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Before It Gets Better: The Short-Term Employment Costs of Structural Reforms

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Before It Gets Better: The Short-Term Employment Costs of Structural Reforms[1]

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Abstract: We exploit long time series of industry-level data in a group of OECD countries to analyze the short-term labor market effects of reforms lowering barriers to entry and dismissal costs. Our estimates show that both policies induce non-negligible transitory employment losses, a result that is confirmed by complementary evidence from case studies of three recently implemented EPL reforms. The strength of these effects varies depending on the underlying industry and labor market structure, and on cyclical conditions: the employment cost of deregulation is higher in economic downturns, negligible in good times. These findings prove robust to a set of specification and sensitivity checks, and are confirmed after standard reverse causality and falsification tests.

Keywords: Employment losses, EPL, Product market regulation, Industry data.

En attendant d'aller mieux : Les coûts à court-terme des réformes structurelles sur l'emploi

Abstract : Nous étudions des séries temporelles longues de données sectorielles pour un groupe de pays de l'OCDE, afin d'analyser les effets à court terme sur le marché du travail des réformes visant à limiter les barrières à l'entrée et le coût des licenciements. Nos estimations montrent que ces deux mesures entraînent des pertes d'emploi transitoires non négligeables, résultat confirmé par des analyses complémentaires tirées d'études de cas portant sur trois réformes récemment mises en œuvre de la législation sur la protection de l'emploi. L'intensité de ces effets varie en fonction de la structure du secteur et du marché du travail ainsi que de la conjoncture : le coût de la déréglementation est plus élevé si elle est mise en œuvre pendant une phase économique descendante, alors qu'il est nul en phase ascendante. Ces résultats ne sont pas sensibles à des nombreux changements de spécification et ils sont confirmés par des tests classiques de falsification et causalité inversé.

Mots-clefs : Pertes d'emploi, Législation sur la protection de l'emploi, Réglementation des marchés des produits, Données sectorielles.

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1. Introduction

Structural reforms of product and labour markets feature prominently in the political agenda to boost and sustain economic growth in the long run. And yet, flexibility enhancing reforms often involve significant reallocation of resources across firms and sectors that may entail costly adjustments, especially in the labour market. Intuitively, net employment would react reflecting two juxtaposed and asynchronous effects on inflows and outflows of labor. Firms operating in reformed markets are likely to respond immediately and in the direction of terminating job positions that are (likely to become) no longer profitable. By contrast, the positive contributions on flows into labour would be slowed by frictions, uncertainty or time-to-build delaying the processes of hiring, firm entry and innovation. Determining whether the balance between these forces implies employment or wage losses on average (and for how long) is a relevant and yet largely unexplored empirical question.

As highlighted in a recent survey of the literature by Boeri et al. (2015), most studies focus on country (or country-industry) specific reforms and analyze their long run impact through steady-state comparisons. Evidence on the short-term consequences of reforms can at best be extrapolated from the small sub-set of these works using a sufficiently brief time horizon. Such results are scattered, however, and do not allow addressing urgent policy questions. If certain reforms entail short run costs, one such question is whether they should be implemented during a recession (when their urgency often becomes more evident and political opposition is weaker; see e.g. Williamson, 1994; Rodrik, 1996) or rather timed to accompany an economic upswing (when job creation is stronger and short-term costs potentially lower). Similarly, it is important to understand whether the initial losses depend on the characteristics of the reformed market (e.g. the diffusion of temporary contracts in the case of reforms of Employment Protection Legislation – EPL hereafter, or the weight of large incumbents in that of reforms of Product Market Regulation – PMR). More generally, a comprehensive assessment of the short-term labour market consequences of reforms would help addressing the usually strong resistance faced by measures that are commonly understood to foster growth and jobs creation.

This paper exploits long time series of cross-country industry data to quantify the short term labour market effects of two important types of structural reforms: those reducing barriers to entry in product markets, and those introducing more flexibility in the legislation governing the dismissals of workers on regular (open-ended) contracts. The analysis contributes to the existing literature in several ways. First, in contrast with studies focusing on specific reforms in a given country, our cross-country focus allows examining the average, as opposed to country-specific, impact of policies that crucially alter the economic environment firms operate in. We study the interaction of both measures with the business cycle, and provide evidence on their interplay with the degree of labor market segmentation and the competitive situation in product markets. Finally, we extensively test whether the average results reflect the particular relevance of some country or specific sub-periods (e.g. the Great Recession).

The second contribution stems from analyzing these issues within a homogeneous empirical framework allowing to account for the potentially confounding role of commonly omitted aggregate shocks. Unlike most cross-country studies, our estimates control for, in particular, business cycle conditions (e.g. the possibility that both employment and regulation react to a country-specific recession), and industry specific shocks (e.g. driven technological change or shocks to demand). We also address and discuss the severity of concerns arising from simultaneity and reverse causality.

Our findings suggest that both product and labor market reforms can entail sizable short term losses in terms of employment and, to a lesser extent, wages. Significantly lowering entry barriers in network industries (energy, transportation and communication industries) induce industry employment falling below the pre-reform level during the first three to four years, with a maximum fall ranging between 1.2%-2% depending on the specification.[1] The employment loss is more pronounced when reforms are implemented during an economic downturn, defined as a period in which the product is growing (but not necessarily located) below potential.

In the case of EPL, aggregate employment estimated to fall by nearly 0.5% in the year following the average flexibility-enhancing reform of dismissal regulation.[2] The loss increases to 0.7% for reforms implemented during an economic downturn, and in excess of 1% in countries where temporary contracts are used less intensively. By contrast, employment losses are negligible in countries with significant labour market dualism. This latter finding is remarkable as those are the countries that can expect the greatest long run benefits from such reforms, due to their impact in reducing the relative use of fixed term contracts (see e.g. Lepage-Sautier et al 2013).

Evidence from country case studies and exploiting micro-data confirms the insights from the aggregate analysis of employment protection reforms. These entailed heavy employment losses in countries experiencing an economic downturn (e.g. Estonia in 2009) and where lower in those characterized by high labour market segmentation (Spain in 2012 and Slovenia in 2013).

The following section briefly details how our analysis and results relate to the existing literature. Section 3 details our empirical methodology to quantify the transitory consequences of product and labor market reforms and section 4 presents all our core findings and checks their robustness. Section 5 concludes.

2. Background and related literature

Product or labor market reforms entailing significant adjustment costs in the short run is compatible with well-known models in industrial organization and labour economics. The easiest example is probably that of standard search and matching frameworks a la Mortensen and Pissarides (1999), where flexibility enhancing reforms of employment protection can be introduced as a way to lower termination costs. Such policy will have negative short term employment effects because of its asymmetric impact on firing and hiring flows: while the destruction of inefficient job matches (those yielding negative revenue but not being destroyed to save on the costs of adjustment) would be immediate, the frictional hiring process implies that newly-profitable job vacancies are filled slowly(see Cahuc and Zylberberg, 2004; Cacciatore and Fiori, 2016; and Cacciatore et al., 2016).

The reasoning is similar in the case of product market liberalizations. Recent studies showed that, in markets dominated by large firms, incumbents react to increased competitive pressures reducing prices and increasing efficiency, even before new firms enter the market (e.g. Goolsbee and Syverson, 2008; Bridgman et al., 2011; and Brueckner et al., 2013). Whenever such re-organization implies that large firms re-organize and downsize, the initial impact of pro-competitive reforms on aggregate (e.g. industry-level) employment could be negative. By contrast, the positive employment contribution of new firms takes long to materialize as successful entrants expand only gradually.

[1] This result refers to a hypothetical reform implying a fall in the underlying regulation index corresponding to second tercile of the distribution or reforms implemented in two consecutive years. See section 4.1 for more details. For comparison, employment in the sample grew by 0.3% per year, on average.

[2] One implying a fall in the corresponding regulation index equal to the sample average

Against this theoretical background, very little is known empirically as to the average relevance of these potential costs across countries (see Boeri et al 2015 for a survey of the literature). One reason is that it is very difficult to control for an exhaustive list of confounding factors in macro panels, the more so when the specification need to include many lags as in the case of short-term effects estimates. Most empirical work has therefore focused on quantifying their steady-state (i.e. long-run) effects, and typically does not account for confounding country-specific shocks.[3] Some analyses exploit reform episodes specific to a country. Few of these steady-state studies, those focusing on a short time window around the reform, provide some indirect evidence on its short run effect. In the case of EPL, the results confirm that the impact of easing dismissal restrictions on employment and worker flows tend to become more positive (less negative) over time (von Below and Thoursie, 2010; Malk, 2013; Martins, 2009).[4] However, such evidence is scattered, difficult to generalize and does not allow for a more comprehensive assessment of the transitory impact of reforms, including for example their interaction with the business cycle or the different characteristics of national labor markets. One contribution of this paper is to address the main estimation issues in cross country comparisons using industry data and running difference-in-difference experiments exploiting variation in industry exposure to the policy. Specifically, we will compare employment responses to flexibility enhancing EPL reforms across industries with different propensities to make staff adjustments, for whom dismissal restrictions binds with different intensities.

In the case of PMR reforms, the available evidence is largely confined to one specific industry, the retail sector, where liberalization reforms are found to have no negative employment impact (Bertrand and Kramarz, 2002; Viviano, 2008; Skuterud, 2005; Burda and Weil, 2005). This interesting result, however, likely reflects the particular competitive setting in that industry, with deregulation often implying the entry of large, efficient competitors whereas incumbents are too small to strategically anticipate entry by cutting staffing. Employment dynamics might be different in more concentrated markets or cases in which regulation rather shelters large dominant players. To test whether this is the case, we look at the reduction of entry barriers in three network industries characterized by the presence of large incumbent firms: Energy (electricity and gas), Transport (air, rail, road transport) and Communications (post and telecommunications).[5]

A concomitant work by Bouis et al (2016) partly overlaps with the present analysis in looking at the short term consequences of reforming entry regulation in network industries. They restrict to the case of large reforms, adopt a slightly different empirical specification and mainly focus on variables as real value added, prices and productivity (see Section 4.1 for more details). Their results indicate strong positive short term effects on these outcomes; when looking at

[3] Early cross-country analyses focusing on long run impact of reforms include, for example, Nickell and Layard, 1999; Nicoletti and Scarpetta, 2003; Alesina et al, 2005; Koeniger, 2005; Inklaar et al 2008; Aghion et al, 2009; Bassanini and Duval, 2009.

[4] For example, von Below and Thoursie (2010) show that separations increased faster than hiring in the first two years following the Swedish 2001 reform. Malk (2013) finds no significant effect on hiring in the two years following the 2009 Estonian reform, while separations increased. On the other hand, Martins (2009) finds no effect on separations while the effects on hiring materialised only 3 years after a 1989 Portuguese reform. Similarly, Behaghel et al. (2008) find that a 1992 French reform on EPL for older workers had an effect on older-men transitions between unemployment and employment that increased over time.

[5] Plausibly, the results of the analysis apply to other concentrated industries, or to instances in which large incumbents are sheltered from competition by other impediments, as cross-border trade and investment barriers (as in business and professional services or the construction sector, due to regulations such as the requirement that foreign firms work through local partners). Similarly, significant barriers to domestic and cross national competition exist in public procurement, which accounts for a large fraction of public spending in most advanced economies.

employment, they find little evidence of negative effects from product market deregulation, irrespective of the underlying business cycle condition.

3. The empirical setting for estimating employment responses to structural reforms

The analysis leverages on industry level data from EUKLEMS and the OECD STAN datasets covering 23 advanced economies and up to 37 years (1975-2012), and mainly adopts gross or net job losses (gains) as the metric to quantify costs (benefits) of reforms.[6] These data are combined with long time series of detailed indicators of barriers to entry in product markets and of regulations on the dismissal of regular workers, available from the OECD. Specifically, from the OECD PMR database we constructed a measure of changes in entry barriers in three network industries between 1975 and 2012: Energy (electricity and gas), Transport (air, rail, road transport) and Communications (post and telecommunications).[7] Labor market reforms are quantified by changes in the indicator of stringency of regulation for individual dismissals of workers on permanent contracts, available between 1985 and 2012 from the OECD database on Employment Protection Legislation. The data are detailed in Appendix A2, and summary statistics of the main variables are shown in Table A9.[8]

To estimate the behavior of labour market outcomes following changes in regulation we adopt the time-series empirical model used to compute unbiased impulse response functions to some event – the so-called local projection estimator (Jorda, 2005; Teulings and Zubanov, 2014). The model runs a system of simultaneous equations of the outcome variable at various horizons starting at time t, on the reform variable at time t and a set of controls. The sequence of coefficients estimated on the reform variable at each horizon provides a semi-parametric estimate of the impulse response function. The supplementary Appendix C provides more details on the methodology. In what follow we detail its application to product and labor market reforms.

3.1. The case of product market reforms in network industries

To establish whether lowering barriers to entry has negative short term impacts on employment, a useful starting point is the dynamic equation:

$$\Delta E_{cit} = \beta_0 \Delta BE_{cit} + \sum_{k=1}^{T} (\beta_k \Delta BE_{cit-k} + \delta_k \Delta E_{cit-k}) + X_{cit}\gamma + D_{ct} + D_{it} + D_{ci} + \varepsilon_{cit} \qquad (1)$$

where $\Delta \mathrm{E}_{c,i,t} = \ln \mathrm{L}_{c,i,t} - \ln \mathrm{L}_{c,i,t-1}$ measures year-on-year employment growth in a network industry *i*, country *c* and time *t*, $\Delta \mathrm{BE}_{cit}$ is the change in regulation at *t* and *ε* is an error

[6] The base sample covers annual data from EUKLEMS for the period 1975-2007 with a specific industry breakdown between 1 and 2-digits of the ISIC rev.3 classification. For those countries for which OECD STAN data are available, the time coverage of the sample is extended to the period 1975-2012 by collating EUKLEMS data with data from the most recent version of OECD STAN.

[7] The analysis exploits the ETCR section of the OECD PMR database (see Koske et al., 2015). It focuses on the sub-indexes capturing legislated entry barriers and vertical integration (when applicable), and varying from 0 (lowest regulation) to 6 (highest regulation). Looking at the time patterns of the indicators suggest that product markets have been almost exclusively subject to deregulating reforms, with rare episodes of re-regulation.

[8] The analysis of the effect of barriers to entry mainly focusses on total employment, since reliable EUKLEMS data for dependent employment are not available for most countries before the mid-1980s. Results are however robust to replacing total employment with wage and salary employment as dependent variable (see Table A2).

term.[9] This autoregressive specification conveniently account for delayed effects of reforms through the parameters (β_k), as well as for the possible persistence of employment changes.[10] The vector of controls *X* accounts for the potentially confounding role of other institutions or forms of regulation, and matrix *D* contains a set of bi-dimensional dummies controlling for country-industry, industry-year and country-year specific shocks, respectively.

Identification of the parameters of interest (*βs*) requires assuming that product market reforms are exogenous to unobserved determinants of employment changes. The set of bi-dimensional dummies allows accounting for a number of potentially relevant confounding factors as (i) country-specific shocks to employment growth common across industries (e.g. the business cycle and economy-wide policy reforms); (ii) industry-specific shocks to employment growth common across countries (such as those related to the evolution of technology and global demand); and (iii) country-industry specific linear trends in the evolution of employment (e.g. due to changes in the long-run patterns of international specialisation). Conditional on this large set of controls, identification hinges on comparing employment growth in a reform year across industries and over time.

Even so, the identifying assumptions would be violated if industry reforms respond to current or past shocks to industry employment. For example, resistance to reform might be milder following large negative employment shocks. Or, on the contrary, when employment is on the rise and organisational changes are less likely to threaten the jobs of insiders. Our strategy to address these concerns is twofold. First, we will perform alternative tests of the severity of these (reverse-causality) issues. One consists in augmenting (1) with forward terms of changes in regulation. Finding that future reforms affects current employment would provide evidence of reverse causality. Another consist in performing Granger-causality tests, which amount to regressing the change in regulation at time *t* (ΔBE) on lagged employment changes, and testing that the latter have no individual or cumulative impact. Second, we will compute (and largely rely on) impulse response functions that do not factor in the contemporaneous effect (β_0), which is most likely to reflect simultaneity biases.

To illustrate (see the supplementary Appendix C for details) we implement the local projection method estimating a system of h equations. In each equation the dependent variable is yearly employment growth over a h-year horizon (i.e. $\Delta E_{c,i,t}$, $\Delta E_{c,i,t+1}$, …, $\Delta E_{c,i,t+h}$ - the first regression corresponding to specification (1)). The employment response to a unitary change in the variable BE_t at any period $t+\tau$ (the impulse response function, IR(τ)) is given by the sequence of

[9] About 95% of changes in the OECD index of entry barriers in network industries (ΔBE) have a negative sign. The estimated *β*s can therefore be associated to product market deregulation.

[10] In all estimations, standard errors are clustered at the country-industry level, to account for serial correlation in the residual. The number of lags *T* is chosen based on statistical criteria as the Bayesian's (BIC) or Akaike's (AIC). Note that, if the parameters δ_k are not of interest the equation (1) can be rewritten substituting recursively all terms of the lagged dependent variable, leading to an infinite series of ΔBE terms on the right-hand side, that are approximated with: $\Delta E_{cit} = \sum_{k=0}^{M} \beta_k \Delta BE_{cit-k} + X_{cit}\gamma + D_{ct} + D_{it} + D_{ci} + \varepsilon_{cit}$, with again *M* set on the basis of BIC or AIC statistics (see e.g. Teulings and Zubanov, 2014). This would allow addressing the fact that, as shown by Nickell (1981), the coefficients of the lagged dependent variable in equations (1) are usually biased, and might yield biased estimates of the coefficients of interest if covariates are correlated. The disadvantage from eliminating the lagged dependent value is the loss of efficiency. To check the relevance of these issues, we will compare evidence from both specifications.

the coefficients on the reform variable (β_0) as estimated in each equation: $IR(\tau) = \sum_{h=0}^{\tau} \beta_{0h}$.[11] Our baseline estimate of the IR function discards the contemporaneous effect, setting $\beta_{00} = 0$.

To test whether the impact of deregulation varies over the business cycle, specification (1) is augmented to include interactions between the change in regulation (*ΔBE*) and the change in the output gap (*ΔOG*). Because the output gap is defined as the difference between actual and potential output (as drawn from the OECD Economic Outlook database), *ΔOG* takes negative values when the economy is contracting. Hence, for example, a negative sign on this interaction term would suggest that the short run impact of deregulation on employment levels is more negative when economic activity is contracting while it is less harmful during recoveries. Clearly, specification (1) also allows examining the impact of deregulation on other industry labour market outcomes such as wages.

3.2. The case of dismissal regulation

The specification adopted in the case of EPL is very similar to (1). The main independent variable, however, has to be specified differently to reflect three core differences in the underlying policy of interest. The first is that, unlike the case of PMR, EPL indicators feature positive and negative changes: many countries implemented protection-raising EPL reforms during the observational windows. Our focus on deregulating reforms requires allowing for asymmetric employment responses to reforms moving in opposite direction. We therefore separately estimate the consequences of flexibility-enhancing reforms (FE_{ct}, reform episodes implying $\Delta EPL_{ct} < 0$) and protection-raising reforms (PR_{ct}, when $\Delta EPL_{ct} > 0$). The second difference is that changes in the continuous EPL index are typically small, rare and measured with significant error (see OECD, 2013). Therefore, the employment impact of EPL reforms is better captured by indicator rather than by continuous variables. Accordingly we define FE as a dummy variable taking value 1 when the EPL indicator decreases and 0 otherwise (and proceed similarly for PR). [12]

The third difference is that, unlike the case of barriers to entry, employment protection legislation is not industry-specific. Yet, analysing its labour market impact exploiting industry-data has several advantages in terms of identification (see e.g. Bassanini et al., 2009; Cingano et al., 2010; Haltiwanger et al., 2014; Griffith and Macartney, 2014; and Caroli and Godard, 2016). Following the literature, we adopt the so-called interaction model, pioneered by Rajan and Zingales (1998), and write the variable of interest as $WFE_{ctj} = D_j * FE_{ct}$, interacting country-level changes in EPL with an industry-specific measure of exposure to the reform (D_j). The main intuition behind interaction models is that, if EPL reforms have any short term effects on employment, these should be stronger among firms that, in the absence of regulation, have greater propensity to make staff

[11] In the practical implementation of the estimator we follow Teulings and Zubanov, 2014 and include in matrix *X* a set of forward terms ($\sum_{f=1}^{h} \beta_{fh} \Delta BE_{ci,t+f}$) capturing the potentially confounding effects of reforms implemented after time *t*.

[12] All episodes of EPL reform in the baseline sample entail a change in the indicator of EPL stringency for regular contracts by less than 0.4 points (in absolute terms) except for the 1994 Spanish reform which is associated to a large fall in the indicator (1.19 points). As discussed in OECD (2013) this fall clearly overstates the actual extent of the Spanish reform (due to the suppression of the procedure for administrative authorization of dismissals only in the case of individual redundancies). This suggests that it is crucial to check the robustness of any result obtained using the size of EPL reforms treating that reform as an outlier. In the Appendix we show that results similar to our baseline's can be obtained focusing on the restricted and continuous version of the specification ($\Delta E_{cjt} = \theta_0 D_j \Delta EPL_{ct} + \sum_{k=1}^{T} \left(\theta_k D_j \Delta EPL_{ct-k} + \rho_k \Delta E_{cjt-k}\right) + X_{cjt}\delta + \nu_{ct} + \nu_{jt} + \nu_{cj} + \xi_{cjt}$) but they are sensitive to the inclusion of Spain in the sample (see Table A4).

adjustments on the external labour market. Accordingly, for each industry D_j is measured by the dismissal rate of the corresponding industries in the US. The United States is the least regulated country in the OECD as regards legislation for individual dismissals; using the US as benchmark therefore avoids possible estimation bias resulting from a correlation between EPL stringency and the cross-industry dismissal distribution.

The estimation model (written so as to emphasize flexibility-enhancing reforms) becomes:

$$\Delta E_{cjt} = \theta_0 D_j FE_{ct} + \sum_{k=1}^{T} \left(\theta_k D_j FE_{ct-k} + \rho_k \Delta E_{cjt-k} \right) + W_{cjt}\delta + \nu_{ct} + \nu_{jt} + \nu_{cj} + \xi_{cjt} \qquad (2)$$

where matrix W includes the current and lagged dummies for PR reforms ($\sum_{k=0}^{T}\left(\mu_k D_j PR_{ct-k}\right)$), as well as other controls to be detailed. Note that using indicator variables implies that the θ coefficients can be interpreted as the employment responses to a reform of historically average extensiveness, as measured by the average negative changes of indicator across all reform episodes of the sample.

As for the case of PMR, the specification can be extended to quantify the interaction between EPL reform and business cycle. Perhaps more relevantly, it allows assessing whether greater flexibility in dismissals regulation has differential employment impacts depending on the degree of segmentation (duality) of the underlying labor market. There are in fact good theoretical reasons to expect that this type of reform has only a limited impact on job destruction in the short-term in dual labour markets, since temporary contracts are likely to be used to fill volatile positions (i.e. at risk of destruction when the firm is hit by a negative shock) while the incentive to terminate these contracts is unaffected by the reform.

Unbiased estimates of the θ coefficients will allow plotting the employment responses to flexibility-enhancing reforms, as we will detail in section 4.2.[13] Note that the identification issues discussed for the case of PMR are likely attenuated by the use of specification (2). For example, it seems safer to assume that economy-wide reforms as those of employment protection legislation are not driven by idiosyncratic fluctuations of specific industries. Omitted economy-wide labour market institutions (as unemployment benefit generosity, labour tax wedge, collective bargaining and regulation for hiring on temporary contracts)[14] are also unlikely to drive or contaminate our results. In fact, their aggregate impact is absorbed by country-year fixed effects, and interacting them with dismissal intensity (D_j) shows that their current or lagged changes have no explanatory power for changes in employment.

4. Empirical evidence on the short term consequences of structural reforms

4.1 Lowering barriers to entry in product markets

Table 1 presents baseline estimates of the relationship between year-on-year changes in regulation of entry barriers and log employment, based on the alternative equations illustrated in

[13] Specification (2) only allows for direct quantifications of the differential effects of EPL reforms (on EPL binding -- high-dismissal- industries relative to non-binding industries. The conditions and procedures required to inferring a lower bound to the aggregate effect of EPL reforms requires are discussed in e.g. Bassanini et al. (2009) and Bassanini and Garnero (2013), and will be briefly summarized in our case in section 4.2.

[14] These policies and institutions are typically included in aggregate unemployment studies (e.g. Blanchard and Wolfers, 2000, Nickell et al., 2005, Bassanini and Duval, 2009).

the previous section. Column 1 focuses on the simple simultaneous association. The estimated coefficient suggests that, controlling for aggregate effects and industry-specific trends, a decrease in the indicator of stringency of barriers to entry of 1 point is associated with a decrease in employment of 0.66%. The short-term loss is not insignificant from an economic point of view if compared with the average growth rate of employment in the sample between 1975 and 2007 (0.3%).[15] A similar result is obtained if one lags the regulatory variable one year (col. 2): a decrease in the indicator of stringency of barriers to entry is associated to a decrease in employment of 0.7% the following year. These findings are essentially unaltered if more dynamic models are estimated (where the structure of lags is chosen by minimizing the BIC statistic). Results in columns 3 and 4 focus on a 2-lag versions of (1) - the first imposing $\beta_0 = 0$. Finally, column 5 shows a 3-lag specification of the regression model obtained by recursive substitution of the lags of ΔE_{cit} (see footnote 10).

The above estimates imply that the average employment response to lower barriers to entry in network industries is U-shaped, with the loss reaching its maximum 3 years after the reform, and beginning being reabsorbed afterwards. Figure 1, Panel A plots the response to a reform that lowered the OECD regulation index (which ranges from 0 to 6) by 1 point,[16] based on the coefficients estimated in column 4. Industry employment would be around 1.2 percentage points below its initial level in the third year following that reform.

Importantly, the above responses are plotted without factoring in the contemporaneous effect of changes in barriers to entry, a conservative assumption motivated by the uncertainty on the reliability of parameter β_{0h}. Accounting for of *all* estimated βs, as in Figure 1, Panel B, would yield to an aggregate employment fall of nearly 2% (as opposed to 1.2%) three years after the reform. Importantly, the analysis does not provide evidence that the short-term employment adjustments are accompanied by a significant fall in average hourly wages (See Appendix figure A1).

The U-shaped pattern of employment is consistent with the idea that labour market outcomes following the removal of entry barriers in network industries reflect two offsetting but asynchronous forces: the immediate re-organization of (large) incumbents and the gradual expansion of successful entrants. This interpretation is also consistent with evidence shown by Gal and Hijzen (2016) who find that deregulation in network industries depresses employment among large incumbents in the short run.

Interaction with the business cycle: Are these employment losses smaller or larger in an economic downturn? On the one hand, the contribution of deregulation to labor shedding could be marginal in a period of large job destruction. On the other hand, the high uncertainty characterizing downturns might significantly slow job creation, lowering the number of new firms or the pace at which they grow.[17] Allowing the employment impact of deregulation to vary along the cycle provides supportive evidence for the latter hypothesis, as is illustrated by Figure 2 (see Appendix Table A1 for the full set of econometric results). The two panels plot the employment response to a reform implemented when the growth rate of the output gap is, respectively, larger (upturns) and

[15] Restricting to more recent periods, employment growth amounted to 0.014% between 1990 and 2007 and to -0.039% if one includes the Great Recession, i.e. the period 2008 to 2012

[16] For reference, more than one-sixth of the reform episodes in the sample implied a fall of the index of at least one point in one year. In one third of the reform episodes in the sample a one point fall is obtained cumulating changes over two consecutive years. See the supplementary appendix A2 for more details.

[17] These supply side responses might be further worsened by negative interactions with aggregate demand. Recent model-based simulations suggest, for example, that reforms implemented when monetary policy has hit the zero lower bound (ZLB) may fuel expectations of prolonged deflation, thus lowering aggregate demand by increasing the real interest rate (Eggertsson et al 2014).

smaller (downturns) than potential output growth. Comparing these two scenarios suggests more pronounced employment losses for pro-competitive reforms implemented during downswings than during an expansionary phase. In both cases, industry employment reaches a minimum three years after the reform, but this is 1.4% below the level that would have been observed without the reform during a downturn. In the upturn scenario the fall is smaller and not statistically significant.

Robustness and specification tests: As discussed in section 3.1, the main threat to identification of equation (1) is the possibility that reforms are implemented in response to changes in industry employment. Tables 2 and Table 3 present alternative tests of the relevance of reverse causality concerns. First, the baseline model is re-estimated including one forward term – that is the change in regulation in the following year ($\Delta BE_{ci,t+1}$). If reforms react to employment changes with some delay, one would expect this term to be significant (and the estimated effect of reforms to be affected). The results reported in Table 2, however, do not support this hypothesis. Table 3 presents a more formal test of whether current changes in barriers to entry are affected by past changes in employment ("Granger-causality"). Specifically, the table reports F-tests for the (joint and separate) significance of parameters π_1 and π_2, estimated in model like: $\Delta BE_{cit} = \sum_{k=1}^{2} \pi_k \Delta E_{ci,t-k} + \sum_{l=1}^{2} \varphi_l \Delta BE_{ci,t-l} + \mathbb{D} + \omega_{cit}$. Consistent with the previous findings, past employment changes do not have a significant impact on current changes in regulation (neither separately nor cumulatively).

The employment response pattern of Figure 1 is confirmed across a number of sensitivity and specification checks, summarized in Table 4. Specifically, in columns 2 to 4 the regression underlying the response function (col. 4 in Table 1) is augmented to include industry-level confounders such as the growth in intermediate inputs and real value added. Column 5 accounts for the potentially confounding role of changes in the degree of public ownership - another dimension of regulation captured by the OECD indicators, and column 6 accounts for potential spillover effects from reforms in other industries (e.g. the possibility that lowering barriers to entry in the energy market might affect employment dynamics in the transport industry).[18] Column 7 changes the definition of the dependent variable (i.e. using salaried as opposed to total employment), and column 8 uses un-weighted rather than weighted regressions. Finally the results are robust to variations of sample of countries, as shown in detail in Appendix Figure A2), and to extending the time window to include the Great Recession years (see Table A2).[19]

A concomitant work by Bouis et al (2016) also looks at the short term consequences of reforming entry regulation in network industries. Their preferred employment specification shows negative effects on impact, which are just marginally below standard levels of statistical significance, however, and reabsorbed already in the first year. More generally, they find little evidence of negative effects from product market deregulation, irrespective of the underlying business cycle condition. While applying the same methodology (the local projection estimator) their approach differs from ours in two main respects. First, the authors restrict to large reforms of

[18] Spillover effects are captured by means of a weighted measure of regulation in "other" network industries: $WBE_{dit} = \sum_{-i} Exp_{i,-i} * BE_{c,-i,t}$, where $Exp_{i,-i}$ are coefficients from the US Inverse Leontief Matrix measuring how many units of input *-i* (e.g. energy) have to be produced (at any stage of the value chain) to produce one additional unit for final demand in network industry *i* (transport).

[19] The analysis also explored whether the strength of a given reform varies with the *level* of regulation (e.g. is stronger in high than low regulated countries). However, interacting the change in regulation (ΔBE_{ci}) with an indicator for regulation being above/below the sample median (the year before the reform) attracted highly non-significant coefficients throughout the core specifications. A further extension showed that the employment response does not vary significantly across the specific network industry implementing the reform (i.e. whether in Energy, Transport or Communication).

entry regulation. These are identified examining various editions of country-specific OECD Economic Surveys over a long time period, looking evidence of legislative action on these domains (similarly to the "narrative approach" first proposed by Romer and Romer, 2004). Second, they adopt a somewhat less stringent specification, which includes only industry-specific linear trends, as opposed to more flexible industry-year dummies. Both departures contribute to explain the differences with the results reported here.

4.2 Lowering dismissal costs on regular contracts

Flexibility-enhancing reforms of dismissal legislation are also estimated to significantly lower employment in the short run. Table 5 report the coefficients estimated from alternative specifications of the interaction model (2), which are all quantitatively similar. Column 1 refers to the preferred specification with 3 lags of each variable, while Column 2 refer to a more parsimonious version with 2 lags only. In columns 3 and 4 the two models are replicated on the extended estimation sample, matching EUKLEMS data to STAN to cover the longer period 1985-2012.

A deregulation reform of average historical size, picked up by the dummy variable $FE_{c,t}$, is estimated to lower (log) employment in industry j by $\Delta E_{cj} = -0.3 * D_j$ on impact. In the data, industry dismissal rates D_j vary between around 2% (in Electricity, gas and water supply) and around 9% (in Textiles, leather and footwear), with an interquartile range of nearly 1.7 percentage points (the differential between wood manufactures and motor vehicles). Hence, the estimated coefficient implies that employment in the dismissal-intensive industry (wood) would fall by about 0.5 percentage points relative to motor vehicles in the reform year. The table also suggests this negative effect would persist in the year following the reform and to start reverting afterwards.

Under further assumptions, the coefficients estimated in Table 6 can be used to infer the economy-wide loss from flexibility-enhancing EPL reforms, obtained aggregating industry specific losses: $\widehat{\Delta E}_c = \sum_j Sh_{jc} * \Delta\hat{E}_{cj}$, where Sh_{jc} is the employment share of industry j (see e.g. Bassanini et al., 2009, and Bassanini and Garnero, 2013). In particular, a lower bound can be backed up if the sign of the effect in the control (the low dismissal) industries is either zero or the same as in high dismissal industries.[20] Importantly, in this application the aggregation is made imposing the conservative assumption that reforms have no effect on all industries with dismissal rate below the first quartile of the D_j distribution (i.e. for these industries we impose $\widehat{\Delta E}_{cj} = 0$).[21]

[20] In principle, this assumption would be violated if, for example, by increasing dismissals in EPL-binding industries EPL reforms expanded the supply of labour in other industries, whose employment would therefore grow. In practice, however, these general–equilibrium effects tend to be negligible. Using multiple identification strategies, Bassanini and Garnero (2013) find no long-run effect of EPL for regular contracts on either job-to-jobless separations or job-to-job ones entailing a change of industry. Replicating the analysis of equation (2) using Bassanini and Garnero's data on separations suggests that 73% of the increase in separations following a flexibility-enhancing EPL reforms is due to job-to-job separations within the same industry, while only 6% is due to transitions involving an industry change (transitions are defined over an one-year horizon).

[21] As a further, indirect test of whether general-equilibrium mechanisms offset the direct impact of EPL reforms, in the following section 4.3 we compare the signs of the aggregate employment effects computed with the interaction model (2) with those on estimated using a regression-discontinuity approach on high-frequency aggregate unemployment data. If general equilibrium mechanisms were relevant, the two exercises should lead to very different conclusions on the labour market consequences of EPL reforms. This does not turn out to be the case.

The economy-wide short-term effect computed following this procedure is plotted in Panel A of Figure 3. Aggregate employment is estimated to fall by 0.32% on impact, reach a cumulative loss of 0.48% about one year after the reform, and start recovering afterwards. Consistently with the idea that employment losses would tend to moderate wage claims, Panel B shows that EPL reforms also have small negative short-term consequences on average wages, which fall by 0.44% the year following the implementation of the policy.[22]

In the interaction model (2), identification requires that no omitted factor impacts on performance in a way that varies monotonically with industry-specific US dismissal rates. This assumption can be tested in the case of relevant observable policies and institutions. Appendix Table A3 shows results obtained by re-estimating equation (2) replacing *FE* and *PR* with dummies for reforms of the following labour market policies and institutions: unemployment benefit, average gross replacement rate, collective bargaining coverage, collective bargaining coordination, the average labour tax wedge and the index of employment protection legislation on temporary contracts (EPT).[23] As expected, none of the interactions of these reform dummies with the US dismissal rate is significantly associated with changes in employment, which provides empirical support for the identification assumption.

The Appendix also reports a host of robustness checks of the baseline estimates including estimating the restricted and continuous specification ($\Delta \mathrm{E_{cjt}} = \theta_0 \mathrm{D_j} \Delta \mathrm{EPL_{ct}} + \sum_{\mathrm{k}=1}^{\mathrm{T}} \left(\theta_{\mathrm{k}} \mathrm{D_j} \Delta \mathrm{EPL_{ct-k}} + \rho_{\mathrm{k}} \Delta \mathrm{E_{cjt-k}} \right) + \mathrm{X_{cjt}} \delta + \nu_{\mathrm{ct}} + \nu_{\mathrm{jt}} + \nu_{\mathrm{cj}} + \xi_{\mathrm{cjt}}$), which is akin to that estimated in the case of PMR (see Table A4)[24] or changing the sample excluding countries one-by-one (see Appendix Figure A3).[25]

Interaction with business cycle and labor market dualism As suggested by basic models with adjustment costs, the employment losses from EPL reforms are larger and longer-lasting in downturns than upturns.[26] This is again obtained by interacting the reform variable with the year-on-year change in the output gap, measured at the time when the reform was implemented (see appendix Table A6, Panel A). Results suggest that a reform implemented when the output gap falls – i.e. in a downturn – induces an employment contraction that is twice as large as in the case of upturns, and persists for at least 2 years before becoming statistically insignificant (Figure 4). Taking estimates at face value, an average reform undertaken when the output gap is falling by 1 percentage point induces employment to fall by 0.7% after 2 years. Interestingly, a similar exercise interacting EPL reforms with the level (as opposed to the change) of the output gap yielded no significant results, suggesting that whether the economy is contracting or expanding matters more than its distance to potential. Put another way, reforms implemented when the

[22] The underlying specification is similar to (2) except for the introduction of controls for compositional effects adding the share of the low educated in the industry workforce and the contemporaneous change in industry employment in column 2. The corresponding estimation results are reported in Appendix Table A5.

[23] These variables are those typically included in aggregate unemployment studies (e.g. Blanchard and Wolfers, 2000, Nickell et al., 2005, Bassanini and Duval, 2009).

[24] This is contingent on accounting the 1994 Spanish reform as an outlier. As discussed in OECD (2013), the corresponding fall in the OECD indicator is an outlier (-1.19 points; the second-highest change being of -0.4 points) that clearly overstates the actual extent of the reform.

[25] The effect of the reform is remarkably stable except when removing of Spain or Netherlands (when it significantly increase), and Denmark or Slovakia (when it tends towards zero). Excluding simultaneously countries at both ends of the spectrum confirms the baseline result, however.

[26] Standard adjustment-cost models would suggest that the share of unprofitable jobs that survives only because of high firing costs is larger in downturns, leading to greater job destruction when these costs are lifted (see e.g. Cahuc and Zylberberg, 2004; and Cacciatore et al., 2016 for a recent contribution).

economy is starting to recover but still plagued by high cyclical unemployment are less harmful than reforms implemented when the output is above potential but the gap is falling.

The above evidence is consistent with the idea that firing costs induce employers to hoard labour in bad times. In dual labour markets, however, employers have a strong incentive to use fixed-term contracts for positions that become unprofitable when the firm is hit by a negative idiosyncratic shock. Because flexibility-enhancing EPL reforms affecting regulations for regular contracts do not alter the incentive to terminate temporary contracts, their short-term impact on job destruction can be expected to be limited in very segmented labour markets.[27] The impulse response functions plotted in Figure 5 suggests that this is indeed the case. EPL reforms have negligible effects if the share is around 15% (that is 5% above the sample median, see Panel A) and sizable (-1.1% one year after the reform, Panel B) if it is around 5%. Detailed estimation results are reported in Appendix Table A6, panel B.

4.3 Supportive evidence from three case studies

The labour market reforms implemented in Estonia (July 2009), Spain (February 2012) and Slovenia (April 2013) allow testing the previous results by means of very different estimation approach and data. They all implied sizable, one-shot changes in EPL for regular contracts[28], thus lending themselves to a regression time-discontinuity approach on high-frequency data.[29] Moreover, they were implemented at different phases of the business cycle (the onset of a large downturn in Estonia, and at or close to the crisis trough in Slovenia and Spain), and in countries featuring different shares of fixed-term contracts (among the smallest in Estonia, and close to the largest in Slovenia and Spain) both in terms of stocks and as regards hiring patterns (Appendix Figure A4).

Based on the evidence presented in section 4.2, one would expect the labor market costs of the reform to be highest in Estonia than in Slovenia and Spain.

Table 7 presents results obtained estimating a regression-discontinuity model looking at average unemployment as the outcome variable. Conditional on observable controls and a 5th order polynomial time trend, the unemployment rate increased by nearly of 2 percentage points (or, of about 15%) in Estonia, against 0.5 points in the case of Slovenia; unemployment did not change in Spain.[30] Interestingly, most of the increase in unemployment was concentrated in the first year of implementation, becoming statistically insignificant in the second.

These results are confirmed if excluding 3 calendar months around (or up to 10 months before) the reform, to account for possible threshold manipulation – e.g. employers postponing dismissals between the announcement and implementation of the reform (see Appendix Table A7 for detailed results for each country). Moreover, placebo experiments fictitiously anticipating the date of the reform by 3 months suggest that the estimated coefficients are not capturing changes in omitted variables.

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[27] In the long run, however, flexibility-enhancing EPL reforms affecting regulations for regular, open-ended contracts are typically found to reduce dualism (see e.g. Lepage-Saucier et al., 2013)

[28] They are all included among the 10 largest episodes (in terms of changes in the corresponding OECD indicator) since 1998.

[29] In the case of Spain, the EPL reform was coupled with a simultaneous decentralisation of collective bargaining. The regression-discontinuity approach adopted here estimates, therefore, the joint effect of both reforms. By contrast, there were no major concomitant reforms in the other two countries.

[30] The findings are consistent with the evaluations of Malk (2013), who found that separations increase more than hires in Estonia immediately after the reform, and Izquierdo et al. (2013) and Puente and Font (2013), who showed that employment levels did not worsen after the Spanish reform.

In the case of Estonia, additional evidence can be obtained using other Baltic countries as a control group, estimating a difference-in-difference model (in particular Lithuania, see Malk, 2013).[31] To properly account for relevant compositional effects (e.g. due to the extremely different incidence of immigration in the two countries, immigrants being at higher risk of unemployment in recessions) individual data from the European Labour Force Survey are used to estimate a probit model on the joint sample. In the model, the probability of being unemployed in a given month is expressed as a function of a large set of individual and aggregate covariates and is allowed to diverge between the two countries in the aftermath of the reform.[32] Results show that the unemployment probability increased by 1.5-percentage-point the two years following the reform (see Appendix Table A8, and the corresponding note for details), a figure strikingly close to that estimated using aggregate unemployment figures and a simple regression-discontinuity design.

As in the previous case, these findings are robust to excluding observations close to the date of reform enforcement, controlling for polynomial time trends or including also Latvia in the control group (see Appendix Table A8). Moreover, a placebo experiment in which the date of the reform is fictitiously anticipated by 3 months yields an insignificant estimate coefficient.

5. Conclusion

We investigated the short term effects of reforms that ease anti-competitive product market regulation and employment protection legislation finding that both can entail significant short term employment losses. The losses are higher when the policies are introduced during downswings, when their political cost may appear lower, but much smaller and statistically insignificant when reforms are implemented during upswings. Moreover, reforms of dismissal legislation have no adverse effects in segmented labour markets – those where this type of reform is often considered to be the most needed (see e.g. Boeri and Jimeno, 2016). While not immune from estimation concerns, our core findings prove fairly robust to an ample set of tests, and confirm the idea that the adjustments triggered by flexibility enhancing reforms may be significantly hindered by product or labor market frictions.

Exploring more in detail the mechanics of such adjustments would be extremely important. Using detailed firm level data would allow, for example, studying employment decisions of both incumbents and entrants following market deregulation. It would also make possible to characterize the losers from the product and labour market reforms in terms of traits as age, skill, tenure at firm and start assessing the distributional implications of reforms, over and above their average effects. This would in turn help understanding which policy measures would be more effective in attenuating the losses.

[31] Using Lithuania as a control for reform implemented in Estonia can be justified on several grounds. Both countries are small open economies with the same trading partners, and display a similar evolution of real GDP, industrial production and retail turnover before and after July 2009. Before the reform, they were characterised by very similar trends in unemployment as well as stocks and flows of temporary contracts (see Appendix Fig. A4). Finally, no significant changes in labour market policies and institutions occurred in Lithuania over the period considered.

[32] See the note to Appendix Table A8 for details on the specifications.

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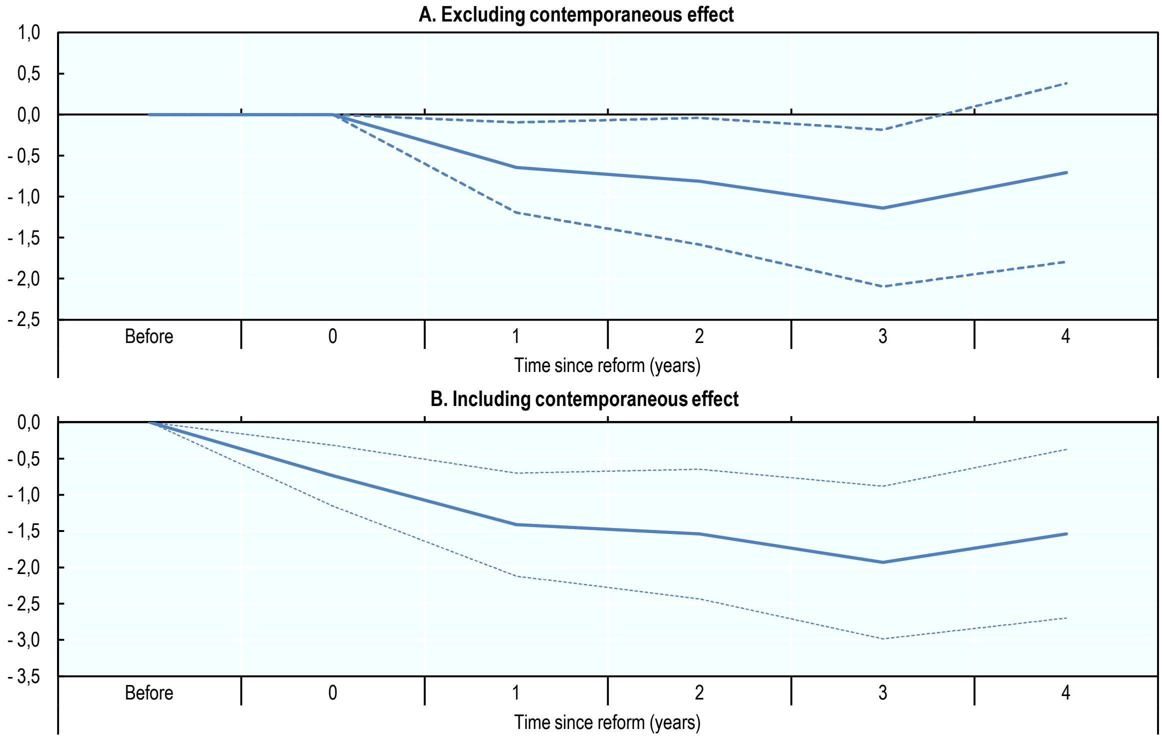
Figures

Figure 1. **Competition-enhancing reforms and employment in network industries**

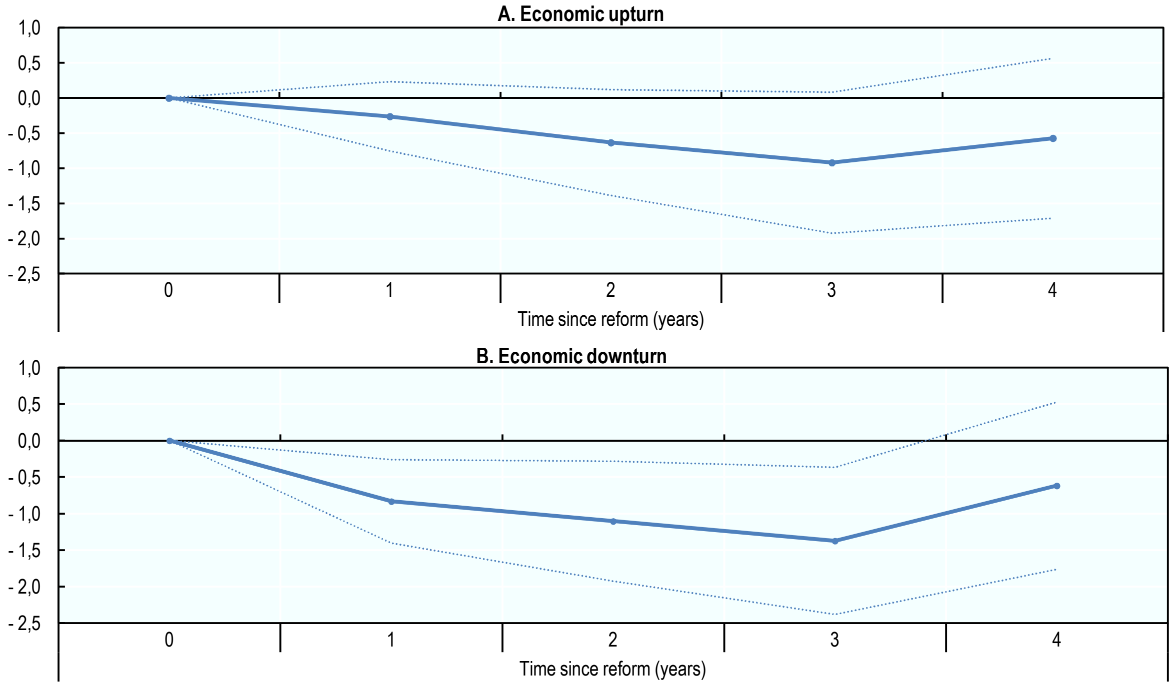
Estimated cumulative change in industry employment up to 4 years following the reform, in percentage



Notes: The chart reports point estimates and 90%-confidence intervals of the cumulated employment effect of PMR reforms lowering entry barriers. Estimates refer to the case of a reform lowering the OECD indicator of regulatory barriers to entry in network industries (Energy, Transport and Communication, ETCR) by one point. Employment levels before the reform are normalised to 0. The underlying parameters are estimated allowing employment growth in each network industry to depend on lagged values of industry regulation as well as on lagged employment changes. Confidence intervals are obtained by clustering errors on countries and industries.

Figure 2. **The employment effects of competition-enhancing reforms in upturns and downturns**

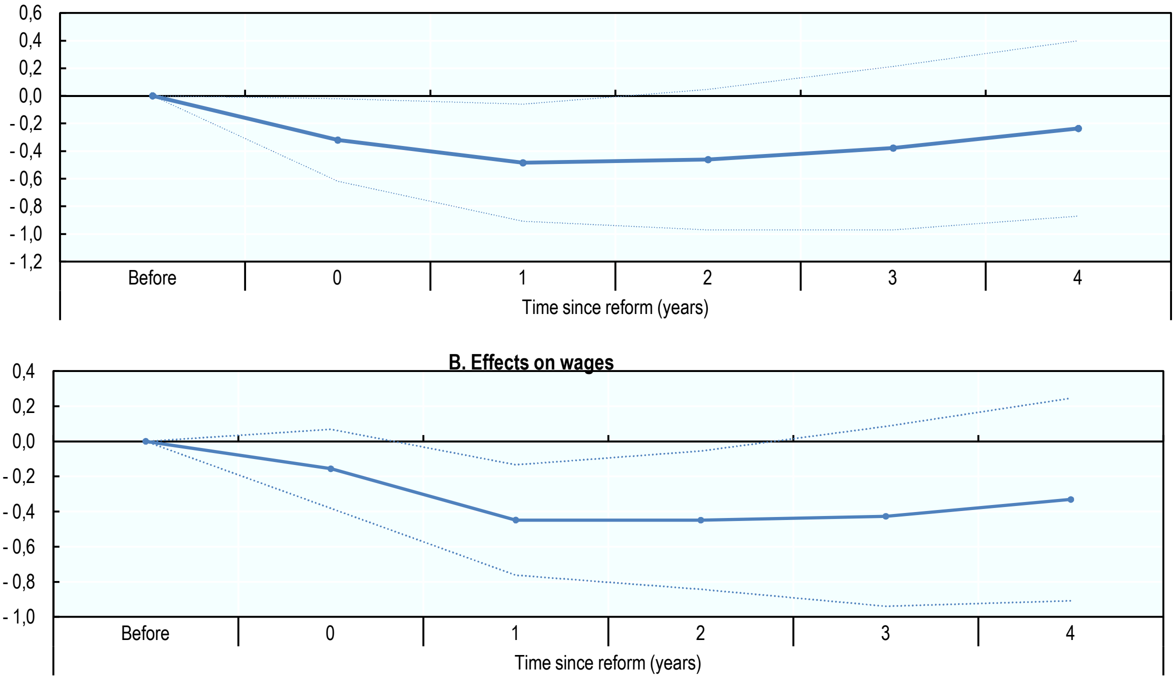
Estimated cumulative change in industry employment up to 4 years following the reform, in percentage



Notes: The chart reports point estimates and 90%-confidence intervals of the cumulated employment effect of PMR reforms lowering entry barriers. Estimates refer to the case of a reform lowering the OECD indicator of PMR in network industries (Energy, Transport and Communication, ETCR) by one point. Employment levels before the reform are normalised to 0, and the contemporaneous effect of the reform (β_{00}) is set to 0, as in Panel A of Figure 3.1. The underlying parameters are estimated allowing employment growth in each network industry to depend on lagged values of industry regulation as well as on lagged employment changes. Panel A plots the employment effects of reforms implemented as the output gap grows by 1 percentage point (i.e. the growth rate of output is 1 percentage point larger than the growth of potential output, indicating an economic upturn). Panel B refers to periods when the output gap falls by 1 percentage point (indicating an economic downturn). Confidence intervals are obtained by clustering errors on countries and industries.

Figure 3. **The impact of flexibility-enhancing EPL reforms on aggregate employment**

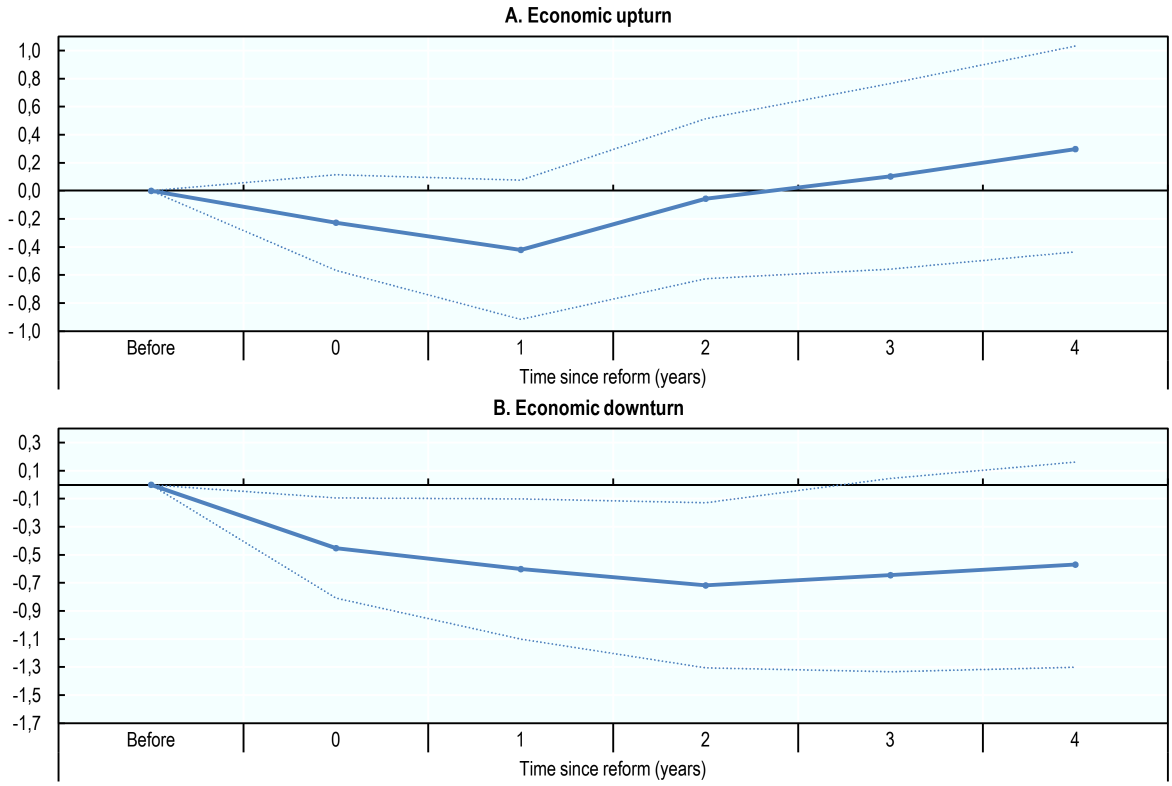
Estimated cumulative change in business-sector employment up to 4 years following the reform, in percentage



Notes: The chart reports point estimates and 90%-confidence intervals of the cumulated effect of changes in employment protection legislation (EPL) for regular contracts on (i) wage and salary employment levels and (ii) average wage levels in the non-agricultural/non-mining business sector. Both estimates are obtained estimating model (2), with levels before the reform normalised to 0. The wage regression further controls for compositional effects adding the share of the low-educated in the industry workforce and the change in industry employment. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases and 0 otherwise. They can therefore be interpreted as the effect of a flexibility-enhancing reform of an average size (reducing the indicator by 0.2 points). Estimates are obtained by assuming that, in each industry, the impact of EPL is greater, the greater the US dismissal rate in that industry. Business-sector aggregation is obtained by assuming that EPL reforms would have no short-term effect on employment in an hypothetical industry whose US dismissal rate would be equal to or lower than the first quartile of the distribution. Confidence intervals are obtained by clustering errors on countries and industries.

Figure 4. **Flexibility-enhancing EPL reforms and employment in different stages of the business-cycle**

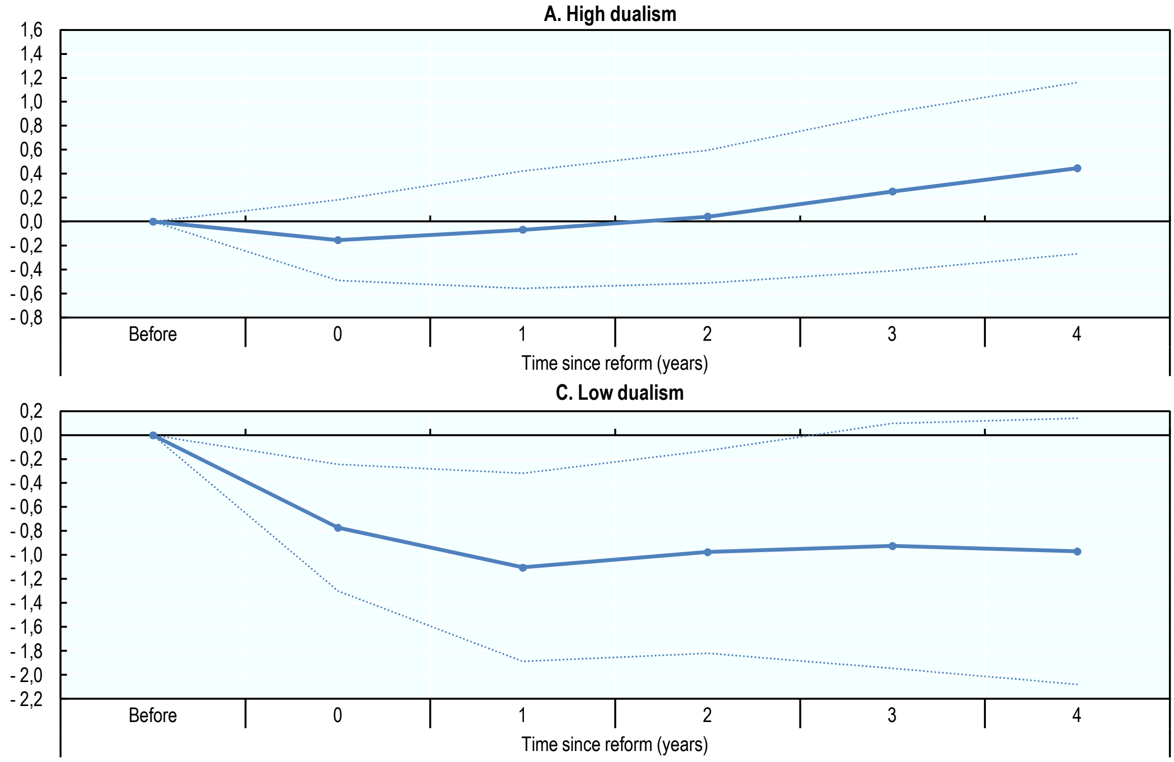
Estimated cumulated change of business-sector employment up to 4 years since the reform, in percentage



Notes: The charts report point estimates and 90%-confidence intervals of the cumulative effect of changes in employment protection legislation (EPL) for regular contracts on average wage and salary employment in the non-agricultural/non-mining business-sector, obtained from difference-in-difference estimators, with levels before the reform normalised to 0. Economic upturn (economic downturn) stands for a scenario in which the output gap was growing (falling) by 1 percentage point at the time of the reform. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases and 0 otherwise. They can therefore be interpreted as the effect of a flexibility-enhancing reform of an average size (reducing the indicator by 0.2 points). Estimates are obtained by assuming that, in each industry, the impact of EPL is greater, the greater the US dismissal rate in that industry. Business-sector aggregation is obtained by assuming that EPL reforms would have no short-term effect on employment in an hypothetical industry whose US dismissal rate would be equal to or lower than the first quartile of the distribution. Interaction terms between EPL reform dummies and changes in the output gap are included in the specifications and used to infer the effects reported in different panels. Confidence intervals are obtained by clustering errors on countries and industries.

Figure 5. **Incidence of fixed-term contracts, flexibility-enhancing EPL reforms and employment**

Estimated cumulative change of business-sector employment up to 4 years following the reform, in percentage



Notes: The charts report point estimates and 90%-confidence intervals of the cumulative effect of changes in employment protection legislation (EPL) for regular contracts on average wage and salary employment in the non-agricultural/non-mining business-sector, obtained from difference-in-difference estimators, with levels before the reform normalised to 0. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases and 0 otherwise. They can therefore be interpreted as the effect of a flexibility-enhancing reform of an average size (reducing the indicator by 0.2 points). Interaction terms between EPL reform dummies and the average share of fixed-term contracts in wage and salary employment are included in the specifications and used to infer the effects reported in the different panels. Estimates are obtained by assuming that, in each industry, the impact of EPL is greater, the greater the US dismissal rate in that industry. Business-sector aggregation is obtained by assuming that EPL reforms would have no short-term effect on employment in a hypothetical industry whose US dismissal rate would be equal to or lower than the first quartile of the distribution. Incidence of fixed-term contracts is defined as the share of these contracts in wage and salary employment. Its median, computed on all observations in the sample, is 10.35%. “High dualism” corresponds to 5 percentage points above the median. “Low dualism” to 5 percentage points below. Confidence intervals are obtained by clustering errors on countries and industries.

Tables

Table 1. **The short-run effect of deregulation in network industries**

	(1)	(2)	(3)	(4)	(5)
Δ BE_{cit}	0.0066**			0.0074***	0.0071***
	(0.0026)			(0.0025)	(0.0025)
Δ BE_{cit-1}		0.0068*	0.0061*	0.0061*	0.0070*
		(0.0035)	(0.0034)	(0.0034)	(0.0037)
Δ BE_{cit-2}			0.0013	0.0022	0.0017
			(0.0031)	(0.0031)	(0.0033)
Δ BE_{cit-3}					0.0026
					(0.0034)
Δ log E_{cit-1}			0.0459	0.0460	
			(0.0522)	(0.0520)	
Δ log E_{cit-2}			-0.1226*	-0.1264**	
			(0.0617)	(0.0608)	
Observations	1891	1833	1753	1753	1695
R-squared	0.650	0.650	0.646	0.649	0.642

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on EUKLEMS (1975-2007) data. The estimates refer to alternative specifications of model (2). Coefficients in columns 1 to 5 are obtained weighting observations with the industry employment share in the country (taken on average between 1975 and 2007). All specifications account for country-by-industry, country-by-time and industry-by-time dummies. Standard errors, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table 2. **Robustness to including forward terms**

	(1)	(2)	(3)	(4)	(5)
Δ BE_{cit}	0.0067**			0.0074***	0.0072***
	(0.0026)			(0.0026)	(0.0026)
Δ BE_{cit-1}		0.0066*	0.0057*	0.0057*	0.0068*
		(0.0033)	(0.0032)	(0.0033)	(0.0035)
Δ BE_{cit+1}	-0.0006	0.0002	-0.0001	-0.0000	0.0007
	(0.0031)	(0.0031)	(0.0029)	(0.0029)	(0.0030)
Observations	1822	1764	1684	1684	1626
R-squared	0.654	0.654	0.649	0.652	0.645

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on EUKLEMS data. The estimates are obtained augmenting the specifications in Table 1 with a forward term ($\Delta BE_{ci,t+1}$). Coefficients in columns 1 to 5 are obtained weighting observations with the industry employment share in the country (taken on average between 1975 and 2007). All specifications account for country-by-industry, country-by-time and industry-by-time dummies. Standard errors, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table 3. **Granger-causality tests of reverse causality**

	Not including Δlog Employment (t) (1)	Including Δlog Employment (t) (2)
F-test on Δlog Employment (t-1)	0,19	0,2
F-test on Δlog Employment (t-2)	2,39	1,94
F-test, cumulative impact	0,54	0,38

Note: The table presents F-tests of the coefficients of the first two lags of employment growth ($\Delta E_{ci,t-1}$and $\Delta E_{ci,t-2}$) in models where the change in Barriers to entry (ΔBE_{cit}) is the dependent variable. The full specification also includes two lags of ΔBE_{cit} country-by-industry, country-by-time and industry-by-time dummies. "F-test, cumulative impact" is for the F-test on the sum of both lagged Δlog Employment coefficients. F-statistics are distributed as F(1,68) under the null (test statistics are obtained by clustering errors at the country-by-industry level). None of the reported statistics is significant at standard levels.

Table 4. **Robustness to specification and sensitivity tests**

VARIABLES	(1) Baseline	(2) Int. Inputs	(3) V. Added	(4) Both	(5) Pub. Own.	(6) Spillover	(7) Dep. Emp	(8) Unweighted
Δ BE$_{cjt}$	0.0074***	0.0078***	0.0077***	0.0080***	0.0077***	0.0076***	-0.0019	0.0066**
	(0.0025)	(0.0025)	(0.0026)	(0.0025)	(0.0025)	(0.0025)	(0.0085)	(0.0032)
Δ BE$_{cjt-1}$	0.0061*	0.0061*	0.0062*	0.0062*	0.0062*	0.0065*	0.0092*	0.0062*
	(0.0034)	(0.0034)	(0.0034)	(0.0033)	(0.0034)	(0.0034)	(0.0048)	(0.0032)
Δ BE$_{cjt-2}$	0.0022	0.0024	0.0024	0.0025	0.0024	0.0032	0.0043	0.0009
	(0.0031)	(0.0031)	(0.0031)	(0.0031)	(0.0031)	(0.0031)	(0.0037)	(0.0033)
Δ log E$_{cit-1}$	0.0460	0.0455	0.0456	0.0453	0.0459	0.0389	0.0163	0.0170
	(0.0520)	(0.0522)	(0.0518)	(0.0520)	(0.0523)	(0.0516)	(0.0517)	(0.0454)
Δ log E$_{cit-2}$	-0.1264**	-0.1190**	-0.1275**	-0.1204**	-0.1284**	-0.1323**	-0.1855**	-0.1442**
	(0.0608)	(0.0595)	(0.0606)	(0.0592)	(0.0614)	(0.0606)	(0.0833)	(0.0566)
Observations	1753	1753	1753	1753	1753	1753	1213	1,753
R-squared	0.649	0.651	0.650	0.652	0.650	0.652	0.582	0.574

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on EUKLEMS (1975 2007) data, except in column 7 where it is wage and salary employment. The estimates refer to alternative specifications of model (2). All coefficients are obtained weighting observations with the industry employment share in the country (taken on average between 1975 and 2007), except for column 8 which presents OLS. The specifications in columns 2 and 3 include the contemporaneous industry-level growth rate of intermediate inputs and real value added (column 4 has both). Column 5 accounts for changes in the degree of public ownership (variable PUB OWN in the ETCR database). In column 6 the specification augmented with: $WBE_{dit} = \sum_{-i} Exp_{i,-i} * BEc,-i,t$, where $Exp_{i,-i}$ are coefficients from the US Inverse Leontief Matrix measuring how many units of input -i (e.g. energy) are required (at any stage of the value chain) to produce one additional unit for final demand in network industry i (transport). All specifications account for country-by-industry, country-by-time and industry-by-time dummies. Standard errors, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table 5. **The short-run effects of lowering dismissal costs on employment**

	Base sample		Extended sample		Base sample, unweighted	
	(1)	(2)	(3)	(4)	(5)	(6)
FE_t*DR	-0.0029*	-0.0030*	-0.0031**	-0.0033**	-0.0032**	-0.0033*
	(0,0016)	(0,0017)	(0,0014)	(0,0015)	(0,0016)	(0,0017)
FE_{t-1}*DR	-0.0009	-0.0009	-0.0004	-0.0006	0.0018	0.0020
	(0,0016)	(0,0014)	(0,0017)	(0,0014)	(0,0020)	(0,0020)
FE_{t-2}*DR	0.0009	0.0008	0.0003	0.0003	0.0012	0.0014
	(0,0016)	(0,0014)	(0,0013)	(0,0014)	(0,0014)	(0,0013)
FE_{t-3}*DR	0.0001		0.0005		-0.0007	
	(0,0014)		(0.0018)		(0,0016)	
Observations	7.590	8.052	9.091	9.574	7.590	8.052
R-squared	0.532	0.510	0.529	0.524	0.399	0.390

Notes: Dependent variables is changes in log wage and salary employment. FE: dummy variable for flexibility-enhancing reforms of EPL for regular contracts; DR: industry-level US dismissal rate (in %). All specifications control for lags of changes in log employment (same number as for FE*DR), changes in the output gap and a dummy for protection-raising EPL reforms (both interacted with DR and also including the same number of lags as for FE*DR) as well as country-by-time, industry-by-time and country-by-industry dummies. In columns 1 to 4 the observations are weighted by the average industry share in the country's non-agricultural/non-mining business sector. The base sample is the EUKLEMS sample (1985-2007); the extended sample is the combined EUKLEMS-STAN sample (1985-2012). Standard errors, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table 7. **Recent EPL reforms and unemployment**

Estimated average effect within 2 years from the reform in percentage points

	Estonia	Slovenia	Spain
Estimated average effect (% points)	1.92***	0.55*	0.08
	(0.58)	(0.29)	(0.62)
Observations	*84*	*84*	*84*
R-squared	*0.995*	*0.990*	*0.997*

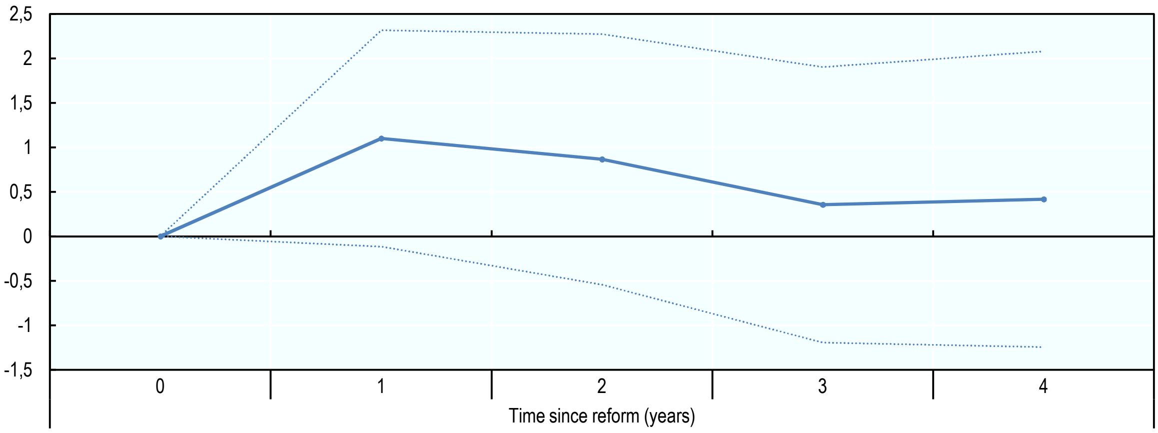
Notes: The dependent variable is the standardised unemployment rate. Estimates based on regression-discontinuity models fitted on monthly data from Eurostat, Instituto Nacional de Estadistica (INE) and Statistical Office of the Republic of Slovenia (SORS). Each specification controls for the 3-month-lagged industrial production and retail turnover indexes, a 5^{th} order polynomial time trend (heterogeneous between the pre- and post-reform period) and month dummies. Robust standard errors are in parentheses. ***, **, * statistically significant at 1%; 5% and 10% levels respectively

APPENDIX A1: ADDITIONAL RESULTS

A1.1 Additional Figures

Figure A1. **Competition-enhancing reforms and wages in network industries**

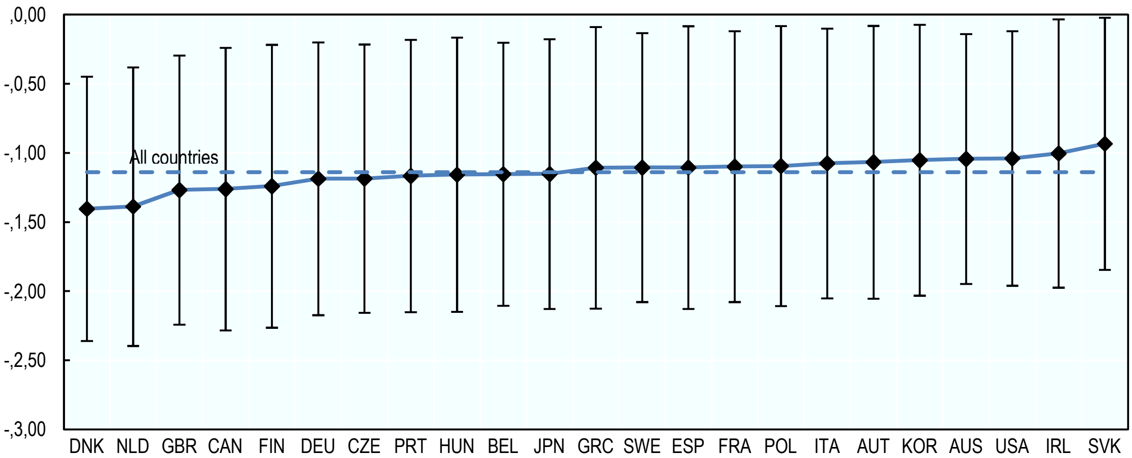
Estimated cumulated change in industry wages up to four years since the reform, in percentage



Note: The chart reports point estimates and 90%-confidence intervals of the cumulated effect of reforms lowering entry barriers in network industries on average industry wage. Estimates refer to the case of a reform lowering the OECD indicator of regulation in Energy, Transport and Communication (ETCR) by one point. Wage levels before the reform are normalised to 0. The underlying parameters are estimated allowing wage growth in each network industry to depend on lagged values of industry regulation as well as on lagged wage changes. The workforce composition is accounted for by the share of the low-educated in total hours worked, and changes in industry employment. Confidence intervals are obtained by clustering errors on countries and industries.

Figure A2. **Competition-enhancing reforms and employment in network industries**
Robustness to varying the country sample

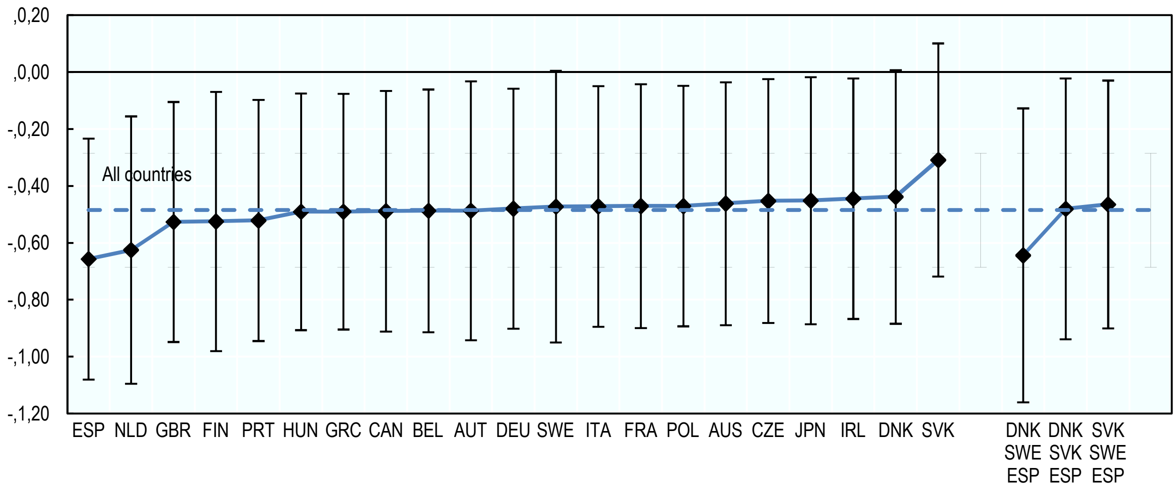
Estimated cumulated change in industry employment three years after the reform, in percentage



Note: The chart reports point estimates and 90%-confidence intervals of the cumulated employment effect of PMR reforms lowering entry barriers three years after the reform. The baseline estimate, reported in the top panel of Figure 1 (at year 3), is represented by a dotted line. Each diamond indicates the corresponding value estimated dropping from the sample the country indicated in the X-axis. Estimates refer to the case of a reform lowering the OECD indicator of PMR in network industries (Energy, Transport and Communication, ETCR) by one point. The underlying parameters are estimated from model (1). Confidence intervals are obtained by clustering errors on countries and industries.

Figure A3. **Flexibility-enhancing EPL reforms and employment: Robustness to varying the country sample**

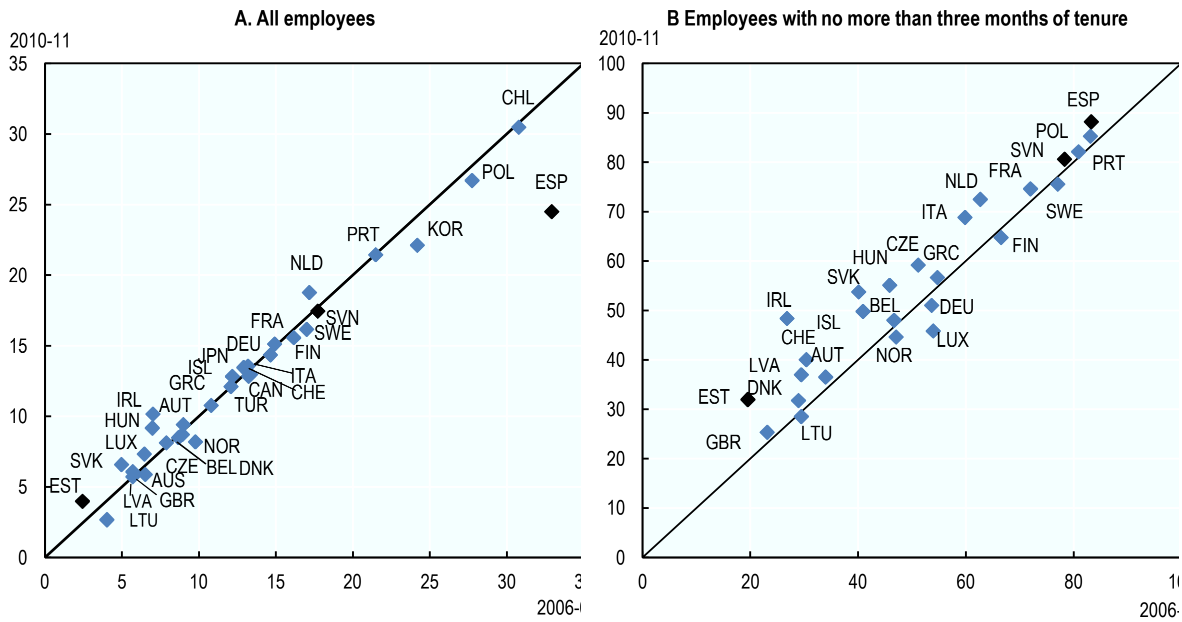
Estimated cumulative change in wage-and salary business-sector employment one year after the reform, in percentage



Notes: The chart reports point estimates and 90%-confidence intervals of the cumulative effect of changes in employment protection legislation (EPL) for regular contracts on wage and salary employment levels in the non-agricultural/non-mining business sector one year after the reform. Employment levels before the reform normalised to 0. Each diamond indicates the effect estimated dropping from the sample the country indicated in the X-axis. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases and 0 otherwise. They can therefore be interpreted as the effect of a flexibility-enhancing reform of an average size (reducing the indicator by 0.2 points). Estimates are obtained by assuming that, in each industry, the impact of EPL is greater, the greater the US dismissal rate in that industry. Business-sector aggregation is obtained by assuming that EPL reforms would have no short-term effect on employment in a hypothetical industry whose US dismissal rate would be equal to or lower than the first quartile of the distribution. Confidence intervals are obtained by clustering errors on countries and industries.

Figure A4. **Incidence of fixed-term contracts in total wage and salary employment and new hires**

Percentage of wage and salary employees with a fixed-term contract, 2006-2007 and 2011-2012



Notes: calculations based on OECD Labour Force Statistics Database and EU LFS microdata. Estonia, Slovenia and Spain, the case studies examined in section 4.3, are indicated by black diamonds.

A1.2 Additional Tables

Table A1 **The short-run effect of deregulation in upturns and downturns**

	WLS (1)	WLS (2)	WLS (3)	WLS (4)	WLS (5)
Δ BE_{cit}	0.0069***			0.0077***	0.0077***
	(0.0026)			(0.0026)	(0.0026)
Δ BE_{cit-1}		0.0056*	0.0050	0.0050	0.0058*
		(0.0030)	(0.0030)	(0.0030)	(0.0032)
Δ BE_{cit-2}			0.0027	0.0036	0.0033
			(0.0029)	(0.0028)	(0.0031)
Δ BE_{cit-3}					0.0025
					(0.0033)
Δ BE * ΔOG	-0.0003			-0.0001	-0.0002
	(0.0014)			(0.0013)	(0.0014)
Δ BE * ΔOG (*t* -1)		-0.0026**	-0.0028**	-0.0028**	-0.0027*
		(0.0013)	(0.0013)	(0.0014)	(0.0014)
Δ BE * ΔOG (*t* -2)			0.0009	0.0008	0.0005
			(0.0016)	(0.0017)	(0.0018)
Δ BE * ΔOG (*t* -3)					0.0000
					(0.0019)
Observations	1 750	1 684	1 618	1 618	1 552
R-squared	0.646	0.647	0.661	0.664	0.660

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on EUKLEMS data (1975-2007). The estimates refer to alternative specifications of model (1), augmented with the interaction between (current and lagged) changes in BE and (current and lagged) changes in the output gap (ΔOG_{ct}). The output gap is defined as the difference between current and potential output, so its first difference is positive in upturns, and negative during downturns. Coefficients in columns 1 to 5 are obtained weighting observations with the industry employment share in the country (taken on average between 1975 and 2007). Standard errors, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table A2. **The short-run effect of deregulation in network industries (1975-2012)**

	WLS (1)	WLS (2)	WLS (3)	WLS (4)	WLS (5)
Δ BE_{cit}	0.0059**			0.0067***	0.0062**
	(0.0025)			(0.0025)	(0.0025)
Δ BE_{cit-1}		0.0067*	0.0065*	0.0064*	0.0070*
		(0.0036)	(0.0035)	(0.0036)	(0.0038)
Δ BE_{cit-2}			0.0020	0.0027	0.0020
			(0.0030)	(0.0029)	(0.0032)
Δ BE_{cit-3}					0.0042
					(0.0036)
Δ log E_{cit-1}			0.0459	0.0460	
			(0.0622)	(0.0616)	
Δ log E_{cit-2}			-0.1126*	-0.1154*	
			(0.0630)	(0.0625)	
Observations	2 012	1 962	1 877	1 876	1 849
R-squared	0.629	0.628	0.623	0.624	0.618

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on a sample obtained collating EUKLEMS and STAN data (1975-2012). The estimates refer to alternative specifications of model (1). Coefficients are obtained weighting observations with the industry employment share in the country (taken on average between 1975 and 2007).

Standard errors, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table A3. **Other institutional reforms**

	Unemployment benefits gross replacement rate	Collective bargaining coverage	Collective bargaining coordination	Tax wedge	EPT
	(1)	(2)	(3)	(4)	(5)
R_t*DR	-0.0029	-0.0015	-0.0031	-0.0019	-0.0009
	(-1.42)	(-1.13)	(-1.13)	(-1.26)	(-0.76)
R_{t-1}*DR	0.0005	0.0003	0.0008	-0.0008	-0.0009
	(0.15)	(0.21)	(0.39)	(-0.57)	(-0.69)
R_{t-2}*DR	-0.0015	0.0035	0.0005	0.0008	0.0008
	(-0.46)	(1.47)	(0.28)	(0.48)	(0.59)
R_{t-3}*DR	0.0025	-0.0036	-0.0010	0.0026	0.0001
	(0.78)	(-1.26)	(-0.64)	(1.63)	(0.07)
Observations	7,062	5,214	7,590	6,402	7,590
R-squared	0.583	0.604	0.532	0.601	0.531

Notes: The dependent variable is the yearly growth rate of wage and salary employment. R(t): dummy variable for flexibility-enhancing reforms of the policy/institutions indicated in the column title; DR: industry-level US dismissal rate (in %). All specifications control for changes in the output gap and for protection-raising reforms of the policy/ institutions indicated in the column title (both interacted with DR; 3 lags of each are also included), 3 lags of changes in log employment as well as country-by-time, industry-by-time and country-by-industry dummies. Unemployment benefit gross replacement rate, average coverage of collective bargaining and the average labour tax wedge are in percentage. Coordination is measured by the ICTWSS index, varying between 1 and 5 from the least to the most coordinated. EPT indicates the regulation on hiring on temporary contracts and is measured by the OECD EPT indicator varying between 0 and 6 from the least to the most restrictive. The observations are weighted by the average industry share in the country's non-agricultural/non-mining business sector. T-statistics, adjusted for clustering at the country-by-industry level, in parentheses.

Table A4. **Quantitative EPL indicators**

	Base sample, WLS	Base sample, OLS	Extended sample, WLS
	(1)	(2)	(3)
SFE t*DR	-0.0206***	-0.0180**	-0.0244***
	(-2.92)	(-2.10)	(-3.49)
SFE t-1*DR	-0.0054	0.0109	-0.0037
	(-0.50)	(0.85)	(-0.41)
SFE t-2*DR	-0.0036	0.0060	-0.0041
	(-0.34)	(0.71)	(-0.41)
SFE t-3*DR	0.0050	0.0047	0.0062
	(0.63)	(0.65)	(0.56)
Observations	7.172	7.172	8.629
R-squared	0,521	0,393	0,515

Notes: The dependent variable is the yearly growth rate of wage and salary employment. SFE: size of flexibility-enhancing EPL reforms measured as absolute change in EPL for regular contracts if negative and 0 otherwise; DR: industry-level US dismissal rate (in %). All specifications control for changes in the output gap and size of protection-raising EPL reforms (both interacted with DR; 3 lags of each are also included), 3 lags of changes in log employment as well as country-by-time, industry-by-time and country-by-industry dummies. Observations from Spain are excluded from the sample. The observations are weighted by the average industry share in the country's non-agricultural/non-mining business sector, except in column 2. The base sample is the EUKLEMS sample (1985-2007); the extended sample is the combined EUKLEMS-STAN sample (1985-2012). T-statistics, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table A5. **The short-run effects of lower dismissal costs on wages**

	Log Wage		
	Base	Employment composition	Unweighted (OLS)
	(1)	(2)	(3)
FE_t*DR	-0.0005	-0.0014	0.0007
	(0,0016)	(0,0012)	(0,0013)
FE_{t-1}*DR	-0.0023*	-0.0022*	-0.0023*
	(0,0012)	(0,0012)	(0,0013)
FE_{t-2}*DR	-0.0003	0.0000	-0.0021
	(0,0013)	(0,00 1)	(0,0016)
FE_{t-3}*DR	0.0013	0.0002	0.0002
	(0,0016)	(0,0020)	(0,0014)
Observations	7.590	6.622	7.590
R-squared	0.369	0,434	0.290

Notes: Dependent variable in columns 1 to 3 are changes in log average wage; in column 4 it is percentage share of workers with less than upper secondary education in hours worked. FE: dummy variable for flexibility-enhancing reforms of EPL for regular contracts; DR: industry-level US dismissal rate (in %). All specifications control for lags of changes in log employment (same number as for FE*DR), changes in the output gap and a dummy for protection-raising EPL reforms (both interacted with DR and also including the same number of lags as for FE*DR) as well as country-by-time, industry-by-time and country-by-industry dummies. The specification of Column 6 also controls for the change in the logarithm of wage and salary employment and of the share of low-educated employees in hours worked. The observations in columns 1 and 2 are weighted by the average industry share in the country's non-agricultural/non-mining business sector. The sample is the EUKLEMS sample (1985-2007). Standard errors, adjusted for clustering at the country by-industry level, in parentheses.***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table A6. **Interactions with the business cycle and labour market dualism**

	Panel A			**Panel B**		
	The interaction variable (INT) is:					
	Output gap (*OG*)			Share of temporary contracts (*ShT*)		
Dep var is:	Employment	Wage	Low educ share	Employment	Wage	Low educ share
FE_t*DR	-0.0030*	-0.0006	-0.0022	-0.0031	0.0001	0.0795
	(-1.77)	(-0.33)	(-0.05)	(-1.63)	(0.09)	(1.38)
FE_{t-1}*DR	-0.0010	-0.0023*	0.0488	-0.0004	-0.0030**	0.0919**
	(-0.60)	(-1.85)	(1.43)	(-0.20)	(-1.98)	(2.02)
FE_{t-2}*DR	0.0019	-0.0001	0.0311	0.0019	0.0001	0.1103*
	(1.48)	(-0.10)	(0.77)	(1.49)	(0.10)	(1.81)
FE_{t-3}*DR	0.0002	0.0012	-0.0073	0.0001	0.0015	0.0305
	(0.15)	(0.81)	(-0.15)	(0.10)	(0.78)	(0.48)
INT_t *FE_t*DR	0.0101	-0.0035	0.0785	0.0056**	-0.0027	-0.0089
	(1.10)	(-0.31)	(0.37)	(2.34)	(-1.54)	(-0.15)
INT_{t-1}*FE_{t-1}*DR	0.0008	-0.0005	0.1130	0.0031	0.0010	0.0696
	(0.07)	(-0.06)	(0.45)	(1.15)	(0.68)	(1.32)
INT_{t-2}*FE_{t-2}*DR	0.0227*	0.0039	0.0100	0.0004	-0.0011	0.0789
	(1.82)	(0.62)	(0.03)	(0.26)	(-0.89)	(1.31)
INT_{t-3}*FE_{t-3}*DR	0.0018	-0.0038	0.2390	0.0017	-0.0021	0.0652
	(0.18)	(-0.36)	(1.13)	(1.24)	(-1.35)	(0.67)
Observations	7590	7590	6424	6226	6226	5060
R-squared	0.532	0.369	0.552	0.506	0.373	0.583

Notes: Dependent variables are indicated in column titles: the yearly growth rate of wage and salary employment, and the yearly growth rate of average wage, and the percentage share of workers with less than upper secondary education in hours worked. FE: dummy variable for flexibility-enhancing reforms of EPL for regular contracts; DR: industry-level US dismissal rate (in %); OG: output gap (in %); ShT: Share of fixed-term contracts in wage and salary employment (in %). All specifications control for lags of changes in log employment (same number as for FE*DR), changes in the output gap and a dummy for protection-raising EPL reforms (both interacted with DR and also including the same number of lags as for FE*DR) as well as country-by-time, industry-by-time and country-by-industry dummies. Changes in the output gap (in %) are included in cols 1 to 3 and the share of fixed-term contracts (in %) in cols 4 to 6. The observations are weighted by the average industry share in the country's non-agricultural/non-mining business sector. The base sample is the EUKLEMS sample (1985-2007); the extended sample is the combined EUKLEMS-STAN sample (1985-2012). T-statistics, adjusted for clustering at the country-by-industry level, in parentheses. ***, **, and * denote coefficients significantly different from zero at 99%, 95% and 90% confidence level, respectively.

Table A7. **Recent EPL reforms and unemployment: sensitivity analysis**

Estimated average effect within two years from the reform in percentage points

	(1)	(2)	(3)	(4)	(5)
A. Estonia					
	No season adjustment	Demogr cntrls	Business Cycle cntrls	Drop months around reform	Placebo reform
Estimated average effect (% points)	2.57**	1.69***	1.85***	1.97*	1.22
	(2.32)	(2.83)	(2.76)	(1.76)	(1.19)
Observations	84	84	84	81	84
R-squared	0.991	0.996	0.995	0.996	0.994
B. Slovenia					
	(1)	(2)	(3)	(4)	(5)
	No season adjustment	Demogr cntrls	Business Cycle cntrls	Drop months around reform	Placebo reform
Estimated average effect (% points)	1.11***	0.50*	0.62*	1.33*	-0.04
	(2.83)	(1.78)	(1.89)	(1.95)	(-0.13)
Observations	84	84	84	81	84
R-squared	0.987	0.991	0.989	0.990	0.989
C. Spain					
	(1)	(2)	(3)	(4)	(5)
	No season adjustment	Demogr cntrls	Business Cycle cntrls	Drop months around reform	Placebo reform
Estimated average effect (% points)	0,34	0,26	-0,29	-0,65	-0,42
	(0,49)	(0,43)	(-0.53)	(-0.63)	(-0.97)
Observations	84	84	84	81	84
R-squared	0,996	0,997	0,998	0,997	0,997

Notes: The dependent variable is the seasonally-adjusted standardised unemployment rate. Estimates based on regression-discontinuity models fitted on monthly data. Each specification controls for the 3-month-lagged industrial production and retail turnover indexes, a 5^{th} order polynomial time trend (heterogeneous between the pre- and post-reform period) and month dummies, except when differently indicated. In column 1, the dependent variable is not adjusted for seasonality. In column 2, the share of youth and of women in labour force is included. In column 3, indexes of retail turnover and industrial production are included as contemporaneous variables. In column 4, three months around the reform date are excluded from the sample. In column 5, a fictitious reform is hypothesized, occurring 3 months before the true reform. Robust t-statistics are in brackets. ***, **, * statistically significant at 1%; 5% and 10% levels respectively.

Table A8. **The 2009 Estonian EPL reform and unemployment: using Lithuania as a comparison country**

Estimated average effect within two years from the reform in percentage points						
	(1)	(2)	(3)	(3)	(4)	(5)
	Baseline	Add Latvia as ctrl	Business Cycle cntrls	Add time trend	Drop months around reform	Placebo reform
Estimated average effect (% points)	1.49**	2.33***	1.83***	3.35***	1.51*	1.11
	(2.14)	(3.46)	(3.32)	(2.80)	(1.90)	(1.32)
Observations	166.250	241.267	166.250	166.250	156.040	166.250

Notes: Marginal percentage effects on the probability of being unemployed, obtained by estimating a probit model with observations weighted by cross-sectional weights. Marginal effects are identified by the interaction between a country dummy for Estonia and a dummy for the post-July 2009 period. The baseline specification controls for gender, 3 educational attainment classes, 15 age classes, 3 classes for the degree of urbanisation, a dummy for being born in the country of residence, 23 classes for the duration of residence in the country if foreign born, country dummies, dummies for calendar months, a dummy for the post-July 2009 period and the 3-month-lagged industrial production and retail turnover. In column 1, only Lithuania is used as comparison. In column 2, both Lithuania and Latvia are used. In column 3, logarithms of the industrial production and real turnover in the retail sector are added. In column 4, a 5th order polynomial in time (months) is included. In column 5, three months around the reform date are excluded from the sample. In column 6, a fictitious reform is hypothesized 3 months before the true reform. Robust t-statistics, obtained by adjusting for clustering on countries and months in parentheses. ***, **, * statistically significant at 1%; 5% and 10% levels respectively.

APPENDIX A2: DETAILED DATA DESCRIPTION

The base sample covers annual data from EUKLEMS for the period 1975-2007, covering 23 OECD countries and 22 non-agricultural/non-mining business-sector (ISIC rev.3) industries. For those countries for which OECD STAN data are available, the time coverage of the sample is extended to the period 1975-2012 by collating EUKLEMS data with data from the last version of STAN. As this dataset adopts the ISIC rev.4 classification, a mapping has been established by using employment data at the 3 digit level from EU LFS (tested on years for which both classifications are available). Such mapping is however imperfect and breaks in the industry classification can severely alter the estimated short-run dynamics; moreover, the extension likely exacerbated measurement error. Accordingly, in this chapter, the collated sample is used only in sensitivity analyses.

Countries in the sample include: Australia, Austria, Belgium, Canada, the Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Korea, the Netherlands, Poland, Portugal, the Slovak Republic, Spain, Sweden, the United Kingdom and the United States. In the EPL analysis, the sample excludes the United States (whose dismissal rates are used as a benchmark) and Korea (because of lack of data).

The PMR analysis exploits the ETCR section of the OECD PMR database (see Koske et al., 2015). More specifically, it focuses on the sub-indexes capturing legislated entry barriers and vertical integration (when applicable), varying from 0 (lowest regulation) to 6 (highest regulation). For example, in the case of the electricity industry, the indicator of industry-specific entry barriers is the simple average of three sub-indicators concerning third-party access (free, regulated, no access), existence of a wholesale pool and minimum consumption threshold that consumers must exceed in order to be able to choose their electricity supplier. In the sample, changes in the indicator have negative sign in around 95% of cases. More than one-sixth of the reform episodes implied a fall of the index of at least one point (the hypothetical reform used in the paper) in one year. In one third of the reform episodes in the sample a one point fall is obtained cumulating changes over two consecutive years. Based on the methodology illustrated in Conway and Nicoletti (2006), a 1-point reduction in the regulation index could be obtained, for example by: guaranteeing regulated third party access (TPA) to the electricity transmission grid and liberalising the wholesale market for electricity; allowing free entry to competitors in at least some markets in gas production/import and opening the retail market to consumer choice; removing regulations restricting the number of competitors allowed to operate a business in national post or other courier activities; removing restrictions on the number of airlines allowed to operate on domestic routes; or disallowing professional bodies or representatives of commercial interests from specifying or enforcing pricing guidelines or entry regulations in road transport. In the data, changes by 1 point or more in the indicator correspond to, for example, the implementation of the British Telecommunications Act in 1982 (opening a second fixed link network in competition with British Telecom), or the Electricity Act and the unbundling of the UK Central Electricity Generating Board (CEGB) in 1989; the Canadian National Transportation Act (NTA) and Motor Vehicle Transport Act (MVTA) of 1988; the Japanese Telecommunication Laws of the late 1980s and the Australian Telecommunications (Consumer Protection and Service Standards) Act of 1999; the 2003 French Electricity Law allowing any EU supplier to trade on the French territory (and more broadly the consequences of the EU liberalization directives of the electricity and gas markets adopted since the mid-1990s).

EPL reforms are quantified on the basis of changes in the indicator of stringency of EPL for individual dismissals of workers on permanent contracts from the OECD database on

Employment Protection Legislation. Unlike the case of product market deregulation, EPL reforms have historically both lowered and increased the degree of protection in the labour market. The implied range of variation in the OECD indicator of EPL stringency for regular contracts, however, is rather small. All but one reform episodes in the main sample (1985-2007) entail a change by less than 0.4 points in absolute terms. The 1994 Spanish reform is quantified as lowering the EPL indicator for individual dismissals by 1.19 points. Yet, there are reasons to believe this is a clear overstatement (see OECD, 2013, for a discussion). This suggested adopting an indicator function, rather than using the continuous variable. When Spain is excluded from the sample either indicator yields essentially the same result.

Further data used in robustness checks are sourced from the OECD Taxben , Taxing wages and EPL databases (Unemployment benefit average gross replacement rate, average collective bargaining coverage, average labour tax wedge and regulation on hiring on temporary contracts), and the ICTWSS database (http://www.uva-aias.net/208) for collective bargaining variables.

In the last section on country specific reforms, the standardised unemployment rate is from the OECD Labour Force Statistics. Industrial production and retail turnover are from national statistical offices (Eurostat in the case of Estonia). The shares of open ended contracts, youth and older workers in new contracts are from national administrative sources (SEPE for Spain as well as SRDAP and IMAD for Slovenia).

Table A9: Descriptive statistics

Panel A. PMR sample: network industries (1975-2007)

	Obs.	Mean	Std.Dev.
Δ log employment	1891	.0035	.0426
Δ log (wage and salary) employment	1351	-.0001	.0471
Δ log average wage (wage and salary employment)	1351	.0185	.0550
Δ barriers to entry (0-6 scale)	1891	-.1446	.3906
Δ public ownership (0-6 scale)	1891	-.0696	.2708
Δ barriers to entry in other network industries (I-O weighted)	1891	-.0604	.3023
Δ output gap (%)	1750	.104	1.586
Δ log intermediate inputs (volume)	1891	.0517	.0921
Δ log value added (volume)	1891	.0409	.0679

Panel B. EPL sample: business sector industries, non-mining, non-agricultural (1985-2007)

	Obs.	Mean	Std.Dev.
Δ log (wage and salary) employment	8976	0.0011	0.0603
Δ log average wage	8976	0.0178	0.062
Δ EPL (dismissals of workers on regular contracts)	8976	-0.0091	0.0739
Flexibility enhancing reform dummy (FE_t)	8976	0.3302	1.333
Protection raising reform dummy (PR_t)	8976	0.1143	0.8001
Δ output gap (%)	8976	0.1569	1.491
Share of fixed-term contracts (%)	7612	11.99	7.079
US Industry dismissal rate (%)	22	5.1810	1.7025

Notes: Statistics computed on the corresponding full samples. The number of observations actually used in the regressions might be lower due to inclusion of lag (lead) variables.

APPENDIX A3. FURTHER ESTIMATION DETAILS

A3.1 Impulse response functions. If a negative short-run employment impact of deregulation is established, then a key question from a policy perspective is how long it takes for this negative effect to be re-absorbed. To explore this issue the chapter relies on impulse-response functions projecting the impact of a reform on subsequent employment levels. Specifically, the analysis will focus on impulse-response functions obtained using the local-projection estimator developed by Jordà (2005) and Teulings and Zubanov (2014), which is more robust to misspecification, in particular at longer lags, than standard vector autoregressive (VAR) recursive estimation techniques. The algorithm allows estimating the cumulated response of the reform implemented at time t after $\boldsymbol{\tau}$ periods ($\boldsymbol{CR(\tau)}$) by estimating a system of $\boldsymbol{\tau}$ simultaneous equations:

$$\Delta E_{cit+h} = \sum_{f=1}^{h} \beta_{fh} \Delta BE_{ci,t+f} + \sum_{k=0}^{T} \beta_{kh} \Delta BE_{ci,t-k} + \sum_{k=1}^{T} \delta_{kh} \Delta E_{ci,t-k} + \mathbb{D} + \varepsilon_{cit} \tag{3}$$

for $h \in (0, \tau)$ and then deriving the cumulated impact of the reform as the sum of the coefficients estimated on the variable ΔBE_{cjt} in each equation: $IR(\tau) = \sum_{h=0}^{\tau} \beta_{0h}$. Note that, except for the first term, the right-hand side of this specification are those in the previous equation (1). The variables in the first term on the right-hand side ($\sum_{f=1}^{h} \beta_{fh} \Delta BE_{ci,t+f}$) are introduced to capture the potentially confounding effects of reforms implemented after time *t*. The minimum value of τ for which $CR(\tau)$ is not significantly different from 0 will indicate the duration of the employment impact of reforms.

In computing the impulse response functions of the direct impact of PMR reforms, the contemporaneous effect (i.e. the estimated β_{0h}, when h=0) is not taken into account in the baseline analysis to account for its potential inconsistency due to omitted variable bias. Hence, the functions only account for the effects of the reform one period ahead (i.e., with h=1... τ). As discussed later, this choice turns out to be a conservative one: the employment effects of deregulation become significantly stronger allowing for contemporaneous effects.

A3.2 Regression discontinuity methods. The short term labour market impact of the three case studies examined in section 4.3 is estimated through discontinuities in the (seasonally-adjusted) standardised unemployment rate. The general regression-discontinuity model, estimated on monthly data, is written as:

$$u_t = Y_t\beta + \delta I_{t>R} + \sum_{s=1}^{5} \lambda_s (t-R)^s + \sum_{s=1}^{5} \mu_s I_{t>R} (t-R)^s + D_t + e_t$$

where *u* is the unemployment rate at time *t*, *R* is the date of the reform, *I* is the indicator function (which equals 1 after the reform and 0 before), *D* stands for monthly dummies. Greek letters are parameters to be estimated, and *e* is a standard error term. *Y* is a vector of aggregate confounding factors, including the logarithms of the industrial production and real turnover in the retail sector. The sample window in these regression-discontinuity experiments covers five years before the reform and two years after (or up to the latest available data).

The parameter of interest is δ. A significant estimate for this parameter suggests a significant impact of the reform. The key identification assumption is that, conditional on control variables labour market performance evolves in a smooth way. To isolate the effect of the reform from that of the business cycle, the estimation models also include a polynomial time trends up to the 5th order. Following standard practice (see e.g. Imbens and Lemieux, 2008; Lee and Card, 2008), polynomial trends are allowed to differ before and after the reform.

To validate the empirical model, placebo tests are run by fictitiously setting the value of R to some date preceding (but sufficiently close to) the reform. To control for possible manipulations around the threshold, baseline models are re-estimated by excluding from the sample a 3-month window centred on the reform date.