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Does Deregulation Work? Reassessing the Unemployment Effects of Employment Protection

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Abstract

Using new data, this article examines the effect of employment protection legislation (EPL) on aggregate and youth unemployment in advanced economies and Central and Eastern Europe during 1980–2009. The results offer no clear support for the argument that EPL is a cause of unemployment. Although 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. While the analysis suggests some scope for complementary reforms of EPL and the tax wedge in tackling youth unemployment, the findings on the whole indicate that government efforts to tackle unemployment by deregulating EPL alone may well be futile.

1. 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 and unclear empirical findings, many studies that have informed policy recommendations draw a link between

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strict EPL and poor labour market performance. While the orthodox view espoused by the OECD Jobs Study (1994) and related literature (Scarpetta 1996; Siebert 1997) has been subsequently toned down (Bassanini and Duval 2006; OECD 2004) and challenged by recent research (Baccaro and Rei 2007; Baker *et al*. 2005), much of the literature agrees that EPL has an adverse effect at least on youth unemployment (Addison and Teixeira 2003; Bertola *et al*. 2002; Botero *et al*. 2004; Esping-Andersen 2000; European Commission 2006; OECD 2004).

This article re-examines the effects of EPL on aggregate and youth unemployment by relying on newly constructed annual data measuring the strictness of EPL (Allard 2010; Avdagic 2012a). 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 into its core components that govern permanent and temporary contracts. The data include not only the usual group of advanced OECD economies, but also 10 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 different models and estimators, and examines if individual countries have disproportionate influence on coefficient estimates.

The findings of the article can be summarized as follows: EPL has no adverse effect on aggregate unemployment. Perhaps more surprising is the finding that there is no direct link between EPL and youth unemployment either. The only finding that appears robust concerns the interaction between EPL and the tax wedge, which suggests some scope for reform complementarity when tackling youth unemployment. On the whole, the analysis suggests that EPL is not a key culprit for unemployment, and thus government efforts to tackle unemployment by liberalizing employment laws alone may well be futile.

The article is organized into three sections. Section 2 discusses the main theoretical arguments and previous empirical findings about the effects of EPL on unemployment. Section 3 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 thorough investigation of the direct and indirect effects of EPL on unemployment. Section 4 concludes by discussing the main findings.

2. 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. In this line of reasoning, strict job security provisions 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 and Teixeira 2003). 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 (especially young and inexperienced) 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, therefore, makes it clear that strict EPL reduces labour fluctuations over the cycle. However, the overall effect on unemployment levels is theoretically ambiguous and may depend on issues such as the functional form of labour demand functions, the discount rate, labour turnover and wage flexibility (Bertola 1992). 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).

The empirical literature reflects this ambiguity. 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 unemployment (e.g. Blanchard and Wolfers 2000; Elmeskov *et al.* 1998, Nickell *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 and Salardi 2013; Baccaro and Rei 2007; Baker *et al.* 2005; Howell *et al.* 2007; Vergeer and Kleinknecht 2012).[1]

Considering separately regulations for regular (EPR) and temporary (EPT) contracts offers more nuanced predictions about the impact of employment protection on aggregate, and especially youth unemployment. When EPR is strict, employers have an incentive to rely on the less costly temporary contracts. The increasing reliance on temporary contracts increases the rate of hiring, but also job loss rates due to the shorter length of employment spells. The upshot of this is that deregulation of temporary contracts should affect labour market flows, but its impact on unemployment rates again can be determined only empirically. Reflecting 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 and 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 (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 these contracts. In contrast, the 'segmentation view' emphasizes the increasing divisions between permanent and temporary workers. In this view,

deregulation of temporary contracts offers an incentive for employers to increasingly use these contracts, especially in countries where EPR is high (Boeri and Garibaldi 2009). 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, therefore, may 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.

Most of the empirical literature agrees that EPL has adverse effects on youth labour market performance (Addison and Teixeira 2003; Esping-Andersen 2000; European Commission 2006; OECD 1994, 2004). Some authors qualify this position by showing that the impact of EPL may be different in different countries, depending on other institutions, such as the nature of the education system (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 article reassesses this evidence based on new data covering both advanced and new market economies.

3. Exploring the link between EPL and unemployment

The last couple of decades have witnessed a large number of reforms. Systematic qualitative data on EPL reforms in Europe (Avdagic 2012b; FRDB-IZA 2010) 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 in key issues that affect all workers, such as severance payments, the notice period or the obligation to consult unions prior to dismissals. While these data are too crude to allow a proper assessment of EPL effects, it provides a quick first glance at the relationship between EPL and unemployment. A fixed-effect logistic regression of a simple model in which a decline in 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.[2] Clearly, this model permits at best partial conclusions since these data capture only the occurrence of a particular type of reform, but not more fine-grained differences, 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 unemployment by using new, annual data on EPL strictness.

Following a brief description of the data, the analysis is divided into two parts. In line with most of the literature that supports the deregulatory view, the focus in the first part is on dynamic fixed-effects models that examine the effects of EPL on the levels of unemployment. The second part considers more briefly two alternative models: the first is a nonlinear model examining the effects of EPL via interactions with macroeconomic shocks, while the second is an error correction model (ECM) investigating the short-term and long-term effects of changes in EPL on changes in unemployment.

Data

The analysis covers 31 countries, including all 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.[3] 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 reflect 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. This index measures the strictness of EPL 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 International Labour Organization's (ILO) 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. Table 1 presents the summary statistics of this indicator.

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

TABLE 1
EPL Index, 1980–2009

	Mean	*s.d.*	*Min*	*Max*
Australia	1.06178	0.29404	0.36458	1.28333
Austria	2.24988	0.13351	2.08973	2.40074
Belgium	2.35590	0.25410	2.09405	2.66696
Bulgaria	1.52945	0.24157	1.31035	1.78952
Canada	0.98225	0.40788	0.10417	1.16518
Czech Republic	2.01908	0.27078	1.72751	2.48793
Denmark	1.97191	0.35293	1.68750	2.66667
Estonia	1.97121	0.32843	1.63525	2.47751
Finland	2.02213	0.32797	1.51777	2.34000
France	3.04687	0.21810	2.58140	3.30134
Germany	2.86217	0.35948	2.40402	3.28021
Greece	3.56285	0.64502	1.70223	3.84509
Hungary	1.63624	0.16326	1.08631	1.78985
Ireland	1.36248	0.03563	1.32694	1.42426
Italy	3.10710	0.36823	2.75357	3.70878
Japan	1.75574	0.07717	1.59286	1.85327
Latvia	1.64568	0.54369	1.04993	2.33118
Lithuania	2.39110	0.14119	1.96511	2.55837
Netherlands	2.28581	0.53124	1.33854	2.78854
New Zealand	0.57629	0.14208	0.37917	0.97083
Norway	2.51126	0.09771	2.38914	2.70164
Poland	1.84335	0.21862	1.60516	2.15675
Portugal	3.62536	0.14067	3.53750	4.08646
Romania	2.19965	0.39539	1.73611	2.79861
Slovak Republic	2.13652	0.21698	1.75579	2.36640
Slovenia	2.26874	0.20626	1.66171	2.50380
Spain	2.88990	0.61202	2.28170	3.90923
Sweden	3.10205	0.40620	2.64107	3.66354
Switzerland	1.04793	0.17650	0.74747	1.15491
United Kingdom	1.67998	0.04469	1.63973	1.76250
United States	0.46900	0.22560	0.13854	0.61771
Total	2.08163	0.87927	0.10417	4.08646

EPL, employment protection legislation.

unemployment across two levels of earnings (67 per cent and 100 per cent of average wage). The calculations for CEE countries combine information on benefits provided by the 'Social Security Programs Throughout the World' reports 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 co-ordination 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).

Assessing the Unemployment Effects of EPL: A Dynamic Model

This section estimates a dynamic model of unemployment that has been used widely in the literature (Nickell *et al*. 2005; see also Baccaro and Rei 2007; IMF 2003; Layard *et al*. 1991). In this model, labour market institutions determine the equilibrium level of unemployment, while macroeconomic controls account for short-term deviations from the equilibrium level. The model has the following form:

$$u_{i,t} = \beta_0 + \beta_1 u_{i,t-1} + \sum\nolimits_j \gamma_j x_{j,it} + \sum\nolimits_k \eta_k z_{k,it} + \sum\nolimits_n \delta_n v_{n,it} + \alpha_i + \lambda_t + \varepsilon_{i,t} \quad (1)$$

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, α_i and λ_t are country and year dummies, respectively, and $\varepsilon_{i,t}$ is the stochastic residual. 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 the unemployment benefit replacement rate (BRR), the tax wedge (TW), union density (UD) and wage bargaining co-ordination (BC). Generous *unemployment benefits* are commonly thought to increase unemployment because they imply a high reservation wage, which makes unemployed individuals both more reluctant to search for jobs and to accept available jobs. The *tax wedge*, the difference between the labour cost to employers and the take-home wage for employees, is generally expected to reduce the demand for labour, and in particular for young workers. However, the actual impact of this variable depends on the distribution of taxes between employers and labour. If employees carry most of the tax burden, labour demand may not be affected. 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 the employment of young and low-skill workers. In addition, unions may support the deregulation of temporary employment while trying to protect the job security of permanent workers as their core constituents (King and Rueda 2008; Palier and Thelen 2010). As a result, 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. Finally, the effect of *wage bargaining co-ordination* is generally considered to be beneficial for labour market performance. Because unions in co-ordinated 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 (OECD 1997; 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, the change in inflation should be negatively related to unemployment in the short run. The *terms of trade* 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 should increase unemployment because an increase in real interest rates is likely to reduce aggregate demand (Baker *et al*. 2005). Models of youth unemployment also include the ratio of youth to adult (25–54 years) population as an additional control.

The 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 the common properties of this type of data, including panel heteroscedasticity and contemporaneous correlation of the error terms. A feasible generalized least square (FGLS) estimator, which is commonly used in the literature that supports the deregulatory view, was used as an additional check. This model assumes country-specific heteroscedasticity and employs a Prais–Winsten transformation to address a first order (AR1) autoregressive structure in the errors (a common estimated rho). This estimator, however, is not designed to correct for contemporaneously correlated errors, which characterize these data and thus can produce artificially small errors (Beck and Katz 1996). None the less, if the FGLS models yield similar results to OLS-PCSE, this should increase our trust in the credibility of the results. For reasons of space, Tables 2 and 3 report only the OLS-PCSE estimates, while the text indicates if the FGLS models offer considerably different conclusions.

Given the dynamic nature of these models and thus the potential concerns about the Nickell bias,[4] OLS estimates were also checked against the least squares dummy variable model (LSDV) with the Kiviet correction (Kiviet 1995).[5] 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.

The first four columns of Table 2 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 aggregate and youth unemployment, respectively, while columns 3 and 4 consider the impact of regulations for temporary and regular contracts

TABLE 2
The Effect of EPL on Unemployment in the EU and OECD Countries

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Unemployment, $_{t-1}$	0.813***	—	0.813***	—	0.813***	—	0.814***	—	—
	(0.026)		(0.027)		(0.027)		(0.027)		
Youth unemployment, $_{t-1}$	—	0.873***	—	0.871***	—	0.873***	—	0.874***	0.756***
		(0.023)		(0.023)		(0.024)		(0.023)	(0.086)
EPL	0.166	0.504	—		—	—	0.130	0.379	0.788
	(0.220)	(0.382)					(0.239)	(0.405)	(1.049)
Replacement rate	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)
Tax wedge	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)
Wage co-ordination	−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)
Union density	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)
Growth	−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)
Inflation	−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)
Terms of trade	−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)
Real interest rate	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)
Youth share	—	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)			
EPL × replacement rate	—	—	—	—	—	—	0.425	1.005	—
							(0.536)	(1.127)	
EPL × tax wedge	—	—	—	—	—	—	−0.721	−4.286***	—
							(0.744)	(0.961)	
Minimum wage	—	—	—	—	—	—	—	—	5.507
									(7.051)
EPL × minimum wage	—	—	—	—	—	—	—	—	−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
R^2	0.949	0.958	0.949	0.958	0.949	0.959	0.949	0.958	0.961

Note: Standard errors in parentheses: * significant at 10%, ** significant at 5% and *** significant at 1% level.
EPL, employment protection legislation; EPR, employment protection for regular contracts; EPT, employment protection for temporary contracts.

TABLE 3
The Effects of EPL on Youth Unemployment in Different Education Settings

	(1) *Low signalling*	*(2)* *High signalling*	*(3)* *Low signalling*	*(4)* *High signalling*
Youth unemployment, $_{t-1}$	0.8072***	0.8410***	0.8086***	0.8279***
	(0.0369)	(0.0753)	(0.0372)	(0.0803)
EPL	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)
Replacement rate	–1.4792	4.4038	–1.4757	4.3130
	(1.5068)	(7.0004)	(1.7614)	(7.1050)
Tax wedge	1.4143	3.6469	2.5564	4.1489
	(2.4543)	(3.3214)	(3.2454)	(3.4659)
Wage co-ordination	–0.3957**	0.0247	–0.3683*	0.0584
	(0.1739)	(0.2646)	(0.1925)	(0.2687)
Union density	–5.4324*	32.1754***	–4.9846	29.5835***
	(2.9450)	(11.2675)	(3.7006)	(10.8579)
Growth	–0.4724***	–0.5659***	–0.4679***	–0.5647***
	(0.0473)	(0.1603)	(0.0474)	(0.1616)
Inflation	–0.1346	–0.0073	–0.1285	–0.0087
	(0.1114)	(0.0611)	(0.1116)	(0.0611)
Terms of trade	–0.0317*	–0.0211	–0.0307*	–0.0254
	(0.0179)	(0.0213)	(0.0178)	(0.0229)
Real interest rate	0.1602***	–0.0150	0.1629***	–0.0274
	(0.0446)	(0.0747)	(0.0455)	(0.0748)
Youth ratio	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
R^2	0.958	0.953	0.958	0.953

Note: Standard errors in parentheses: * significant at 10%, ** significant at 5% and *** significant at 1% level.
EPL, employment protection legislation; EPR, employment protection for regular contracts; EPT, employment protection for temporary contracts.

separately. The coefficients on 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 Phillips–Perron tests (Maddala and Wu 1999) reject the null of no co-integration.[6] The EPL coefficients are positively signed, but not statistically significant.[7] When EPL is disaggregated, however, strict EPR seems to be associated with higher youth unemployment. However, this result is not robust: it does not hold in the FGLS model, and 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 where GDP growth is the only control, including the models where EPL is the only institutional variable.

Columns 5 and 6 examine the interaction between EPR and EPT to evaluate the 'integration' and 'segmentation' hypotheses (discussed above). The interaction coefficient is statistically significant only in the model with youth unemployment. This suggests that deregulation of temporary contracts reduces youth unemployment, particularly when the regulations for regular contracts are strict. While this seems to confirm the 'integration' scenario, the result is again not robust and survives neither the Jackknife procedure nor the use of the alternative estimators.[8]

The next three columns assess if the impact of EPL may be adverse when associated with another institutional rigidity. These columns replicate the model proposed by Bassanini and Duval (2009), which examines complementarities across labour market reforms. A negative and significant interaction coefficient would suggest reform complementarity. A negative sign implies that the detrimental effect of each institution on unemployment is smaller the stricter the other institution, so that liberalizing reforms should be undertaken together to maximize their impact.[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 EPL 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 EPL and unemployment benefits follows a similar logic. Generous unemployment benefits may reduce workers' incentives to look for jobs, which consequently may also discourage vacancy posting (IMF 2003). Finally, column 9 examines if strict EPL may be associated with high youth unemployment in contexts where the minimum wage is high. The only interaction that is statistically significant is the interaction with the tax wedge in the model with youth unemployment (column 8). 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. Apart from this finding, however, Table 2 provides no evidence that the EPL is responsible for either aggregate or youth unemployment.

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. However, Granger causality tests 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 unemployment 0.26, $p = 0.77$). Further evidence comes from Granger 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, confirming that causation runs from EPL to unemployment. There is also no 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 2.[10]

Table 3 examines in more detail the effects of EPL on youth unemployment by dividing the countries into two groups according to the type of the education system. Previous research has shown that vocational training institutions facilitate transitions from school to employment, thus reducing youth unemployment (Breen 2005; Gangl 2003; Wolbers 2007). Systems that emphasize general skills tend to have higher youth unemployment unless EPL enables easy hiring and firing. In contrast, education systems that emphasize specific skills and direct links with employers provide signals about the quality and likely productivity of particular workers for specific jobs. In such systems, as Breen (2005) has argued, employers may 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 mitigates the adverse effects of strict EPL.

One way to assess this hypothesis would be to include the interaction between EPL and the proportion of pupils who follow vocational tracks. A drawback to using this indicator is that there are no sufficiently long and comparable series for all CEE countries. Importantly, 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 pupils 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. To avoid this problem, the analysis presented here relies on Breen (2005), as well as Busemeyer (2009) and additional secondary literature, to divide the countries into two groups. Countries with high educational signalling include Austria, Belgium, Denmark, Finland, Germany, Japan, the Netherlands, Norway, Slovenia, Sweden and Switzerland.[11] Countries with low educational signalling include all English-speaking countries, France, Greece, Italy, Portugal, Spain, and all CEE countries apart from Slovenia.

Table 3 reports the results of this analysis. Column 1 and 2 show that while EPL increases youth unemployment in countries with low education signalling, it is associated with lower unemployment when education signalling is high. If we disaggregate EPL, it becomes clear that the adverse effect of employment protection in countries with low signalling is primarily due to the regulations for regular contracts, while the beneficial effect in countries

with high signalling is related to the regulations of temporary contracts. The latter finding seems to suggest that in systems with high signalling, strict EPT may lead employers to offer more permanent contracts to young workers, thus lowering labour turnover and youth unemployment. On the whole, these results are in line with the expectations of scholars emphasizing the differential effects of EPL (Breen 2005), and the varieties of capitalism literature more broadly (Estevez-Abe *et al*. 2001). However, while the results are robust to the use of the alternative estimators, the Jackknife analysis reveals that the EPL coefficients are no longer significant if Greece or the United Kingdom is omitted from the first group, and Belgium or Germany from the second.

Alternative Models

This section examines two alternative models of EPL effects on unemployment. The first model, proposed by Blanchard and Wolfers (2000), assumes that EPL (and other institutions) affects the level and persistence of unemployment by amplifying the effects of economic shocks. 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. This model captures the interaction between institutions and common unobservable shocks, which are treated as time effects:

$$u_{i,t} = \lambda_t\left(1 + \sum\nolimits_j \gamma_j x_{j,it}\right) + \alpha_i + \varepsilon_{i,t} \tag{2}$$

where $u_{i,t}$ is unemployment in country i at time t, α_i is the country effect for country i, λ_t is the time effect for year t, and x_j is the same set of 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 nonlinear least squares.

Table 4 reports the results of this analysis. Following Blanchard and Wolfers (2000), institutions are expressed as deviations from the sample means. The coefficients of the time dummies (not reported) capture the impact of shocks in a country where all institutions are at the sample mean, and the coefficients of institutions reflect the additional effect of shocks when the given institution increases one unit above the sample mean. The results show no evidence that EPL amplifies the adverse effects of shocks on either aggregate or youth unemployment.

The second alternative model examines if changes in unemployment are related to changes in EPL. The analysis employs ECM to assess both the short-term and long-term effects of changes in EPL. 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

TABLE 4
Interactions between Shocks and Institutions

	(1) *Unemployment*	*(2)* *Youth unemployment*
EPL	0.0104	0.4458
	(0.1016)	(1.4377)
Replacement rate	0.4632*	1.1851
	(0.2393)	(4.2014)
Tax wedge	0.4252	1.7849
	(0.3812)	(5.7901)
Wage co-ordination	–0.0823**	–0.6444
	(0.0396)	(2.3812)
Union density	1.2108**	–5.8369
	(0.5436)	(23.9604)
N	500	279
R^2	0.806	0.883
R^2_a	0.798	0.877

Notes: Nonlinear least squares; time and country dummies omitted. Rogers robust standard errors in parentheses: * significant at 10%, ** significant at 5%, *** significant at 1%.
EPL, employment protection legislation.

that EPL and unemployment share an equilibrium relationship. Accordingly, any change in EPL would force unemployment to gradually adjust to a value that reproduces the equilibrium state given the new value of EPL. Although ECM has not been used often in the literature on the subject, it 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 low for a long time in the face of strict employment protection. The model has the following form:

$$\Delta u_{i,t} = \alpha + \beta_1 u_{i,t-1} + \sum \beta^j x^j_{i,t-1} + \sum \beta^j_\Delta \Delta x^j_{i,t} + \varepsilon_t \tag{3}$$

where u is unemployment (aggregate or youth) and x are independent variables (including both 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. Given the need to correct for heteroscedasticity, the model is estimated using OLS with PCSE.

Table 5 reports the estimates of this model. In all models, the coefficient of the lagged dependent variable is negative and in the range from 0 to –1, which implies that the incremental effects of a shock to EPL are progressively reduced over time, so that unemployment converges to a long-term equilibrium. Column 1 suggests that EPL has a lasting effect on aggregate unemployment. When EPL is disaggregated (column 2), it becomes clear that it is

TABLE 5
The Effects on EPL on Changes in Aggregate and Youth Unemployment

	(1) *D.UR*	*(2)* *D.UR*	*(3)* *D.YUR*	*(4)* *D.YUR*
Unemployment, $_{t-1}$	–0.1188***	–0.1179***	—	—
	(0.0402)	(0.0424)		
Youth unemployment, $_{t-1}$	—	—	–0.1638***	–0.1613***
			(0.0496)	(0.0522)
Δ EPL	–0.1310	—	0.1579	—
	(0.2932)		(0.9188)	
EPL, $_{t-1}$	0.2722*	—	0.3888	—
	(0.1604)		(0.3728)	
Δ Replacement rate	–2.1757**	–2.1041**	–3.9387*	–3.9399
	(0.9166)	(0.9569)	(2.3070)	(2.4758)
Replacement rate, $_{t-1}$	–0.4661	–0.4444	–1.4575*	–1.8158**
	(0.4352)	(0.4861)	(0.8198)	(0.8311)
Δ Tax wedge	0.0873	–0.0169	2.0711	1.8687
	(1.0670)	(1.0779)	(3.3953)	(3.4591)
Tax wedge, $_{t-1}$	–0.0843	0.0836	–1.1412	–0.6395
	(0.6358)	(0.6786)	(2.4740)	(2.7836)
Δ Wage co-ordination	–0.1960***	–0.2364***	–0.3172	–0.3685
	(0.0568)	(0.0609)	(0.2324)	(0.2463)
Wage co-ordination, $_{t-1}$	–0.1318***	–0.1212**	–0.2406	–0.2158
	(0.0459)	(0.0517)	(0.1682)	(0.1910)
Δ Union density	6.4501*	4.7155	15.7365*	10.4147
	(3.5149)	(3.4071)	(9.5293)	(9.7055)
Union density, $_{t-1}$	0.5078	0.4420	2.5863	1.7404
	(1.6822)	(1.6335)	(3.3849)	(3.2328)
Δ Growth	–0.2204***	–0.2079***	–0.3682***	–0.3552***
	(0.0284)	(0.0304)	(0.0875)	(0.0909)
Growth, $_{t-1}$	–0.3567***	–0.3562***	–0.5874***	–0.5897***
	(0.0295)	(0.0317)	(0.0866)	(0.0910)
Δ Inflation	0.0207	0.0220	–0.0719	–0.0585
	(0.0185)	(0.0223)	(0.0488)	(0.0530)
Inflation, $_{t-1}$	–0.0134	–0.0109	–0.0752	–0.0571
	(0.0264)	(0.0312)	(0.0730)	(0.0809)
Δ Terms of trade	0.0013	0.0002	0.0211	0.0231
	(0.0118)	(0.0120)	(0.0417)	(0.0420)
Terms of trade, $_{t-1}$	–0.0075*	–0.0072	–0.0418***	–0.0379***
	(0.0044)	(0.0049)	(0.0117)	(0.0135)
Δ Real interest rate	0.0667**	0.0677**	0.1229	0.1283
	(0.0268)	(0.0281)	(0.0827)	(0.0853)
Real interest rate, $_{t-1}$	0.1101***	0.1126***	0.2333**	0.2442**
	(0.0297)	(0.0315)	(0.1022)	(0.1047)
Δ EPR	—	0.2241	—	1.9841***
		(0.2682)		(0.7438)
EPR, $_{t-1}$	—	0.2640**	—	0.6935
		(0.1236)		(0.5195)
Δ EPT	—	–0.1220	—	–0.2211
		(0.1830)		(0.5199)
EPT, $_{t-1}$	—	0.1015	—	0.0101
		(0.0916)		(0.2153)
Δ Youth ratio	—	—	29.5633***	28.8977***
			(7.3259)	(9.8637)
Youth ratio, $_{t-1}$	—	—	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
R^2	0.635	0.640	0.493	0.502

Standard errors in parentheses: * significant at 10%, ** significant at 5%, *** significant at 1%. EPL, employment protection legislation; EPR, employment protection for regular contracts; EPT, employment protection for temporary contracts.

primarily changes in regulations for regular 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 per cent.[12] However, this effect is not robust and hinges 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, United Kingdom, United States, Austria, Denmark and Finland — while dropping Spain makes this coefficient not only insignificant, but also negative.

Models of youth unemployment (columns 3 and 4) detect no statistically significant permanent effect of employment protection, only a transitory effect of the regulations governing regular contracts. This finding implies 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 since EPR cannot change continuously. Even this weak transitory effect, however, is not robust as it depends entirely on the presence of Lithuania in the sample. Taken together, therefore, ECMs do not show sufficient evidence of an adverse effect of employment protection on unemployment.

4. Discussion and concluding remarks

Despite a wide array of models and specifications, this analysis has not found clear evidence of 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 the unemployment-reducing effects of cuts in labour taxes, no model shows robust evidence of independent effects of employment protection. Considering regulations for permanent and temporary contracts separately reveals some statistically significant associations with unemployment outcomes. However, none of these associations survive the use of alternative estimators or the Jackknife procedure. From these results, it seems justified to conclude that the effects of other variables, such as bargaining co-ordination and GDP growth, carry far greater weight than EPL in determining unemployment.

While these findings support recent research that questions the empirical evidence behind the deregulatory argument (Avdagic and Salardi 2013; Baccaro and Rei 2007; Baker *et al.* 2005; Noelke 2011), they clash with studies that report adverse effects of EPL on unemployment (e.g. Blanchard and Wolfers 2000; Elmeskov *et al.* 1998; Nickell *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 large impact on our conclusions. However, using the EPL measure from Nickell *et al*. (2005) or the OECD EPL index does not affect appreciably the baseline results presented here. On the other hand, using our EPL index with other data from Nickell *et al*. (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*. (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 EPL 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 seems 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 education systems, and that the Jackknife analysis suggests very different conclusions about the role of this institution in different countries, supports this interpretation. The actual effect of EPL may depend on the overall institutional set-up and interactions between labour markets, social policy, skill regimes and product markets — features that may not be adequately captured by country fixed effects. In line with Ragin's (1987) idea of 'multiple conjectural causation', this interpretation implies that EPL does not have a consistent causal effect on unemployment that applies universally.[13] To put it differently, although the analysis presented here does not find strong evidence that EPL 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 configuration. However, judging by the findings of this analysis, calls for across-the-board deregulation of employment protection seem to be clearly unwarranted.

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Notes

1. Note that these studies, including Avdagic and Salardi (2013), do not focus on the effects of EPL specifically, but rather on the overall effects of labour market institutions on unemployment. As such, these studies consider neither the effects of EPL for regular and temporary contracts separately nor the interactions between EPL and other institutions considered here.
2. 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 with one-, two- and three-year lags. None of these suggest that EPL liberalization leads to a decline in unemployment. For reasons of space, these estimates are not shown here.
3. For reasons of comparability, the analysis excludes the first three years of post-communist economic transformation when these countries experienced particularly profound macroeconomic shocks.
4. 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.
5. The LSDV models with the Kiviet correction were run using a bootstrap method with 50 and 500 repetitions. Estimates are obtainable upon request.
6. Unit root tests were run with one lag and two lags, with and without drift, with and without trend, and with and without the demean option.
7. This is not the case in FGLS models where the coefficients on EPL are negatively signed, albeit still insignificant.
8. 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.
9. The interactions are specified as products of deviations of institutions from their sample mean. More formally, when a single interaction between institutions X^k and X^h is included, the term $\sum_n \delta_n \nu_{n,it}$ from equation (1) becomes $\delta_{k,h}\left(X_{it}^k - \bar{X}^k\right)\left(X_{it}^h - \bar{X}^h\right)$. Accordingly, the partial derivative of unemployment with respect to X^k would be $\partial U/\partial X^k = \gamma_k + \delta_{kh}\left(X_{it}^h - \bar{X}^h\right)$. When δ_{kh} is negative, the marginal unemployment effect of institution X^k is larger, the lower the value of (i.e. the more liberal) X^h. Put differently, the lower X^h, the greater the potential gain from reforms liberalizing X^k (see Bassanini and Duval 2009: 46).
10. Since this estimator is designed for small T panels, I follow Bassanini and Duval (2009) and estimate these models on five-year averaged data.
11. 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 and firms are not as involved in skill formation as they are in Germany, Sweden is not a general skills system (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).

12. This value is obtained by dividing the parameter for the lagged level of EPR by minus the parameter for the lagged dependent variable.
13. Daveri and Tabellini (2000) advance a similar argument with respect to the effects of labour taxes on unemployment. Their findings show that the consequences of taxes are not the same everywhere, and their impact depends on other institutions, such as the nature of wage setting arrangements or union centralization.

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