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The impact of endogenous product and labour market reforms on unemployment: New evidence based on local projections

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Abstract

We examine the impact of structural reforms on unemployment in 25 OECD countries between 1970-2020. Our local projection (LP) results suggest that labour market reforms reduce unemployment, but only after four years (for youth unemployment it takes longer for the effect to set in), while product market reforms have no statistically significant effect on unemployment. However, if we control for the endogeneity of reforms, the effect of labour market reform becomes statistically significant within a year, while product market reforms temporarily increase unemployment. We also find that labour market reforms hardly have an impact on unemployment in the euro area and that labour market reforms do not significantly affect unemployment when the economy is below trend. The effects of structural reforms on unemployment are stronger under high collective bargaining.

JEL-codes: E62; H30; J21; J65; L43; L51; O43; O47

Key words: unemployment; structural reforms; local projections; endogeneity of reforms; nonlinearities; collective bargaining; AIPW

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"Flexible labour and product markets are essential to help euro area countries respond optimally and rapidly to shocks and to avoid the higher costs of lost output and higher unemployment associated with the slower and more protracted adjustment of rigid economies. The gains from reforms will clearly be larger when reforms are more ambitious and when they are implemented jointly with reforms in other areas. In this light, more efforts are warranted to deregulate product markets, where reform effort has been muted in recent years. Further labour market reform is also necessary and will help to reduce structural unemployment." (ECB, 2014, p. 62).

1. Introduction

International organisations and central banks often call for structural reforms, as illustrated by the quote from the European Central Bank (ECB) above. These reforms not only relate to the labour market, but also to product markets as competition in the product market is an important determinant of employment: in imperfectly competitive markets firms restrict output and thus employment (Griffith et al., 2007). More precisely, structural reforms involve deregulating retail trade, professional services and certain segments of network industries, primarily by reducing barriers to entry; easing hiring and dismissal regulations for regular workers; and increasing the ability of and incentives for the non-employed to find jobs (Duval and Furceri, 2018). As pointed out by Bordon et al. (2018), reducing unemployment benefits may lower unemployment because this increases the cost of being unemployed. However, the effects of job protection reforms are ambiguous (Boeri et al., 2015) as layoffs are likely to rise in the short run if firing constraints are relaxed. Product market reforms can also have ambiguous short-run effects (Blanchard and Giavazzi, 2003). On the one hand, inefficient firms may be forced to exit the market due to more competition, while on the other hand, new entrants may invest more and create new jobs.

Quite a few studies have investigated the impact of structural reforms on unemployment (see Boeri et al., 2015, Parlevliet et al., 2018, and Campos et al., 2018 for reviews). A substantial part of previous research on the effects of structural reforms is based on simulations of Dynamic Stochastic General Equilibrium (DSGE) models. These models often feature monopolistic competition in both the goods and the labour markets. As a result, goods are priced with a mark-up over marginal costs and wages are characterized by a mark-up over the marginal rate of substitution between consumption and hours worked. Structural reforms are typically modelled as permanent negative shocks to mark-ups, representing more competition in product and labour markets (see, for instance, in't Veld et al., 2018). Alternatively, Cacciatore et al. (2016) consider a DSGE model with labour market search in which mark-ups depend endogenously on the number of firms in the markets. In this case, the effect of a reform aimed at improving competition is simulated assuming a reduction in entry costs which boosts entry and reduces mark-ups.

However, Campos et al. (2018: 27) argue against DSGE models posing that "A problem with this approach is that the simulations just confirm a priori beliefs: in most DSGE models, unemployment is voluntary. Structural reforms are interpreted as an intervention that changes the relative price of leisure versus labour (e.g., by reducing unemployment benefits). In addition, most DSGE models are based on calibrations, as acknowledged by all authors. They are not empirical evidence."

Other studies present estimates of the impact of structural reforms on (un)employment using panel or cross-section data.[1] For instance, Berger and Danniger (2007) report for a sample of OECD countries between 1990 and 2004 that lower levels of product and labour market regulation foster employment growth. The results of Griffith et al. (2007) suggest that the increase in competition due to product market reform leads to higher employment. Bouis et al. (2012a) find that unemployment benefit reforms (especially a reduction in unemployment benefit duration) boost employment. However, they also find some evidence that a reduction in the unemployment benefit replacement rate and job protection reforms can entail short-term losses in severely depressed economies. Bouis et al. (2012b) report similar results for the impact of reducing unemployment benefits. Bordon et al. (2018) investigate the impact of structural reforms on employment, controlling for endogeneity using local projections. Their results suggest that structural reforms have a lagged but positive impact on employment. This positive effect tends to be larger once the endogeneity of the decision to reform is taken into account. Both labour and product market reforms increase employment rates by about a little over one percentage point over 5 years. Duval et al. (2020) examine major reforms of job protection legislation for permanent workers covering 26 advanced economies over the period 1970–2013. The authors report that the short-term effects of job protection deregulation vary depending on prevailing macroeconomic conditions at the time of reform—they are positive in an expansion, but become negative in a recession.

In this paper, we examine the impact of labour and product market reforms on unemployment in 25 OECD countries for the 1970-2020 period. We use the local projections (LP) approach (Jordà, 2005) and reform indicators put together by Duval et al. (2018) and updates thereof as provided by Wiese et al. (2023).[2] LP has been widely used to analyze the dynamic effects of policy shocks (Jordà and Taylor, 2016; Alpanda et al., 2021; Thommen, 2022; Hülsewig and Rottmann, 2023). LP is a flexible alternative to vector autoregression models since it does not impose dynamic restrictions. To alleviate the bias caused by overlapping forecast horizons, we follow Teulings and Zubanov (2014) and include the leads of the reform dummies in our models. Reforms are likely to be endogenous. For example, reforms are more likely to be implemented during periods of economic crises (Drazen & Grilli 1993), while government ideology may also matter (Potrafke, 2010). However, most previous research does not take

[1] A few studies examine reforms in individual countries, like the Harz reforms in Germany, which aimed at reducing unemployment, by increasing working hour flexibility, job matching and work incentives. However, Bradley and Kügler (2019) conclude that although these reforms shortened the typical duration of unemployment, they did not reduce unemployment as a whole.

[2] Our data do not allow us to use a difference-in-differences (DiD) approach which is only a viable methodology when two types of groups are in the sample: 1. A treatment group that is eventually treated somewhere during the observed period. 2. A control group that is not treated in the observed period. We do not have a suitable control group since in all countries product market reforms took place, while Luxembourg is the only country without labour market reform. Furthermore, for most countries there are many reforms in the observed period. This invalidates a DiD approach. DiD also requires that no treatment must have taken place before the observed period, neither for the treatment nor the control group, a possibility that we cannot exclude.

this endogeneity properly into account. We control for the endogeneity of structural reforms using the Augmented Inverse Probability Weighted (AIPW) estimator proposed by Jordà and Taylor (2015), following Glynn and Quinn (2010).

The papers most closely related to our work are Bordon et al. (2018) and Duval et al. (2020). Bordon et al. (2018) investigate the impact of structural reforms on employment using OECD labour market reform indicators and the local projection approach, while controlling for endogeneity. However, unlike Bordon et al. (2018), who use the OECD reform indicators, we examine the impact of reforms on unemployment using the updated Duval et al. (2018) narrative reform indicators provided by Wiese et al. (2023). According to Duval and Furceri (2018), these indicators identify the exact timing of major legislative and regulatory actions by advanced economies since the early 1970s in key labour and product market policy areas. Furthermore, they capture reforms in areas for which OECD indicators exist but do not cover all relevant policy dimensions. Duval et al. (2020) also use the Duval et al. (2018) database and local projections, but these authors do not control for endogeneity of reforms. Furthermore, they focus on a subset of labour market reforms, whereas the present paper considers broader measures of both labour market and product market reforms.

In contrast to most previous research, we also examine the impact of structural reform for different types of unemployment (long-term versus short-term unemployment, unemployment across different age groups, and gender). Our paper also differs from these previous studies by not only providing evidence for 25 OECD countries but also zooming in on the subset of euro area countries. It is often argued that price and wage flexibility is particularly important in a currency union, as countries can no longer adjust to asymmetric shocks through exchange rate changes and the common monetary policy cannot take country-specific developments into account. This implies that a high degree of national economic flexibility is indispensable, notably so if the frequency of asymmetric shocks is high and countries' business cycles are not synchronized (and labour mobility and international risk sharing are low).[3] We examine whether structural reforms in the euro area have a different impact on unemployment than reforms in countries outside the euro area. A few previous studies addressed structural reform in the euro area as well. For instance, Rünstler (2021) uses a narrative dataset of reforms and panel VAR models to estimate the macro-economic effects of labour market reforms in 9 euro-area countries between 1998Q1 and 2018Q4 and reports that such reforms increase employment, but only lead to a temporary decline in unemployment. Our paper employs LP and a different reform database, while we also consider product market reforms.

Our findings suggest that labour market reforms reduce unemployment. Whereas in the LP model this effect becomes statistically significant only after 4 years, when labour market reforms are endogenized the effect becomes statistically significant within a year. For youth unemployment it takes

[3] Several studies have examined whether European integration has stimulated structural reform. Campos et al. (2020) observe that the Single Market and the euro fostered reforms in product but not in labour or financial markets. Likewise, Bednarek et al. (2010) report that European monetary integration did not induce labour market reform.

longer for the effect of labour market reform to set in, while the effect of labour market reform on female unemployment remains statistically significant throughout the forecasting horizon, while the effect on male unemployment becomes insignificant after three years. The AIPW results suggest that product market reforms, for some time, increase unemployment, in contrast to the findings based on the simple LP model. Our findings for non-euro area countries are consistent with those for the full sample. However, our results suggests that labour market reforms hardly have a significant effect on unemployment in the euro area. We also find that labour market reforms do not affect unemployment when the economy is below trend. Furthermore, the effects of structural reforms on unemployment are stronger for high collective bargaining coverage observations.

The remainder of the paper is organized as follows. Section 2 discusses the data used. Section 3 outlines our methodology, while section 4 presents our main findings. Section 5 offers a robustness analysis, while section 6 concludes.

2. Data and stylized facts[4]

Structural reform refers to major policy changes in product market regulation and employment protection legislation for regular workers. These are the kind of reforms routinely advocated by think tanks and international organizations such as the IMF and the OECD (see, for example, IMF, 2016). Major reforms of product and labour market regulation are identified by Duval et al. (2018) and updated until 2020 by Wiese et al. (2023), using documented legislative and regulatory actions reported in all available *OECD Economic Surveys* for 25 advanced economies, as well as additional country-specific sources.[5] The approach also considers both reforms and "counter-reforms"—i.e., policy changes in the opposite direction. For each country, our reform variable in each area takes value 0 in non-reform years, 1 in reform years, and -1 in counter-reform years. Labour market reforms can be split into employment protection legislation (EPL) reforms and unemployment benefits (UB) reforms. The former capture that it becomes easier to fire employees, while the latter capture reductions in the level of unemployment benefits.

The reform database has several advantages as it identifies: the precise nature and exact timing of major legislative and regulatory actions in key labour and product market policy areas and the precise reforms that underpin what otherwise looks like a gradual decline in OECD policy indicators without any obvious or noticeable break (for example, the series of reforms that took place in the telecommunications industry in many countries in the mid-late 1990s). Furthermore, the database captures reforms in areas for which OECD indicators exist but do not cover all relevant policy dimensions and documents and describes the precise legislative and regulatory actions that underpin

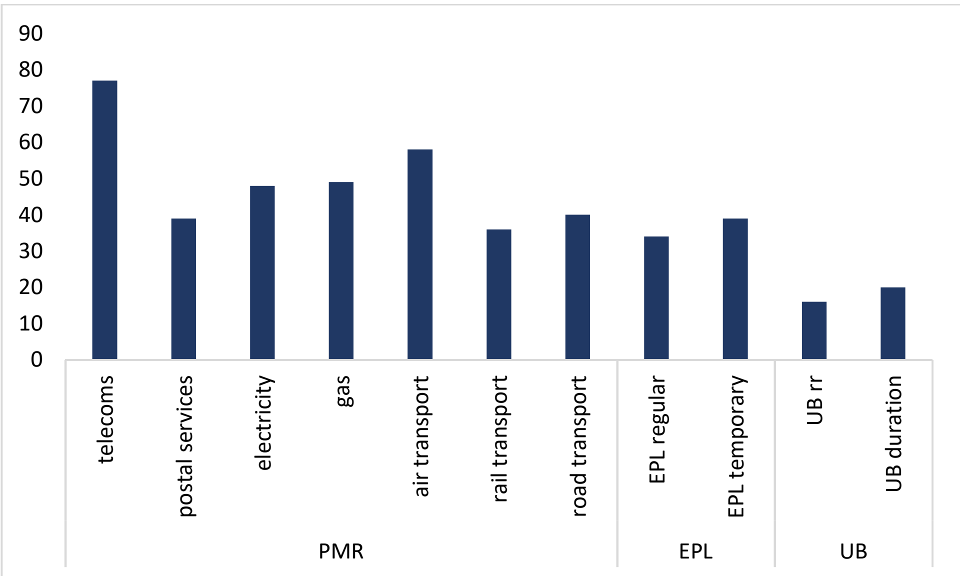
[4] This section heavily draws on Wiese et al. (2023).

[5] The 25 countries covered are Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States.

observed large changes in OECD indicators. Finally, compared with other existing databases on policy actions in the area of labour market institutions, such as the European Commission's *Labref* or the ILO's *EPLex* database, the approach taken by Duval et al. (2018) and Wiese et al. (2023) allows identifying a rather limited set of major legislative and regulatory reforms, as opposed to just a long list of actions that in some cases would be expected to have little or no bearing on macroeconomic outcomes. This is particularly useful for empirical analyses that seek to identify, and then estimate, the dynamic effects of reform shocks. The strengths of this narrative reform database come with one limitation; because two large reforms in a given area (for example, employment protection legislation) can involve different specific actions, like a major simplification of the procedures for individual and collective dismissals), only the average impact across major historical reforms can be estimated.

Table 1 presents stylized facts on reforms—that is, decreases in regulation—and counter-reforms—that is, increases in regulation. The latter are relatively rare events in product markets, while they can account for up to 25% of total shocks in the labour market. Figure 1 provides the number of reforms identified in the sample and illustrates the heterogeneity of reform efforts across regulatory areas. Product market reforms (PMR) have been more frequently implemented, in particular in telecommunications and air transport. In general, fewer major reforms have been implemented in the areas of employment protection legislation for regular workers. The vast majority of product and labour market reforms in our sample were implemented during the 1990s and the 2000s (see Figure 2 for details on labour market reforms). Exceptions are reforms in the area of rail transport, which were also undertaken in the 1980s.

Fig. 1. Number of reforms by area (25 advanced economies, 1970-2020)



Source: Wiese et al. (2023).

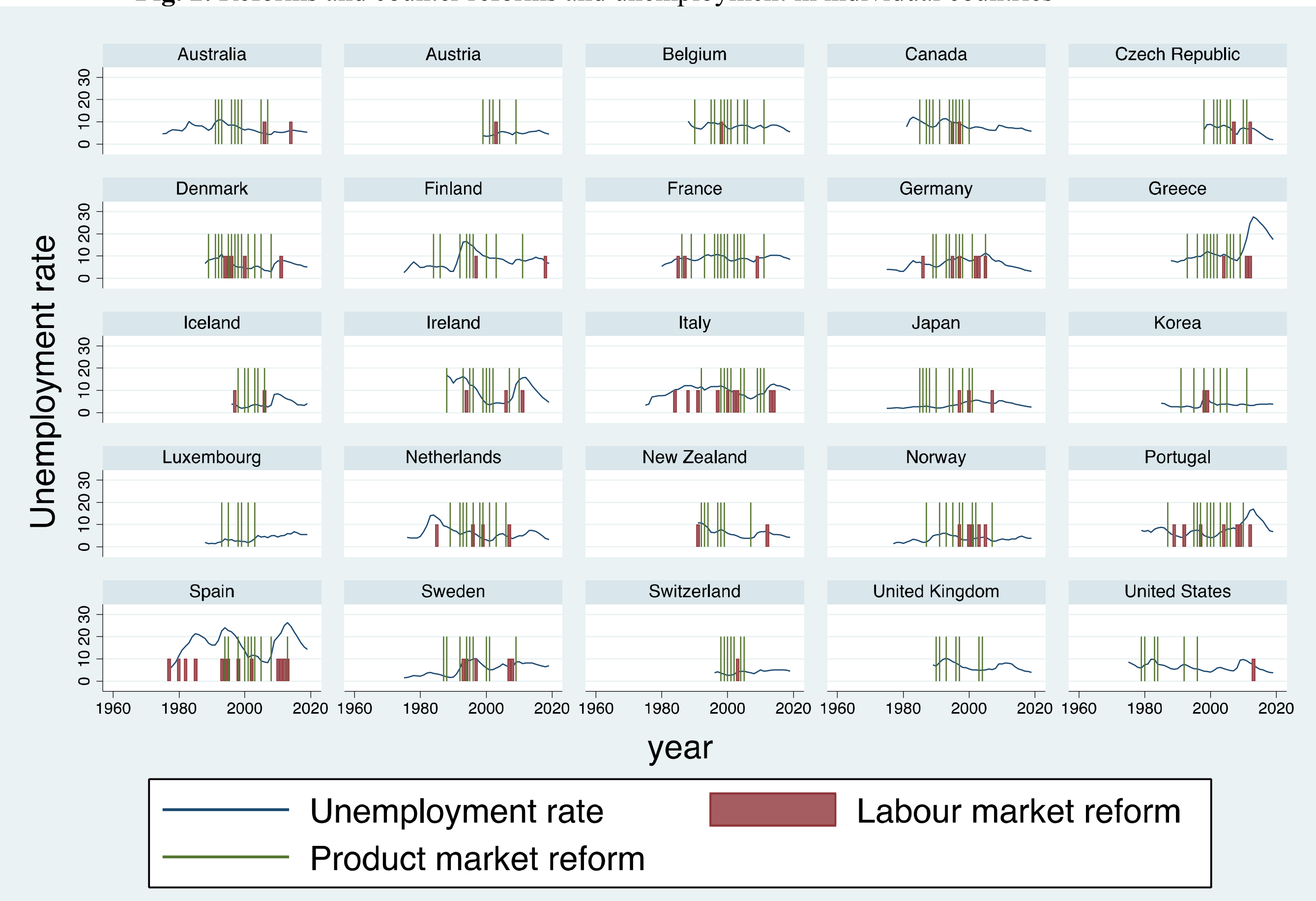
Table 1. Number of reform categories (25 advanced economies, 1970-2020)

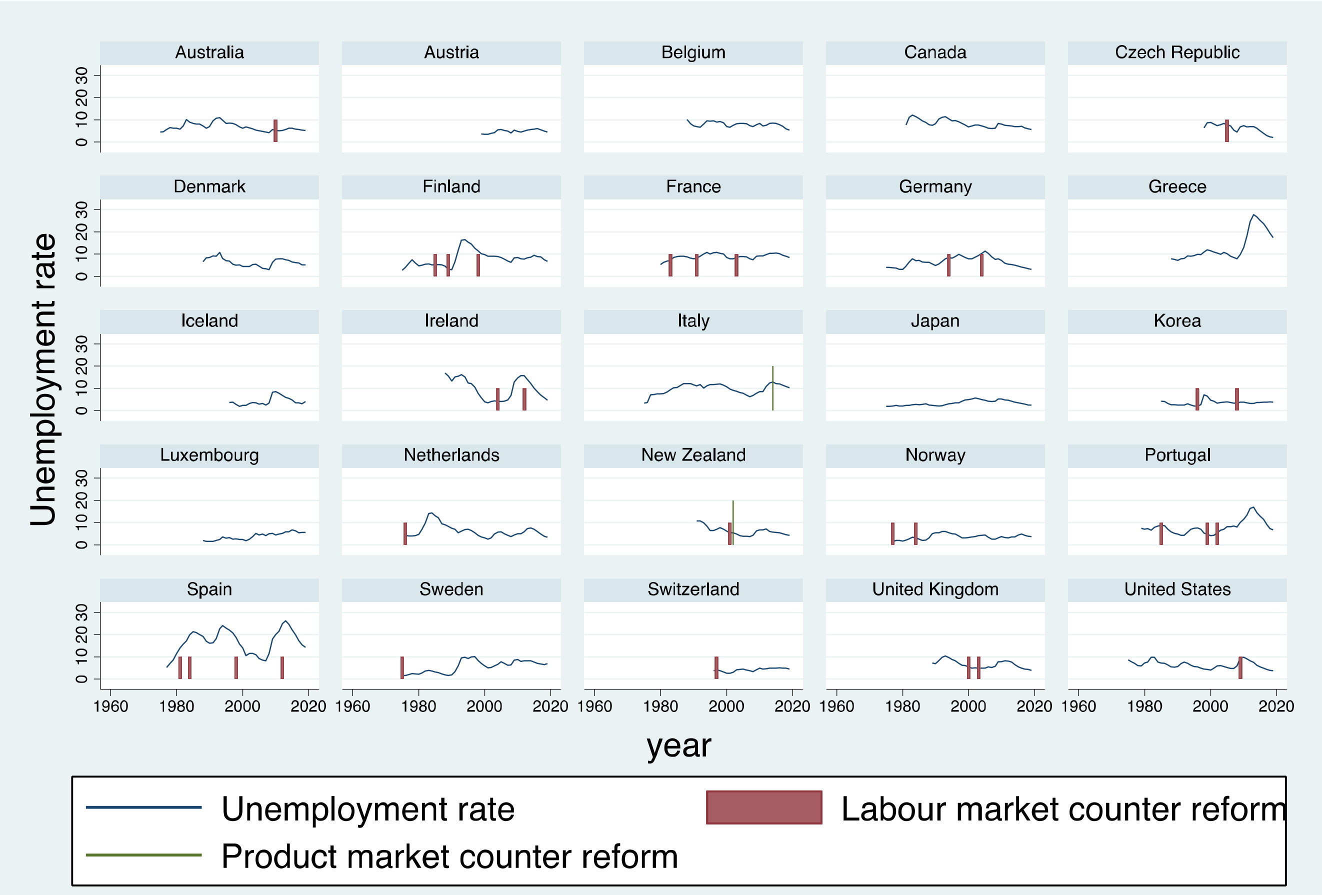
Reform type	**Number of reforms**	**Number of counter reforms**	**Reforms (% of total)**	**Counter-reforms (% of total)**
Product market reforms	224	2	99.1	0.9
Labour market reforms	84	30	73.4	26.6
Employment protection legislation (EPL) reforms	60	21	74.1	25.9
Unemployment benefit (UB) reforms	24	9	72.7	27.3

Note: The total number of observations is 911 (based on the 1-year forecast estmation sample).

Figure 2 shows the level of unemployment (taken from the OECD) and labour market reform in the 25 countries in our sample.

Fig. 2. Reforms and counter reforms and unemployment in individual countries





Note: The total number of observations is 911 (based on 1-year forecast estmation sample).

3. Methodology[6]

3.1 Local Projections

Structural reforms—in any area—tend to have evolving effects over an extended period of time. We estimate impulse response functions (IRFs) by applying Jordà´s (2005) LP method. This LP approach has been advocated by Auerbach and Gorodnichenko (2012, 2013) and Romer and Romer (2019) as a flexible alternative to Vector Autoregressions and/or distributed lag models. The LP approach is also flexible to accommodate a panel structure and does not constrain the shape of IRFs, thereby allowing to analyse different types of policy shocks (Auerbach and Gorodnichenko, 2013; Jordà and Taylor, 2016; Ramey and Zubairy, 2018; Romer and Romer, 2019; Born et al., 2020).

Given the panel data nature of our data, we prefer the LP method over commonly used VAR models for the following reasons. First, we employ a large panel dataset with a constellation of fixed effects, which makes a direct application of standard VAR models more difficult. Second, under the LP method only equations for the variables of interest have to be estimated, thereby significantly economizing on

[6] This section draws on de Haan and Wiese (2022).

the number of estimated parameters. Moreover, lag augmentation prevents the need to correct standard errors for serial correlation in the regression residuals. Hence, local projection inference is more robust than standard VAR inference, whose validity depend sensitively on the persistence of the data and on the length of the forecast horizon (Olea and Plagborg-Møller, 2021). Third, although local projection estimates are asymptotically identical to VAR estimates (Plagborg-Møller and Wolf, 2021), lag-augmented local projections, as in our case, are asymptotically valid over both stationary and non-stationary data over a wide range of forecast horizons. Fourth, the LP method is particularly suited to estimating non-linearities (for example, how the effect of structural reform shocks depends on the business cycle or collective bargaining coverage), as its application is much more straightforward compared to non-linear structural VAR models, such as Markov-switching or threshold-VAR models.[7] Moreover, it allows for incorporating various time-varying features of source (recipient) economies directly and allow for their endogenous response to structural reform shocks.

The basic LP regression model that we estimate takes the following form:

$$\ln U_{i,t+h} - ln U_{i,t} = \alpha_i + \beta_{1jh} \sum_{j=0}^{5} d_{i,t-j} + \beta_{2lh} \sum_{l=0}^{4} \left(ln U_{i,t-l} - \ln U_{i,t-1-l} \right) + \beta_{3h} \sum_{h=1}^{h} d_{i,t+h} + \beta'_{4ch} \sum_{c=0}^{1} X_{i,t-c} + \delta_t + u_{i,t+h} \quad (1)$$

where U denotes the unemployment rate[8]; h is the forecast horizon set at 1 to 7 years, since the effect of reforms can take time to materialize. Time and country fixed-effects, α_i and δ_t, respectively, are included. $d_{i,t}$ denotes the reform indicators (or, in one of the sensitivity analyses, the labour market counter reform indicator). We use information criterion (AIC and BIC) to determine the number of lags: we employ 5 lags of the reform indicator and 5 lags of the dependent variable. Since previous reforms may impact unemployment ahead in time, we include the number of leads of the treatment indicator equal to the forecast horizon such that the term $\beta_{3h} \sum_{h=1}^{h} d_{i,t+h}$ captures the Teulings and Zubanov (2014) correction. Including the leads avoids the bias that results from overlapping forecast horizons.[9] $X_{i,t}$ is a vector of additional control variables, which includes the contemporaneous and first lag of real GDP growth, the output gap (calculated with the HP filter, lambda=100), and the annual percentage change in the consumer price index (from OECD). These variables affect the results and their coefficients are significant in most regressions. The error term in the following panel estimations is

[7] See Miyamoto et al. (2019) for a recent application of local projections to the estimation of non-linearities and interaction effects of shocks using a large panel dataset, as it is the case with our sample.

[8] Fischer type panel stationarity test suggest that the unemployment rates are stationary, so the change in them will also be stationary. However, as shown in Figure A1 in the online Appendix, the distribution of the level of the unemployment rate is very skewed. We therefore use the log of the unemployment rate, so when we take differences in the regressions, we therefore are estimating the percentage change in the unemployment rate.

[9] The bias increases with the forecast horizon, see Teulings and Zubanov (2014). The leads of the treatment dummies ensure that it is registered in the data if the outcome for a specific observation is affected by a treatment ahead in time. This most often is the case for control observations, i.e., country-year pairs where no reform took place. However, reforms may occur repeatedly within our forecast horizon of 7 years. In that case, the Teulings and Zubanov (2014) approach also registers that the outcome of a treated observation may be affected by later treatments, which otherwise would have meant an upward bias in the effect of reforms.

likely to be correlated across countries. This correlation would be difficult to address in the context of VAR models, but it is easy to handle in the LP method by either clustering standard errors or using the Spatial Correlation Consistent (SCC) standard errors (Driscoll-Kraay, 1998) (as we do) allowing for arbitrary correlations of the errors across countries and time.

All in all, we have 911 observations when we project one period ahead, this decreases with 25 observations for each additional year-ahead forecast. Thus, when we forecast 7 years ahead, we end up with 761 observations.

In our robustness analysis, we examine whether the effect of structural reforms on unemployment depends on the state of the business cycle and the level of collective bargaining, as some previous research suggests (cf. Bouis et al., 2012a; Duval et al., 2020; Schnabel, 2020). For this purpose, we examine whether the effect of reforms is different for positive ('boom') and negative output gaps ('slump'), or whether the level of collective is above or below the sample median. The estimated model then becomes:

$$\begin{aligned} \ln U_{i,t+h} - lnU_{i,t} = I_{i,t}^{boom} \left[\alpha_i + \beta_{1jh} \sum_{j=0}^{5} d_{i,t-j} + \beta_{2lh} \sum_{l=0}^{4} \left(lnU_{i,t-l} - \ln U_{i,t-1-l} \right) + \beta_{3h} \sum_{h=1}^{h} d_{i,t+h} + \beta_{4ch}' \sum\nolimits_{c=0}^{1} X_{i,t-c} + \delta_t \right] \\ +(1 - I_{i,t}^{boom}) [\alpha_i + \beta_{1jh} \textstyle\sum_{j=0}^{5} d_{i,t-j} + \beta_{2lh} \sum_{l=0}^{4} \left(lnU_{i,t-l} - \ln U_{i,t-1-l} \right) + \beta_{3h} \sum_{h=1}^{h} d_{i,t+h} + \beta_{4ch}' \sum_{c=0}^{1} X_{i,t-c} + \delta_t] + e_{i,t+h} \end{aligned} \tag{2}$$

3.2 AIPW model

The major drawback of equation (1) is that it ignores that the likelihood that structural reforms are introduced in countries/years where the expected benefits of reform is higher than in countries/years where no reforms are introduced. Failing to account for this can lead to selection bias. Following de Haan and Wiese (2022), we therefore proceed with a quasi-experimental method, namely the Augmented Inverse Probability Weighted (AIPW) estimator proposed by Jordà and Taylor (2015) and Glynn and Quinn (2010).

In the first step, we estimate logit models to estimate the probability of product market and labour market reforms one period ahead. As controls we use: the other reform indicator, the output gap, real GDP growth, the employment rate, inflation rate, and the lag of these economic variables. By including labour market reforms as predictor of product market reforms in t+1, and vice versa for labour market reforms, we control for the possibility that labour and product market reforms may be related (Fiori et al., 2012). The output gap, GDP growth rates, the unemployment rate and the inflation rate capture the idea that reforms are more likely to occur after times of economic crisis (Drazen and Grilli, 1993). We also include ideology of government (capturing the idea that the political colour of a government determines policies; Hibbs, 1977), political fragmentation of government and the effective number of parties in government (capturing the idea that more (politically) fragmented governments may find it harder to implement economic reforms; Alesina and Drazen, 1991), years in office (as reforms become less likely the longer a government holds office; Haggard and Webb, 1993), and (legislative and executive) elections (capturing the idea that reforms are less likely close to elections; Alesina et al., 2006).[10] We also include the 3rd degree polynomial of the time since the previous reform to handle duration dependence and time and country fixed effects despite of the incidental parameter problem in the logit model.

In the second step, we use local projections, but weighing observations inversely according to the predicted probabilities from the logit model. Specifically, observations in which a reform took place are assigned a weight (*w*) by the inverse of *p,* the probability score, (*w=1/p*). Whereas the observations without reform receive a weight of the inverse of one minus the probability score (*w=1/(1-p)*). This places more weight on observations that are comparable and hence reduces treatment selection bias. The augmented weighting adds an adjustment factor to the treatment effect when the estimated probability scores are close to zero or one. The method is doubly robust and only requires one of the following two conditions to hold: The conditional mean model is correctly specified or the probability score model is correctly specified. Weighting can be interpreted as removing the correlation between the covariates and the reform indicator, and regression removes the direct effect of the covariates (see Imbens and Wooldridge, 2009 for more details). Furthermore, separate conditional mean (OLS) models are estimated for the treated and the non-treated observations. This means that we do not assume that the effect of the covariates on the outcome is identical in the treated and non-treated group, as it is implicitly assumed in a simple LP setup (Jordà and Taylor, 2016). We report the Average Treatment Effect (ATE), which is calculated as the average difference between treated and non-treated (control) observations based on the weighted OLS regression line for both groups.

In the second stage AIPW regressions, we use the same specification as in equation (1). However, to correct for the imported uncertainty from the first stage propensity score estimation in the second stage, we calculate block-bootstrapped standard errors in our AIPW models. That is, we construct the bootstrap by repeatedly drawing blocks of observations, i.e., drawing countries rather than individual observations with replacement. This way, serial correlation in the error terms is also taken into account. First, we test whether spatial dependence is present in the disturbances between the cross-sectional units when using standard errors clustered at the country level. For this purpose, we use the Pesaran (2015) test, which is standard normally distributed. So, a value of the test statistic outside the [-1.96, +1.96] interval rejects the null hypothesis of weak cross-sectional dependence. Although the tests sometimes reject the hypothesis, we use the cluster-bootstrapped errors since cross-sectional dependence does not bias our point estimates; it only leads to an efficiency loss (see Elhorst, 2013).

[10] See Table A.1 in the Appendix for a description of the variables used.

4. Empirical results

4.1 Baseline LP results

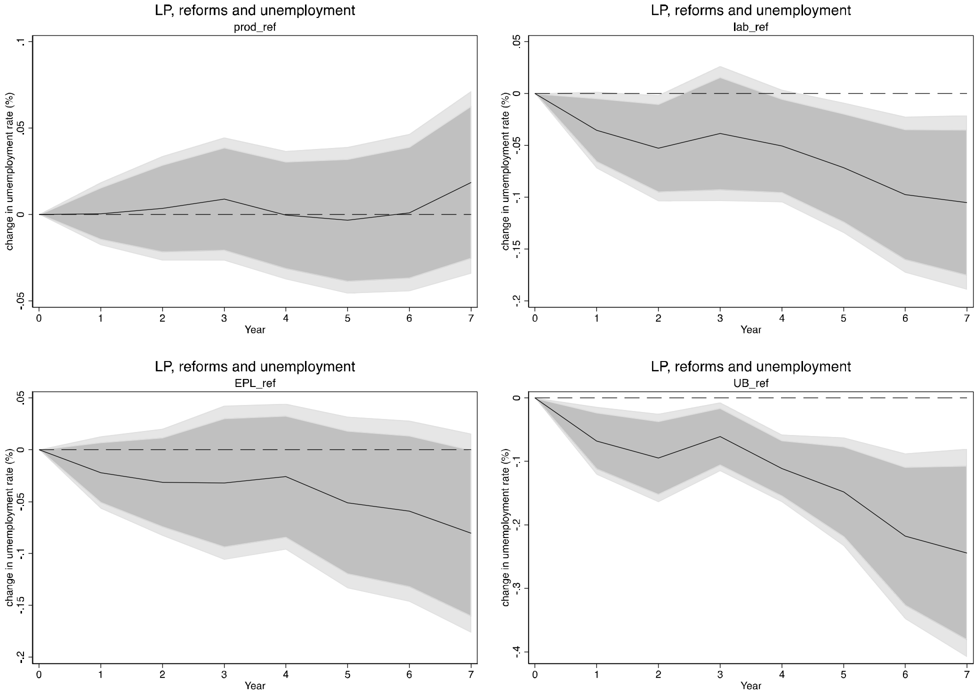
We begin the analysis with the baseline unconditional responses of unemployment to the different structural reforms. Although these LP results are likely to be biased because of reform selection, we use them as benchmark for the AIPW models (to show the severity of the bias), but also to perform specification tests that cannot be conducted in the AIPW models. Figure 3 plots the response of unemployment to structural reform as a black line together with the 90 and 95 percent confidence bands in dark and light grey, respectively. Our results suggest that a product market reform does not significantly affect unemployment. In contrast, labour market reforms are followed by a decrease in unemployment with the point estimates becoming statistically significant after four years. The graphs in the lower part of Figure 3 show the outcomes if we distinguish between two types of labour market reforms, namely EPL and UB reforms. The results suggest that the unemployment-reducing effect of labour market reforms is notably due to UB reforms; in fact, EPL reforms do not have a statistically significant impact on unemployment.

Canova (2022) shows that when the dynamic evolution of cross-sections is not homogeneous, the implied estimates are biased in terms of both magnitude and effects' propagation. We obtain similar results when accounting for dynamic heterogeneity (Pesaran, 2006; Canova, 2022): results for the (simple and weighted) averaged country estimates compare well with the panel estimates, indicating that any bias due to dynamic heterogeneity is small (at the 5% significance level).[11] Including all of the cross-sections in the case of heterogeneity would distort the average dynamic effects of interest.

Next, we estimate the LP model for euro-area (EA) countries and non-euro-area countries. The results are shown in Figure 4. While our findings for non-EA countries are consistent with our full sample results, the findings for EA countries change quite remarkably. In this subsample, product market reforms lead to a higher level of unemployment, whereas (both types of) labour market reforms do not affect unemployment. These results therefore do not support the view by the ECB (2014) that structural reforms will reduce unemployment in the EA. But before we draw strong conclusions, we consider possible endogeneity of reforms.

[11] As Canova (2022) suggests for sufficiently large T, regardless of whether spatial dynamics are homogeneous or not, the proposed approach employs time series variations within a unit to measure the dynamic effect of the policy innovation at horizon h, separately for each i. The cross-sectional mean of unit-individual estimates provides an estimate of the typical effect, and this estimate will be consistent for any h for N large. This is a time series counterpart of the cross-sectional estimate derived from estimating equation (1). We constructed both a simple and a weighted average of individual estimates where in the latter case weights were given by the number of observations available in each individual time-series estimation for each horizon h. Results of the Canova-type test (simple average; weighted average not shown) for the null hypothesis of equality in coefficients are displayed at the bottom of Tables A2-A5 in the Appendix for completeness. The individual country-specific estimates for each horizon h for the four categories of reforms as displayed in Figure 3 are available upon request.

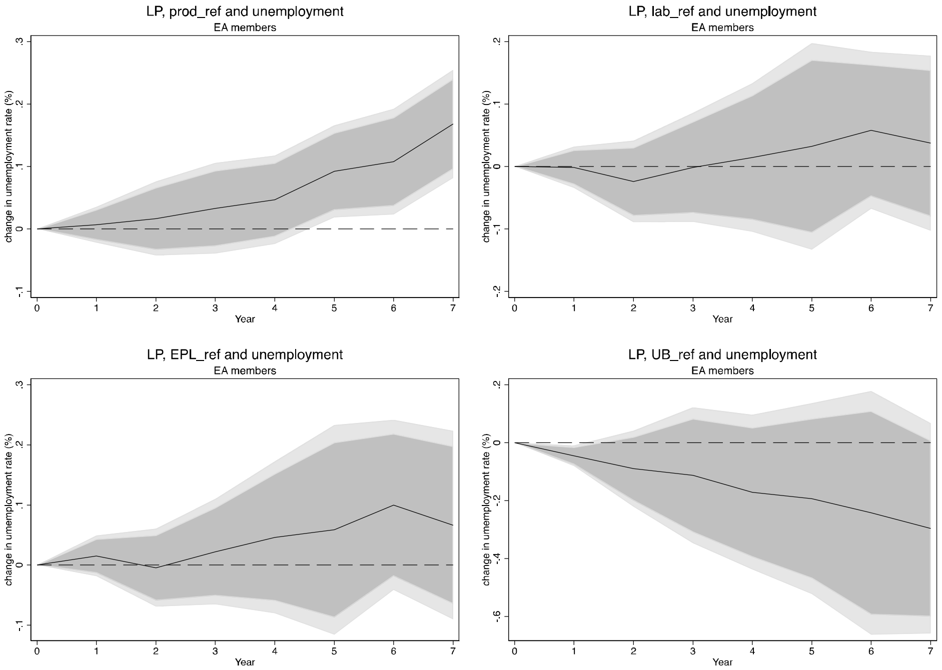
Fig. 3. Unconditional Local Projections: effect of product and labour market reforms on unemployment: full sample



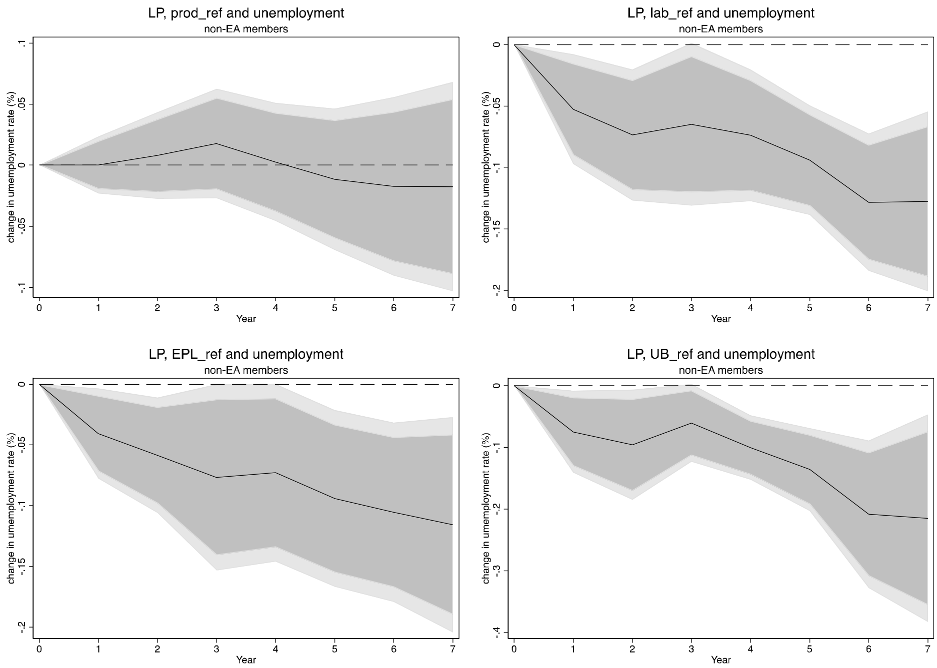
Notes: The solid black lines in the figure plots the impulse responses of product market (upper left panel) and labour market (upper right panel) reforms on unemployment. The panels in the lower part show impulse responses for EPL (left panel) and UB (right panel) reforms. Year=1 is the first year after a reform took place at year=0. So, the position of the line at e.g., year=7 shows the change in unemployment 7 years after the reform. The dark grey shaded areas display the 90% SCC error bands; the light grey shaded areas display the 95% SCC error bands. The underlying regressions are shown in Tables A2-A5 in the Appendix.

Fig. 4. Local Projections: effect of product and labour market reforms on unemployment: EA countries versus non-EA countries

EA countries



Non-EA countries



Notes: See notes to Figure 3.

4.2 Endogenizing reforms

In an ideal Randomized Controlled Trial (RTC) setting where treatments are assigned randomly, we would expect the probability density function for each control variable included in equation (1) to be the same for each sub-population of treated and control units. The overlap of the densities should be close to perfect. For example, the distribution of the output gap should be similar for the subpopulation where a major product market reform takes place and the subpopulation of all other (control) observations. A simple way to check whether this condition holds is to do a test of equality of means between the subsamples. This is done in Table 2. As evident, in the full sample, the balance for several variables between treated and control observations is a cause of concern. This is an indication that we cannot assume that treatments are assigned randomly as is done in the simple LP analysis above. This suggests that labour and product market reforms cannot be viewed as exogenous events. When we split the sample, there is clear evidence for selection bias in EA countries, but less so in non-EA countries.

Table 2. Balancing tests of covariates: Reforms

					Full sample				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Output gap	Output gap (-1)	Inflation	Inflation (-1)	GDP growth	GDP gr. (-1)	Unemployment	Unemploym. (-1)	Unemploym. (-2)
Product market	-0.010	-0.018	-1.043***	-1.152***	0.005**	0.005**	-0.011	-0.006	-0.010
reforms	(0.016)	(0.016)	(0.296)	(0.315)	(0.002)	(0.002)	(0.012)	(0.012)	(0.013)
Labour market	-0.060**	-0.031	-0.191	-0.298	-0.007**	-0.007**	0.034*	0.050***	0.069***
reforms	(0.023)	(0.024)	(0.443)	(0.472)	(0.003)	(0.003)	(0.018)	(0.018)	(0.019)
EPL reforms	-0.046*	-0.006	0.214	0.239	-0.009**	-0.009**	0.049**	0.057***	0.058***
	(0.027)	(0.028)	(0.517)	(0.550)	(0.003)	(0.003)	(0.021)	(0.022)	(0.022)
UB reforms	-0.085**	-0.088**	-1.135	-1.546*	-0.002	-0.003	-0.006	0.027	0.087***
	(0.042)	(0.043)	(0.800)	(0.851)	(0.005)	(0.005)	(0.033)	(0.033)	(0.034)
Joint reforms	0.001	-0.001	3.517***	3.745***	-0.018***	-0.018***	0.004	0.007	0.009*
	(0.007)	(0.007)	(0.130)	(0.138)	(0.001)	(0.001)	(0.005)	(0.005)	(0.005)
Obs.	911	911	911	911	911	911	911	911	911
					EA countries				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Product market	0.083**	0.070**	0.887***	0.554***	0.008*	0.010**	-0.002	-0.006	-0.052**
reforms	(0.035)	(0.035)	(0.179)	(0.184)	(0.004)	(0.004)	(0.021)	(0.021)	(0.020)
Labour market	-0.103**	0.000	0.187	0.491*	-0.020***	-0.017***	0.080***	0.074**	0.093***
reforms	(0.049)	(0.049)	(0.265)	(0.263)	(0.006)	(0.006)	(0.029)	(0.029)	(0.029)
EPL reforms	-0.084	0.022	0.190	0.726**	-0.022***	-0.019***	0.094***	0.075**	0.069**
	(0.054)	(0.054)	(0.291)	(0.286)	(0.006)	(0.007)	(0.031)	(0.032)	(0.032)
UB reforms	-0.161	-0.088	0.141	-0.561	-0.010	-0.002	0.010	0.054	0.174***
	(0.110)	(0.109)	(0.589)	(0.586)	(0.013)	(0.014)	(0.064)	(0.065)	(0.064)
Obs.	250	250	250	250	250	250	250	250	250
					Non-EA countries				
Product market	-0.031	-0.026	0.594	0.375	-0.000	-0.002	0.027	0.050*	0.055*
reforms	(0.031)	(0.032)	(0.718)	(0.763)	(0.004)	(0.004)	(0.027)	(0.028)	(0.028)
Labour market	-0.043*	-0.048*	-0.186	-0.463	-0.000	-0.003	0.014	0.041*	0.060**
reforms	(0.026)	(0.026)	(0.596)	(0.633)	(0.003)	(0.003)	(0.023)	(0.023)	(0.023)
EPL reforms	-0.031	-0.026	0.594	0.375	-0.000	-0.002	0.027	0.050*	0.055*
	(0.031)	(0.032)	(0.718)	(0.763)	(0.004)	(0.004)	(0.027)	(0.028)	(0.028)
UB reforms	-0.062	-0.085*	-1.678*	-2.036*	-0.000	-0.004	-0.012	0.019	0.062
	(0.044)	(0.044)	(0.999)	(1.061)	(0.005)	(0.005)	(0.038)	(0.039)	(0.039)
Obs.	661	661	661	661	661	661	661	661	661

Notes: Each row is the result of a regression where each variable in columns 1-9 has been regressed on a dummy for reform, equal to 1 if a reform took place. Robust Standard errors were used but not reported: *** p<0.01, ** p<0.05, * p<0.

When policy interventions like labour and product market reforms are driven by endogenous responses to control variables, the observed treatment and control units can be viewed as being oversampled from the part of the distribution in which the propensity score of treatment reaches high values. The simple local projections presented in Figures 3 and 4 are based on the sampled distribution and will therefore be biased. Too much weight is given to treated observations with a high probability of treatment and too little weight is given to control observations with a high probability of treatment. Inverse weighting using propensity scores shifts the probability mass away from the oversampled region of the distribution towards the under-sampled region. This shift rebalances the sample such that we can view the re-weighted sample as reconstructing the true distribution of outcomes under treated and control observations. In other words, we can view the rebalancing as if we had observed a random sample for each group, unaffected by endogenous responses to control variables. Thus, the regression for both the control group and the treatment group are less susceptible to bias and their difference can be used to calculate an unbiased estimated of the ATE of reforms on economic growth (see Imbens and Wooldridge, 2009 and Jordà and Taylor, 2015 for more details).

Table 3 shows the first-stage regressions. In the logit model for labour market reforms, we also add institutional variables capturing the strictness of hiring and firing conditions for workers on temporary or regular contracts. This takes a level effect into account, as countries with very flexible hiring and firing conditions are typically less likely to reform the labour market (Turrini et al., 2015).

The models in Table 3 have a high predictive ability: the ‘area under the ROC curve’ is almost 0.9 and is statistically significantly different from 0.5. The graphs in Figure 5 provide smooth kernel density estimates of the distribution of the propensity scores for treatment and control units to check for overlap. In the ideal RCT setting, the overlap between the distribution of propensity scores for treated and control units would be near identical. The plotted density of product market reforms is based on column (1) in Table 5, while that of labour market reform is based on column (2). The graphs in Figure 5 make clear that we have considerable overlap between the distributions for treated and control units. This indicates that we have a satisfactory logit model that can be used to identify the ATEs properly using our quasi-experimental estimation strategy. Unfortunately, we cannot estimate sensible first stage logit models when we split the labour market reforms in EPL and UB reforms (results available on request). Therefore, we continue with the AIPW for the overall reform indicators only.

Fig. 5. Overlap of propensity scores for different types of reforms

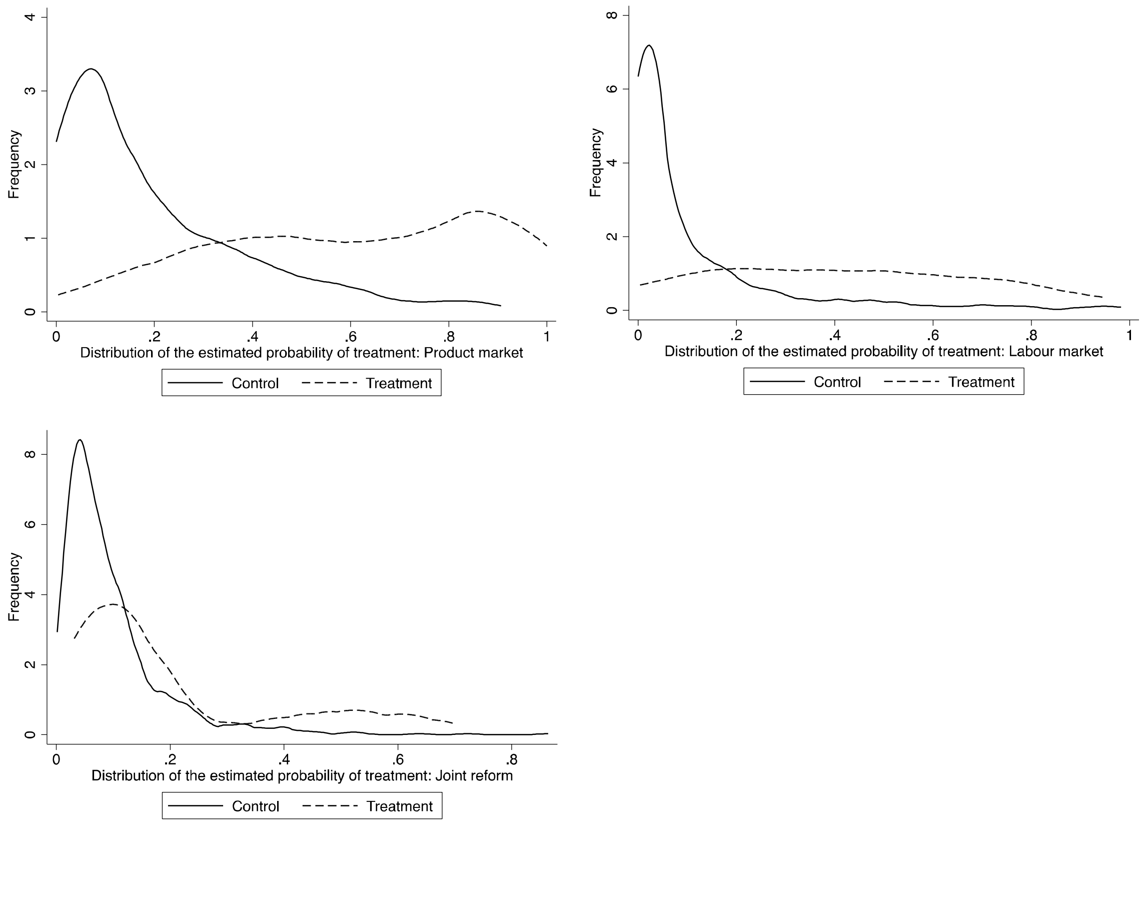


Table 3. Logit for the propensity scores of treatments in t+1, marginal effects

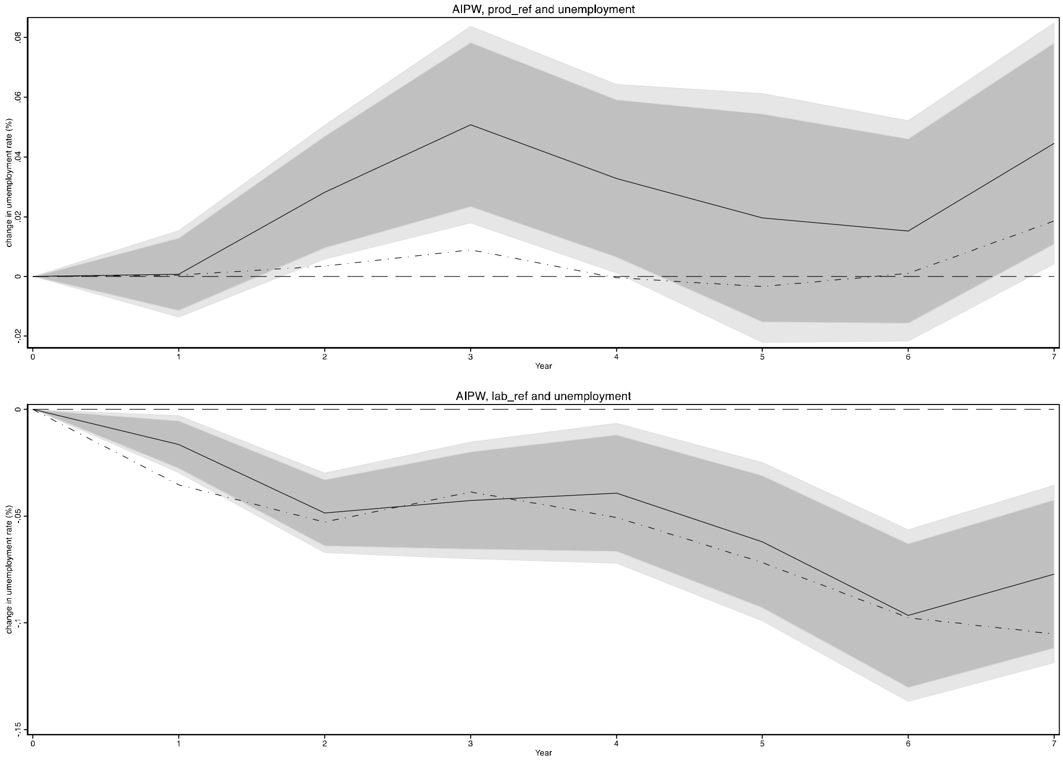
VARIABLES	(1) Product market reform	(2) Labour market reform	(3) Joint reforms
Labour mark. reform	0.063		
	(0.048)		
Product mark. reform		0.027	
		(0.028)	
Output gap	0.159	0.865**	0.150
	(0.307)	(0.336)	(0.171)
Output gap (-1)	-0.352	-0.524	-0.070
	(0.337)	(0.365)	(0.192)
Inflation rate	-0.012	-0.005	-0.002
	(0.012)	(0.013)	(0.008)
Inflation rate (-1)	-0.024**	-0.001	-0.007
	(0.012)	(0.011)	(0.007)
GDP growth	-2.649	-5.689**	-0.575
	(2.398)	(2.460)	(1.210)
GDP growth (-1)	-0.622	-0.202	1.016
	(1.239)	(1.254)	(0.747)
Unemployment rate	0.020	0.036*	0.034**
	(0.022)	(0.019)	(0.014)
Unemployment rate (-1)	-0.042	0.001	-0.005
	(0.034)	(0.029)	(0.022)
Unemployment rate (-2)	0.009	-0.006	-0.024*
	(0.021)	(0.017)	(0.013)
Government ideology	0.047**	-0.009	-0.012
	(0.021)	(0.018)	(0.012)
Political fragmentation	0.054	-0.046	-0.100**
	(0.078)	(0.065)	(0.047)
Government yrs. in office	0.009	-0.019***	-0.004
	(0.006)	(0.006)	(0.004)
Effective number parties	-0.052	0.087**	0.038**
	(0.048)	(0.035)	(0.017)
Elections	-0.022	-0.006	0.002
	(0.032)	(0.027)	(0.022)
Strictness of temporary employment protection		0.061** (0.024)	0.015 (0.010)
Strictness of regular employment protection		0.116* (0.066)	0.028** (0.013)
Time and country FEs	Yes	Yes	No
3^{rd} degree polynomial of time since last reform	Yes	Yes	Yes
Observations	702	603	786
Area under ROC curve	0.873	0.884	0.747

Notes: The table reports the marginal effects at the means of a logit specification to predict the probability of treatment in t+1. In model 3 treatment is defined as observations in which both a product market reform and a labour market reform occurred simultaneously, there are 23 treatments in that case. As a consequence, we not include the fixed-effect in the first stage logit model for the joint reforms as we lose too many observations when including them. Robust standard errors are shown in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

4.3 Quasi experimental results

Figure 6 shows the AIPW results for the full sample of countries (underlying regression results are shown in the first panels in Tables A6 and A7). The results for labour market reforms confirm the outcomes of the simple LP model: this type of reform reduces unemployment. Whereas in the LP model the effect became significant after 4 years, when reforms are endogenized the effect becomes significant within a year. The AIPW results for product market reform suggest that these reforms for some time increase unemployment, thus differing substantially from findings based on the simple LP model. So, our results suggest that it is crucial to take endogeneity of structural reform into account when analysing the effects of structural reform on unemployment.

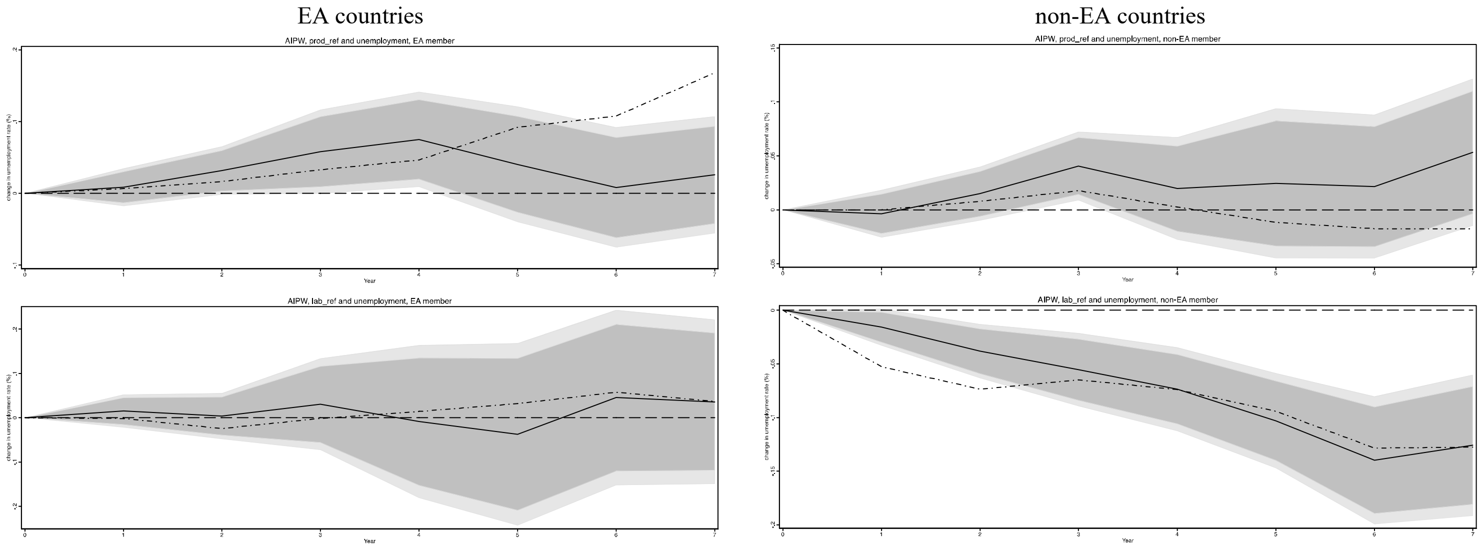
Fig. 6. AIPW results: Full sample



Notes: The solid black lines in the figure plots the impulse responses of product market (upper panel) and labour market (lower panel) reforms on unemployment. The dark grey shaded areas display the 90% error bands, the light grey shaded areas display the 95% error bands. The dotted-dashed line displays impulse responses from the simple LP regressions.

Figure 7 shows the results for the subsamples of EA countries and non-EA countries. Because of lack of degrees of freedom, we had to exclude the time fixed effects in the outcome regressions. Also, we truncated the propensity scores due to the low number of treated observations which makes the estimator highly sensible to a few treated observations with high propensity scores. We truncated at 0.9 and 0.1 such that all propensity scores above or below these numbers get assigned 0.9 or 0.1. For consistency reasons, we do this for both the product and labour market reforms. As with the LP results, our findings for the sample of non-EA countries are consistent with our full sample AIPW results: labour market reforms reduce unemployment and do so fast, while product market reforms temporarily increase unemployment. For EA countries, however, our results are very different: our evidence suggests that structural reforms have no impact on unemployment in the euro area. This is not due to fewer structural reforms in EA countries compared to non-EA countries. Since 1999, there were 65 and 27 product and labour market reforms in 12 EA countries, against 45 and 19 product and labour market reforms in 13 non-EA countries. We also checked whether the GIPS countries drive these results. These countries introduced reforms during the sovereign debt crisis when they had high and increasing unemployment rates. It turns out that if we drop these countries one by one, there is (weak) evidence that labour market reforms reduce unemployment. For illustrative purposes, Figure A2 shows the results for Portugal (the graphs for the other countries are available on request).

Fig. 7. AIPW results: subsamples



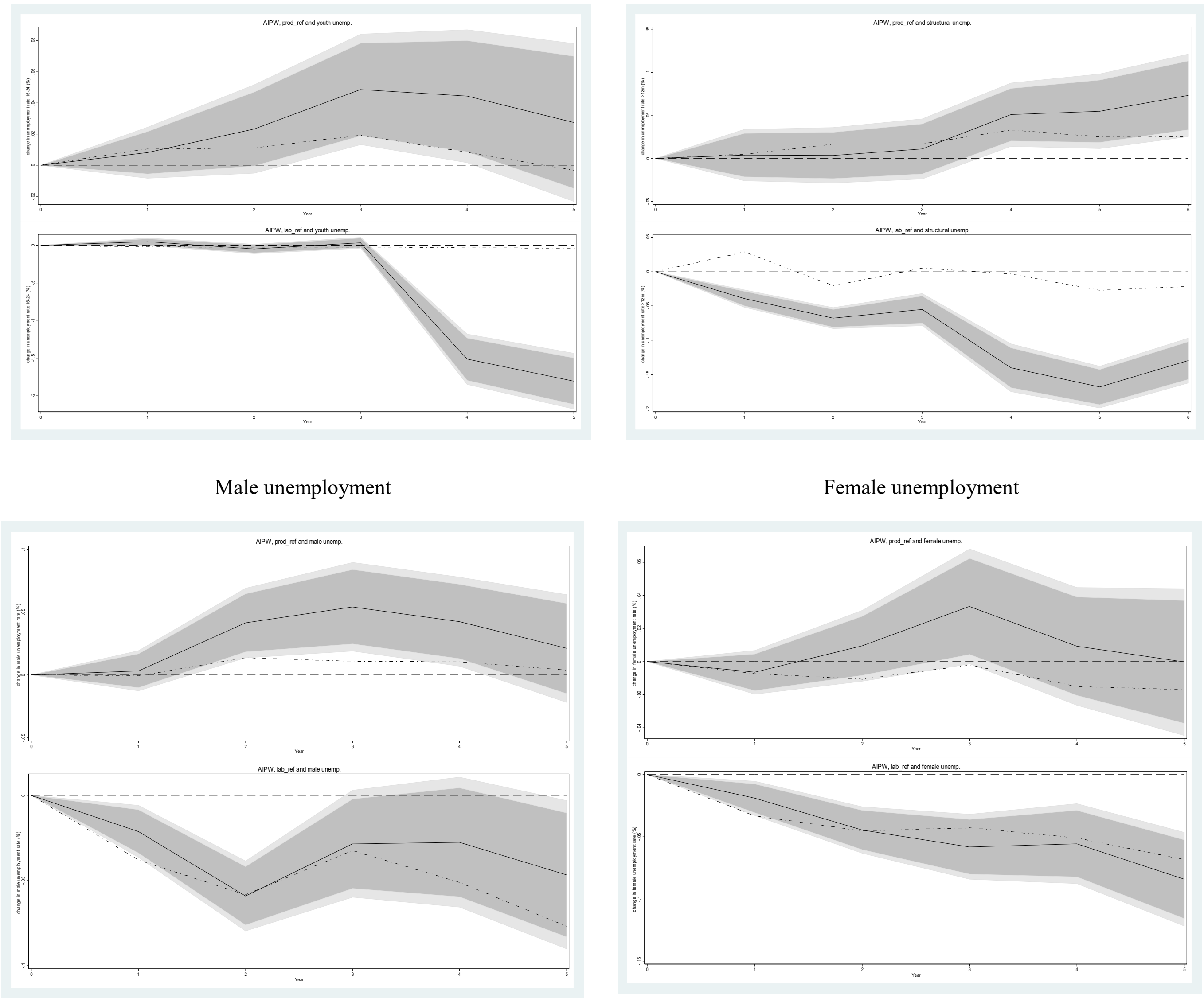
Notes: The solid black lines in the figure plots the impulse responses of product market (upper panels) and labour market (lower panels) reforms on unemployment for euro area (left-hand side panels) and non-euro area countries (right-hand side panels). The dark grey shaded areas display the 90% error bands, the light grey shaded areas display the 95% error bands. The dotted-dashed line displays impulse responses from the simple LP regressions.

4.4 Effect of reform on different types of unemployment

Workers are distinguished by age and gender, while we also distinguish between short-term and long-term unemployment and between different education levels of the unemployed. From Figure 8, we observe that product market reforms increase each type of unemployment, consistent with our findings for overall unemployment. One caveat concerning the effect of reforms on long-term unemployment is in order though. The drop in long-term unemployment may reflect that the long-term unemployed were able to find a job or that they withdrew from the labour force. Carrillo-Tudela et al. (2021) report that the Hartz reforms in Germany induced a large fraction of the long-term unemployed to deregister as jobseekers. Labour market reforms reduce unemployment but for youth unemployment it takes longer for the effect to set in. Another notable difference is that the effect of labour market reform on female unemployment remains statistically significant throughout the forecasting horizon, while the effect on male unemployment becomes insignificant after three years. Finally, Figure A3 in the Appendix shows the impulse response of structural reforms on unemployment for different levels of education. The responses are very similar (and statistically not different from one another), although the strongest and fastest effect is found for the unemployed with intermediate levels of education. Due to more limited data availability of the different types of unemployment the number of observations is lower than in in our analyses of general unemployment, see table A1 in the appendix.

Fig. 8. AIPW results: different types of unemployment

Youth unemployment | Long-term unemployment



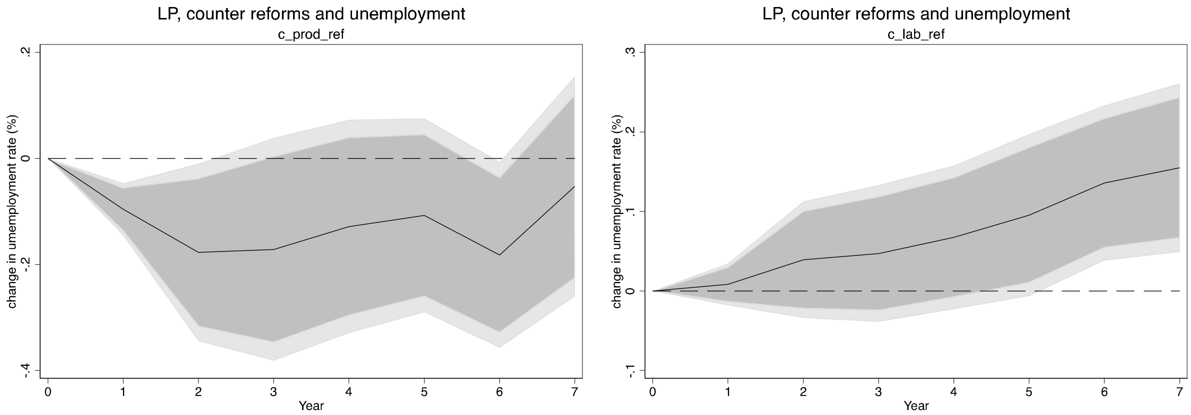
Notes: The solid black lines in the figure plots the impulse responses of product market (upper panels) and labour market (lower panels) reforms on different types of unemployment. The dark grey shaded areas display the 90% error bands, the light grey shaded areas display the 95% error bands. The dotted-dashed line displays impulse responses from the simple LP regressions. Regression tables available on request.

5. Robustness analysis

5.1 Results counter reforms

Although there is some weak evidence of selection on covariates for the labour market reforms (see Table A8 in the Appendix) the low number of counter reforms does not allow us to estimate the impact of labour market counter reforms on unemployment using AIPW. So, for the unconditional and conditional effects of counter reforms we report LP estimates only. Figure 9 shows that our findings for counter reforms are broadly consistent with our results for endogenized reforms: product market counter reforms hardly affect unemployment, while labour market counter reforms increase unemployment after some time (Tables A9 and A10 in the Appendix show the underlying estimations). As there are too few counter reforms in EA countries, we decided against estimating the model for counter reforms for our subsamples.

Fig. 9. Unconditional Local Projections: Effect of product and labour market counter reforms on unemployment (full sample)



Note: The solid black lines in the figure plots the impulse responses of product counter market reforms (left panel) and labour market counter reforms (right panel) on unemployment. The dark grey shaded areas display the 90% SCC error bands; the light grey shaded areas display the 95% SCC error bands.

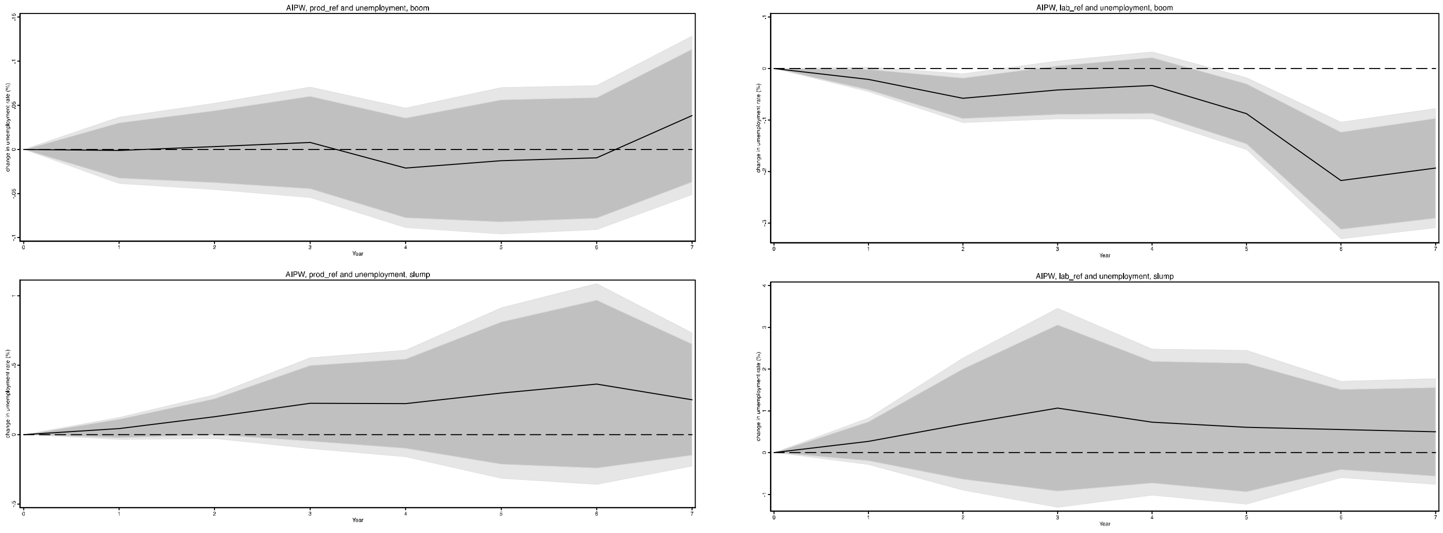
5.2 Are effects of structural reform conditional on the business cycle?

We check whether our main AIPW findings are sensitive to the countries' business cycle position (detailed results are in the last panels of Tables A6 and A7). There is some evidence that the effects of structural reforms depend on the business cycle. For instance, Gehrke and Weber (2018) observe that labour market reforms have substantially weaker beneficial effects in the short run when implemented in recessions.[12] Figure 10 shows the outcomes. The results suggest that when the economy is booming, labour market reforms reduce the unemployment rate; in contrast, when the economy is below trend, labour market reforms do not significantly affect unemployment. The results for counter reforms are consistent with these findings (available on request).

[12] These authors point to theoretical labour market models that give rise to asymmetric effects of policy over the course of the economy. For instance, in case of a downward wage rigidity, the wage channel of structural reforms may be less effective in recessions when wage growth is low. Likewise, in case jobs are rationed in recessions, matching frictions—and thus also reductions in frictions—are less influential in determining labour market outcomes.

Fig. 10. AIPW Projections: effect of product and labour market reforms on unemployment conditional on business cycle

Product market reform: boom (upper part) and bust (lower (part) Labour market reform: boom (upper part) and bust (lower (part)



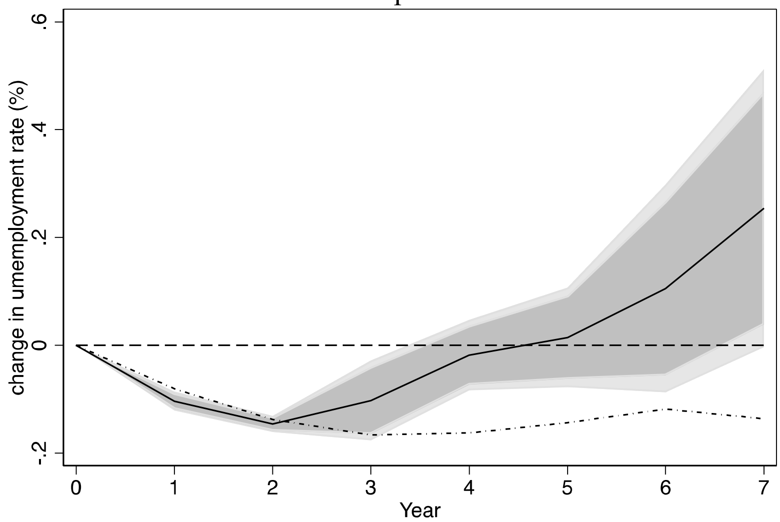
Notes: The solid black lines in the figure plots the impulse responses of product market (left-hand side panels) and labour market (right-hand side panels) reforms on unemployment. The dark grey shaded areas display the 90% SCC error bands; the light grey shaded areas display the 95% SCC error bands. The panels in the upper part show projections for country-years when the economy is running above the trend, while the panels in the lower part show country-years when the economy is running below the trend based on the output gap; the trend is based on the HP filter. We do not truncate the propensity scores when estimating the AIPWs conditional on the business cycle.

5.3 Joint product and labour market reforms

Additionally, we analyse the joint effect of labour and product market reforms. In practice, that amounts to analysing whether reforms work better or worse when implemented as broad reform packages, i.e., simultaneous reforms in both the product and labour market. Unfortunately, we only have 23 observations in which major reforms occur in both the product and labour market simultaneously. Therefore, it is not possible to conduct this analysis for the EA and non-EA countries separately. There are simply too few observations in which joint reforms occurred to conduct the AIPW analysis. As a result, we also exclude the country and time fixed effects in the first stage AIPW propensity score model. In the second stage AIPW model, we include the product and labour market reform indicators as controls.

The results reported in Figure 11 and Table A11 suggest that the effect of joint labour and product market reforms is negative in the short term, but in the medium term the effect becomes positive, but only marginally significant at the 10% level after 7 years.

Fig. 11. AIPW Projections: Joint effect of product and labour market reforms on unemployment: full sample



Notes: The solid black lines in the figure plots the impulse responses of joint product and labour market reforms on unemployment. The dark grey shaded areas display the 90% error bands, the light grey shaded areas display the 95% error bands. The dotted-dashed line displays impulse responses from the simple LP regressions.

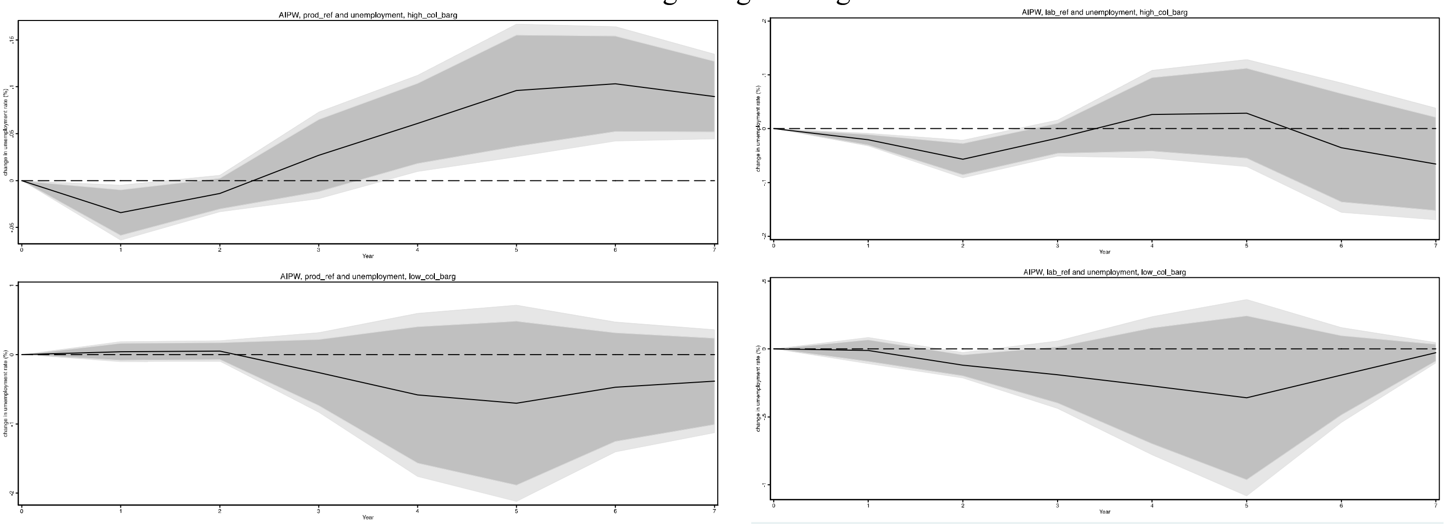
5.4 Are effects of structural reform conditional on collective bargaining?

Collective bargaining refers to negotiations between workers' representatives, usually organized in trade unions, and employers on several aspects of employment contracts, such as wages, overtime pay, bonuses, working hours, and health and safety rules in the workplace (Thommen, 2022). Collective bargaining may increase labour market efficiency by correcting market failures (like information asymmetries and excessive firm power) and by reducing the transaction costs of all parties involved. However, collective bargaining may also introduce labour market distortions, for instance if unions and insiders have excessive power (Schnabel, 2020). While the literature on the effects of collective bargaining on economic performance is extensive (OECD, 2019; Schnabel, 2020), little is known about how collective bargaining may affect the impact of structural reform on unemployment.[13] Here we examine the conditioning effect of an important dimension of collective bargaining, namely the extent to which the resulting collective bargaining agreements directly affect the working conditions of the workforce. This so-called coverage rate refers to the percentage of workers in an economy or industry whose terms and conditions of employment are determined by collective rather than individual bargaining (Schnabel, 2020). We employ data from the OECD on the adjusted collective bargaining coverage rate, which is defined as the number of employees covered by a collective agreement in force as a proportion of the number of eligible employees equipped (i.e., the total number of employees minus the number of employees legally excluded from the right to bargain). Figure 12 shows how structural reforms affect unemployment for observations with a below and above median level of the adjusted

[13] Thommen (2022) examines reforms of national collective bargaining systems carried out between 2000 and 2018 in EU countries and observes that reforms to make wage bargaining institutions more flexible have either no effect or a detrimental effect on employment in the first few years after their implementation.

collective bargaining coverage rate (detailed results are in Table A12). The results suggest that the effects of structural reforms on unemployment are stronger for high collective bargaining coverage observations.

Fig. 12. AIPW Projections: effect of product and labour market reforms on unemployment conditional on collective bargaining coverage



Notes: The solid black lines in the figure plots the impulse responses of product market (left-hand side panels) and labour market (right-hand side panels) reforms on unemployment. The dark grey shaded areas display the 90% SCC error bands; the light grey shaded areas display the 95% SCC error bands. The panels in the upper part show projections for country-years with above median collective bargaining coverage, while the panels in the lower part show country-years with below median collective bargaining.

5.5 Nickel bias

Finally, a cause of concern about our estimates may be the Nickell (1981) bias. Specifically, we estimate a dynamic panel model with fixed-effects. As Nickell (1981) shows, the demeaning process creates a correlation between the regressor and the error term which creates a bias in the estimated coefficient of the lagged dependent variable. If the independent variables of interest are correlated with the lagged dependent variable their coefficients may be biased as well. This is particular a problem in a large N, small T context. We have small N and relatively large T. The bias can be gauged in the following way.

If the AR(1) coefficient β_2 on $\Delta y_{i,t}$ is positive (as in most cases in our estimates), the bias is invariably negative, so that the persistence of the β_2 coefficient on $\Delta y_{i,t}$ will be underestimated. For reasonably large values of T, the limit of β_2 on $\Delta y_{i,t}$ as $N \rightarrow \infty$ will be approximately $-(1 + \beta_2)/(T - 1)$. In our case $\beta_2 \approx 0.25$, so that the bias will be about -0.025, i.e., around 1/10 of the estimated coefficient. This is even assuming that N tends to infinity, which is far from the case in our application. Furthermore, the correlation between the labour and product market indicators and $\Delta y_{i,t-1}$ is low and negative. The correlation coefficient for product (labour) market reforms and the lagged unemployment rate is -0.07 (-0.05). Because of this negative correlation, the Nickell bias also leads to an underestimation of the impulse responses of reforms on unemployment. This, in combination with the relative low size of the biased AR(1) term and the large T relative to N leads us to conclude that the Nickell bias in our case is negligible.[14]

6. Conclusions

Our findings suggest that labour market reforms reduce unemployment. Whereas in the LP model this effect becomes statistically significant only after 4 years, when reforms are endogenized it becomes statistically significant within a year. We take endogeneity into account by applying the Augmented Inverse Probability Weighted estimator proposed by Jordà and Taylor (2015) and Glynn and Quinn (2010). The AIPW results suggest that product market reforms, for some time, increase unemployment, in contrast to the findings based on the simple LP model. So, our results show that it is crucial to take endogeneity of structural reforms into account when analysing their effects on unemployment. The results for counter reforms are consistent with our findings for reforms. We also estimate our models for euro-area countries and non-euro-area countries. The findings for the latter subsample are consistent with those for the full sample. However, our results suggests that labour market reforms only have a limited impact on unemployment in the euro area. Product market reforms increase each type of unemployment, whether categorized by age, gender or duration. Note that the drop in long-term unemployment may reflect that the long-term unemployed were able to find a job or that they withdrew from the labour force. In turn, labour market reforms reduce all types of unemployment but for youth unemployment it takes longer for the effect to set in. Furthermore, we find that labour market reforms do not affect unemployment when the economy is below trend. We also analyse the joint effect of labour and product market reforms and results suggest that the effect is negative in the short term, but in the medium term the effect becomes positive, but only weakly significant. Finally, conditioning regressions show that the effects of structural reforms on unemployment are stronger for high collective bargaining coverage observations.

[14] GGM estimation is not suited in cases of large T and small N. Rather a method based on recursive substitutions could be used. But as noted in Teulings and Zubanov (2014), a disadvantage of such an approach is a sizeable efficiency loss.

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Online Appendix

Figures

Fig. A1. Distribution of unemployment

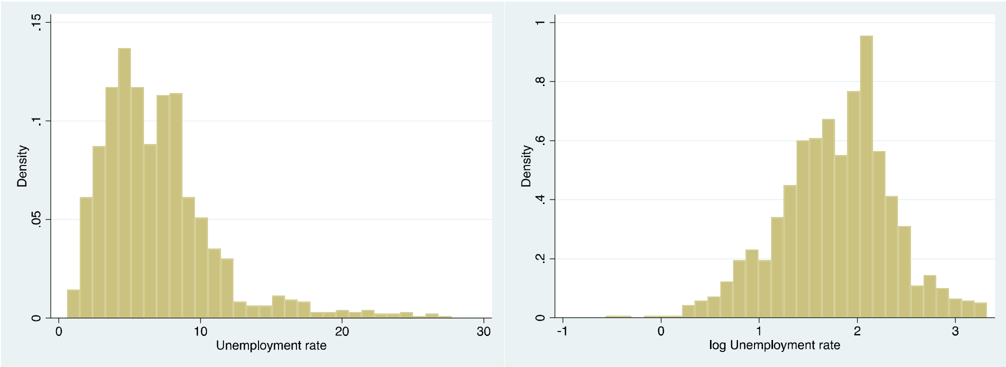
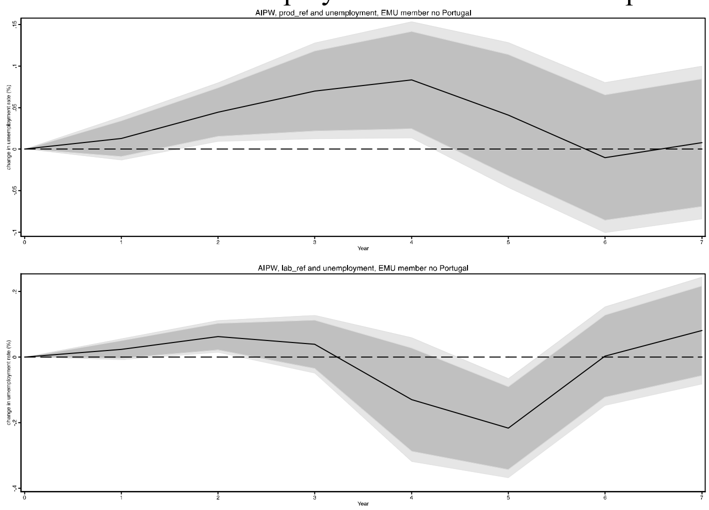


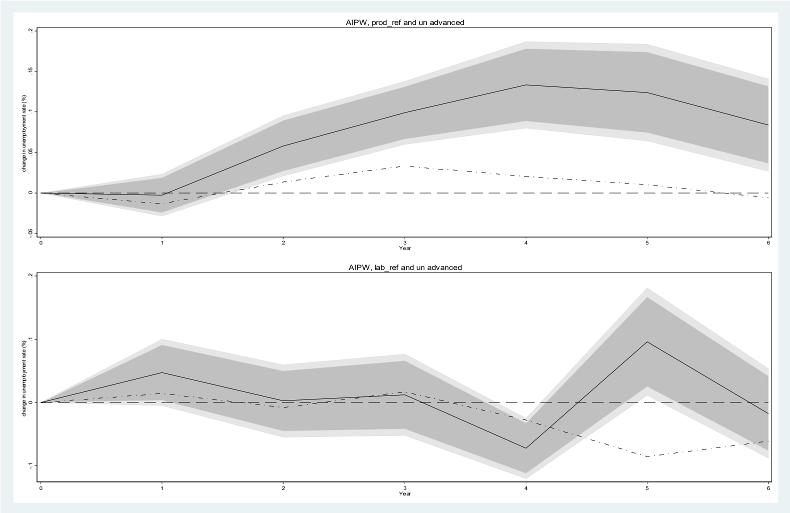
Fig. A2. Reforms and unemployment: EA countries except Portugal



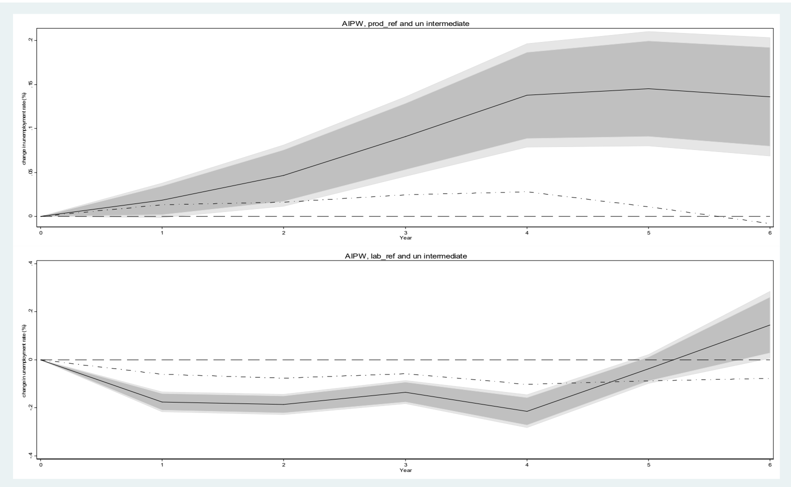
Note: The solid black lines in the figure plots the impulse responses of product market (upper panels) and labour market (lower panels) reforms on unemployment for euro area countries excluding Portugal.

Fig. A3. Impact of structural reform on unemployment: different education levels

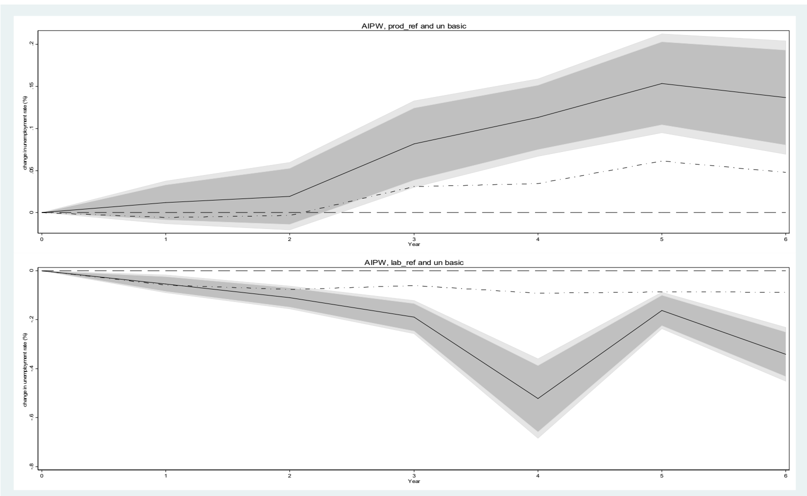
Advanced education level



Intermediate education level



Basic education level



Note: The solid black lines in the figure plots the impulse responses of product market (upper panels) and labour market (lower panels) reforms on unemployment for different education levels of the unemployed.

Tables

Table A.1 Descriptive statistics

Variable	Description	Obs.	Mean	S.D.	Min	Max	Source
Unemployment rate	People of working age without work, who are available for work, and have taken specific steps to find work	911	7.159	4.063	1.49	27.695	OECD
Youth unemployment rate	Unemployment rate of those working age people between 15-24 years old	501	11.697	9.128	3.050	59.249	OECD
Long-term unemployment rate	Unemployment rate of those working age people in unemployment status for 12 months or more	742	31.269	17.082	0.223	76.167	OECD
Female unemployment rate	Females of working age without work, who are available for work, and have taken specific steps to find work	900	7.862	5.101	1.586	31.620	OECD
Male unemployment rate	Males of working age without work, who are available for work, and have taken specific steps to find work	902	6.620	3.685	0.804	25.601	OECD
Unemployment rate with basic education	Unemployment rate of those working age people that have basic education degrees	435	11.569	5.876	1.660	35.100	OECD
Unemployment rate with intermediate education	Unemployment rate of those working age people that have intermediate education degrees	433	7.401	4.577	1.630	31.250	OECD
Unemployment rate with advanced education	Unemployment rate of those working age people that have advanced education degrees	425	4.684	2.897	1.000	20.860	OECD
Annual percentage change in the Unemployment rate	Log differences of the unemployment rate, annual	911	0.007	0.158	-0.379	0.979	OECD
Product and labour market (counter) reforms	See main text	911	See main text	See main text	0.000	1.000	Wiese et al. (2023), update of Duval et al. (2018)
Output gap	Calculated using HP-filter to real GDP at constant 2017 national prices (in 2017 US$), with λ=100	911	-0.001	0.204	-1.088	1.106	PWT
Collective bargaining	The number of employees covered by a collective agreement in force as a proportion of the number of eligible employees equipped (i.e., the total number of employees minus the number of employees legally excluded from the right to bargain)	911	65.318	28.310	11.6	100	OECD
Inflation rate	Annual growth rate in the consumer price index	911	3.496	3.871	-4.478	28.385	OECD
Economic growth	Log difference of real GDP per capita at constant 2017 national prices (in 2017 US$)	911	0.018	0.025	-0.102	.219	PWT
Ideology of government	The sum of the number of seats taken by each government party times each parties' ideological colour divided by total number of seats held by the government. Ideology is defined in terms of stated economic policy intentions (1=left wing, 2=centre, 3=right-wing)	911	2.077	0.818	1.000	3.000	Wiese et al. (2023)
Political fragmentation	The weighted squared difference between each individual government parties' ideological colour and the ideology of the government as a whole. The weights are defined as the number of seats held by each government party relative to the total number of seats held by the government	909	0.191	0.312	0.000	1.000	Wiese et al. (2023)
Government years in office	The number of years in which the same government held office	909	3.879	2.863	1.000	18.000	Wiese et al. (2023)
Effective number of parties	The inverse of the sum of the squared seats shares of each government in office	911	1.615	0.741	0.964	4.080	Wiese et al. (2023)

Any election, legislative or executive	Equal to 1 in years where either a legislative and/or executive election took place, otherwise equal to 0	911	0.327	0.469	0.000	1.000	Wiese et al. (2023)
Strictness of employment protection, temporary employment	Index that measures the costs and procedures involved in dismissing individuals or groups of temporary workers, and the procedures involved in hiring workers on fixed-term or temporary work agency contracts	764	1.778	1.247	0.250	5.250	OECD
Strictness of employment protection, regular employment	Index that measures the costs and procedures involved in dismissing individuals or groups of workers, and the procedures involved in hiring workers for regular employment	764	2.132	0.969	0.093	5.000	OECD

Note: based on estimation sample for the 1 year forecast.

Table A.2 Local Projections: Product market reforms and unemployment

	(1) unemp1 b/se	(2) unemp2 b/se	(3) unemp3 b/se	(4) unemp4 b/se	(5) unemp5 b/se	(6) unemp6 b/se	(7) unemp7 b/se
Product mark. reforms	0.0004	0.0035	0.0089	-0.0004	-0.0034	0.0010	0.0186
	(0.0093)	(0.0154)	(0.0182)	(0.0189)	(0.0216)	(0.0232)	(0.0269)
Prod. mark. ref. (-1)	0.0013	0.0079	-0.0020	-0.0105	-0.0099	0.0050	0.0300
	(0.0111)	(0.0122)	(0.0158)	(0.0193)	(0.0209)	(0.0236)	(0.0282)
Prod. mark. ref. (-2)	0.0078	-0.0042	-0.0140	-0.0148	-0.0040	0.0196	0.0182
	(0.0121)	(0.0137)	(0.0161)	(0.0172)	(0.0187)	(0.0215)	(0.0257)
Prod. mark. ref. (-3)	-0.0144	-0.0230	-0.0232	-0.0158	0.0044	0.0029	0.0087
	(0.0092)	(0.0150)	(0.0153)	(0.0176)	(0.0195)	(0.0233)	(0.0244)
Prod. mark. ref. (-4)	-0.0070	-0.0118	-0.0133	-0.0029	-0.0143	-0.0147	-0.0247
	(0.0080)	(0.0119)	(0.0142)	(0.0181)	(0.0209)	(0.0243)	(0.0215)
Prod. mark. ref. (-5)	-0.0023	-0.0042	0.0024	-0.0138	-0.0201	-0.0280	-0.0324
	(0.0071)	(0.0141)	(0.0181)	(0.0224)	(0.0251)	(0.0248)	(0.0260)
Output gap	0.2723	0.8150***	1.5127***	2.1194***	2.5577***	2.6834***	2.6350***
	(0.1702)	(0.2484)	(0.2522)	(0.2495)	(0.2703)	(0.2897)	(0.3336)
Output gap (-1)	0.0035	-0.1616	-0.5454*	-0.9722***	-1.3664***	-1.5248***	-1.5033***
	(0.1761)	(0.2701)	(0.2798)	(0.2926)	(0.3147)	(0.3640)	(0.4351)
Inflation	0.0065**	0.0098**	0.0057	0.0017	-0.0038	-0.0097	-0.0062
	(0.0030)	(0.0040)	(0.0053)	(0.0062)	(0.0072)	(0.0084)	(0.0103)
Inflation (-1)	-0.0031	-0.0054	-0.0025	-0.0011	0.0037	0.0111	0.0115
	(0.0033)	(0.0055)	(0.0062)	(0.0063)	(0.0056)	(0.0068)	(0.0084)
GDP growth	-1.8284*	-4.6582***	-8.3959***	-11.5168***	-13.9512***	-14.3352***	-14.0331***
	(1.0816)	(1.6397)	(1.7756)	(1.8491)	(1.8649)	(2.2881)	(2.6817)
GDP growth (-1)	-1.1540***	-2.1199***	-2.4425***	-2.3535***	-1.7252*	-1.1746	-0.6681
	(0.3507)	(0.5033)	(0.5854)	(0.7434)	(0.9134)	(1.0363)	(1.2354)
Unemployment	0.2616***	0.2891***	0.1368**	-0.0240	-0.1197	-0.1141	-0.2431**
	(0.0515)	(0.0663)	(0.0589)	(0.0688)	(0.0795)	(0.0998)	(0.1070)
Unemployment (-1)	-0.0280	-0.1608**	-0.2462***	-0.2727***	-0.2240**	-0.3203***	-0.2398**
	(0.0411)	(0.0612)	(0.0602)	(0.0648)	(0.0858)	(0.0809)	(0.1062)
Unemployment (-2)	-0.0834**	-0.1018**	-0.1163**	-0.1011	-0.2126***	-0.1802*	-0.1415
	(0.0370)	(0.0432)	(0.0480)	(0.0806)	(0.0684)	(0.0939)	(0.1033)
Unemployment (-3)	-0.0044	0.0009	0.0171	-0.0921	-0.0708	-0.0393	-0.0515
	(0.0525)	(0.0580)	(0.0897)	(0.0730)	(0.0850)	(0.0865)	(0.0930)
Unemployment (-4)	-0.0214	-0.0719	-0.1765***	-0.1567***	-0.1275**	-0.1277	-0.1316
	(0.0294)	(0.0471)	(0.0465)	(0.0546)	(0.0605)	(0.0922)	(0.1261)
Prod. mark. ref. (1)	0.0044	0.0039	0.0029	0.0055	0.0005	0.0009	0.0106
	(0.0097)	(0.0111)	(0.0164)	(0.0183)	(0.0194)	(0.0227)	(0.0245)
Prod. mark. ref. (2)		0.0109	0.0045	-0.0022	0.0006	-0.0025	0.0032
		(0.0155)	(0.0168)	(0.0167)	(0.0225)	(0.0235)	(0.0241)
Prod. mark. ref. (3)			-0.0029	-0.0052	-0.0077	-0.0030	-0.0090
			(0.0263)	(0.0248)	(0.0177)	(0.0220)	(0.0252)
Prod. mark. ref. (4)				-0.0137	-0.0151	-0.0195	-0.0126
				(0.0318)	(0.0289)	(0.0237)	(0.0292)
Prod. mark. ref. (5)					-0.0129	-0.0102	-0.0102
					(0.0349)	(0.0321)	(0.0266)
Prod. mark. ref. (6)						-0.0140	-0.0091
						(0.0330)	(0.0310)
Prod. mark. ref. (7)							-0.0142
							(0.0381)
Constant	0.1862***	0.2425**	0.4931***	0.6213***	0.8272***	1.0291***	0.9143***
	(0.0466)	(0.1145)	(0.1447)	(0.1181)	(0.1097)	(0.1073)	(0.1579)
Observations	911	886	861	836	811	786	761
Country and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	25	25	25	25	25	25	25
R^2	0.536	0.580	0.606	0.626	0.621	0.590	0.545
Canova t-test (p-value)	0.842	0.583	0.039	0.000	0.052	0.080	0.130

Notes: Estimates of eq. (1). Canova (2022) test comparing the averaged time series estimates with the panel estimate. Spatial correlation consistent standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3 Local Projections: Labour market reforms and unemployment

	(1) unemp1 b/se	(2) unemp2 b/se	(3) unemp3 b/se	(4) unemp4 b/se	(5) unemp5 b/se	(6) unemp6 b/se	(7) unemp7 b/se
Labour mark. reforms	-0.0353*	-0.0528**	-0.0386	-0.0506*	-0.0718**	-0.0976**	-0.1052**
	(0.0188)	(0.0261)	(0.0331)	(0.0276)	(0.0320)	(0.0383)	(0.0429)
Output gap	0.2597	0.7790***	1.4585***	2.0176***	2.4473***	2.5840***	2.5885***
	(0.1579)	(0.2304)	(0.2285)	(0.2365)	(0.2961)	(0.3273)	(0.3499)
Output gap (-1)	0.0097	-0.1493	-0.5347**	-0.9204***	-1.3027***	-1.4616***	-1.4905***
	(0.1631)	(0.2490)	(0.2469)	(0.2440)	(0.2758)	(0.3105)	(0.3339)
Inflation	0.0068**	0.0100**	0.0053	0.0007	-0.0051	-0.0112	-0.0068
	(0.0031)	(0.0045)	(0.0052)	(0.0059)	(0.0068)	(0.0081)	(0.0102)
Inflation (-1)	-0.0039	-0.0069	-0.0040	-0.0020	0.0026	0.0092	0.0068
	(0.0032)	(0.0054)	(0.0063)	(0.0062)	(0.0058)	(0.0083)	(0.0088)
GDP growth	-1.7469*	-4.5965***	-8.3239***	-11.2974***	-13.8043***	-14.3205***	-14.4219***
	(0.9774)	(1.5061)	(1.5877)	(1.5976)	(1.7949)	(2.0808)	(2.2200)
GDP growth (-1)	-1.2313***	-2.1974***	-2.5613***	-2.6150***	-2.0352**	-1.5728*	-1.0487
	(0.3639)	(0.5378)	(0.6044)	(0.6571)	(0.8061)	(0.9001)	(1.0040)
Unemployment	0.2604***	0.2728***	0.1148*	-0.0672	-0.1768**	-0.1611	-0.2518**
	(0.0539)	(0.0693)	(0.0587)	(0.0689)	(0.0756)	(0.0971)	(0.1199)
Unemployment (-1)	-0.0310	-0.1608**	-0.2618***	-0.3031***	-0.2397**	-0.3171***	-0.2135*
	(0.0434)	(0.0675)	(0.0659)	(0.0635)	(0.0904)	(0.0925)	(0.1064)
Unemployment (-2)	-0.0816**	-0.1054**	-0.1332**	-0.1083	-0.1998***	-0.1432	-0.0956
	(0.0391)	(0.0481)	(0.0499)	(0.0787)	(0.0711)	(0.0856)	(0.0948)
Unemployment (-3)	-0.0065	-0.0137	0.0082	-0.0849	-0.0442	0.0066	0.0226
	(0.0527)	(0.0586)	(0.0858)	(0.0762)	(0.0799)	(0.0823)	(0.0898)
Unemployment (-4)	-0.0216	-0.0567	-0.1469***	-0.1028*	-0.0449	-0.0091	-0.0041
	(0.0280)	(0.0506)	(0.0510)	(0.0547)	(0.0511)	(0.0825)	(0.1119)
Constant	0.1915***	0.2640**	0.5198***	0.6415***	0.8448***	1.0499***	0.9659***
	(0.0428)	(0.1030)	(0.1271)	(0.0959)	(0.0805)	(0.0772)	(0.1253)
Observations	911	886	861	836	811	786	761
Reform leads & lags	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	25	25	25	25	25	25	25
R^2	0.545	0.590	0.625	0.653	0.661	0.646	0.617
Canova t-test (p-value)	0.996	0.045	0.220	0.445	0.901	0.492	0.600

Notes: Estimates of eq. (1). Canova (2022) test comparing the averaged time series estimates with the panel estimate. Spatial correlation consistent standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4 Local Projections: EPL reforms and unemployment

	(1) unemp1 b/se	(2) unemp2 b/se	(3) unemp3 b/se	(4) unemp4 b/se	(5) unemp5 b/se	(6) unemp6 b/se	(7) unemp7 b/se
EPL reforms	-0.0220	-0.0315	-0.0319	-0.0260	-0.0510	-0.0593	-0.0805
	(0.0178)	(0.0263)	(0.0379)	(0.0359)	(0.0422)	(0.0445)	(0.0489)
Output gap	0.2644	0.7958***	1.4797***	2.0698***	2.5284***	2.6944***	2.7111***
	(0.1653)	(0.2396)	(0.2435)	(0.2497)	(0.2834)	(0.2990)	(0.3204)
Output gap (-1)	0.0064	-0.1625	-0.5522**	-0.9750***	-1.3944***	-1.5859***	-1.6441***
	(0.1713)	(0.2592)	(0.2689)	(0.2738)	(0.2887)	(0.3115)	(0.3433)
Inflation	0.0069**	0.0098**	0.0048	0.0007	-0.0054	-0.0113	-0.0070
	(0.0031)	(0.0045)	(0.0051)	(0.0060)	(0.0071)	(0.0082)	(0.0104)
Inflation (-1)	-0.0038	-0.0066	-0.0033	-0.0016	0.0032	0.0096	0.0072
	(0.0032)	(0.0054)	(0.0064)	(0.0065)	(0.0060)	(0.0082)	(0.0089)
GDP growth	-1.7851*	-4.6592***	-8.4695***	-11.6426***	-14.4371***	-15.1351***	-15.4600***
	(1.0407)	(1.5696)	(1.7391)	(1.7794)	(1.8197)	(2.0174)	(2.1100)
GDP growth (-1)	-1.1988***	-2.2133***	-2.5282***	-2.5371***	-1.8464**	-1.4288	-0.8426
	(0.3696)	(0.5496)	(0.6207)	(0.7108)	(0.8828)	(0.9944)	(1.1719)
Unemployment	0.2574***	0.2708***	0.1119*	-0.0522	-0.1621**	-0.1605*	-0.2609**
	(0.0529)	(0.0676)	(0.0571)	(0.0630)	(0.0688)	(0.0867)	(0.1109)
Unemployment (-1)	-0.0293	-0.1643**	-0.2520***	-0.2966***	-0.2442***	-0.3194***	-0.2290**
	(0.0421)	(0.0637)	(0.0621)	(0.0597)	(0.0866)	(0.0843)	(0.0995)
Unemployment (-2)	-0.0832**	-0.1054**	-0.1345***	-0.1221	-0.2126***	-0.1720*	-0.1204
	(0.0393)	(0.0478)	(0.0493)	(0.0804)	(0.0720)	(0.0916)	(0.1013)
Unemployment (-3)	-0.0073	-0.0128	0.0030	-0.0850	-0.0548	-0.0013	0.0020
	(0.0540)	(0.0576)	(0.0862)	(0.0750)	(0.0831)	(0.0851)	(0.0937)
Unemployment (-4)	-0.0222	-0.0655	-0.1522***	-0.1179**	-0.0648	-0.0406	-0.0403
	(0.0284)	(0.0500)	(0.0493)	(0.0532)	(0.0509)	(0.0830)	(0.1129)
Constant	0.1902***	0.2663**	0.5178***	0.6452***	0.8587***	1.0738***	0.9935***
	(0.0429)	(0.1032)	(0.1304)	(0.1006)	(0.0887)	(0.0854)	(0.1286)
Observations	911	886	861	836	811	786	761
Reform leads & lags	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	25	25	25	25	25	25	25
R^2	0.537	0.585	0.617	0.643	0.647	0.627	0.592
Canova t-test (p-value)	0.732	0.076	0.539	0.751	0.584	0.145	0.271

Notes: Estimates of eq. (1). Canova (2022) test comparing the averaged time series estimates with the panel estimate. Spatial correlation consistent standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5 Local Projections: Unemployment benefit reforms and unemployment

	(1) unemp1 b/se	(2) unemp2 b/se	(3) unemp3 b/se	(4) unemp4 b/se	(5) unemp5 b/se	(6) unemp6 b/se	(7) unemp7 b/se
UB reforms	-0.0676**	-0.0947**	-0.0612**	-0.1111***	-0.1478***	-0.2180***	-0.2443***
	(0.0272)	(0.0354)	(0.0275)	(0.0270)	(0.0435)	(0.0666)	(0.0837)
Output gap	0.2504	0.7662***	1.4356***	1.9823***	2.3743***	2.4751***	2.4344***
	(0.1560)	(0.2276)	(0.2328)	(0.2455)	(0.3098)	(0.3589)	(0.4050)
Output gap (-1)	0.0146	-0.1317	-0.4904*	-0.8561***	-1.2103***	-1.3523***	-1.3545***
	(0.1630)	(0.2531)	(0.2589)	(0.2755)	(0.3273)	(0.3911)	(0.4457)
Inflation	0.0068**	0.0107**	0.0068	0.0017	-0.0024	-0.0078	-0.0046
	(0.0030)	(0.0043)	(0.0051)	(0.0059)	(0.0070)	(0.0078)	(0.0098)
Inflation (-1)	-0.0033	-0.0062	-0.0034	-0.0001	0.0036	0.0100	0.0091
	(0.0031)	(0.0053)	(0.0057)	(0.0057)	(0.0053)	(0.0070)	(0.0075)
GDP growth	-1.6923*	-4.4085***	-7.8673***	-10.6001***	-12.7186***	-12.9558***	-12.6546***
	(0.9832)	(1.5150)	(1.5765)	(1.7395)	(2.0590)	(2.6352)	(3.0353)
GDP growth (-1)	-1.1572***	-2.1088***	-2.4814***	-2.4631***	-1.8677**	-1.2275	-0.6425
	(0.3473)	(0.5119)	(0.5930)	(0.6548)	(0.8290)	(0.9087)	(1.0413)
Unemployment	0.2672***	0.2913***	0.1439**	-0.0431	-0.1363*	-0.1141	-0.2216*
	(0.0526)	(0.0678)	(0.0536)	(0.0648)	(0.0796)	(0.1072)	(0.1189)
Unemployment (-1)	-0.0321	-0.1600**	-0.2657***	-0.2875***	-0.2264**	-0.3234***	-0.2278*
	(0.0423)	(0.0675)	(0.0600)	(0.0644)	(0.0911)	(0.0935)	(0.1179)
Unemployment (-2)	-0.0832**	-0.1050**	-0.1120**	-0.0862	-0.2021***	-0.1538*	-0.1294
	(0.0386)	(0.0434)	(0.0480)	(0.0758)	(0.0674)	(0.0893)	(0.1012)
Unemployment (-3)	-0.0027	-0.0018	0.0208	-0.0939	-0.0578	-0.0298	-0.0321
	(0.0510)	(0.0602)	(0.0864)	(0.0727)	(0.0805)	(0.0863)	(0.0880)
Unemployment (-4)	-0.0228	-0.0629	-0.1726***	-0.1361**	-0.1008*	-0.0850	-0.0791
	(0.0273)	(0.0463)	(0.0466)	(0.0523)	(0.0537)	(0.0846)	(0.1141)
Constant	0.1837***	0.2363**	0.4822***	0.5854***	0.7782***	0.9811***	0.8948***
	(0.0401)	(0.1012)	(0.1230)	(0.0909)	(0.0714)	(0.0662)	(0.1052)
Observations	911	886	861	836	811	786	761
Reform leads & lags	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	25	25	25	25	25	25	25
R^2	0.545	0.585	0.616	0.639	0.642	0.620	0.584
Canova t-test (p-value)	0.369	0.131	0.836	0.687	0.230	0.533	0.310

Notes: Estimates of eq. (1). Canova (2022) test comparing the averaged time series estimates with the panel estimate. Spatial correlation consistent standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6 AIPW: Product market reforms and unemployment

Full sample

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE_IPWRA	0.001	0.028**	0.051***	0.033**	0.020	0.015	0.045**
	(0.007)	(0.011)	(0.017)	(0.016)	(0.021)	(0.019)	(0.021)
Observations	675	675	675	675	675	675	675

EA and non-EA

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE_IPWRA_EA	0.008	0.032*	0.058*	0.075**	0.040	0.008	0.026
	(0.013)	(0.017)	(0.030)	(0.034)	(0.041)	(0.043)	(0.042)
Observations	166	166	166	166	166	166	166
ATE_IPWRA_nonEA	-0.004	0.015	0.041**	0.020	0.024	0.022	0.053
	(0.011)	(0.013)	(0.016)	(0.024)	(0.035)	(0.034)	(0.035)
Observations	509	509	509	509	509	509	509

Conditional on business cycle

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE_IPWRA_boom	-0.001	0.003	0.008	-0.021	-0.013	-0.009	0.038
	(0.019)	(0.025)	(0.032)	(0.035)	(0.042)	(0.042)	(0.046)
ATE_IPWRA_slump	0.044	0.129	0.225	0.223	0.298	0.363	0.252
	(0.041)	(0.081)	(0.167)	(0.196)	(0.314)	(0.370)	(0.246)
Observations	675	675	675	675	675	675	675

Notes: Block-bootstrapped standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7 AIPW Labour market reforms and unemployment

Full sample							
	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE_IPWRA	-0.016**	-0.048***	-0.043***	-0.039**	-0.062***	-0.097***	-0.077***
	(0.007)	(0.010)	(0.014)	(0.017)	(0.019)	(0.021)	(0.021)
Observations	589	589	568	568	568	568	546

EA and non-EA							
	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE_IPWRA_EA	0.015	0.004	0.031	-0.008	-0.037	0.045	0.036
	(0.019)	(0.026)	(0.053)	(0.088)	(0.105)	(0.101)	(0.095)
Observations	185	185	174	174	174	174	163
ATE_IPWRA_nonEA	-0.016*	-0.038***	-0.055***	-0.074***	-0.103***	-0.140***	-0.126***
	(0.009)	(0.013)	(0.017)	(0.020)	(0.023)	(0.030)	(0.034)
Observations	404	404	394	394	394	394	383

Conditional on business cycle							
	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE_IPWRA_boom	-0.021*	-0.058**	-0.042	-0.033	-0.088**	-0.218***	-0.193***
	(0.012)	(0.024)	(0.029)	(0.033)	(0.036)	(0.058)	(0.059)
ATE_IPWRA_slump	0.271	0.680	1.071	0.728	0.604	0.553	0.501
	(0.287)	(0.809)	(1.217)	(0.894)	(0.942)	(0.590)	(0.648)
Observations	589	589	568	568	568	568	546

Notes: Block-bootstrapped standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8. Balance tests: Counter reforms

Full sample									
VARIABLES	(1) Output gap	(2) Output gap (-1)	(3) Inflation	(4) Inflat. (-1)	(5) GDP growth	(6) GDP gr. (-1)	(7) Unemployment	(8) Unemp. (-1)	(9) Unemp. (-2)
Product market	-0.064	-0.135	-2.041	-1.794	-0.003	-0.018	0.004	0.000	0.044
counter reforms	(0.145)	(0.146)	(2.741)	(2.917)	(0.018)	(0.018)	(0.112)	(0.115)	(0.115)
Labour market	-0.007	-0.017	1.396*	2.022***	0.004	0.001	0.000	0.017	0.025
counter reforms	(0.038)	(0.038)	(0.718)	(0.762)	(0.005)	(0.005)	(0.029)	(0.030)	(0.030)
EPL counter	0.024	0.011	0.382	0.426	0.004	0.001	-0.035	0.005	0.020
reforms	(0.045)	(0.046)	(0.855)	(0.910)	(0.006)	(0.006)	(0.035)	(0.036)	(0.036)
UB counter	-0.078	-0.080	3.666***	5.604***	0.004	0.001	0.081	0.042	0.037
reforms	(0.069)	(0.069)	(1.292)	(1.368)	(0.008)	(0.009)	(0.053)	(0.054)	(0.055)
Obs.	911	911	911	911	911	911	911	911	911
EA countries									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Product market	-0.225	-0.232	-1.542	-0.582	-0.017	-0.037	0.053	0.141	0.249*
count. reforms	(0.245)	(0.243)	(1.302)	(1.302)	(0.029)	(0.030)	(0.143)	(0.144)	(0.143)
Labour market	-0.128	-0.059	0.531	0.962*	-0.004	-0.006	0.066	0.013	0.039
count. reforms	(0.093)	(0.093)	(0.499)	(0.494)	(0.011)	(0.011)	(0.054)	(0.055)	(0.055)
EPL counter	-0.166	-0.072	0.520	0.985*	-0.008	-0.013	0.095	0.066	0.056
reforms	(0.101)	(0.100)	(0.538)	(0.533)	(0.012)	(0.012)	(0.059)	(0.060)	(0.059)
UB counter	0.104	0.021	0.566	0.776	0.022	0.029	-0.111	-0.296**	-0.065
reforms	(0.245)	(0.243)	(1.306)	(1.301)	(0.029)	(0.030)	(0.143)	(0.143)	(0.144)
Obs.	250	250	250	250	250	250	250	250	250
Non-EA countries									
Product market	0.080	-0.051	-1.472	-1.814	0.015	0.004	-0.038	-0.131	-0.152
count. reforms	(0.187)	(0.191)	(4.304)	(4.573)	(0.023)	(0.024)	(0.164)	(0.168)	(0.170)
Labour market	0.032	-0.002	1.530*	2.202**	0.006	0.003	-0.021	0.017	0.020
count. reforms	(0.040)	(0.040)	(0.911)	(0.966)	(0.005)	(0.005)	(0.035)	(0.036)	(0.036)
EPL counter	0.099**	0.043	0.365	0.245	0.009	0.006	-0.086**	-0.019	0.006
reforms	(0.049)	(0.050)	(1.123)	(1.193)	(0.006)	(0.006)	(0.043)	(0.044)	(0.044)
UB counter	-0.093	-0.087	3.622**	5.731***	0.000	-0.003	0.102*	0.081	0.046
reforms	(0.067)	(0.068)	(1.524)	(1.610)	(0.008)	(0.008)	(0.058)	(0.060)	(0.060)
Obs.	661	661	661	661	661	661	661	661	661

Table A.9 Local Projections: Product market counter reforms and unemployment

	(1) unemp1 b/se	(2) unemp2 b/se	(3) unemp3 b/se	(4) unemp4 b/se	(5) unemp5 b/se	(6) unemp6 b/se	(7) unemp7 b/se
Prod. mark. counter reforms	-0.0959***	-0.1772**	-0.1716	-0.1284	-0.1073	-0.1822**	-0.0524
	(0.0251)	(0.0853)	(0.1072)	(0.1026)	(0.0933)	(0.0894)	(0.1057)
Count. ref (-1)	-0.0486	-0.0600	-0.0441	-0.0566	-0.1529**	-0.1093	-0.0897
	(0.0685)	(0.1071)	(0.1208)	(0.1311)	(0.0590)	(0.0822)	(0.1019)
Count. ref (-2)	0.0143	0.0376	0.0219	-0.0807	0.0943	0.1214	0.1484
	(0.0317)	(0.0477)	(0.0622)	(0.1006)	(0.0792)	(0.0882)	(0.0960)
Count. ref (-3)	-0.0221	-0.0845	-0.2404*	-0.1966	-0.1313	-0.0795	-0.0359
	(0.0457)	(0.0756)	(0.1382)	(0.2203)	(0.2112)	(0.2099)	(0.2525)
Count. ref (-4)	0.0219	-0.0826	0.0413	0.0414	0.0903	0.1554	0.0716
	(0.0504)	(0.1348)	(0.1938)	(0.1916)	(0.2288)	(0.2958)	(0.3467)
Count. ref (-5)	-0.1163	-0.0188	-0.0016	0.0639	0.1488	0.0608	0.0160
	(0.0998)	(0.1584)	(0.1593)	(0.1758)	(0.2099)	(0.2355)	(0.3000)
Output gap	0.2700	0.8115***	1.5013***	2.0994***	2.5377***	2.6815***	2.6523***
	(0.1653)	(0.2450)	(0.2492)	(0.2484)	(0.2719)	(0.2975)	(0.3508)
Outp. gap (-1)	0.0056	-0.1584	-0.5331*	-0.9531***	-1.3502***	-1.5306***	-1.5382***
	(0.1747)	(0.2741)	(0.2859)	(0.3030)	(0.3223)	(0.3709)	(0.4453)
Inflation	0.0065**	0.0096**	0.0064	0.0026	-0.0026	-0.0086	-0.0058
	(0.0031)	(0.0040)	(0.0052)	(0.0061)	(0.0073)	(0.0080)	(0.0106)
Inflation (-1)	-0.0029	-0.0050	-0.0028	-0.0009	0.0032	0.0101	0.0104
	(0.0034)	(0.0046)	(0.0056)	(0.0051)	(0.0047)	(0.0069)	(0.0080)
GDP growth	-1.7938*	-4.6213***	-8.2785***	-11.3849***	-13.8243***	-14.3096***	-14.1590***
	(1.0575)	(1.6537)	(1.7767)	(1.8789)	(1.9105)	(2.3323)	(2.7535)
GDP growth (-1)	-1.1773***	-2.1833***	-2.5553***	-2.4490***	-1.7805*	-1.1604	-0.6046
	(0.3682)	(0.5391)	(0.6187)	(0.7722)	(0.9705)	(1.0960)	(1.2897)
Unemployment	0.2626***	0.2894***	0.1398**	-0.0248	-0.1191	-0.1082	-0.2279**
	(0.0516)	(0.0673)	(0.0584)	(0.0656)	(0.0772)	(0.0981)	(0.1104)
Unemploym. (-1)	-0.0283	-0.1598**	-0.2489***	-0.2756***	-0.2243**	-0.3164***	-0.2364**
	(0.0410)	(0.0619)	(0.0586)	(0.0626)	(0.0855)	(0.0846)	(0.1061)
Unemploym. (-2)	-0.0795**	-0.1027**	-0.1166**	-0.1017	-0.2120***	-0.1786*	-0.1490
	(0.0386)	(0.0458)	(0.0502)	(0.0838)	(0.0711)	(0.0941)	(0.1052)
Unemploym. (-3)	-0.0059	-0.0000	0.0195	-0.0838	-0.0576	-0.0327	-0.0478
	(0.0536)	(0.0588)	(0.0893)	(0.0717)	(0.0816)	(0.0837)	(0.0889)
Unemploym. (-4)	-0.0218	-0.0658	-0.1637***	-0.1462**	-0.1241*	-0.1305	-0.1367
	(0.0283)	(0.0430)	(0.0463)	(0.0591)	(0.0666)	(0.0983)	(0.1307)
Reforms (1)	-0.0329*	-0.1510***	-0.2270***	-0.1973**	-0.1222	-0.0804	-0.1330
	(0.0175)	(0.0374)	(0.0825)	(0.0976)	(0.0798)	(0.0766)	(0.1328)
Reforms (2)		-0.0459	-0.1425**	-0.2012**	-0.1534	-0.0649	-0.0236
		(0.0319)	(0.0531)	(0.0943)	(0.1012)	(0.0927)	(0.0877)
Reforms (3)			0.0138	-0.1088	-0.1885	-0.1619	-0.0810
			(0.1370)	(0.1786)	(0.2314)	(0.2540)	(0.2391)
Reforms (4)				0.0171	-0.0505	-0.0972	-0.0500
				(0.1513)	(0.1825)	(0.2367)	(0.2646)
Reforms (5)					0.0077	-0.0770	-0.1264
					(0.1927)	(0.2410)	(0.2984)
Reforms (6)						-0.0437	-0.1180
						(0.1715)	(0.2222)
Reforms (7)							0.0322
							(0.2275)
Constant	0.1835***	0.2363**	0.4803***	0.6015***	0.8100***	1.0211***	0.9185***
	(0.0410)	(0.1018)	(0.1236)	(0.0983)	(0.0918)	(0.0972)	(0.1379)
Observations	911	886	861	836	811	786	761
Country and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	25	25	25	25	25	25	25
R^2	0.537	0.580	0.608	0.627	0.622	0.590	0.543

Notes: Spatial correlation consistent standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10 Local Projections: Labour market counter reforms and unemployment

	(1) unemp1 b/se	(2) unemp2 b/se	(3) unemp3 b/se	(4) unemp4 b/se	(5) unemp5 b/se	(6) unemp6 b/se	(7) unemp7 b/se
Lab. market counter reforms	0.0084	0.0393	0.0473	0.0676	0.0955*	0.1357***	0.1551***
	(0.0134)	(0.0374)	(0.0438)	(0.0460)	(0.0519)	(0.0497)	(0.0541)
Output gap	0.2508	0.7681***	1.4448***	2.0148***	2.4232***	2.5287***	2.4631***
	(0.1737)	(0.2568)	(0.2673)	(0.2656)	(0.2799)	(0.2968)	(0.3427)
Output gap (-1)	0.0198	-0.1253	-0.4885	-0.8799***	-1.2485***	-1.3957***	-1.3826***
	(0.1811)	(0.2809)	(0.2997)	(0.3197)	(0.3452)	(0.3909)	(0.4600)
Inflation	0.0070**	0.0102**	0.0055	0.0009	-0.0042	-0.0108	-0.0076
	(0.0030)	(0.0040)	(0.0053)	(0.0060)	(0.0075)	(0.0088)	(0.0112)
Inflation (-1)	-0.0036	-0.0060	-0.0026	-0.0004	0.0035	0.0103	0.0092
	(0.0031)	(0.0050)	(0.0058)	(0.0054)	(0.0052)	(0.0070)	(0.0080)
GDP growth	-1.6685	-4.3528**	-7.9455***	-10.7764***	-13.0300***	-13.2203***	-12.8096***
	(1.1095)	(1.7067)	(1.8738)	(1.9775)	(2.0360)	(2.4337)	(2.8314)
GDP growth (-1)	-1.1757***	-2.1619***	-2.4622***	-2.3738***	-1.6736*	-0.9917	-0.3600
	(0.3541)	(0.5171)	(0.5913)	(0.7325)	(0.8754)	(0.9736)	(1.1505)
Unemployment	0.2626***	0.2883***	0.1339**	-0.0271	-0.1220*	-0.1123	-0.2337**
	(0.0492)	(0.0618)	(0.0517)	(0.0599)	(0.0707)	(0.0927)	(0.1075)
Unemployment (-1)	-0.0260	-0.1604***	-0.2446***	-0.2687***	-0.2140**	-0.3039***	-0.2152*
	(0.0386)	(0.0588)	(0.0567)	(0.0582)	(0.0837)	(0.0858)	(0.1165)
Unemployment (-2)	-0.0852**	-0.1053**	-0.1178**	-0.1047	-0.2171***	-0.1858*	-0.1604
	(0.0368)	(0.0429)	(0.0477)	(0.0807)	(0.0733)	(0.1008)	(0.1109)
Unemployment (-3)	-0.0037	0.0024	0.0200	-0.0877	-0.0637	-0.0362	-0.0558
	(0.0524)	(0.0574)	(0.0878)	(0.0734)	(0.0876)	(0.0924)	(0.1006)
Unemployment (-4)	-0.0223	-0.0728	-0.1772***	-0.1541***	-0.1275**	-0.1340	-0.1346
	(0.0268)	(0.0454)	(0.0456)	(0.0542)	(0.0610)	(0.0960)	(0.1243)
Constant	0.1729***	0.2180*	0.4558***	0.5585***	0.7492***	0.9436***	0.8446***
	(0.0449)	(0.1117)	(0.1391)	(0.1143)	(0.1113)	(0.1214)	(0.1682)
Observations	911	886	861	836	811	786	761
Counter reform leads & lags	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Countries	25	25	25	25	25	25	25
R^2	0.537	0.581	0.608	0.630	0.629	0.604	0.565

Notes: Spatial correlation consistent standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11 Joint product and labour market reform and unemployment

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE_AIPW	-0.104***	-0.146***	-0.103***	-0.018	0.015	0.105	0.254*
	(0.009)	(0.008)	(0.038)	(0.033)	(0.047)	(0.098)	(0.132)
Observations	745	720	696	672	647	623	598

Notes: Block-bootstrapped standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.12 Reforms and unemployment conditional on collective bargaining

Product market reform	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
ATE col. barg. below median	0.043	0.052	-0.257	-0.581	-0.702	-0.467	-0.384
	(0.075)	(0.077)	(0.294)	(0.602)	(0.724)	(0.479)	(0.380)
ATE col. barg. above median	-0.034**	-0.014	0.027	0.061**	0.096***	0.103***	0.090***
	(0.015)	(0.010)	(0.024)	(0.026)	(0.036)	(0.031)	(0.023)
Observations	675	675	675	675	675	675	675
Labour market reform	**Year 1**	**Year 2**	**Year 3**	**Year 4**	**Year 5**	**Year 6**	**Year 7**
ATE col. barg. below median	-0.012	-0.120**	-0.191	-0.272	-0.360	-0.193	-0.029
	(0.050)	(0.048)	(0.127)	(0.260)	(0.369)	(0.179)	(0.039)
ATE col. barg. above median	-0.021***	-0.057***	-0.018	0.027	0.029	-0.036	-0.066
	(0.006)	(0.018)	(0.017)	(0.042)	(0.051)	(0.062)	(0.053)
Observations	589	589	568	568	568	568	546

Notes: Block-bootstrapped standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$