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IZA DP No. 11011

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ABSTRACT

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

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.

JEL Classification: J23, L51, L11

Keywords: employment losses, EPL, product market regulation, industry data

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* A previous, incomplete version of this paper circulated as "A Bitter Medicine? Short-term Employment Impact of Deregulation in Network Industries", IZA Discussion Paper n.9187, July 2015. Comments of Lawrence Kahn, Romain Duval, Eve Caroli, Luc Behaghel, Dominique Meurs, two anonymous referees and participants to the 3rd OECD/IZA Employment Seminar, October 2015, are gratefully acknowledged. All errors are ours. The views expressed here are those of the authors and cannot be attributed to the Bank of Italy, the OECD or its member countries. Andrea Bassanini gratefully acknowledges financial support from CEPREMAP.

Introduction

Flexibility enhancing reforms of product and labour markets feature prominently in the political agenda to boost and sustain economic growth in the long run. And yet, such 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 labour. Firms operating in reformed markets are likely to respond immediately and in the direction of terminating job positions that are (or 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 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 product or labor market reforms, analyzing their long run impact through steady-state comparisons. Evidence on their short-term consequences 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 Employment Protection Legislation – EPL hereafter) or on the regulatory stance in other markets (e.g. the degree of labor market flexibility in the case of pro-competitive reforms of Product Market Regulation – PMR). More generally, a comprehensive assessment of the short-term labour market consequences of product and labor market 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 types of structural reforms that feature prominently in the public discourse as well in the policy-oriented academic literature (see next section): 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 restricting to case studies our cross-country focus allows examining average, rather than country-specific, impacts of policies that crucially alter the economic environment firms operate in. We study the interaction of PMR and EPL reforms with the business cycle, and provide evidence on their interplay with the degree of labor market segmentation and the competitive situation in product markets. We also investigate their interdependence, asking whether the employment effects of each type of deregulation depends on the stringency of the other. 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, which allows us accounting 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 by technological change or shocks to demand). We also address and discuss the severity of concerns arising from simultaneity and reverse causality.

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 à 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 negative impact on employment is likely to be more important if reforms of dismissal regulations are considered to be not credible and at risk of reversal, since employers will accelerate dismissals and delay hiring until the political horizon is clarified (see e.g. Bertola and Ichino, 1995).

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 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.

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 needs to include many lags as in the case of short-term effects estimates. Hence, most cross-country empirical work has focused on quantifying their steady-state (i.e. long-run) effects, typically not accounting for confounding country-specific shocks.[1] Some steady-state analyses exploit reform episodes specific to a country and, when focusing on a short time window around the reform, provide some indirect evidence on its short run effect.

[1] 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.

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).[2] 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, and hence for which 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 deter entry by reorganizing and cutting staff. 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).[3]

[2] 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 (2014) 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.

[3] 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.

A concomitant work by Bouis et al (2016) also looks at the short-term consequences of PMR reforms in network industries. Their results indicate strong positive short term effects on output. However, when looking at employment, they find little evidence of negative effects from product market deregulation, irrespective of the underlying business cycle condition. The possible reasons behind the different results relative to this work will be extensively discussed when presenting our findings.

The empirical setting for estimating employment responses to structural reforms

Our main data source is EUKLEMS, which provides long time series of homogeneous data on 22 business sector industries in 23 advanced economies over 32 years (1975-2007).[4] The analysis adopts industry net job losses (gains) as the main metric to quantify costs (benefits) of reforms. 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, we used the OECD PMR database to construct 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).[5] Labor market reforms

[4] In a sensitivity analysis, the time coverage is extended to 2012 for a few countries by collating EUKLEMS data with data from the most recent version of the OECD STAN database. The mapping between the two databases is however imperfect due to substantial differences in the underlying industry classification (EUKLEMS adopts a specific industry breakdown between 1 and 2-digits of the ISIC rev.3 classification, STAN uses ISIC rev.4). While we exploit a detailed mapping of ISIC rev. 4 industries into ISIC rev.3 industries, our procedure unavoidably introduces measurement error. For this reason, the resulting dataset will be only used in robustness exercises.

[5] 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. When matched with our baseline dataset of industry employment, these data allow studying the evolution of barriers to entry in 3 industries (Energy, Transport and Communications). EUKLEMS allows a finer breakdown into 5 industries for which ETCR data are available (Electricity, Gas, Land transport, Air transport, and Communications) for a very limited number of countries that we will use in a sensitivity analysis in this paper. In principle, the OECD STAN dataset would also allow for a breakdown into 5 industries. However, achieving a suitable coverage in terms of, in particular, the time dimension would require collating different vintages of STAN, mixing data collected and published using two radically-different industry classifications (ISIC rev.4 and ISIC rev.3, the change having being implemented in 2008 in most countries). Absent a precise correspondence table between classifications, extending the time window to the length available in EUKLEMS would likely lead to substantial measurement error. This is because even if the general denomination of the 5 industries did not change between classifications, the underlying sub-industry composition did change. To get a sense of the potential implications of such break one can compare the growth rate of employment in the two most recent edition of STAN using each classification in the overlapping country-year cells. Across the two editions annual employment growth differs by more than 1 percentage points in 27% of the cases, a disparity that can reach 42 percentage points. While focusing on a slightly more aggregate industry classification, our analysis exploits a large time window of homogenous data ending in 2007, thus limiting measurement concerns. In a sensitivity analysis, we use a detailed, country-specific map from ISIC rev.4 industries into ISIC rev.3 at the 1-letter level of ISIC rev.3 for services to extend the sample to 2012.

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 A7.[6]

To estimate the short-term impact of changes in regulation on net job changes we adopt the so-called local projection estimator, a time-series empirical model used to compute unbiased impulse response functions to some event (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, which we cumulate to infer the profile of employment after the reform. In what follows we detail its application to the cases of product and labor market reforms.

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:

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

where $\Delta E_{c,i,t} = \ln L_{\mathrm{c,i,t}} - \ln L_{c,i,t-1}$ measures year-on-year employment growth in a network industry *i*, country *c* and time *t*, ΔBE_{cit} is the change in regulation at *t* and ε is an error term.[7] This autoregressive specification conveniently accounts for delayed effects of reforms through the parameters β_k, as well as for the possible persistence of employment changes.[8] The vector of

[6] 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. As we will see, results are however robust to replacing total employment with wage and salary employment as dependent variable.

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

[8] 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, 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

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 unobserved 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 equation (1) with forward terms of changes in regulation. Finding that future reforms affects current employment would provide evidence of reverse causality. Another consists 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. We also address the issue of omitted confounding factors by exploiting political variables as instrument for deregulation (as suggested by Bertrand and Kramarz, 2002), an approach that requires dropping country-time dummies, however. Last but not least, we will largely rely on response functions that do not factor in the

e.g. Teulings and Zubanov, 2014). This would allow addressing the so-called Nickell-bias issue (Nickell, 1981). Because this will have the drawback of a loss of efficiency, we will compare results from both specifications.

contemporaneous effect (β_0), which is most likely to reflect simultaneity biases (see Teulings and Zubanov, 2014).

More specifically, we implement the local projection method simultaneously estimating a system of $h+1$ equations written as:

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

when η = 1… h, and

$$\Delta E_{cit} = \sum_{k=0}^{T} \beta_{k0} \Delta BE_{ci,t-k} + \sum_{k=1}^{T} \delta_{k0} \Delta E_{ci,t-k} + \mathbb{D} + \varepsilon_{cit},$$

when $\eta = 0$ (which corresponds to specification (1)).[9] In each equation the dependent variable is yearly employment growth (i.e. $\Delta E_{c,i,t}$, $\Delta E_{c,i,t+1}$, …, $\Delta E_{c,i,t+h}$).

The cumulated employment response to a unitary change in regulation at time t measured at time $t+h$ is given by the sequence of the coefficients estimated on $\Delta BE_{ci,t}$ in each equation: $CR(h) = \sum_{\eta=0}^{h} \beta_{0\eta}$. As highlighted above, our preferred estimate of CR in fact discards the contemporaneous effect to limit simultaneity concerns (in that case the function is obtained as: $\widetilde{CR}(h) = \sum_{\eta=1}^{h} \beta_{0\eta}$, discarding the coefficient β_{00} estimated when $\eta = 0$).

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.

It can be argued that the indicator of barriers to entry is very sensitive to even small changes in the legislation. In fact, in our base sample (1975-2007), we count 454 reductions and 38 increases in 1891 observations. Some of these changes might be measured with noise, which would result in imprecise estimates. To overcome this issue, in a robustness exercise we replace ΔBE with

[9] Following Teulings and Zubanov (2014) the set of forward terms ($\sum_{f=1}^{h} \beta_{fh} \Delta BE_{ci,t+f}$) is also introduced, to capture the potentially confounding effects of reforms implemented after time t.

a dummy variable taking value 1 in the case of large reforms – that we define as those implying a change in the indicator of barriers to entry greater than 2 standard deviations. We identified 133 reforms, all corresponding to episodes of deregulation, with no large reform reversal in the sample.

The case of dismissal regulation

In the case of EPL the local projection method illustrated above is applied to a specification 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 specific interest in deregulating (i.e. flexibility-enhancing) policies 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 an indicator rather than by a continuous variable. Accordingly, in our baseline model, we define FE as a dummy variable taking value 1 when the EPL indicator decreases and 0 otherwise (and proceed similarly for PR).[10]

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).

[10] As there are only 26 flexibility-enhancing reform episodes in our baseline sample, it makes no sense to distinguish between large and small reforms. Therefore, in contrast to the case of barriers to entry, our dummies track any change in the EPL indicator. 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 with a specification using the quantitative OECD indicator ($\Delta E_{cjt} = \theta_0 D_j \Delta EPL_{ct} + \sum_{k=1}^{T} \left(\theta_k D_j \Delta EPL_{ct-k} + \rho_k \Delta E_{cjt-k}\right) + X_{cjt}\delta + \nu_{ct} + \nu_{jt} + \nu_{cj} + \xi_{cjt}$), although they are sensitive to the inclusion of Spain in the sample (see Table A5).

Following the literature, we adopt the interaction model pioneered by Rajan and Zingales (1998; see Appendix A3 for details). This amounts to writing the variable of interest as the interaction between the country-level indicator of reforms (FE_{ct}) and an industry-specific measure of exposure to the reform (D_j), measured by the dismissal rate of the corresponding US industry.[11]

The main intuition behind interaction models is that, if EPL reforms have any short-term employment effect these should be stronger among firms that, in the absence of regulation, have greater propensity to make staff adjustments on the external labour market.

The estimation model (written so as to emphasize flexibility-enhancing reforms) then 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 the νs are double-dimensional fixed effects and the matrix W includes the current and lagged changes in the output gap as well as dummies for protection-raising reforms interacted with dismissal intensity ($\sum_{k=0}^{T}\left(\gamma_k D_j \Delta OG_{ct-k}\right)$ and $\sum_{k=0}^{T}\left(\mu_k D_j PR_{ct-k}\right)$, respectively), plus, in robustness checks, other controls to be detailed. Note that FE_{ct} being an indicator variable, 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 the EPL indicator in the sample).

As for the case of PMR, the specification can be extended to quantify the interaction between EPL reforms and business cycle. Perhaps more relevantly, it allows assessing whether allowing for 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 short-term impact on job destruction in dual labour markets, since temporary contracts are likely to be used to fill

[11] 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. As we will show, our results are robust to adopting alternative measures of exposure to the reform.

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 unlikely to be increased by the reform.[12]

Unbiased estimates of the θ coefficients will allow plotting the differential employment response to flexibility-enhancing reforms of high-dismissal industries relative to low-dismissal industries. The cumulated response of employment is obtained, as in the case of PMR reforms, by estimating a system of $h+1$ equations written as:

$$\Delta E_{cjt+\eta} = \sum_{f=1}^{\eta} \theta_{f\eta} D_j FE_{ci,t+f} + \sum_{k=0}^{T} \theta_{k\eta} D_j FE_{cj,t-k} + \sum_{k=1}^{T} \rho_{k\eta} \Delta E_{cj,t-k} + \mathbb{W} + \xi_{cit+\eta},$$

with $\eta = 1\ldots\ h$, and $\mathbb{W} = W_{cjt}\delta_\eta + \nu_{\eta,ct} + \nu_{\eta,jt} + \nu_{\eta,cj}$ (the above equation corresponding to specification (2) when $\eta = 0$). The differential cumulated employment response at time *t* measured at time $t+h$ is given by the sequence of the coefficients estimated on $D_j FE_{cj,t}$ in each equation: $CR(h) = \sum_{\eta=0}^{h} \theta_{0\eta}$. 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) are also unlikely to drive or contaminate our results: their aggregate impact is absorbed by country-year fixed effects and their effect is unlikely to vary as a function of dismissal intensity.[13] To substantiate this hypothesis, we will show that the interactions between current and lagged changes in these institutions and dismissal intensity (D_j) have little or no explanatory power for changes in employment (see the results section).

[12] If any, a reduction in EPL for open-ended contracts would increase incentives to convert fixed-term contracts in open-ended ones (see for example Cahuc, Charlot and Malherbet, 2016).

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

Empirical evidence on the short-term consequences of structural reforms

Lowering barriers to entry in product markets

Table 1 presents baseline estimates of the relationship between year-on-year changes in the indicator of stringency of entry barriers and log employment in network industries. Column 1 focuses on the simple simultaneous association (i.e. imposing T=0 in model (1)). The estimated coefficient suggests that, controlling for aggregate effects and industry-specific shocks, a 1–point fall in the regulation index (which ranges from 0 to 6) is associated with a decrease in employment of 0.66%.[14] The short-term loss is appreciable 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 lagging the regulatory variable one year (col. 2). These findings are essentially confirmed in the remainder of the table, allowing for more complex dynamic structures of the model. Specifically, we first focus on the autoregressive distributed lag model (1), where the lag length (T=2) has been determined minimizing the BIC statistic. Results in column 3 are obtained imposing no contemporaneous coefficient (β_0 = 0); column 4 looks at the unrestricted version. Second, we estimate a model excluding the lags of ΔE_{cit} and including additional lags of ΔBE_{cit} (column 5; see footnote 8 for the discussion of this type of specification).

Figure 1 shows that the implied employment response to lower barriers to entry is U-shaped, with a loss reaching its maximum 3 years after the reform and then being gradually reabsorbed. Both Panels refers to a reform that lowered the OECD regulation index by 1 point, and are based on the coefficients estimated in column 4. The important difference is that Panel A plots the function computed without factoring in the contemporaneous effect of changes in barriers to entry β_{0h}, a conservative assumption motivated by the uncertainty on the reliability of the parameter. In this case, industry employment would be around 1.2% below its initial level in the

[14] 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.

[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

third year following that reform. The cumulated response function in Panel B, which accounts for of all estimated βs, indicates a larger aggregate employment fall, close to 2%.

The U-shaped pattern of employment is consistent with the idea that labor 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. Allowing the employment impact of deregulation to vary along the cycle provides supportive evidence for the latter hypothesis, as is illustrated by Figure 2. The two panels plot the employment response to a reform implemented when the growth rate of the output gap is, respectively, one percentage-point larger (upturns) and 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 no-reform scenario during a downturn. In upturns, the fall is smaller and not statistically significant.

The role of employment protection legislation. If the costs of PMR deregulation are mainly due to large firm downsizing (due to e.g. entry deterrence) they should be larger in countries were dismissals are less costly.[16] As flexibility-enhancing reforms of dismissal rules are rare and the EPL indicator varies much more across country than over time (see above), we split the sample into two

[16] On the other hand, these costs could be shorter-lived if lighter EPL leads to more hiring / faster reallocation of displaced workers.

groups of strict EPL and light EPL countries.[17] Panels A and B of Figure A1 show cumulated employment responses for the two groups of countries, respectively. Deregulation has significant negative short-run employment effects only in the case of light EPL countries. The employment response in strict EPL countries is not statistically different from zero, albeit also negative. Yet, this latter result is explained by a handful of observations of those four initially-highly-regulated countries whose level of the EPL indicator eventually fell below the threshold because of significant reforms (Finland, Korea, Austria and Spain). Excluding post-reform observations for these countries (around 10% of the observations in the group) shows that the employment response in high-EPL economies is flat in the first three years (and it becomes almost significantly positive afterwards, see Panel C). This suggests that product market deregulation tend to be more costly the lighter the regulation of dismissals.

Robustness and specification tests. As discussed above, the main threat to identification of equation (1) is the possibility that reforms are implemented in response to changes in industry employment. In order to explore this issue, the baseline model is re-estimated including one forward term – 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. Adding up to two to four leads of the regulation variables only marginally affects the baseline results of Table 1, with the additional terms attracting non-significant coefficients.[18]

The employment response pattern of Figure 1 is confirmed across a number of other sensitivity and specification checks, summarized in Table 3. Specifically, in columns 2 to 4 the

[17] For each country, we take the earliest available value of the EPL indicator for individual dismissals over the sample and we take the cross-country median of these values. Then we define strict-EPL countries as those above this cross-country median. Defining then as light-EPL countries the other countries, we end up with two groups of approximately equal size.

[18] The only implication of adding more forward terms is that the effect of deregulation would appear slightly less immediate but more persistent. The first lag of the change in barriers to entry becomes insignificant in these models but the sum of the coefficients of the first and the second lag of this change become significant (while they were not significant in our baseline models). Standard Granger causality tests of whether current changes in barriers to entry are affected by past changes in employment yield similar conclusions. Specifically, Table A1 in the Appendix reports F-tests for the (joint and separate) significance of parameters π_1 and π_2, estimated in models 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 Table 2, past employment changes do not have a significant impact on current changes in regulation (neither separately nor cumulatively).

regression underlying the response function (col. 4 in Table 1) is augmented by including 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).[19] Column 7 changes the definition of the dependent variable (i.e. using salaried as opposed to total employment), and column 8 uses unweighted rather than weighted estimators.

Finally, the results are robust to variations of the sample of countries, as shown in detail in Appendix Figure A2), to extending the time window to include the Great Recession years (see Table A2), to redefining the indicator of barriers-to-entry deregulation as a dummy variable taking value 1 in the case of large reforms and 0 otherwise (Table A3), as well as to disaggregating further the industry breakdown up to 5 industries (Table A4).[20]

Instrumental variable results: We also address the issue of omitted confounding factors by exploiting political variables as instrument for deregulation, as suggested by Bertrand and Kramarz (2002), an approach that requires modifying the baseline empirical specification. From the Comparative Political Data Set (Armingeon et al. 2014) we drew measures of the political color of the government, the size of its parliamentary majority, the newness of the government, and of whether it is composed by technocrats or follows a technocratic one.[21] Since all these variables only vary by country and time, country-year dummies are replaced by a set of co-variates capturing

[19] 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).

[20] When using a breakdown of network industries into 5 industries, our estimates suggest a delayed effect of the reforms, which is significant only after 2 lags. Yet, this does not seem to be a consequence of the change in industry breakdown but rather of the change in the underlying sample, as the number of country-year pairs falls from 587 in Table 1 to 431 in Table A4. Indeed, estimating our model in this smaller subsample of country-year pairs after having re-aggregated the data from the 5-industry breakdown into the original 3-industry partition leads to the same result (col. 2).

[21] More precisely, we use four instruments: a dummy for change in the ideological composition of the government, the share of right-wing seats in parliament, a dummy for technocratic new government, and a dummy for political government following a technocratic one. The idea is that reforms are more often undertaken by governments as soon as they take office, and in particular if they are right-wing or technocratic with large parliamentarian support. By contrast, political governments succeeding technocratic ones are less likely to deregulate further once elected, no matter their political colour. The choice of instruments (among all possible political variables) is also guided by their significance in the first-stage regressions.

the aggregate dynamics of the labour market.[22] The identification assumption is that, conditional on aggregate labour market performance, political variables affect performance in network industries only through changes in industry regulation.[23]

The IV results are reported in Table 4.[24] Odd columns exploit all available instruments while even columns present exactly-identified models using only the most significant instrument; columns 3 to 6 also include deregulation of public ownership, considered alternatively as an exogenous or endogenous co-variate. The estimated effect of changes in barriers to entry are always statistically significant at the 10% level and are 3 times larger than OLS estimates even in exactly-identified models. This indicates that OLS might be biased towards the origin, for example because deregulation is more likely when industry employment is growing. At the same time, IV estimates are less precise than OLS suggesting that the latter can still be considered as conservative lower bounds to the true effect.

Comparison with related work: In a concomitant work, Bouis et al (2016) also look at the short-term consequences of reforming entry regulation in network industries, finding different results. Their preferred estimates imply that deregulation has negative employment effects on impact, which are just marginally below standard levels of statistical significance, however, and reabsorbed already in the first year.

There are several candidate explanations for the different results in the two papers, which otherwise apply the same methodology (the local projection estimator). First, Bouis et al (2016) adopt a less stringent empirical specification, absorbing industry-specific confounders through industry-specific (linear) trends as opposed to industry-year dummies. The latter approach allows to fully account for the possibility that competition enhancing reforms in an industry may correlate

[22] These include changes the logarithm of the harmonised unemployment rate (ILO definition), lagged changes in the output gap and changes in a number of labour market institutions that are typically included in aggregate regressions of labour-market performance and that are also used as covariates in the analysis of EPL reforms (see above). Specifically: unemployment-benefit average net replacement rate, the average labour tax wedge, collective bargaining coverage, collective bargaining coordination, and the index of EPL on temporary contracts, plus the index of EPL for regular contracts.

[23] Conditional on aggregate labour market performance, it seems unlikely that a negative employment shock to one network industry might lead to major political changes, given that these industries are small. As a further sensitivity check we re-estimate the two baseline IV models excluding transport and storage, the only industry representing more than 2.5% of business-sector employment, and obtain similar results.

[24] In order to avoid inflating the number of instruments required for identification, Table 4 reports only IV estimates of the model with no lagged terms (note that, in IV estimates, contemporaneous terms are interpretable).

with shocks to global demand or technology in that industry, and encompasses the former as a specific case. Empirically, this difference matters: replicating our baseline regression using industry-specific trends implies a much smaller (non-significant) employment fall on impact which is quickly reabsorbed in the following years (see Appendix Figure A3). This suggests that industry deregulation is positively correlated with shocks to industry employment, and not taking this into account would lead to upward-biased estimates. [25]

Second, Bouis et al (2016) measure deregulation in network industries through an indicator variable denoting the subset of "large" reforms, as opposed to our yearly changes in the regulation index. We adopted a similar approach in our context using a large reforms dummy indicating any change in the OECD regulation index that was larger than twice its standard deviation (in absolute value). Our main findings were not affected (see Appendix Table A3), suggesting that the change of variable is not a relevant driver of the difference in findings.[26]

A third issue is that Bouis et al. (2016) conduct their analysis using a disaggregation into 5 network industries, while we adopt a 3-industry classification in our baseline specifications. Yet, the different level of disaggregation does not appear to matter either, as we show exploiting the availability of a finer industry breakdown for a subset of our countries and years in our dataset (see above and Appendix Table A4).

One last potential source of divergence, whose relevance cannot unfortunately be assessed directly, is the different source of industry data underlying the two analyses.[27]

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[25]Bouis et al (2016) justify their choice by arguing that reforms in one industry often occur at the same time in all countries. In our sample, however, reforms are rather staggered: they most often occur in a minority of countries in the same year and never in more than 75% of them. This suggests that the within industry performance of reforming and non-reforming countries can always be meaningfully compared even controlling for common shocks affecting the industry each year.

[26] Bouis et al (2016) identify large reforms by looking for evidence of legislative action on the domains of network industry browsing various editions of the OECD Economic Surveys (a "narrative approach" in the spirit of Romer and Romer, 2004). We are unfortunately unable to check how their list of large reforms differs from ours. Identifying large reforms as those with a change of the indicator at least twice as large as the standard deviation delivers a sample of 133 reform episodes, which is close to the figure reported by Bouis et al. (2016) and suggests that the two variables may not differ significantly.

[27] While we use industry data from EUKLEMS, the baseline dataset in Bouis et al. (2016) is assembled collating different vintages of OECD STAN data. As mentioned in the data section, obtaining a long enough time series of data in STAN requires dealing with the changes in the industry classification that occurred at the end of the 2000s, which is a difficult task and may lead to serious measurement error. A more precise assessment of the severity of this issue would only be possible knowing the details of the methodology adopted by the authors to preserve the concordance, but this information is unfortunately unavailable.

Lowering dismissal costs on regular contracts

In the short run, flexibility-enhancing reforms of dismissal legislation are estimated to significantly lower employment in dismissal-intensive relative to other industries. Table 5 reports 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. While the above results are obtained weighting observations with the industry employment share, columns 3 and 4 report estimates obtained from unweighted regressions. Finally, in columns 5 and 6 the two initial models are replicated on an sample covering the period 1985-2012 (this extended sample is matching EUKLEMS data to STAN). All regressions also include interactions for protection raising reforms (about one third of the reform episodes in the data), whose estimated coefficients are insignificant and are therefore not reported.[28]

Taking point estimates at face value, a deregulation reform of average historical size, picked up by the dummy variable $FE_{c,t}$, would 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 a dismissal-intensive industry (Wood) would fall by about 0.5 percentage points relative to Motor vehicles in the reform year. The table also suggests that this negative effect would persist in the year following the reform and start reverting afterwards.

Figure 3 plots the cumulated response implied by the coefficients estimated in column 1. Specifically, it illustrates the relative employment patterns of two industries whose dismissal rates

[28] The estimated parameters on the interaction terms $D_j PR_{ct-k}$ are always highly insignificant statistically and very small quantitatively. In the baseline regressions (cols. 1 and 2), for example, PR reforms have a positive contemporaneous impact on employment which is, respectively, ten and five time smaller than the coefficient on flexibility enhancing reforms. Both coefficients are highly insignificant, just as those on the lagged terms. These results strongly suggest that increasing EPL does not hurt employment in dismissal-intensive industries.

differ by 1 percentage point. On impact employment is estimated to fall by 0.3% more in the dismissal-intensive industry than in the other industry. This gap reaches a cumulative loss of 0.4% about one year after the reform, and start recovering afterwards.

Robustness and specification tests. 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. Table 6 shows that the impact of EPL reforms is not altered by augmenting equation (2) with relevant observable policies and institutions.[29] The (negative) coefficient on the contemporaneous effect of flexibility enhancing reforms, in particular, remains very similar in magnitude to that of the baseline specification.[30]

The Appendix reports a number of other robustness checks of the baseline estimates including (i) measuring EPL reforms through changes in the corresponding quantitative OECD indicator (a specification similar to that estimated in the case of PMR, see Table A5)[31]; (ii) changing the industry interaction variable replacing US layoff rates with, e.g., UK layoff rates (Table A6);[32] and (iii) changing the sample excluding countries one-by-one (see Figure A4).[33]

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 (see e.g. Cahuc and Zylberberg, 2004, or Cacciatore et al., 2016). We

[29] The labour market institutions considered include: the index of employment protection legislation for regular contracts, unemployment-benefit average net replacement rate, the average labour tax wedge, collective bargaining coverage, collective bargaining coordination, and the index of employment protection legislation on temporary contracts (see footnote 13). Their estimated coefficient has no obvious interpretation in this interaction framework, except for EPL for temporary workers, whose impact might plausibly be stronger in high layoffs industries. The insignificant coefficient estimated in col. 6 of Table 6 suggests no direct impact on employment of reforms of hiring rules for temporary contracts. This is consistent with the findings of Kahn (2010), who also found evidence that, while not affecting overall employment, such reforms lead to a shift away from permanent to temporary jobs.

[30] By contrast the coefficients of other institutional variables (interacted with US layoff rates) are always insignificant (we report only the contemporaneous coefficient for brevity), consistent with our identification assumption, with the only partial exception of net replacement rates. Indeed, the coefficient of the latter is significant in the baseline specification when both EPL reforms and the output gap (interacted with the US layoff rates) are included, but becomes insignificant if some of these variables are removed, showing that its significance is less robust than that of EPL reforms which are always significant no matter the controls included (results available from the authors upon request).

[31] This is conditional 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.

[32] We also exploited the availability of country-industry layoff rates for a (small) set of countries (Australia, France, Germany, the United Kingdom and the United States) to compute a third indicator that does not reflect time invariant country specificities. Following Ciccone and Papaioannou (2006), this was obtained estimating an industry dummy in a country-industry layoff rate regression on country and industry fixed effects.

[33] The effect of the reform is remarkably stable except when removing 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.

found support for this prediction interacting our reform variable with the year-on-year change in the output gap. The two panels of Figure 4 replicate the thought exercise of Figure 3 in the case of two reforms implemented when the output gap falls – i.e. in a downturn – and when it rises (in both cases, by 1 percentage point). In a downturn, the employment contraction is twice as large as in the case of upturns on impact, and persists for at least 2 years. Specifically, 2 years after a reform undertaken when the output gap is falling, employment would be 0.7% lower in the dismissal-intensive industry than in a less-intensive industry.

Interestingly, interacting EPL reforms with the level (as opposed to the change) of the output gap yielded no significant coefficient. Taken together, the two results suggest that whether the economy is contracting or expanding matters more than its distance to potential. Put another way, they suggest that reforms implemented when the 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 are unlikely to increase the incentive to terminate temporary contracts, their short-term impact on job destruction can be expected to be limited in very segmented labour markets. The estimated employment response plotted in Figure 5 suggests that this is indeed the case. EPL reforms have negligible effects if the share of temporary contracts is around 15% (that is 5% above the sample median, see Panel A). By contrast they have a sizable impact if this share is around 5% (Panel B). Taken at face value, in non-segmented markets employment in a dismissal-intensive industry would be 1.1% lower than in a less-intensive industry 1 year after the reform.

The role of barriers to entry. Theory and empirical evidence suggests that lower EPL heightens both entry and exit of firms (see e.g. Koeniger, 2002, Bottasso, Conti and Sulis, 2017). Would then lower administrative barriers to entrepreneurship attenuate the costs of flexibility enhancing EPL reforms? One can expect this to occur if low barriers enhance the positive effect of EPL reforms on firm entry and hiring, at least initially. However, greater competition might also imply that firms are more reactive to changes in the costs of firing, leading to greater employment adjustments. And the higher entry of firms could imply more hiring on risky and non-viable activities, leading to subsequent greater exit. We investigate this empirical question in the same way as we did for the symmetric case (the impact of EPL levels on the effect of PMR reforms), and split the sample in two groups of countries with high and low economy-wide administrative barriers to entry.[34] As shown in Figure A5, our estimates imply that EPL reforms have a significant negative employment impact only in high-barriers countries (five times greater on impact, than in deregulated countries) suggesting that the higher entry/higher hiring channel dominates.[35]

Aggregate effects: The results in Table 5 are consistent with standard models with adjustment costs, which imply that dismissal regulations have a greater impact on employment adjustments in industries featuring more volatile optimal employment level (and therefore greater dismissal intensity; see e.g. Cahuc and Zylberberg, 2004, Bassanini and Garnero, 2013). In these models, reform-induced adjustments have the same sign in all industries, implying that the estimated differential effect would also be informative on changes in aggregate employment. This theoretical conclusion, however, is conditional on the assumption that cross-industry general equilibrium effects of EPL are not too large. It would not hold if, in particular, by increasing separations in high-dismissal industries the reform expanded labour supply and employment in low-dismissal industries (Zweimüller, 2009).

In practice, however, these potentially offsetting general–equilibrium effects have been shown to be small, as very few transitions involve a change in industry[36]. This suggests that the

[34] For each country, we take the earliest available value of the average of economy-wide indicators for administrative barriers to start-ups for corporations and sole proprietors (sourced from the OECD Regulation Database) and we take the cross-country median of these values. Then we define low-barrier countries as those below the median. Defining then as high-barrier countries the other countries, we end up with two groups of approximately equal size.

[35] There is also some (albeit statistically insignificant) evidence that this higher entry also leads to greater exit and subsequent job destruction, since the negative impact of EPL in low-barrier countries is somewhat delayed (although due to the lack of precision of the estimates one needs to be very cautious in interpreting the results).

[36] For example, Bassanini et al. (2009) shows that, at the 2-digit level, industries are quite segmented: separations leading to an industry change are only a small fraction of all separations. Moreover, Bassanini and Garnero (2013), using

potential positive employment effects of reforms in low-dismissal industries are unlikely to offset the negative effects in high-dismissal industries, which translates in an overall negative short-run employment impact of EPL reforms (whose magnitude, however, cannot be estimated in our framework).

Further evidence suggesting that EPL reforms have negative short term aggregate effects is provided in Appendix A4, where we assessed the consequences of three recently implemented reforms on unemployment. These implied sizable, one-shot reductions in EPL for regular contracts in 3 OECD countries: Estonia (2009), Spain (2012) and Slovenia (2013). The analysis, conducted using a thoroughly different empirical approach and data, confirms that lowering dismissal regulation entails aggregate employment costs in the short term, as captured by a temporary increase in the average probability of unemployment (see Appendix for details).[37]

Conclusions

We investigated the short-term effects of reforms that ease anti-competitive product market regulation and employment protection legislation exploiting long time series of country industry data. Our analysis shows that both can entail significant short-term losses in terms of employment. Significantly lowering entry barriers in network industries (energy, transportation and communication industries), for example, induces industry employment falling below the pre-reform level during the first three to four years, with a maximum fall ranging between 1.2%-2%. Similarly,

multiple identification strategies, find no long run effect of EPL for regular contracts on job-to-job separations entailing a change of industry.

[37] The main exercise exploits individual (Labor Force Survey) data and a difference-in-differences setting to study changes in the probability of unemployment following the 2009 EPL reform in Estonia. Lithuania, a small-open economy featuring similar pre-reform dynamics of economic activity and unemployment (Figs. A6 and A7) but no policy changes, is used as a control. The results show that unemployment probability in Estonia increased by a sizable 1.5-percentage-point in the [two] years following the reform (Table A8); they are robust to a battery of specification, sensitivity and falsification checks (see the Appendix for details). A second exercise estimates the change in aggregate unemployment around each of the three reform episodes, estimating the same regression discontinuity model in each country. Comparing the results across reforms is interesting because of different extents of the reforms (among the largest in Estonia and Slovenia, close to the median in Spain - based on the OECD indicator), different phases of the business cycle at implementation (the onset of a large downturn in Estonia, and just before the crisis trough in Slovenia and Spain), and the different shares of fixed term contracts (very small in Estonia, and average in Slovenia and close to the largest in Spain). Consistent with the results from the cross-country-industry analysis, Estonia experienced the highest increase in unemployment, while the effects of the reform were smaller in Slovenia and statistically insignificant in Spain (Table A9).

the year following the typical EPL reform aggregate employment is estimated to be significantly below the pre-reform level.

The losses are higher for policies implemented during downswings, when their political cost may appear lower, but much smaller and statistically insignificant when reforms are introduced 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). Finally, while the costs of lowering entry barriers are higher when EPL is light, those of dismissals legislation benefits from light PMR. Based on the latter finding, a highly regulated country interested in reforming both domains could minimize the short-term costs by deregulating product markets first. While not immune from estimation concerns, the results prove fairly robust to an ample set of tests, and are confirmed by micro-econometric analysis of three recent reforms of employment protection legislation.

All in all, our findings 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 labor 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.

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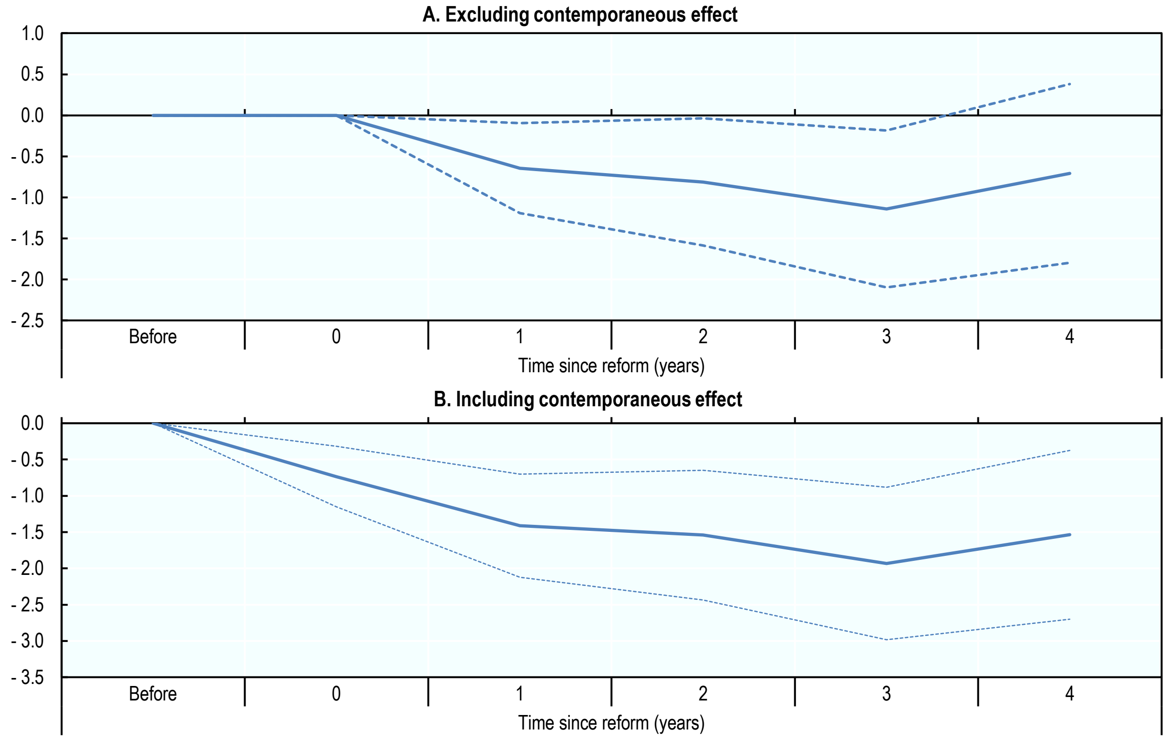
Figures

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

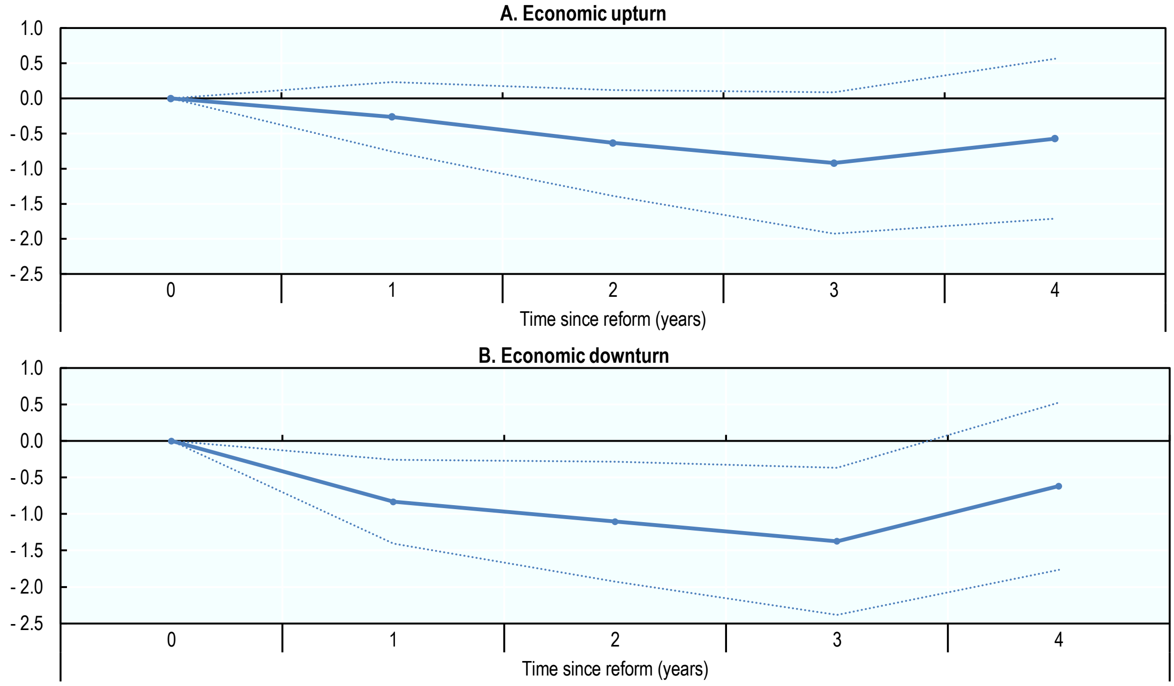
Estimated cumulative change in industry employment up to 4 years following the reform, in percentage



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 contemporaneous and lagged values of industry regulation as well as on lagged employment changes. The response function in Panel A is computed setting the contemporaneous effect of the reform to 0; the estimated contemporaneous effect is accounted for in Panel B. See the text for more details. Confidence intervals are obtained by clustering errors on countries and industries.

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

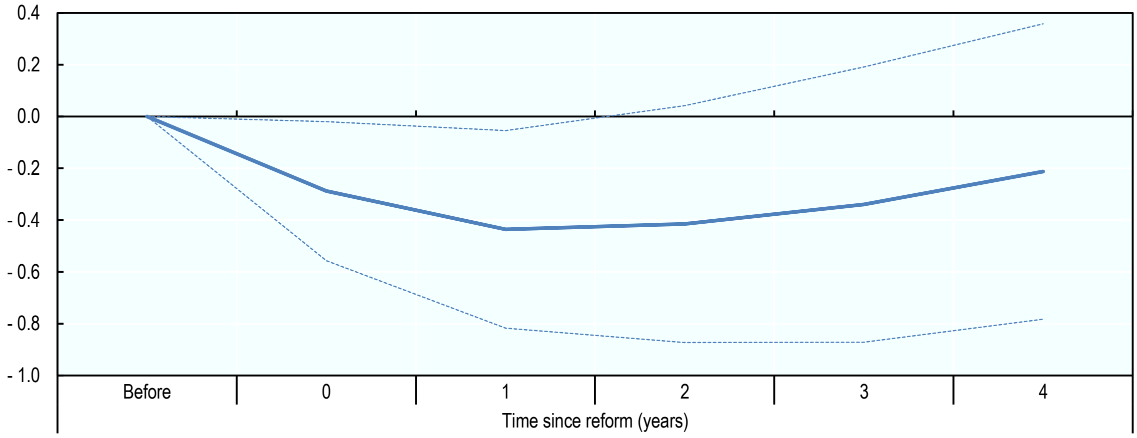
Estimated cumulative change in industry employment up to 4 years following the reform, in percentage



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 is set to 0, as in Panel A of Figure 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**

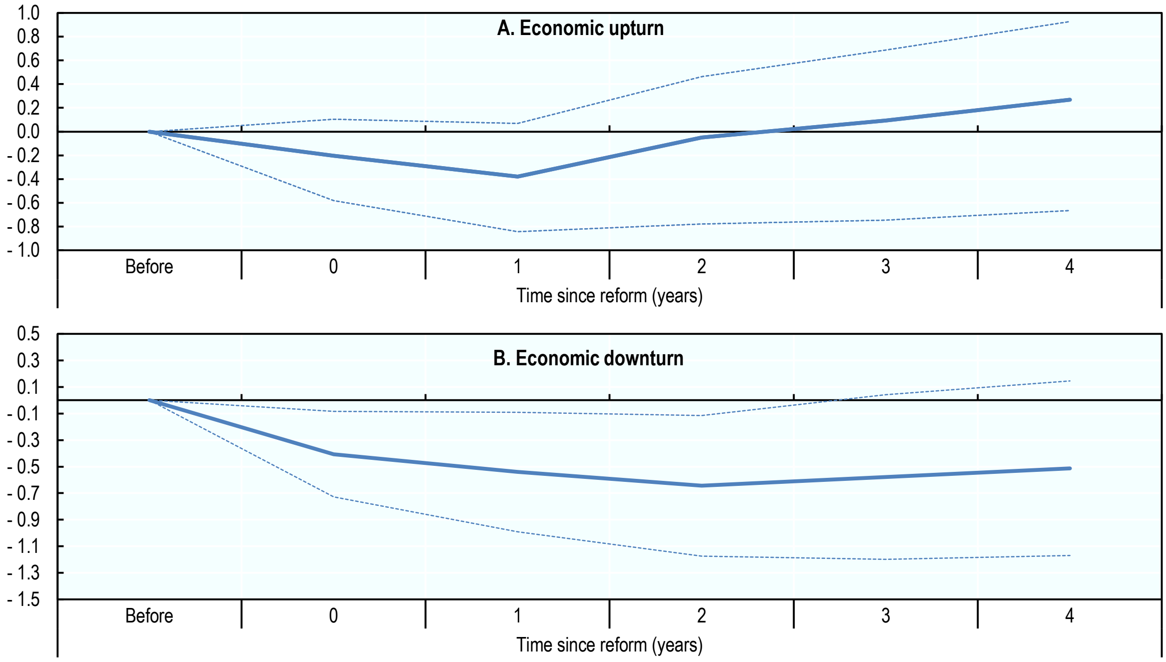
Estimated cumulative change of the employment percentage-point gap between 2 industries whose US dismissal rates differ by 1 percentage point, up to four years following the reform



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 the difference in wage and salary employment levels between 2 industries in the non-agricultural/non-mining business sector whose US dismissal rates differ by 1 percentage point. Estimates are obtained allowing for 2 lags in model (2), with the difference in employment between the dismissal-intensive and the other industry normalised to 0 before 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) on the dismissal-intensive industry with respect to the other industry. 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**

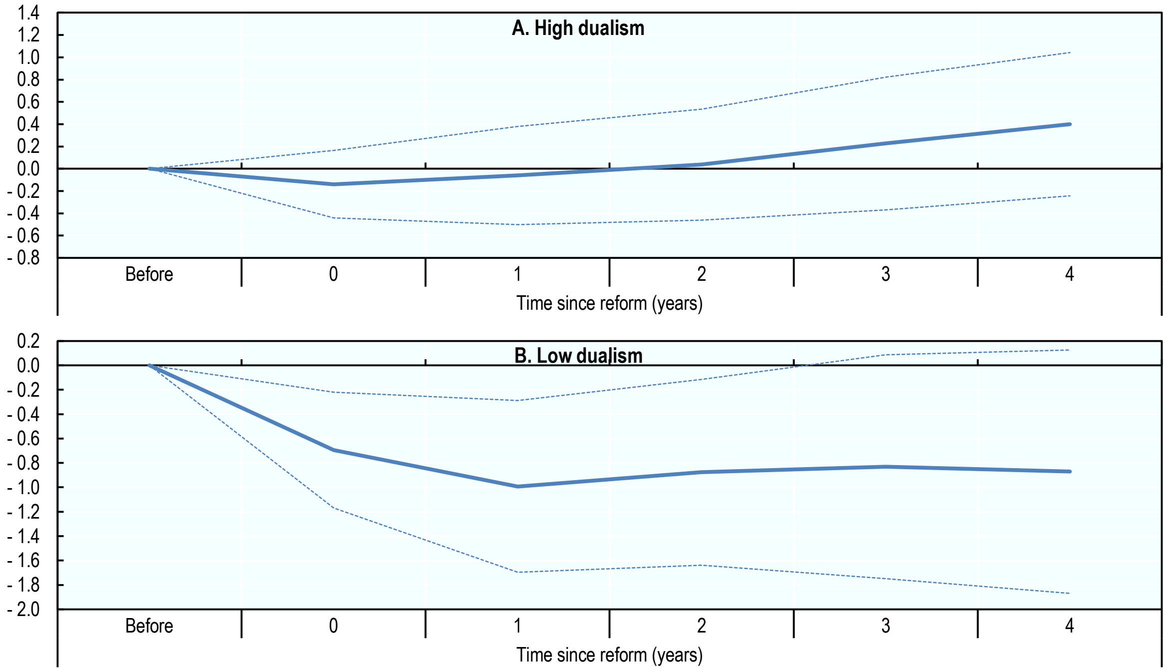
Estimated cumulative change of the employment percentage-point gap between 2 industries whose US dismissal rates differ by 1 percentage point, up to four years following the reform



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 the difference in wage and salary employment levels between 2 industries in the non-agricultural/non-mining business sector whose US dismissal rates differ by 1 percentage point, with the difference in employment between the dismissal-intensive and the other industry normalised to 0 before the reform. 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. The chart reports estimated effects conditional to these two cases. 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) on the dismissal-intensive industry with respect to the other industry. 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 the employment percentage-point gap between 2 industries whose US dismissal rates differ by 1 percentage point, up to four years following the reform



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 the difference in wage and salary employment levels between 2 industries in the non-agricultural/non-mining business sector whose US dismissal rates differ by 1 percentage point, with the difference in employment between the dismissal-intensive and the other industry normalised to 0 before the reform. The effect of EPL in different industries is allowed to vary as a function of the incidence of fixed-term contracts in each country and year. The 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. Panel A and B presents estimated effects conditional to these levels of incidence of fixed-term contracts. 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. 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 (1), with *ΔBE* measuring changes in the OECD index of entry barriers in network industries and *logE* measuring log industry employment. Observations are weighted with the average (1975-2007) industry employment share in the country. 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)	(6)	(7)
Δ BE_{cit}	0.0067**			0.0074***	0.0072***	0.0078***	0.0084***
	(0.0026)			(0.0026)	(0.0026)	(0.0028)	(0.0030)
Δ BE_{cit-1}		0.0066*	0.0057*	0.0057*	0.0068*	0.0053	0.0048
		(0.0033)	(0.0032)	(0.0033)	(0.0035)	(0.0033)	(0.0033)
Δ BE_{cit-2}			0.0009	0.0019	0.0015	0.0028	0.0038
			(0.0034)	(0.0034)	(0.0036)	(0.0033)	(0.0035)
Δ BE_{cit-3}					0.0027		
					(0.0034)		
Δ BE_{cit+1}	-0.0006	0.0002	-0.0001	-0.0000	0.0007	-0.0008	-0.0022
	(0.0031)	(0.0031)	(0.0029)	(0.0029)	(0.0030)	(0.0030)	(0.0031)
Δ BE_{cit+2}						0.0047	0.0043
						(0.0031)	(0.0032)
Δ BE_{cit+3}							-0.0008
							(0.0030)
Δ BE_{cit+4}							0.0032
							(0.0032)
Observations	1822	1764	1684	1684	1626	1,615	1,479
R-squared	0.654	0.654	0.649	0.652	0.645	0.660	0.682
$\Delta BE_{cit-1}+\Delta BE_{cit-2}$ (P-value)			0.166	0.110	0.111	0.089*	0.097*

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on EUKLEMS data. *ΔBE* measures changes in the OECD index of entry barriers in network industries. The estimates of the first five columns are obtained augmenting each specification in Table 1 with one forward term ($\Delta BE_{ci,t+1}$). Columns 6 and 7 augment the preferred specification with 2 and 4 forward terms, respectively. Two lags of the dependent variable are included in Columns 3,4,6 and 7. The last line reports the p-value of a F test on the significance of the sum of the coefficients of $\Delta BE_{ci,t-1}$ and $\Delta BE_{ci,t-2}$. Observations are weighted with the average (1975-2007) industry employment share in the country. All specifications include 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. **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)
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 (1), with ΔBE measuring changes in the OECD index of entry barriers in network industries. Observations are weighted with the average (1975-2007) industry employment share in the country, 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: $WBE_{dit} = \sum_{-i} Exp_{i,-i} * BE_{c,-i,t}$, where $Exp_{i,-i}$ are coefficients from the US Inverse Leontief Matrix measuring how many units of input *-i* (e.g. energy) are required (at any stage of the value chain) to produce one additional unit for final demand in network industry *i* (transport). All specifications include two lags of the dependent variable as well as 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 4. **IV estimates**

	(1)	(2)	(3)	(4)	(5)	(6)
Instruments:	All instr	New gov only	All instr	New gov only	All instr	New gov + After tech
Other PMR var:	-	-	Pub Own (exog)	Pub Own (exog)	Pub Own (endog)	Pub Own (endog)
Δ BE_{cit}	0.0331***	0.0234*	0.0336***	0.0246*	0.0285**	0.0220*
	(0.0123)	(0.0134)	(0.0126)	(0.0136)	(0.0140)	(0.0114)
Observations	933	933	933	933	933	933
R-squared	0.288	0.348	0.292	0.348	0.220	0.335
AP F-test Δ BE_{cit}	7.24	10.25	6.85	10.07	6.69	13.72
AP F-test Δ $PubOwn_{cit}$					2.78	5.50
Overid. (P-value)	0.377		0.380		0.396	

Notes: The dependent variable is the yearly growth rate of total employment in network industries computed on EUKLEMS (1975 2007) data. ΔBE measures changes in the OECD index of entry barriers in network industries. All covariates except changes product market regulation are treated as exogenous. Extent of right-wing government support, a dummy for change in the ideological composition of the government (New gov), a dummy denoting start of technocratic governments and a dummy denoting start of a political government after a technocratic one (After tech) are all used as instruments for endogenous variables except where differently specified. AP F is the Acemoglu-Pischke first-stage F statistics. Only P-values are reported for Hansen J overidentification tests. All specifications control for country-by-industry and industry-by-time dummies as well as changes in the logarithm of the unemployment rate, lagged changes in the output gap and changes in the index of employment protection legislation for regular contracts, unemployment-benefit average net replacement rate, the average labour tax wedge, collective bargaining coverage, collective bargaining coordination, and the index of employment protection legislation on temporary contracts. PubOwn stands for the degree of public ownership (variable PUB OWN in the ETCR database). This variable is not included in Columns 1 and 2, is treated as exogenous in Columns 3 and 4 and as exogenous in Columns 5 and 6. Observations are weighted with the average (1975-2007) industry employment share in the country. The base sample is the EUKLEMS sample (1985 2007). Standard error 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		Base sample, unweighted		Extended sample	
	(1)	(2)	(3)	(4)	(5)	(6)
FE_t*DR	-0.0029*	-0.0030*	-0.0032**	-0.0033*	-0.0031**	-0.0033**
	(0,0016)	(0,0017)	(0,0016)	(0,0017)	(0,0014)	(0,0015)
FE_{t-1}*DR	-0.0009	-0.0009	0.0018	0.0020	-0.0004	-0.0006
	(0,0016)	(0,0014)	(0,0020)	(0,0020)	(0,0017)	(0,0014)
FE_{t-2}*DR	0.0009	0.0008	0.0012	0.0014	0.0003	0.0003
	(0,0016)	(0,0014)	(0,0014)	(0,0013)	(0,0013)	(0,0014)
FE_{t-3}*DR	0.0001		-0.0007		0.0005	
	(0,0014)		(0,0016)		(0.0018)	
Observations	7.590	8.052	7.590	8.052	9.091	9.574
R-squared	0.532	0.510	0.399	0.390	0.529	0.524

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. Observations are weighted by the average industry share in the country's non-agricultural/non-mining business sector, except in cols. 3 and 4. 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 6. **Robustness to other institutional reforms**

OTHER_REF is:	Base (1)	Unemployment Benefit (NRR) (2)	Average tax wedge (3)	Wage coordination (4)	Union Coverage (5)	EPL on Temporary (6)
FE_t*DR	-0.0030*	-0.0026*	-0.0029*	-0.0031*	-0.0028*	-0.0028*
	(0.0016)	(0.0016)	(0.0016)	(0.0017)	(0.0017)	(0.0016)
$OTHER\_REF_t$*DR		-0.0002*	-0.0001	0.0016	0.0002	0.0009
		(0.0001)	(0.0002)	(0.0013)	(0.0002)	(0.0013)
Observations	8052	7524	7480	8052	7106	8052
R-squared	0.526	0.574	0.579	0.526	0.595	0.526

Note: The dependent variable is the yearly growth rate of wage and salary employment. FE: dummy variable for flexibility-enhancing reforms of EPL for regular contracts; DR: industry-level US dismissal rate (in %); OTHER_REF: change in the policy/institutions indicated in the column title. All specifications control for a dummy for protection-raising EPL reforms and for changes in the output gap (both interacted with DR and lagged as FE reforms), two lags of changes in log employment, EPL and other reforms, as well as country by time, industry by time and country-by-industry dummies. UB average net replacement rate, average collective bargaining coverage and the average labour tax wedge are in percentages. 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. The base sample is the EUKLEMS sample (1985 2007). Standard error 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.

SUPPLEMENTARY APPENDIX

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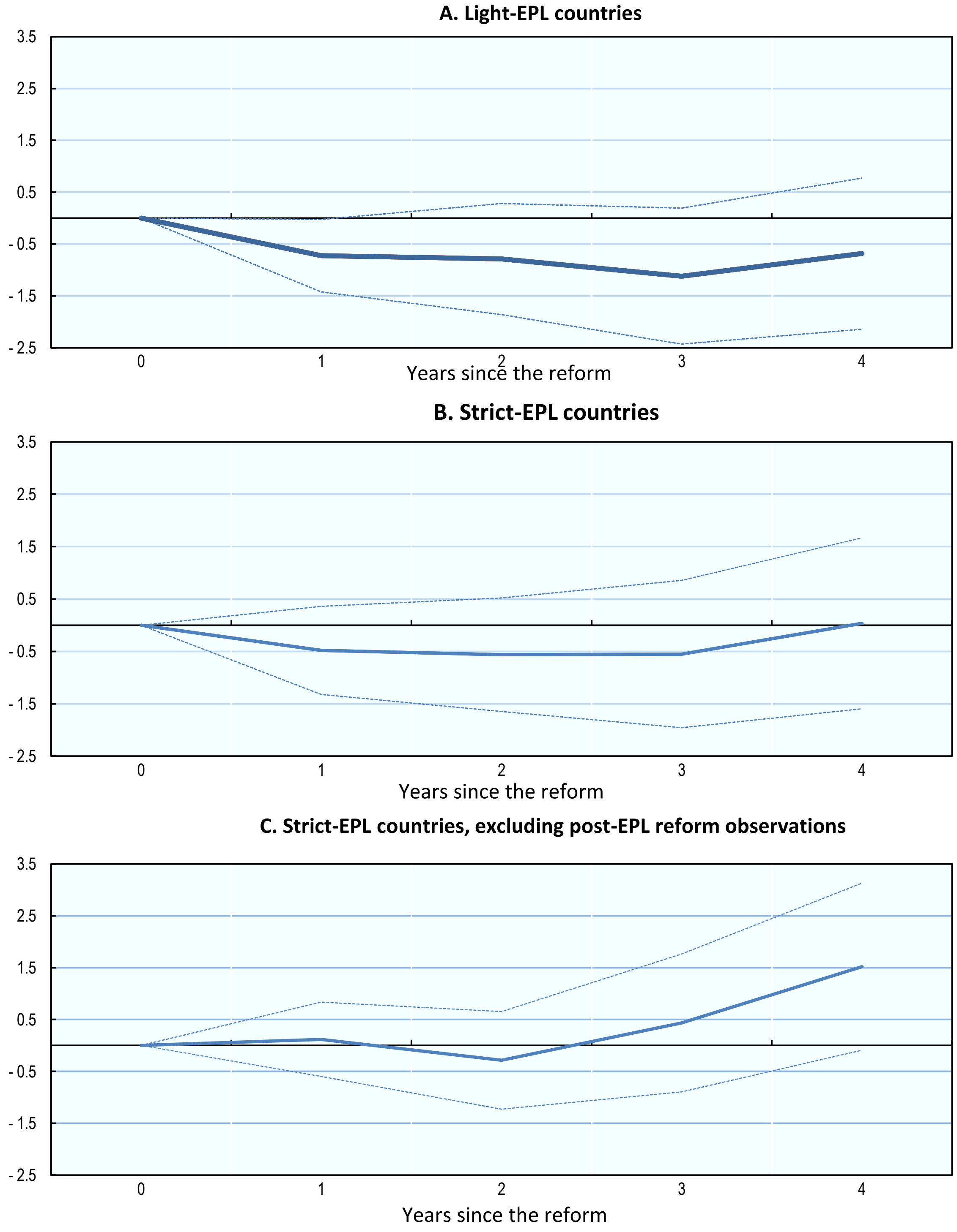
APPENDIX A1: ADDITIONAL RESULTS

A1.1 Additional Figures

Figure A1. **Competition-enhancing reforms and employment in network industries**
Separate estimates for light-EPL and strict EPL-countries

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

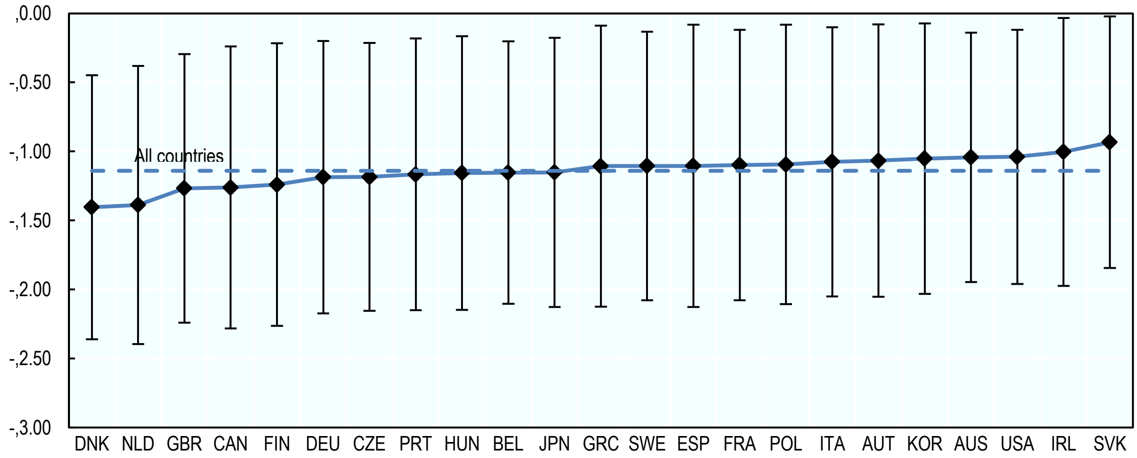


Note: See next page.

Note to Figure A1: The chart reports point estimates and 90%-confidence intervals of the cumulated effect of reforms lowering entry barriers in network industries on average industry employment. Light-EPL countries in Panel A are Australia, Belgium, Canada, Denmark, France, Hungary, Ireland, Japan, Poland, Slovakia, the United Kingdom and the United States. Strict-EPL countries in Panel B are Austria, the Czech republic, Finland, Germany, Greece, Italy, Korea, the Netherlands, Portugal, Spain and Sweden. Panel C reports estimates for Strict-EPL countries obtained by excluding Austria since 2003, Finland since 1992, Korea since 1998 and Spain since 1995. Estimates refer to the case of a reform lowering the OECD indicator of regulation in 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. 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

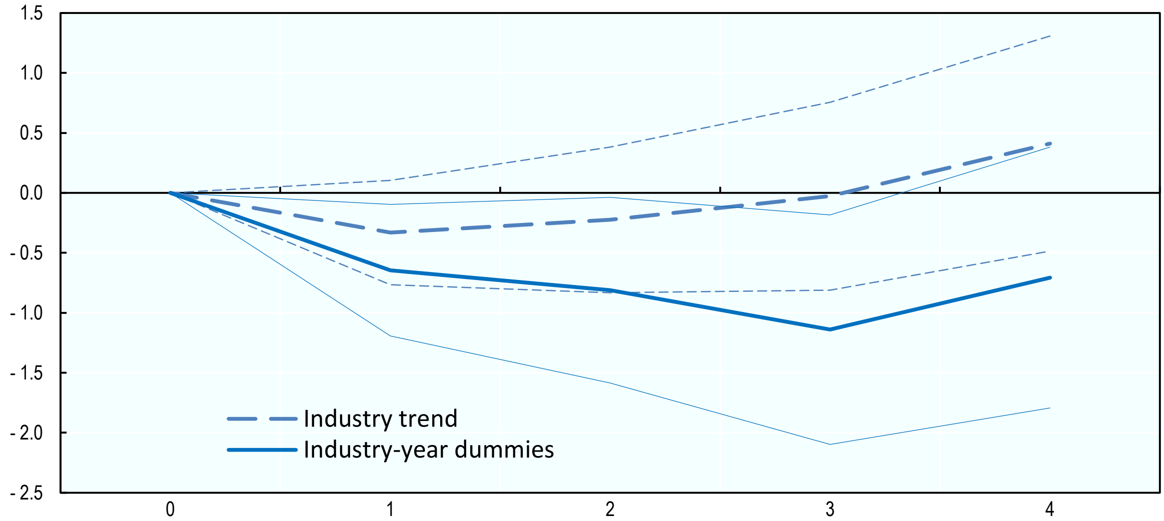
Estimated cumulated change in industry employment three years after the reform, in percentage



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. **Competition-enhancing reforms and employment in network industries**
The role of industry shocks

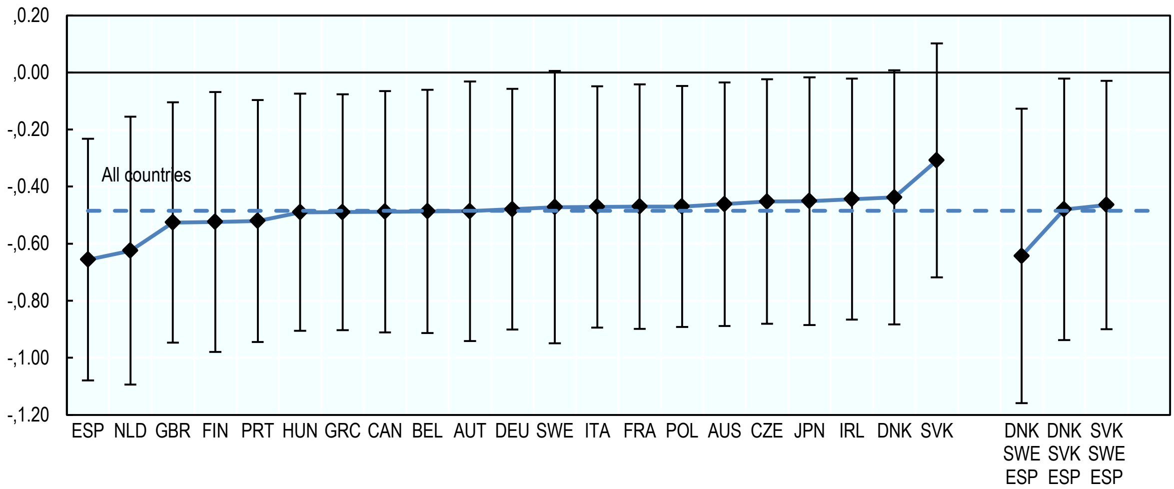
Estimated cumulated change in industry employment years after the reform, in percentage



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 employment. The baseline estimates (continuous line) are compared with those obtained replacing industry-by-year dummies with industry-specific trends (dashed line). Estimates refer to the case of a reform lowering the OECD indicator of regulation in 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 A4. **Flexibility-enhancing EPL reforms and employment: Robustness to varying the country sample**

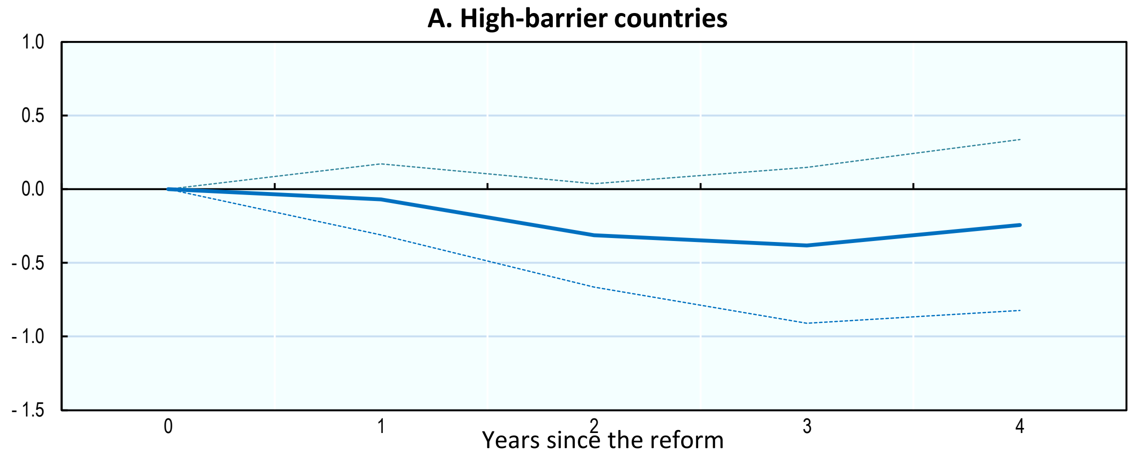
Estimated cumulative change in wage-and salary business-sector employment one year after the reform, in percentage

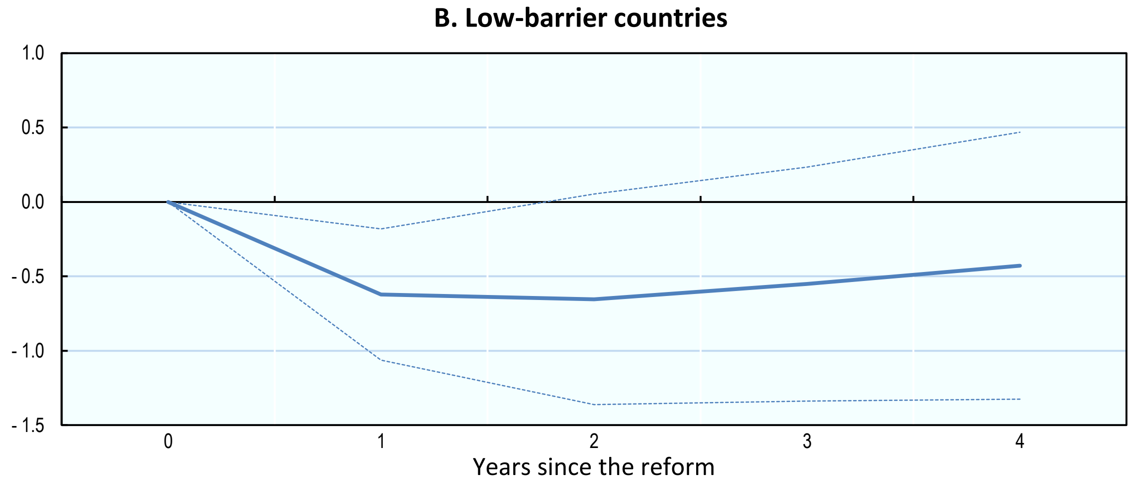


Notes: The chart reports point estimates and 90%-confidence intervals of the cumulative effect of changes in employment protection legislation (EPL) for regular contracts on wage and salary employment levels in the non-agricultural/non-mining business sector one year after the reform. Employment levels 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 A5. **Flexibility-enhancing EPL reforms and employment**
Separate estimates for light-PMR and high-PMR countries

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





Note: The chart reports point estimates and 90%-confidence intervals of the cumulated effect of changes in employment protection legislation (EPL) for regular contracts on the difference in wage and salary employment levels between 2 industries in the non-agricultural/non-mining business sector whose US dismissal rates differ by 1 percentage point. The difference in employment between the dismissal-intensive and the other industry normalised to 0 before the reform. Low-barrier countries are those with the earliest available value of average of the indicators of economy-wide administrative barriers to start-ups of corporations and sole-proprietors below the median – that is Australia, Belgium, Canada, Denmark, Finland, Ireland, the Netherlands, Sweden and the United Kingdom. High-barrier countries are Austria, the Czech republic, France, Greece, Germany, Hungary, Italy, Japan, Poland, Portugal, Slovakia and Spain. 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. Confidence intervals are obtained by clustering errors on countries and industries.

A1.2 Additional Tables

Table A1. **Granger-causality tests of reverse causality (Barriers to Entry and Employment)**

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

	(1)	(2)	(3)	(4)	(5)
Δ BE_{cit}	0.0071***			0.0073***	0.0068***
	(0.0023)			(0.0024)	(0.0024)
Δ BE_{cit-1}		0.0073***	0.0068*	0.0067*	0.0074**
		(0.0028)	(0.0035)	(0.0035)	(0.0037)
Δ BE_{cit-2}			0.0023	0.0031	0.0024
			(0.0030)	(0.0029)	(0.0032)
Δ BE_{cit-3}					0.0043
					(0.0036)
Δ log E_{cit-1}			0.0507	0.0502	
			(0.0611)	(0.0604)	
Δ log E_{cit-2}			-0.1080*	-0.1114*	
			(0.0609)	(0.0604)	
Observations	2.108	2.058	1.970	1.876	1.936
R-squared	0.246	0.247	0.625	0.627	0.621

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on a longer sample (1975-2012) obtained combining EUKLEMS and STAN data. Δ BE_{cit} measures changes in the regulation of entry (with Δ $BE_{cit} < 0$ in case of flexibility-enhancing reforms). The estimates refer to alternative specifications of model (1). Observations are weighted with the average (1975-2007) industry employment share in the country. 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. **The short-run effect of deregulation in network industries using a dummy variable**

	(1)	(2)	(3)	(4)	(5)
Δ $FEBE_{cit}$	-0.0044			-0.0050	-0.0054
	(0.0042)			(0.0044)	(0.0046)
Δ $FEBE_{cit-1}$		-0.0125**	-0.0112**	-0.0112**	-0.0132**
		(0.0053)	(0.0052)	(0.0052)	(0.0057)
Δ $FEBE_{cit-2}$			-0.0016	-0.0023	-0.0024
			(0.0050)	(0.0050)	(0.0053)
Δ $FEBE_{cit-3}$					-0.0074
					(0.0055)
Observations	1891	1833	1753	1753	1695
R-squared	0.649	0.650	0.646	0.647	0.642

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on EUKLEMS (1975-2007) data. Observations are weighted with the average (1975-2007) industry employment share in the country. ΔFEBE stands for a dummy variable equal to 1 when the change in the OECD indicator for barriers to entry is greater, in absolute terms, than 2 standard deviations (133 reforms). All specifications account for country-by-industry, country-by-time and industry-by-time dummies. Specifications corresponding to Columns 3 and 4include also two lags of the dependent variable. 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 A4. **The short-run effect of deregulation in network industries: changing industry breakdown**

	(1)	(2)
Level of disaggregation	5 industries	3 industries, same countries and years
Δ BE_{cit}	0.0040	0.0059*
	(0.0038)	(0.0033)
Δ BE_{cit-1}	-0.0004	0.0041
	(0.0030)	(0.0039)
Δ BE_{cit-2}	0.0074***	0.0056*
	(0.0028)	(0.0031)
Δ log E_{cit-1}	-0.0315	0.0332
	(0.0814)	(0.0703)
Δ log E_{cit-2}	-0.0487	-0.1133
	(0.0395)	(0.0771)
Observations	1,710	1,231
R-squared	0.580	0.699

Note: The dependent variable is the yearly growth rate of total employment in network industries computed on the subset of EUKLEMS (1975-2007) data for which a 5 industries breakdown (as opposed to 3 in the main sample) is available. The underlying network industries are Electricity, Gas, Land transport, Air transport, and Communications. The number of country-year pairs is 431, down from 587 in the baseline sample used in Table 1. In col.2 the number of industries is collapsed to the 3 of the main sample: Energy (Electricity and Gas), Transport (Air and Land) and Communication). Observations are weighted with the average (1975-2007) industry employment share in the country. 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 A5. **Quantitative EPL indicators**

	(1)	(2)	(3)
	Base sample weigthted	Base sample unweigthted	Ext. sample weigthted
SFE_t*DR	-0.0206***	-0.0180**	-0.0244***
	(0.0068)	(0.0083)	(0,0070)
SFE_{t-1}*DR	-0.0054	0.0109	-0.0037
	(0.0103)	(0.0124)	(0,0090)
SFE_{t-2}*DR	-0.0036	0.0060	-0.0041
	(0.0103)	(0.0082)	(0,0100)
SFE_{t-3}*DR	0.0050	0.0047	0.0062
	(0.0077)	(0.0070)	(0,0111)
Observations	7172	7172	8629
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. Observations are weighted by the average industry share in the country's non-agricultural/non-mining business sector employment, 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 A6. **Other industry interactions**

Industry interaction is:	US based (1)	US based (2)	UK based (3)	UK based (4)	Fitted (5)	Fitted (6)
FE_t*DR	-0.0029*	-0.0030*	-0.0036**	-0.0039**	-0.0049*	-0.0053*
	(0.0016)	(0.0016)	(0.0018)	(0.0018)	(0.0027)	(0.0028)
FE $_{t-1}$*DR	-0.0009	-0.0009	0.0010	0.0000	0.0015	0.0017
	(0.0016)	(0.0013)	(0.0019)	(0.0016)	(0.0028)	(0.0024)
FE $_{t-2}$*DR	0.0009	0.0008	-0.0003	-0.0004	0.0030	0.0030
	(0.0015)	(0.0014)	(0.0013)	(0.0013)	(0.0024)	(0.0022)
FE $_{t-3}$*DR	0.0001		-0.0030		-0.0004	
	(0.0014)		(0.0018)		(0.0024)	
Observations	7590	8052	7590	8052	7590	8052
R-squared	0.532	0.526	0.532	0.526	0.532	0.526

Note: The dependent variable is the yearly growth rate of wage and salary employment. FE: dummy variable for flexibility-enhancing reforms of EPL for regular contracts; DR: industry-level dismissal rate (in %) measured as indicated in the column title. OTHER_REF: change in the policy/institutions indicated. All specifications control for a dummy for protection-raising EPL reforms and for changes in the output gap (both interacted with DR and lagged as FE reforms), lags of changes in log employment, and as well as country by time, industry by time and country-by-industry dummies. Industry level dismissal rates are sourced from OECD (2009). In col. 1 and 2, they are measured on US data (CPS) and in col. 3 and 4 on UK data (Quarterly Labour Force). The industry indicators in col. 5 and 6 are measured by the coefficients on industry dummies estimated in a cross-country industry regression of dismissal rates also including country dummies. Industry level dismissal rates are available for Australia (1995-2001); France (2006-07) Germany (2003-07) the

United Kingdom (1997-2005) and the United States (1996-2006, even years only). Observations are weighted by the average industry share in the country's nonagricultural/non-mining business sector. The base sample is the EUKLEMS sample (1985 2007). Standard error 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.

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, 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 on the output gap, which is a key control variable in that analysis).

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). Aggregation of sub-industries (e.g. 3-digit industries) is done by simple averages of sub-industry indicators. 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.

Four aggregate political variables as instruments, all derived from the 2014 edition of the Comparative Political Dataset (Armingeon et al., 2014): extent of right-wing government support, defined as the parliamentary seat share of right-wing (and centre) parties in government (weighted by the number of days in office in a given year); a dummy for change in the ideological composition of the government in that year, where the latter is measured through the Schmidt-index of cabinet composition; and two dummies denoting start and end of a technocratic government. The Schmidt index takes five values: 1 in the case of hegemony of right-wing (and centre) parties, 2 for dominance of right-wing (and centre) parties, 3 in the case of balance of power between left and right, 4 for dominance of social-democratic and other left parties, and 5 in the case of hegemony of social-democratic and other left parties. The dummy used in this article takes value 1 every time the Schmidt index changes value and 0 otherwise.

Further data used in robustness checks are sourced from the OECD Taxben , Taxing wages and EPL databases (Unemployment benefit average net 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. When these variable are not available on a yearly basis, missing values have been interpolated linearly.

Micro-data used in Appendix A4 are from the Estonian Labour Force Survey, while industrial production and retail turnover indexes are from Eurostat. In the RDD exercises for the Estonia, Slovenia and Spain countries, 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 youth and older workers in the labour force are from Labour Force Surveys of each country.

Table A7: 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: USING COUNTRY INDUSTRY DATA TO ESTIMATE THE LABOR MARKET IMPACT OF COUNTRY-WIDE POLICIES

Employment protection legislation only varies across countries and over time. Using aggregate cross-country/time-series data this variation can be used to examine general equilibrium effects. Yet, in such aggregate analysis it is difficult to control for an exhaustive list of confounding factors, as country-level unobserved characteristics. We circumvent this problem by exploiting the availability of cross-country comparable time-series data on industry employment, and a differences-in-differences specification in the spirit of Rajan and Zingales (1998). The basic premise of the analysis is that EPL is more likely to be binding in some industries than others. Consider the partial equilibrium employment response to a change in EPL (that is, on the amount of firing costs the employer expects to pay in the event of future layoffs). If it exists, such response will plausibly be greater in industries where, in the absence of regulations, firms rely more intensively on layoffs to make staffing changes relative to those industries where internal labour markets or voluntary turnover are more important. By comparing differences in employment responses across industries within a country implementing an EPL reform we can draw substantial insights on the short run labor market effects of EPL. This specification, in fact, allows accounting for any other country-specific shock which might act as a confounding factor in an aggregate analysis (i.e. a simultaneous reform or macroeconomic shock, provided that their effect is approximately the same in EPL-binding and other industries) and, in practice, use the low-dismissal industries as a control group for industries in which EPL is binding.

Following the above mentioned literature, industry-level indexes of layoff intensity are computed from US data. This common practice is generally motivated by the fact that the United States is a low regulation (i.e. "frictionless") country, so that using US data mitigates concerns regarding the possible endogeneity of exposure to the level of regulation (the United States is excluded from the analysis). Moreover, layoff rates are not available for a wide range of other countries. As a robustness check we also use UK layoff rates and a "world average" measure of industry exposure to EPL obtained by estimating the following a cross country-industry regression: $\mathrm{Layoff}_{j,c} = \eta_j + \eta_c + \epsilon_{jc}$, where layoff rates ($\mathrm{Layoff}_{j,c}$, available for Australia, France, Germany, the United Kingdom and the United States) are projected on a country dummy (η_j) and an industry dummy (η_c). The latter therefore captures average industry-specific layoff propensity, net of country characteristics (as the level of employment regulation).

APPENDIX A4. THE SHORT TERM COSTS OF EPL REFORMS: COUNTRY CASE STUDIES

In this appendix we present further evidence on the short term labour market consequences of flexibility enhancing EPL reforms examining recent episodes across OECD countries. We aim to test in particular whether one-shot reductions in EPL for regular contracts have negative *aggregate* costs in terms of higher unemployment. We focus on the cases of three recently implemented reductions in EPL in Estonia (July 2009), Spain (February 2012) and Slovenia (April 2013). They implied appreciable changes in the legislation of regular contracts (as measured by changes in the corresponding OECD indicator for individual dismissals). More precisely: the Estonian reform, implying a reduction in the indicator of 0.93 points, was the largest one since 1998; the Slovenian one was the fifth, with a fall of 0.44 points; the Spanish was at the median (0.17 points).[38] Furthermore, all these reforms were essentially implemented at a single date, and by and large without concomitant labour market policies, which allows them to be studied with relatively standard impact evaluation methods using high-frequency data.[39]

Diff-in-diff evaluation of the Estonian reform. We start our analysis with the case of the 2009 Employment Contracts Act which implied a sizable relaxation of employment protection regulations in Estonia.[40] Similar and neighbour Baltic countries did not implement any such reform, which naturally lends itself to using a difference-in-difference methodology to assess the reform consequences. Following Malk (2014), our baseline analysis exploits the labour market patterns of, in particular, Lithuania as a control.[41]

A simple comparison of the time series of the unemployment rates in the Baltic States, drawn from Eurostat's EU Labour Force Survey data, suggests that unemployment did rise faster in Estonia in the first year after the July 2009 reform. The differential relative to Lithuania went from being approximately 0 before the reform to 1.7 percentage-point in the first quarter of 2010 (see Figure A6). After that peak, the Estonian unemployment rate decreased more quickly than in both Lithuania (and Latvia).

[38] As a matter of comparison, the median flexibility-enhancing reform was as large as a decrease of 0.17 points both if considering reforms since 1998 or since 1985 (first available date in the OECD database). Before the reforms the levels of the indicator, which ranges between 0 and 6, were 2.74, 2.60 and 2.36 in Estonia, Slovenia and Spain, respectively.

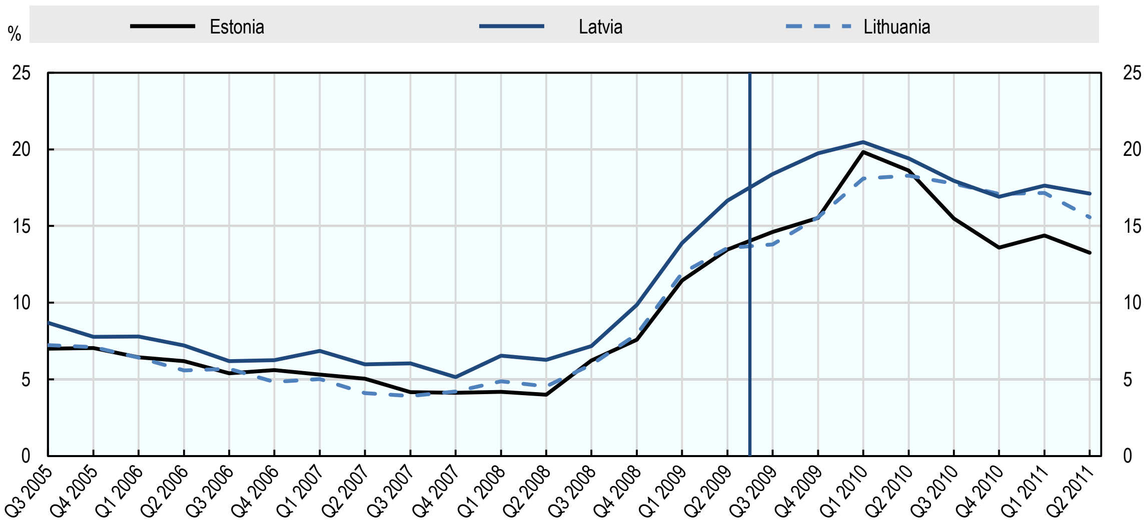
[39] In the case of Spain, the EPL reform was coupled with a simultaneous decentralisation of collective bargaining. The regression-discontinuity approach adopted here estimates, therefore, the joint effect of both reforms. By contrast, there were no major concomitant reforms in the other two countries.

[40] The Estonian Employment Contracts Act that came into force on July 1st, 2009 shortened notice periods and made them more dependent on job tenure, significantly reduced severance pay, with some additional compensation being provided by the Estonian Unemployment Insurance Fund (but with no upfront cost for employers at the time of dismissal). Moreover it made reinstatement in the case of unfair dismissal conditional on the mutual agreement of the parties while compensation for unfair dismissal was reduced to a maximum of three months wages, except in exceptional circumstances.

[41] Using Lithuania as a control for the reform implemented in Estonia can be justified on several grounds. Both countries are small open economies with the same trading partners, have similar demographic structure of the labour force and display a similar evolution of real GDP, industrial production and retail turnover before and after July 2009. Before the reform, they were also characterised by very similar trends in unemployment as well as stocks and flows of temporary contracts. Finally, no significant changes in labour market policies and institutions occurred in Lithuania over the period considered. The case of Latvia as a comparison group is weaker since unemployment was higher in that country before the Estonian reform, and the difference between the two countries was on the rise (see Figure A6).

Figure A6. **Evolution of the unemployment rate in the Baltic countries**

Q3 2004 – Q2 2011, in percentage of the labour force



Notes: The vertical line indicates the date of enforcement of the Estonian EPL reform. Source: EU LFS.

However composition effects and confounding factors might be at play when comparing Estonia and Lithuania. In particular, despite the two countries had similar evolution of business-cycle indicators before and after July 2009, the Estonian industrial production index fell much more than that of Lithuania in the neighbourhood of the reform (see Figure A7). Moreover, the Estonian labour market is more open to immigrants (with 14% of employment being foreign born in 2009 against only 4% in Lithuania), which are often at higher risk of unemployment in recessions. Not controlling for these factors could overstate the adverse effect of the Estonian reform. At the same time the fall in retail turnover was milder in Estonia than in Lithuania, which could act in the opposite direction. For these reasons we go beyond the simple Figure A7 by estimating a probit model in which the individual probability of being unemployed in a given month is allowed to diverge in the aftermath of the reform:

$$Prob(U_{ict} = 1) = F(\alpha + X_{cit}\beta_1 + Y_{ct}\beta_2 + \gamma I_{t>R} + \delta D_{EST} + \theta D_{EST} I_{t>R} + \nu_t) + \theta D_{EST} I_{t>R} + \nu_t) \tag{A1}$$

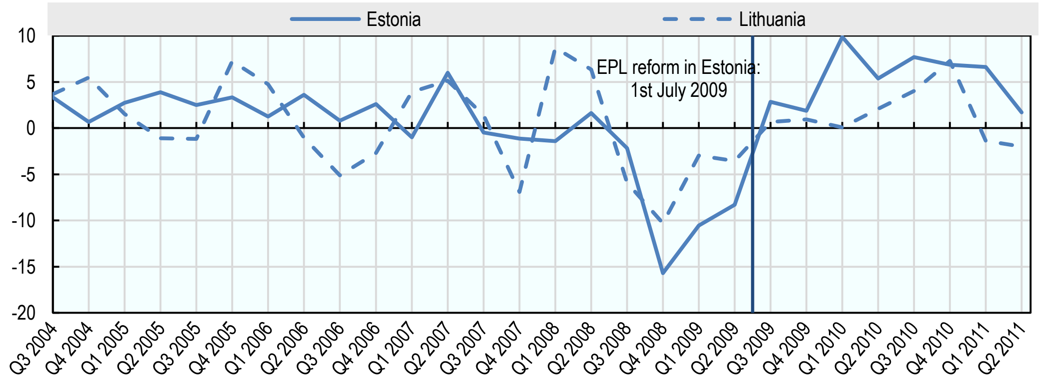
where $I_{t>R}$ is an indicator denoting the post July 2009 period, D_{EST} is a country dummy for Estonia and v are dummies for the calendar month of the reference week. In specification (A1), δ measures the impact of the reform on Estonian unemployment – a significant estimate for this parameter suggests a significant impact of the reform. Vectors X and Y contain, respectively, a large set of individual controls accounting for compositional effects and aggregate covariates.[42] The sample window is restricted to individuals in the labour force and spans the two years before and after the date of enforcement of the Estonian reform.

[42] Individual controls include 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, and 23 classes for the duration of residence in the country if foreign born. Aggregate controls are: the 3-month-lagged changes in industrial production and retail turnover indexes, month dummies, and, in a sensitivity analysis, a 5th order polynomial time trend.

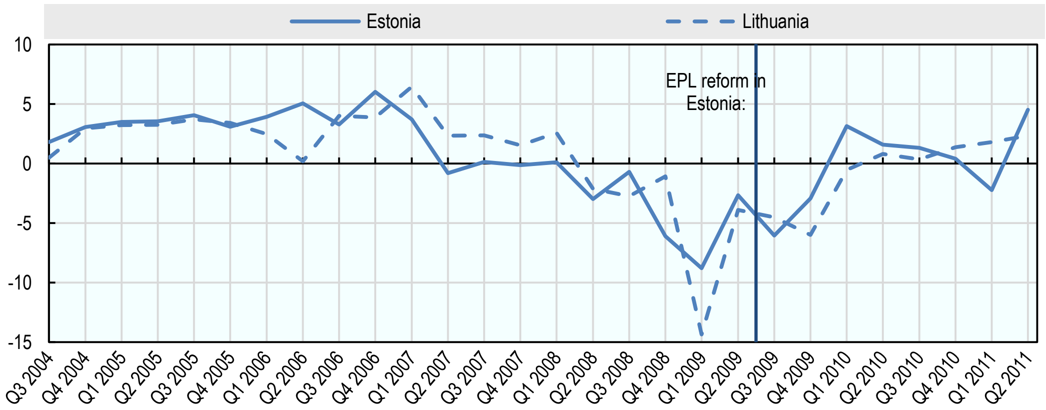
Figure A7. **Evolution of the economic activity in Estonia and Lithuania, Q3 2004-Q2-2011**

Quarterly percentage changes, seasonally-adjusted

A. Growth of the industrial production index



B: Growth of the index of retail turnover (deflated)



Notes: The vertical line indicates the date of enforcement of the Estonian EPL reform. Source: Eurostat.

Malk (2014) adopt a similar approach to study the impact of the reform on the probability of hiring and separations at quarterly frequency. However, both types of flows are studied separately in her paper so that the results do not allow inferring the net effect on either employment or unemployment, thereby assessing the short-term costs of the reform. In addition, we use the interview date as reported in Quarterly LFS data to build a monthly dataset, which allows us to more precisely identify the starting point of the reform on the time line, and to meaningfully include polynomial trends as additional controls for business-cycle fluctuations (in a sensitivity exercise).

Baseline results from estimating (A1) show that unemployment probability in Estonia increased by 1.5-percentage-points following the reform, an increase of more than 10% relative to the average unemployment rate in the previous 12 months. These findings are robust to extending the control group including Latvia, changing the specification of aggregate controls or controlling for polynomial time trends[43] (see Table A8, Panel A, cols. 2 to 4. See also the corresponding note for more details). Moreover, two placebo experiments in which the date of the reform is fictitiously anticipated by 3 and 12 months, respectively, yield insignificant estimated coefficients (cols. 5 and 6). This supports the conclusion that the discontinuity estimated in the baseline model effectively corresponds to a shift occurring in July 2009.

[43] Time trends are allowed to vary between before and after the reform and between countries after the reform.

Table A8. **Difference-in-difference estimates for the Estonian reform**

Panel A: baseline sample

	(1) reform baseline	(2) reform LTU&LVA	(3) reform curr cntrls	(4) reform time trend	(5) placebo 3 months	(6) placebo 1 year
Reform dummy	1.49**	2.33***	1.83***	3.35***	1.11	-0.10
	(0.70)	(0.67)	(0.55)	(1.20)	(0.33)	(0.04)
Observations	166,250	241,267	166,250	166,250	166,250	166,250

Panel B: alternative samples

	(1) reform no manipulation	(2) reform 3-year window	(3) reform backshifted 3-year window	(4) reform frontshifted 3-year window
Reform dummy	1.51*	2.47***	1.43*	2.41***
	(0.71)	(0.71)	(0.67)	(0.70)
Observations	156,040	124,095	123,252	145,863

Notes: Marginal percentage effects on the probability of being unemployed, obtained by estimating a probit model with observations weighted by cross sectional weights. The sample window covers 24 months before and after the Estonian reform, except where elsewhere specified. 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. Reform baseline: Baseline (using only Lithuania as comparison group). Reform LTU&LVA: Using Lithuania and Latvia in the comparison group. Reform curr cntrls: Aggregate variables are contemporaneous instead of lagged 3 months. Reform time trend: 5th order polynomial in time (months) included. Placebo 3 months: Fictitious reform 3 months before the true reform. Placebo 1 year: Fictitious reform 12 months before the true reform. Reform no manipulation: Exclusion from the sample of three months around the reform date. Reform 3-year window: Sample window reduced to 18 months before and after the reform. Reform backshifted 3-year window: Sample window reduced to 24 months before and 12 months after the reform. Reform frontshifted 3-year window: Sample window reduced to 12 months before and 24 months after the reform. Robust standard errors, obtained by adjusting for clustering on countries and months in parentheses. ***, **, * statistically significant at 1%; 5% and 10% levels respectively.

Our results appear also robust to restricting the estimation sample (Table A8, Panel B). First, we consider removing a three-month window around the reform date to allow for possible postponement of dismissals around the date of reform implementation (col. 1). The point estimate turns out even larger, although not significantly so, which provide evidence of no manipulation. We also consider a shorter window around the reform date (18 months before and after, col. 2). If the estimated effect is not spurious, by taking a smaller bandwith around the implementation date one would expect to find a greater estimate, although possibly less significant because of the smaller sample size. This is what we find. We also experimented with advancing or delaying the shorter sample window with similar results (cols. 3 and 4).

RDD evaluation of reforms in Estonia, Slovenia and Spain. The availability of monthly data on aggregate unemployment (as well as on the demographic structure of the labour force) for Slovenia and Spain allows for a similar assessment of the short-term impact of the (large) EPL reforms implemented there in 2013 and 2012, respectively.[44] Because no obvious comparison group is available for these two countries, however, in their case we need to rely on a simpler time-regression-discontinuity model. More specifically, the short term impact is estimated through discontinuities in the (seasonally-adjusted) standardised unemployment rate. For comparison, the same exercise is also presented for Estonia.

The general regression-discontinuity model, estimated on monthly data, is written as:

$$u_t = Y_t\beta + \delta I_{t>R} + \sum_{s=1}^{5}\lambda_s (t-R)^s + \sum_{s=1}^{5}\mu_s I_{t>R}(t-R)^s + D_t + e_t \tag{A2}$$

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 baseline regression-discontinuity experiments covers five years before the reform and two years after (which is up to the latest available data in the case of Slovenia).

The parameter of interest is δ, which captures the average increase in unemployment following the reform. The key identification assumption is that, conditional on all the control variables in (A2), labour market performance evolves in a smooth way. To isolate the effect of the reform from that of the business cycle, all the specifications 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.

The main results are reported in Table A9. The regression discontinuity approach confirms that the Estonian reform had a sizable negative impact on unemployment, which increased by 1.9 percentage points after its implementation (Panel A). The loss is very close to that obtained in the previous diff-in-diff exercise (see Table A8). Average unemployment increased also in Slovenia, albeit by a smaller extent (0.5 points, see Panel B). By contrast, no significant effect is detected in Spain (Panel C). The remainder of the table makes sure that these findings are unaffected by a series of specification and robustness checks. Results in column 2 to 4 show that they hold when changing the dependent variable (using unadjusted unemployment rates), including demographic controls, or replacing lagged cyclical variables with current ones. The employment losses in Estonia and Slovenia are unaffected (if anything larger in magnitude) even if altering the observational window by (i) excluding a three months window centred on the reform date (suggesting no manipulation around the reform date, col. 5) and (ii) restricting the time window to 3 years around the reform. Finally, column 7 proposes placebo tests run by fictitiously anticipating the date of the reform by three months in each case, which implied no significant effect in unemployment, which suggests that the shift we detect in the baseline model effectively occurred at the reform date.

[44] A new Employment Relations Act entered into force in *Slovenia* on April 12th, 2013. The proposed reform reduced notice periods, making them more dependent on service duration. A few amendments were also made to severance pay. Moreover, the reform suppressed the requirement that employers provide proof of having attempted redeployment within the company before making redundancies. In addition, opposition by trade unions can no longer delay the date of dismissal. In *Spain*, the labour market reform was approved by the government in February 12th, 2012. The reform redefined the conditions for a fair dismissal, specifying that a redundancy is always justified if the company faces a persistent decline in revenues or ordinary income. Moreover, in all cases, the employer has no more the obligation of proving that the dismissal is essential for the future profitability of the firm. Monetary compensation for unfair dismissal was reduced by more than 25% and a much lower ceiling was introduced. At the same time, the reform removed a worker's right to interim wages between the effective date of dismissal and the final court ruling. Finally, the reform eliminated the requirement that employers obtain administrative authorisation for collective redundancies.

Table A9. **Time-regression-discontinuity estimates**

Panel A: Estonia

	(1) Baseline	(2) Unempl. Nsa	(3) Demogr. controls	(4) Cycle controls	(5) No manipul.	(6) 3-year window	(7) placebo 3 months
Reform dummy	1.92**	2.57**	1.69***	1.85***	1.97*	3.48***	1.22
	(0,58)	(1,11)	(0,60)	(0,67)	(1,12)	(0,82)	(1,03)
Observations	84	84	84	84	81	36	84
R-squared	0,995	0.991	0.996	0.995	0.996	0,999	0.994

Panel B: Slovenia

	(1) Baseline	(2) Unempl. Nsa	(3) Demogr. controls	(4) Cycle controls	(5) No manipul.	(6) 3-year window	(7) placebo 3 months
Reform dummy	0.55*	1.11***	0.50*	0.62*	1.33*	1.18*	-0.04
	(0,29)	(0,39)	(0,28)	(0,33)	(0,68)	(0,61)	(0,31)
Observations	84	84	84	84	81	36	84
R-squared	0.990	0.987	0.991	0.989	0.990	0,989	0.989

Panel C: Spain

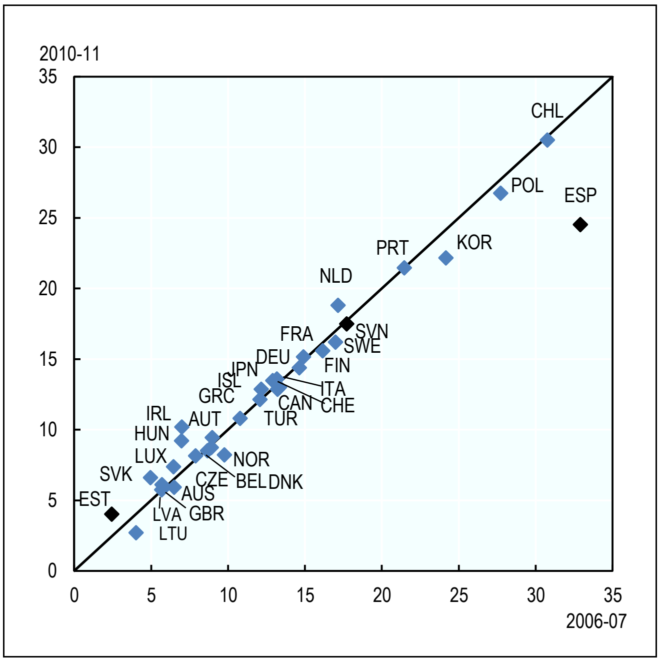
	(1) Baseline	(2) Unempl. Nsa	(3) Demogr. controls	(4) Cycle controls	(5) No manipul.	(6) 3-year window	(7) placebo 3 months
Reform dummy	0.08	0,34	0,26	-0,29	-0,65	-0.65**	-0,42
	(0,62)	(0,69)	(0,60)	(0,55)	(1,03)	(0,32)	(0,43)
Observations	84	84	84	84	81	36	84
R-squared	0.997	0,996	0,997	0,998	0,997	0,999	0,997

Notes: Dependent variable is monthly unemployment rate, in percentage of the labour force and seasonally-adjusted (except in col. 2). The Reform dummy is an indicator for the (two year) period following the reform. Each model includes 5-th order polynomial trends (allowed to vary between before and after the reform), month dummies and 3-month-lagged industrial production and retail turnover indexes. Results in column 2. In column 2, monthly unemployment is not adjusted for seasonality. Column 3 adds the share of youth and of women in labour force, and col. 4 adds contemporaneous indexes of retail turnover and industrial production. In col. 5 three months of observations around the reform date are excluded from the sample. In col. 6 the sample restricted to 18 months before and after the reform. Finally, in col 7 the Reform dummy indicates a period starting 3 months before the true reform. Robust standard errors are in brackets. ***, **, * statistically significant at 1%; 5% and 10% levels respectively.

There are several ways to rationalize the differences in the estimated impact of the three reforms. One is the intensity of the implemented policy change. While the three reforms implied significant changes in the OECD regulation index, those of Estonia and Slovenia feature among the largest in a hypothetical index-based ranking of EPL reforms since 1998, while the Spanish reform stands at the median. A second reason is the different degree of labor market segmentation which, as shown in Figure A8, is highest in Spain, median in Slovenia and almost zero in Estonia. The costs of flexibility enhancing reforms affecting regulations for regular contracts can be expected to be limited in dual labour markets because jobs that are most likely to become unprofitable are have likely been filled with a temporary contract. In fact, in the paper we do not find evidence of that reforms implemented in dual labor markets entail significant short term costs. A third reason behind the difference in the results is the different phase of the business cycle at implementation: the onset of a large downturn in Estonia, and just before the crisis trough in Slovenia and Spain. Both basic models with adjustment costs and our evidence in the paper suggest the employment losses from EPL reforms should be larger in downturns than upturns. Finally, the impact of the 2012 Spanish reform on unemployment may have been attenuated by the concomitant reform of collective bargaining (see above), which favoured decentralization and was found to have had a positive effect on unemployment even in the short term (see e.g. OECD, 2014; Garcia-Perez and Mestres-Domenech, 2017).

Figure A8. **Incidence of fixed-term contracts in total wage and salary employment**

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 are indicated by black diamonds.