The impact of reforms in labour markets and network industries on unemployment: New evidence based on bias-corrected local projections

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Abstract

We examine the impact of structural reforms on unemployment in 25 OECD countries between 1976-2014. Our local projection (LP) results suggest that both reforms of network industries and the labour market do not affect unemployment. We reject dynamic heterogeneity across countries. However, if we control for the bias due to the endogeneity of reforms, the results for both types of reform deviate substantially form those based on the simple LP model. Reforms of network industries increase unemployment while labour market reforms decrease unemployment. These findings suggest that LP analyses of the consequences of reform may be seriously biased.

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

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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).

In this paper, we examine the impact of labour market reforms and reforms in network industries on unemployment in 25 OECD countries for the 1976-2014 period. Following Duval and Furceri (2018), we focus on measures aiming at i) deregulation of retail trade, professional services and certain segments of network industries, primarily by reducing barriers to entry; ii) easing hiring and dismissal regulations for regular workers; and (iii) increasing the ability of and incentives for the non-employed to find jobs. We employ reform indicators put together by Duval et al. (2018) and updates thereof as provided by Wiese et al. (2024). 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.[1]

We use the local projections (LP) approach (Jordà, 2005).[2] LP has been widely used to analyze the dynamic effects of policy shocks (Jordà and Taylor, 2016; Alpanda et al., 2021; de Haan and Wiese, 2022; Thommen, 2022; Hülsewig and Rottmann, 2023; Alesina et al., 2023). Given the panel data nature of our data, we prefer the LP method over 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 the number of estimated parameters.

[1] These indicators have been used in several previous papers; see, for instance, Duval et al. (2020), de Haan and Wiese (2022), and Wiese et al. (2024). Duval et al. (2020) examine the impact of certain labour market reforms on unemployment; de Haan and Wiese (2022) analyse the effects of product and labour market reform on economic growth, while Wiese et al. (2024) research how structural reforms affect income inequality.

[2] The data does not allow the use of a difference-in-differences (DiD) approach, because we do not have a suitable control group. For some countries the pre-treatment period is too short to apply a staggered-DiD (see Figure 1). Likewise, DiD also requires that no treatment must have taken place before the observed period, neither for the treatment nor the control group, but we cannot exclude the possibility that reforms took place earlier. All this invalidates a DiD approach. An additional complication is the occurrence of counter-reforms. Our preferred LP method allows us to take the effect of counter-reforms in the post-treatment period into account.

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 depends sensitively on the persistence of the data and on the length of the forecast horizon (Montiel 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.[3] 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.

Despite its popularity, the LP approach has been criticized. We aim to take these criticisms seriously. More specifically, Canova (2024) points to the potential problem of dynamic heterogeneity across cross-sections when the LP approach is employed for a sample covering several countries. As the author argues: "When the units identically respond over time to the policy change, that is, when the cross-section is dynamically homogeneous, the approach provides useful estimates of the average dynamic effects but under a set of stringent conditions; when these are violated, distortions in the magnitude and the significance of the estimated effects emerge. When instead units evolve independently in response to the policy change, that is, when the cross-section is dynamically heterogeneous, as it is generally the case when cross-region or cross-sector data are employed, the approach fails to consistently measure the average dynamic effects of a policy change, because it is unable to separate policy variations from variations due to random causes." We test whether structural reforms have similar dynamic effects across countries in our simple LP model, using a test proposed by Canova (2024).

Another potential problem with simple LP estimates of the effect of structural reforms on unemployment is that they may be biased because reforms are likely to be endogenous. We use the Augmented Inverse Probability Weighted (AIPW) estimator proposed by Jordà and Taylor (2016), following Glynn and Quinn (2010), to alleviate the bias due to the endogeneity of reforms. This approach works as follows. First, propensity scores are derived from a latent model which explains the probability of implementing a structural reform based on a number of reform predictors, such as the occurrence of economic crises and government fragmentation (see section 4.2 for further details). These propensity scores are used to correct for selection bias by reweighting the sample in the LP outcome regressions such that we achieve a quasi-random distribution of treatment and control observations. The LP model is used to estimate conditional means in the treatment group and in the control group (observations in which no reform took place) separately based on a number of determinants. Finally, the differences in weighted conditional means (where weights are represented by the inverse propensity scores of each observation) at each horizon between the treatment and control groups are computed to estimate the average treatment effects (ATEs) of reforms on unemployment.

[3] Li et al. (2024) examine the bias-variance trade-off in impulse response estimation through a comprehensive simulation study and find that least-squares LP and VAR estimators lie on opposite ends of the bias-variance spectrum: small bias and large variance for LPs, and large bias and small variance for VARs.

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, taking endogeneity of reforms into account. 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. (2024). Duval et al. (2020) also use the Duval et al. (2018) database and local projections, but these authors do not control for the bias due to the endogeneity of reforms. Furthermore, they focus on a subset of labour market reforms, whereas the present paper considers broader measures of labour market reforms and reforms in network industries.

Our local projection (LP) results suggest that both reforms of network industries and the labour market do not affect unemployment. Dynamic heterogeneity across countries can be rejected. However, if we control for the bias due to the endogeneity of reforms, the results for both types of reform deviate substantially form those based on the simple LP model. Reform of network industries increase unemployment while labour market reforms decrease unemployment.

The remainder of the paper is organized as follows. Section reviews previous research on the impact of structural reform on unemployment. Section 3 discusses the data used. Section 4 outlines our methodology, while section 5 presents our main findings. Section 6 offers a robustness analysis, while section 7 concludes.

2. Literature Review

The impact of labour market reforms on unemployment is ambiguous, notably because of the uncertain effects of job protection reforms. 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 less clearcut because on the one hand they decrease labour market rigidity, while on the other they reduce job security. This type of reform may increase unemployment, as layoffs are likely to rise in the short run if firing constraints are relaxed (Boeri et al., 2015). However, lowering firing costs may also increase employers' willingness to hire, thus potentially reducing unemployment, particularly for younger or less-experienced workers (Blanchard and Portugal, 2001). Reforms of network industries (one type of 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; 2024 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.

Other studies present estimates of the impact of structural reforms on (un)employment using panel or cross-sectional data.[4] 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.

3. Data and stylized facts[5]

Major reforms of network industries and labour market regulation are identified by Duval et al. (2018) and updated until 2020 by Wiese et al. (2024), using documented legislative and regulatory actions reported in all available *OECD Economic Surveys* for 25 advanced economies, as well as additional country-specific sources.[6] The database also includes "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,

[4] 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.

[5] This section draws on Wiese et al. (2024).

[6] 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.

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.

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. (2024) 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 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. Reforms of network industries have been frequently implemented, in particular in telecommunications and air transport, while reforms in the area of employment protection legislation occur more frequently than unemployment benefit reforms.

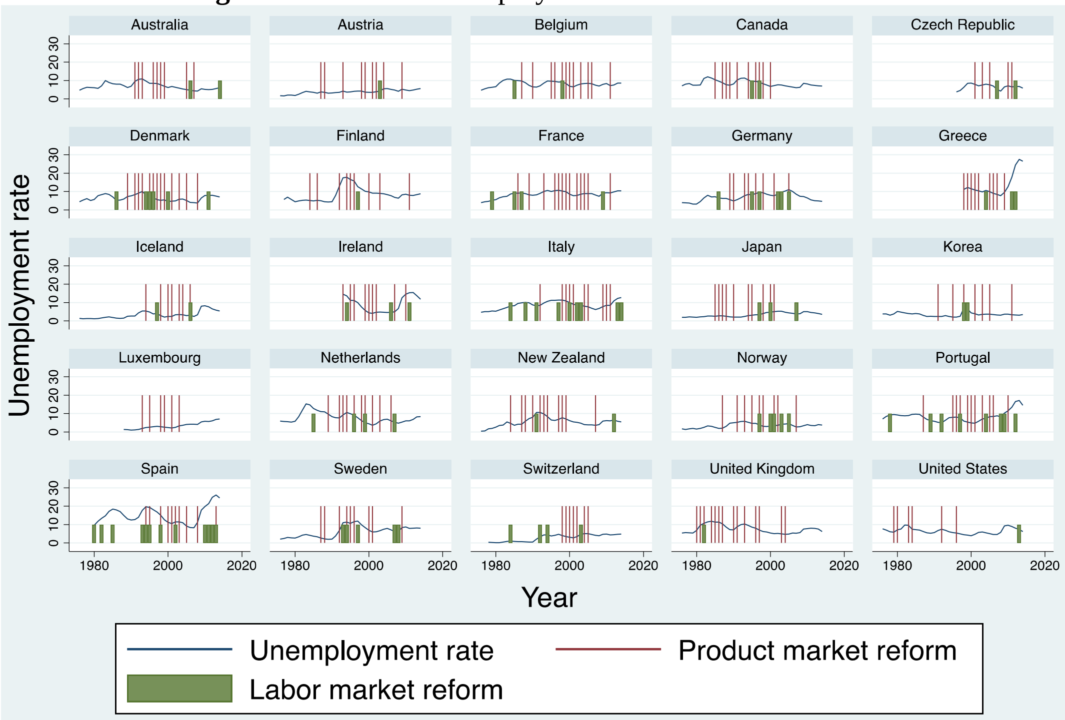
Table 1. Number of reforms by category (25 advanced economies, 1976-2014)

Reform type	**Number of reforms:**	**Reforms (as % of number of observations):**	**Number of counter reforms:**	**Counter reforms (as % of number of observations)**
Network industries reforms	233	26.00%	3	0.3%
Labour market reforms	90	10.04%	32	3.6%
Employment protection legislation (EPL) reforms	62	6.92%	20	2.2%
Unemployment benefit (UB) reforms	28	3.13%	12	1.3%

Note: The total number of observations is 896. In our baseline empirical analysis, we fix the sample size in all regressions such that it is equal to the AIPW baseline estimations with h=6. Thus, the displayed reforms are always in the estimation sample regardless of h (i.e., the forecast horizon). This ensures that the reforms considered are always the same and do not change whenever h changes.

Figure 1 shows the level of unemployment (taken from the OECD) and reforms in the 25 countries in our sample. As the figure shows, we are using an unbalanced panel since the unemployment rate is not available for years before 1976 for some countries.

Fig. 1. Reforms and unemployment in individual countries



Note: The total number of observations is 896.

To validate our narrative reform database, Table 2 shows whether the average yearly change in the OECD network sector regulation index, the index for the strictness of EPL, and the index for the generosity of UB are related to (counter-)reforms. The EPL index is split into employment protection regulation for regular and temporary employment. Clearly reforms are associated with a decline in the OECD indexes, while counter-reforms are associated with an increase in the indexes. Although the coefficient is significant in columns (1) and (2) when our narrative database indicates that no reform takes place, the coefficients of the no-reform dummies are much lower than those for the reform dummies. This indicates that our narrative reform data captures large changes.[7]

[7] Note that using the OECD indexes instead of our narrative reform data implies a much smaller sample. Also, the OECD network sector regulation index does not capture Postal Services which is part of the used narrative database.

Table 2. Average change in the OECD network sector regulation index, EPL strictness index and UB generosity index and narrative (counter-)reforms

Reform type	(1) Network sector regulation index	(2) EPL regular contracts strictness index	(3) EPL temporary contracts strictness index	(4) UB generosity index
No reform	-0.038***	-0.006**	-0.010	0.001
	(0.005)	(0.003)	(0.007)	(0.001)
Reform	-0.270***	-0.141***	-0.497***	-0.022***
	(0.009)	(0.014)	(0.030)	(0.005)
Counter reform	0.206**	0.167***	0.275***	0.068***
	(0.093)	(0.027)	(0.053)	(0.009)
Observations	832	637	637	806

Notes: Standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. The table shows OLS regressions (without a constant) testing whether the change in the OECD indexes for network regulation, the strictness of EPL (for regular and temporary employment), and the generosity of UB are related to reform, no reform, and counter-reform dummies. Based on OECD indices data availability in our baseline estimation sample. The network sector regulation index can take values between 0 and 10, where higher values indicate more regulation. The EPL strictness indexes can take values between 0 and 6, where higher values indicate less flexible firing and hiring conditions. The UB generosity index is bounded between 0 and 1, where higher values indicate more generous unemployment benefits (duration and size). Source of used indicies: OECD.org.

4. Methodology

4.1 Local Projections

We estimate impulse response functions (IRFs) by applying Jordà´s (2005) LP method.[8] The Jordà method simply requires OLS estimation for each forecast horizon, h, of the model for each dependent variable of interest (in our case different indicators of unemployment). We follow the recommendations of Herbst and Johannesen (2024) and include lags of the dependent and independent variables in our dynamic two-way fixed-effect panel data model. The basic LP regression model that we estimate takes the following form:

$$\ln U_{i,t+h} - lnU_{i,t} = \alpha_i + \delta_t + \sum_{j=0}^{3} \beta_{jh}\, d_{i,t-j} + \beta_0\, lnU_{i,t} + \sum_{l=0}^{2} \beta_{lh}\left(lnU_{i,t-l} - \ln U_{i,t-1-l}\right) + \sum_{h=1}^{h} \beta_h\, d_{i,t+h} + \sum_{c=0}^{1} \beta'_{ch}\, X_{i,t-c} + u_{i,t+h} \quad (1)$$

where U denotes the log of the unemployment rate.[9] The forecast horizon h is set at 1 to 6 years, since the effect of reforms can take time to materialize. Country fixed-effects α_i capture unobserved heterogeneity across countries, such as time-invariant institutional variables, while δ_t are time fixed-effects to control for global shocks such as the great recession.[10] The term $\sum_{j=0}^{3} \beta_{jh}\, d_{i,t-j}$ includes the

[8] Montiel Olea et al. (2024) provide a formal proof of Jordà's (2005) claim that conventional LP confidence intervals for impulse responses are robust to misspecification.

[9] 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.

[10] As Canova (2024) points out, time fixed-effects are crucial for proper identification in LP models. Without time fixed-effects, LP cannot distinguish between global shocks that may affect unemployment, and local shocks, such as shocks to labour market institutions in individual countries, that may also affect unemployment.

reform indicator plus 3 lags thereof, such that $\beta_{jh=0}$ captures the response of a reform on unemployment. This term is used to construct the IRFs and their associated confidence intervals. We include the contemporaneous level of the log of unemployment plus two lagged log-changes in unemployment. Upcoming reforms may be in the forecast horizon that we calculate for each h. If reforms affect unemployment, not including future reforms would create a bias. We therefore include the number of leads of the treatment indicator equal to the forecast horizon as suggested by Teulings and Zubanov (2014); the term $\beta_h \sum_{h=1}^{h} d_{i,t+h}$ captures this correction.[11] We also include the leads of the counter-reforms equal to the forecast horizon for the same reason. If reforms affect unemployment, counter-reforms are expected to have the opposite effect. $X_{i,t}$ is a vector of control variables, which consists of: the output gap using the Hamilton (2018) filter on real GDP in PPP (from the Penn World Tables) (which controls for mean reversion in the business cycle), annual inflation (change in consumer price index, taken from the OECD, which controls for a Phillips-curve relation), and per capita real GDP (in PPP) growth (from the Penn World Tables, which controls for Okun's law). We include the contemporaneous and first lag of our controls. These variables affect the results and their coefficients are significant in most regressions. We employ Spatial Correlation Consistent (SCC) standard errors (Driscoll-Kraay, 1998).

The LP approach is 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). However, as pointed out in the Introduction, we test for dynamic heterogeneity across cross-sections as suggested by Canova (2024).[12] This test is based on calculating the coefficient of variation (CV) of the impact of interest to detect deviations from homogeneity. Intuitively this is done by estimating the effect for each h, country-by-country using the time series variation. The effects of those unit specific estimates are used to calculate the CV, i.e., the standard error of the average effect of the country-by-country time series estimates, divided by the average effect. Under homogeneity, the estimated distribution for each h is concentrated around a central value, and the estimated CV will be small (zero in theory). If that is the case, cross-sectional methods display dynamic homogeneity. Under heterogeneity, the estimated distribution will be spread out and the CV will be large. To assess whether the dispersion of the distribution of the cross-sectional estimates is large, critical values are constructed for each h based on the bootstrap procedure as in Canova (2024). Under the null of homogeneity, the absolute value of the CV should not be outside the critical values. We use T=40 for each forecast horizon (although we have

[11] 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. Reforms occur repeatedly within our forecast horizon of 6 years, especially network sector reforms. The Teulings and Zubanov (2014) approach registers that the outcome of an observation may be affected by later reforms, which otherwise would have meant a bias in the effect of reforms.
[12] Canova (2024) shows that LPs that suffer from dynamic heterogeneity are biased.

an unbalanced panel, most of our cross-sectional units have close to 40 observations) with a significance level of 5%. The critical values are reported in Table A1 in the Appendix.

3.2 AIPW model

The major drawback of equation (1) is that it ignores that reforms are not introduced randomly. Structural reforms are introduced when they are politically feasible, for example, during economic crises (Drazen and Grilli, 1993). Failing to account for this will lead to selection bias in the estimated outcomes. This occurs because reforms are more likely to be introduced when, for example, the unemployment rate is high. Failing to account for this means that we would be overestimating the effect of reforms since the forecasts will be affected by the adjustment of unemployment to its long-run equilibrium value ahead in time, because of their timing in crisis year. We therefore proceed with a quasi-experimental method, namely the Augmented Inverse Probability Weighted (AIPW) estimator proposed by Jordà and Taylor (2016) and Glynn and Quinn (2010).

In the first step, we estimate correlated random effect logit models to estimate the probability of reform of network industries and labour markets one period ahead. The correlated random-effects model consistently includes country fixed-effects although the reform variable is binary and the panel is unbalanced. It is fully robust when estimated using the pooled logit estimator, unlike, for example, the fixed effects logit model (Wooldridge, 2019). The first-stage propensity scores model is well able to predict the reforms. As explanatory 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 reforms of network industries in t+1, and vice versa for labour market reforms, we control for the possibility that both types of 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).[13] We also include the 3rd degree polynomial of the time since the previous reform to handle duration.

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

[13] See Table A2 in the Appendix for a description of the variables used. Table A3 provides a correlation matrix.

places more weight on observations that are comparable and hence reduces reform 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, no separate conditional mean (OLS) models are estimated for the treated and the non-treated observations.[14] This means that we 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 LP or VAR setup (Jordà and Taylor, 2016). We report the Average Treatment Effect (ATE), which is calculated as the average difference in the prediction values between treated and non-treated (control) observations based on the weighted OLS regression line.

In the second stage AIPW regressions, we use the same specification as in equation (1), assuming that the controls affect the treated and non-treated sample in the same way. 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).

4. Empirical results

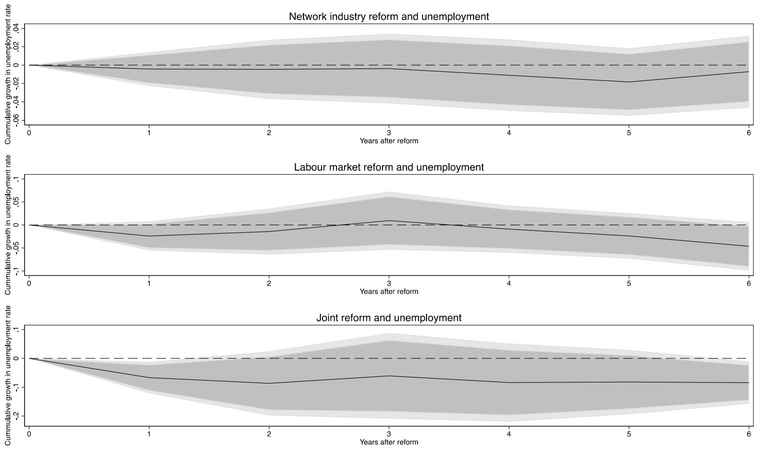
4.1 Baseline LP results

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). Figure 2 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. The underlying regressions for the IRFs shown in Figure 2 are reported in Tables A4-A6 in the Appendix. Our results suggest that a reform of network industries does not significantly affect unemployment. Likewise, labour market reforms do not significantly affect

[14] Note that AIPW analysis can also be done without this assumption. However, we do not have enough degrees of freedom to run separate weighted OLS regressions for the treatment and control group, particularly for labour market reforms and joint reforms there are too few treated observations.

unemployment over the forecast horizon. The Canova (2024) CV tests (shown in the last rows of Tables A4-A6) suggests that in most cases dynamic homogeneity cannot be rejected.

Fig. 2. Unconditional Local Projections: Effect of reform in network industries and labour markets on unemployment



Notes: The solid black lines in the figure plot the impulse responses of reform of network industries (upper panel) and labour market reforms (middle panel) and joint reforms (lower panel) on unemployment. Year=1 is the first year after a reform took place at year=0. So, the position of the line at e.g., year=6 shows the change in unemployment 6 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 for the first two graphs are shown in Tables A4-A6 in the Appendix.

4.2 Taking the bias due to the endogeneity of reforms into account

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 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 3. 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. In other words, structural reforms cannot be viewed as exogenous events. More specifically, Table 3 suggests that reforms of network industries are more likely during good economic times as suggested by the positive coefficients of the output gap, and GDP growth, and the negative coefficients of the change of the unemployment rate and inflation. Labour market reforms are more likely during bad times: GDP growth and unemployment are significantly lower when labour market reforms are implemented. Finally, joint reforms are implemented when unemployment is high.

Table 3. Balance tests of covariates: Reforms

Variables	Output gap	Output gap$_{t-1}$	Inflation	Inflation$_{t-1}$	GDP growth	GDP growth$_{t-1}$	Unempl. rate	Unempl. rate, dif.$_{t-1}$	Unempl. rate, dif.$_{t-2}$
Network industry reforms	0.083***	0.056**	-2.490***	-2.667***	0.003*	0.003	0.201***	-0.030**	-0.028*
	(0.022)	(0.023)	(0.479)	(0.499)	(0.002)	(0.002)	(0.047)	(0.014)	(0.014)
Labour market reforms	-0.038	-0.030	-1.391**	-1.310*	-0.007**	-0.008***	0.396***	0.054***	0.054**
	(0.032)	(0.033)	(0.708)	(0.738)	(0.003)	(0.003)	(0.069)	(0.020)	(0.021)
Joint reforms	0.059	0.030	-1.738	-2.029	0.003	0.000	0.425***	0.008	0.013
	(0.064)	(0.066)	(1.408)	(1.467)	(0.006)	(0.006)	(0.138)	(0.040)	(0.042)

Notes: Each row is the result of a regression where each variable in the columns 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 structural 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 Figure 2 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, 2016 for more details). Table A7 in the Appendix, which shows the post weighting balance tests, suggests that weighting is very effective in removing the selection bias.

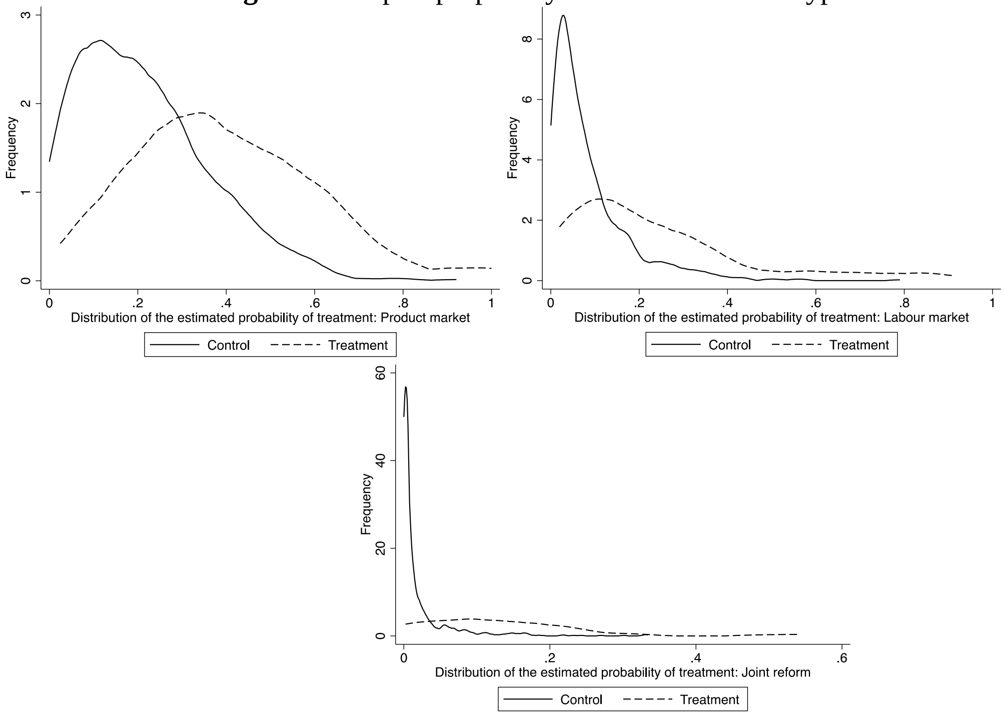
Table 4 shows the first-stage regressions. The models in Table 4 have a high predictive ability: the ‘area under the ROC curve’ is almost 0.8 and is statistically significantly different from 0.5. The graphs in Figure 3 provide smooth kernel density estimates of the distribution of the propensity scores 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 network industries reforms is based on column (1) in Table 4, while that of labour market reform is based on column (2). The graphs in Figure 3 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.

Table 4. Predictors of reforms in t+1 (using correlated random effects logit models)

VARIABLES	(1) Reforms of network industries	(2) Labour market reforms	(3) Joint reforms
Labour mark. reform	0.104**		
	(0.043)		
Reform network industries		0.016	
		(0.021)	
Output gap	-0.224	0.327***	0.076
	(0.146)	(0.098)	(0.054)
Output gap (-1)	0.077	0.083	0.049
	(0.079)	(0.056)	(0.035)
Inflation	-0.019**	0.001	-0.002
	(0.009)	(0.005)	(0.004)
Inflation (-1)	-0.011	-0.003	0.001
	(0.009)	(0.005)	(0.003)
Economic growth	4.014***	-1.706**	-0.114
	(1.148)	(0.769)	(0.453)
Economic growth (-1)	2.171	-2.320**	-0.834
	(1.341)	(1.006)	(0.556)
Log unemployment	0.325**	0.344***	0.097*
	(0.133)	(0.084)	(0.050)
Log unemployment (-1)	-0.220*	-0.223***	-0.056
	(0.122)	(0.073)	(0.043)
Government ideology	0.034*	-0.011	-0.007
	(0.018)	(0.012)	(0.007)
Political fragmentation	-0.001	-0.092*	0.008
	(0.065)	(0.051)	(0.026)
Government yrs. in office	0.002	-0.008*	-0.001
	(0.005)	(0.004)	(0.002)
Effective number parties	-0.055	0.075***	0.023
	(0.043)	(0.025)	(0.018)
Elections	-0.007	0.012	-0.006
	(0.029)	(0.021)	(0.012)
t	0.123***	0.023***	-0.010*
	(0.017)	(0.007)	(0.005)
t2	-0.013***	-0.001***	0.001**
	(0.002)	(0.000)	(0.000)
t3	0.000***	0.000**	-0.000**
	(0.000)	(0.000)	(0.000)
Country FEs (country means of predictor variables)	Yes	Yes	Yes
Observations	896	896	896
Area under ROC curve	0.768	0.794	0.876

Notes: The table reports the marginal effects at the means of a correlated random effects logit specification to predict the probability of treatment in t+1. In column (3), treatment is defined as observations in which both types of reform occurred simultaneously. Robust standard errors are shown in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

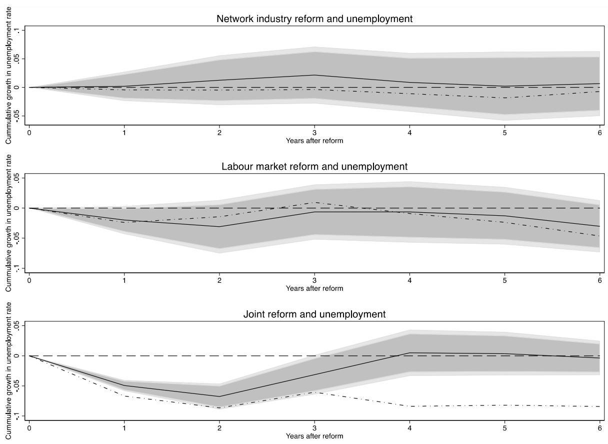
Fig. 3. Overlap of propensity scores for different types of reforms



4.3 Quasi experimental results

Figure 4 shows the AIPW results. The results for both types of reform deviate from those based on the simple LP model, as shown by the dashed-dotted lines in Figure 4. Reform of network industries and labour market reforms have limited effects on unemployment. When both types of reform occur simultaneously, unemployment first drops and then increases and goes back to zero. 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, particularly for joint reforms.

Fig. 4 AIPW results: Effect of reforms in network industries and labour markets on unemployment

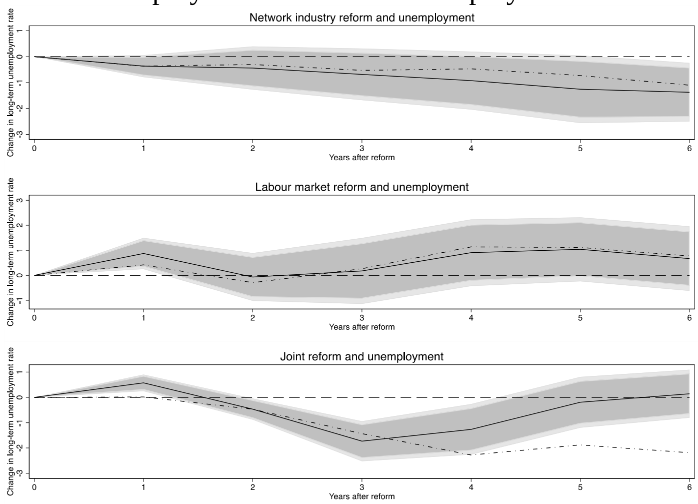


Notes: The solid black lines in the figure plot the impulse responses of network industries reforms (upper panel) and labour market reforms (middle panel) and joint reforms (lower panel) on unemployment. The dark grey shaded areas display the 90% error bands, the light grey shaded areas display the 95% error bands. The dotted-dashed line displays impulse responses from the simple LP regressions. Number of observations: 896

4.4 Effect of reform on long-term unemployment rate as share of unemployed

Finally, we examine the effects of structural reforms on long-term unemployment. The underlying regressions of the IRFs shown are available on request. Figure 5 shows the effects of reform on long-term unemployment. Note that we lose about 150 observations when using long-term unemployment. Figure 5 suggest that although the direction of the impact of structural reforms on long-term unemployment is similar to those on unemployment, the results are less clear-cut.[15] Perhaps the loss of human capital due to long-term unemployment makes that long-term unemployed do not benefit as much from labour market reforms as short-term unemployed.

Fig. 5. AIPW results: effect reform of network industries and labour market reform on long-term unemployment as share of unemployed



Notes: The solid black lines in the figure plot the impulse responses of network industries reforms (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 using the same sample and data. Number of observations 738.

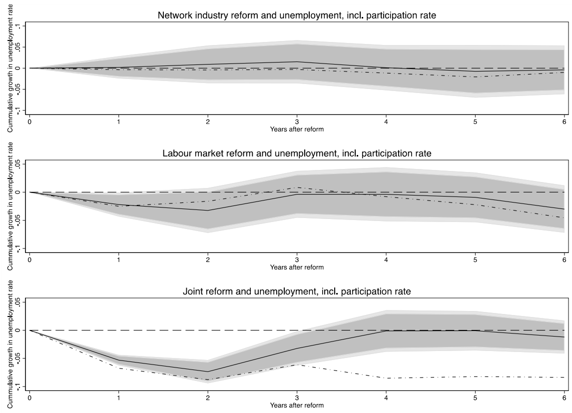
5. Robustness analysis

5.1 Including additional variable in the first and second stage model

We have included the participation rate in the first stage and second stage of the AIPW model when using the growth in the unemployment rate as outcome. The reason we did not include this variable in our baseline model is that it reduces the number of observations to 878. The results, as shown in Figure 6, suggest that our results are robust to the addition of the participation rate.

Fig. 6. AIPW: Effect of structural reforms on unemployment when the participation rate is added in the first stage and second stage of the model

[15] 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.



Note: The solid black lines in the figure plots the impulse responses of structural 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 dotted-dashed line displays impulse responses from the simple LP regressions using the same sample and data. Number of observations 778.

5.2 Dropping countries from the sample

Next, we dropped each country in the sample and redid all estimations to examine how robust the estimation results are to the removal of individual countries. Tables 5, 6, and 7 which present the ATEs, show that our results do not change when individual countries are excluded from the sample.

Table 5. ATEs of network industries reforms when dropping individual countries

Country left out	h=1	h=2	h=3	h=4	h=5	h=6	Observations
Australia	0.002	0.013	0.022	0.006	-0.001	0.006	857
	(0.014)	(0.023)	(0.026)	(0.028)	(0.033)	(0.031)	
Austria	0.006	0.014	0.022	0.009	-0.000	0.008	857
	(0.013)	(0.023)	(0.026)	(0.026)	(0.031)	(0.028)	
Belgium	0.003	0.014	0.022	0.009	0.004	0.010	857
	(0.013)	(0.022)	(0.025)	(0.026)	(0.031)	(0.029)	
Canada	0.001	0.012	0.022	0.007	-0.001	0.003	857
	(0.013)	(0.022)	(0.025)	(0.027)	(0.032)	(0.029)	
Czech Republic	0.003	0.015	0.025	0.013	0.009	0.016	877
	(0.013)	(0.022)	(0.025)	(0.026)	(0.031)	(0.029)	
Denmark	0.003	0.015	0.028	0.013	0.011	0.013	857
	(0.014)	(0.023)	(0.026)	(0.027)	(0.031)	(0.030)	
Finland	0.005	0.019	0.031	0.022	0.012	0.013	858
	(0.013)	(0.022)	(0.025)	(0.027)	(0.033)	(0.030)	
France	0.000	0.009	0.018	0.005	0.002	0.009	857
	(0.013)	(0.022)	(0.025)	(0.026)	(0.031)	(0.029)	
Germany	0.003	0.013	0.022	0.008	0.000	0.003	857
	(0.013)	(0.022)	(0.025)	(0.026)	(0.031)	(0.029)	
Greece	0.002	0.012	0.022	0.008	-0.001	0.002	879
	(0.012)	(0.021)	(0.024)	(0.025)	(0.030)	(0.029)	
Iceland	0.003	0.011	0.015	0.007	-0.001	0.002	857
	(0.012)	(0.021)	(0.023)	(0.024)	(0.029)	(0.028)	
Ireland	0.000	0.011	0.023	0.012	0.007	0.010	874
	(0.012)	(0.021)	(0.024)	(0.025)	(0.029)	(0.027)	
Italy	0.001	0.011	0.020	0.009	0.006	0.009	857
	(0.013)	(0.021)	(0.024)	(0.024)	(0.029)	(0.028)	
Japan	-0.001	0.012	0.024	0.012	0.007	0.014	857
	(0.014)	(0.024)	(0.026)	(0.026)	(0.030)	(0.030)	
Korea	0.004	0.013	0.015	0.007	0.005	0.010	858
	(0.012)	(0.020)	(0.023)	(0.023)	(0.029)	(0.029)	
Luxembourg	0.001	0.011	0.022	0.008	0.002	0.009	869
	(0.013)	(0.021)	(0.024)	(0.025)	(0.030)	(0.028)	
Netherlands	0.002	0.017	0.032	0.016	0.010	0.013	857
	(0.012)	(0.020)	(0.022)	(0.024)	(0.028)	(0.027)	
New Zealand	-0.003	0.008	0.019	0.002	-0.004	0.005	857
	(0.012)	(0.020)	(0.023)	(0.026)	(0.033)	(0.032)	
Norway	-0.005	0.003	0.012	0.008	0.006	0.012	857
	(0.011)	(0.016)	(0.019)	(0.024)	(0.032)	(0.032)	
Portugal	0.001	0.012	0.023	0.015	0.011	0.011	857
	(0.013)	(0.022)	(0.025)	(0.025)	(0.030)	(0.031)	
Spain	0.002	0.015	0.025	0.014	0.007	0.008	861
	(0.012)	(0.020)	(0.023)	(0.025)	(0.030)	(0.029)	
Sweden	0.008	0.022	0.026	0.002	-0.014	-0.010	857
	(0.012)	(0.020)	(0.025)	(0.028)	(0.028)	(0.023)	
Switzerland	-0.002	0.005	0.011	-0.007	-0.011	-0.004	859
	(0.015)	(0.025)	(0.027)	(0.027)	(0.032)	(0.029)	
United Kingdom	0.002	0.012	0.014	-0.007	-0.018	-0.010	857
	(0.013)	(0.023)	(0.025)	(0.027)	(0.031)	(0.027)	
United States	0.004	0.012	0.022	0.017	0.012	0.012	857
	(0.013)	(0.022)	(0.025)	(0.026)	(0.030)	(0.028)	

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

Table 6. ATEs of labour market reforms when dropping individual countries

Country left out	h=1	h=2	h=3	h=4	h=5	h=6	Observations
Australia	-0.020	-0.025	0.004	-0.003	-0.012	-0.034	857
	(0.012)	(0.023)	(0.020)	(0.024)	(0.022)	(0.022)	
Austria	-0.024**	-0.036	-0.011	-0.012	-0.016	-0.033	857
	(0.012)	(0.024)	(0.023)	(0.025)	(0.025)	(0.023)	
Belgium	-0.023*	-0.035	-0.008	-0.013	-0.017	-0.035	857
	(0.012)	(0.023)	(0.023)	(0.024)	(0.023)	(0.022)	
Canada	-0.024*	-0.037	-0.011	-0.015	-0.024	-0.042*	857
	(0.013)	(0.024)	(0.023)	(0.026)	(0.025)	(0.023)	
Czech Republic	-0.019	-0.028	-0.001	0.004	0.000	-0.017	877
	(0.011)	(0.022)	(0.022)	(0.024)	(0.023)	(0.021)	
Denmark	-0.020*	-0.027	-0.005	-0.002	-0.006	-0.018	857
	(0.012)	(0.023)	(0.022)	(0.025)	(0.023)	(0.021)	
Finland	-0.020*	-0.030	-0.006	-0.007	-0.012	-0.029	858
	(0.011)	(0.022)	(0.021)	(0.023)	(0.021)	(0.020)	
France	-0.023*	-0.033	-0.003	-0.003	-0.015	-0.031	857
	(0.013)	(0.025)	(0.024)	(0.027)	(0.025)	(0.024)	
Germany	-0.022*	-0.040*	-0.012	-0.006	-0.013	-0.031	857
	(0.011)	(0.022)	(0.021)	(0.023)	(0.022)	(0.021)	
Greece	-0.015	-0.023	0.002	0.003	-0.005	-0.030	879
	(0.011)	(0.021)	(0.020)	(0.022)	(0.021)	(0.020)	
Iceland	-0.018*	-0.027	-0.010	-0.008	-0.022	-0.037*	857
	(0.010)	(0.021)	(0.025)	(0.025)	(0.024)	(0.021)	
Ireland	-0.021*	-0.037*	-0.016	-0.021	-0.027	-0.045**	874
	(0.012)	(0.022)	(0.020)	(0.022)	(0.021)	(0.020)	
Italy	-0.024*	-0.042*	-0.018	-0.019	-0.030	-0.043**	857
	(0.013)	(0.024)	(0.022)	(0.024)	(0.021)	(0.021)	
Japan	-0.024**	-0.036*	-0.010	-0.009	-0.016	-0.032	857
	(0.011)	(0.022)	(0.022)	(0.024)	(0.022)	(0.021)	
Korea	-0.015	-0.023	0.001	0.004	-0.005	-0.024	858
	(0.010)	(0.020)	(0.021)	(0.022)	(0.023)	(0.022)	
Luxembourg	-0.015	-0.018	0.008	0.008	0.002	-0.019	869
	(0.011)	(0.020)	(0.020)	(0.023)	(0.022)	(0.022)	
Netherlands	-0.017	-0.015	0.006	-0.005	-0.018	-0.043*	857
	(0.012)	(0.022)	(0.023)	(0.027)	(0.025)	(0.023)	
New Zealand	-0.017	-0.027	-0.001	0.000	-0.008	-0.030	857
	(0.013)	(0.025)	(0.024)	(0.027)	(0.024)	(0.022)	
Norway	-0.020	-0.026	-0.001	0.008	0.005	-0.007	857
	(0.013)	(0.024)	(0.025)	(0.027)	(0.025)	(0.021)	
Portugal	-0.027**	-0.039*	-0.005	-0.003	-0.006	-0.024	857
	(0.012)	(0.022)	(0.023)	(0.025)	(0.023)	(0.020)	
Spain	-0.020	-0.027	0.001	-0.005	-0.007	-0.030	861
	(0.012)	(0.024)	(0.024)	(0.027)	(0.025)	(0.022)	
Sweden	-0.021*	-0.034	-0.010	-0.011	-0.019	-0.036*	857
	(0.013)	(0.023)	(0.023)	(0.026)	(0.024)	(0.021)	
Switzerland	-0.013	-0.020	0.004	0.011	0.014	-0.003	859
	(0.012)	(0.023)	(0.024)	(0.025)	(0.021)	(0.019)	
United Kingdom	-0.019*	-0.033	-0.015	-0.020	-0.026	-0.041**	857
	(0.011)	(0.021)	(0.021)	(0.022)	(0.021)	(0.019)	
United States	-0.019	-0.028	-0.002	-0.000	-0.007	-0.024	857
	(0.012)	(0.023)	(0.023)	(0.025)	(0.024)	(0.022)	

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

Table 7. ATEs of joint reforms when dropping individual countries

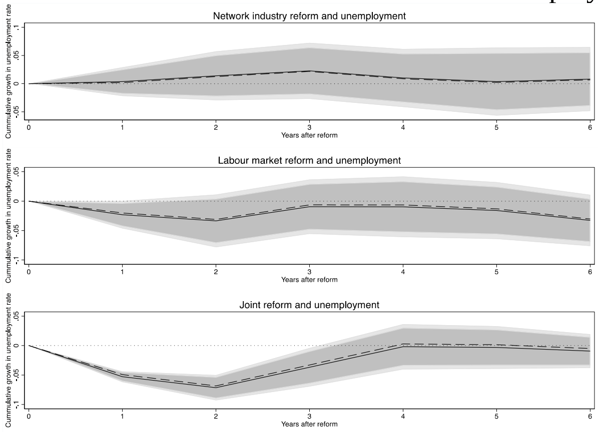
Country left out	h=1	h=2	h=3	h=4	h=5	h=6	Observations
Australia	-0.048***	-0.067***	-0.031*	0.004	0.001	-0.009	857
	(0.005)	(0.011)	(0.017)	(0.021)	(0.019)	(0.015)	
Austria	-0.050***	-0.070***	-0.033*	0.003	0.004	-0.002	857
	(0.005)	(0.012)	(0.018)	(0.021)	(0.019)	(0.015)	
Belgium	-0.051***	-0.065***	-0.041**	-0.018	-0.027	-0.034***	857
	(0.004)	(0.011)	(0.017)	(0.019)	(0.018)	(0.013)	
Canada	-0.052***	-0.072***	-0.035**	-0.003	-0.002	-0.008	857
	(0.005)	(0.012)	(0.018)	(0.021)	(0.019)	(0.015)	
Czech Republic	-0.048***	-0.061***	-0.019	0.020	0.022	0.013	877
	(0.005)	(0.011)	(0.017)	(0.020)	(0.019)	(0.014)	
Denmark	-0.046***	-0.058***	-0.036**	-0.005	-0.008	-0.013	857
	(0.005)	(0.012)	(0.018)	(0.021)	(0.019)	(0.014)	
Finland	-0.046***	-0.059***	-0.021	0.009	0.005	-0.006	858
	(0.004)	(0.010)	(0.017)	(0.020)	(0.018)	(0.014)	
France	-0.050***	-0.067***	-0.030*	0.006	0.005	-0.002	857
	(0.005)	(0.011)	(0.017)	(0.020)	(0.019)	(0.015)	
Germany	-0.045***	-0.055***	-0.004	0.042**	0.049***	0.046***	857
	(0.004)	(0.011)	(0.017)	(0.019)	(0.018)	(0.013)	
Greece	-0.052***	-0.073***	-0.038**	-0.007	-0.010	-0.017	879
	(0.005)	(0.011)	(0.017)	(0.020)	(0.019)	(0.014)	
Iceland	-0.052***	-0.089***	-0.076***	-0.043**	-0.043**	-0.038***	857
	(0.004)	(0.011)	(0.016)	(0.018)	(0.017)	(0.013)	
Ireland	-0.048***	-0.073***	-0.042***	-0.009	-0.012	-0.018	874
	(0.004)	(0.010)	(0.015)	(0.018)	(0.017)	(0.013)	
Italy	-0.049***	-0.063***	-0.023	0.011	0.007	-0.000	857
	(0.005)	(0.011)	(0.017)	(0.020)	(0.019)	(0.015)	
Japan	-0.050***	-0.065***	-0.023	0.015	0.012	-0.000	857
	(0.004)	(0.011)	(0.017)	(0.020)	(0.019)	(0.014)	
Korea	-0.045***	-0.053***	-0.013	0.024	0.017	0.005	858
	(0.003)	(0.009)	(0.016)	(0.019)	(0.017)	(0.013)	
Luxembourg	-0.048***	-0.066***	-0.030*	0.006	0.006	-0.002	869
	(0.005)	(0.011)	(0.017)	(0.020)	(0.019)	(0.015)	
Netherlands	-0.048***	-0.044***	-0.016	-0.007	-0.012	-0.033**	857
	(0.004)	(0.009)	(0.014)	(0.019)	(0.018)	(0.016)	
New Zealand	-0.049***	-0.072***	-0.041**	-0.007	-0.013	-0.020	857
	(0.005)	(0.011)	(0.017)	(0.020)	(0.019)	(0.015)	
Norway	-0.044***	-0.062***	-0.017	0.017	0.043**	0.057***	857
	(0.004)	(0.010)	(0.014)	(0.018)	(0.017)	(0.012)	
Portugal	-0.047***	-0.064***	-0.016	0.017	0.014	0.005	857
	(0.005)	(0.012)	(0.017)	(0.021)	(0.019)	(0.015)	
Spain	-0.042***	-0.047***	0.016	0.062***	0.052***	0.013	861
	(0.005)	(0.011)	(0.014)	(0.015)	(0.015)	(0.013)	
Sweden	-0.052***	-0.082***	-0.056***	-0.021	-0.026	-0.031**	857
	(0.005)	(0.011)	(0.016)	(0.019)	(0.017)	(0.014)	
Switzerland	-0.048***	-0.067***	-0.036**	0.002	0.003	0.001	859
	(0.006)	(0.012)	(0.017)	(0.020)	(0.018)	(0.014)	
United Kingdom	-0.060***	-0.108***	-0.096***	-0.073***	-0.083***	-0.078***	857
	(0.004)	(0.009)	(0.013)	(0.016)	(0.014)	(0.011)	
United States	-0.050***	-0.070***	-0.033**	0.004	0.003	-0.004	857
	(0.005)	(0.011)	(0.017)	(0.020)	(0.019)	(0.015)	

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

5.3 Herbst and Johannesen bias correction

Next, we use the bias corrected LP estimator derived in Herbst and Johanssen (2024). Herbst and Johannsen (2024) show that LPs may be biased. The bias is akin of the Nickell bias, but more severe when present in a LP because the local projection estimates at each forecast horizon no longer can be considered local. The bias is more severe when there is high persistence, i.e., the sum of the coefficients of the lagged dependent variables is higher than 0.9. We have low persistence in most of our models as the sum of the coefficients of the lagged dependent variables is less than 0.5 all specifications when h=1 (when h>1 the persistence adds up because we are estimating cumulative growth rates, so the sum of the coefficients of the lagged dependent variables are not directly comparable at h>1). Nevertheless, we implement the bias correction proposed Herbst and Johannsen (2024) since the bias also increases with the forecast horizon, h, and shorter time-series dimension T. We have relatively low h, but also low T. The results from that estimator are very similar to our previous estimates for eq. (1), as shown in Figure 7. We therefore conclude that our models, due to the generally low persistence, are robust to the bias. Since the times series of most countries in our panel is between 36 and 38 observations, but we have a few countries with less data we apply the Herbst and Johannesen bias correction assuming that T is 35. Notice a smaller T implies a larger correction, as T goes to infinity the correction goes to zero. (we cannot use a country specific T in this case as the bias correction is done directly on the estimated coefficient from the LP). The results for our baseline results are shown below in Figure 7 and confirm the main thrust of the findings reported earlier.

Fig. 7. AIPW: Effect of structural reforms on unemployment



Notes: The solid (dashed) black lines in the figure plot the (non) bias corrected impulse responses of reforms on our dependent variable. Year=1 is the first year after a counter-reform took place at year=0. So, the position of the line at e.g., year=6 shows the cumulative growth in the unemployment rate 6 years after the counter-reform. The dark grey shaded areas display the 90% SCC error bands; the light grey shaded areas display the 95% SCC error bands around the Herbst and Johannsen bias corrected AIPW-LP estimates.

6. Conclusions

This paper explored the impact of structural reforms on unemployment across 25 OECD countries from 1970 to 2020, employing local projection models to delve into the immediate and extended effects of reforms within labour markets and network industries.

Our local projection results suggest that both reforms of network industries and the labour market do not affect unemployment. Furthermore, we observed that dynamic heterogeneity—variations in effects across different countries—could be rejected. Controlling for the bias due to the endogeneity of reforms, the results for both types of reform deviate substantially form those based on the simple LP model. We take endogeneity into account by applying the Augmented Inverse Probability Weighted estimator proposed by Jordà and Taylor (2016) and Glynn and Quinn (2010). Reform of network industries increase unemployment, possibly due to the disruptive effects these reforms can have on existing employment structures and the lag time needed for industries and workers to adjust to new regulatory and market conditions. At the same time, labour market reforms decrease unemployment which could be attributed to the fact that such reforms often aim to make labour markets more flexible, reducing hiring and firing costs, and potentially encouraging employers to create more jobs. These reforms can also lead to enhanced matching between job seekers and openings, improving overall employment efficiency. So, our results show that it is crucial to take endogeneity of structural reforms into account when analysing their effects on unemployment.

The paper´s limitations are primarily related to potential omitted variables that could influence the results and the generalizability of these findings across different economic contexts and temporal periods. The robustness of our conclusions is further constrained by the variability in policy implementation across the analysed countries.

Further research could expand on these findings by including a wider range of structural reforms and examining their impacts on specific economic sectors. Extending this analysis to include non-OECD countries would also provide a more comprehensive understanding of these reforms' global effects.

Our findings emphasize the necessity for policymakers to meticulously plan and implement structural reforms, taking into consideration their timing and magnitude to mitigate adverse impacts on employment. It is vital for labour market reforms, in particular, to be crafted to enhance flexibility while safeguarding employment stability.

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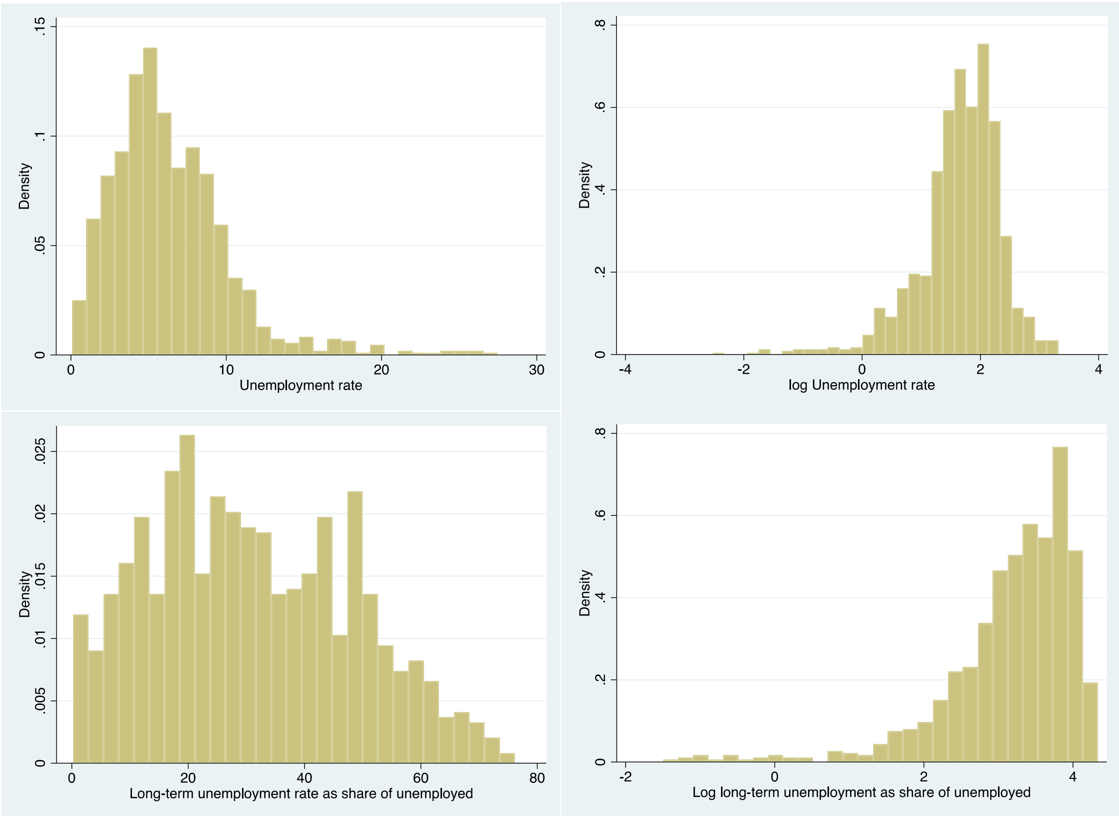
Online Appendix

Figures

Fig. A1. Distribution of unemployment



Tables

Table A1. Critical values based on the bootstrap method from Canova (2024), 5% significance level

	h=1	h=2	h=3	h=4	h=5	h=6	h=7
Critical value	1.335	0.969	1.027	0.999	1.003	1.000	1.001

Note: We use the method described in Canova (2024) to calculate the critical values. We set T=40 and N=20. When estimating the coefficient of variation based on each time-series estimate unit-by-unit, we lose the units without counter reforms such that N becomes lower than what can be used in the panel estimates.

Table A2. 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	896	6.731	3.717	0.186	27.466	OECD
Long-term unemployment rate	Unemployment rate of those working age people in unemployment status for 12 months or more	738	30.363	17.267	0.224	76.167	OECD
Reforms of network industries	See main text	896	0.26	0.439	0	1	Wiese et al. (2024)
Labour market reforms	See main text	896	0.1	0.301	0	1	Wiese et al. (2024)
Output gap	Calculated using Hamilton filter to real GDP at constant 2017 national prices (in 2017 US$)	896	-0.002	0.291	-1.575	1.02	PWT
Inflation rate	Annual growth rate in the consumer price index	896	4.698	6.38	-4.478	83.95	OECD
Economic growth	Log difference of real GDP per capita at constant 2017 national prices (in 2017 US$)	896	0.019	0.026	-0.102	0.111	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)	896	2.072	0.814	0.716	3.171	De Haan and Wiese (2022)
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	896	0.193	0.31	0	1	De Haan and Wiese (2022)
Government years in office	The number of years in which the same government held office	896	3.994	3.047	1	18	De Haan and Wiese (2022)
Effective number of parties	The inverse of the sum of the squared seats shares of each government in office	896	1.636	0.778	0.965	3.932	De Haan and Wiese (2022)
Any election, legislative or executive	Equal to 1 in years where either a legislative and/or executive election took place, otherwise equal to 0	896	0.326	0.469	0	1	De Haan and Wiese (2022)
Collective bargaining	Equal to 1 in years where collective bargaining	889	67.137	27.38	12.2	100	Afonso et al. (2023)

Table A3. Pairwise correlations

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1) Reform network industries	1.000									
(2) Reform labour markets	-0.020	1.000								
(3) Output gap	0.125	-0.040	1.000							
(4) Inflation rate	-0.171	-0.066	-0.087	1.000						
(5) Economic growth	0.056	-0.083	0.725	0.043	1.000					
(6) Ideology of government	0.035	-0.035	-0.017	-0.006	0.037	1.000				
(7) Political fragmentation	-0.006	-0.051	-0.011	-0.031	-0.045	-0.035	1.000			
(8) Government years in office	0.023	-0.049	-0.053	-0.046	-0.049	0.019	-0.015	1.000		
(9) Effective number of parties	-0.043	0.016	-0.018	-0.022	-0.088	0.060	0.617	-0.141	1.000	
(10) Any election	-0.027	0.005	-0.003	0.052	-0.017	0.021	0.007	0.100	-0.019	1.000

Note: Based on estimation sample: 896 observations

Table A.4 Local Projections: Reforms of network industries and unemployment

VARIABLES	(1) unemp1	(2) unemp2	(3) unemp3	(4) unemp4	(5) unemp5	(6) unemp6
Reform	-0.004	-0.005	-0.004	-0.011	-0.018	-0.007
	(0.009)	(0.016)	(0.019)	(0.020)	(0.019)	(0.020)
Reform (-1)	0.004	0.007	0.003	-0.005	0.001	0.006
	(0.012)	(0.015)	(0.017)	(0.017)	(0.018)	(0.016)
Reform (-2)	0.007	0.006	-0.002	0.005	0.008	0.019
	(0.010)	(0.014)	(0.016)	(0.017)	(0.015)	(0.017)
Reform (-3)	-0.012*	-0.021	-0.013	-0.006	0.009	0.008
	(0.007)	(0.013)	(0.015)	(0.020)	(0.022)	(0.025)
Output gap	0.011	0.114	0.198**	0.353***	0.492***	0.494***
	(0.050)	(0.079)	(0.089)	(0.096)	(0.101)	(0.109)
Output gap (-1)	0.002	-0.009	0.033	0.068	0.150*	0.251***
	(0.025)	(0.063)	(0.080)	(0.076)	(0.077)	(0.074)
Inflation	0.003	0.003	0.000	-0.001	-0.005	-0.003
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Inflation (-1)	-0.003**	-0.004*	-0.002	-0.001	0.003	0.002
	(0.001)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Economic growth	-2.951***	-4.344***	-5.057***	-5.894***	-6.464***	-5.915***
	(0.432)	(0.842)	(0.960)	(0.917)	(1.010)	(1.020)
Economic growth (-1)	-0.163	-0.875	-2.171**	-3.385***	-4.790***	-5.280***
	(0.540)	(0.767)	(0.877)	(0.933)	(0.922)	(1.144)
Unemployment	-0.111***	-0.254***	-0.382***	-0.497***	-0.565***	-0.627***
	(0.024)	(0.049)	(0.067)	(0.081)	(0.093)	(0.104)
Unemployment (-1)	0.003	0.074	-0.063	-0.053	-0.073	-0.122
	(0.033)	(0.110)	(0.147)	(0.151)	(0.151)	(0.144)
Unemployment (-2)	0.013	-0.153**	-0.126*	-0.143*	-0.143*	0.020
	(0.065)	(0.062)	(0.066)	(0.080)	(0.077)	(0.071)
Constant	0.155***	0.389***	0.688***	0.972***	1.102***	1.213***
	(0.054)	(0.095)	(0.120)	(0.139)	(0.169)	(0.197)
Leads of reforms and counter reforms	Yes	Yes	Yes	Yes	Yes	Yes
Country and time fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	896	896	896	896	896	896
Number of groups	25	25	25	25	25	25
Canova CV test	2.652§	1.030	0.977	0.790	0.524	1.442§

Notes: SCC standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. § indicates Canova CV test statistic rejects the null of dynamic homogeneity at the 5% level.

Table A.5 Local Projections: Labour market reforms and unemployment

VARIABLES	(1) unemp1	(2) unemp2	(3) unemp3	(4) unemp4	(5) unemp5	(6) unemp6
Reform	-0.024	-0.014	0.009	-0.009	-0.024	-0.046*
	(0.016)	(0.025)	(0.032)	(0.026)	(0.025)	(0.027)
Reform (-1)	-0.005	0.019	0.002	-0.010	-0.030	-0.019
	(0.015)	(0.029)	(0.027)	(0.029)	(0.032)	(0.027)
Reform (-2)	-0.000	-0.017	-0.034	-0.051	-0.042	-0.051*
	(0.016)	(0.018)	(0.025)	(0.032)	(0.028)	(0.028)
Reform (-3)	-0.019	-0.029	-0.045	-0.026	-0.031	-0.030
	(0.013)	(0.022)	(0.027)	(0.024)	(0.024)	(0.022)
Output gap	0.011	0.102	0.205**	0.377***	0.550***	0.570***
	(0.047)	(0.079)	(0.092)	(0.095)	(0.109)	(0.108)
Output gap (-1)	0.007	-0.000	0.033	0.105	0.200***	0.309***
	(0.028)	(0.063)	(0.076)	(0.072)	(0.071)	(0.073)
Inflation	0.003	0.003	0.001	-0.001	-0.005	-0.003
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Inflation (-1)	-0.003**	-0.004*	-0.002	-0.000	0.003	0.003
	(0.001)	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Economic growth	-2.906***	-4.123***	-4.956***	-5.875***	-6.800***	-6.251***
	(0.431)	(0.843)	(0.981)	(0.999)	(1.094)	(1.017)
Economic growth (-1)	-0.283	-1.006	-2.353**	-4.080***	-5.442***	-6.050***
	(0.568)	(0.796)	(0.947)	(1.063)	(1.001)	(1.132)
Unemployment	-0.110***	-0.253***	-0.371***	-0.471***	-0.524***	-0.568***
	(0.024)	(0.048)	(0.064)	(0.075)	(0.085)	(0.094)
Unemployment (-1)	-0.002	0.048	-0.095	-0.112	-0.104	-0.114
	(0.034)	(0.116)	(0.151)	(0.154)	(0.141)	(0.137)
Unemployment (-2)	0.017	-0.145**	-0.139**	-0.128	-0.106	0.043
	(0.067)	(0.068)	(0.067)	(0.079)	(0.078)	(0.079)
Constant	0.158***	0.375***	0.633***	0.885***	0.984***	1.074***
	(0.054)	(0.091)	(0.107)	(0.126)	(0.161)	(0.187)
Leads of reforms and counter reforms	Yes	Yes	Yes	Yes	Yes	Yes
Country and time fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	896	896	896	896	896	896
Number of groups	25	25	25	25	25	25
Canova CV test	-0.734	0.619	0.665	0.889	-0.926	1.706§

Notes: SCC standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. § indicates Canova CV test statistic rejects the null of dynamic homogeneity at the 5% level.

Table A.6 Local Projections: Joint reforms and unemployment

VARIABLES	(1) unemp1	(2) unemp2	(3) unemp3	(4) unemp4	(5) unemp5	(6) unemp6
Reform	-0.067**	-0.087	-0.061	-0.084	-0.082	-0.084**
	(0.027)	(0.057)	(0.076)	(0.069)	(0.057)	(0.037)
Reform (-1)	-0.012	0.006	-0.020	-0.029	-0.042	-0.066**
	(0.038)	(0.067)	(0.062)	(0.053)	(0.035)	(0.031)
Reform (-2)	0.002	-0.016	-0.030	-0.039	-0.059	-0.059
	(0.037)	(0.038)	(0.041)	(0.035)	(0.040)	(0.054)
Reform (-3)	-0.029	-0.046**	-0.061**	-0.079**	-0.067	-0.080*
	(0.018)	(0.022)	(0.027)	(0.032)	(0.040)	(0.044)
Output gap	0.009	0.108	0.203**	0.372***	0.525***	0.539***
	(0.045)	(0.075)	(0.085)	(0.092)	(0.101)	(0.106)
Output gap (-1)	0.004	-0.004	0.037	0.085	0.171**	0.274***
	(0.026)	(0.063)	(0.080)	(0.076)	(0.075)	(0.074)
Inflation	0.003	0.003	0.000	-0.001	-0.005	-0.003
	(0.002)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Inflation (-1)	-0.003**	-0.004	-0.002	-0.001	0.003	0.002
	(0.001)	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
Economic growth	-0.187	-0.898	-2.289**	-3.761***	-5.178***	-5.758***
	(0.519)	(0.718)	(0.865)	(0.949)	(0.939)	(1.157)
Economic growth (-1)	-2.922***	-4.272***	-5.043***	-6.010***	-6.740***	-6.227***
	(0.400)	(0.813)	(0.925)	(0.924)	(1.044)	(1.048)
Unemployment	-0.111***	-0.254***	-0.381***	-0.492***	-0.560***	-0.619***
	(0.025)	(0.050)	(0.067)	(0.079)	(0.091)	(0.100)
Unemployment (-1)	0.002	0.073	-0.062	-0.061	-0.075	-0.116
	(0.033)	(0.109)	(0.146)	(0.150)	(0.147)	(0.144)
Unemployment (-2)	0.015	-0.147**	-0.126*	-0.137	-0.129	0.031
	(0.065)	(0.064)	(0.069)	(0.082)	(0.081)	(0.076)
Constant	0.155***	0.389***	0.687***	0.964***	1.098***	1.210***
	(0.054)	(0.094)	(0.116)	(0.134)	(0.168)	(0.196)
Leads of reforms and counter reforms	Yes	Yes	Yes	Yes	Yes	Yes
Country and time fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	896	896	896	896	896	896
Number of groups	25	25	25	25	25	25
Canova CV test	-12.01§	64.76§	1.565§	3.060§	-6.669§	-1.437§

Notes: SCC standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1. § indicates Canova CV test statistic rejects the null of dynamic homogeneity at the 5% level.

Table A7. Balance tests of covariates after reweighting

Variables	Output gap	Output gap$_{t-1}$	Inflation	Inflation$_{t-1}$	GDP growth	GDP growth$_{t-1}$	Unempl. rate	Unempl. rate, dif.$_{t-1}$	Unempl. rate, dif.$_{t-2}$
Reforms of network	-0.020	-0.014	-0.620	-0.083	0.000	-0.003	0.060	-0.010	-0.015
industries	(0.039)	(0.033)	(0.469)	(0.722)	(0.002)	(0.004)	(0.055)	(0.017)	(0.016)
Labour	0.027	0.000	-0.887	-0.888	-0.003	0.001	0.120*	-0.007	0.001
market reforms	(0.038)	(0.031)	(0.596)	(0.694)	(0.003)	(0.003)	(0.072)	(0.025)	(0.026)
Joint reforms	0.140*	0.082	-1.522*	-1.449	0.007	0.008	-0.041	-0.049	-0.073
	(0.084)	(0.112)	(0.798)	(1.192)	(0.007)	(0.006)	(0.187)	(0.093)	(0.055)
Obs.	896	896	896	896	896	896	896	896	896

Notes: Standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1, indicate if the two-sided t-test is statistically significant.