

Chapter 3

Short-term labour market effects of structural reforms: Pain before the gain?

There is broad consensus that well-designed structural reforms of product and labour markets can have positive effects in the long run. And yet, structural reforms often involve significant reallocation of resources which might entail costly adjustments, especially in the labour market. This chapter exploits long time series of industry-level data in a group of OECD countries to analyse the short-term labour market effects of reforms lowering barriers to entry and the cost of dismissal. It finds that both policies induce non-negligible transitory employment losses on average, 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 labour market structure, and on cyclical conditions. The chapter also discusses policy options that could help attenuate these costs, and whose applicability and aptness may vary across countries.

The statistical data for Israel are supplied by and under the responsibility of the relevant Israeli authorities. The use of such data by the OECD is without prejudice to the status of the Golan Heights, East Jerusalem and Israeli settlements in the West Bank under the terms of international law.

Key findings

In the context of sluggish global labour market conditions and government debts limiting the fiscal policy space, structural reforms feature prominently in the political agenda to boost and sustain growth. A large body of research confirms that reforms of product and labour markets can significantly improve productivity growth and employment in the long run. And yet, structural reforms often involve significant reallocation of resources across firms and sectors that may entail costly adjustments, especially in the labour market. In particular, these reforms may further aggravate households' economic conditions in countries with persistent economic and employment slack.

The chapter exploits long time series of industry-level and aggregate data to provide evidence on the short-term labour market effects of structural reforms. The analysis looks in particular at two types of structural reforms: reductions in barriers to entry in product markets and increased flexibility in regulation governing the dismissal of workers on regular (open-ended) contracts. The chapter also discusses several complementary labour-market policy actions which could be coupled with structural reforms to help attenuate any negative short-term consequences.

The key findings can be summarised as follows:

- Reforms increasing the level of competition in network industries (energy, transportation and communication industries) that are characterised by the presence of large incumbents induce non-negligible short-term losses, with employment in the industry falling below the pre-reform level during the first three to four years. The employment loss is more pronounced when reforms are implemented during an economic downturn. In the long run, neither employment nor average wages are affected by these reforms. However, the analysis also shows that reforming network markets has beneficial long-term consequences for the employment performance of downstream industries (that is those that use network markets' products and services as inputs). These findings differ from those obtained in several earlier studies for the case of retail trade, where regulation typically shelters a large number of relatively small firms against competition from large (and efficient) distributors and reforms have been found to entail no short-term employment losses. Taken together, these results suggest that the characteristics of the reformed market affect the dynamics of employment in the aftermath of pro-competitive reforms.
- Reforms of employment protection legislation (EPL) that reduce dismissal costs are also associated with short-term employment and wage losses, but these are reversed within a few years on average. Moreover, these losses are not statistically significant when reforms are implemented during an economic upswing. The analysis also shows that these short-term costs are less acute in countries with significant labour market dualism, as measured by a high share of fixed-term contracts in employment. Importantly, such countries are also those that experience the greatest benefits from reforms that lower the relative use of fixed-term contracts. Evidence from country case

studies shows that these benefits tend to materialise relatively quickly. In the long run, more flexible dismissal legislation is associated with greater average wages, consistent with previous studies of the relationship between EPL and productivity.

- Labour market reform packages can be designed and implemented in ways that significantly attenuate, if not eliminate, these initial costs. The choice of complementary policies crucially depends on political economy considerations, the stage of the business cycle, the capacity to expand spending and the country-specific labour market institutional framework.
- The adverse effects of structural reforms are likely to be smaller in countries with an effective activation strategy to support jobseekers, especially when activation systems are geared around early interventions of the public employment service (PES) during the notice period preceding job displacement. Yet, if efficient programmes are not already in place, there are limits to how rapidly active labour market policies can be scaled up when unemployment rises, since fine-tuning of these institutions typically takes several years.
- In addition, recent experiences suggest that, in countries with national, regional or branch-level collective bargaining, allowing scope for individual firms to adapt wages and working conditions to their individual situation can limit any short-term job losses resulting from the relaxation of dismissal regulations. More flexibility in working conditions and wage setting allows firms to make use of variables other than employment when adjusting to the required restructuring.
- Alternatively, more flexible dismissal legislation could be introduced and applied only to new hires. There is evidence that such “grandfather clauses” more than offset short-term employment costs of EPL reforms. However, they also clearly delay the desired effects of EPL reform on reallocation and allocative efficiency.
- Finally, countries characterised by relatively low unemployment-benefit entitlements (or tight eligibility rules) and relatively strong fiscal positions, could consider cushioning the short-term effect of structural reforms undertaken in downturns, for example by temporarily extending benefit durations. Recent evidence suggests that, in recessions, such measures have, at worst, no adverse welfare effects.

Introduction

The 2006 reassessment of the OECD Jobs Strategy outlined a number of structural reforms aimed at fostering countries’ adaptability to structural changes and increasing employment and productivity in the long run (OECD, 2006). Reaping the full benefits of such reforms takes time, however, as they often materialise gradually through hiring, firm entry and innovation. In the short run the significant amount of resource reallocation engendered by such policy changes is likely to induce costly labour market adjustments, notably job and income losses (although these may only be transitory). Assessing the relative strength of these opposing effects has clear policy relevance. Nonetheless, the substantial amount of work which has been produced quantifying the long-run benefits of structural reforms contrasts with the much more limited evidence on their short-run consequences. If certain structural reforms entail short-run costs, a second important policy question that arises is whether they should be implemented during a recession, when their urgency often becomes more evident and political opposition is weaker, or, rather, timed to accompany an economic upswing when job creation is stronger and short-term costs potentially lower and/or of shorter duration. Finally, policy makers are

interested in knowing whether and to what extent complementary policy actions can be coupled with structural reforms in order to help offset their negative short-term consequences and strengthen political support.

The chapter provides new empirical evidence on these issues by using mostly aggregate and industry-level data. The analysis largely adopts gross or net job losses (gains) as the metric to quantify costs (benefits) and looks, in particular, at reforms that lower barriers to entry in product markets and make regulations on the dismissal of regular workers more flexible. While the menu of structural policies analysed could potentially be broader, the chapter focusses on these two types of reforms for two reasons. First, the idea that lowering entry barriers and dismissals costs might be accompanied by short-run employment losses is supported by theory. Second – and unlike, for example, trade policies – there are significant margins for further liberalisation in the regulation of both market entry, especially in network industries, and dismissals in many OECD countries. The statistical analysis quantifies the effect of each type of reform at different time horizons, and explores whether the short-term effect varies with the stage of the business cycle and the characteristics of the labour market. This approach provides the basis for a broad assessment of the extent and timing of the labour market consequences of future policy action in these areas.

The analysis of policies that might facilitate the reallocation of resources spurred by structural reforms begins with a brief discussion of the existing evidence on the potential effectiveness of macroeconomic policies, as well as the constraints on their use. The microeconomic evidence is then considered in greater detail, including both recent reform packages aimed at smoothing the transition to the new institutional setting and the potential for active and passive labour market policies to attenuate the negative side effects of structural reforms on jobs and earnings.

The chapter is structured as follows. Section 1 focuses on the short- and long-run effects of lowering entry barriers, looking particularly at the liberalisation of network service industries and distinguishing between direct impacts and indirect effects on service users. Section 2 looks at the consequences of an easing of regulations on dismissals, their interaction with the cycle and with the degree of labour market segmentation, relying on both cross-country/cross-industry regression analysis and case studies of recent reform experiences. Finally, Section 3 discusses the political economy of structural reforms and the potential role of complementary labour market policies in attenuating short-run reform costs. This section assesses the relative attractiveness of alternative reform packages and the implications in terms of reform design.

1. Product market regulation

Product market reforms that lower barriers to entry aim at improving efficiency in the production of goods and services, and making the price setting process more competitive. They include measures to open up markets to domestic or foreign competition by removing, for example, permits and licences, tariffs or non-tariff barriers as well as legal and administrative barriers. Competition enhancing policies of this type have been shown to be beneficial in the long run not only in terms of lowering final prices, but also for enhancing firms' productivity and innovative efforts and improving the efficiency of resource allocation across existing and new production units (i.e. market incumbents and entrants; see Boeri et al., 2015 for a review). Indeed, over the past decade many

OECD countries have approved significant reforms of product markets, particularly in non-tradable service industries, which implied sizable reductions in regulation indicators. Yet, resource reallocation can involve significant frictions and thus result in significant adjustment costs, particularly in the labour market. For example, incumbent firms may react to increased competitive pressure through re-organisation and downsizing (in some case, exit) with the aim of reducing costs and lowering prices; their response is likely to be quick or even anticipated as it aims, in considerable part, at deterring entry. The positive employment contribution of new firms, however, takes longer to materialise as successful entrants expand only gradually. As a result, the reallocation of workers from shrinking to growing firms may end up being a lengthy and costly process involving considerable unemployment.

Several previous studies have shown that pro-competitive product markets regulatory reforms generally have had a positive effect on total employment in the long run (e.g. Peoples, 1998; Alesina et al., 2005; Griffith, Harrison and Macartney, 2007; and Fiori et al., 2012) and involve a significant reallocation of jobs from less to more productive firms (Andrews and Cingano, 2014). Empirical analyses focussing on employment dynamics in the aftermath of reforms in the retail sector show that such reallocation has no negative impact on employment even in the short run (Bertrand and Kramarz, 2002; Viviano, 2008; Skuterud, 2005; Burda and Weil, 2005; and Boeri, Cahuc and Zylberberg, 2015, for a survey). This finding, however, likely reflects the particular competitive situation in the retail industry, where deregulation often implies the entry of large, efficient competitors, whereas incumbents are too small to strategically anticipate entry by cutting staffing. As suggested by Bassanini (2015), employment dynamics are likely to be very different in more concentrated markets or cases in which regulation rather shelters large dominant players. Recent studies have shown that, in such cases, the incumbent response often consists of reducing prices and increasing efficiency, even before new competitors enter the market (e.g. Goolsbee and Syverson, 2008; Bridgman, Gomes and Teixeira, 2011; and Brueckner, Lee and Singer, 2013). Hence, the initial impact of pro-competitive reforms on industry-level employment could be negative because large incumbents re-organise and downsize well before entrants start hiring.

This section extends the research literature by focusing on the reduction of entry barriers in three network industries: energy (electricity and gas), transport (air, rail, road transport) and communications (post and telecommunications). Network industries provide an interesting case study of the labour market consequences of pro-competitive reforms for a number of reasons. First, large incumbent firms usually play an important role in these industries. In contrast to retail trade, deregulation is likely to impact on the employment decisions of players that can significantly affect the overall labour market, at least in the short run. Hence, the results of the analysis are likely to be relevant for, and extend to, the case of pro-competitive reforms implemented in industries or markets characterised by the presence of large dominant players. Second, the services produced by these industries serve as key inputs to most other branches, inducing strong forward linkages to the rest of the economy. Hence, the benefits from achieving greater efficiency are likely to extend beyond these markets. Another advantage of focussing on network industries is that the markets for energy, transportation and communication continue to offer substantial room for further deregulation in many OECD countries, despite the recent waves of reforms, and nearly all emerging economies (OECD, 2014a). Despite their accounting for a relatively small share of total employment, reforming network industries

has the potential to generate non-negligible labour market adjustments, both directly and indirectly. Finally, the focus on network industries takes advantage of the availability of long time series of detailed OECD indicators of the level of anti-competitive barriers for a large cross-section of countries.

The chapter's focus on network industries does limit the scope of the analysis of product market reform, since barriers to competition certainly are not confined to these industries, nor are they limited to the specific regulatory impediments that are quantified by the OECD indicators. Indeed, economy-wide administrative barriers to entry remain high in some OECD countries (notably, Mexico and Turkey) and in most emerging economies. The regulation of firm exit has long been recognised as an important determinant of the entrepreneurship and entry rates in any branch of economic activity (Brandt, 2005), yet is inefficiently designed in several OECD countries.¹ It is also the case that cross-border trade and investment are still limited in a number of key economic activities, ranging from business/professional services to the construction sector, due to regulations such as the requirement that foreign firms work through local partners.² 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.³ Finally, poor judicial enforcement of property rights or of competition laws can continue to blunt the incentive to invest in a market even after reforms have formally eliminated entry barriers.

The analysis presented in this section is divided into two main subsections. The first looks at the (short- and long-term) consequences of reforms lowering barriers to entry in network industries on own industry employment and wages (the *direct* effect of reforms). The second assesses the impact of these reforms on the labour market performance of all other industries in the business-sector (the *indirect* effect).

The direct labour market consequences of competition-enhancing reforms

The empirical analysis in this subsection will quantify the labour market consequences of product market reforms implemented over the past three decades in three network industries (energy, transport and communication) exploiting industry level data for 23 OECD countries (and up to 37 years) (see Box 3.1).⁴ Pro-competitive reforms of product market regulation (PMR) are quantified on the basis of changes in the OECD indicator of barriers to entry (sourced from the *OECD Product Market Regulation Database*), with reductions indicating competition-enhancing reforms and increases indicating increased protection of market incumbents.⁵ Examples of reforms in network industries include the separation (unbundling) of energy supply and generation from the operation of transmission networks, ensuring non-discriminatory access to bottleneck infrastructure (natural monopolies) to potential competitors, removing regulations restricting the number of competitors in the postal services, or lowering the licensing requirements in road freight transport.

The basic regression model indicates that lowering barriers to entry in network industries induces a net loss in employment which reaches its maximum 3 years after the reform and begins being reabsorbed afterwards (see Figure 3.1; see Box 3.1 for details on the estimation method). Based on the estimated impulse response coefficients (measuring average effects across the three broad network industries), industry employment would be around 1.2% below its initial level in the third year following a reform that lowered the regulation index by 1 point (the index ranges from 0 to 6).⁶ This response pattern is

Box 3.1. Estimating the labour market consequences of regulatory reforms: The case of network industries

Short-term analysis. A simple way to investigate the relationship between industry regulation and employment (or another measure of labour market performance) in the short run is to estimate a first-difference dynamic equation allowing for both contemporaneous and lagged impacts of regulation (see OECD, 2016a; and Bassanini, 2015):

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

where $\Delta E_{cit} = \ln L_{cit} - \ln L_{cit-1}$ is the annual change in the logarithm of employment in country c , industry i and time t , and ΔBE is the change in regulation. The vector X accounts for the potentially confounding role of other forms of industry regulation (i.e. the extent of public ownership) or the burden of barriers to entry in other industries; ε stands for a standard error term. Including lagged values of the dependent variable accounts for possible persistence in employment changes. The number of lags T is chosen based on statistical criteria as the Bayesian's (BIC) or Akaike's (AIC). In all estimations, standard errors are clustered at the country-industry level.

The bi-dimensional fixed-effects D_{ct} , D_{it} and D_{ci} are intended to capture, respectively: i) country-specific shocks to employment growth common to all 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. The comparison with other industries, however, might not be a valid counterfactual if there are spillover effects, e.g. if PMR reforms in the energy market affects employment dynamics in the transport industry. To check for the presence of cross-industry spillovers, the baseline specification is therefore augmented with the average change in regulation in "other" network industries.* This control attracts a small and highly non-significant coefficient, suggesting a minor role for spillover effects within network industries.

With this rich set of controls, the estimated coefficients would not be interpretable as the aggregate impact of deregulation on employment in the presence of country-industry shocks to employment growth that are neither common to all other industries in the country, nor shared with the same industry across countries, nor captured by long term country-specific industry trends, nor reflecting cross-industry spillover effects, and yet are systematically correlated with PMR deregulation. To further account for these concerns the analysis presents alternative tests of reverse-causality. One consists of including forward terms of regulation. A finding that future regulation affects current employment would provide evidence of reverse causality. Granger-causality tests are also performed, 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.

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

Box 3.1. Estimating the labour market consequences of regulatory reforms: The case of network industries (cont.)

The results of the short-term analysis are represented plotting impulse-response functions obtained using the local-projection estimator developed by Teulings and Zubanov (2014). Impulse response functions provide a simple way to illustrate how the impact of deregulation (if any) evolves over time. Because the estimated contemporaneous coefficient (β_0) might still be affected by simultaneity or reverse causality biases, the functions are obtained using only the coefficients estimated on lagged regulation (β_k) and no statement is made as regards the effect of deregulation in its immediate aftermath. See OECD (2016a) for more details, where alternative impulse-response figures are presented that include the contemporaneous coefficient.

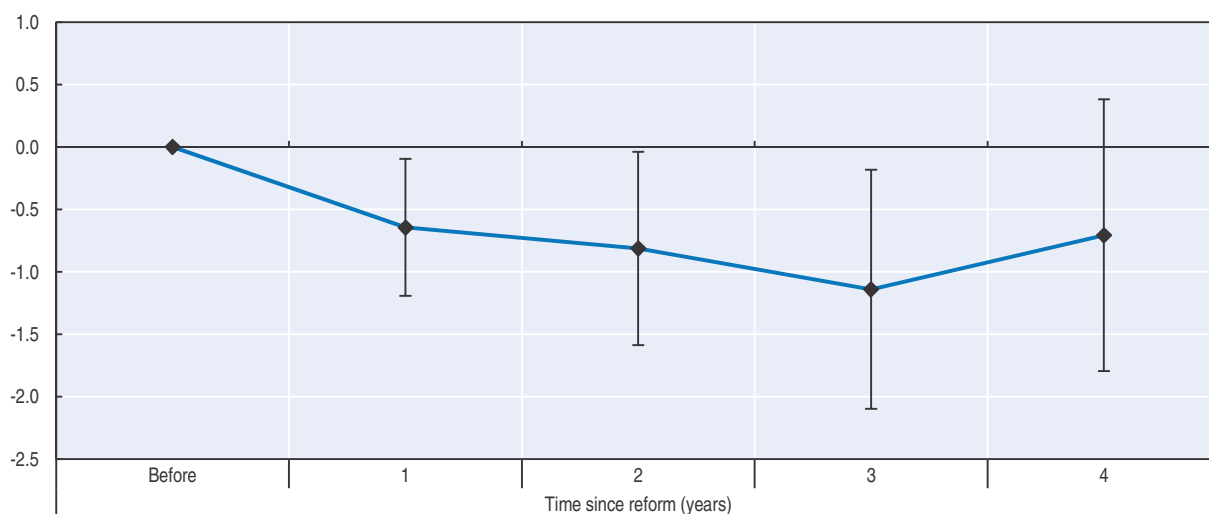
Long term analysis. The longer term relationship between regulation and labour market outcomes is estimated in a static panel setting:

$$E_{cit} = \beta_{LR} BE_{cit} + \eta_{ct} + \eta_{it} + \eta_{ci} + \epsilon_{cit}$$

where E_{cit} is the (log of) employment in country c , industry i and time t , BE is the level of regulation in the industry and the η s are bi-dimensional fixed-effects.


* Specifically, equation (1) is augmented with the annual change in the term: $WBE_{dit} = \sum_{-i} Exp_{i,-i} * BE_{c,-i,t}$, where $Exp_{i,-i}$ are coefficients from the US Inverse Leontief Matrix measuring how many units of input $-i$ have to be produced (at any stage of the value chain) to produce one additional unit for final demand in industry i .

Figure 3.1. **Competition-enhancing reforms and employment in network industries**
Estimated cumulative change in industry employment up to four years following the reform, in percentage



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

Source: OECD estimates based on EU KLEMS and the OECD Product Market Regulation Database.

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confirmed across a number of alternative specifications of the estimating equation (for example accounting for the potentially confounding role of changes in the degree of public ownership – another dimension of regulation captured by the OECD indicators) or of the dependent variable (i.e. using salaried as opposed to total employment).⁷ Moreover, the results are robust to extending the time window to include the Great Recession years and to variations of country sample (see OECD, 2016a).⁸ No evidence is found that the impact of the reforms varies with the initial level of regulation or that the impact of pro-competitive reforms is non-linear in initial regulation (being, for example, stronger in high than low regulated countries).⁹ Finally, the employment response does not vary depending on the specific network industry implementing the reform. The short-term loss is not insignificant from an economic point of view if compared with the average growth rate of employment in the sample between 1975 and 2007 (0.3%).¹⁰

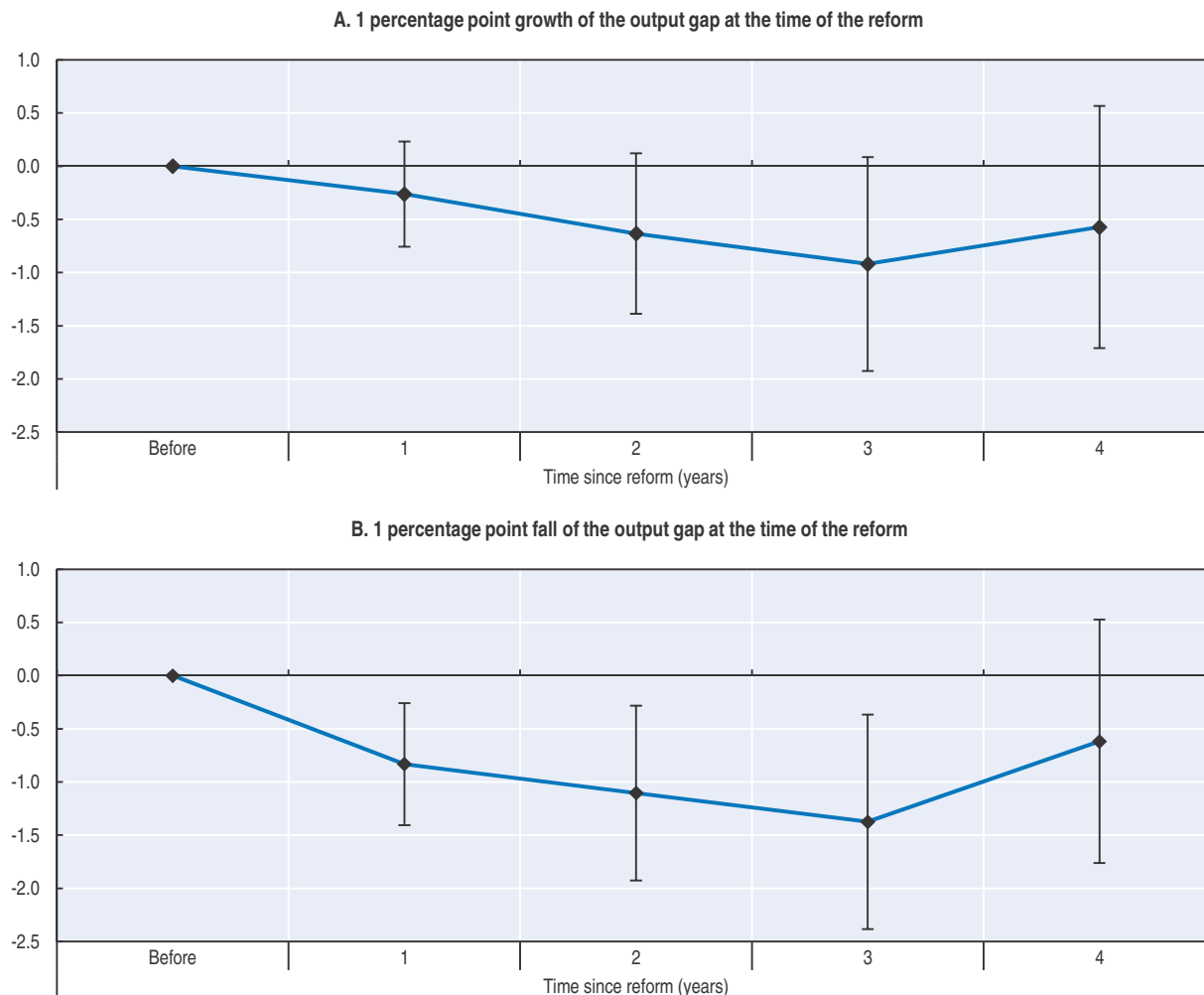
The U-shaped pattern of employment is consistent with the idea that labour market outcomes following the removal of entry barriers in network industries reflect two offsetting but asynchronous forces: the immediate re-organisation of (large) incumbents and the gradual expansion of successful entrants. This interpretation is also consistent with preliminary evidence by Gal and Hijzen (2016) who find that, in the short run, deregulation depresses employment in large firms with respect to small firms in network industries.¹¹ While new jobs tend to eventually compensate for the initial losses, the analysis does not find evidence that competition enhancing reforms increase employment in the long run, as other researchers have found in the case of retail trade (see OECD, 2016a, Table 3.A1.6). Looking across alternative empirical specifications and samples for estimating the long-run impact of industry employment reveals no significant impact.¹²

Importantly, the analysis does not provide evidence that the short-term employment adjustments are accompanied by a significant fall in wages. In fact, replicating the analysis using average hourly industry wages reveals no significant effect. The positive but small and statistically insignificant estimated wage impact in the base model disappears when the model is extended to control for composition effects (by including changes in employment and in the share of employees with less than upper secondary education in total hours worked). This suggests that the burden of the employment adjustment weighs disproportionately on low-wage, low-productivity workers, whose displacement artificially raises the average of observed wages (for more details see OECD, 2016a).

Economic downturns are often seen as good times to implement structural reforms; their urgency and public support are often higher than in good times (see Section 3 for a discussion). But are the short-term economic costs of reform smaller or larger in an economic downturn? On the one hand, the contribution of deregulation to labour shedding would be marginal in a period of large job destruction. On the other hand, the high uncertainty characterizing downturns might also significantly slow the job creation stimulated by structural reforms, by lowering the number of new firms or how rapidly they grow.¹³ Allowing the employment impact of deregulation to vary along the cycle provides supportive evidence for the latter hypothesis, as is illustrated by Figure 3.2. The two panels plot the employment response to a reform implemented when the growth rate of output is, respectively, 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


Figure 3.2. **The employment effects of competition-enhancing reforms in upturns and downturns**

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



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

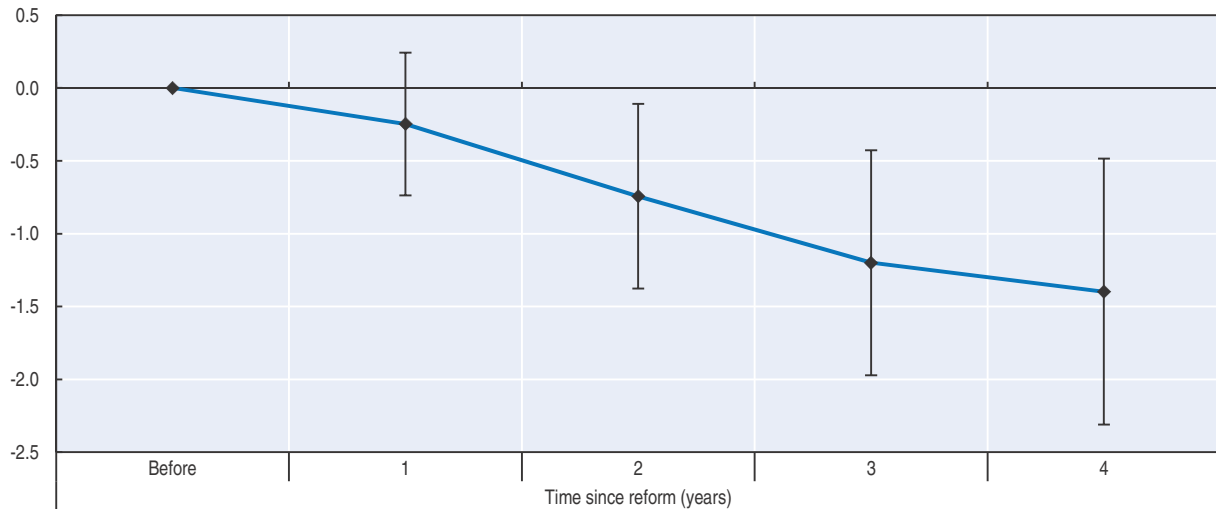
Source: OECD estimates based on EU KLEMS and the OECD Product Market Regulation Database.

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phase. In both cases, industry employment reaches a minimum three years after the reform, but this is 1.4% below the level that would have been observed absent for the reform during a downturn and is smaller and statistically insignificant in the upturn scenario.


Consistent with the idea that pro-competitive reforms induce an efficiency-enhancing re-structuring process, lower regulation is followed by falling prices. The level of the industry output deflator falls on impact and continues decreasing in the years following the reform (Figure 3.3). Taking the estimated coefficients at face value, four years after the reform the industry price index is nearly 1.5% below its pre-reform level. In the long run,

Figure 3.3. Competition-enhancing reforms and prices in network industries
 Estimated cumulative change in industry output deflator up to four years following the reform, in percentage



Note: The figure reports point estimates and 90%-confidence intervals of the cumulated price 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. Price levels before the reform are normalised to 0. The underlying parameters are estimated allowing price changes in each network industry to depend on lagged values of industry regulation as well as on lagged price changes. Confidence intervals are obtained by clustering errors on countries and industries.

Source: OECD estimates based on EU KLEMS and the OECD Product Market Regulation Database.

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this fall approaches 3% (see OECD, 2016a). Significant declines in price are consistent with the large literature emphasising the sizable efficiency gains in terms of total factor (or labour) productivity (e.g. Olley and Pakes, 1996; Disney, Haskel and Heden, 2003; and Schmitz, 2005) and of production costs per unit of output (Knittel, 2002; and Fabrizi, Rose and Wolfram, 2007). The magnitude of the price declines estimated here probably understate the broader gains from enhanced competition, which likely include better quality services. While the quality dimension is difficult to capture with the data used in this chapter,¹⁴ the positive association between deregulation and service quality has been highlighted in a number of works exploiting detailed microdata in the case of transportation (Mazzeo 2003; and Greenfield, 2014), legal services (Domberger and Sherr, 2003), retail trade (Matsa, 2011), health (Bloom et al., 2015; and Cooper et al., 2011) and education (Hoxby, 2000).

The indirect labour market consequences of competition-enhancing reforms

Improved outcomes in network industries are in turn likely to have spill-over effects on the performance of activities using these services as production inputs (i.e. downstream industries). For example, if deregulation implies that service inputs become cheaper or that their quality improves, the unit cost of production among users of such input would lower, potentially favouring their competitiveness and expansion. Moreover, lower monopoly power upstream would increase users' incentives to improve efficiency and innovate if it reduces the share of rents that would be appropriate by suppliers (Bourlès et al., 2013).¹⁵ These predictions are confirmed in the long run, as intensive users of services tend to benefit from input deregulation both in terms of their value added and export shares (Barone and Cingano, 2011) and in terms of productivity (Arnold et al., 2011;

and Bourlès et al., 2013). Similar findings might be expected when looking at employment or wages, though (as in the case of the reformed industries) the underlying adjustments might imply non-negligible transition costs.

The strength of the indirect effects of deregulation is examined assuming that, if increased competition in input production benefits downstream employment (e.g. because of the lower input price, better quality or improved market efficiency) then these benefits should be relatively stronger for intensive input users. This assumption, which has been exploited in single-country investigations (Forlani, 2010; and Arnold, Javorcik and Mattoo, 2011) and cross-section analyses (Allegra et al., 2004; Faini et al., 2006; Barone and Cingano, 2011; and Bourlès et al., 2013), can be brought to the chapter's richer data by estimating an interaction model where the effect of upstream regulation is allowed to vary across users depending on their dependence on the regulated input (see Box 3.2, and OECD, 2016a for details).

For the purpose of this chapter, the input-dependence models described in Box 3.2 are estimated on a sample covering 19 non-agricultural/non-mining business-sector industries, 22 OECD countries and up to 37 years.¹⁶ Consistent with the existing evidence on productivity, the analysis shows that network deregulation has a positive impact on users' employment in the long run; whether this effect already materialises in the short run is, however, unclear.

The implied long-run effects are highly statistically significant and can be large enough to be of considerable economic importance. Comparing two industries whose overall dependence on network industries differs by one percentage point, a policy uniformly lowering regulation in each network industry by one point would increase long-run employment in the most dependent industry by 0.65 percentage points relative to the least dependent industry. Aggregating the effect at the country level following the methodology and assumptions illustrated in Box 3.2, the same policy would raise employment in a representative country by around 1% (see OECD, 2016a).

Figure 3.4 further illustrates the potential indirect benefits from deregulation plotting the aggregate long-run employment gains from adopting the "lightest regulatory practice" observed around 2012 in each upstream industry. This best-practice benchmark is defined as the average of the three lowest levels of anticompetitive regulation observed across countries and should therefore be considered an ambitious, although not unrealistic, policy goal. The baseline specification implies that the long-run employment gains from such reforms would exceed 3% in the most highly regulated countries (Mexico, Israel and Korea), while falling below 1% in countries that are already close to (or represent) the best practices in some sectors (e.g. the United Kingdom and Australia). The (simple) average gains across OECD countries would be of about 2%.

This result is robust to a number of alternative specification and variable choices (see OECD, 2016a). These include using unweighted as opposed to weighted estimation,¹⁷ using salaried (rather than total), employment as the dependent variable, or augmenting the specification to account for the extent of public ownership in the deregulated industry. They are also robust to extending the time window to include the Great Recession years, and to variations of country sample.¹⁸ On the other hand, no evidence is found that the impact of upstream reforms varies depending on the initial level of regulation (being, for example, stronger in a more highly regulated country). Finally, the analysis reveals that the benefits from service deregulation are stronger (and only statistically significant) among

Box 3.2. Estimating the indirect effects of deregulation in network industries

To assess whether reforming network industries matters for labour market outcomes in the rest of the economy, the chapter exploits the methodology originally developed by Rajan and Zingales (1998) which exploits cross-country/cross-industry variation to assess the relevance of country-level policies while accounting for country fixed effects. This approach attenuates the estimation concerns that would arise in a standard cross-country regression (e.g. omitted variables, reverse causality). The approach relies upon the assumption that each industry has a “technological” characteristic implying a specific degree of exposure to regulation in upstream network industries (i) and that this characteristic varies across downstream industries (j). If a measure of industry exposure (Exp_{ji}) is available, then the impact of upstream regulation on downstream employment can be estimated looking at the interaction model $E_{cj} = \theta(Exp_{ji} * BE_{ci}) + \eta_c + \eta_j + \xi_{cj}$, where E_{jc} measures the (log of) employment in industry j and country c , BE captures the level of anti-competitive regulation in the market for input i , and the fixed effects η capture country and industry time invariant characteristics. If upstream regulation matters for downstream employment E_{jc} then one would expect high exposure industries to react more strongly to deregulation (i.e. to estimate $\theta < 0$).

For the purposes of this chapter, the above *input-dependence model* is adapted to fit a time-series framework. The adapted model can be used to estimate both short and long term coefficients and to the impact of deregulation in more than one network industry. To examine the short run consequences of deregulation on employment the following equation is estimated:

$$\Delta E_{cjt} = \theta_0 \Delta WBE_{cjt} + \sum_{k=1}^T (\theta_k \Delta WBE_{cjt-k} + \rho_k \Delta E_{cjt-k}) + v_{ct} + v_{jt} + v_{cj} + \xi_{cjt} \quad (1)$$

where $WBE_{djt} = \sum_i Exp_{ij} * BE_{cit}$ measures the overall exposure to the stringency of product market regulation in network industries i (Energy, Transport and Communication) in each downstream industry j , and ΔE_{cjt} measures year-on-year employment growth. The set of bi-dimensional fixed-effects v are intended to capture potential confounding factors as described in Box 3.1.

Following the literature, Exp_{ji} is measured using input-output coefficients. Specifically, the exposure of industry j to each service is measured by the corresponding coefficient of the Inverse Leontief Matrix, which describes how many units of input i are required (at any stage of the value chain) to produce one additional unit for final demand in industry j (*input dependence*). Hence, the identification assumption is that high-intensity users of a regulated input would benefit more than low intensity users from policies that enhance competition in the production of that input. To reduce the estimation concerns arising if input dependence responds to the level of regulation, the analysis exploits country invariant input-output coefficients. Following the literature, these correspond to those measured in a benchmark (or frictionless) country characterised by low levels of regulation. US input-output coefficients are used for this purpose. Accordingly, the United States is excluded from the estimation sample. An alternative approach is also used which consists of taking industry-averages of input-output coefficients measured across all countries, after having netted out input intensities specific to each country or to the level of regulation (see OECD, 2016a for details). Based on this approach, the most dependent industries include food products, basic metals, non-metallic and rubber and plastics products; and the least dependent industries include electrical and optical equipment, real estate activities, wholesale and retail trade, and financial intermediation.

As in the case of direct effects (see Box 3.1), impulse-response functions are obtained using the local-projection estimator developed by Teulings and Zubanov (2014). Differently from that case, however, the employment responses will be computed also accounting for the contemporaneous coefficient (θ_0), as reverse causality issues are less of a concern in the framework of indirect effects. See OECD, 2016a, for more details.

The long-run, indirect effects of regulation in network industries are estimated in the static version of the input-dependence model:

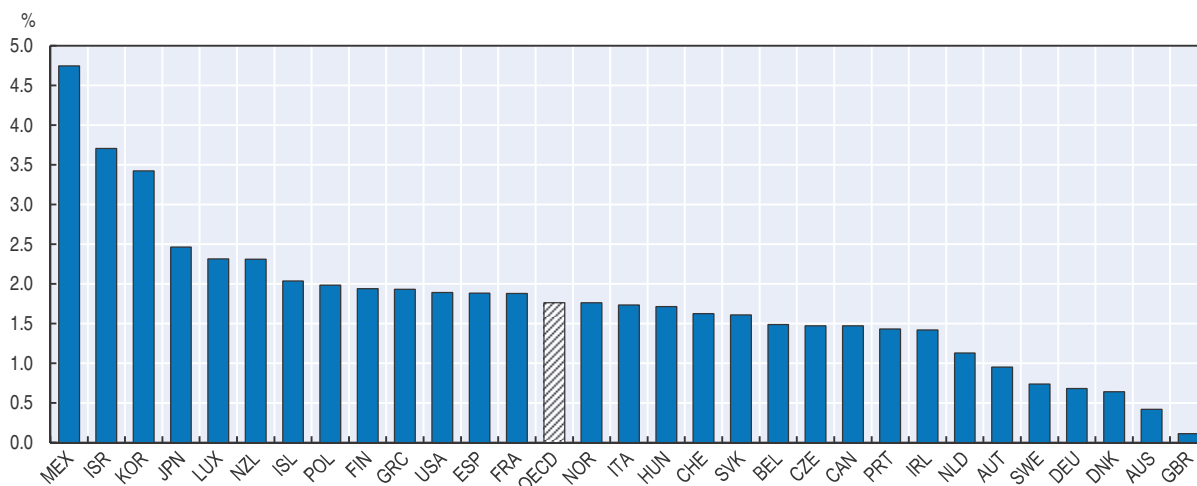
$$E_{cjt} = \theta_{LR} WBE_{cjt} + \mu_{ct} + \mu_{jt} + \mu_{cj} + \epsilon_{cjt} \quad (2)$$

Box 3.2. Estimating the indirect effects of deregulation in network industries (cont.)

Strictly speaking, country-industry interaction models such as (2) only allow the differential impact of regulation on industry outcomes to be estimated. The aggregate consequences on overall employment can be recovered only by imposing the (strong) assumption that one or more low-exposure industries would actually not be affected by the reform (see Guiso et al., 2004; and Bassanini, Nunziata and Venn, 2009 for a discussion). In this case, the aggregate effect of reforms can be computed in two steps (see Guiso et al., 2004). In the first step, the estimated coefficient θ is used to predict the employment gains in country c and industry j : $\Delta \ln E_{cj} = \hat{\theta}_{LR} \sum_i Exp_{ij} * \Delta BE_{ci}$, where the changes in entry barriers (ΔBE_{ci}) can be hypothetical or reflect actual reforms. In the second step, industry specific gains are aggregated to the country level $\Delta \ln E_c = \sum_j Sh_{jc} * \Delta \ln E_{cj}$, where Sh_{jc} is the (employment) share of industry j in country c . Importantly, all of the aggregation exercises presented in this chapter impose the (conservative) assumption that, the reforms in any regulated service i have no effect on industries whose exposure to the regulated service (Exp_{ij}) is lower than (or equal to) the first quartile of the distribution of exposure.

Figure 3.4. Long-run indirect effects of competition-enhancing reforms in network industries

Aggregate employment changes in downstream industries from reaching “lightest practices” upstream, in percentage



Note: The figure reports the estimated long-run percentage point increase in employment in the non-agricultural/non-mining business sector (excluding network industries) following deregulation of barriers to entry in network industries. Regulation and employment levels in 2013 are taken as starting points. The underlying parameters are obtained estimating an input-dependence model, which assumes that the downstream impact of deregulation is greater the greater users' exposure to the regulated input. Business-sector aggregation is computed assuming that reforms of barriers to entry would have no effect on employment in a hypothetical industry whose exposure is equal to or lower than the first quartile of the distribution of exposure. The figures plotted refer to a thought experiment in which regulation in each network industry is reduced to the “lightest practice”, defined as the average of the three lowest levels of industry regulation observed across countries.

Source: OECD estimates based on EU KLEMS and the OECD Product Market Regulation Database.

StatLink  <http://dx.doi.org/10.1787/888933384698>

manufacturing producers than it is for other service producers (e.g. in the retail sector). This finding is in line with the idea that efficiency improvements generate the largest gains in industries or markets exposed to international competitive pressures, where even small productivity advantages can induce large increases in market shares.

In the short term, by contrast, the estimated strength of indirect effects of upstream deregulation is not robust to the estimation method. Weighted estimates would suggest that the long term employment benefits unfold quickly, with aggregate employment increasing by 0.2% in the reform year and in excess of 0.4% three years after the reform. In

sharp contrast, unweighted estimates show negative and non-significant short-term responses of users' employment. The results remain unclear when looking at salaried employment, extending the sample to include the Great Recession years or allowing the coefficient to vary between manufacturing and service users. Overall, the analysis is unable to determine how quickly the long-run positive employment effects unfold.

Overall, the evidence provided in this section suggests that increasing the level of competition in network industries induces modest, but non-negligible, direct costs in terms of industry-level employment losses. These losses are rapidly reabsorbed, but the reform does not result in significant long-run employment or wage gains in the industries directly affected by the reform. Both findings contrast with prior evidence on the impact of lowering entry barriers in retail trade, which appears to cause no short-term employment losses and long-term gains. One plausible explanation for these discrepancies is the different conditions in the underlying market. Lifting entry barriers is likely to entail short-term job losses in concentrated markets, as large incumbents react to (the threat of) enhanced competition by re-organizing and reducing overstaffing in an attempt to increase productivity and lower prices. Retail trade regulation, on the other hand, typically limits the presence of large and efficient distributors in a market dominated by many relatively small firms. The results of this section therefore underline the likelihood that whether or not competition-enhancing reforms induce short-term employment costs is likely to depend on the characteristics of the regulated industry. In particular, the extent to which large players dominate the market is likely to be important, because large firms are probably more likely to re-organise and downsize in an attempt to deter entry. On the positive side, the analysis also shows that reform-induced reorganisation in upstream markets can have a positive long-term impact on the employment performance of downstream industries. The following section will apply a similar estimation framework to analyse the labour market impacts of reforms of employment protection legislation.

2. Employment protection legislation

The employment protection legislation (EPL) is defined in this chapter as the set of rules governing the hiring and, especially, firing of workers. There is a theoretical consensus that inefficient statutory dismissal protection may inhibit efficient job separations and, indirectly, reduce efficient job creation by imposing implicit and explicit costs when a firm adjusts its workforce to keep it at its optimal level (e.g. Mortensen and Pissarides, 1994, 1999). In principle, the inefficiencies otherwise implied by job security provisions could be avoided via wage adjustments, private payments or the design of efficient contracts (Lazear, 1990). However, wage rigidities, financial market imperfections or uncertainty about the future of the firm appear to prevent these channels from operating fully so that EPL creates positive firing costs which imply that the optimal strategy for firms is to reduce both hiring and firing, with an ambiguous effect on average employment and its fluctuation over the business cycle (e.g. Bentolila and Bertola, 1990; and Mortensen and Pissarides, 1999). The inefficiently slow resource reallocation that results from firing regulations is also likely to result into lower productivity (e.g. Hopenhayn and Rogerson, 1993). Moreover, excessively stringent layoff regulations might discourage firms from experimenting with new technologies, which are characterised by higher mean returns but also higher variance, due to the associated risk of paying high firing costs in the case of venture failure (Saint-Paul, 2002; and Bartelsman et al., 2004).

Employment protection is also a key determinant of the degree of labour market dualism – that is the extent to which employment is divided between protected permanent contracts and precarious temporary contracts with high barriers of transitions between the two types of positions. When the use of fixed-term contracts is liberalised while maintaining strict employment protection regulations for open-ended contracts, firms react by substituting temporary for regular workers (since the former are cheaper to terminate at the end date of the contract), with no long-run effect on employment. In addition, a large asymmetry between the employment protection provisions applying to the two types of contracts reduces the conversion rate of fixed-term contracts into permanent ones, thereby transforming fixed-term contracts into a trap rather than a stepping stone into more stable employment. Overall, the theoretical literature suggests that a large wedge between regulations for temporary and permanent contracts is likely to contribute to the emergence of a persistent divide across workers holding different types of contract in terms of both current working conditions and future prospects (e.g. Boeri, 2011; and Bentolila, Dolado and Jimeno, 2008).

Most empirical studies investigating medium/long-term effects of flexibility-enhancing EPL reforms suggests that they have, at worst, no or a limited positive impact on employment levels in the long run (see OECD, 2013, for a survey). There is also strong evidence that flexible dismissal regulations increase both separations and hirings in the long run, and foster a more efficient reallocation of resources (see Martin and Scarpetta, 2012, for a survey). As a result, reducing the cost of dismissal for firms is typically found to have a positive impact on productivity and economic growth in the long run (see e.g. Autor, Kerr and Kugler, 2007; Bassanini, Nunziata and Venn, 2009). A vast empirical literature tends to confirm the theoretical prediction on the effect of EPL on labour market dualism by showing that the incidence of temporary contracts usually is higher the more rigid the regulations concerning dismissal for permanent contracts and the less restrictive the legislation about hiring on and renewal of temporary contracts (see OECD, 2014b for a survey).

The short-term consequences of liberalisation of dismissal restrictions have been less studied. In a standard search and matching theoretical model, a reduction in termination costs induces the immediate destruction of those job matches that, before the reform, were inefficient (that is, yielding negative revenue) but were not destroyed only to save adjustment costs in the expectation of a future rebound of product demand and employment needs. By contrast, due to matching frictions those job vacancies becoming viable because the reduction in termination costs has increased their expected profitability are filled only after a slow search and hiring process. In addition, newly-profitable high-risk activities may require building up new infrastructure and equipment, which takes time. This implies that, in the short run, separations would be expected to increase faster than hiring, causing employment to fall (see e.g. Mortensen and Pissarides, 1999; Cacciatore and Fiori, 2016; and Cacciatore et al., 2016).

Empirical analyses explicitly examining the short-term effects of EPL reforms on employment and worker flows are rare, since the objective of most micro-econometric studies has been to estimate the steady-state effect of these reforms. However, a few of these studies focus on a short time window around the reform and, as such, provide some evidence on short-term effects.¹⁹ For example, von Below and Thoursie (2010) study a 2001 Swedish reform that introduced an exemption for small firms to the rigid application of the

last-in-first-out principle for the selection of redundant workers. Comparing firms just above and below the threshold, they find that the reform increased both hiring and separations in a similar way in the medium-run. However, separations increased faster in the first two years after the reform while the effect on hiring was more gradual. Malk (2013) looked at the effect of a 2009 reform that radically relaxed dismissal regulations in Estonia using Lithuania as a comparison group. She finds no significant effect on hiring in the two years following the reform, while separations, particular involuntary ones, increased. Martins (2009) evaluate the effect of a 1989 reform in Portugal which enlarged the definition of fair dismissal and somewhat simplified dismissal procedures, in particular for small firms. Comparing the effects between small and large firms, he finds no effect on separations, but a significant positive impact on hiring and thus net job creation, which however materialised no earlier than 3 years after the reform. Similar results are found by Behaghel, Crépon and Sédillot (2008), who assess the introduction in France of an exemption to the tax on firing workers aged 50 years or more. In 1992, firms were exempted from paying the tax if the employee was recruited after having reached age 50. The authors do not study the short-term effect of this reform on firing patterns, but find that the reform increased transitions from unemployment to employment for older men, with the effect becoming larger as time goes by. Overall, these studies tend to find that the impact of flexibility-enhancing reforms of dismissal restrictions on employment and worker flows tend to become more positive (less negative) over time.²⁰

There is even less research on short-term effects of relaxations of EPL on earnings. Martins (2009) also explores the effect on average wages and finds that the reform reduced them by 3% in the first three years, but half of this negative effect had already disappeared two years later. Similarly, van der Wiel (2010) finds that a 1999 Dutch reform – which reduced notice periods and suppressed their dependency on worker age – had a significant negative effect on the wages of affected workers.

The short-term effects of EPL reforms have almost never been analysed in the cross-country comparative literature. The reason is that it is very difficult to control for an exhaustive list of confounding factors in cross-country/time-series empirical models. This identification issue is even more severe in the case of short-term effects since dynamic models with many lags are typically required, thereby limiting further the number of confounding factors that can be included.²¹ The typical solution to solve the omitted-variable problem in macro panels is to run difference-in-difference experiments by adding to the data one additional dimension – that has to do with the predicted intensity of the effect of policy of interest while being unrelated with possible omitted factors. In the case of EPL, the literature has typically resorted to cross-country/cross-industry data, identifying the effect of regulations by comparing their effect across industries with different propensities to make staff adjustments and, therefore, where dismissal restrictions are more or less likely to be binding (see e.g. Bassanini, Nunziata and Venn, 2009; Cingano et al., 2010; Haltiwanger, Scarpetta and Schweiger, 2014; Griffith and Macartney, 2014; and Caroli and Godard, 2016). In the next subsection, this approach is used to study short-term effects of EPL reforms, how these effects vary over the business cycle and whether they differ across economies with different degrees of labour market dualism. Complementary evidence stemming from three additional country case studies is also provided.

Short-run labour market effects of EPL reforms: Evidence from industry-level, cross-country data

For the purpose of this chapter, the difference-in-difference approach is adapted to study the short-term impact of reforms of dismissal legislation on wage and salary employment (see Box 3.3) on a sample covering 22 non-agricultural/non-mining business-sector industries, 21 OECD countries and up to 27 years.²² EPL reforms are quantified on the basis of changes in the indicator of stringency of EPL for individual dismissals of workers on permanent contracts,²³ with reductions indicating flexibility-enhancing reforms and increases indicating protection-raising reforms.²⁴ More precisely, to the extent that changes in the indicator are typically small and often imprecisely

Box 3.3. Estimating the effect of employment protection reforms for regular contracts: Industry-level difference-in-difference estimates

For the purpose of this chapter, the effects of employment protection reforms on dependent employment,^a wages and skill shares have been estimated using a reduced-form model on industry-level data, with an approach similar to that followed in the previous section (see Box 3.2). The estimation strategy is based on the assumption that dismissal regulations are more binding on firms' behaviour in industries that, in the absence of regulation, have greater propensity to make staff adjustments on the external labour market. Formally, the model can be written as:

$$\Delta E_{cjt} = \theta_0 D_j \Delta EPL_{ct} + \sum_{k=1}^T (\theta_k D_j \Delta EPL_{ct-k} + \rho_k \Delta E_{cjt-k}) + X_{cjt} \delta + v_{ct} + v_{jt} + v_{cj} + \xi_{cjt}$$

where E measures the (log of) employment (or one of the other performance variables) in industry j country c and time t , EPL captures the stringency of dismissal regulation on permanent contracts and D stands for the propensity to make staff adjustments in response to shocks. As in the case of the previous section, the optimal number of lags is chosen on the basis of Akaike's and Bayesian information criteria. The set of bi-dimensional fixed-effects v aim at capturing potential confounding factors, as described in Box 3.1. X stands for a vector of additional confounding factors that vary across countries, industries and years. Among these, it is key to control for (simultaneous and lagged) changes in the output gap interacted with the staff adjustment propensity, since it has been shown that more volatile industries tend to suffer from larger employment swings over the business-cycle (e.g. OECD, 2012). Following Bassanini, Nunziata and Venn (2009), the propensity to make staff adjustments is measured through US dismissal rates, and excluding the United States from the analysis. Using a benchmark defined for the United States – the least regulated country in the OECD as regards legislation for individual dismissals – avoids possible estimation bias resulting from a correlation between EPL stringency and the cross-industry dismissal distribution.^b

The objective of this chapter, however, is to identify the short-term effect of flexibility-enhancing reforms. As the recent history of OECD countries also includes protection-raising EPL reforms that increased the EPL indicators, it is crucial to separate positive and negative changes of EPL in the analysis. Another complication is that changes in the EPL indicators are typically small, rare and measured with significant error (see OECD, 2013). For this reason, the baseline model makes use of a dummy variable taking value 1 when the EPL indicator decreases and 0 otherwise to measure flexibility-enhancing EPL reforms. A separate dummy variable taking value 1 when the EPL indicator increases and 0 otherwise is also included in the model. The estimated coefficient of the dummy for decreases in the EPL indicator captures the change in wage and salary employment associated with a reform of historically average extensiveness, as measured by the average negative changes of indicator across all reform episodes of the sample. Formally the estimation model can be written:

$$\Delta E_{cjt} = \theta_0 D_j FE_{ct} + \mu_0 D_j PR_{ct} + \sum_{k=1}^T (\theta_k D_j FE_{ct-k} + \mu_k D_j PR_{ct-k} + \rho_k \Delta E_{cjt-k}) + X_{cjt} \delta + v_{ct} + v_{jt} + v_{cj} + \xi_{cjt} \quad (1)$$

where FE and PR stand for the flexibility-enhancing and protection-raising reform dummies, respectively.

Box 3.3. Estimating the effect of employment protection reforms for regular contracts: Industry-level difference-in-difference estimates (cont.)

This approach has become increasingly popular in the literature as a way to overcome omitted variable and reverse causality issues in the analysis of the effects of EPL. In fact, all omitted aggregate institutions whose impact on the performance variable is unlikely to vary across industries as a function of their dismissal intensity are controlled for through fixed effects.^c Reverse causality is also less of a concern in this framework since it would imply that economy-wide reforms are significantly affected by idiosyncratic fluctuations of specific industries. The sign of the estimated θ parameters can therefore be given a causal interpretation.

Rigorously speaking, the approach adopted here allows identifying only differential effects between EPL-binding and other industries. As discussed in e.g. Bassanini, Nunziata and Venn (2009) and Bassanini and Garnero (2013), inferring a lower bound to the aggregate effect of EPL reforms from interaction models such as (1) requires assuming that the sign of the effect in industries where EPL is not binding is either zero or the same as in EPL-binding industries (see also Box 3.2). In principle, this assumption would be violated if, for example, by increasing dismissals in EPL-binding industries EPL reforms expanded the supply of labour in other industries, whose employment would therefore grow. In practice, however, these *general-equilibrium* effects tend to be negligible, as discussed in OECD, 2016a. Moreover, as the effect of EPL on firms' staff adjustment policies (the *direct*, partial-equilibrium effect of EPL) depends on the extent to which regulations are binding, it is unlikely that reforms of the latter have opposite effects on these policies in binding and non-binding industries. This suggests that the sign of the estimated θ parameters provides also an indication of the sign of the aggregate effect of EPL reforms, as in standard difference-in-difference models.^d

A quantitative estimate of aggregate effects is then derived based the same conservative assumption made in the previous section (see Box 3.2), that is, by taking into account the relative size of business-sector industries and imposing the assumption that EPL reforms would have no short-term effect on employment in an hypothetical industry whose US dismissal rate would be equal to or lower than the first quartile of the distribution. Finally, impulse-response functions are derived using local projection estimators à la Teulings and Zubanov (see Boxes 3.1 and 3.2).

The long-term effects are estimated by adapting the above model to a simple static framework assuming the strength of the effect of protection-raising and flexibility-enhancing reforms on steady-state equilibrium employment is the same:

$$E_{cjt} = \theta_{LR} D_j EPL_{ct} + X_{cjt} \delta_{LR} + v_{ct} + v_{jt} + v_{cj} + \xi_{cjt}$$

where the LR suffix indicates long-run parameters.

- a) In contrast with the PMR analysis, the analysis of this section is performed in terms of the effect on wage and salary employment. In the PMR analysis, total employment was used instead, in order to exploit longer time series of reliable data, with wage and salary employment being used only in sensitivity analyses. This consideration is less important for the analysis of EPL reforms, because the sample is anyhow limited by the availability of EPL data to the post-1985 period. Wage and salary employment also appears a more suitable dependent variable since EPL reform effects are expected to be stronger for dependent employment.
- b) Industry-level data on dismissals are available only for few countries, which prevents the use of the alternative benchmark-construction method adopted in the previous section.
- c) As standard in this literature, the validity of the statement that other economy-wide reforms are controlled for in this framework is checked by including one-by-one interactions between the industry-specific US dismissal rate and other labour market reforms, such as of unemployment benefit generosity, labour tax wedge, collective bargaining and regulation for hiring on temporary contracts. These interactions always turn out to be insignificant as expected (see OECD, 2016a). The variables considered refer to changes in the policies and institutions that are typically included in aggregate unemployment studies (e.g. Blanchard and Wolfers, 2000, Nickell, Nunziata and Ochel, 2005, Bassanini and Duval, 2009; and Gal and Theising, 2015).
- d) Clearly there might be other potential general-equilibrium mechanisms at play, offsetting the direct impact of EPL reforms. In order to check their relevance, the next subsection will compare the aggregate effects on employment computed here with those on unemployment estimated using a regression-discontinuity approach on high-frequency aggregate data. If offsetting general equilibrium mechanisms were relevant, the two exercises should lead to very different conclusions on the labour market consequences of EPL reforms. In fact, the regression-discontinuity results also clearly indicate that EPL reforms are followed by short-term costs.

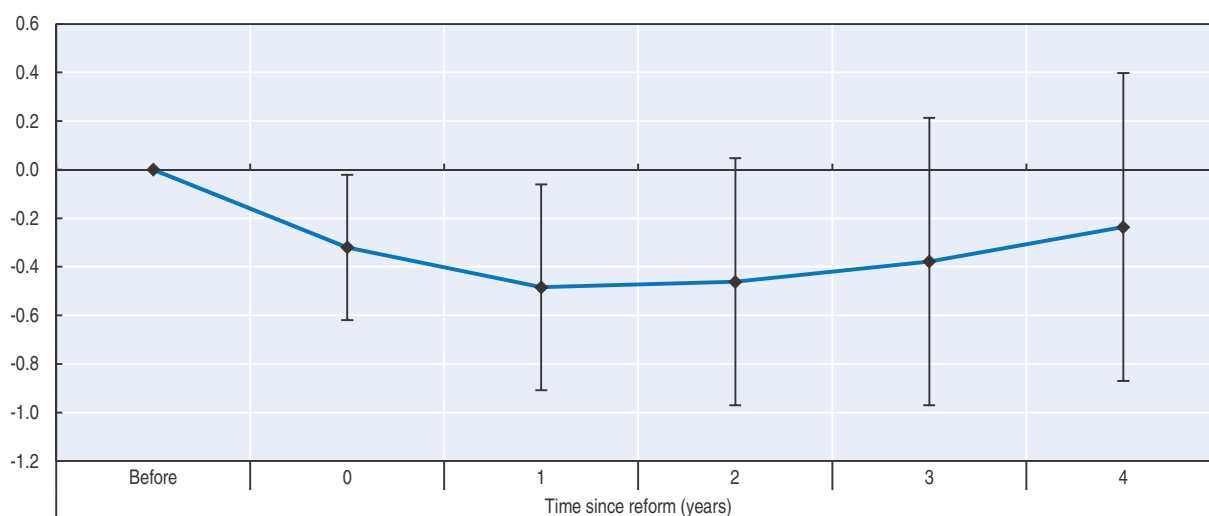
measured,²⁵ flexibility-enhancing EPL reforms are measured through a dummy variable taking value 1 when the EPL indicator decreases and 0 otherwise.²⁶ It follows that the estimated coefficients capture the change in wage and salary employment associated with a reform of historically average extensiveness, as measured by the average change across the indicator in the reform episodes of the sample.²⁷ The estimation procedure is based on the assumption that EPL is more frequently binding on firms' behaviour and thereby its changes have potentially stronger effects on gross worker flows and employment in industries that, in the absence of regulation, have a greater propensity to make staff adjustments on the external labour market, as measured by US dismissal rates.

Flexibility-enhancing reforms of dismissal legislation are estimated to significantly lower employment in the short run.²⁸ Taking the model at face value and comparing two industries that are 1 percentage point apart in terms of dismissal rates,²⁹ the contraction of wage and salary employment in the year following an average EPL reform is estimated to be larger by 0.29% in the most dismissal-intensive industry. Deriving aggregate effects subject to the same assumptions used in the previous section (see Box 3.3), this translates into an aggregate employment fall of 0.32% of business-sector wage and salary employment (Figure 3.5). The cumulative fall of business-sector wage and salary employment is estimated to reach a peak of 0.48% about one year after the reform, after which point employment begins to recover.³⁰

The overall short-term employment cost of reforms is also significant from an economic point of view. In fact, the statistics of Figure 3.5 imply that the typical flexibility-enhancing EPL reform between 1985 and 2007 lowered the business-sector employment

Figure 3.5. **Flexibility-enhancing EPL reforms and business-sector employment**

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



Note: The figure reports point estimates and 90%-confidence intervals of the cumulated 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, obtained from difference-in-difference estimators, with levels before the reform normalised to 0. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases and 0 otherwise. They can therefore be interpreted as the effect of a flexibility-enhancing reform of an average size (reducing the indicator by 0.2 points). Estimates are obtained by assuming that, in each industry, the impact of EPL is greater, the greater the US dismissal rate in that industry. Business-sector aggregation is obtained by assuming that EPL reforms would have no short-term effect on employment in an hypothetical industry whose US dismissal rate would be equal to or lower than the first quartile of the distribution. Confidence intervals are obtained by clustering errors on countries and industries.

Source: OECD estimates based on EU KLEMS and the OECD Employment Protection Legislation Database.

StatLink  <http://dx.doi.org/10.1787/888933384707>

growth rate in the first two years by as much as 25% – a figure that would be even larger if average employment growth is computed including the Great Recession.³¹ This confirms the insight from standard search and matching models which point to a short-lived employment contraction following reforms that reduce firing costs due to an immediate increase in firing and a slow reaction of hiring (see above). In turn, the same theories would suggest that the greater – albeit temporary – unemployment induced by the reform would moderate wage claims, thereby inducing downward pressure on wages. A rough look at average patterns of hourly wages in the aftermath of the reform using the same methodology does not suggest any short-term effect of EPL reforms on average wages (Figure 3.6, Panel A). Yet, this result could be a consequence of unaccounted compositional effects since those who were fired because of the reform and would have not been laid-off otherwise are likely to have been on average less productive and less well paid than retained workers. Controlling for compositional effects, flexibility-enhancing EPL reforms appear to have a small lagged effect on wages, while no immediate effect is observed (Figure 3.6, Panel B). In particular, one year after the reform, average wages appear to be significantly lower by 0.44%³² than in the pre-reform period. Moreover, in contrast with employment patterns, this wage contraction is still significant two years after the reform.³³

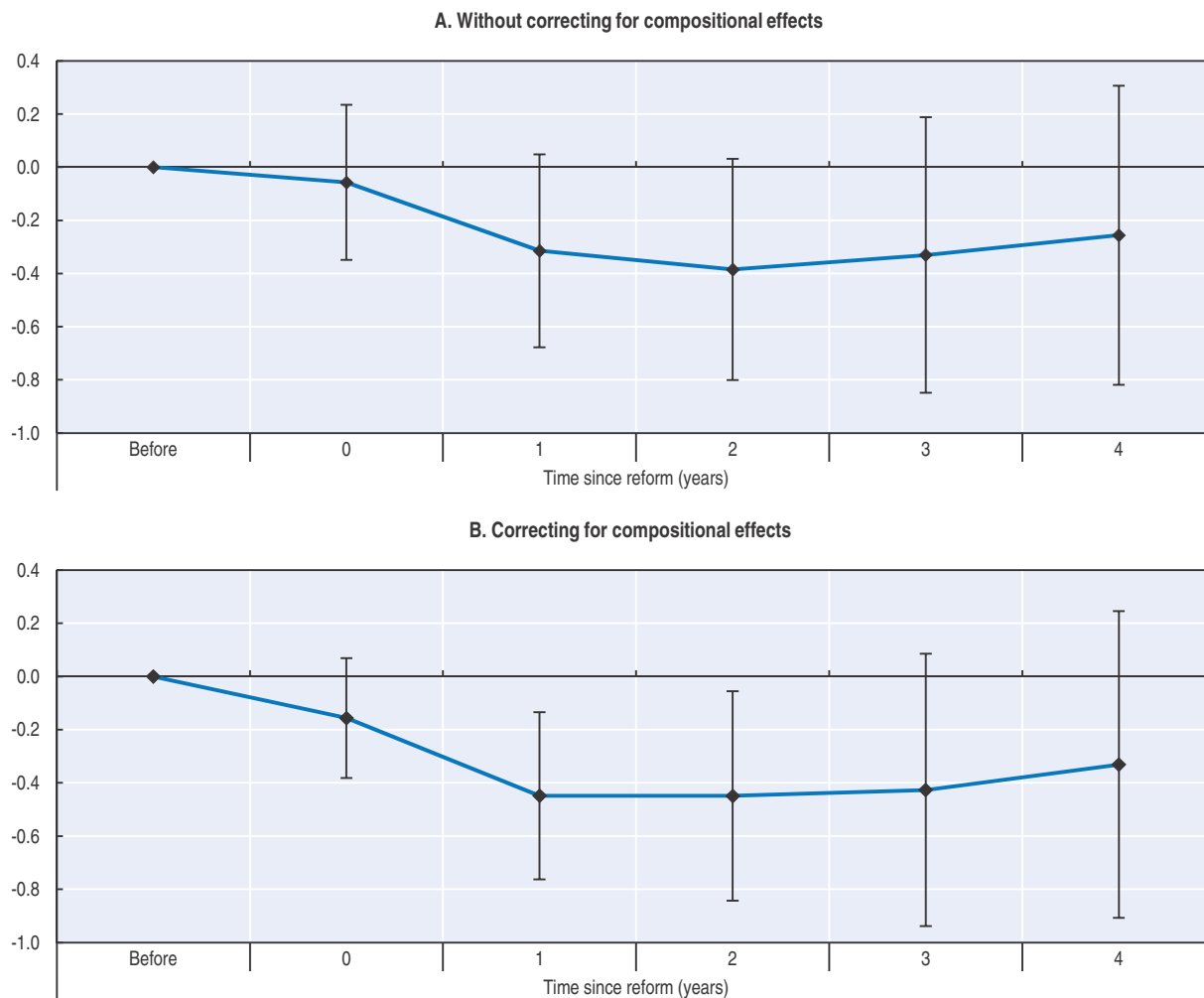
Overall, these results provide evidence that EPL reforms tend to be costly in the short run, both in terms of employment and wages, although these effects tend to disappear within few years. Does this imply that EPL reforms have no significant impact on employment and wages in the long run? As-discussed above, theory yields ambiguous predictions on the impact of EPL on employment levels in the long run and most empirical work tends to suggest that this impact is at best minor. The analysis conducted for this chapter is no exception in this respect, as shown by the first bar in Figure 3.7, which reports an insignificant negative effect of EPL reforms on wage and salary employment in the business-sector in the long run, obtained using the same methodology as above applied to a simple static model (see Box 3.3 for details).

Reforms aimed at reducing the cost of dismissals, however, are undertaken first and foremost to reduce duality and increase productivity growth in the long run. Indeed there is empirical evidence that these reforms tend to foster productivity growth and reduce the share of temporary contracts in total employment (see for example previous OECD work – e.g. OECD, 2010 and 2012; and Bassanini, Nunziata and Venn, 2009 – as well as OECD, 2013 and 2014b for surveys). In turn, greater productivity is likely to translate into higher wages.³⁴ Figure 3.7 indeed shows a positive long-run impact of EPL reforms on the wage level. Taking estimated coefficients at face value and under the same assumptions used to evaluate short-term effects, a flexibility-enhancing reform of dismissal legislation of average historical size would raise average hourly wages by 0.4% in the long run. This effect does not appear to be due to changes in the composition of labour. This is perhaps not surprising since EPL reforms are estimated here to have a positive effect on the relative employment of the low-skilled, which would tend, if anything, to lower average wages. The typical flexibility-enhancing reform is found to increase the share of those with less than upper secondary education in total hours worked by 0.2 percentage points, or 6.6% at the sample average.

Bélot, Boone and van Ours (2007) suggests that, from a growth perspective, the optimal level of employment protection is strictly positive since excessively lax regulations can reduce employees' incentives to invest in firm-specific knowledge. On the basis of their


Figure 3.6. **Flexibility-enhancing EPL reforms and business-sector wages**

Estimated cumulative change since the reform in business-sector average hourly wages up to four years following the reform, in percentage



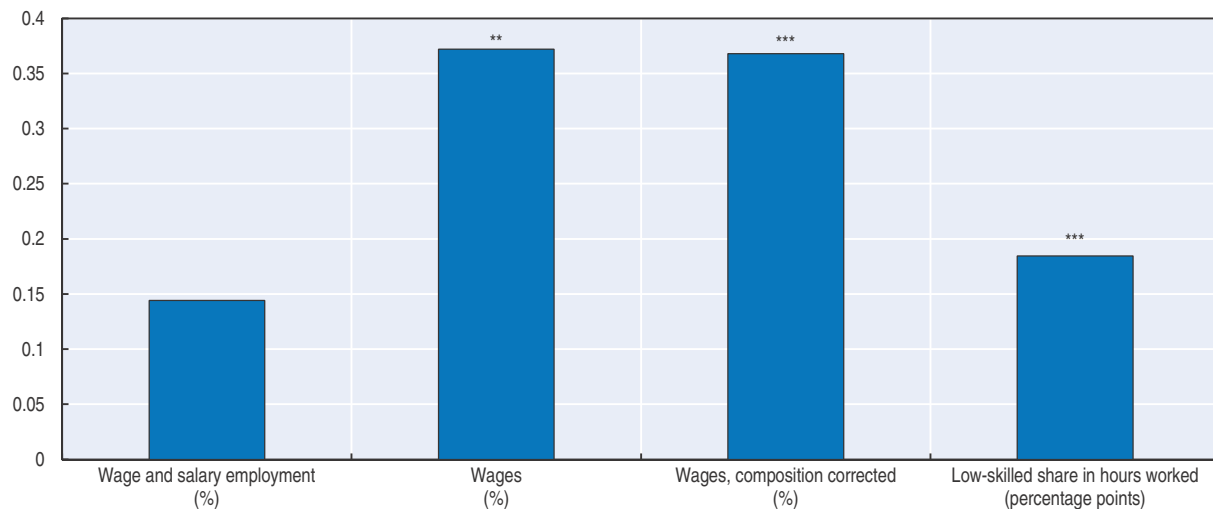
Note: The figures report point estimates and 90%-confidence intervals of the cumulative effect of changes in employment protection legislation (EPL) for regular contracts on average wage levels in the non-agricultural/non-mining business-sector, obtained from difference-in-difference estimators, with levels before the reform normalised to 0. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases and 0 otherwise. They can therefore be interpreted as the effect of a flexibility-enhancing reform of an average size (reducing the indicator by 0.2 points). 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. Figures reported in Panel B are obtained from a specification controlling for changes in wage and salary employment and the share of the low-educated in total hours worked. Confidence intervals are obtained by clustering errors on countries and industries.

Source: OECD estimates based on EU KLEMS and the OECD Employment Protection Legislation Database.

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theoretical work, one could expect to find a positive long-run impact of dismissal regulations on either employment or wages in countries where these regulations are less stringent. However, no evidence supporting this theory is found, when heterogeneous effects of EPL for regular contracts between high and low-EPL countries are included in the empirical model considered here. If anything, there is some weak evidence that the effect of reforms on wages becomes stronger as regulation decreases (see OECD, 2016a).

Figure 3.7. Long-run labour market effects of flexibility-enhancing EPL reforms
 Estimated business-sector effects of a 0.2-point reduction in the EPL indicator for individual dismissals



Note: The figure reports point estimates and significance of the level of the strictness indicator for employment protection legislation (EPL) for regular contracts in the non-agricultural/non-mining business sector, obtained from difference-in-difference estimators. Estimates are normalised by multiplying them by the average annual fall in the EPL indicator computed over the sample of negative changes in that indicator. Thus, they can be interpreted as the estimated long-run impact of a reform of average size. 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 effect on employment in a hypothetical industry whose US dismissal rate would be equal to or lower than the first quartile of the distribution. Reported impacts are in percentages for employment and wages and percentage points for the share of the low educated. ***, ** denote significance at the 1%, 5% level, respectively, obtained by clustering errors on countries and industries.

Source: OECD estimates based on EU KLEMS and the OECD Employment Protection Legislation Database.

StatLink  <http://dx.doi.org/10.1787/888933384723>

In an economic downturn job losses are already widespread, often implying much financial hardship for households (see e.g. Venn, 2011). Assessing whether flexibility-enhancing reforms of dismissal regulations amplify job destruction when they are implemented during downturns is of key importance for policy-makers in order to evaluate the best time for enacting reforms. On the one hand, standard adjustment-cost models would suggest that the share of unprofitable jobs that survives only because of high firing costs is larger in downturns, leading to greater immediate job destruction when these costs are lifted (see e.g. Cahuc and Zylberberg, 2004; and Cacciatore et al., 2016 for a recent contribution). On the other hand, the number of firms at risk of bankruptcy soars in bad times (see e.g. OECD, 2013, Chapter 4). In this case, high dismissal costs and, in particular, binding restrictions on collective dismissals are likely to result in elevated rates of firm destruction in downturns, since in most countries firms pay no or lower dismissal costs if redundancies are due to firm closure. As a consequence, reducing dismissal costs in downturns might end up lowering the number of jobs that are destroyed, even if it increases the frequency of dismissals conditional on firm survival.

Additional results obtained by extending the models estimated in this section show that flexibility-enhancing EPL reforms appear to result in larger and longer-lasting short-run employment losses when they enacted in downturns, rather than during upturns.³⁵ The specifications underlying Figure 3.5 have been re-estimated after including interaction terms between EPL reform dummies and the year-on-year change in the output gap, measured at the time when the reform was implemented (see OECD, 2016a, Table 3.A2.2).³⁶ Estimation results suggest that employment contracts temporarily following

flexibility-enhancing EPL reforms that are enacted at all stages of the business cycle, but the estimated losses are smaller in upturns and just miss being significant when reforms are undertaken at a time when the output gap is improving by one percentage point (Figure 3.8, Panel A).³⁷ By contrast, a reform implemented when the output gap falls by an equal magnitude – that is in a downturn – induces an employment contraction that not only is twice as large, as in upturns, but also persists for at least two years before becoming statistically insignificant (Figure 3.8, Panel C).³⁸ Taking estimates at face value, if reforms are undertaken when the output gap is falling by 1 percentage point, business-sector wage and salary employment is estimated to be 0.7% lower two years after a reform of average depth than it would have been in the absence of the reform. Moreover, the estimated recovery path following the employment trough is estimated to be quite flat, whereas it is steeper for reforms enacted during an economic upturn.³⁹ Finally, with a stable output gap – representing approximately the 4th decile of the distribution – flexibility-enhancing reforms of dismissal legislation induce statistically significant but short-lived employment losses (Figure 3.8, Panel B), quite similar to the baseline case (cf. Figure 3.5).⁴⁰

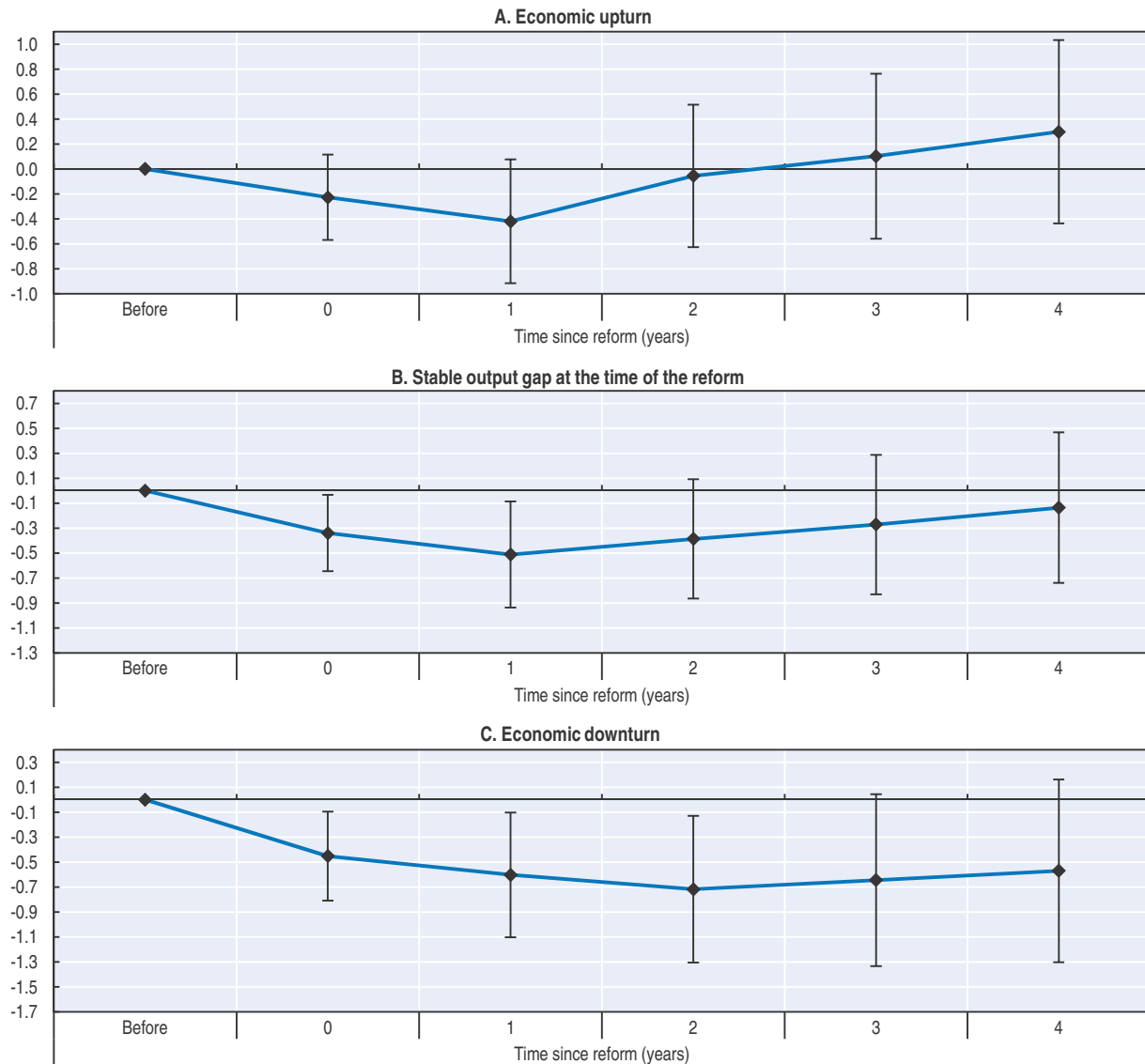
Interactions between the reform indicator and the level of the output gap (instead of its annual change) at the time of the reform were also included in a separate specification but they always turned out insignificant. This suggests that whether the economy is contracting or expanding matters more than the position with respect to potential output as regards short-time effects of EPL reforms. To put it another way, implementing reforms when the economy is starting to recover, despite being still in a situation of high cyclical unemployment, seems less likely to yield significant adverse effects on employment in the short-run than reforms implemented when the output gap is higher but falling.⁴¹

The evidence presented up to here is consistent with the idea that firing costs induce employers to hoard labour in bad times. That is, firms will retain some workers in jobs that, in the absence of regulation, would be terminated when the firm is hit by a negative shock and then replaced when demand perspectives improved. In dual labour markets, however, where fixed-term contracts can be used in a relatively flexible way and the gap in termination costs between open-ended and fixed-term contracts is large, employers have a strong incentive to use fixed-term contracts for positions that become uneconomical in downturns or when the firm is hit by a negative idiosyncratic shock. In fact, there is evidence that the larger the share of temporary contracts in an economy the higher the rate of separation and, in downturns, the greater the job destruction rate (see e.g. OECD, 2012; and Bassanini and Garnero, 2013). In the long run, flexibility-enhancing EPL reforms affecting regulations for regular, open-ended contracts are typically found to reduce the dualism of these labour markets (see e.g. OECD, 2010, 2014b; and Lepage-Saucier, Schleich and Wasmer, 2013). But, this type of reform can be expected to have only a limited impact on job destruction in the short-term, in dualistic economies, since temporary contracts are likely to be used to fill volatile positions and the incentive to terminate these contracts is unaffected by the reform.⁴²

Estimation results suggest that the impact of flexibility-enhancing EPL reforms is insignificant in countries where the share of employees with a fixed-term contract is high (Figure 3.9).⁴³ For example, the employment impact of these reforms is estimated to be only marginally significant when this share is at the sample median (10.35%). By contrast, for a share of fixed-term contracts around 15%, the cumulated employment impact of these reforms is estimated to be close to 0 immediately after the reform and already higher than what would have occurred without policy action two to three years later, albeit not

Figure 3.8. **Flexibility-enhancing EPL reforms and employment in different stages of the business-cycle**

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



Note: The figures report point estimates and 90%-confidence intervals of the cumulative effect of changes in employment protection legislation (EPL) for regular contracts on average wage and salary employment in the non-agricultural/non-mining business-sector, obtained from difference-in-difference estimators, with levels before the reform normalised to 0. Economic upturn (economic downturn) stands for a scenario in which the output gap was growing (falling) by 1 percentage point at the time of the reform. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases and 0 otherwise. They can therefore be interpreted as the effect of a flexibility-enhancing reform of an average size (reducing the indicator by 0.2 points). 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. 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.

Source: OECD estimates based on EU KLEMS and the OECD Employment Protection Legislation Database.


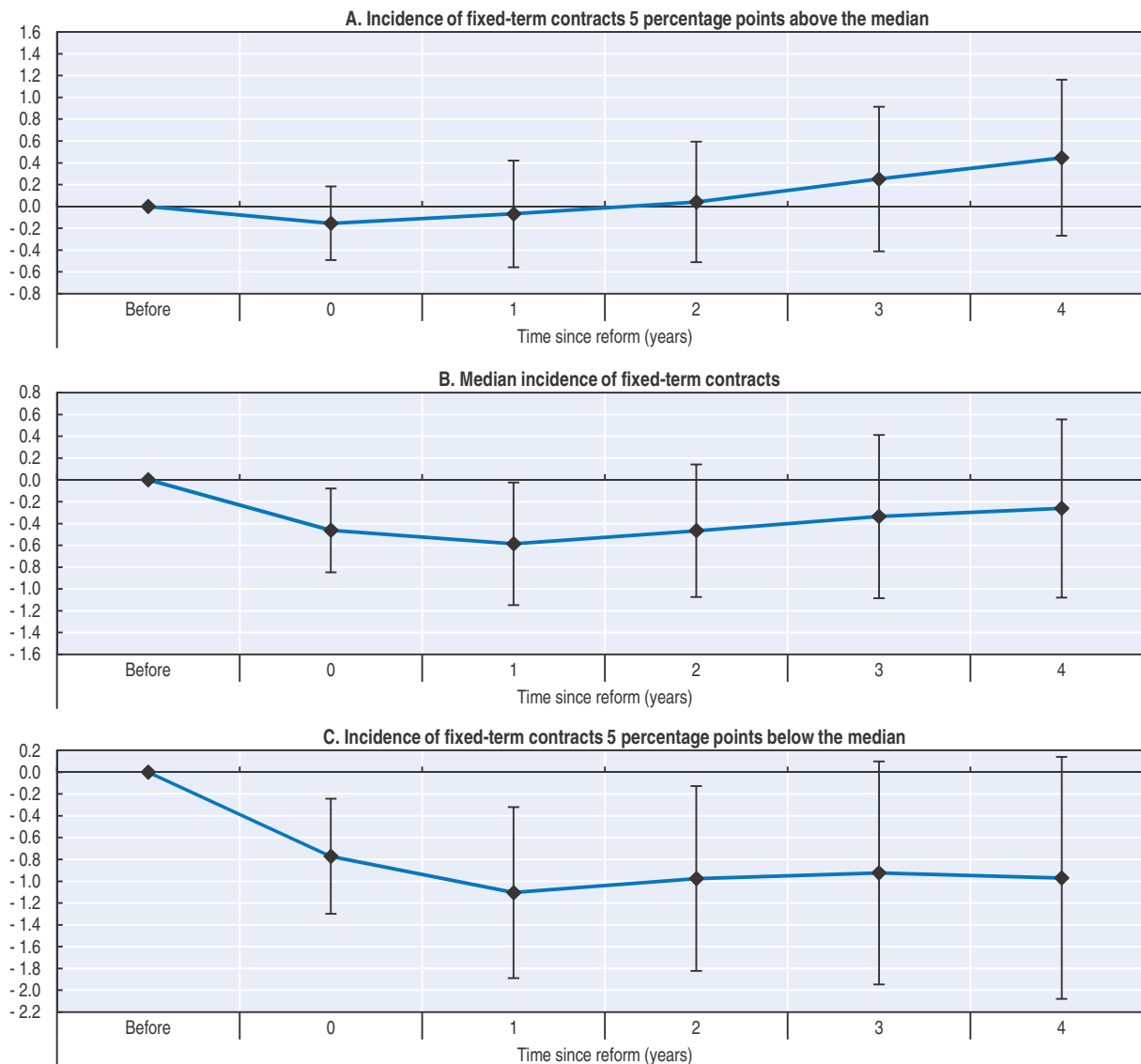
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Figure 3.9. **Incidence of fixed-term contracts, flexibility-enhancing EPL reforms and employment**

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



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

Source: OECD estimates based on EU KLEMS and the OECD Employment Protection Legislation Database.

StatLink  <http://dx.doi.org/10.1787/888933384743>

significantly so. In addition, in these more highly-dual labour markets, reforms of dismissal legislation appear to benefit low-educated workers, whose share in total hours is estimated to become quickly higher than would have occurred without the reform (Figure 3.10). As these workers are typically over-represented in fixed-term contracts and are highly-exposed to firm-specific shocks,⁴⁴ this finding tends to confirm that in dual labour markets the boost to hiring from EPL reforms tends to offset any hike in job destruction in the immediate aftermath of the policy change for the workers who are most at risk of precarious employment.

The short-term effect of reforms lowering dismissal costs on the level and composition of business-sector employment appears to be much more adverse in labour markets characterised by low incidence of temporary contracts.⁴⁵ In a country with a share of fixed-term contracts 5 percentage points below the sample median, a flexibility-enhancing reform of EPL for regular contracts of historically average depth is estimated to induce a contraction of wage and salary employment in the business-sector of up to 1.1% one year after the reform, before recovering very slowly (Figure 3.9, Panel C). Moreover, in this case, EPL reforms do not appear to have any significant effect on the skill mix of employment (Figure 3.10, Panel C).

The evidence presented in this subsection suggests that flexibility-enhancing reforms of EPL for regular contracts tend to induce short-term employment losses. These losses, however, appear to be smaller if not insignificant when reforms are implemented in booms and especially when they are undertaken in countries with a significantly dual labour market. This latter finding is remarkable in the sense that countries with dual labour markets are also those that can expect the greatest long-run benefits from these reforms, due to their impact in reducing the relative use of fixed-term contracts. The next subsection supplements this cross-country evidence by looking at a few recent reform episodes while making use of complementary identification strategies. These country studies also allow to analyse the short-term benefits of the reforms, (in particular, their effectiveness in reducing dualism).

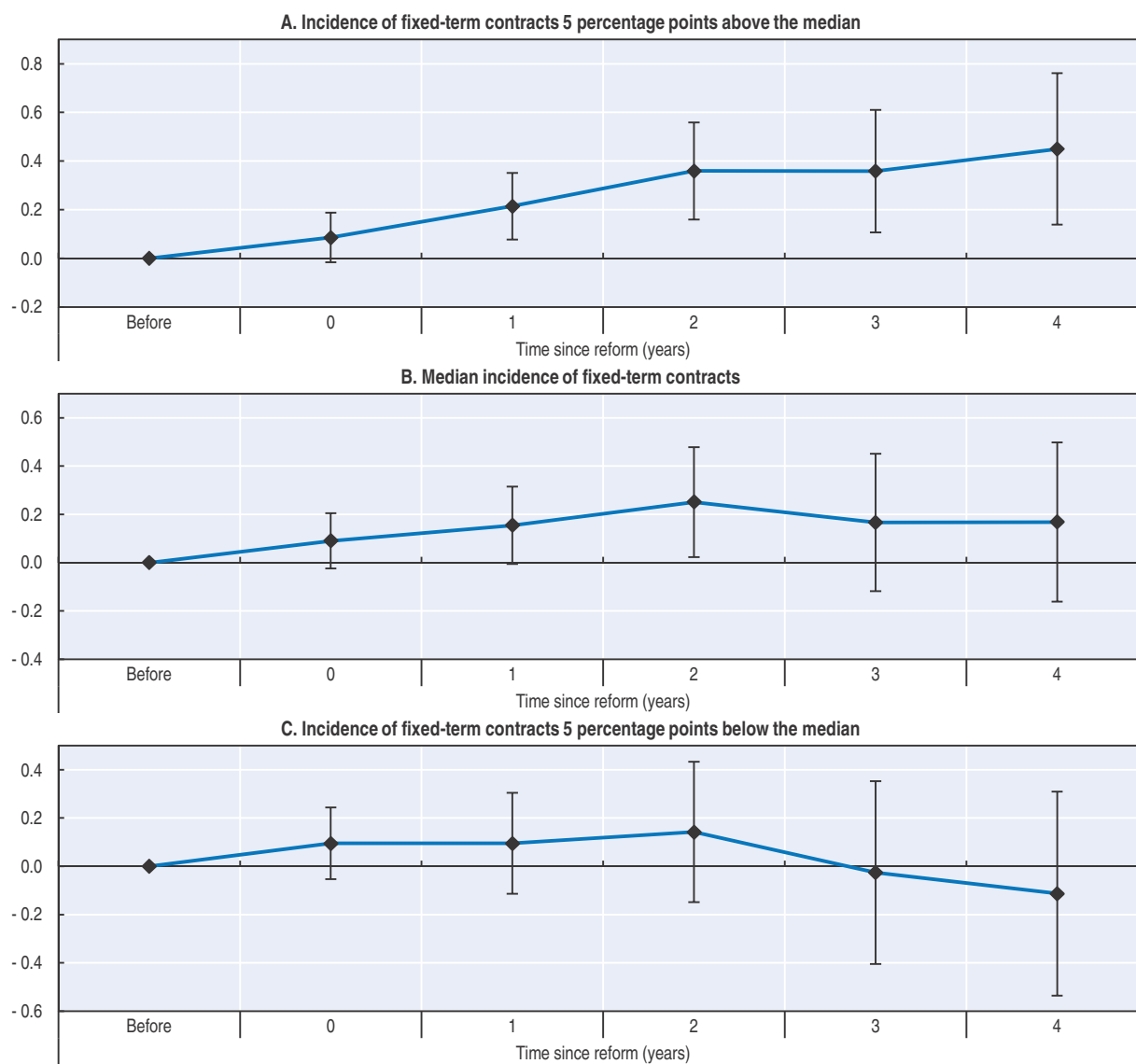
Short-run labour market effects of EPL reforms: Evidence from three country studies

This subsection studies the three, recent labour market reforms implemented in Estonia (July 2009), Spain (February 2012) and Slovenia (April 2013). Although there were important differences between these reform packages, EPL liberalisation for regular contracts was a key pillar of all of them (see Box 3.4). In the case of Estonia and Slovenia, complementary reforms in other areas played a minor role. However, in the case of Spain EPL reforms were accompanied by a simultaneous decentralisation of collective bargaining and measures allowing employers to achieve greater internal flexibility so as to avoid redundancies (e.g. by adapting hours worked, wage and working conditions). In all three cases, the change in EPL for regular contracts was large in historical perspective.⁴⁶

In terms of the analysis of their short-run impact, the advantage of the three reform episodes considered in this subsection is that all or most of the new regulations entered in force at a single date, with subsequent changes being relatively small and by and large in the same direction. This temporal pattern allows the identification of the effects using a regression discontinuity approach (see Box 3.5). By contrast, key framework conditions, as identified by the previous subsection, differed across these countries at the time the reforms were implemented. In fact, they were introduced just after the onset of a large


Figure 3.10. **Incidence of fixed-term contracts, flexibility-enhancing EPL reforms and low-skilled employment**

Estimated cumulative change in the share of low-educated workers in total hours worked in the business-sector up to four years following the reform, in percentage points



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

Source: OECD estimates based on EU KLEMS and the OECD Employment Protection Legislation Database.

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Box 3.4. Recent EPL reforms in Estonia, Slovenia and Spain

In *Estonia*, a new Employment Contracts Act came into force on 1 July 2009, in the middle of a sharp GDP contraction. Notice periods were shortened and made more dependent on job tenure. Moreover, severance pay was significantly reduced, with some additional compensation being provided by the Estonian Unemployment Insurance Fund (but with no upfront cost for employers at the time of dismissal). Last but not least, reinstatement in the case of unfair dismissal was made conditional on the mutual agreement of the parties while compensation was reduced to a maximum of three months wages, except in exceptional circumstances. The only additional significant policy change brought about by the reform was an increase in employers' contributions to the Unemployment Insurance Fund from 0.9% to 4.2% of the gross wage.

The *Spanish* labour market reform was approved by the government in 12 February 2012. Substantial changes were introduced with respect to dismissal legislation. 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 and that the employer does not have to prove 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. Prior to this change, employers often exercised the option to declare a dismissal unfair and pay upfront the corresponding compensation, so as to close the procedure and avoid paying interim wages. Indeed, this was the most commonly-used dismissal mechanism by employers before the reform rendered it obsolete. Finally, the reform eliminated the requirement that employers obtain administrative authorisation for collective redundancies. The reform of EPL was also accompanied by a large reform of collective bargaining which allowed increased flexibility on the intensive margin. In particular, a greater priority was given to collective bargaining agreements at the firm level over those at the branch or regional level and firms were allowed greater latitude to opt-out from a collective agreement and adopt measures to enhance internal flexibility so as to limit job destruction. In addition, the reform limited the extension of collective bargaining agreements to a maximum period of one year after their expiration in the absence of agreements on their renewal.

A new Employment Relations Act entered into force in *Slovenia* on 12 April 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. The reform was accompanied by some changes as regards temporary contracts. In particular, it is no longer possible for employers to hire a series of workers on fixed-term contracts to fill the same post for a cumulative period of more than two consecutive years. In addition, the reform has imposed a maximum quota on temporary-work-agency employment in the user-firm, if fixed-contracts are used. Unemployment insurance contributions are no longer paid for the first two years after hiring a worker on an open-ended contract, while they were increased for fixed-term contracts.

Source: OECD (2013, 2014b); Malk (2013).

Box 3.5. Estimating the impact of EPL reforms using regression-discontinuity models

The estimation strategy followed in this subsection identifies the joint effect of all the provisions included in each reform by comparing labour market performance before and after the date when the reform took effect. Two performance variables are examined: the standardised unemployment rate and the share of open-ended contracts in new contracts (new hires plus conversions). The key identification assumption is that, conditional on control variables included in the model, labour market performance evolves in a relatively smooth way, so that any discontinuous jump in performance can be attributed to the labour market reform (and other institutional changes occurring simultaneously). In order to properly isolate the effect of the reform from that of the business-cycle (which is key for the validity of the smoothness assumption), the estimation models include a number of aggregate covariates and, most importantly, polynomial time trends up to the 5th order. Following standard practice (see e.g. Imbens and Lemieux, 2008; and Card and Lee, 2008), polynomial trends are allowed to differ before and after the reform). The general regression-discontinuity model, which is estimated on monthly data, can be written as:

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

where P is the performance variable (unemployment rate or share of permanent contracts) 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, and Greek letters stand for parameters to be estimated, except for ε , which represents a standard error term. The sample window covers five years before the reform and two years after.^a Y is a vector of aggregate confounding factors that include the changes in the logarithms of the industrial production and real turnover in the retail sector,^b when the unemployment rate is the dependent variable, and the level and change in the unemployment rate as well as the share of youth and older workers in new contracts, when the share of open-ended contracts is the dependent variable.

The parameter of interest is δ . A significant estimate for this parameter suggests a significant impact of the reform. In order to account for the possibility that the effects of the reform are short-lived, an additional dummy is included that takes value 1 one year after the enforcement date in certain specifications.

Misspecification of the empirical model might cause a discontinuous shift in performance around the date of a reform even when this shift occurs before the reform (and cannot therefore be attributed to it). To validate the empirical model, in the light of this possibility, placebo tests are run by fictitiously setting the value of R to some date preceding the reform (but sufficiently close to it). If discontinuous shifts in performance are really induced by the reform, then no effect should be found at these earlier dates. This is the case for all the results discussed in this subsection, where placebo tests are run by anticipating the date of the reform by three months (see OECD, 2016a).

A second issue concerns possible manipulations around the threshold. For example, if the reduction in firing costs were anticipated, employers could delay firing to the post-enactment period in order to take advantage of the new rules. As an additional robustness check, baseline models are re-estimated by excluding from the sample a three-month window centred on the reform date. While this appears a sufficiently long period in the case of Spain,^c it could remain too short in the case of Estonia and Slovenia. In Slovenia the elements of the bill concerning the EPL reform were made public in June 2012, although the final approved text was much different from the initial bill. In Estonia, the new draft of the Employment Contracts Act was made public in the first half of 2008. The results presented in this subsection are however robust to excluding the period from June 2012 to May 2013 for Slovenia and from July 2008 to July 2009 in Estonia, which suggests that these findings are not invalidated by manipulation issues.

- a) Standardised unemployment rates are from the OECD Labour Force Statistics. Industrial production and retail turnover are from national statistical offices (Eurostat in the case of Estonia). The shares of open-ended contracts, youth and older workers in new contracts are from national administrative sources (SEPE for Spain as well as SRDAP and IMAD for Slovenia).
- b) These indicators are lagged three months to take into account lags between output shocks and employment effects (see e.g. OECD, 2012). The three-month window was chosen because that maximised the significance of these variables.
- c) The details and breadth of the Spanish reforms were never mentioned in the programme of the party that won the November 2011 elections and were not made public until well after the inaugural address that the Prime Minister gave in front of the parliament at the end of December 2011 (see OECD, 2014c for more details). It is therefore reasonable to assume that if threshold manipulation occurred, that is if firms postponed certain choices until the approval of the reform, this phenomenon concerned, at worst, only the period January-March 2012.


Figure 3.11. **Incidence of fixed-term contracts in total wage and salary employment and new hires**

Percentage of wage and salary employees with a fixed-term contract, 2006-07 and 2010-11



Note: Estonia, Slovenia and Spain are indicated in black.

Source: OECD calculations based on OECD Labour Force Statistics Database and EU LFS microdata.

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downturn in Estonia, while they were undertaken at or close to the crisis trough in Slovenia and Spain. Moreover, Estonia, on the one hand, and Slovenia and Spain, on the other hand, represented two extremes in terms of labour market dualism before the reforms: Estonia was one of the countries with the smallest share of fixed-term contracts in the OECD, while Slovenia and Spain were close to the top of that distribution, both in terms of stocks and as regards hiring patterns (Figure 3.11).

Table 3.1 presents the estimated average unemployment effect of the reforms in the first two years as obtained from regression-discontinuity models. Conditional on observable controls and a 5th order polynomial time trend, the Spanish reform is estimated to have had no short-term consequences on unemployment. By contrast, the reforms in Slovenia and Estonia appear to have been associated with an increase in the unemployment rate of at least one half of a percentage point (representing at least a 5% increase in unemployment).⁴⁷ Most of the increase in unemployment was concentrated in the first year of implementation. Indeed, in both cases, the post-reform unemployment hike is estimated to have become statistically insignificant in the second year following the reform.⁴⁸

Results from regression-discontinuity models such as those presented in Table 3.1 must be taken with much caution, however, since, by design, estimated coefficients capture the effects of all other changes occurring in the same month of the reform, provided that they are not controlled for by observable confounding factors. Moreover, standard errors in Table 3.1 are remarkably large suggesting that these models deliver relatively imprecise estimates, particularly in the case of Estonian and Spanish reforms.⁴⁹


In the case of Estonia, however, additional evidence can be obtained by considering individual data from the European Labour Force Survey and using other Baltic countries as a control group, as suggested by Malk (2013).⁵⁰ In particular, Lithuania appears to be a

Table 3.1. Recent EPL reforms and unemployment
 Estimated average effect within two years from the reform in percentage points

	Estonia	Slovenia	Spain
Estimated average effect (% points)	1.92*** (3.29)	0.55* (1.88)	0.08 (0.13)
Observations	84	84	84
R-squared	0.995	0.990	0.997

Note: The dependent variable is the standardised unemployment rate. Estimates based on regression-discontinuity models fitted on monthly data. Each specification controls for the three-month-lagged changes of the industrial production and retail turnover indexes, a 5th order polynomial time trend (heterogeneous between the pre- and post-reform period) and month dummies. Robust t-statistics are in parentheses. ***, **, * statistically significant at 1%, 5% and 10% levels, respectively.

Source: OECD calculations based on *OECD Labour Force Statistics Database*, and aggregate time series from Eurostat, Instituto Nacional de Estadística (INE) and Statistical Office of the Republic of Slovenia (SORS).

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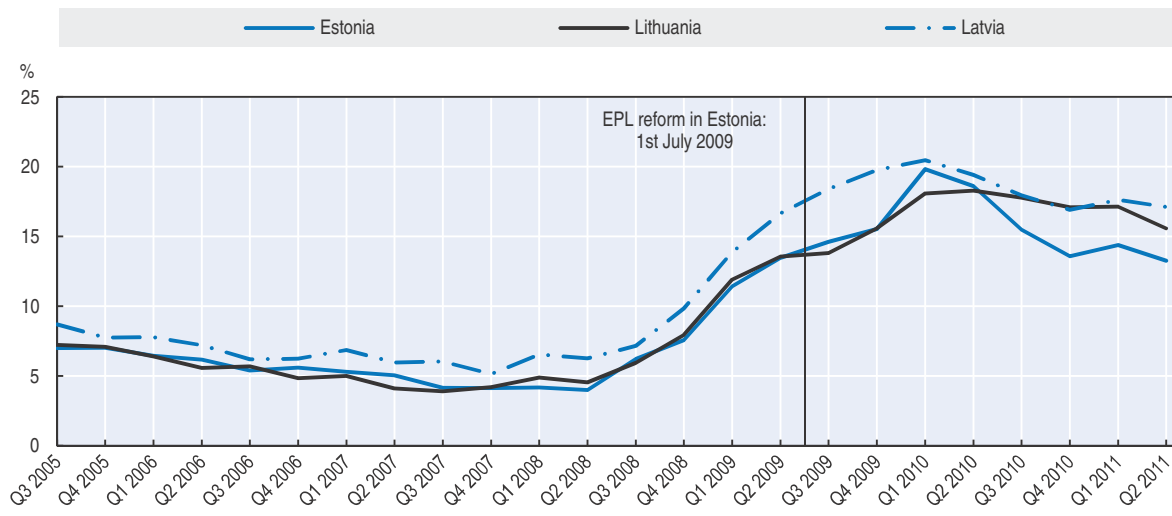
suitable control group: both countries are small open-economies with the same trading partners; they display a similar evolution of real GDP, industrial production and retail turnover before and after July 2009 (see OECD, 2016a, Figure 3.A2.5); before the Estonian reform they were characterised by very similar trends in unemployment (Figure 3.12) as well as stocks and flows of temporary contracts (see Figure 3.11 above); and no significant changes in labour market policies and institutions occurred in Lithuania in this period. This suggests that different unemployment performance after the Estonian reform could be cautiously attributed to that policy intervention.

A simple comparison of the time series of the unemployment rates in the Baltic States after July 2009 suggests that unemployment did rise faster in Estonia in the first year after the reform, when it was 0.7 percentage points above the Lithuanian one on average, with a peak reached in the first quarter of 2010. After that peak, the Estonian unemployment rate went down more rapidly than in the other Baltic countries, and two years after the reform it was lower than in both Latvia and Lithuania.

However composition effects and confounding factors might be in play. In particular, despite many similarities in the demographic structure of the labour markets of the two countries (see e.g. Malk, 2013), the Estonian labour market is more open to immigrants (with 14% of employment being foreign born in 2009 against only 4% in Lithuania). As immigrants are often at higher risk of unemployment in recessions (see e.g. OECD, 2015a), not controlling for this factor could overstate the adverse effect of the Estonian reform. On the other hand, the Lithuanian business-cycle appears to lag slightly behind the Estonian one (see OECD, 2016a, Figure 3.A2.5), which could instead understate the effect of the reform. In order to overcome these issues, a probit model is estimated on the joint sample of the two countries in which the probability of being unemployed in a given month is a function of a large set of individual and aggregate covariates and is allowed to diverge between Lithuania and Estonia in the aftermath of the Estonian reform.⁵¹ This difference-in-difference model suggests that the Estonian reform was associated with an average 1.5-percentage-point increase in the probability of being unemployed – significant at the 5% level – in the two years following the reform (see OECD, 2016a, Table 3.A2.9), a figure strikingly close to that estimated with regression-discontinuity aggregate models (cf. Table 3.1 above).⁵²


Figure 3.12. **Evolution of the unemployment rate in the Baltic countries**

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



Note: The vertical line indicates the date of enforcement of the Estonian labour market reform (1 July 2009, that is at the beginning of Q3 2009).

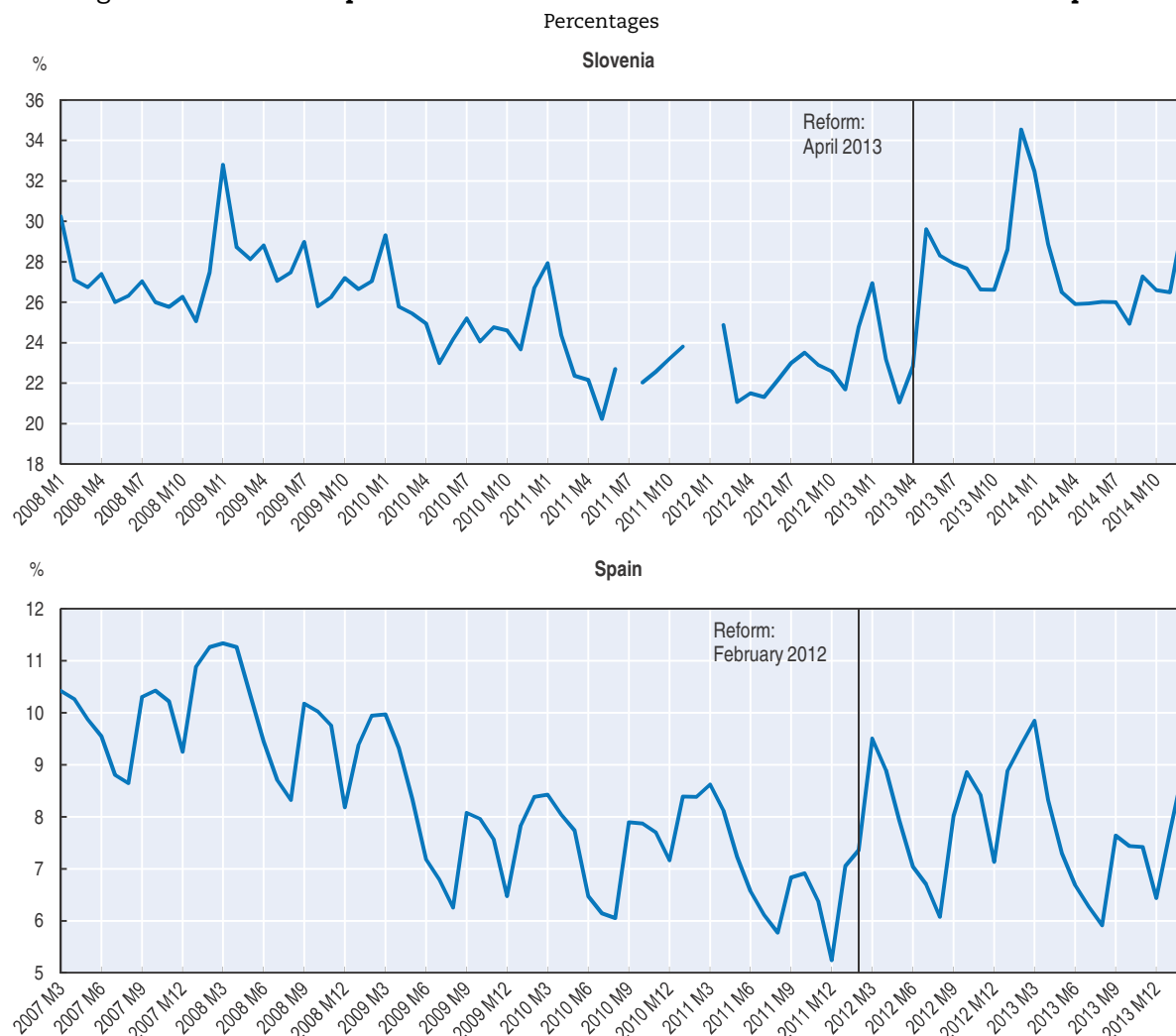
Source: OECD calculations based on quarterly EU Labour Force Survey microdata.

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Overall the estimates presented in this subsection appear consistent with the findings emerging by the estimation of industry-level difference-in-difference models presented above and tend to confirm that flexibility-enhancing EPL reforms may induce some short-term employment losses. A crucial question is, therefore, how fast benefits from these reforms materialise. Empirical evidence available in the literature suggests that benefits in terms of productivity growth may take time to unfold fully (see e.g. Autor, Kerr and Kugler, 2007; and Bassanini, Nunziata and Venn, 2009). By contrast, it seems natural to expect that, by reducing the gap in termination costs between open-ended and temporary contracts, flexibility-enhancing reforms should immediately raise the share of the former in total hiring (see e.g. Lepage-Saucier, Schleich and Wasmer, 2013).


Administrative data available for Spain and Slovenia shed some light on the latter issue by looking at the monthly evolution of new contracts (including any change of contracts with the same employer). Visual examination of the raw time series suggests that in both countries the reforms stopped (and possibly managed to reverse) the downward trend in the share of open-ended contracts in new contracts (Figure 3.13).⁵³

These reforms occurred, however, in a period of large economic fluctuations. This suggests that visual inspection of the time series should be handled with care. For this reason, regression-discontinuity models similar to those estimated for unemployment were fit by replacing the dependent variable with the share of open-ended contracts and adjusting the list of confounding factors (see Box 3.5). Baseline estimates suggests that, in the two years following EPL reforms, the average share of open-ended contracts in new contracts increased by 10.8 and 3.1 percentage points in Slovenia and Spain, respectively (Table 3.2). In both cases the increase amounted to almost 50% of the share of open-ended contracts before the reforms – a large impact in economic terms.⁵⁴ Moreover, the analysis reveals that this dualism-reducing effect was already sizable in the first year.⁵⁵

Figure 3.13. **Share of permanent contracts in new contracts in Slovenia and Spain**

Note: The vertical lines indicate the date the labour market reforms came into effect. The Slovenian data exclude groups of ten Social Security registrations with the same employers on the same day. Data for July and December 2011 as well as January 2012 were excluded from the figure, since administrative changes implied a re-registration of a large number of existing contracts.

Source: OECD calculations based on data from Servicio Público de Empleo Estatal (SEPE), Statistični register delovno aktivnega prebivalstva (SRDAP) and Institute of Macroeconomic Analysis and Development (IMAD).

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These findings are robust to excluding observations close to the reform date (see OECD, 2016a, Table 3.A2.8).⁵⁶ Moreover, as in the case of unemployment-rate regressions, placebo experiments in which the date of each reform is fictitiously anticipated by three months yield insignificant results, confirming that the observed shift in the share of open-ended contracts did not occur before the reform. Interestingly, a more disaggregate analysis suggests that 80% of the impact of the Spanish reform is due to new hires and only 20% is due to conversions.⁵⁷


Overall, these findings suggest that extensive EPL reforms, such as those considered in this subsection, tend to quickly modify the hiring patterns of employers by strengthening the relative attractiveness of open-ended contracts with respect to temporary contracts. In other words, the benefits in terms of reducing dualism emerge very rapidly. However, this

Table 3.2. **Recent EPL reforms and share of permanent contracts in new contracts**
 Estimated average effect within two years from the reform in percentage points

	Slovenia	Spain
Estimated coefficient (% points)	10.82*** (6.46)	3.12*** (6.41)
Observations	63	84
R-squared	0.932	0.978

Note: The dependent variable is the share of permanent contracts in new contracts. Estimates based on regression-discontinuity models fitted on monthly data. Each specification controls for level and changes in the standardised unemployment rate, the share of youth and older workers in new contracts, a 5th order polynomial time trend (heterogeneous between the pre- and post-reform period) and month dummies. The specification for Slovenia excludes data for July, December and January of each year. Robust t-statistics are in parentheses. *** statistically significant at the 1% level.

Source: OECD calculations based on OECD *Labour Force Statistics Database* and data from Servicio Público de Empleo Estatal (SEPE), Statistical Office of the Republic of Slovenia (SORS), Statistični register delovno aktivnega prebivalstva (SRDAP) and Institute of Macroeconomic Analysis and Development (IMAD).

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encouraging evidence must be considered in combination with the findings presented above suggesting that flexibility-enhancing EPL reforms might also engender temporary declines in total employment. Indeed, the results reported in this section are remarkably consistent in suggesting that non-negligible employment contraction (and unemployment expansion) is likely to follow EPL liberalisations – despite the variety of methodologies employed. This is especially the case if reforms are undertaken in the middle of a downturn (before the crisis trough) and in less dual labour markets.⁵⁸ Yet, the comparison of the Spanish and Slovenian experiences, where reforms were undertaken at approximately the same point of the business cycle and in a similar context in terms of dualism, suggests that complementary reforms can be put in place that limit short-term adverse effects on employment. Indeed, the Spanish EPL reform was accompanied by a major reform of collective bargaining. The next section examines reform design and complementary policy actions that are likely to minimise or even offset potential short-term costs of flexibility-enhancing structural reform (and/or limit their short-term negative effects on individual welfare).

3. Designing structural reforms that limit short-term costs

The results shown in this chapter suggests that the expansionary stage of the business cycle is the best time to implement reforms of product and labour market regulations, at least as regards minimising their possible short-run employment costs. However, there are political economy reasons why many reforms take place during economic downturns and this heightens the importance of strategies to minimise the associated short-run costs.

There is considerable evidence that structural reforms – defined in the broadest sense to also include, e.g. fiscal, trade and capital-market reforms – are typically undertaken in bad economic times (e.g. Drazen and Easterly, 2001), when it is easier to form large coalitions favouring policy changes. This occurs for several reasons. On the one hand, crises increase the perception that there is no alternative to reforms. As Dani Rodrik put it, “reform naturally becomes an issue only when policies are perceived not to be working. A crisis is just an extreme case of policy failure” (Rodrik, 1996, p. 27). On the other hand, severe deterioration of economic performance may lead interest groups to accept more easily reforms requiring them to give up some of their advantages, either in exchange for greater long-run benefits or under the social pressure of other groups whose conditions are worsening (see e.g. Drazen, 2000). In the words of John Williamson, “a sufficiently acute

crisis may also create a consensus that the old order has failed and needs to be replaced, leading individuals and groups to accept that their special interests need to be sacrificed (along with those of other special interest groups) on the altar of the general good” (Williamson, 1994, p. 19).

The argument that costly reforms are more easily implemented in bad times might also be applied to liberalisations of dismissal regulations, insofar as the share of protected workers shrinks in downturns, while the number of workers who are unemployed or precariously employed expands and this latter group increasingly demands policy action. This argument is also likely to apply to the case of rent-reducing reforms affecting specific industries to the extent that policy-makers can then argue that a more equal sharing of the costs of the economic crisis requires measures to reduce the rents enjoyed by specific firms and workers. However, the political equation is complicated. For example, the demand for protection of insiders is also likely to become more intense in bad times, as unemployment risk increases, and this can induce governments to postpone costly reforms. In practice, there is some evidence that EPL reforms become more frequent in economic downturns, although many of them appear to have been undertaken only close to or after recession troughs.⁵⁹ By contrast, there is no evidence that reforms of barriers to entry in network industries are undertaken more frequently in bad than in good times.⁶⁰ Moreover, comparing the reform patterns in retail trade and professional services in recent years with those of the pre-crisis period suggests that reforms in these sectors are also no more frequent in bad times than in good times (Koske et al., 2015).

A growing body of macroeconomic research strengthens the case that structural reforms undertaken in bad economic times should be coupled with complementary policy actions to minimise adverse effects. This includes the recent debate in the theoretical literature as regards whether structural reforms should be accompanied by expansionary macroeconomic policy (e.g. Eggertsson and Krugman, 2012; Correia et al., 2013; and Cacciatore et al., 2016). Similarly, recent model-based simulations calibrated to the euro area countries indicate that reforms implemented when monetary policy has hit the zero lower bound would magnify the negative effect of the high uncertainty characterizing downturns on job creation (Eggertsson, Ferrero and Raffo, 2014). However, it is hard to find a consensus for expansionary fiscal policy in a period of mounting government debt. Similarly, in the present context of weak recovery from the deep financial and economic crisis, the margin of manoeuvre for monetary policy can be limited as interest rates are already closed to the zero or even negative. This might require the use of unconventional monetary policy such as expanding the central bank’s balance sheet and channelling liquidity to the real economy (see e.g. Coeuré, 2014).

In these circumstances EPL and PMR reforms could also be accompanied by additional labour market reforms. The remainder of this subsection reviews evidence from recent country experiences to shed light on possible policy packages and reform designs that are likely to reduce or even fully offset short-term costs of EPL and PMR reforms, especially those undertaken in bad economic times.

Scaling-up activation strategies in times of crisis?

During the past three decades, many OECD countries have sought to transform their welfare states by linking benefit systems with services to promote employment, so-called “activation” strategies. Effective activation policies typically combine measures to ensure that jobseekers have the *motivation* to search actively and move quickly to a new job with

actions to expand labour demand and *opportunities* – for example, increasing the range of job vacancies registered with the public employment service (PES) – and interventions to increase the *employability* of those who are less employable – who are typically offered intensive case management and placement services, and/or participation in other programmes such as training or subsidised employment (see OECD, 2015b).

It is often suggested that active measures have little net impact in a recession because the economy is demand-constrained and “there are no jobs”. *A fortiori*, one would be tempted to apply this argument even more forcefully to situations where job losses are increased by specific structural reforms undertaken in downturns. In the recent recession, however, many of the countries with a strong activation approach, such as, for example, Australia, Austria, Norway, Switzerland and the United Kingdom, did not allow unemployment spells to become passive and experienced relatively modest or short-lived increases in unemployment (OECD, 2013, 2015b). There is also some evidence that training has a more positive impact in bad than in good economic times (because of a weaker “lock-in effect” – see e.g. Lechner and Wunsch, 2009; Nordlund, 2009; and Kluge, 2010). This finding is particularly important in the context of structural reforms, since workers who are displaced because of the new policy measures and would not have been made redundant otherwise are likely to require requalification and guidance towards new careers.

Systems that work better in the case of displaced workers are geared around early interventions of the PES, possibly occurring already during the notice period. However, various factors including lack of incentives for both employers and workers can make early intervention ineffective in practice (OECD, 2015c). Providing better incentives to various actors involved in these interventions, including employers and employees may help to address these challenges. On the employer side, sanctions for non-compliance of the legislation concerning advance notice of mass layoffs are used as one way to improve employer incentives to co-operate with public authorities early on. On the worker side, in order to ensure early contact with employment services, several countries have resorted to extending job-search obligations to workers even before the announced date for employment termination. For example, in Switzerland, as part of the required job-search efforts, unemployed workers also need to give proof of job-search activities between dismissal notification and the first interview at the PES to receive unemployment benefits (Duell et al., 2010). A similar preventative approach was adopted in Germany as part of the Hartz reforms, where workers are obliged to register as jobseekers three months before their job ends or, for those with shorter notice, within three days after receiving notice of dismissal (Mosley, 2010). This registration obligation allows the PES to make referrals to vacancies before the first unemployment benefit payment. In Sweden, effective early interventions is achieved through the co-operation of social partners in setting-up specialised institutions (Job Security Councils) that provide re-employment services to workers on notice of displacement and are funded by employer contributions, which has, however, the drawback of increasing the tax wedge (OECD, 2015d).

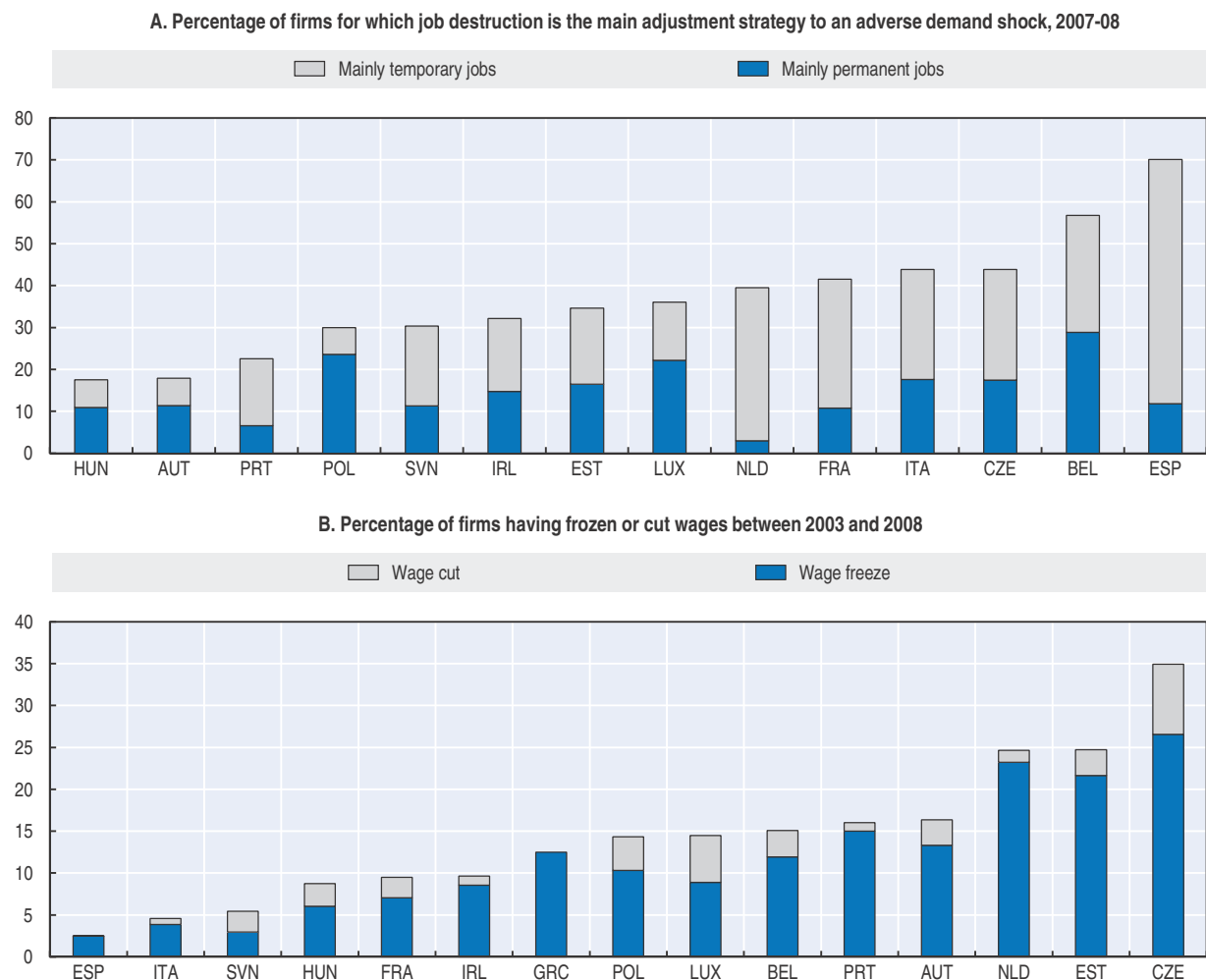
Even though there is some evidence that certain activation programmes that were set up during a recession worked well (see e.g. Michaelides, 2013; Martins and Pessoa e Costa, 2014), there are limits to how rapidly active labour market policies can be set-up or up-scaled when unemployment rises in an economic downturn. The effective functioning of the PES, as well as high-quality training and job creation measures, all depend on having assembled the necessary skilled professionals and infrastructure, such as buildings, equipment and IT systems, where rapid change has an up-front organisational cost. As a

result, scaling up efforts during an adverse shock may be difficult (OECD, 2015b), except when the upscaling in downturns is well planned in advance and fully integrated in the functioning of the system (as in Denmark and Switzerland). Indeed, fine tuning these types of organisational changes in order to make them effective typically takes several years (see OECD, 2013). It is therefore probable that structural reforms have more modest adverse effects where an effective activation strategy is already in place or where specific rapid-intervention packages have been previously set-up in anticipation of liberalisation reforms.⁶¹ However, it is unlikely that combining reforms of product market and dismissal legislation in bad economic times with simultaneous activation reforms would reduce adverse employment effects in the short run.

Reforming collective bargaining: Evidence from Spain


Decentralising collective bargaining and facilitating the possibility for employers to opt-out of higher level agreements in times of crisis could dampen the short-term job destruction induced by PMR and EPL reforms. It has often been argued that centralised or co-ordinated bargaining allows wage-setters to internalise externalities associated with wage increases and may thus deliver better outcomes in terms of average unemployment over the business cycle. However, these bargaining structures may also impede idiosyncratic wage adjustments in times of crisis which can be a major problem if the variance of firm or industry shocks is large. Bargaining at the sectoral (or regional) level often induces cross-sector imitation, pushing wages upwards in boom times and delaying the required wage adjustments in times of crisis. The relative flexibility associated with firm-level bargaining, by contrast, allows a better adjustment of wage growth to firm-level productivity growth, and may thus contribute to saving jobs in bad times insofar as they allow firms to use margins other than employment (e.g. wages, working time or working conditions) to adjust to negative shocks (see e.g. OECD, 1994, 2006; Flanagan, 1999; Haucap and Wey, 2004; and Boeri, 2014).⁶² In practice, two-tier systems combining firm-level and multi-employer agreements are common. In most countries with a two-tier system, however, the so-called “*favourability principle*” implies that firm level agreements are applicable only if they are more favourable to employees than higher-level agreements. The evidence suggests that this setup leaves firms as unable to adjust wages and working hours as their peers in systems covered only by branch agreements (Boeri, 2014).

The 2012 labour market reform in Spain coupled a relaxation of dismissal regulations with greater flexibility in the collective bargaining system (see Box 3.2 above). Before the reform, the latter was essentially a two-tier system with branch and regional agreements dominating firm-level agreements. Derogation clauses were possible, but they were seldom applied since they could be voided by a court ruling. This helps to explain why the Spanish economy was characterised by a strong reliance on employment adjustments to absorb shocks. For example, before the onset of the crisis, the main adjustment strategy to a demand shock for about 70% of Spanish firms was to reduce employment – mainly by suppressing temporary jobs – while this strategy was preferred by only 40% of firms, on average, in other EU countries according to the Eurosystem’s WDN Survey (Figure 3.14, Panel A). In fact, wage cuts or wage freeze were very rarely undertaken by Spanish firms in the five years preceding the crisis (Figure 3.14, Panel B). Evidence from the follow-up WDN surveys show that wage cuts and wage freezes remained much less frequent in Spain than in most other European countries until 2012, despite the severity of the Great Recession in that country (European Central Bank, 2010; and Boeri and Jimeno, 2015).

Figure 3.14. **Adjustment strategies to adverse shocks used by European firms before the crisis**

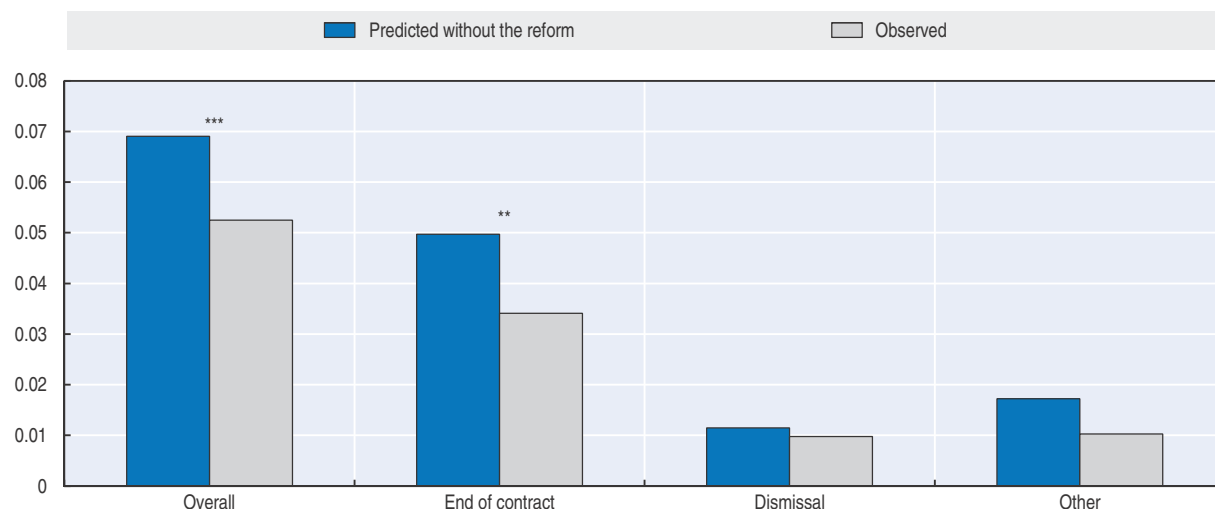
Note: Countries are selected on the basis of available data.

Source: Eurosystem's WDN Survey (Fabiani et al., 2010; Babecký et al., 2009; European Central Bank, 2010).

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The 2012 reform raised the incentives for firms to adopt internal-flexibility measures as an alternative to terminations. In particular, it introduced the principle of the dominance of firm-level agreements over higher-level agreements. It also made it easier for employers to opt-out of collective agreements or to introduce internal-flexibility measures even in the absence of consensus among social partners at the company level, while simultaneously reducing the possibility that these derogations could be voided by a court ruling (see OECD, 2014c for more details). At the same time, the reform simplified firing procedures and reduced the associated costs to employers. As a consequence, the overall effect of the reform package on job losses was *a priori* ambiguous.

The available evidence suggests that the reform resulted in a marked reduction of separations, particularly for temporary workers. Estimates of regression-discontinuity models similar to those used in the previous section show that the average separation rate fell by 24% in the aftermath of the reform (Figure 3.15; see OECD, 2014c, for more details). The aggregate effect is almost entirely driven by contract terminations for temporary workers (which fell by almost one-third), while no impact is found on dismissal rates.

Figure 3.15. **The effect of the 2012 labour market reform in Spain on quarterly separation rates at the establishment level**

Note: The figure shows predicted average establishment-level separation rates in the post-reform period as obtained from the estimation of a regression-discontinuity model on quarterly data for the period 2006-12. “Predicted without the reform” indicates the empirical predictions of what separation rates would have been in the absence of the reform. For each establishment, separation rates are defined as the ratio of separations in a quarter divided by the average of total employment between the start and the end of the period. The asterisks refer to the significance level of the estimated effect of the reform on each separation rate. ***, **, * significant at the 1%, 5% and 10% level, respectively.

Source: OECD estimates on the basis of data from the Encuesta de Coyuntura Laboral (ECL). See OECD (2014b) for the detailed estimation method and results.

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The differential effect on separations by type of contract sheds light on the possible complementarity between EPL and collective-bargaining reforms. In principle, terminations of temporary contracts should not be much affected by an EPL reform lowering the costs of dismissals on permanent contract. Hence, the fall in temporary-contract termination rates can be interpreted as the effect of the measures facilitating internal flexibility as an alternative to job destruction. In turn, this implies that dismissal rates would have significantly increased absent these measures.⁶³ This suggests that, in countries with higher level collective-bargaining regimes, coupling reforms relaxing employment protection with others geared to make collective bargaining more flexible can effectively offset short-term job losses.⁶⁴

Grandfather clauses in EPL reforms

One way to dampen the upsurge of dismissals following EPL reforms is to introduce “grandfather clauses”. This means preserving workers’ accrued entitlements at the date of the reform, for example by applying the new rules only to new hires or allowing workers to choose between their entitlements with the new rules and those with the old rules, but with the old rules applied as if the dismissal occurred on the date the reform was enforced.⁶⁵ From a theoretical viewpoint, this type of reform should have no impact on dismissals since it does not lower the cost of destroying existing job matches. By contrast, new vacancies would become *ex ante* more profitable, since the expected cost of destruction (i.e. in the case of a negative shock) is lower. The combination of these two effects should in principle result in a temporary increase in the number of new hires and employment levels (the so-called “Honeymoon effect”, see Boeri and Garibaldi, 2007). As

time goes by, the share of employees eligible for the old, more protective rules shrinks via attrition, so that the dismissal and separation rates increase and, possibly, employment goes down to the initial level. On the negative side, such a reform is likely to delay the effect of EPL liberalisation in improving overall reallocation and efficiency, by temporarily maintaining inefficient job matches. By preserving the protection level of current insiders, grandfather clauses in EPL reforms are also likely to weaken the initial impact on reducing labour market segmentation.⁶⁶

The limited available evidence suggests that EPL reforms with grandfather clauses indeed allow avoiding the expected short-term negative effects and actually have a small positive impact on employment and hiring on permanent contracts in the short run.⁶⁷ For example, Kugler, Jimeno and Hernanz (2005) find that the 1997 Spanish reform that introduced a new type of open-ended contract with lower protection had, in the first three post-reform years, a positive effect on employment levels of eligible groups with respect to non-eligible ones. Similarly Sestito and Viviano (2016) use data from one Italian region (Veneto) to evaluate the 2015 Jobs Act that introduced a new type of contract for large employers with no right to reinstatement in case of unfair dismissal. This new contract applies to all new hires on open-ended contracts since 13 March 2015. By comparing firm hiring behaviour just above and just below the threshold of eligibility, the authors find that, in the first 9 months of implementation of the new regulations, the share of open-ended contracts in new hires and the rate of conversions from temporary to permanent contracts increased significantly in the Veneto region. Similarly, current work undertaken by the OECD to evaluate the recent reforms of dismissal regulation in Portugal shows, using a difference-in-differences approach, that the large reduction of severance pay, implemented with preservation of accumulated rights by means of three reforms between 2011 and 2013, resulted in an expansion of on-the-job search, hiring and the share of open-ended contracts in new hires, but had no effect on transitions from employment to non-employment (OECD, 2016b).⁶⁸ Overall, these findings suggest that grandfathering of EPL reforms might effectively dampen short-term employment costs. However, more research is needed to investigate the effects on economic efficiency and inequality when this type of clauses is applied.

Sustaining the income of displaced workers: Evidence from the United States

A few countries that reformed dismissal regulations during the Great Recession tried simultaneously to cushion the earnings losses of the displaced workers by raising average income levels during the unemployment spell. They did so by making the unemployment benefit system more universal and, in some cases, raising benefit levels for the lowest-income households.⁶⁹ Whether unemployment benefit generosity should be pro-cyclical, however, is the subject of an intense debate. The answer depends on whether the effects of benefit generosity on individual welfare (through better opportunities of consumption smoothing) and on agents' behaviour (such as recipients' job-search effort and firms' labour-demand) also vary with the cycle.⁷⁰

Most of the recent evidence on these issues is based on the extension of potential benefit duration in the United States during the recent recession (and its phasing-out). Unemployment insurance in the United States is available for up to six months following job loss in normal times in most states, plus 20 additional weeks in states experiencing high unemployment rates. In past recessions, the Congress has frequently authorised additional weeks of insurance on an ad hoc basis. In June 2008, the Congress enacted the

Box 3.6. The US unemployment insurance system and the business-cycle

Since the Social Security Act of 1935, in the United States unemployment insurance (UI) benefits are normally available for 26 weeks [under the joint federal-state Unemployment Compensation (UC) programme]. The conditions for eligibility (e.g. regarding individual work history and wages preceding job loss, availability for work and active job search) vary across states as do the benefit levels, typically amounting to about half of the claimant's pre-separation weekly wage. States provide most of the funding and pay for the actual benefits provided to workers; the federal government pays only the administrative costs.

Normal UI benefits can be supplemented and extended, through a combination of permanent and temporary programmes, during episodes of economic distress. The permanent Extended Benefits (EB) programme, enacted in 1970, provides up to 20 weeks of additional unemployment compensation in states whose unemployment rate is above a specified threshold. Typically, an overall unemployment rate above 8% combined with a 10% increase in the unemployment rate over the previous two years triggers a 20-week extension (a rate above 6.5% is required for a 13-week extension). Normally the federal government and the states split the cost of EB, but the 2009 Recovery Act temporarily authorised full federal funding, which continued through 2013.

In response to the recent Great Recession, Congress further enacted the Emergency Unemployment Compensation (EUC) in 2008. This was a temporary programme further extending the maximum benefit period which was fully funded at the federal level. At its peak, after four tiers of extensions, the EUC provided up to 34 weeks of emergency federal benefits in all states and up to 53 weeks in states with unemployment rates of 8.5% or higher. Overall, between November 2009 and September 2012, individuals in states that met eligibility requirements for EB and all EUC tiers could receive up to 99 weeks of UI payments (26 weeks of regular benefits, 20 weeks of EB, and 53 weeks of EUC). Starting in September 2012, many states had become ineligible for EUC tiers and EB benefits due to declines in their unemployment rates. Moreover, the maximum number of UI weeks available was lowered from 99 to 93. The programme expired in 2013. Since January 2014, no state has had UI benefits available beyond the normal duration.

Source: US Social Security Administration (1997); Isaacs and Whittaker (2014); Whittaker and Isaacs (2015); and Hagedorn, Manovskii and Mitman (2015).

Emergency Unemployment Compensation (EUC) programme, which, in a series of extensions, brought maximum statutory benefit durations to as long as 99 weeks between late 2009 and 2013, when it expired (see Box 3.6).

Recent work focusing on individual job search response to such extensions found only small effects on the duration of unemployment spells (Rothstein, 2011; and Farber and Valletta, 2015). More specifically, these studies found that benefit extensions slightly reduced the exit rate from unemployment, but this largely occurred through increased labour force attachment (i.e. higher incentives to engage in and report active job search, increasing the recorded active population) rather than reduced job finding. Marinescu (2015) showed that this latter finding can be explained (at least in part) by the increased hiring rate per application sent (a "search externality" whereby non-eligible job seekers benefit from the lower job search by benefit recipients).⁷¹

The evidence on the role of labour-demand externalities – which would imply that lower number of applicants for each job and/or higher reservation wage would induce firms to post fewer vacancies – is more controversial. Using random variation in

the application of extension rules in the recent recession,⁷² Chodorow-Reich and Karabarbounis (2016a, 2016b) find that extension of potential benefit duration had only very limited effects on overall unemployment and the reservation wage of the unemployed during the recession. By contrast, Hagedorn et al. (2013, 2015) argue that the extensions had a strong negative impact on job creation. They obtain this result by exploiting the geography of benefit extensions as well as their recent abrupt withdrawal on 1 January 2014. Comparing neighbouring county pairs exposed to different reductions in potential duration (i.e. due to being located in different states) they estimate very large positive effects of the benefit cut on labour demand, concluding that 1.8 million additional jobs were created in 2014. However, this result is contradicted by Marinescu (2015) who finds no effect of the benefit extensions on vacancies posted on a large American online job aggregator. Furthermore, she shows that the border county design cannot recover the causal impact of unemployment insurance on applications and vacancies due to large cross-county spillovers. In particular, she shows that employment and unemployment levels of residents in the smaller county of a pair are more heavily affected by benefit generosity in the larger county than by benefit generosity in their own county, which invalidates Hagedorn et al. (2013, 2015) identification strategy.

Finally, several studies have considered the possibility that the positive effect of benefit generosity on duration might not be exclusively driven by moral hazard. In the presence of imperfect credit and insurance markets, unemployment benefits allow liquidity-constrained unemployed individuals to smooth consumption, thereby increasing welfare while continuing to search for a good job match (see, for example, Chetty, 2008). Using cross-state/over-time variation in unemployment duration and consumption patterns in the United States, Kroft and Notowidigdo (2011) found that the elasticity of consumption levels to benefit generosity increases more during economic downturns than the elasticity of unemployment duration. These results could be affected by omitted cross-state institutional changes, but Schmieder, von Wachter and Bender (2012b) find consistent results exploiting a German age-discontinuity in benefit entitlement on a large microdata sample. In particular, they find that the effect of potential unemployment insurance duration on the length of non-employment spells is at worst slightly negative in bad times, while the effect on the duration of benefit receipt is strongly countercyclical. These findings appear to be due to the fact that benefit exhaustion increases dramatically in slumps and suggest that the liquidity-constraint effect dominates the moral hazard effect during economic downturns.

Overall, the available evidence suggests that extending unemployment benefit programmes in bad times has, at worst, no adverse welfare effects. This suggests that countries characterised by relatively low benefit entitlements (or tight eligibility rules) and undertaking structural reforms in bad times could consider cushioning their short-term effect on displacement by temporarily extending benefit durations and/or enlarging benefit coverage. Such measures are likely to be more effective if coupled with strict enforcement of rigorous job-search requirements to limit moral hazard (as discussed above). Yet, the policy would be costly and, as discussed above, harder to implement in countries with large and mounting government debt.

Conclusions

This chapter investigated the short-term effects of reforms that ease anti-competitive product market regulation and employment protection legislation. The key finding of the

chapter is that, while yielding benefits in the long run, these reforms can entail short-term employment losses. This cost is higher in industries most directly affected by the reforms and when the policies are introduced during downswings. By contrast, the estimated employment losses are much smaller and statistically insignificant when reforms are implemented during upswings. Moreover, reforms of dismissal legislation appear to have no adverse effects in segmented labour markets with a high share of fixed-term contracts – those where this type of reform is likely to be most needed.

While these results suggest that it might be desirable to enact regulatory reforms of product and labour markets at the beginning of a recovery or during an expansionary phase, political-economy considerations may often induce policy makers to make structural reforms during economic downturns, when it is easier to build sufficient political support for action. The chapter discusses the pros and cons of complementary policies that can be put in place to minimise short-term employment costs and/or cushion their impact on the income of workers who lose their jobs – such as activation schemes, reforms of collective bargaining and/or temporary extensions of unemployment insurance. The choice of complementary policies crucially depends on the available resources and on the availability of the necessary infrastructure (particularly regarding effective early interventions during the period of unemployment). In the case of the relaxation of dismissal regulations, grandfathering could be an alternative way of reducing short-run costs, albeit at the price of slowing the beneficial effects of the reform on efficiency and segmentation.

Notes

1. A good insolvency regime should inhibit premature liquidation of sustainable businesses, favour a rapid reallocation of resources in case of bankruptcy and offer bankrupt entrepreneurs the chance for a “fresh start”. According to the World Bank indicator measuring weaknesses in existing insolvency law (see World Bank, *Doing Business Database 2016*) several OECD could better address existing procedural and administrative bottlenecks (including Turkey, Hungary, Poland Greece, Italy, Israel, Spain and Mexico).
2. For a discussion of the case of the European Single Market, see European Commission (2015a).
3. In the European Union, for example, government expenditure on works, goods and services represents around 19% of EU GDP, accounting for more than EUR 2.3 trillion annually (European Commission, 2015b). Distortions to competition can be associated with the several steps and criteria used in the tendering procedures, or deriving from the market power and potential abuses of the public buyer (see Graells, 2015).
4. The base sample covers annual data from EU KLEMS for the period 1975-2007 for three industries (energy, transport and communication) as defined in the ISIC Rev. 3 classification (these industries representing an intermediate level between 1 and 2-digits of that classification). 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. 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 EU KLEMS data with data from the last version of OECD STAN. As this dataset adopts the ISIC rev.4 classification, a mapping has been established by using employment data at the 3-digit level from EU LFS (tested on years for which both classifications are available). Such mapping is however imperfect and breaks in the industry classification can severely alter the estimated short-run dynamics; moreover, the extension likely exacerbated measurement error. Accordingly, in this chapter, the collated sample is used only in sensitivity analyses. The analysis of the effect of barriers to entry mainly focusses on total employment, since reliable EU KLEMS data for dependent employment are not available for most countries before the mid-1980s. Results are however robust to replacing total employment with wage and salary employment as dependent variable.

5. The analysis exploits the ETCR section of the *OECD PMR Database* (see Koske et al., 2015, and the references therein for more details on the data and methodology underlying the *PMR Database*). The ETCR indicators measure the level of regulation in three network industries: Energy (electricity and gas), Transport (air, rail, road transport) and Communications (post and telecommunications). More specifically, this chapter focuses on the sub-indexes capturing legislated entry barriers and vertical integration (when applicable), varying from 0 (lowest regulation) to 6 (highest regulation). For example, in the case of the electricity industry, the indicator of industry-specific entry barriers is the simple average of three sub-indicators concerning third-party access (free, regulated, no access), existence of a wholesale pool and minimum consumption threshold that consumers must exceed in order to be able to choose their electricity supplier. The ETCR indicators have now been computed for a time series spanning the years 1975 up to 2013. Table 3.A3.1 in OECD (2016a) reports the latest available values by network industry. 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.
6. 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. 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 liberalisation directives of the electricity and gas markets adopted since the mid-1990s).
7. Note that the response function plotted in Figure 3.1 does not account for the estimated contemporaneous effects, which might be affected by simultaneity or reverse causality biases. As shown in Figure 3.A1.2 of OECD (2016a), factoring these effects in would make the short-term negative impact of entry deregulation on employment significantly larger.
8. Bassanini (2015) shows that these results are also robust to including additional industry-level confounders such as the growth in intermediate inputs and real value added.
9. This was obtained allowing the estimated impact of changes in the regulation at t to vary depending on the level of regulation being above/below the sample median at $t-1$.
10. The average annual growth rate of total employment in network industries was a tiny 0.014% between 1990 and 2007 (and -0.039% between 1990 and 2012).
11. By contrast, these results differ from those of Bouis, Duval and Eugster (2015) who do not find significant short-term employment costs of reforming network industries using industry-level OECD STAN data. Two factors might explain this difference. One is that in the most updated STAN Database, before 2008 ISIC Rev. 3 industries are mapped into ISIC rev.4 through an inevitably imperfect conversion table. By increasing measurement error, this might bias estimates towards 0. The other is that Bouis, Duval and Eugster (2015) use a large-reform approach, in which reforms events are identified through a dummy variable taking value 1 if the indicator changes by more than two standard deviations. The difference between their results and those presented here could suggest that large reforms induce a more rapid entry of new competitors thereby speeding up hiring and limiting short-term costs relative to the case of smaller or more gradual reforms.
12. The average estimate is an employment loss of around 1.8%, but this estimate is nowhere near being significant at standard levels of statistical acceptance.

13. These supply side effects might be aggravated by negative interactions with aggregate demand. This would be the case, for example, of reforms implemented when monetary policy has hit the zero lower bound (ZLB), according to recent model-based simulations calibrated on the case of euro area countries (Eggertsson, Ferrero and Raffo, 2014). Absent the room for appropriate accompanying monetary stimulus, structural reforms would not support economic activity in the short run, and may well be contractionary. In the model, this occurs because reforms fuel expectations of prolonged deflation, increase the real interest rate, and depress aggregate demand.
14. EU KLEMS data report quality-adjusted deflators for ICT goods and services, but the database relies on national deflators for the remaining industries (Timmer et al., 2007). This issue is particularly important for the analysis because hedonic deflators are much less frequently-used in services.
15. Bourlès et al. (2013) show that, if markets for intermediate inputs are imperfect and downstream firms have to negotiate with (and share their rents with) service suppliers, then high regulation would increase suppliers' market and bargaining power, reducing incentives to improve efficiency downstream. Their framework also allows for greater entry upstream favouring competition among users, as access to downstream markets is constrained by the amount and quality of available inputs. For example, they argue that restrictive licensing or business conduct regulations in transport services would discourage development of efficient and innovative distribution channels.
16. The base sample covers annual data from EU KLEMS for the period 1975-2007 and the same countries as in the previous subsection with the exception of the United States (whose input intensities are used to construct the interaction term). The industry classification is an intermediate level between 1 and 2 digit levels of the ISIC Rev. 3 classification, and corresponds to the 2-letter NACE Rev. 1 classification. As in the previous subsection, the sample is extended to cover the period 1975-2012 in robustness checks.
17. With weighted estimation, each country-industry cell is weighted by its employment share (average taken over 1975-2007). Hence, larger industries in each country contribute more to the estimated coefficients. With unweighted estimation, each cell is attributed the same weight.
18. The results are also robust to the choice of the input intensity measure (United States vs. average).
19. There is a more abundant literature studying short-term effects of reforms tightening the stringency of dismissal rules (e.g. Autor, Donohue III and Schwab, 2006; Autor Kerr and Kugler, 2007; Marinescu, 2009; Kugler and Pica, 2008; Centeno and Novo, 2012; and Cingano et al., 2016). However, the impacts of protection-raising and flexibility-enhancing EPL reforms are not necessarily symmetric. It is therefore not obvious that findings concerning the effect of the former could be used to predict the impact of the latter.
20. The only exception is Bauer et al. (2007), who study the short-lived increase in the exemption threshold for certain EPL rules in Germany in the 1990s. In 1996, the size threshold for exemption from certain limitations concerning fair dismissal was raised from five to ten employees. A new government, however, moved this threshold back to five in 1999. Bauer et al. (2007) compare firms just above and just below the 10-employee threshold and find no impact on either hiring or separations in the three years in which the new threshold was active.
21. For example, Bouis et al. (2012a) analyse the effect of several structural reforms using a dynamic model with several lags but including policies one at a time. It is therefore impossible to gauge whether estimated effects are due to the policy under study or to other policy changes occurring at a close-by date. Moreover, the approach followed by Bouis et al. (2012a), by focussing only on large reforms, appears particularly unsuitable to study the effects of liberalisations of dismissals, which are relatively rare events. As a result of adopting that approach, the estimated effect of EPL in that study depends entirely on only two EPL reforms (Spain in 1994 and Korea in 1998). These issues apply to large extent also to IMF (2016) that controls for large reforms but not for small but frequent policy changes in other institutions.
22. The base sample covers annual data from EU KLEMS for the period 1985-2007 and the same countries and industries as in the previous section with the exception of the United States (whose dismissal rates are used as a benchmark) and Korea (because output-gap data are unavailable). As in the previous section the sample is extended to cover the period 1985-2012 in robustness checks.
23. The level of these indicators for each OECD country and the latest available year is reported in Table 3.A3.2 in OECD (2016a).
24. In contrast with the case of product market deregulation, where episodes of re-regulation are rare and minor, EPL reforms have historically gone in both directions. As this chapter focuses on flexibility-enhancing reforms, it is crucial that the estimated specifications allow for short-run-effect heterogeneity between the impacts of liberalisation and protection-increasing reforms.

25. All reform episodes in the main sample (1985-2007) 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 quantified by the EPL indicator for individual dismissals as a reduction of 1.19 points. Yet, the suppression of the procedure for administrative authorisation of dismissals limited to the case of individual redundancies – which characterised that reform – is typically overstated in the quantitative EPL indicators (see OECD, 2013, for a discussion). Not surprisingly, therefore, results obtained using directly the change in the EPL indicator as reform variable are extremely sensitive to the inclusion of this outlier. Once Spain is excluded from the sample, the use of either quantitative or qualitative indicators yields essentially the same results. These findings are also robust to further exclusion of additional countries (see OECD, 2016a).
26. Baseline models include three lags of all variables, as suggested by on Akaike's and Bayesian information criteria.
27. That is 0.2 points, no matter the sample used (1985-2007 or 1985-2012).
28. This result is robust to the number of lags included in the specification (2 or 3), the choice of the estimation sample (EU KLEMS only – 1985-2007 – or matched EU KLEMS-STAN – 1985-2012) and the use of weighted or unweighted estimators. The estimates are also robust to the elimination of countries one-by-one from the sample. By contrast, no evidence is found that the impact of the reforms varies with the initial level of dismissal regulation (see OECD, 2016a).
29. Corresponding to about 60% of a standard deviation of the cross-industry distribution of US dismissal rates.
30. Business-sector employment is estimated to become already insignificantly different from what the level would have reached in the absence of the reform two years after the reform's enactment, although this result is partly due to widening standard errors as a function of time (see Figure 3.5). The recovery from the initial employment fall is, however, estimated to be much faster when unweighted estimators are used (see OECD, 2016a, Figure 3.A2.1), suggesting that recovery is possibly faster in smaller industries (typically manufacturing industries, where greater competition is likely to make output and employment more sensitive to firm efficiency).
31. The average annual growth rate of wage and salary employment in the non-agricultural non-mining business sector was 1% between 1990 and 2007 and 0.8% between 1990 and 2012. The cumulative effect of the average EPL reform within the first two years is found to be 0.44% when estimated in the latter sample.
32. Significant at the 5% level.
33. By contrast, the share of low-educated workers appears unaffected by EPL in the short run.
34. As shown in OECD (2012), however, not all productivity gains shows up in nominal wage gains of the industries more directly affected by the policy reform. The main reason for this is that, due to competitive pressures, most of the improvement in productivity is translated into lower quality-adjusted prices, suggesting that workers also benefit from these reforms as consumers.
35. By contrast, the impact of EPL reforms on wages does not seem to vary over the business cycle (see OECD, 2016a).
36. The output gap measures the difference between actual and potential GDP. A negative value of the interaction term indicates downturns.
37. The output gap was improving by one percentage point or more in about one quarter of the sample observations and falling by that amount in about a quarter of the sample.
38. Aggregate effects are obtained under the same assumptions as for Figure 3.5.
39. The cumulative employment impact of an EPL reform during an upturn is even estimated to be positive 4 years after the reform, albeit not significantly so.
40. By contrast, the impact of EPL reforms on wages does not seem to vary over the business cycle (see OECD, 2016a, Table 3.A2.2).
41. This result is broadly in line with those reported in IMF (2016).
42. Since these reforms will reduce the cost for employers of converting fixed-term contracts into open-ended ones, thereby increasing the opportunity cost of termination, it is even possible that job destruction will fall in the short run.

43. Aggregate effects are obtained under the same assumptions as for Figure 3.5. Caution must be exerted in interpreting interactions between aggregate structural variables since they often yield unstable estimates (see e.g. Bassanini and Duval, 2009, for a discussion). However, the same specification underlying Figure 3.5 has been re-estimated with similar results replacing the aggregate share of fixed-term contracts in wage and salary employment with the industry-specific one.
44. The cost of filling an unskilled position is typically lower than in the case of skilled workers. As a consequence, employers have greater incentive to terminate unskilled jobs in the event of a negative shock and then to re-open these positions when the business climate improves (see e.g. Dolado, Felgueroso and Jimeno, 2000; Gautier et al., 2002).
45. Although more frequent in countries with highly-dual labour markets, reforms relaxing dismissal legislation are not uncommon in countries with a low incidence of temporary contracts. For example, this was the case for the 2003 Austrian reform, which introduced a system of individual savings accounts to replace redundancy payments for dismissals in a labour market with only about 7% of temporary workers. Similar examples can also be found in many other economies, and in particular, since 2000, in the United Kingdom, Ireland and many Eastern European countries (see the next subsection for the case of the 2009 reform in Estonia).
46. The OECD indicator of stringency of employment protection against individual and collective dismissals counts 36 flexibility-enhancing reform episodes in OECD countries since 1998. The reforms considered in this subsection are all among the ten largest episodes in terms of magnitude of the reduction in the indicator.
47. These results are robust to excluding observations close to the date of reform enforcement – to take into account the fact that once the measures are announced employers could postpone dismissals until their implementation, thereby artificially reducing unemployment before the reform enactment (see OECD, 2016a, Table 3.A2.7). Moreover, placebo experiments suggest that the estimated coefficients of Table 3.1 are not due to the shift of omitted variables occurring at a different date close to the date of reform enforcement (see Box 3.5). The findings for Estonia and Spain are also consistent with the evaluations of Malk (2013), Izquierdo, Lacuesta and Puente (2013) and Puente and Font (2013). The former finds that separations increase more than hires in Estonia immediately after the reform, while the latter two finds that employment levels did not worsen after the Spanish reform while the elasticity of employment changes to declines in GDP decreased.
48. With respect to the first year after the reform, the unemployment rate is estimated to have dropped in the second year by 1.5 and 0.7 percentage points in Estonia and Slovenia, respectively. Both these estimates are statistically significant at the 1% level. No difference between the first and the second year is estimated in the case of Spain.
49. In the case of Estonia and Spain standard errors are as large as 0.6 percentage points. By contrast, in the case of Slovenia, the standard error is smaller than 0.3 percentage points.
50. Latvia and Lithuania have been also used as a control group to evaluate the 2000 corporate tax reform in Estonia (Masso et al., 2013).
51. Included controls are the 3-month-lagged industrial production and retail turnover indexes, gender, 3 educational attainment classes, 15 age classes, 3 classes for the degree of urbanisation, a dummy for the country of birth, 23 classes for the duration of residence in the country if foreign born, 12 month dummies, 2 country dummies, one post-2009 indicator and its interaction with country dummies. The sample window is restricted to 2 years before and after the date of enforcement of the Estonian reform.
52. These findings are robust to excluding observations close to the date of reform enforcement, controlling for polynomial time trends or including also Latvia in the control group (see OECD, 2016a, Table 3.A2.9). Moreover, a placebo experiment in which the date of the reform is fictitiously anticipated by three months yields an insignificant estimate coefficient.
53. The Slovenian data exclude bunches of ten Social Security registrations with the same employers on the same day. Data for July and December 2011 as well as January 2012 were excluded from the figure, since administrative changes implied a re-registration of a large number of existing contracts.
54. More precisely, these EPL reforms are associated with an increase of 47% and 45% of the share of open-ended contracts in new contracts in Slovenia and Spain, respectively, as compared to the average share in the 12 months preceding each reform.
55. A slight but insignificant increase in the effect in the second year is estimated in Slovenia (0.3 percentage points), while no change in the effect between the two years is observed in Spain.

56. Results are also robust to the exclusion of a 10-month window before the reform in Slovenia, as in the case of unemployment models. In the baseline model for this country, observations for July, December and January of each year are excluded from the sample, to avoid that missing values bias the estimates of month dummies. Results are however robust to the inclusion of these observations (see OECD, 2016a, Table 3.A2.8).
57. Data on conversions and new hires are available only for Spain. The estimates suggest that the Spanish reform raised the share of new hires on permanent contracts in new contracts by 2.5 percentage points. By contrast, the share of conversions in new contracts increased by 0.6 percentage points. To put these figures into context, in the 12 months preceding the reform, these two shares averaged 3.7% and 3.2% of new contracts, respectively. These findings are consistent with those of OECD (2014c), which are nonetheless based on a shorter post-reform time window.
58. In this case, however, EPL reforms cannot yield benefits in term of further reduction of dualism.
59. Between 1985 and 2012 the correlation between changes in the EPL index for regular contracts and the output gap is 0.10, rising up to 0.29 since the onset of the Great Recession. Over the same periods, the correlations between changes in the EPL index and changes in the output gap are 0.07 and 0.19, respectively. The comparison of these sets of correlations suggests that, at least in recent years, EPL reforms tend to be undertaken slightly more frequently in bad times but not necessarily in the recession phase.
60. Between 1975 and 2012 the correlation between changes in the indicator of stringency of anti-competitive regulation of entry barriers in network industries and the output gap is 0.05 and with the change in the output gap is -0.01. Restricting the attention to the period since the onset of the Great Recession, these correlation coefficients become slightly more negative, which does not support the idea that the worse the cyclical conditions the greater the probability and size of the reforms in this area.
61. For example, a few countries (such as Australia or the United States) set-up programmes for trade displaced workers in anticipation of trade liberalisation episodes.
62. Jimeno and Thomas (2013) show that sectoral or centralised bargaining systems can deliver the same flexibility as decentralised systems if companies can easily opt-out of collective agreements in times of crisis.
63. Available evidence also suggests that the newly-introduced measures to foster internal flexibility and flexible collective bargaining played an important role in the decline in unit labour costs in Spain since 2012 relative to other euro area countries (see e.g. Izquierdo, Lacuesta and Puente, 2013; BBVA, 2013).
64. Collective bargaining reforms decentralising the level of negotiation, however, could lead to wage losses and worse working-conditions in the short-run that could offset job gains. Another issue, which is left for future research is whether such measures concerning collective bargaining should be permanent or temporary.
65. For example, the 1999 Dutch reform of notice periods included a partial grandfather clause insofar that the employer had to calculate both the new term of notice at the time of dismissal and the old one for the employee's tenure and age on the 1 January 1999 and then apply the most generous of the two to the employee.
66. Workers on precarious jobs are initially given access to opportunities that remain less protected than the jobs of insiders and this difference would only disappear gradually by attrition. In fact, dismissal of those already on a permanent contract at the time of the reform becomes relatively more costly than that of workers hired after the reform. As a consequence, firms might be induced to apply a last-in-first-out principle to save on dismissal costs, which will introduce inequalities into the system.
67. The effect on wages is, however, less clear, since the outside option (which depends also on the severance pay the employee would be entitled to when switching to a different job) of those with an open-ended contract at the time of the reforms would be reduced, exerting downward pressure on wages. Van der Wiel (2010) finds that wages of affected workers went down after the 1999 reform of notice periods in the Netherlands, which is likely to reflect lower bargaining power or outside option.
68. Honeymoon effects are also documented as regards two-tier reforms in which temporary contracts are liberalised while maintaining regulation on permanent contracts unchanged (see e.g. Boeri, 2011). In this case, however, steady-state employment might end up being lower than in the previous equilibrium (see e.g. Kahn, 2010).

69. The 2012 labour market reform in Italy, that restricted the conditions under which courts could order reinstatement as a remedy for unfair dismissal, simultaneously reformed the unemployment benefit system by extending its coverage and making it more generous for certain family situations. The 2012 Portuguese reform of unemployment benefits also extended unemployment insurance coverage and, for jobless households, temporarily increased its initial replacement rate, even though benefit generosity, and in particular unemployment assistance, was tightened in other cases (see OECD, 2016b).
70. On the effects of benefit generosity on welfare, job search effort and externalities, see for example, Levine (1993); Card, Chetty and Weber (2007); Chetty (2008); Schmieder, von Wachter and Bender (2012a); Tatsiramos and van Ours (2014); Lalive, Landais and Zweimüller (2015); and Mitman and Rabinovich (2015).
71. Marinescu (2015) used new data on state-level job applications and job vacancies from a large American online job board covering about 30% of all vacancies in the United States to show that unemployment-insurance extensions did generate a negative impact on the number of applications submitted (suggesting a decrease in average job search effort), but that this also increased the hiring rate per application sent (the search externality), and therefore contributed to reducing the unemployment rate.
72. Extensions rules are triggered by state unemployment, as measured in real time (see Box 3.6). It is not infrequent that UI extensions are erroneously triggered because of measurement error in real-time unemployment time-series. These non-systematic errors can be identified once the corrected series are produced some time later, and provide a natural experiment to distinguish the effect of the worsening economic conditions triggering extensions from the effect of the extensions themselves.

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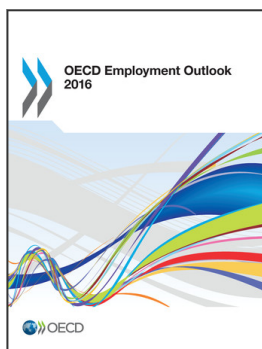
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