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Analyses of Topical Policy Issues

The impact of increasing labour market rigidity on employment growth in OECD countries

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ABSTRACT

This paper examines how labour market counter-reforms, i.e., measures that make it more difficult to hire and fire workers and that increase unemployment benefits, affect employment growth in 25 OECD countries. Although counter-reforms occur frequently, they have received little attention in empirical research. The theoretical effects of labour market counter-reforms on employment are ambiguous. For instance, higher firing costs may decrease employment as employers are less willing to hire. However, employment may also increase as layoffs may decline due to more stringent firing constraints. Moreover, the effects may depend on the type of contract; workers with a temporary contract have less employment protection. Increasing their protection may increase labour supply notably in an economic upswing when the labour market is tight. Our results suggest that employment protection counter-reforms for temporary workers increase employment growth when the economy is performing above trend. The effects of counter-reforms of unemployment benefits are insignificant.

1. Introduction

Understanding the dynamics and consequences of counter-reforms is critical for policymakers and international organizations working to support economic transitions and general economic development. For example, in the model of Galiani et al. (2017), a benevolent but politically myopic international organization may reduce social welfare, because it fails to account for the fact that an overly aggressive structural reform may trigger costly reversals that outweigh the benefits of the reform.[1] Although counter-reforms

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[1] Structural reforms refer to a broad range of measures aimed at altering the fundamental economic, institutional, and regulatory frameworks within which businesses and individuals operate. These reforms are designed to enhance the economy's efficiency, productivity, and growth potential in a balanced manner. They target various aspects of the economy, including labour markets, tax systems, and regulatory environments, to remove obstacles to efficient and equitable production of goods and services. This can involve making labour markets more flexible, simplifying tax codes, reducing bureaucratic red tape, and encouraging innovation in key industries (Da Silva et al., 2017).

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occur frequently, they have received little attention in empirical research. It is telling that in their review of the literature on reforms in advanced economies, Campos et al. (2024) mention counter-reforms only twice. The focus of most previous studies has been on the drivers and effects of reforms.[2] But there are exceptions. For example, Campos and Horváth (2012) analyse what drives the probability of counter-reforms in a sample of 25 Central and Eastern European countries.

In this paper, we examine the impact of labour market counter-reforms on employment growth for an almost balanced sample of 25 OECD countries between 1973 and 2012. More specifically, we consider measures that make it more difficult to hire and fire workers and measures that reduce the incentives for the inactive to find work. We use a narrative-based dataset of (counter-)reform indicators compiled by Duval et al. (2018) to construct counter-reform dummies. We employ the local projections (LP) approach (Jordà, 2005) which has been widely used to analyse the dynamic effects of policy shocks (Alesina et al., 2024; Haan and Wiese, 2022; Hülsewig and Rottmann, 2023; Jordà and Taylor, 2016). To alleviate the bias introduced by overlapping forecast horizons, we follow Teulings and Zubanov (2014) and include the leads of the counter-reform dummies in our models. We also test whether the counter-reforms have similar dynamic effects across countries, using a test recently proposed by Canova (2024).

The impact of labour market reforms on (un)employment has been studied extensively. Theoretically, the impact of labour market reforms on unemployment is ambiguous, as they often decrease labour market rigidity and reduce job security (Bordon et al., 2018). Reducing employment protection may increase unemployment, because layoffs are likely to increase in the short run when firing constraints are relaxed (Boeri et al., 2015). However, lowering firing costs may also increase employers' willingness to hire, thus potentially reducing unemployment, especially for younger or less experienced workers (Blanchard and Portugal, 2001). Moreover, the impact of employment protection may differ across workers with a permanent and those with a temporary contract. Several countries face a persistent dual labour market with a large discrepancy between the protection of workers with permanent and temporary contracts.[3] Workers who have been employed long enough benefit from high levels of employment protection, while those who have just been hired enjoy virtually none. This creates a 'revolving door' through which workers without a permanent contract move between short-term employment and unemployment (Dolado et al., 2021).

Several studies have investigated the impact of structural reforms on (un)employment (for reviews, see Boeri et al., 2015; Campos et al., 2018; 2024, and Parlevliet et al., 2018). Much of the 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 characterised by a premium 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, 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 to improve competition is simulated by assuming a reduction in entry costs which increases entry and reduces mark-ups.

Other studies present estimates of the impact of structural reforms on (un)employment using panel or cross-sectional data. For example, Bouis et al. (2012) find that unemployment benefit reforms (in particular, a reduction in the duration of unemployment benefits) boost employment. However, they also find some evidence that a reduction in the unemployment benefit replacement rate and employment protection reforms can lead to employment losses in severely depressed economies. Bordon et al. (2018) investigate the impact of structural reforms on employment using local projections. Their results suggest that structural reforms have a lagged but positive impact on employment. Duval et al. (2020) examine major reforms in employment protection legislation for permanent workers covering 26 advanced economies over the period 1970–2013. The authors report that the short-term employment effects of deregulating employment protection vary with the macroeconomic conditions prevailing at the time of reform—being positive in an expansion, but negative in a recession. Walter (2023) finds that the labour market reform in Germany reduced the short-term unemployment by 0.4 %. Finally, Rünstler (2024) reports that reforms of unemployment benefits and employment protection legislation for workers with a permanent contract in euro area countries led to a sustained increase in employment.

We are not aware of any studies that focus on counter-reforms in the labour market. A key question is whether counter-reforms have the inverse effect of reforms on employment growth as discussed above. Counter-reforms that reduce labour market flexibility may lead to higher unemployment in the short run (Blanchard and Giavazzi, 2003; OECD, 2019). Counter-reforms can be divided into employment protection legislation (EPL) and unemployment benefits (UB) counter-reforms.

Counter-reforms in EPL can reduce firms' incentives to hire new workers, especially in uncertain economic conditions. The literature suggests that strict EPL may discourage labour market dynamism by increasing the costs associated with workforce adjustments (Boeri and van Ours, 2008). For example, higher severance payments or stricter procedures for dismissals may make firms more reluctant to expand their workforce (Cahuc and Zylberberg, 2004; OECD, 2020). Especially in economies with already high levels of labour market rigidity, counter-reforms could exacerbate unemployment, particularly among vulnerable groups such as young workers or those with low skill levels (Nickell, 1997; OECD, 2019). On the other hand, offering more employment protection to workers on temporary contracts may attract new entrants to the labour market, particularly when the labour market is tight.

Economic theory suggests that more generous UB can have a significant effect on the incentives of the unemployed to search for work. For example, higher benefits may reduce the urgency for the unemployed to find a new job, potentially prolonging the duration of unemployment (see Krueger and Meyer, 2002; Meyer, 1990). Moreover, an increase in UB may change the relative attractiveness of employment, leading some individuals to (temporarily) leave the labour force. The impact of changes in UB also extends to wage

[2] For some recent contributions, see e.g. Duval and Furceri (2018) and de Haan and Wiese (2022).

[3] Obadić et al. (2023) report that even in the European Union there is no convergence in employment protection legislation.

Table 1
Number of reforms by category (25 advanced economies, 1973–2012).

Reform type	Number of reforms	Number of counter-reforms	Reforms (% of total shocks)	Counter-reforms (% of total shocks)
Labour market reforms	83	41	66.9 %	33.1 %
Employment protection legislation (counter-)reforms, permanent and temporary workers	57	22	71.3 %	28.7
Employment protection legislation (counter-)reforms for workers with permanent contract	28	14	66.6 %	33.3 %
Employment protection legislation (counter-)reforms for workers with temporary contracts	37	10	78.7 %	21.3 %
Unemployment benefit reforms	26	18	59.1 %	40.9

Note: The total number of observations is 974 (based on the 7-year forecast estimation sample).
Source: Wiese et al. (2024).

bargaining and employment costs. If unemployment benefits provide a higher income base during periods of unemployment, workers may demand higher wages to return to work (Layard et al., 1991). This increase in labour costs could make firms more cautious about hiring, thereby slowing employment growth (Blanchard and Portugal, 2001). The extension of UB may temporarily cushion income losses, but prolonged or overly generous unemployment benefits may reduce job search motivation and lead to longer unemployment spells (Krueger and Meyer, 2002). However, counter-reforms in UB can stabilise aggregate demand by maintaining consumer confidence and household spending, which can mitigate increases in unemployment during economic downturns (Auerbach and Gorodnichenko, 2012).

Our results suggest that the effects of labour market counter-reforms depend on the prevailing economic conditions and are not uniform across different types of counter-reforms. When distinguishing between EPL counter-reforms for workers with a permanent and a temporary contract, we find that the latter have a significant positive effect on employment growth when the economy is performing above trend, i.e., when the labour market is tight. However, the effects of counter-reforms of UB on employment growth are insignificant.

The rest of the paper is structured as follows. Section 2 discusses the data used. Section 3 outlines our methodology, while Section 4 presents our main results. Section 5 provides a robustness analysis. Finally, Section 6 concludes and discusses policy implications.

2. . Data and stylized facts

The main impact variable we use is employment from the Penn World Tables (PWT) version 10.1. We focus on the cumulative employment growth projected stepwise forward in time (with annual frequency), i.e., 0 to 1, 0 to 2 etc., up to 0 to 7 years after the counter-reform. It is calculated as the log difference of employment as share of the population. We use the logarithm as the distribution of the change in employment is rather skewed.

Labour market counter-reforms are taken from the Duval et al. (2018) database, which is based on documented legislative and regulatory measures reported in all available *OECD Economic Surveys* for 25 advanced economies, as well as additional country-specific sources.[4]

We define a counter-reform as a significant increase in labour market regulation to distinguish it from a reform, i.e., a significant deregulation of the labour market. This implies that a counter-reform need not follow a reform. When a counter-reform follows a reform, we use the term reform reversal instead. Note that we use the term counter-reforms following the terminology as used in the source database of Duval et al. (2018). Labour market counter-reforms can be divided into counter-reforms of employment protection legislation (EPL) and counter-reforms of unemployment benefits (UB). The former capture policies that make it more difficult to fire workers (with a temporary or permanent contract), while the latter capture increases in the level of unemployment benefits (duration or amount). EPL counter-reforms can further be split into counter-reforms of permanent contracts and counter-reforms of temporary contracts. The reform database has several advantages in identifying: the precise nature and exact timing of major legislative and regulatory measures in key labour market policy areas and the precise counter-reforms that underpin what otherwise appears to be a gradual increase in OECD policy indicators without any obvious or noticeable break. Moreover, the database captures counter-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 measures that underpin observed large changes in the OECD indicators. Finally, compared to 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) allows for the identification of a rather limited set of key legislative and regulatory counter-reforms, as opposed to a long list of actions that in some cases would be expected to have little or no impact on macroeconomic outcomes. This is particularly useful for empirical analyses that seek to identify, and then estimate, the dynamic effects of counter-reform shocks.

Table 1 presents stylised facts on reforms—i.e., decreases in regulation—and counter-reforms—i.e., increases in regulation. The latter account for about 30 % of the total labour market shocks.

[4] The 25 countries are displayed in Figure A1 in the online Appendix.

Table 2
Average change in the EPL strictness or UB generosity index from the OECD and narrative (counter-)reforms.

Reform type	(1) EPL temporary contracts strictness index	(2) EPL permanent contracts strictness index	(3) UB generosity index
No reform	−0.009	−0.005*	0.001
	(0.008)	(0.003)	(0.001)
Reform	−0.509***	−0.145***	−0.029***
	(0.033)	(0.015)	(0.008)
Counter-reform	0.236***	0.167***	0.086***
	(0.060)	(0.026)	(0.010)
Observations	614	614	896

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 the strictness of EPL (for workers with a permanent and a temporary contract) and the generosity of UB are related to reform, no reform, and counter-reform dummies. The results are based on OECD strictness and generosity index data availability in our 7-year forecast estmation sample. The EPL strictness index 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). Both come from OECD.org.

To validate our narrative (counter-)reform database, Table 2 shows how the annual change in the OECD indexes of EPL strictness (for permanent and temporary employment) and UB generosity are related to reform, no reform, or counter-reform dummies.[5] Years without shocks in our narrative reform indicator are not associated with any statistically significant change in the underlying OECD indicators of strictness or generosity. Clearly reforms are associated with a decrease in EPL strictness or UB generosity UB, while counter-reforms are associated with an increase in EPL strictness and UB generosity.

Figure A1 in the online Appendix provides country-specific details on labour market reforms together with our key dependent variable (the growth of employment as share of the population growth). Table A1 in the Appendix provides summary statistics for all data used.

3. Methodology

We estimate impulse response functions (IRFs) using the local projections (LP) approach of Jordà (2005).[6] This method is a flexible alternative to VARs, as local projections are not subject to dynamic restrictions. The LP approach uses OLS to directly estimate the IRFs at each forecast horizon by changing only the forecast for the outcome variable on the left-hand side of the regression equation, while the right-hand side remains identical for each forecast horizon, except for one important aspect as explained below. This is in contrast to a VAR where the IRFs are based on forward iterations from an underlying system of equations model. We follow the recommendations of Herbst and Johannesen (2024) and include lags of the dependent and independent variables in our dynamic two-way fixed effects panel data model. The Jordà method simply requires the estimation of a series of regressions for each horizon, h, and for each dependent variable of interest (in our case the growth of employment as a share of the population). The basic linear LP regression model that we estimate is:

$$\Delta \log y_{i,t+h} = \alpha_{i,h} + \delta_{t,h} + \sum_{j=0}^{4} \beta_{j,h} d_{i,t-j} + \sum_{l=0}^{4} \beta_{l,h} \left(log y_{i,t-l} - log y_{i,t-1-l} \right) + \sum_{m=1}^{h} \beta_h d_{i,t+h} + \sum_{c=0}^{1} \beta'_{c,h} X_{i,t-c} + u_{i,t+h} \quad (1)$$

where h = 1…,7 is the forecast horizon, and $\Delta \log y_{i,t+h} = \log y_{i,t+h} - \log y_{i,t}$ denotes the cumulative growth rate of employment over the forecast horizon. α_i denotes country fixed effects to 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.[7] $d_{i,t}$ denotes the counter-reform dummies that capture our externally identified shocks to labour market regulation. The dummy either captures counter-reforms in regulation for EPL and UB together, or counter-reforms in each type of regulation. Hence, $\beta_{j=0,h}$ measures the

[5] The OECD EPL strictness indexes are only avialable from 1985 onwards, for some countries even later.

[6] Montiel Olea et al. (2024) provide a formal proof of the claim in Jordà (2005) that conventional LP confidence intervals for impulse responses are robust to misspecification. These authors write that: "In stark contrast to LP, small amounts of misspecification cause conventional VAR confidence intervals for impulse responses to suffer from severe under-coverage asymptotically. We derive analytically the worst-case bias and coverage of VARs over all possible misspecification processes, subject to a constraint only on the overall magnitude of the misspecification. A "no free lunch" result for VARs emerges: the worst-case bias and coverage distortion are small if, and only if, the asymptotic variance is close to that of LP. This worst-case result is practically relevant—VAR confidence intervals severely undercover even when the misspecification term: (i) is small in magnitude; (ii) has dynamic properties that cannot be ruled out *ex ante* based on economic theory; and (iii) is difficult to detect *ex post* with model specification tests." (p. 3).

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

conditional mean of shocks to labour market regulation for each forecast horizon h on $\Delta\log y_{i,t+h}$, and is used to construct the IRFs and their associated confidence intervals. To avoid attrition and to facilitate comparability of results across different forecast horizons, we fix the sample at the data that is available at the longest forecast horizon (h = 7).[8] This gives us 974 observations for all regressions.

Treatment lags are included to capture the effect that previous shocks may have on the outcome variable. We use the Bayesian Information Criterion (BIC) to determine the lag length which tells us to use 4 lags of the treatment variable. We also include 4 lags of $\Delta\log y_{i,t}$ and the contemporaneous term to control for serial correlation in the error term, $u_{i,t+h}$ as suggested by the BIC. The data for the dependent variable are stationary as $|\sum_{l=0}^{4} \beta_{l,h=1}| < 1$ in all our specifications.[9] In fact, $\sum_{l=0}^{4} \beta_{l,h=1}$ is <0.5 for both our dependent variables when h = 1, which means that the persistence in the estimated models is low. Therefore, the estimated IRFs from the LPs are unlikely to be much affected by the bias that can result from a relatively short time dimension combined with high persistence, as shown by Herbst and Johanssen (2024).[10] In our case $t = 39$.

The term $\sum_{h=1}^{h} \beta_h d_{i,t+h}$ captures the Teulings and Zubanov (2014) correction. The leads of counter-reform dummies are included to avoid the bias arising from overlapping forecast horizons.[11] In addition, we also include the leads of labour market reform dummies to avoid the bias that they may cause if the reforms are in the forecast horizon of our model and have an opposite effect on the outcome. The coefficients on the leads of the reform and counter-reform dummies are statistically significant for most combinations of $y_{i,t}$ and h, indicating the need to control for overlapping forecast horizons. $X_{i,t}$ is a vector of control variables. $X_{i,t}$ contains the contemporaneous value and the first lag of the change in physical capital (gross investment relative to GDP) and the percentage change from year to year in the human capital index, both from PWT 10.01. These variables are important for employment growth as they control for the effect of changes in the physical and human capital stock on $\Delta\log y_{i,t+h}$. We also include the output gap to control for the business cycle. It is calculated using the Hamilton (2018) filter to real GDP data from PWT 10.01.

We examine whether counter-reforms of unemployment benefits and employment protection have different effects. Furthermore, in line with the literature on dual labour markets, we disaggregate EPL counter-reforms into those affecting workers with a temporary and those affecting workers with a permanent contract. Here we expect counter-reforms affecting workers with a temporary contract to have stronger effects than those affecting workers with a permanent contract.

LP estimates may be biased if counter-reforms are driven by a particular variable. Structural reforms are more likely to occur after times of economic crisis (Drazen and Grilli, 1993). Similarly, reforms are more likely under certain political circumstances. For example, reforms are typically more likely after a new government takes office (Haggard and Webb, 1993), while more politically fragmented governments may find it more difficult to implement reforms (Alesina and Drazen, 1991). Similarly, counter-reforms could be endogenous in which case LP estimates would be biased. As shown by Jordà and Taylor (2016), the estimation of LP impulse responses relies on the assumption that the mean values of the covariates are the same in the treatment and control groups. For this reason, we apply a so-called balance test, which can be explained as follows. In an ideal RCT setting, where treatments are randomly assigned, we would expect the probability density function for each control variable included in the model to be the same for each sub-population of treated and control units. The overlap of the densities should be close to perfect. A simple way to check that this condition is met is to test for equality of means between the sub-samples. The results of the balance tests are shown in Table 3. The balance tests do not detect counter-reform selection. One covariate is significant at the 5 % level for counter-reforms of unemployment benefits, but this is not sufficient to indicate reform selection bias, as it may be the result of a type I error. Therefore, there is no need to use more complicated treatment selection estimators.

In all our LPs, both linear and non-linear (see below), we use Spatial Correlation Consistent (SCC) standard errors as proposed by Driscoll and Kraay (1998). We test whether there is spatial dependence in the disturbances between the cross-sectional units when using standard errors clustered at the country level as is often done in the LP literature. For this purpose, we use the Pesaran (2015) test, which is standard normally distributed. A value of the test statistic outside the interval [−1.96, 1.96] rejects the null hypothesis of weak cross-sectional dependence in favour of cross-sectional dependence. The test is often significant.[12]

Canova (2024) proposes a test to determine whether the panel (repeated cross-sections) LP estimator exhibits dynamic heterogeneity. The test is based on calculating the coefficient of variation (CV) of the impact of the variable 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 these 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 (theoretically zero). When this is the case, cross-sectional

[8] In practice, this almost balances our panel. We have 39 observations per country when $h = 7$, except for Poland for which we have 38 observations. With 25 countries in our sample, this amounts to 974 observations.

[9] This finding is confirmed using Fisher-type panel stationarity tests which are available on request. According to these tests, the other variables on the right-hand side of the model estimated are also stationary.

[10] We confirm this prediction in a robustness test.

[11] The bias increases with the forecast horizon, see Teulings and Zubanov (2014). The leads of the counter-reform dummies ensure that it is registered in the data when the outcome for a specific observation is affected by a counter-reform ahead in time. This most often is the case for country-year pairs where no counter-reform took place. However, counter-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 counter-reforms.

[12] Results are available on request. The SSC standard errors are also cluster robust in addition to being robust to spatial correlation, see Driscoll and Kraay (1998).

Table 3
Balancing tests for labour market (LM) counter-reforms in $t+1$.

	(1) Gross capital formation	(2) Lagged Gross capital formation	(3) Differenced Human capital index	(4) Lagged Differenced Human capital index	(5) Output gap	(6) Lagged output gap	(7) Employment growth	(8) Lagged Employment growth
Counter-reforms LM	0.648 (0.521)	0.762* (0.414)	0.119 (0.095)	0.108 (0.091)	−0.073 (0.062)	0.006 (0.045)	−0.092 (0.405)	−0.192 (0.333)
Counter-reforms UB	0.954 (0.734)	1.174** (0.520)	0.337 (0.221)	0.308 (0.220)	−0.109 (0.101)	−0.066 (0.072)	−0.904 (0.536)	−0.574 (0.417)
Counter-reforms EPL	0.382 (0.752)	0.410 (0.504)	−0.058 (0.055)	−0.054 (0.057)	−0.042 (0.070)	0.062 (0.060)	0.550 (0.381)	0.116 (0.424)
Counter-reforms EPLp	0.554 (1.021)	0.821* (0.424)	−0.017 (0.116)	−0.035 (0.117)	−0.096 (0.086)	0.039 (0.077)	0.956* (0.528)	0.004 (0.666)
Counter-reforms EPLt	−0.048 (0.523)	−0.233 (0.590)	−0.168 (0.105)	−0.139 (0.120)	0.048 (0.064)	0.110* (0.044)	0.094 (0.435)	0.412 (0.368)
Obs.	974	974	974	974	974	974	974	974

Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. EPLp and EPLt refer to employment protection of workers with a permanent contract and those with a temporary contract, respectively.

methods show dynamic homogeneity. If there is heterogeneity, the estimated distribution will be spread out and the CV will be large. To assess whether the spread 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 (since we have an almost balanced panel) with a 5 % significance level. The critical values are shown in Table A2 in the Appendix.[13]

The effects of counter-reforms may be non-linear if the effect depends on the economic conditions at the time of their introduction. We therefore examine whether the impact of labour market counter-reforms depends on the business cycle as proxied by the output gap.[14] In line with the findings of Duval et al. (2020) that labour market reforms increase employment in expansions, but decrease employment in recessions, we hypothesize that the effects of counter-reforms are different in boom periods and periods with slack.

As discussed in Auerbach and Gorodnichenko (2012), the LP approach to estimating nonlinear effects is equivalent to the smooth transition autoregressive (STAR) model developed by Granger and Teräsvirta (1993). The advantage of this approach is threefold. First, compared with a model in which each dependent variable would be interacted with a measure of the business cycle position, it allows a direct test of whether the effect of counter-reform varies across different regimes such as recessions and expansions. Second, compared to estimating structural vector autoregressions for each regime, it allows the effect of counter-reforms to change smoothly between recessions and expansions by considering a continuum of states in computing the impulse response functions, thus making the response more stable and precise. Finally, the smooth transition approach takes into account the intensity of the deviation between actual and trend GDP, and models the continuous, gradual transitions between economic states rather than assuming abrupt shifts.

More specifically, we estimate:

$$\Delta \log y_{i,t+h} = \alpha_{i,h} + \delta_{t,h} + \sum_{j=0}^{4} \beta_{j,h}^{L} F(z_{i,t}) d_{i,t-j} + \sum_{j=0}^{4} \beta_{j,h}^{H} (1 - F(z_{i,t})) d_{i,t-j} + \sum_{l=0}^{4} \beta_{l,h} \left(\log y_{i,t-l} - \log y_{i,t-1-l} \right) + \sum_{m=1}^{h} \beta_h d_{i,t+h} + \sum_{c=0}^{1} \beta'_{c,h} X_{i,t-c} + u_{i,t+h} \tag{2}$$

where,

$$F(z_{it}) = \frac{\exp(-\gamma z_{it})}{1 + \exp(-\gamma z_{it})}, \ \gamma > 0$$

with z_{it} being the output gap normalised to have zero mean and unit variance. The weights assigned to each regime vary between 0 and 1 according to the weighting function $F(.)$, so that $F(z_{it})$ can be interpreted as the probability of being in a given state of the economy,

[13] Canova (2024) shows that when the dynamic evolution of individual cross-sections is not homogeneous, LP panel estimates can be biased.

[14] The output gap, which measures the deviation of actual GDP from trend GDP, is distinct from the concept of economic growth. The output gap does not directly capture the rate of economic growth but instead reflects whether an economy is operating above or below its potential output. As such, two countries with vastly different growth rates could exhibit similar output gaps if their respective GDP levels are similarly deviated from their long-term trends.

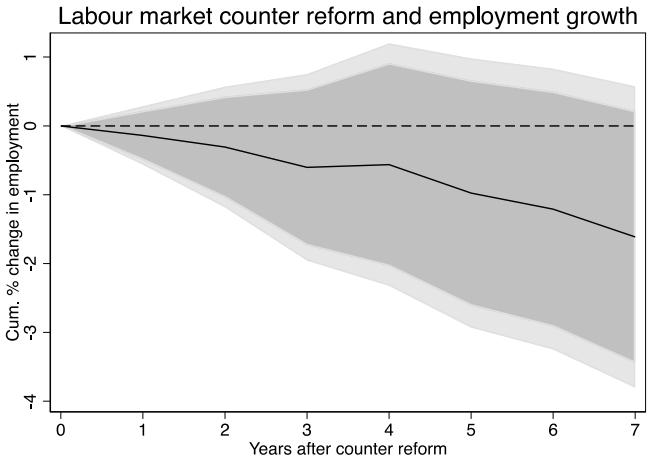

Fig. 1. Impulse responses based on local projections of labour market counter-reforms on employment growth. *Notes*: The solid black lines in the figure plot the impulse responses of labour market counter-reforms on employment growth. Year=1 is the first year after a counter-reform took place at year=0. So, the position of the line at e.g., year=7 shows the change in employment growth 7 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. The underlying regressions are shown in Table A4 in the Appendix.

boom or bust, i.e., when the economy is running above or below its long-run trend. The coefficients $\beta^{L}_{j=0,h}$ and $\beta^{H}_{j=0,h}$ are used to construct the IRFs and the associated confidence interval for counter-reforms introduced during a boom or bust period. They respectively capture the impact of counter-reforms at each horizon h in the case of slack ($F(z_{it})\approx 1$ when z goes to minus infinity) and booms ($1\text{-}F(z_{it})\approx 1$ when z goes to plus infinity), respectively. We choose $\gamma = 2.5$, so that the economy spends about 20 percent of the time in a recessionary regime, defined as $F(z_{it})>0.8$ (as Auerbach and Gorodnichenko, 2012), which is close to the typical business cycle pattern of many advanced economies (see Figure A3 in the Appendix).

State-dependent LPs (whether smooth transition or not) have been used extensively (e.g., Alpanda et al., 2021; Haan and Wiese, 2022; Ortmans and Tripier, 2021; and Ramey and Zubairy, 2018). Plagborg-Møller and Wolf (2020) show that in a linear framework, LPs and VAR models estimate the same IRFs. In our state-dependent context, the local projection method offers two important advantages over VARs. First, LPs provide a simple way to account for state dependence, especially in a panel framework. Second, unlike regime-switching VARs, they do not require one to take a position on the duration of a given state or on the mechanism that triggers the transition between states.

One important caveat is that the state should be uncorrelated with the macroeconomic shock. As Gonçalves et al. (2024) show, when the state is exogenous, the LP estimates recover the population response regardless of the size of the shock. However, when the state depends on the macroeconomic shock, the LPs only recover the conditional response to a small shock, but not the response to larger shocks. Table A3 in the Appendix presents the unconditional correlation coefficients between counter-reforms and the smooth-transition function $F(z_{it})$, which is function of the output gap z_{it}, as used in eq. (2) for our estimation sample. As Table A3 shows, the correlation is very low and statistically insignificant. So, we conclude that our results when estimating eq. (2) are unlikely to be affected by a bias due to a high correlation between the shocks and the state.

4. Empirical results

4.1. The basic linear LP approach

Fig. 1 presents the IRF of the LP estimates for the effects of labour market counter-reforms on employment growth rates. The graphs show the effects of all labour market counter-reforms (detailed estimation results can be found in Table A4 in the Appendix). The results suggest that labour market counter-reforms have a negative but statistically insignificant effect on employment growth. However, as shown in the last row in Table A4, the Canova test often indicates dynamic heterogeneity in the cross-sectional effect on employment growth. This may reflect the fact that countries introduced different types of labour market counter-reforms. As the next step, we therefore distinguish between counter-reforms in unemployment benefits and counter-reforms in employment protection legislation, where for the latter we distinguish between protection of workers with temporary or fixed contracts. Fig. 2 shows the IRFs for UB and EPL counter-reforms on employment growth (Tables A5 to A8 in the Appendix present the model estimates).

Again, we find no clear evidence (statistically speaking) that counter-reforms have an impact on employment growth (Fig. 2). However, the effects of EPL counter-reforms seem to differ between workers with a permanent contract and those with a temporary contract. In particular, the latter type of counter-reform has a positive and statistically significant effect on employment growth from the time of the reform reversal until 4 years after the counter-reform. This may be because the increased difficulty of hiring and firing workers with a permanent contract may lead firms to rely more on temporary workers, who can be hired and fired flexibly under the

conditions of such counter-reforms. Note that the Canova test fails to reject the null of dynamic homogeneity across countries when we split EPL counter-reforms into counter-reforms for temporary and permanent contracts.

As a next step, we examine whether the business cycle conditions affect the impact of labour market counter-reforms and whether taking the business cycle into account reduces the problem of dynamic heterogeneity across countries.

4.2. Smooth transition LP approach

Fig. 3 shows the impulse responses when we consider the cyclical when analysing the impact of labour market counter-reforms on employment growth, using the smooth transition approach. The results are similar compared to those based on the simple LP approach which ignores the prevailing business conditions at the time of the counter-reform. However, the Canova test now often (but not always) indicates dynamic homogeneity.

Our results suggest that the effects of labour market counter-reforms on the growth of labour market counter-reforms are mostly insignificant in boom periods and periods with slack. This is also the case when we distinguish between EPL and UB counter-reforms. However, when we distinguish between EPL counter-reforms for workers with permanent and temporary contracts, we find that the latter have a significant positive effect on employment growth when the economy is performing above trend. Perhaps workers who become unemployed are more willing to take a temporary contract if they are offered more protection. Or the increased protection may attract people who were inactive into the labour market, in the face of a tight labour market during the boom.

5. Robustness analysis

As a first robustness test, we re-estimated the results conditional on the business cycle using the Hodrik-Prescott filter with high smoothing parameter (λ=100) instead of the Hamilton (2018) filter to generate the output gap. In general, the smooth transition results are similar in terms of sign, but the error bands are narrower indicating more statistically significant impulse responses. These results are available upon request.

Next, we use the bias-corrected LP estimator derived in Herbst and Johanssen (2024). Herbst and Johannsen (2024) show that LPs can be biased. The bias is similar to the Nickell bias, but more severe when present in an LP model because the local projection estimates at each forecast horizon can no longer 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 greater than 0.9. We have low persistence in most of our models as the sum of the coefficients of the lagged dependent variables is <0.5 in all specifications when h = 1 (when h > 1 the persistence adds up because we estimate cumulative growth rates, so the sum of the coefficients of the lagged dependent variables are not directly comparable

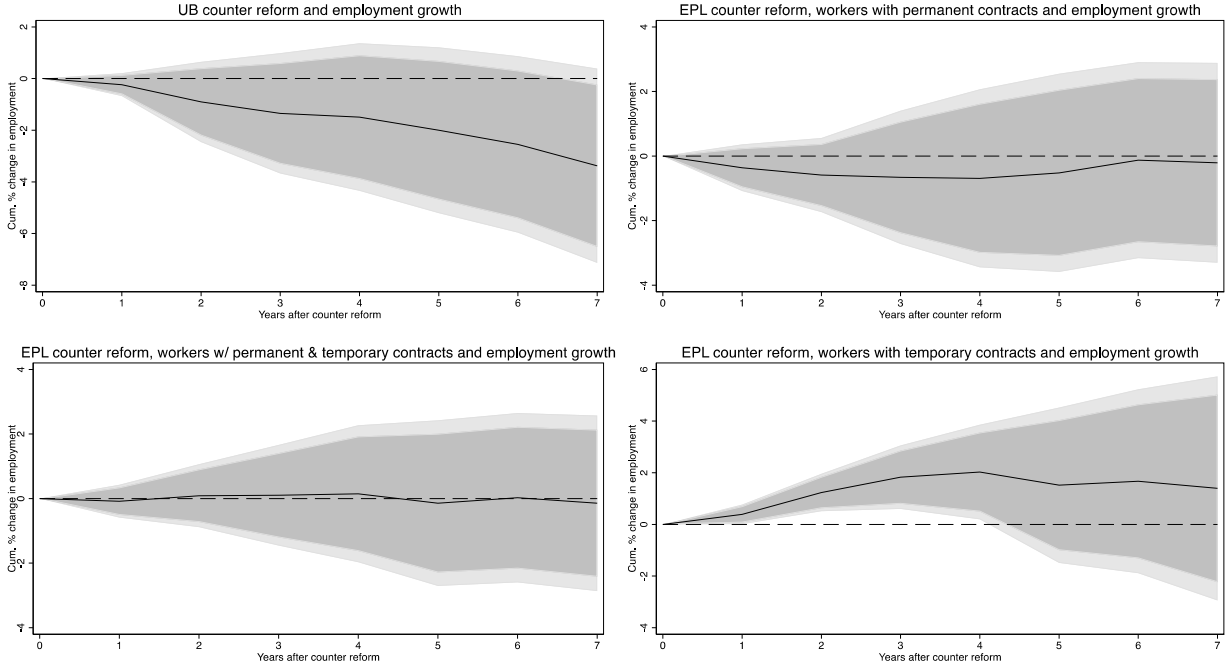

Fig. 2. Impulse responses based on local projections of unemployment benefits and employment protection legislation counter-reforms on employment growth. *Notes*: The solid black lines in the figure plot the impulse responses of labour market counter-reforms on employment growth. Year=1 is the first year after a counter-reform took place at year=0. So, the position of the line at e.g., year=7 shows the change in employment growth as share of the population 7 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. The underlying regressions are shown in Tables A5 to A8 in the Appendix.

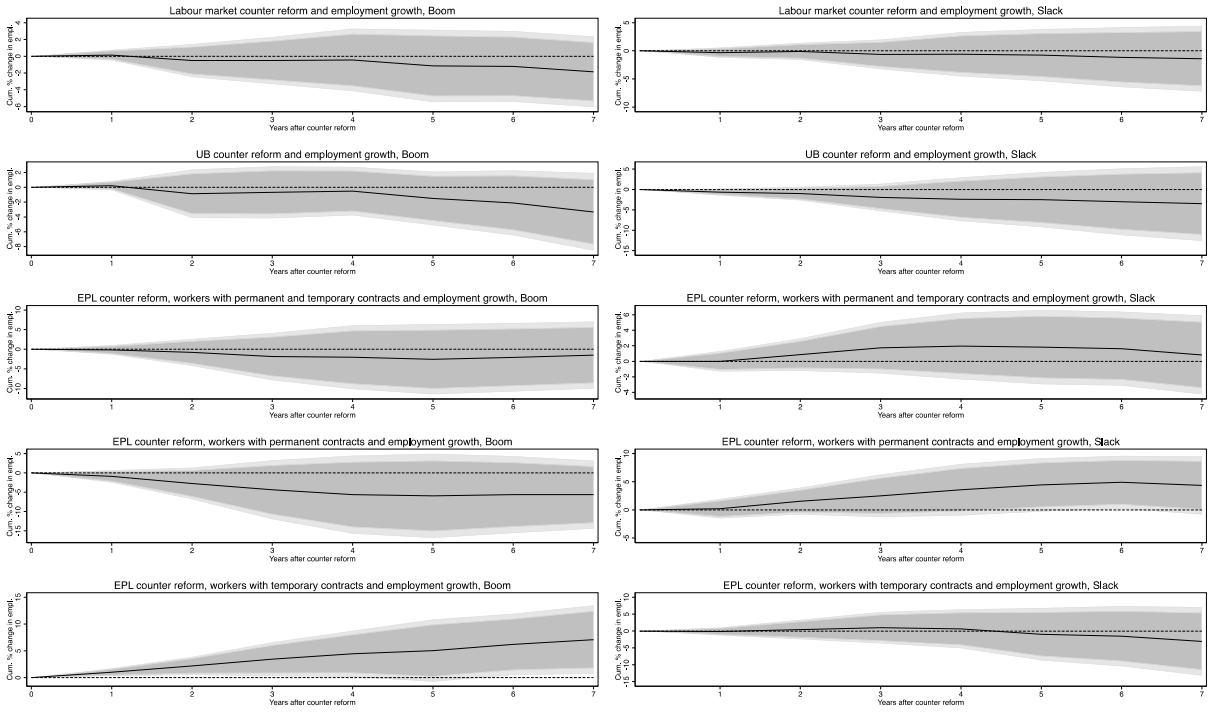

Fig. 3. Impulse responses based on smooth transition approach of labour market counter-reforms on employment growth conditional on the business cycle. *Notes*: The solid black lines in the figure plot the impulse responses of labour market counter-reforms on employment growth. The dark grey shaded areas display the 90 % SCC error bands; the light grey shaded areas display the 95 % SCC error bands. The panels in the left part show projections for country-years when the economy is running above the trend, while the panels in the right part show country-years when the economy is running below the trend based on the output gap; the trend is based on the Hamilton (2018) filter. There are 972 observations in each regression as we lose two more observations for Poland. The underlying regressions are shown in Tables A9 to A13 in the Appendix.

when $h > 1$). Nevertheless, we implement the bias correction proposed by Herbst and Johannsen (2024), since the bias also increases with the forecast horizon, h, and the shorter time series dimension *t*. The results of this estimator are very similar to our previous estimates for eq. (1), as shown in Figure A3 in the Appendix. We therefore conclude that our models are robust to the bias. We also apply the bias correction to our non-linear LP estimates and again find that the bias is negligible.[15]

Finally, we drop each country in turn from the models used in the previous section. Table A14 in the Appendix reports the results. Our findings are robust to the exclusion of any specific country although the results generally become somewhat stronger in terms of effect size and significance when either Portugal or Spain is excluded. Our results are also robust when we decompose labour market counter-reforms into EPL counter-reforms (also separately for workers with permanent and temporary contracts) and UB counter-reforms, and also when we use the smooth transition approach to estimate the effects conditional on the position of the business cycle at the time the counter-reforms are introduced. These results are available upon request.

6. Conclusion

This paper empirically examines the effects of labour market counter-reforms on employment growth in 25 OECD countries between 1973 and 2012, using a narrative-based dataset of reforms. By disaggregating the counter-reforms into those affecting employment protection legislation (EPL) for workers with a permanent and a temporary contract, and unemployment benefits (UB), our analysis reveals nuanced effects that vary significantly across worker types and economic conditions. The local projections (LP) approach is used to estimate the dynamic effects of counter-reforms, allowing for a flexible examination of responses over time without imposing the restrictive dynamics typical of vector autoregression models. This method provides clarity on the temporal nature of the effects of counter-reforms, highlighting how these effects evolve and persist (in our case) up to seven years after implementation.

Our results suggest that the effect of counter-reforms is influenced by the prevailing economic conditions and varies between different types of reforms. Labour market counter-reforms can be divided into two types: employment protection legislation (EPL) and unemployment benefits (UB) counter-reforms. When we distinguish between EPL counter-reforms for workers with a permanent and those with a temporary contract we find that the latter have a significant positive effect on employment growth when the economy is performing above trend.

[15] These results are avaible on request.

These findings have implications for policy makers. First, our results do not suggest that most counter-reforms have a positive effect on employment growth so that they may not be very attractive from a political economy perspective. However, since most of the effects we find are not negative either, politicians may be tempted to introduce counter-reforms if their constituencies would demand it. Second, the different effects on workers with temporary and permanent contracts underline the need for a balanced approach to EPL policies in order to avoid unintended consequences such as substitution effects between different types of employment contracts. Finally, similar to labour market reforms, the effects of (some) counter-reforms depend on the prevailing economic conditions, suggesting that the timing of counter-reforms is important.

While our paper provides extensive evidence, it is not without limitations. The generalisability of the results outside the OECD context remains uncertain, and the effects may differ in economies with different labour market structures. In addition, our paper primarily captures the short- to medium-term effects of counter-reforms; longer-term dynamics remain less understood. Future research could focus on extending the geographical and temporal scope of the analysis to include emerging market economies. Another promising area is to delve deeper into the sector-specific effects of such counter-reforms, which could provide more granular insights useful for sectoral policy interventions.

CRediT authorship contribution statement

Rasmus Wiese: Writing – review & editing, Writing – original draft, Software, Methodology, Formal analysis, Conceptualization. **João Tovar Jalles:** Writing – review & editing, Writing – original draft, Conceptualization. **Jakob de Haan:** Writing – review & editing, Writing – original draft, Conceptualization.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.eap.2025.03.010.

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