[7]

The next three columns report the estimates of the models that examine if the impact of EPL may be adverse when associated with another institutional rigidity.[8] As such, these models indicate possible complementarities across labour market reforms (see Bassanini and Duval, 2006). A negative and significant interaction coefficient would suggest reform complementarity.[9] Columns 7 and 8 test the impact of the interactions between EPL on the one hand, and the tax wedge and unemployment benefits on the other. In theory, one channel through which these interactions work is the interdependence of the search intensities of workers and employers. High labour taxes may discourage vacancy posting because they reduce either the demand for or supply of labour. By increasing the costs of hiring and firing, strict employment protection also discourages vacancy posting. Consequently, the search intensity of workers may be reduced because the likelihood of finding a job is smaller. The adverse effects of these two institutions therefore may amplify each other. The interaction between employment protection and unemployment benefits follows a similar logic. Strict employment protection may reduce

[7] The interaction coefficient is no longer significant if any of the following countries is dropped from the analysis: Australia, Belgium, Canada, Finland, Greece, Japan, the Netherlands, Portugal, Spain or Hungary.

[8] The interactions are specified as deviations from cross-country and over-time sample means. Using such formulation, the coefficients of these institutions in levels can be interpreted as the coefficients of a country that has the average level of a given institution (Nunziata, 2002, p. 9).

[9] For a detailed elaboration see Bassanini and Duval (1996: 21).

vacancy postings, and this effect may be augmented by generous benefits.[10] The latter institution reduces workers' incentives to look for jobs, which consequently may also discourage vacancy posting (IMF, 2003). Column 9 examines an interaction with the minimum wage, which should impact primarily youth unemployment. One could hypothesize that strict EPL does not have an adverse effect on employers' willingness to employ young workers as long as wages for youth are sufficiently low. In other words, strict EPL may be associated with high youth unemployment only in contexts where the minimum wage is high. As evident, the only interaction that is statistically significant is the interaction with the tax wedge in the model with youth unemployment (column 6). This result appears to be robust to the use of the alternative estimators and a Jackknife analysis. The results are also unaffected if the two interactions in models 7 and 8 are tested separately.

[Table 1 about here]

Overall, Table 1 fails to provide convincing evidence about the impact of job security regulations on unemployment. While the results indicate that these regulations may have an adverse effect on youth unemployment when combined with high labour taxes, evidence about the independent impact of EPL is scarce and seems to be highly context dependent, as indicated by the Jackknife procedures. One potential objection to these results is that they may reflect endogeneity issues and that the causality may go from unemployment to EPL rather than vice versa. Indeed, it is reasonable to posit that high unemployment may lead policy makers to reform employment protection. However, Granger causality tests, performed on models that include two lags of both unemployment and EPL, show no evidence of reverse causality (the F-statistic of the joint significance of the lags of aggregate unemployment is 1.54, p=0.21, and of youth

[10] If employment protection enables a high investment in skills, and consequently higher productivity gains, these relationships may not hold.

unemployment 0.26, p=0.77). Additional causality tests on models that also include lags of the other labour market institutions confirm the conclusion that causation does not run from unemployment to EPL. Further evidence comes from Granger causality models fitted to each of the countries. The mean F-statistic is equal to 2.68 for the effect of EPL on aggregate unemployment, and 1.39 for the effects of unemployment on EPL, again suggesting that causation runs from EPL to unemployment rather than vice versa. There is also no clear evidence of reverse causality with respect to the link between EPL and youth unemployment, with the mean F-statistic being 2.38 for the effects of EPL and 2.08 for the effects of youth unemployment. Of course, endogeneity problems may still be present if omitted variables influence simultaneously EPL and unemployment. However, difference GMM models (Arellano and Bond, 1991), where EPL and other institutional variables dated *t - 2* and earlier are used as instruments, generate results similar to the baseline point estimates presented in Table 1.[11]

Table 2 examines in more detail the effects of EPL on youth unemployment by dividing the countries into two groups according to the role of the educational system in signalling the quality and suitability of job seekers for specific jobs. Previous research has shown that vocational training institutions facilitate labour market transitions from school to employment, thus reducing youth unemployment (Breen, 2005; Gangl, 2003; Wolbers, 2007). The crucial distinction between the countries concerns the extent to which educational systems enhance specific (as opposed to general) skills, and the extent to which the education system provides direct links to employers, such as via firm-based training schemes. Education systems that

[11] Since this estimator is designed for small T panels, I follow Bassanini and Duval (2009) and estimate these models on 5-year averaged data. Two versions of these models were estimated. In the first version, all institutional variables were treated as endogenous. In the second version, apart from EPL only those institutional variables that were significant in the baseline models were treated as endogenous. Estimates are obtainable upon request.

emphasize specific skills and links with employers provide signals about the quality and likely productivity of particular workers for specific jobs. In this way, as Breen (2005) has argued, educational signalling can mediate the impact of employment protection on youth unemployment. In line with the varieties of capitalism literature (Estevez-Abe et al., 2001), Breen (2005) maintains that there are two paths to good youth labour market performance. When EPL is strict, employers are reluctant to hire new workers because they cannot fire them easily should the need arise. Thus, deregulated labour markets, in which both hiring and firing is easy, should have lower levels of youth unemployment. However, if the education system sends clear signals about the quality of job seekers, employers will be less concerned about dismissal regulations since they can be more confident that the person they are hiring is well suited for the job. In other words, high signalling should mitigate the adverse effects of strict EPL.

One way to assess this hypothesis would be to incorporate into the models the interaction between EPL and the proportion of pupils in upper secondary-level education who follow vocational tracks. There are, however, two drawbacks to using this indicator. First, while the data is readily available for OECD countries, there are no sufficiently long and comparable series for all CEE countries. The more important drawback, however, is that this indicator does not capture the actual nature of vocational education in different contexts (Breen, 2005). While countries where educational signalling is high (e.g. Austria and Germany) tend to have relatively high proportions of students enrolled in vocational education, high participation in vocational tracks does not guarantee the kind of educational signalling that employers may find useful. For example, countries such as Italy and the Czech Republic have a high proportion of students involved in vocational training (indeed, higher than in Germany and Austria respectively), but vocational schools have weak links with employers and are often inadequate in teaching the

specific skills required by employers. Thus using the participation rates in vocational education as a proxy for educational signalling may lead to erroneous conclusions. To avoid this problem the analysis presented here relies on secondary literature to divide the countries into two groups according to the extent of educational signalling. Countries with high educational signalling include: Austria, Belgium, Denmark, Finland, Germany, Japan, the Netherlands, Norway, Slovenia, Sweden and Switzerland. Countries with low educational signalling include all English speaking countries, France, Greece, Italy, Portugal, Spain, and all CEE countries apart from Slovenia.

[Table 2 about here]

Table 2 reports the results of this analysis.[12] Column 1 and 2 show that the effect of employment protection is significant in both education settings, but while EPL increases youth unemployment in countries with low educational signalling, it is associated with lower unemployment when educational signalling is high. If we disaggregate EPL on regulations for standard and temporary contracts, it becomes clear that the adverse effect of EPL in countries with low educational signalling is primarily due to the regulations for standard contracts, while the beneficial effect of EPL in countries with high educational signalling is related to the regulations of temporary contracts. On the whole, these results seem to confirm the expectations of scholars emphasizing the differential effects of EPL (Breen, 2005) and the varieties of capitalism literature more broadly. The results are robust to the use of the alternative estimators and the EPL coefficients keep their sign in the Jackknife analysis, however, they lose their

[12] Sweden is not an entirely clear case because its vocational system avoids specialization and enhances transferable vocational skills. But although its vocational system is school-based, rather than workplace-based, and firms are not as involved in skill formation as they are in Germany, Sweden is not a general skills system reminiscent of liberal market economies (Busemeyer, 2009). For this reason, Sweden is included in the group with high educational signalling. It should be noted that shifting Sweden to the group with low educational signalling does not affect the results appreciably (estimates obtainable upon request).

significance if Greece or the UK is omitted from the first group, and Belgium or Germany from the second.

Does EPL affect unemployment through interactions with shocks?

The previous analysis did not find much evidence about the adverse effect of EPL on unemployment. However, it is possible that EPL plays a more indirect role and that it affects unemployment by amplifying the effects of economic shocks. Blanchard and Wolfers (2000: C17) have argued that labour market institutions may affect both the impact of shocks on unemployment as well as the persistence of unemployment in response to shocks. For example, with respect to the first channel, a slowdown in productivity growth may result in unemployment unless wages are adjusted downwards, and this adjustment may be more difficult in systems with strict EPL where workers may be more reluctant to accept wage cuts. Similarly, once the adverse shocks generate an increase in unemployment, EPL and other institutions may prolong the time needed for unemployment to return to its normal level. To examine this hypothesis, Blanchard and Wolfers (2000) propose the following model that captures the interaction between institutions and common unobservable shocks, which are treated as time effects:

$$u_{i,t} = \lambda_t(1 + \sum_j \gamma_j x_{j,it}) + \alpha_i + \varepsilon_{i,t} \quad [2]$$

In this model $u_{i,t}$ is unemployment in country *i* at time *t*, α_i is the country effect for country *i*, λ_t is the country-unvarying time effect for year t, and x_j is the same set of labour market institutions considered in the linear models above. The effects of common shocks depend on labour market

institutions, and the coefficients γ_j capture this indirect effect of institutions on unemployment. The model is estimated via non-linear least squares.

Table 3 reports the results of this analysis. To ease the interpretation of the results, it needs to be noted that institutions in Blanchard and Wolfers's analysis are expressed as deviations from the sample means. In line with Baccaro and Rei (2007), the analysis presented here extends this formulation and considers annual data in both levels and deviations. Because the assumption of i.i.d. residuals is untenable (cf. Blanchard and Wolfers 2000, p. 20), the models are estimated using Rogers robust standard errors. These correspond to White standard errors adjusted to account for the possible correlation within a cluster (i.e. country) and country-specific heteroskedasticity. When data are in levels the coefficients on the time dummies (not reported) indicate the impact of shocks on unemployment in a country in which all institutional variables are set to zero. In this case, the coefficients of the institutional variables capture the additional effect of shocks on unemployment when a given institution increases by one unit. When data are in deviations the coefficients of the time dummies capture the impact of shocks in a country where all institutions are at the sample mean, and the coefficients of institutions in this case reflect the additional effect of shocks when the given institution increases one unit above the sample mean.

[Table 3 about here]

As Table 3 shows, there is no evidence that EPL either amplifies or mitigates the adverse effects of shocks on unemployment. As in the linear models above, other institutions seem to have more influence, although we cannot draw strong conclusions about their influence either since the way in which data are expressed evidently has a big impact on the results, particularly in the models of aggregate unemployment.

Does EPL affect changes in unemployment?

Given that the analysis above has not detected strong effects of EPL on the levels of unemployment, this section examines if changes in unemployment are related to changes in EPL. The analysis employs error correction models (ECM) to assess both short-term and long-term effects of changes in EPL.[13] The short-term effect captures a possibility that some portion of changes in EPL immediately affects unemployment in the next time period. The long-term effect implies that EPL and unemployment share an equilibrium relationship. Accordingly, any change in EPL would disturb the equilibrium with unemployment, thus forcing unemployment to gradually adjust to a value that reproduces the equilibrium state given the new value of EPL. As such, ECM presents a nice fit with theory that underpins the deregulatory view: it is based on the notion of a long-run equilibrium, which implies that unemployment cannot remain high (low) for a long time in the face of liberal (strict) employment protection. In addition, as outlined above, the advantage of ECM lies in providing a framework for understanding short- and long-run movements in unemployment.

The ECM model effectively measures how far EPL is out of equilibrium with unemployment and the speed at which unemployment returns to its equilibrium value. The model has the following form:

$$\Delta Y_{i,t} = \alpha + \beta_1 Y_{i,t-1} + \sum \beta^j X^j_{i,t-1} + \sum \beta^j_\Delta \Delta X^j_{i,t} + \varepsilon_t \quad [3]$$

[13] Simultaneously, ECM models address mild serial correlation in the baseline models of youth unemployment that remains despite the introduction of the lagged dependent variable.

where Y is unemployment (aggregate or youth) and Xs are independent variables (EPL, other labour market institutions, and macroeconomic controls). The subscripts *i* and *t* represent the particular country and year, the superscript *j* stands for the particular independent variable, while Δ is the first difference estimator. The coefficients on the lagged levels of independent variables provide an estimate of the long-term (permanent) effect of change in these variables on unemployment. The coefficients on the changes in independent variables are an estimate of the short-term (transitory) impact of a change in these variables. Since the results of tests suggest the need to correct for heteroskedasticity, the model is estimated using OLS with panel corrected standard errors.

[Table 4 about here]

Table 4 reports the estimates of this model. The models examine the effects of both overall strictness of employment protection and the strictness of regulations governing regular and temporary employment separately. Models 1 and 2 examine the effects on aggregate unemployment, while models 3 and 4 focus on youth unemployment. In all cases, the coefficient of the lagged dependent variable is negative and in the range from 0 to -1, which confirms that ECM is the correct choice. This also implies that the incremental effects of a shock to EPL are progressively reduced over time, so that unemployment converges to a long-term equilibrium. Columns 1 and 2 suggest that EPL has a lasting effect on aggregate unemployment. When EPL is disaggregated on its regular and temporary employment components, it becomes clear that it is primarily changes in regulations for standard contracts that have a lasting effect on unemployment. The results suggest that an increase in one unit of EPR (say from 2 to 3) increases the long-term equilibrium level of unemployment by 2.29 percent (obtained by dividing the parameter for the lagged level of EPR by minus the parameter for the lagged dependent

variable, $\beta^{EPL}/-\beta_1$). However, this effect is not robust and hinges entirely on the presence of France and Finland in the sample. Similarly, dropping any of the following countries from Model 1 makes EPL insignificant: Latvia, Romania, Italy, Norway, UK, US, Austria, Denmark, Finland, while dropping Spain makes this coefficient not only insignificant, but also negative. A closer look reveals that bargaining coordination is the only institutional variable that has a robust, albeit negative, effect on aggregate unemployment in both short and long run.

As far as youth unemployment is concerned, Models 3 and 4 detect no statistically significant permanent effect of employment protection, only a transitory effect of the regulations governing standard contracts. Although this transitory effect is not robust, as it hinges entirely on the presence of Lithuania in the sample, it is worthwhile considering its potential implications. The estimates of Model 4 suggest that an increase in EPR will increase youth unemployment in the next period, but since EPR exhibits only transitory effects, unemployment should revert back to its original level unless EPR changes continuously. However, given that the strictness of employment protection is defined in terms of an index that ranges from 0 to 6, EPR cannot grow without limit and will therefore have no lasting effect on youth unemployment unless the parameter for its lagged level is different from zero. Taken together, therefore, error correction models do not show sufficient evidence about an adverse effect of employment protection, nor do they show that a reduction in employment protection is beneficial in the sense that it helps to reduce unemployment or youth unemployment.

4. Discussion and concluding remarks

Despite a wide array of models and specifications, this analysis has not found sufficiently strong evidence about adverse effects of employment protection on unemployment. While one model of

youth unemployment suggests that a reduction in the strictness of employment protection may augment unemployment-reducing effects of cuts in labour taxes, no model shows sufficiently robust evidence about the direct or independent effects of employment protection. Disaggregating EPL on regulations for permanent and temporary contracts reveals some statistically significant associations with unemployment outcomes. However, none of these associations remains significant if we use the alternative estimators or the Jackknife procedure. From these results it seems justified to conclude that the effects of other variables, such as bargaining coordination and macroeconomic variables carry far greater weight than employment protection regulations in determining unemployment.

While these findings support recent research that questions the empirical evidence behind the argument about the need to deregulate labour market institutions in general (Avdagic & Salardi, 2013; Baccaro & Rei, 2007; Baker et al. 2005; Noelke, 2011), they clash with a number of studies that report adverse effects of EPL on unemployment (e.g. Blanchard & Wolfers, 2000; Elmeskov et al., 1998; Nickel et al., 2005; OECD, 1994; Scarpetta, 1996; Siebert, 1997) or youth unemployment (Bertola et al., 2002; Esping-Andersen, 2000; European Commission, 2006; OECD, 2004). How can we explain the lack of statistically significant effects of EPL in this analysis? Three potential reasons come to mind.

The first one is that the data used here are different from those used in research that emphasizes adverse effects of employment protection. Given that the role of employment protection is theoretically ambiguous and can be determined only empirically, the choice of data may have a big impact on our conclusions. Using the EPL measure from Nickell et al (2001; 2005) or the OECD EPL index does not affect appreciably the baseline results presented here. However, using our EPL index with other data from Nickell et al (2001; 2005) to re-estimate their

models suggests a positive effect of EPL, albeit only in models that include institutional interactions. Given that the correlation coefficients between the EPL index used here and the measures used by Nickell et al (2001; 2005) and the OECD are high (0.87 and 0.89 respectively), it is more likely that differences in other data (including the coverage of countries and the time period) are responsible for the different results. Still, this is probably not the whole story.

The second possibility is that EPL simply does not have strong effects on unemployment. This could be the case because either the positive and negative effects of employment protection balance out, or as Freeman has argued for labour market institutions in general, "bargaining settlements and regulations that are truly expensive to an economy" are effectively ruled out (2008: 25).

Finally, an explanation that may be the most credible is that the effects of EPL are not universal, and that this institution may have different effects in different countries or time periods. The fact that there seem to be different effects of employment protection on youth unemployment in different educational systems, and that the Jackknife analysis suggests very different conclusions about the role of this institution in different samples of countries, supports this interpretation. The actual effect of EPL may depend on the overall institutional setup and interactions between labour markets and other spheres, such as social policy, skill regimes and product markets – features that may not be adequately captured by country fixed effects in our models. In line with Ragin's (1987) idea of 'multiple conjectural causation', this interpretation implies that employment protection does not have a consistent causal effect on unemployment that applies universally. To put it differently, although the analysis presented here does not find strong evidence that employment protection affects unemployment adversely in general, it cannot exclude a possibility that this institution may contribute to high unemployment in some countries

depending on the overall institutional configurations. However, judging by the findings of this analysis, calls for across the board deregulation of employment protection seem to be clearly unwarranted.

TABLES

Table 1: The effect of EPL on unemployment in the EU and OECD Countries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	UR	YUR	UR	YUR	UR	YUR	UR	YUR	YUR
L.ur	0.813***		0.813***		0.813***		0.814***		
	(0.026)		(0.027)		(0.027)		(0.027)		
L.yur		0.873***		0.871***		0.873***		0.874***	0.756***
		(0.023)		(0.023)		(0.024)		(0.023)	(0.086)
EP	0.166	0.504					0.130	0.379	0.788
	(0.220)	(0.382)					(0.239)	(0.405)	(1.049)
BRR	1.306***	-1.067	1.382***	-1.298	1.354**	-1.989*	1.053***	-1.472	-4.401***
	(0.428)	(0.982)	(0.469)	(1.055)	(0.576)	(1.165)	(0.394)	(0.913)	(1.439)
TW	0.981	-0.880	1.037	-0.366	1.050	-0.157	1.178*	0.464	-4.008
	(0.698)	(0.877)	(0.670)	(0.925)	(0.720)	(0.981)	(0.704)	(1.042)	(4.199)
BC	-0.236***	-0.285*	-0.253***	-0.273*	-0.256***	-0.292**	-0.233***	-0.236	-0.307
	(0.068)	(0.146)	(0.065)	(0.144)	(0.064)	(0.147)	(0.071)	(0.150)	(0.300)
UD	2.855***	0.553	2.964***	0.248	3.014***	1.258	3.072***	0.983	-8.498**
	(0.755)	(1.971)	(0.811)	(2.335)	(0.780)	(2.530)	(0.768)	(2.189)	(3.506)
GDP	-0.284***	-0.508***	-0.278***	-0.496***	-0.278***	-0.497***	-0.284***	-0.507***	-0.432***
	(0.019)	(0.040)	(0.018)	(0.040)	(0.018)	(0.040)	(0.019)	(0.038)	(0.105)
CPI	-0.003	-0.096	-0.002	-0.094	-0.002	-0.093	-0.004	-0.105	-0.127**
	(0.036)	(0.063)	(0.036)	(0.068)	(0.036)	(0.068)	(0.036)	(0.066)	(0.060)
TOT	-0.019***	-0.029**	-0.019***	-0.025*	-0.019**	-0.027*	-0.017**	-0.020	-0.040
	(0.007)	(0.014)	(0.007)	(0.015)	(0.008)	(0.015)	(0.007)	(0.013)	(0.039)
RIR	0.028	0.148***	0.028	0.150***	0.028	0.151***	0.029	0.151***	0.157*
	(0.020)	(0.036)	(0.020)	(0.037)	(0.020)	(0.037)	(0.020)	(0.037)	(0.085)
ry2554		7.592		7.564		6.602		9.367*	14.631***
		(5.370)		(5.355)		(5.550)		(5.353)	(5.466)
EPR			0.102	0.929***	0.099	0.857***			
			(0.105)	(0.331)	(0.118)	(0.323)			
EPT			0.091	0.085	0.087	-0.013			
			(0.096)	(0.127)	(0.120)	(0.149)			
EPR*EPT					0.011	0.223*			
					(0.069)	(0.123)			
EP*BRR							0.425	1.005	
							(0.536)	(1.127)	
EP*TW							-0.721	-4.286***	

							(0.744)	(0.961)	
MW									5.507
									(7.051)
EP*MW									-1.003
									(5.631)
Constant	2.502***	2.942	2.544***	-0.915	2.532***	-0.406	2.245***	1.142	1.758
	(0.850)	(1.947)	(0.861)	(3.589)	(0.821)	(3.516)	(0.852)	(2.034)	(6.548)
Country and year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	451	429	448	426	448	426	451	429	223
R2	0.949	0.958	0.949	0.958	0.949	0.959	0.949	0.958	0.961

Notes: Standard errors in parentheses:* significant at 10%, ** 5%, and *** 1% level.

Table 2: The effects of EPL in different education settings

	(1)	(2)	(3)	(4)
	low signalling	high signalling	low signalling	high signalling
L.yur	0.8072***	0.8410***	0.8086***	0.8279***
	(0.0369)	(0.0753)	(0.0372)	(0.0803)
EP	0.9509**	-1.4512**		
	(0.4063)	(0.6394)		
EPR			0.9205**	0.1600
			(0.4229)	(1.2342)
EPT			0.3110	-0.8101**
			(0.3351)	(0.3736)
BRR	-1.4792	4.4038	-1.4757	4.3130
	(1.5068)	(7.0004)	(1.7614)	(7.1050)
TW	1.4143	3.6469	2.5564	4.1489
	(2.4543)	(3.3214)	(3.2454)	(3.4659)
BC	-0.3957**	0.0247	-0.3683*	0.0584
	(0.1739)	(0.2646)	(0.1925)	(0.2687)
UD	-5.4324*	32.1754***	-4.9846	29.5835***
	(2.9450)	(11.2675)	(3.7006)	(10.8579)
GDP	-0.4724***	-0.5659***	-0.4679***	-0.5647***
	(0.0473)	(0.1603)	(0.0474)	(0.1616)
CPI	-0.1346	-0.0073	-0.1285	-0.0087
	(0.1114)	(0.0611)	(0.1116)	(0.0611)
TOT	-0.0317*	-0.0211	-0.0307*	-0.0254
	(0.0179)	(0.0213)	(0.0178)	(0.0229)
RIR	0.1602***	-0.0150	0.1629***	-0.0274
	(0.0446)	(0.0747)	(0.0455)	(0.0748)
ry2554	7.7816	9.2931	7.8856	6.8153
	(8.1310)	(14.6090)	(10.2971)	(14.1415)
Constant	6.5395**	-9.1642	0.1475	-10.4924
	(2.9332)	(7.5814)	(4.5146)	(8.8811)
country and year effects	Yes	Yes	Yes	Yes
N	267	162	264	162
r2	0.958	0.953	0.958	0.953

Notes: Standard errors in parentheses:* significant at 10%, ** 5%, and *** 1% level.

Table 3: Interactions between Shocks and Institutions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	UR levels Rogers se	UR levels	UR deviationsRogers se	UR deviations	YUR levels, Rogers se	YUR levels	YUR deviationsRogers se	YUR deviations
EP	0.0328	0.0328	0.0104	0.0104	0.1534	0.1534	0.4458	0.4458
	(0.3399)	(0.1197)	(0.1016)	(0.0368)	(0.1815)	(0.1207)	(1.4377)	(0.5754)
UD	3.8285	3.8285**	1.2108**	1.2108***	-2.0084	-2.0084**	-5.8369	-5.8369
	(5.8270)	(1.7205)	(0.5436)	(0.1617)	(2.0123)	(0.8974)	(23.9604)	(9.4474)
BRR	1.4647	1.4647**	0.4632*	0.4632***	0.4078	0.4078	1.1851	1.1851
	(1.3826)	(0.7190)	(0.2393)	(0.1401)	(0.9524)	(0.5988)	(4.2014)	(2.1319)
TW	1.3446	1.3446	0.4252	0.4252**	0.6142	0.6142	1.7849	1.7849
	(2.4396)	(0.9473)	(0.3812)	(0.1788)	(0.7807)	(0.5565)	(5.7901)	(2.5626)
BC	-0.2603	-0.2603***	-0.0823**	-0.0823***	-0.2217	-0.2217**	-0.6444	-0.6444
	(0.2078)	(0.0843)	(0.0396)	(0.0205)	(0.1977)	(0.1055)	(2.3812)	(0.9632)
N	500	500	500	500	279	279	279	279
r2	0.806	0.806	0.806	0.806	0.883	0.883	0.883	0.883
r2_a	0.798	0.783	0.798	0.783	0.877	0.859	0.877	0.859

Notes: Non-linear least squares; time and country dummies omitted.

Table 4: The effects on EPL on changes in aggregate and youth unemployment

	(1)	(2)	(3)	(4)
	D.UR	D.UR	D.YUR	D.YUR
L.ur	-0.1188***	-0.1179***		
	(0.0402)	(0.0424)		
L.yur			-0.1638***	-0.1613***
			(0.0496)	(0.0522)
D.EP	-0.1310		0.1579	
	(0.2932)		(0.9188)	
L.EP	0.2722*		0.3888	
	(0.1604)		(0.3728)	
D.BRR	-2.1757**	-2.1041**	-3.9387*	-3.9399
	(0.9166)	(0.9569)	(2.3070)	(2.4758)
L.BRR	-0.4661	-0.4444	-1.4575*	-1.8158**
	(0.4352)	(0.4861)	(0.8198)	(0.8311)
D.TW	0.0873	-0.0169	2.0711	1.8687
	(1.0670)	(1.0779)	(3.3953)	(3.4591)
L.TW	-0.0843	0.0836	-1.1412	-0.6395
	(0.6358)	(0.6786)	(2.4740)	(2.7836)
D.BC	-0.1960***	-0.2364***	-0.3172	-0.3685
	(0.0568)	(0.0609)	(0.2324)	(0.2463)
L.BC	-0.1318***	-0.1212**	-0.2406	-0.2158
	(0.0459)	(0.0517)	(0.1682)	(0.1910)
D.UD	6.4501*	4.7155	15.7365*	10.4147
	(3.5149)	(3.4071)	(9.5293)	(9.7055)
L.UD	0.5078	0.4420	2.5863	1.7404
	(1.6822)	(1.6335)	(3.3849)	(3.2328)
D.GDP	-0.2204***	-0.2079***	-0.3682***	-0.3552***
	(0.0284)	(0.0304)	(0.0875)	(0.0909)
L.GDP	-0.3567***	-0.3562***	-0.5874***	-0.5897***
	(0.0295)	(0.0317)	(0.0866)	(0.0910)
D.CPI	0.0207	0.0220	-0.0719	-0.0585
	(0.0185)	(0.0223)	(0.0488)	(0.0530)
L.CPI	-0.0134	-0.0109	-0.0752	-0.0571
	(0.0264)	(0.0312)	(0.0730)	(0.0809)
D.TOT	0.0013	0.0002	0.0211	0.0231
	(0.0118)	(0.0120)	(0.0417)	(0.0420)
L.TOT	-0.0075*	-0.0072	-0.0418***	-0.0379***
	(0.0044)	(0.0049)	(0.0117)	(0.0135)
D.RIR	0.0667**	0.0677**	0.1229	0.1283
	(0.0268)	(0.0281)	(0.0827)	(0.0853)
L.RIR	0.1101***	0.1126***	0.2333**	0.2442**
	(0.0297)	(0.0315)	(0.1022)	(0.1047)
D.EPR		0.2241		1.9841***

		(0.2682)		(0.7438)
L.EPR		0.2640**		0.6935
		(0.1236)		(0.5195)
D.EPT		-0.1220		-0.2211
		(0.1830)		(0.5199)
L.EPT		0.1015		0.0101
		(0.0916)		(0.2153)
D.ry2554			29.5633***	28.8977***
			(7.3259)	(9.8637)
L.ry2554			11.6900***	10.0754**
			(3.2647)	(4.0426)
Constant	0.2711	1.9320**	1.0114	1.2658
	(0.7756)	(0.8641)	(3.7814)	(3.8839)
country and year effects	Yes	Yes	Yes	Yes
N	423	420	409	406
R2	0.635	0.640	0.493	0.502

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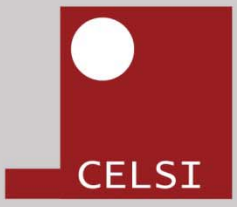
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