**SUPPLEMENTARY ONLINE APPENDIX**

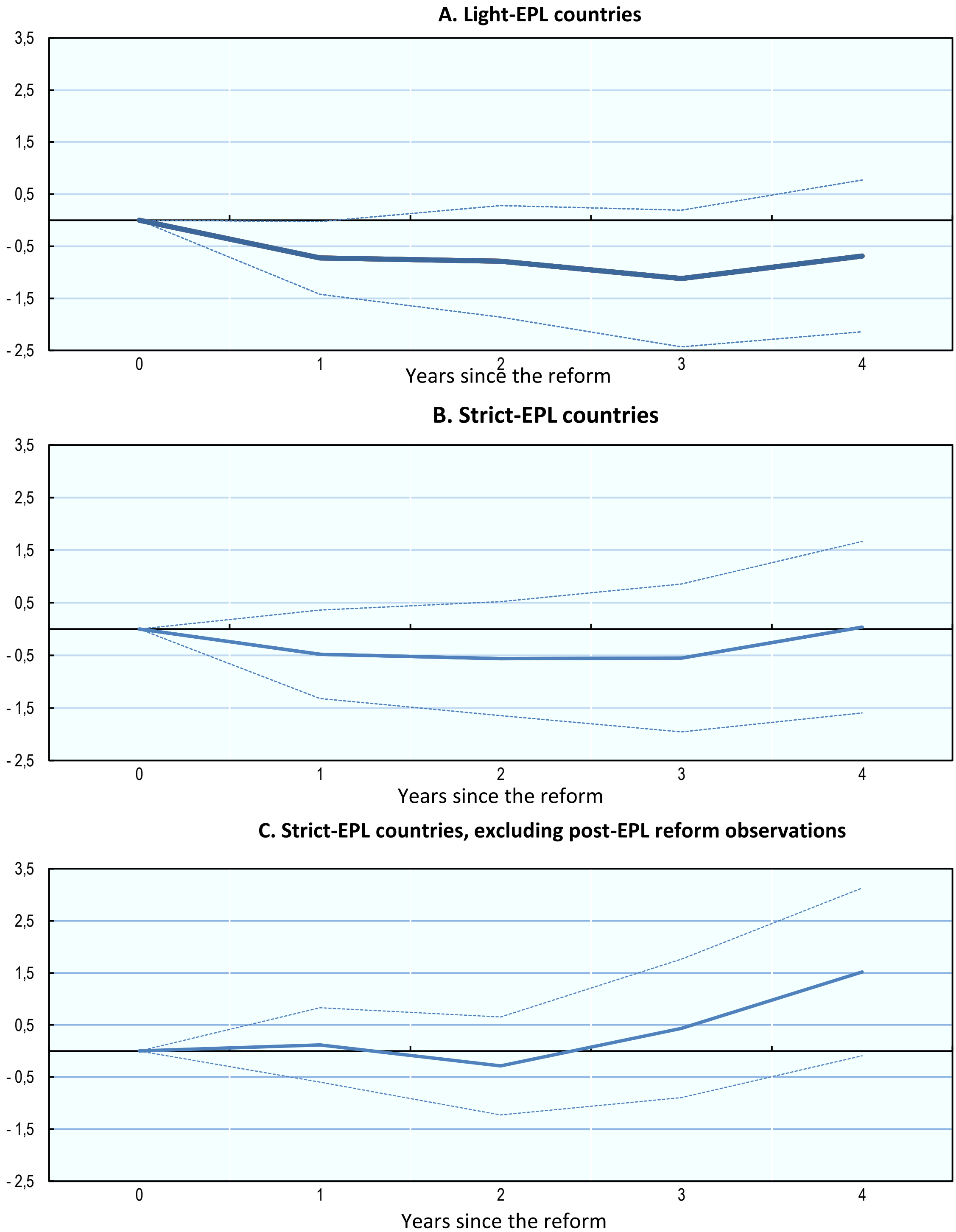
# Before It Gets Better: The Short-Term Employment Costs of Regulatory Reforms

**Contents**

## APPENDIX A

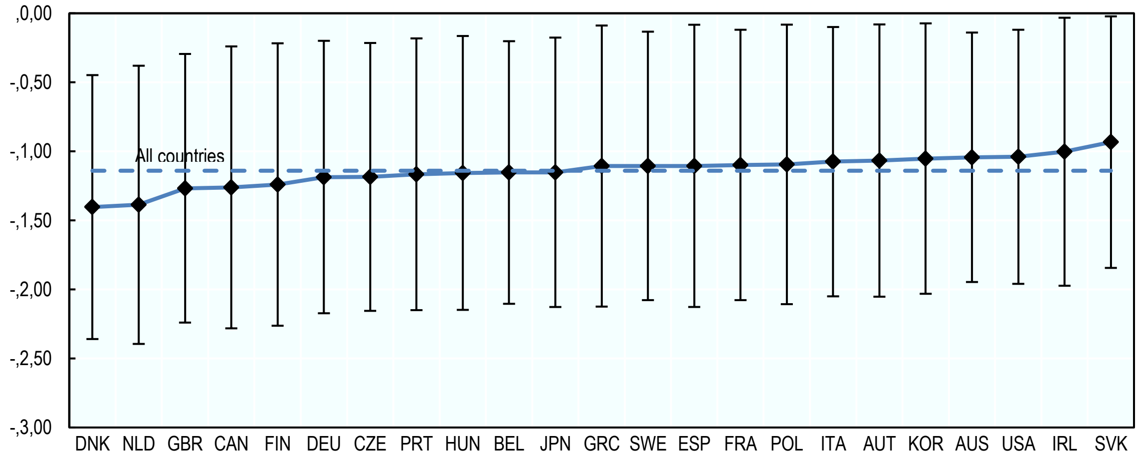
## Additional Results

***Figure A.1.* Competition-Enhancing Reforms and Employment in Network Industries: light-EPL vs. strict-EPL countries**



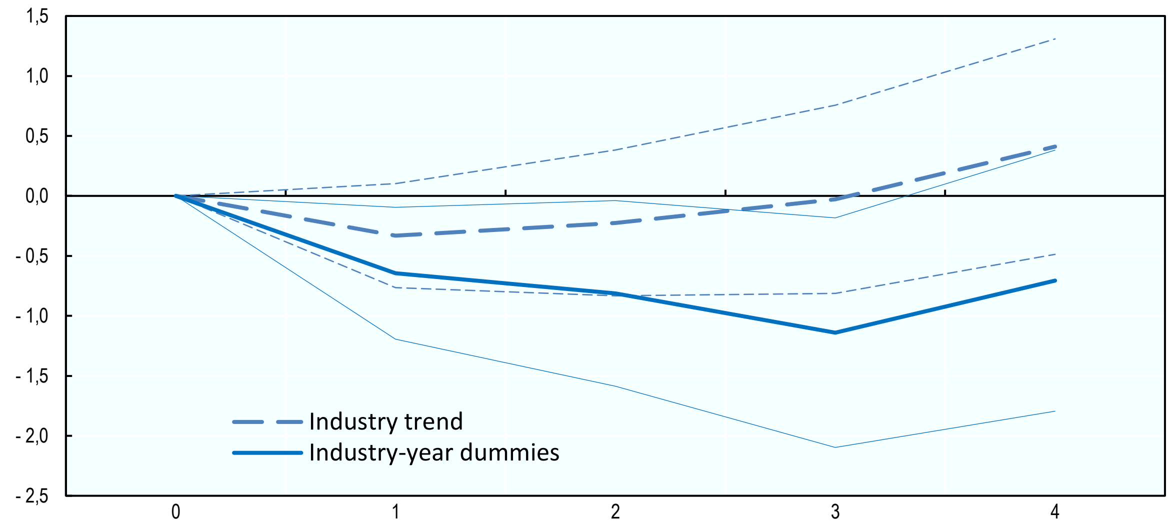

*Notes:* Estimated cumulated change in industry employment years after the reform, in percentage. The chart reports point estimates and 90% confidence intervals of the cumulated effect of reforms lowering entry barriers in network industries on average industry employment. Light-EPL countries in panel A are Australia, Belgium, Canada, Denmark, France, Hungary, Ireland, Japan, Poland, Slovakia, the United Kingdom, and the United States. Strict-EPL countries in panel B are Austria, the Czech Republic, Finland, Germany, Greece, Italy, Korea, the Netherlands, Portugal, Spain, and Sweden. Panel C reports estimates for strict-EPL countries obtained by excluding Austria since 2003, Finland since 1992, Korea since 1998, and Spain since 1995. Estimates refer to the case of a reform lowering the OECD indicator of regulation in Energy, Transport and Communication (ETCR) by one point. Employment levels before the reform are normalized to 0. The underlying parameters are estimated allowing employment growth in each network industry to depend on lagged values of industry regulation as well as on lagged employment changes. The workforce composition is accounted for by the share of the low-educated in total hours worked and changes in industry employment. Confidence intervals are obtained by clustering errors on countries and industries.

***Figure A.2.*** **Competition-Enhancing Reforms and Employment in Network Industries: Robustness to Varying the Country Sample.**



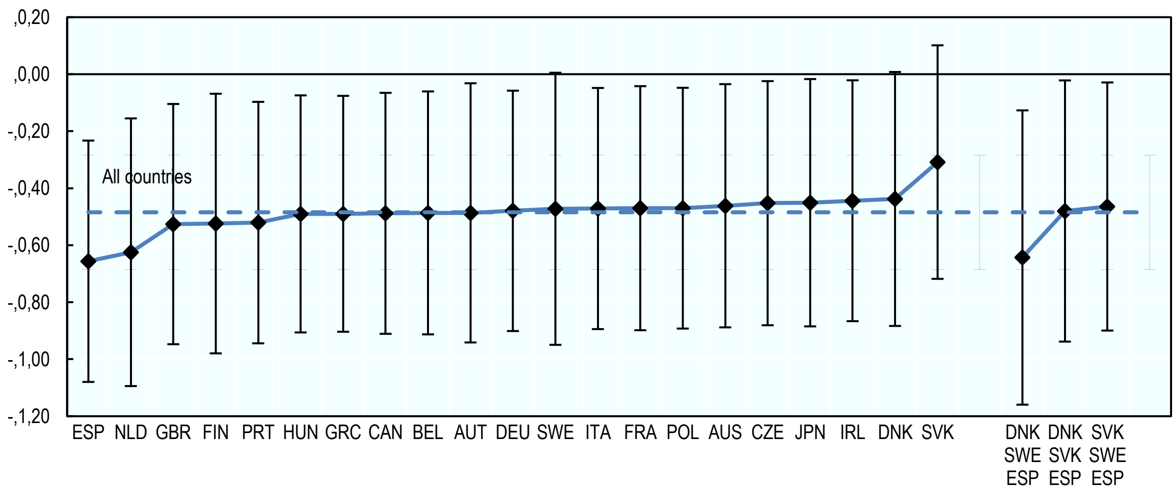

*Notes:* Estimated cumulated change in industry employment three years after the reform, in percentage. The chart reports point estimates and 90% confidence intervals of the cumulated employment effect of PMR reforms lowering entry barriers. The baseline estimate, reported in the top panel of Figure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

***Figure A.3.*** **Competition-Enhancing Reforms and Employment in Network Industries: The Role of Industry Shocks**



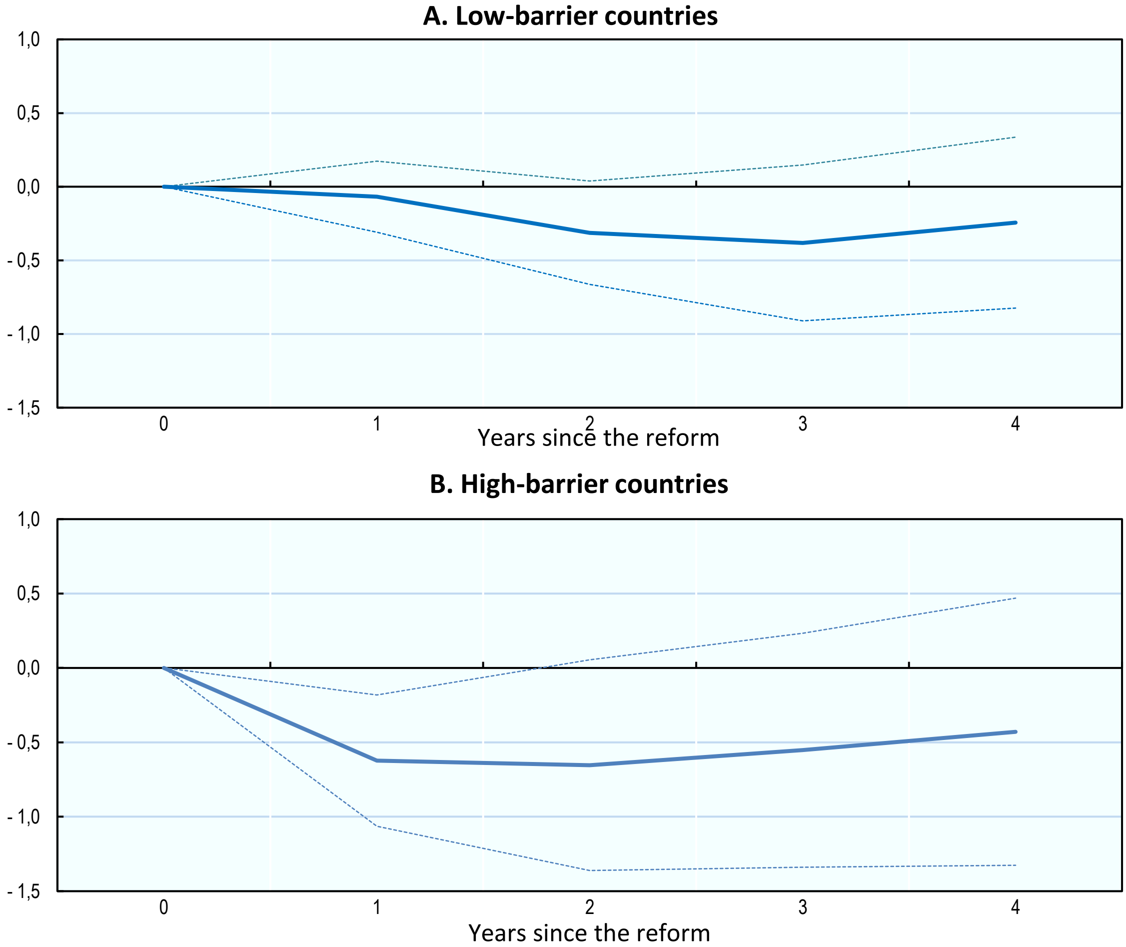

*Notes:* Estimated cumulated change in industry employment up to four years after the reform, in percentage. The chart reports point estimates and 90% confidence intervals of the cumulated effect of reforms lowering entry barriers in network industries on average industry employment. The baseline estimates (continuous line) are compared with those obtained by replacing industry-by-year dummies with industry-specific trends (dashed line). Estimates refer to the case of a reform lowering the OECD indicator of regulation in Energy, Transport and Communication (ETCR) by one point. Employment levels before the reform are normalized to 0. The underlying parameters are estimated allowing employment growth in each network industry to depend on lagged values of industry regulation as well as on lagged employment changes. Confidence intervals are obtained by clustering errors on countries and industries.

***Figure A.4.*** **Flexibility-Enhancing EPL Reforms and Employment: Robustness to Varying the Country Sample**




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

**Figure A.5. Flexibility-Enhancing EPL Reforms and Employment: light-PMR vs. high-PMR countries**




*Notes:* Estimated cumulated change in industry employment years after the reform, in percentage. The chart reports point estimates and 90% confidence intervals of the cumulated effect of changes in employment protection legislation (EPL) for regular contracts on the difference in wage and salary employment levels between two industries in the non-agricultural/non-mining business sector whose US dismissal rates differ by 1 percentage point. The difference in employment between the dismissal-intensive and the other industry normalized to 0 before the reform. Low-barrier countries in panel A are those with the earliest available value of average of the indicators of economy-wide administrative barriers to start-ups of corporations and sole proprietors below the median—that is, Australia, Belgium, Canada, Denmark, Finland, Ireland, the Netherlands, Sweden, and the United Kingdom. High-barrier countries in panel B are Austria, the Czech Republic, France, Germany, Greece, Hungary, Italy, Japan, Poland, Portugal, Slovakia, and Spain. Estimates refer to the effect of an indicator variable taking value 1 when the quantitative indicator of EPL for regular contracts decreases, and 0 otherwise. Confidence intervals are obtained by clustering errors on countries and industries.

**Table A.1. Granger-Causality Tests of Reverse Causality (Barriers to Entry and Employment)**

| | Not including Δlog Employment ($t$) | Including Δlog Employment ($t$) |
|---|---|---|
| | (1) | (2) |
| $F$-test on Δlog Employment ($t$-1) | 0.19 | 0.2 |
| $F$-test on Δlog Employment ($t$-2) | 2.39 | 1.94 |
| $F$-test, cumulative impact | 0.54 | 0.38 |

*Notes:* The table presents $F$-tests of the coefficients of the first two lags of employment growth ($\Delta E_{ci,t-1}$and $\Delta E_{ci,t-2}$) in models for which the change in barriers to entry ($\Delta BE_{cit}$) is the dependent variable. The full specification also includes two lags of $\Delta BE_{cit}$ country-by-industry, country-by-time, and industry-by-time dummies. "$F$-test, cumulative impact" is the $F$-test on the sum of both lagged Δlog employment coefficients. $F$-statistics are distributed as F(1.68) under the null (test statistics are obtained by clustering errors at the country-by-industry level). None of the reported statistics is significant at standard levels.

**Table A.2. Short-Run Effect of Deregulation in Network Industries (1975–2012)**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $\Delta$ $BE_{cit}$ | 0.0071*** | | | 0.0073*** | 0.0068*** |
| | (0.0023) | | | (0.0024) | (0.0024) |
| $\Delta$ $BE_{cit-1}$ | | 0.0073*** | 0.0068* | 0.0067* | 0.0074** |
| | | (0.0028) | (0.0035) | (0.0035) | (0.0037) |
| $\Delta$ $BE_{cit-2}$ | | | 0.0023 | 0.0031 | 0.0024 |
| | | | (0.0030) | (0.0029) | (0.0032) |
| $\Delta$ $BE_{cit-3}$ | | | | | 0.0043 |
| | | | | | (0.0036) |
| $\Delta$ log $E_{cit-1}$ | | | 0.0507 | 0.0502 | |
| | | | (0.0611) | (0.0604) | |
| $\Delta$ log $E_{cit-2}$ | | | –0.1080* | –0.1114* | |
| | | | (0.0609) | (0.0604) | |
| Observations | 2,108 | 2,058 | 1,970 | 1,876 | 1,936 |
| $R$-squared | 0.246 | 0.247 | 0.625 | 0.627 | 0.621 |

*Notes:* The dependent variable is the yearly growth rate of total employment in network industries computed on a longer sample (1975–2012) obtained by combining EUEU KLEMSKLEMS and STAN data. $\Delta$ $BE_{cit}$ measures changes in the regulation of entry (with $\Delta$ $BE_{cit} < 0$ in case of flexibility-enhancing reforms). The estimates refer to alternative specifications of model (1). Observations are weighted with the average (1975–2007) industry employment share in the country. Standard errors, adjusted for clustering at the country-by-industry level, in parentheses.
***, **, and * denote coefficients significantly different from zero at 99%, 95%, and 90% confidence level, respectively.

**Table A.3. Short-Run Effect of Deregulation in Network Industries Using a Dummy Variable**

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| $\Delta$ $FEBE_{cit}$ | –0.0044 | | | –0.0050 | –0.0054 |
| | (0.0042) | | | (0.0044) | (0.0046) |
| $\Delta$ $FEBE_{cit-1}$ | | –0.0125** | –0.0112** | –0.0112** | –0.0132** |
| | | (0.0053) | (0.0052) | (0.0052) | (0.0057) |
| $\Delta$ $FEBE_{cit-2}$ | | | –0.0016 | –0.0023 | –0.0024 |
| | | | (0.0050) | (0.0050) | (0.0053) |
| $\Delta$ $FEBE_{cit-3}$ | | | | | –0.0074 |
| | | | | | (0.0055) |
| Observations | 1,891 | 1,833 | 1,753 | 1,753 | 1,695 |
| $R$-squared | 0.649 | 0.650 | 0.646 | 0.647 | 0.642 |

*Notes:* The dependent variable is the yearly growth rate of total employment in network industries computed on EU KLEMS (1975–2007) data. Observations are weighted with the average (1975–2007) industry employment share in the country. $\Delta$FEBE stands for a dummy variable equal to 1 when the change in the OECD indicator for barriers to entry is greater, in absolute terms, than two standard deviations (133 reforms). All specifications account for country-by-industry, country-by-time, and industry-by-time dummies. Specifications corresponding to columns (3) and (4) include also two lags of the dependent variable. Standard errors, adjusted for clustering at the country-by-industry level, in parentheses.
***, **, and * denote coefficients significantly different from zero at 99%, 95%, and 90% confidence level, respectively.

**Table A.4. Short-Run Effect of Deregulation in Network Industries: Changing Industry Breakdown**

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

*Notes:* The dependent variable is the yearly growth rate of total employment in network industries computed on the subset of EU KLEMS (1975–2007) data for which a five-industries breakdown (as opposed to three in the main sample) is available. The underlying network industries are Electricity, Gas, Land transport, Air transport, and Communications. The number of country-year pairs is 431, down from 587 in the baseline sample used in Table 1. In column (2) the number of industries is collapsed to the three of the main sample: Energy (Electricity and Gas), Transport (Air and Land) and Communication. Observations are weighted with the average (1975–2007) industry employment share in the country. All specifications account for country-by-industry, country-by-time, and industry-by-time dummies. Standard errors, adjusted for clustering at the country-by-industry level, in parentheses.
***, **, and * denote coefficients significantly different from zero at 99%, 95%, and 90% confidence level, respectively.

**Table A.5. Quantitative EPL Indicators**

| | (1) | (2) | (3) |
|---|---|---|---|
| | Base sample weighted | Base sample unweighted | Ext. sample weighted |
| $SFE_t$*DR | –0.0206*** | –0.0180** | –0.0244*** |
| | (0.0068) | (0.0083) | (0.0070) |
| $SFE_{t-1}$*DR | –0.0054 | 0.0109 | –0.0037 |
| | (0.0103) | (0.0124) | (0.0090) |
| $SFE_{t-2}$*DR | –0.0036 | 0.0060 | –0.0041 |
| | (0.0103) | (0.0082) | (0.0100) |
| $SFE_{t-3}$*DR | 0.0050 | 0.0047 | 0.0062 |
| | (0.0077) | (0.0070) | (0.0111) |
| Observations | 7,172 | 7,172 | 8,629 |
| *R*-squared | 0.521 | 0.393 | 0.515 |

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

**Table A.6. Other Industry Interactions**

| Industry interaction is: | US based (1) | US based (2) | UK based (3) | UK based (4) | Fitted (5) | Fitted (6) |
|---|---|---|---|---|---|---|
| $FE_t$*DR | –0.0029* | –0.0030* | –0.0036** | –0.0039** | –0.0049* | –0.0053* |
| | (0.0016) | (0.0016) | (0.0018) | (0.0018) | (0.0027) | (0.0028) |
| $FE_{t-1}$*DR | –0.0009 | –0.0009 | 0.0010 | 0.0000 | 0.0015 | 0.0017 |
| | (0.0016) | (0.0013) | (0.0019) | (0.0016) | (0.0028) | (0.0024) |
| $FE_{t-2}$*DR | 0.0009 | 0.0008 | –0.0003 | –0.0004 | 0.0030 | 0.0030 |
| | (0.0015) | (0.0014) | (0.0013) | (0.0013) | (0.0024) | (0.0022) |
| $FE_{t-3}$*DR | 0.0001 | | –0.0030 | | –0.0004 | |
| | (0.0014) | | (0.0018) | | (0.0024) | |
| Observations | 7,590 | 8,052 | 7,590 | 8,052 | 7,590 | 8,052 |
| *R*-squared | 0.532 | 0.526 | 0.532 | 0.526 | 0.532 | 0.526 |

*Notes:* The dependent variable is the yearly growth rate of wage and salary employment. FE: dummy variable for flexibility-enhancing reforms of EPL for regular contracts; DR: industry-level dismissal rate (in %) measured as indicated in the column title. OTHER_REF: change in the policy/institutions indicated. All specifications control for a dummy for protection-raising EPL reforms and for changes in the output gap (both interacted with DR and lagged as FE reforms), lags of changes in log employment, and as well as country-by-time, industry-by-time and country-by-industry dummies. Industry-level dismissal rates are sourced from OECD (2009). In column (1) and (2), they are measured on US data (CPS) and in column (3) and (4) on UK data (Quarterly Labour Force). The industry indicators in column (5) and (6) are measured by the coefficients on industry dummies estimated in a cross-country industry regression of dismissal rates also including country dummies. Industry-level dismissal rates are available for Australia (1995–2001), France (2006–07), Germany (2003–07), the United Kingdom (1997–2005), and the United States (1996–2006, even years only). Observations are weighted by the average industry share in the country's non-agricultural/non-mining business sector. The base sample is the EU KLEMS sample (1985–2007). Standard error adjusted for clustering at the country-by-industry level, in parentheses.
***, **, and * denote coefficients significantly different from zero at 99%, 95%, and 90% confidence level, respectively.

## APPENDIX B

### Detailed Data Description

The base sample covers annual data from EU KLEMS for the period 1975–2007, covering 23 OECD countries and 22 non-agricultural/non-mining business-sector (ISIC rev.3) industries.[1] Network industries are aggregated into three industries (Energy, Transport, and Communications). EU KLEMS allows also a finer breakdown into five industries (Electricity, Gas, Land transport, Air transport, and Communications) for a very limited number of countries that we will use in a sensitivity analysis in this article. In principle, the OECD STAN data set would also allow for a breakdown of network industries into five industries (Energy, Land transport, Air transport, Post, and Telecommunications); however, achieving a suitable coverage in terms of, in particular, the time dimension would require collating different vintages of STAN, mixing data collected and published using two radically different industry classifications (ISIC rev.4 and ISIC rev.3, the change having being implemented in 2008 in most countries). Absent a precise correspondence table between classifications, extending the time window to the length available in EU KLEMS would likely lead to substantial measurement error, because even if the general denomination of the five industries did not change between classifications, the underlying sub-industry composition did change. For a sense of the potential implications of such a break, compare the growth rate of employment in the two most recent editions of STAN using each classification in the overlapping country-year cells. Across the two editions, annual employment growth differs by more than 1 percentage point in 27% of the cases, a disparity that can reach 42 percentage points. While focusing on a slightly more aggregate industry classification, our analysis exploits a large time window of homogenous data ending in 2007, thus limiting measurement concerns.

For those countries for which OECD STAN data are available, we use a detailed, country-specific map from ISIC rev.4 industries into ISIC rev.3 at the 1-letter level of ISIC rev.3 for services to extend the sample to 2012 by collating EU KLEMS data with data from the last version of STAN. The mapping between classifications has been established by using employment data at the 3-digit level from EU LFS (tested on years for which both classifications are available). Such mapping is, however, imperfect and breaks in the industry classification can severely alter the estimated short-run dynamics; moreover, the extension likely exacerbated measurement error. Accordingly, the collated sample is used only in sensitivity analyses.

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

The PMR analysis exploits the ETCR section of the OECD PMR database (see Koske, Wanner, Bitetti, and Barbiero 2015). More specifically, it focuses on the sub-indexes capturing legislated entry barriers and vertical integration (when applicable), varying from 0 (lowest regulation) to 6 (highest regulation). Aggregation of sub-industries (e.g., 3-digit industries) is done by simple averages of sub-industry indicators. For example, in the case of the electricity industry, the indicator of industry-specific entry barriers is the simple average of three sub-indicators concerning third-party access (free, regulated, no access), existence of a wholesale pool, and a minimum consumption threshold that consumers must exceed in order to be able to choose their electricity supplier. In the sample, changes in the indicator have negative sign in approximately 95% of cases. More than one-sixth of the reform episodes implied a fall of the index of at least one point (the hypothetical reform used in the article) 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

[1] The EU KLEMS project ("EU level analysis of capital (K), labour (L), energy (E), materials (M) and service (S) inputs") is a statistical and analytical research project to analyse productivity and growth funded by the European Commission ( see http://ec.europa.eu/economy_finance/publications/pages/publication9467_en.pdf

in Conway and Nicoletti (2006), a one-point reduction in the regulation index could be obtained, for example, by guaranteeing regulated third-party access (TPA) to the electricity transmission grid and liberalizing 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 one point or more in the indicator correspond to, for example, the implementation of the British Telecommunications Act in 1982 (opening a second fixed link network in competition with British Telecom), or the Electricity Act and the unbundling of the UK Central Electricity Generating Board (CEGB) in 1989; the Canadian National Transportation Act (NTA) and Motor Vehicle Transport Act (MVTA) of 1988; the Japanese Telecommunication Laws of the late 1980s and the Australian Telecommunications (Consumer Protection and Service Standards) Act of 1999; the 2003 French Electricity Law allowing any EU supplier to trade on the French territory (and more broadly the consequences of the EU liberalization directives of the electricity and gas markets adopted since the mid-1990s).

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

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

Further data used in robustness checks are sourced from the OECD Taxben, Taxing wages and EPL databases (Unemployment benefit average net replacement rate, average collective bargaining coverage, average labor tax wedge, and regulation on hiring on temporary contracts), and the ICTWSS database (http://www.uva-aias.net/208) for collective bargaining variables. When these variables are not available on a yearly basis, missing values have been interpolated linearly.

Micro-data used in Appendix D are from the Estonian Labour Force Survey, whereas industrial production and retail turnover indexes are from Eurostat. In the RDD exercises for the Estonia, Slovenia, and Spain countries, the standardized unemployment rate is from the OECD Labour Force Statistics. Industrial production and retail turnover are from national statistical offices (Eurostat in the case of Estonia). The shares of youth and older workers in the labor force are from Labour Force Surveys of each country.

**Table B.1 Descriptive Statistics**

**Panel A. PMR Sample: Network Industries (1975–2007)**

| | Observations | Mean | Standard deviation |
|---|---|---|---|
| Δ log employment | 1,891 | 0.0035 | 0.0426 |
| Δ log (wage and salary) employment | 1,351 | –0.0001 | 0.0471 |
| Δ log average wage (wage and salary employment) | 1,351 | 0.0185 | 0.0550 |
| Δ barriers to entry (0-6 scale) | 1,891 | –0.1446 | 0.3906 |
| Δ public ownership (0-6 scale) | 1,891 | –0.0696 | 0.2708 |
| Δ barriers to entry in other network industries (I-O weighted) | 1,891 | –0.0604 | 0.3023 |
| Δ output gap (%) | 1,750 | 0.104 | 1.586 |
| Δ log intermediate inputs (volume) | 1,891 | 0.0517 | 0.0921 |
| Δ log value added (volume) | 1,891 | 0.0409 | 0.0679 |

**Panel B. EPL Sample: Business Sector Industries, Non-mining, Non-agricultural (1985–2007)**

| | Observations | Mean | Standard deviation |
|---|---|---|---|
| Δ log (wage and salary) employment | 8,976 | 0.0011 | 0.0603 |
| Δ log average wage | 8,976 | 0.0178 | 0.062 |
| Δ EPL (dismissals of workers on regular contracts) | 8,976 | –0.0091 | 0.0739 |
| Flexibility enhancing reform dummy ($FE_t$) | 8,976 | 0.3302 | 1.333 |
| Protection raising reform dummy ($PR_t$) | 8,976 | 0.1143 | 0.8001 |
| Δ output gap (%) | 8,976 | 0.1569 | 1.491 |
| Share of fixed-term contracts (%) | 7,612 | 11.99 | 7.079 |
| US Industry dismissal rate (%) | 22 | 5.1810 | 1.7025 |

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

## APPENDIX C

### Using Country Industry Data to Estimate the Labor Market Impact of Country-Wide Policies

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

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

## APPENDIX D

### The Short-Term Costs of EPL Reforms: Country Case Studies

In this appendix we present further evidence on the short-term labor market consequences of flexibility-enhancing EPL reforms by examining recent episodes across OECD countries. We aim to test in particular whether one-shot reductions in EPL for regular contracts have negative *aggregate* costs in terms of higher unemployment. We focus on the cases of three relatively recent reductions in EPL implemented in Estonia (July 2009), Spain (February 2012), and Slovenia (April 2013). They implied appreciable changes in the legislation of regular contracts (as measured by changes in the corresponding OECD indicator for individual dismissals). More precisely, the Estonian reform, implying a reduction in the indicator of 0.93 points, was the largest one since 1998; the Slovenian one was the fifth, with a fall of 0.44 points; the Spanish was at the median (0.17 points).[2] Furthermore, all these reforms were essentially implemented at a single date, and by and large without concomitant labor market policies, which allows them to be studied with relatively standard impact evaluation methods using high-frequency data.[3]

*Difference-in-Difference Evaluation of the Estonian Reform*

We start our analysis with the case of the 2009 Employment Contracts Act, which implied a sizable relaxation of employment protection regulations in Estonia.[4] Similar and neighboring Baltic countries did not implement any such reform, which naturally lends itself to using a difference-in-difference methodology to assess the reform consequences. Following Malk (2014), our baseline analysis exploits the labor market patterns of, in particular, Lithuania as a control.[5]

A simple comparison of the time series of the unemployment rates in the Baltic States, drawn from Eurostat's EU Labour Force Survey data, suggests that unemployment did rise faster in Estonia in the first year after the July 2009 reform. The differential relative to Lithuania went from being approximately 0 before the reform to 1.7 percentage points in the first quarter of 2010 (see Figure D.1). After that peak, the Estonian unemployment rate decreased more quickly than in both Lithuania and Latvia.

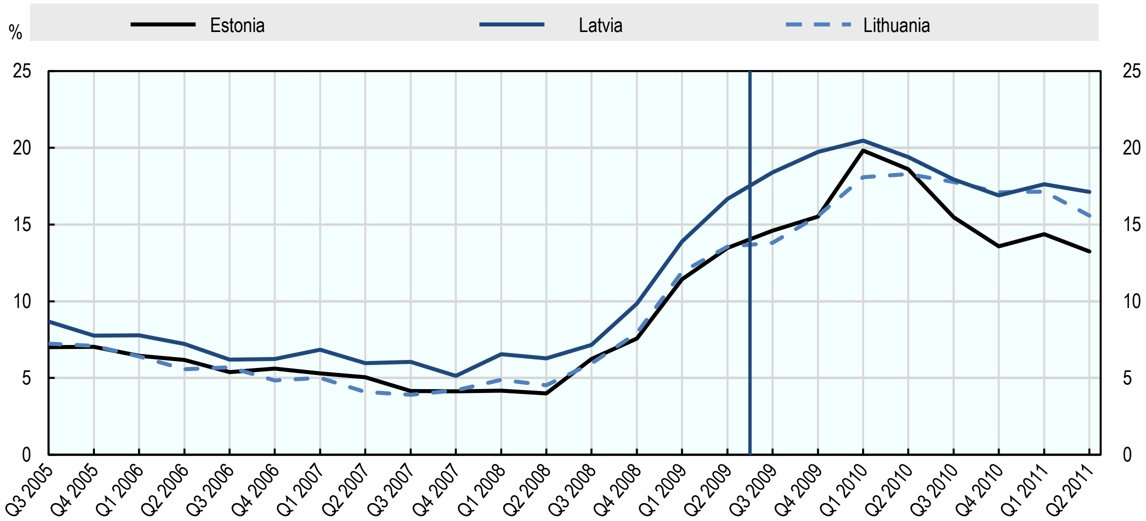
[2] As a matter of comparison, the median flexibility-enhancing reform was as large as a decrease of 0.17 points both if considering reforms since 1998 or since 1985 (first available date in the OECD database). Before the reforms the levels of the indicator, which range between 0 and 6, were 2.74, 2.60,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

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

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

[5] Using Lithuania as a control for the reform implemented in Estonia can be justified on several grounds. Both countries are small open economies with the same trading partners; have similar demographic structure of the labor force; and display a similar evolution of real GDP, industrial production, and retail turnover before and after July 2009. Before the reform, they were also characterized by very similar trends in unemployment as well as stocks and flows of temporary contracts. Finally, no significant changes in labor market policies and institutions occurred in Lithuania over the period considered. The case of Latvia as a comparison group is weaker since unemployment was higher in that country before the Estonian reform, and the difference between the two countries was on the rise (see Figure D.1).

Figure D.1. **Evolution of the Unemployment Rate in the Baltic Countries**




*Source:* EU LFS.

*Notes:* Q3 2004—Q2 2011, in percentage of the labor force. The vertical line indicates the date of enforcement of the Estonian EPL reform.

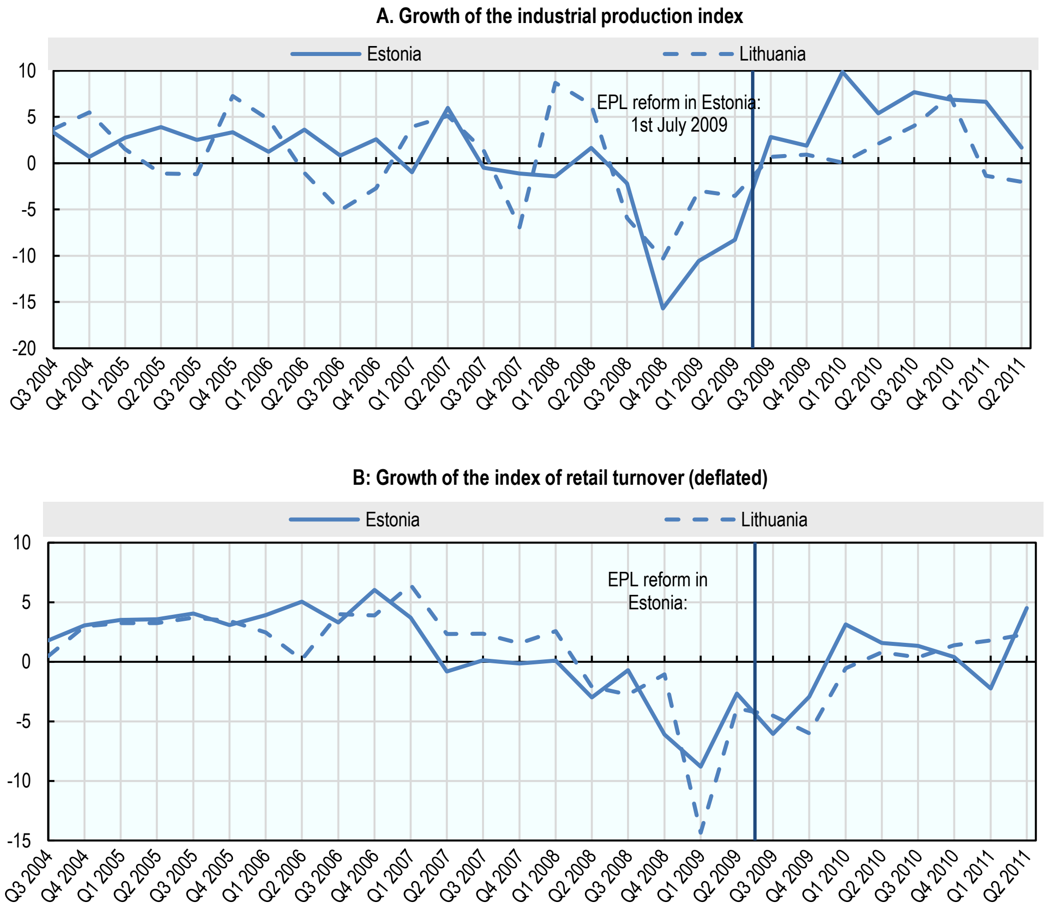
However, composition effects and confounding factors might be at play when comparing Estonia and Lithuania. In particular, despite that the two countries had a similar evolution of business-cycle indicators before and after July 2009, the Estonian industrial production index fell much more than that of Lithuania in the neighborhood of the reform (see Figure D.2). Moreover, the Estonian labor market is more open to immigrants (with 14% of employment being foreign born in 2009 compared to only 4% in Lithuania), who are often at higher risk of unemployment in recessions. Not controlling for these factors could overstate the adverse effect of the Estonian reform. At the same time, the fall in retail turnover was milder in Estonia than in Lithuania, which could act in the opposite direction. For these reasons we go beyond the simple Figure D.2 by estimating a probit model in which the individual probability of being unemployed in a given month is allowed to diverge in the aftermath of the reform:

(D1) $$Prob(U_{ict} = 1) = F(\alpha + X_{cit}\beta_1 + Y_{ct}\beta_2 + \gamma I_{t>R} + \delta D_{EST} + \theta D_{EST} I_{t>R} + \nu_t) + \theta D_{EST} I_{t>R} + \nu_t)$$

where $I_{t>R}$ is an indicator denoting the post July 2009 period, $D_{EST}$ is a country dummy for Estonia, and $\nu$ are dummies for the calendar month of the reference week. In specification (D1), $\delta$ measures the impact of the reform on Estonian unemployment—a significant estimate for this parameter suggests a significant impact of the reform. Vectors $X$ and $Y$ contain, respectively, a large set of individual controls accounting for compositional effects and aggregate covariates.[6] The sample window is restricted to individuals in the labor force and spans the two years before and after the date of enforcement of the Estonian reform.

[6] Individual controls include gender, 3 educational attainment classes, 15 age classes, 3 classes for the degree of urbanization, a dummy for being born in the country of residence, and 23 classes for the duration of residence in the country if foreign born. Aggregate controls are the 3-month-lagged changes in industrial production and retail turnover indexes, month dummies, and, in a sensitivity analysis, a 5th order polynomial time trend.

Figure D.2. **Evolution of the Economic Activity in Estonia and Lithuania, Q3 2004–Q2-2011**




*Source:* Eurostat.

*Notes:* Quarterly percentage changes, seasonally adjusted. The vertical line indicates the date of enforcement of the Estonian EPL reform.

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

Baseline results from estimating (D1) show that unemployment probability in Estonia increased by 1.5 percentage points following the reform, an increase of more than 10% relative to the average unemployment rate in the previous 12 months. These findings are robust to extending the control group including Latvia, changing the specification of aggregate controls, or controlling for polynomial time trends.[7] (See Table D.1, panel A, columns (2) to (4). See also the corresponding note for more details.) Moreover, two placebo experiments in which the date of the reform is fictitiously anticipated by 3 and 12 months, respectively, yield insignificant estimated coefficients (columns (5) and (6)). This supports the conclusion that the discontinuity estimated in the baseline model effectively corresponds to a shift occurring in July 2009.

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

Table D.1. **Difference-in-Difference Estimates for the Estonian Reform**

**Panel A: Baseline sample**

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

**Panel B: Alternative samples**

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

*Notes:* Marginal percentage effects on the probability of being unemployed, obtained by estimating a probit model with observations weighted by cross-sectional weights. The sample window covers 24 months before and after the Estonian reform, except where elsewhere specified. Marginal effects are identified by the interaction between a country dummy for Estonia and a dummy for the post July 2009 period. The baseline specification controls for gender, 3 educational attainment classes, 15 age classes, 3 classes for the degree of urbanization, a dummy for being born in the country of residence, 23 classes for the duration of residence in the country if foreign born, country dummies, dummies for calendar months, a dummy for the post July 2009 period, and the 3-month lagged industrial production and retail turnover. Reform baseline: Baseline (using only Lithuania as comparison group). Reform LTU&LVA: Using Lithuania and Latvia in the comparison group. Reform curr cntrls: Aggregate variables are contemporaneous instead of lagged 3 months. Reform time trend: 5th order polynomial in time (months) included. Placebo 3 months: Fictitious reform 3 months before the true reform. Placebo 1 year: Fictitious reform 12 months before the true reform. Reform no manipulation: Exclusion from the sample of 3 months around the reform date. Reform 3-year window: Sample window reduced to 18 months before and after the reform. Reform backshifted 3-year window: Sample window reduced to 24 months before and 12 months after the reform. Reform frontshifted 3-year window: Sample window reduced to 12 months before and 24 months after the reform. Robust standard errors obtained by adjusting for clustering on countries and months in parentheses.
***, **, * statistically significant at 1%; 5%, and 10% levels, respectively.

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

*RDD Evaluation of Reforms in Estonia, Slovenia, and Spain*

The availability of monthly data on aggregate unemployment (as well as on the demographic structure of the labor force) for Slovenia and Spain allows for a similar assessment of the short-term impact of the (large) EPL reforms implemented there in 2013 and 2012, respectively.[8] Because no obvious comparison group is available for these two countries, however, in their case we need to rely on a simpler time-regression-discontinuity model. More specifically, the short-term impact is estimated through discontinuities in the (seasonally adjusted) standardized unemployment rate. For comparison, the same exercise is also presented for Estonia.

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

$$u_t = Y_t\beta + \delta I_{t>R} + \sum_{s=1}^{5}\lambda_s (t-R)^s + \sum_{s=1}^{5}\mu_s I_{t>R}(t-R)^s + D_t + e_t \qquad \text{(D2)}$$

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

The parameter of interest is $\delta$, which captures the average increase in unemployment following the reform. The key identification assumption is that, conditional on all the control variables in (D2), labor market performance evolves in a smooth way. To isolate the effect of the reform from that of the business cycle, all the specifications also include polynomial time trends up to the 5th order. Following standard practice (see, e.g., Imbens and Lemieux 2008; Lee and Card 2008), polynomial trends are allowed to differ before and after the reform.

The main results are reported in Table D.2. The regression discontinuity approach confirms that the Estonian reform had a sizable negative impact on unemployment, which increased by 1.9 percentage points after its implementation (panel A). The loss is very close to that obtained in the previous difference-in-difference exercise (see Table D.1). Average unemployment increased also in Slovenia, albeit by a smaller extent (0.5 points, see panel B). By contrast, no significant effect is detected in Spain (panel C). The remainder of the table makes sure that these findings are unaffected by a series of specification and robustness checks. Results in column (2) to (4) show that they hold when changing the dependent variable (using unadjusted unemployment rates), including demographic controls, or replacing lagged cyclical variables with current ones. The employment losses in Estonia and Slovenia are unaffected (if anything larger in magnitude) even if altering the observational window by 1) excluding a three-months window centred on the reform date (suggesting no manipulation around the reform date, column (5)), and 2) restricting the time window to three years around the reform. Finally, column (7) proposes placebo tests run by fictitiously anticipating the date of the reform by three months in each case, which implied no significant effect in unemployment, which suggests that the shift we detect in the baseline model effectively occurred at the reform date.

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

Table D.2. **Time-Regression-Discontinuity Estimates**

**Panel A: Estonia**

| | (1) Baseline | (2) Unempl. Nsa | (3) Demogr. controls | (4) Cycle controls | (5) No manipul. | (6) 3-year window | (7) Placebo 3 months |
|---|---|---|---|---|---|---|---|
| Reform dummy | 1.92** | 2.57** | 1.69*** | 1.85*** | 1.97* | 3.48*** | 1.22 |
| | (0.58) | (1,11) | (0.60) | (0.67) | (1.12) | (0.82) | (1,03) |
| Observations | 84 | 84 | 84 | 84 | 81 | 36 | 84 |
| $R$-squared | 0.995 | 0.991 | 0.996 | 0.995 | 0.996 | 0.999 | 0.994 |

**Panel B: Slovenia**

| | (1) Baseline | (2) Unempl. Nsa | (3) Demogr. controls | (4) Cycle controls | (5) No manipul. | (6) 3-year window | (7) Placebo 3 months |
|---|---|---|---|---|---|---|---|
| Reform dummy | 0.55* | 1.11*** | 0.50* | 0.62* | 1.33* | 1.18* | –0.04 |
| | (0.29) | (0.39) | (0.28) | (0.33) | (0.68) | (0.61) | (0.31) |
| Observations | 84 | 84 | 84 | 84 | 81 | 36 | 84 |
| $R$-squared | 0.990 | 0.987 | 0.991 | 0.989 | 0.990 | 0.989 | 0.989 |

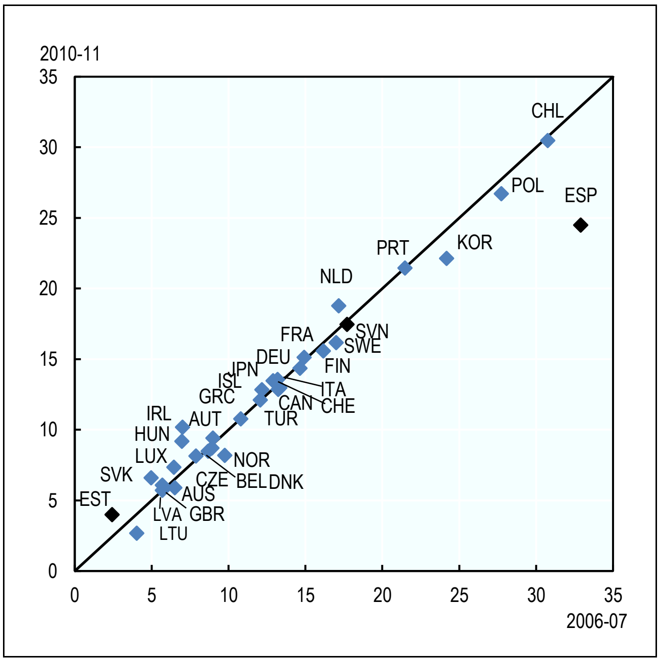
**Panel C: Spain**

| | (1) Baseline | (2) Unempl. Nsa | (3) Demogr. controls | (4) Cycle controls | (5) No manipul. | (6) 3-year window | (7) Placebo 3 months |
|---|---|---|---|---|---|---|---|
| Reform dummy | 0.08 | 0.34 | 0.26 | –0.29 | –0.65 | –0.65** | –0.42 |
| | (0.62) | (0.69) | (0.60) | (0.55) | (1.03) | (0.32) | (0.43) |
| Observations | 84 | 84 | 84 | 84 | 81 | 36 | 84 |
| $R$-squared | 0.997 | 0.996 | 0.997 | 0.998 | 0.997 | 0.999 | 0.997 |

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

There are several ways to rationalize the differences in the estimated impact of the three reforms. One is the intensity of the implemented policy change. The three reforms implied significant changes in the OECD regulation index, whereas those of Estonia and Slovenia feature among the largest in a hypothetical index-based ranking of EPL reforms since 1998, and the Spanish reform stands at the median. A second reason is the different degree of labor market segmentation which, as shown in Figure D.3, is highest in Spain, median in Slovenia, and almost zero in Estonia. The costs of flexibility-enhancing reforms affecting regulations for regular contracts can be expected to be limited in dual labor markets because jobs that are most likely to become unprofitable have likely been filled with a temporary contract. In fact, in the article we do not find evidence that reforms implemented in dual labor markets entail significant short-term costs. A third reason behind the difference in the results relates to the phase of the business cycle at implementation: the onset of a large downturn in Estonia, and just before the crisis trough in Slovenia and Spain. Both basic models with adjustment costs and our evidence in the article suggest the employment losses from EPL reforms should be larger in downturns than upturns. Finally, the impact of the 2012 Spanish reform on unemployment may have been attenuated by the concomitant reform of collective bargaining (see above), which favored decentralization and was found to have had a positive effect on unemployment even in the short term (see, e.g., OECD 2014; García-Pérez and Mestres-Domènech 2017).

Figure D.3. **Incidence of Fixed-Term Contracts in Total Wage and Salary Employment**




*Notes:* Percentage of wage and salary employees with a fixed-term contract, 2006–2007 and 2011–2012. Calculations based on OECD Labour Force Statistics Database and EU LFS microdata. Estonia, Slovenia, and Spain are indicated by black diamonds.

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