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The macroeconomic impact of euro area labour market reforms: evidence from a narrative panel VAR

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Abstract

Using new quarterly narrative evidence, this paper examines the macroeconomic impact of reforms of unemployment benefits (UB) and employment protection legislation (EPL) in the euro area from a Bayesian narrative panel VAR. The approach complements existing micro-econometric evidence by aligning short- and medium-term effects in a unified framework and assessing state dependencies. Liberalising reforms result in temporary wage declines and highly persistent increases in economic activity and employment. In contrast to UB reforms, the effects of EPL reforms on employment emerge only gradually.

JEL Classification: E32, J08, O43

Keywords: Employment Protection Legislation, Unemployment Benefits, Narrative Identification, Discriminant Regression

Non-technical Summary

Labour market reforms have been a central part of economic policies in the euro area during episodes of stagnation in individual economies. Important examples are the reforms to employment protection legislation (EPL) implemented after the 2008 Financial Crisis and the German Hartz reforms about a decade earlier. However, while the effects of unemployment benefit reforms are largely undisputed, the empirical literature on EPL is rather inconclusive. Studies based on micro data typically find little effect of EPL reforms on employment, but there is mild evidence for a medium-term effect. Studies based on macroeconomic data find hardly any evidence.

This paper uses a narrative panel VAR to estimate the macro-economic effects of reforms in the euro area in between 1998 Q1 and 2018 Q4. It employs is a new database that provides quarterly information on the legal adoption and implementation dates of 35 reforms events. Reforms are related to unemployment benefit schemes, employment protection legislation of regular labour contracts, and legislation targeting temporary contracts.

The narrative VAR finds that unemployment benefit reforms lead to a relatively quick increase in employment and a moderate decline in the real wage. In the medium term, the effect on employment remains, while real compensation reverts back to baseline. The responses to reforms of regular contract EPL are similar, but the response of employment builds up gradually and reaches its full scale only after about six years. In the short term, the effects of EPL reforms depend on the state of the business cycle: in states of low growth the response of real activity and employment is more delayed. Some of the reforms had sizeable medium-term effects. In particular, the German Hartz reforms and EPL reforms in Portugal after 2007 altogether raised GDP and employment by above 2% in these countries. Reforms in the Netherlands, Italy, and Spain had smaller but still significant effects. By contrast, the effects of temporary contract EPL reforms were small.

1 Introduction

Labour market reforms have been a central part of economic policies in individual euro area countries during episodes of stagnation. After 2008, a number of reforms were implemented in the economies most heavily affected by the Financial Crisis. About a decade earlier, the Hartz reforms had aimed at the recovery of the German labour market. The weight put on these reforms in policy debates is yet not fully matched by the academic literature. The impact of unemployment benefit (UB) reforms is undisputed, but the findings on employment protection legislation (EPL) remain inconclusive. In their review of the empirical literature, Boeri, Cahuc, and Zylberberg (2015) infer that the short-run effects of relaxing EPL on employment are ambiguous. While there is some micro-econometric evidence for a positive longer-term effect, studies inspecting the long-run linkages between labour market institutions and unemployment rates from cross-country panel regressions do not find significant relations. From annual narrative indicators Duval and Furceri (2018) find short-term effects on employment only for periods of high growth.

This paper explores a narrative panel VAR approach to estimating the macroeconomic effects of labour market reforms in the euro area in between 1998 Q1 and 2018 Q4. At the core of the identification strategy is a database developed by Aumond, Di Tommaso, and Rünstler (2021) that provides quarterly information on the legal adoption and implementation dates of reforms to unemployment benefit schemes, employment protection legislation of regular labour contracts, and legislation targeting temporary contracts. The data used in this study cover 35 reforms in 9 economies.

The narrative VAR identifies the effects of reforms from their impact on the VAR forecast errors around the dates of individual reform events. The identification scheme is close to the proxy VAR approach used, for instance, by Mertens and Ravn (2012) and Mertens and Montiel-Olea (2018) for studying the effects of fiscal policies. However, reflecting the

fact that labour market reforms are very difficult to quantify, identification is solely based on the timing of reform events and neither requires the scale of events nor the precise timing of the resulting shocks to be known. Instead, the model provides estimates of the latter. Hence, the identification scheme essentially amounts to defining a treatment effect from the narrative information (Stock and Watson, 2018). While it is important to control for the dependency of reform events on the state of the economy, the inclusion of a large information set in the VAR and the use of quarterly information on the timing of reforms facilitate this task.

I find that both unemployment benefit reductions and deregulations of regular contract EPL lead to persistent increases in economic activity and employment. The response is relatively fast for UB reforms. However, it remains initially small and builds up only gradually for EPL reforms, which may explain the inconclusive findings of micro-econometric studies. For both reform categories, declines in unemployment and real compensation prove largely temporary as the labour force catches up with employment in the medium term. Some of the reforms had a sizeable impact. In particular, the German Hartz reforms and EPL reforms in Portugal after 2007 altogether raised GDP and employment by above 2% in these countries. Several reforms in the Netherlands, Italy, and Spain had smaller but still significant effects. By contrast, the impact of reforms to temporary contract EPL reforms remained generally small and, with few exceptions, insignificant.

One potential factor in the delayed response of employment to EPL reforms is state dependencies. It has been argued that both temporary and regular contract EPL reforms have more delayed effects during recessions (Boeri and Garibaldi, 2007; Cacciatore et al., 2016). I study state dependence by re-estimating the model separately for subsets of events implemented in states of low and high euro area GDP growth. I find longer delays in the response of GDP and employment to EPL reforms during periods of low growth, but the differences vanish at longer horizons leaving the medium-term impact unaffected.

Section 2 of the paper presents the narrative indicators and reviews the empirical literature. Section 3 discusses the identification scheme and its panel VAR implementation. Section 4 presents the results. Section 5 concludes.

2 Labour Market Reforms

I identify the effects of labour market reforms from a set of dummy variables that indicate the timing and direction of reforms, taking values of $+1$ in case of a deregulation, -1 in case of a regulatory tightening, and zero otherwise. Aumond et al. (2021) provide a list of major reforms implemented in the 11 initial euro area member states (excl. Luxembourg) over the period of 1998 Q1 to 2018 Q4, which includes quarterly legal adoption and implementation dates. The database builds on Duval et al. (2018). In a first step, the set of reforms is identified from OECD country reports. For a reform to qualify as significant the report must either use strong normative language or the reform must be mentioned repeatedly across different editions of the reports. In a second step, legal adoption and implementation dates are derived from the ILO Labour Law, the European Commission LABREF databases, and national legal sources. Legal adoption refers to the date of approval of the law by national parliaments, while implementation refers to the date at which the law came into force.

The data used in this study cover three different categories of reforms. 13 reforms relate to employment protection legislation (EPL) on regular contracts addressing conditions and procedures for individual or collective dismissals, such as severance payments, terms of notice, and rules for fair dismissal. 16 reforms relate to EPL on temporary contracts, which outline the conditions under which workers can be hired under this type of contracts. Regulation usually concerns the type of jobs and activities in which these contracts are allowed, their maximum duration, and conditions for their renewal or their termination.

Finally, six reforms of unemployment benefit schemes (UB) affect either replacement rates or benefit duration or both. For the majority of EPL reforms, legislation came into force either immediately in the same quarter when the law was legally adopted or in the quarter thereafter. By contrast, for unemployment benefit reforms three out of six events have implementation lags of beyond four quarters.

Table 1: Number of Reform Events

	1998-2018		1998	2005	2010	2015	Implementation Lag		
	All	+	2004	2009	2014	2018	0	1	> 1
EPL Regular	13	11	3	2	6	2	4	6	3
EPL Temporary	16	9	10	1	2	3	5	10	1
UE Benefits	6	5	0	2	2	2	0	3	3

The table shows the number of events together with the number of regulatory easings (+). The implementation lag denotes the difference between legal adoption and implementation.

The list of reforms is shown in Annex C. The data exclude various reforms from the original data set that took place in periods subject to large macroeconomic shocks for reasons other than the reforms. Most importantly, I remove three EPL reforms in Greece in between 2010 and 2012 and an UB reform in Portugal in 2012, as these events interfere with cuts in either public sector or minimum wages in the same or subsequent quarter. Otherwise, labour market reforms did not coincide with major fiscal policy measures, as they typically involve consultations with social partners and are therefore adopted with longer delays. In particular, although several countries imposed freezes on public sector and minimum wages in between 2010 and 2014, cuts took place already in 2010 before EPL reforms. Several deregulations of regular contract EPL after 2010 were accompanied by minor elements of tightening in temporary contract regulations. In this case, I remove the entry in the latter index.

Related Literature

Studies on labour market reforms can be classified by whether they use micro- or macro-econometric approaches and whether they study short-term or long-run effects.

First, there is a vast micro-econometric literature that studies individual reforms by exploiting differential treatments of employment groups, e.g. related to firm size or sectoral separations. In their review, Boeri et al. (2015) conclude that UB reforms have marked and fast effects on unemployment duration, while the findings on the short-run effects of EPL reforms are ambiguous. Both hiring and lay-off rates increase after a deregulation, but studies tend to find a negative net effect on employment. There is some evidence for positive effects of regular contract EPL deregulation beyond the short run. Autor et al. (2006) exploit the different timing in the implementation of common EPL across U.S. states. They find employment by about 1 % lower 5 years after a tightening. Micco and and Pagés (2006) find EPL tightness to be negatively related to the employment rate in industries that are exposed to volatile shocks.

Only a few studies examine the effects of reforms on wages, with mixed results. Tatsiramos and van Ours (2012) conclude that there are on average no effects of changes in UB schemes on the wage paid in the post-unemployment job. As to EPL reforms, van der Wiel (2010) reports an increase in wages after a tightening in the terms of notice for elderly workers in the Netherlands. Exploiting differences in the treatment of small and large firms, Martins (2009) finds that EPL deregulation lowered wages in Portugal, whereas Leonardi and Pica (2013) find the opposite for Italy. The literature offers two opposing explanations: workers may accept wage reductions as a compensation for job security, whereas efficiency wage considerations predict that tighter EPL raises insider power and thereby drives up wages. Regarding the latter effect, Ordine et al. (2017) report that a deregulation of temporary contracts raised the wages of workers on permanent contracts.

Second, macro-econometric studies examine the long-run linkages between labour market institutions and unemployment from cross-country panel regressions (Lazear, 1990; Nickell et al., 2005; di Tella and McCulloch, 2005). These studies find a role for UB reforms, but little effect for EPL. Endogeneity bias and measurement error are of general concern, as the regulatory stance and labour market conditions may be co-determined by common factors, while the regulatory stance is difficult to quantify (de Haan and Parlevliet, 2018).

Third, most closely related to this paper is a small number of studies that inspect the impact of reforms on aggregate employment from local projection methods based on annual narrative reform indicators. Bouis et al. (2012) focus on UB reforms measured by shifts in the OECD regulatory stance indicators, while Duval and Furceri (2018) use a narrative dataset that also includes EPL reforms. Both studies find effects of unemployment benefit reforms. The latter study also finds effects of EPL reforms for the case that the economy is in a state of high growth.

3 A Bayesian Narrative Panel VAR

The narrative VAR identifies the effects of reforms from their immediate impact on the VAR forecast errors around the dates of individual reform events. The identification scheme is close to the proxy VAR approach as used e.g. for estimating the effects of fiscal policy shocks (Mertens and Ravn, 2012; Mertens and Montiel-Olea, 2018). The limited nature of the information on labour market reforms requires yet some modification of the standard method. First, the statistical framework underlying the identification scheme must be adapted to the case of binary indicators. I use a discriminant regression proposed by Budnik and Rünstler (2020) for this purpose.[1] Second, the low number of reform events

[1]The utilization of binary narrative information for identifying structural shocks in VARs has been proposed by Antolin-Diaz and Rubio-Ramirez (2016) and Ludvigson et al. (2016). These studies use restrictions on the sign of innovations in specific periods for a small number of events.

in individual countries requires a panel VAR approach in estimation. I therefore estimate the immediate impact of reforms jointly for all countries, while allowing for cross-country heterogeneity in VAR dynamics from a Bayesian panel VAR with partial shrinkage as proposed by Canova (2005).

Section 3.1 discusses identification. Section 3.2 describes the estimation of the model.

3.1 Identification with Sparse Binary Instruments

Consider the reduced-form VAR for $n \times 1$ vector y_t over periods $t = 1, \ldots, T$,

$$y_t = \mu + \sum_{s=1}^{p} B_s y_{t-s} + F x_t + u_t, \qquad u_t \sim N(0, \Sigma). \tag{1}$$

where μ is a constant term and x_t is a vector of control variables of dimension $k \times 1$. The VAR forecast errrors u_t are assumed to be normally distributed with covariance Σ.

The key assumption of the model is that the $n \times 1$ vector of the VAR forecast errors has a structural representation that isolates the labour market reform innovation θ_t as the first element of a vector of innovations ϵ_t such that $\mathbb{E}\epsilon_t \epsilon_t^T = I_n$,

$$A_0 u_t = \epsilon_t = \begin{pmatrix} \theta_t \\ \epsilon_t^+ \end{pmatrix}. \tag{2}$$

Denote with α^T the first row of matrix A_0, implying $\alpha^T u_t = \theta_t$. The narrative VAR aims at identifying α from information on the sign of reform innovations for specific events. Such type of information defines an instrument z_t that takes values of $z_t = \text{sign}(\theta_t)$ for a limited number of $m < T$ periods and $z_t = 0$ otherwise and provides the conditions

$$\begin{aligned} \mathbb{E}(\theta_t | z_t \neq 0) &= \gamma z_t \\ \mathbb{E}(\theta_t | z_t = 0) &= 0 \\ \mathbb{E}(\epsilon_t^+ | z_t) &= 0, \end{aligned} \tag{3}$$

where $\gamma = \mathbb{E}(\theta_t z_t | z_t \neq 0)$ is the expected absolute value of the reform innovation. Conditions (3) show that z_t formally acts like a treatment effect, identifying α from the mean shift in the resulting innovations θ_t for non-zero z_t. Consequently, vector α may be estimated by maximising the expected value of sign-adjusted innovations $\theta_t z_t$ for $z_t \neq 0$. Once α is known, the dynamic response of the system to reform innovations can be derived.

Stock and Watson (2018) discuss the requirements for the identifiability of vector α in the general context of proxy VARs, of which the above model is a special case. First, the relevance condition $\gamma > 0$ in equations (3) ensures that instrument z_t picks up events that generate relevant policy innovations. Second, the exogeneity condition $\mathbb{E}(\epsilon_t^+ | z_t) = 0$ requires that events included in z_t are independent of other contemporaneous shocks in the system.

Third, an invertibility condition requires that the VAR forecast errors fully span innovations θ_t such that the entire impact of events is represented by $\alpha^T u_t$. This condition is embodied in equation (2). As discussed by Stock and Watson (2018), any effect of the event missed by forecast errors in period t would necessarily materialise in forecast errors in subsequent periods. With a sparse binary instrument, potential non-invertibility issues can therefore be explored by inspecting the impact of events on the VAR forecast errors in subsequent periods (see Annex B). One reason for non-invertibility is that relevant variables are missing in the VAR. Hence, the latter should include a sufficiently large information set (Forni and Gambetti, 2008).

3.2 Estimation

The low number of reform events shown in Table 1 requires a panel approach to estimate the model jointly across countries. I therefore consider the set of VAR models

$$
\begin{aligned}
y_{c,t} &= \mu_c + \sum_{s=1}^{p} B_{c,s} y_{c,t-s} + F_c x_t + u_{c,t}, \qquad & u_{c,t} &\sim N(0, \Sigma_c) \\
\alpha_c u_{c,t} &= \theta_{c,t}, & \theta_{c,t} &\sim N(0,1)
\end{aligned}
\tag{4}
$$

for countries $c = 1, \ldots, C$. Estimation proceeds in two steps. First, I obtain draws of the reduced-form VAR coefficients for the individual countries under partial shrinkage. Conditional on the resulting VAR forecast errors $u_{c,t}$, I then obtain draws of coefficients α_c based on a discriminant (DC) regression, which relates the forecast errors to instruments $z_{c,t}$. The DC regression is estimated jointly for all countries. Coefficients α_c are hence set to be identical across countries apart from a final re-scaling step.

Panel VAR with Partial Shrinkage Prior

There are strong benefits from using a partial shrinkage approach as proposed by Canova (2005). Partial shrinkage allows for heterogeneity in coefficients across countries and thereby provides improved forecast errors compared to a standard panel VAR. At the same, time it exploits the similar macroeconomic dynamics across countries and permits the inclusion of a larger information set compared to VARs estimated separately for the individual countries. I use a model version of Jarocinski (2010), which estimates the optimal degree of shrinkage.

Let $B_{c,+} = (B_{c,1}^T, \ldots, B_{c,p}^T)$ and denote with $\beta_c = \text{vec}\, B_{c,+}$ the vector of coefficients related to the endogenous variables in VAR (4) for country c. The model assumes that coefficients β_c for individual countries are normally distributed with common mean β and a certain covariance matrix $\lambda\Omega$,

$$
p(\beta_c | \beta, \lambda, F) = N(\beta, \lambda\Omega). \tag{5}
$$

Matrix $\lambda\Omega$ determines the degree of shrinkage across countries in the posterior distribution of the VAR coefficients β_c. Parameter λ controls the overall amount of shrinkage governing

the trade-off between bias and efficiency gains. The prior for λ is specified as an inverse gamma ($IG(v,s)$) distribution, $p(\lambda) \propto \lambda^{-(v+1)} \exp(-\frac{s}{\lambda})$. I set $v+1 = s = 0$ resulting in an uninformative prior. Ω is a fixed diagonal matrix defining parameter-specific tightness.[2]

The model is completed by specifying diffuse priors for mean coefficients β, coefficients μ_c and F_c in equation (4), and diffuse Jeffrey priors $p(\Sigma_c) \propto |\Sigma_c|^{-\frac{1}{2}(n+1)}$ for error covariances. Draws from the posterior distributions of the VAR coefficients β_c for country c and the corresponding forecast errors $u_{c,t}$ are obtained from the Gibbs sampler of Jarocinski (2010). The coefficients emerge as weighted averages of those estimated for country c in isolation and of the common mean β, with weights depending on the degree of shrinkage.

Discriminant Regression

Given draws of the VAR forecast errors $u_{c,t}$, I proceed with obtaining draws of α_c from joint estimation of a discriminant (DC) regression proposed by Budnik and Rünstler (2020). The task is to find linear combinations $a^T u_{c,t}$ that identify the treatment effect represented by $z_{c,t}$ in equations (3). Since the cases of $z_{c,t} = +1$ and $z_{c,t} = -1$ are symmetric, equation (3) can be transformed into a purely binary classification problem by considering $z^*_{c,t} = \delta_{c,t} z_{c,t}$ and corresponding residuals $u^*_{c,t} = \delta_{c,t} u_{c,t}$, where $\delta_{c,t} = -1$ if $z_{c,t} = -1$ and $\delta_{c,t} = 1$ otherwise.

Estimation then amounts to maximizing a function of the difference in the conditional means $\mathbb{E}(a^T u^*_{c,t} | z^*_{c,t} = 1) - \mathbb{E}(a^T u^*_{c,t} | z^*_{c,t} = 0)$. One way to estimate a is linear discriminant analysis, a basic version of which can be implemented from the regression

$$z^*_{c,t} = a_0 + a^T u^*_{c,t} + \zeta_{c,t}, \tag{6}$$

as described by Maddala (2013: 18ff). With jointly normally distributed forecast errors, the ordinary least squares estimate of the DC regression is efficient and gives rise to

[2] I set the diagonal element $\Omega_{c,kk}$ related to coefficient $B_{cp,ij}$ as $\Omega_{kk} = \widehat{\sigma}_i^2 / \widehat{\sigma}_j^2$, where $\widehat{\sigma}_i^2$ is the median across countries of sample residual variances from univariate 4^{th}-order autoregressions of series $y_{c,ti}$.

standard inference. These properties emerge from the fact that the regression compares two distinct groups of observations defined by $z^*_{c,t}$. They allow for direct sampling from the posterior of the model parameters based on a Normal-Gamma conjugate prior. A re-scaling step gives $\alpha_c = (a^T \Sigma_c a)^{-1} a$ to achieve unit variance of innovations $\theta_{c,t}$.

In case of heteroscedasticity in residuals across countries, the efficiency of the DC regression can be improved by using standardised residuals. This is achieved by setting $u^*_{c,t} = \delta_{c,t} S_c^{-1} u_{c,t}$, where S_c is a diagonal matrix containing the standard deviations of $u_{c,ti}$ on the main diagonal, together with a corresponding re-scaling step. The posterior distributions of a and σ_ζ and the mapping of vector a into α_c are described in Annex A.

4 Results

The sample covers the initial member states of the euro area excluding Luxembourg over the period of 1998 Q4 to 2018 Q4. The VAR contains seven endogenous variables $y_{c,t}$, i.e. the log-levels of real GDP, the harmonized index of consumer prices, total employment, the labour force, nominal compensation per employee, and real house prices together with the spread of the 10-year government bond yield vis-a-vis Germany. The latter two series are added to control for the specific dynamics of euro area economies during the financial and sovereign debt crises. Predetermined control variables $x_{c,t}$ include the euro area 3-month shadow rate due to Krippner (2013) and the log-differences of US GDP and world commodity prices, together with several dummies to account for outliers in the series. Preliminary estimates indicate that the shadow rate does not respond to reform shocks.[3]

The number of lags of endogenous variables is set to $p = 6$, while the predetermined

[3]The majority of data is taken from the Eurostat National Accounts and its Labour Force Survey. Government bond yields stem from the ECB Interest Rates Statistics. Euro area shadow rate estimates of Krippner (2013) are available at https://www.rbnz.govt.nz/. US real GDP and world commodity price indices stem from the IMF IFS and OECD MEI databases, respectively.

controls enter at lags 1 and 2. I use standardised residuals in the DC regression and exclude the yield spread and house prices from the latter, since the two series are not intrinsically related to labour market developments.

4.1 Average Impact of Reforms

The narrative VAR requires the precise timing of innovations related to reform events to be defined. The choice of the relevant periods is not immediately obvious and includes the possibility that events are represented by innovations in more than one period. With a sparse binary indicator the timing of reform innovations can be explored by setting the instrument to non-zero values in different periods around the dates of events and inspecting the resulting innovations. Respective estimates are shown in Annex B. They suggest that major innovations occur both at the implementation date and the subsequent quarter. The lagged innovations most likely emerge from administrative practices and regulations on hiring and lay-off procedures that delay the adjustment. As a result, a reform implemented towards the end of a quarter would predominantly materialise in an innovation in the subsequent quarter.

For the main estimates, I therefore set instruments z_t to non-zero values at the implementation date and the subsequent quarter. For temporary contract EPL I use only the subsequent quarter, as suggested by the preliminary estimates. The robustness of the results to these choices will be discussed in section 4.4.

Figures 1 to 3 show the dynamic effects of the reforms, as measured by their average impact. For each draw, I obtain the impact of an event by re-scaling the corresponding country IRF with the sum of reform innovations θ_t in the two periods. I then average the re-scaled IRFs across events, adjusted for the direction of reforms.

Figure 1: Average Impact of Regular Contract EPL Reforms

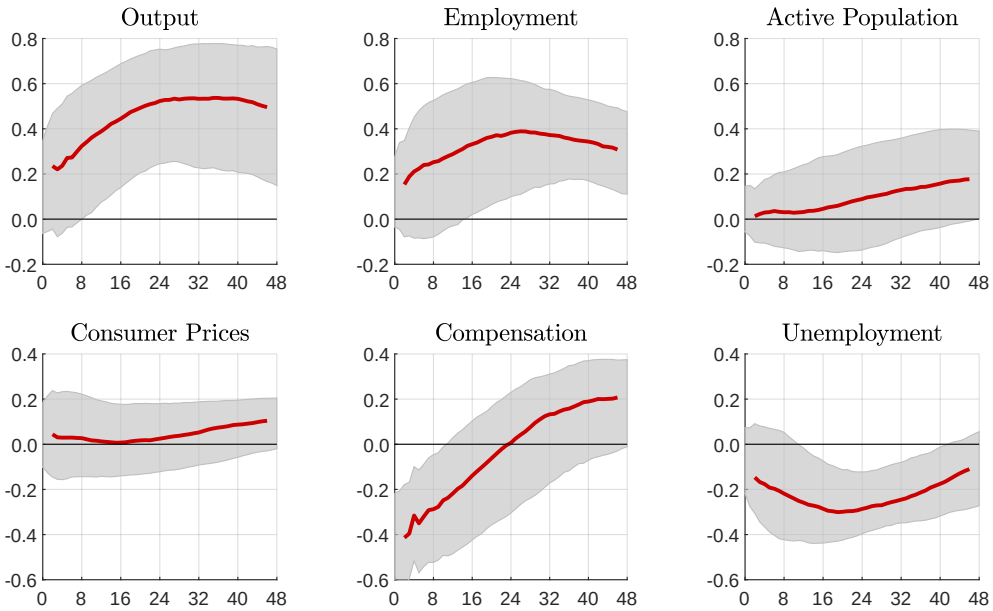
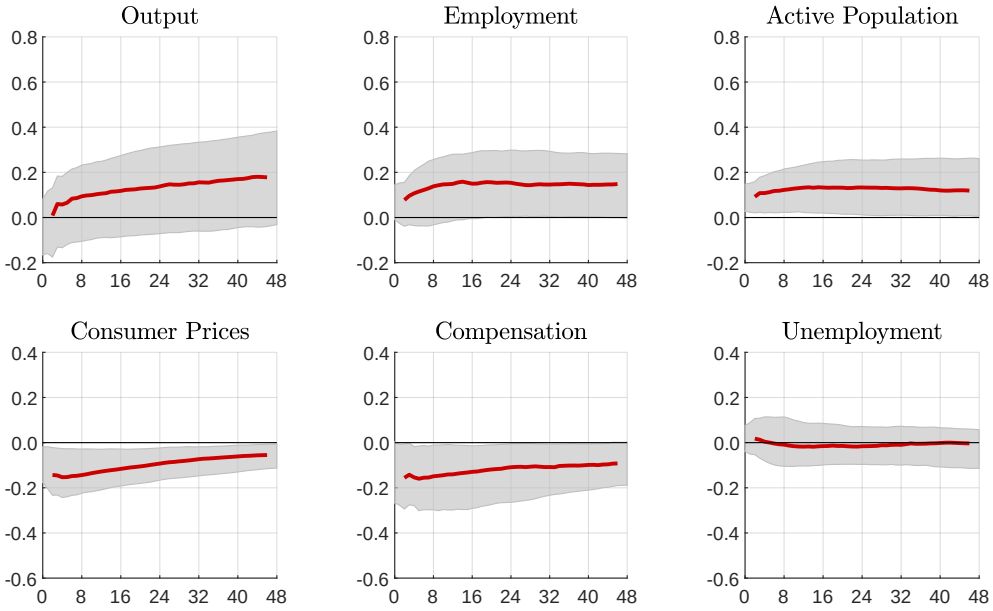
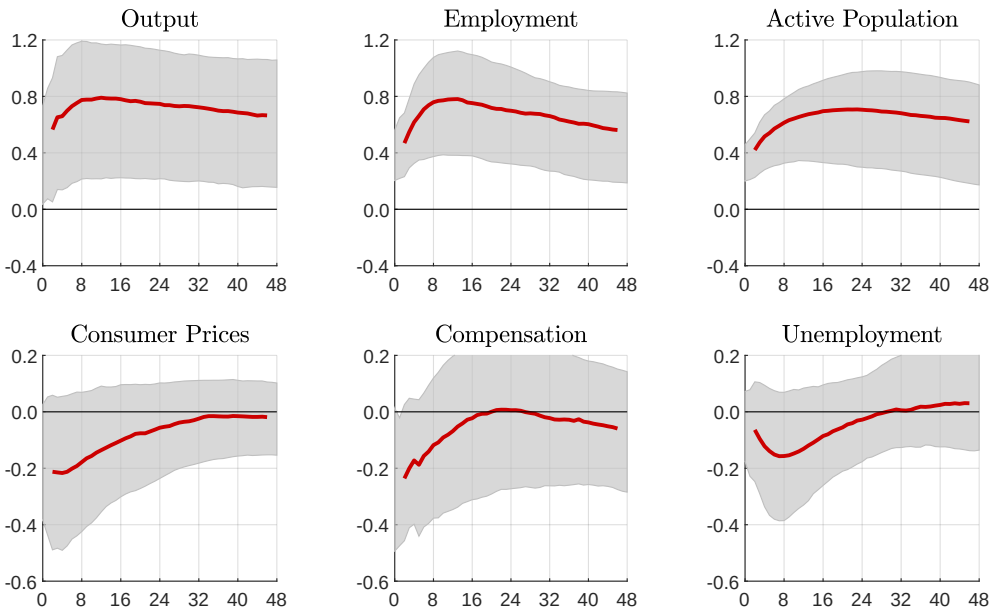


Figure 2: Average Impact of Temporary Contract EPL Reforms



The graphs show the average impact of reform events over a horizon of 48 quarters. Shaded areas represent [0.1,0.9] uncertainty bands.

Figure 3: Average Impact of Unemployment Benefit Reforms



See Figure 1 for explanations.

The estimates find fairly large average effects of UB and regular contract EPL reforms, while those of temporary contract EPL are very small and only weakly significant. In all cases, output, employment, and active population permanently increase after a liberalisation. Compensation per employee initially declines but reverts back to baseline in the medium term, while short-run declines in the unemployment rate dissipate as active population catches up with employment.

The effects of UB and regular contract EPL reforms differ in two important ways. First, for EPL reforms the responses of GDP, employment, and the labour force remain small at impact and build up only gradually reaching their full scale after about six to eight years. By contrast, UB reforms trigger an immediate response in the three series, including a large shift in labour supply. As a result, the decline in unemployment turns out larger for EPL reforms. The fast employment response to UB reforms is in line with the micro-

econometric evidence discussed in section 2, while the gradual response to EPL reforms may explain the ambiguous findings from studies focusing on the short run. Second, in contrast to UB reforms, regular contract EPL reforms also induce a shift in labour productivity, which accounts for about one third of the medium-term increase in GDP. Micro-econometric evidence suggests such effect due to various channels such as improved skill allocation, higher innovative activity, and increased worker effort (Boeri et al., 2015).

In both cases, reforms also feed immediately into the compensation per employee. As regards regular contract EPL reforms, this effect may partly stem from reforms of severance payments, which are a component of the compensation of employees in the national accounts. However, it is also in line with the evidence discussed in section 2 based on other type of reforms. The weak wage response to UB reforms may reflect composition effects as unemployment benefit reforms affect mostly unskilled labour, while the decline of wages in this segment may be counterbalanced by wage increases for skilled labour due to the increase in economic activity. Such outcome is, for instance, suggested by a meta analysis of Havranek et al. (2020) which finds the elasticity of substitution between skilled and unskilled labour to be below unity.

4.2 Individual Events

Table 2 reports the results for individual UB reforms. The table shows the median innovations together with the responses of GDP and employment after 40 quarters and of compensation per employee after 4 quarters. The German Hartz reform and a reform in the Netherlands in 2006, which raised employment by 1.6% and 0.9%, respectively, turn out as the largest events. These reforms involved substantial reductions in the duration of unemployment benefits. Dustmann et al. (2014) argue that the recovery of the German labour market in this period did not stem predominantly from the Hartz reforms, but owed to a more general stance of wage moderation in the preceding years. The cur-

rent estimates suggest that the reform provided a significant contribution to employment. Among the remaining events, reforms in Ireland consisted of moderate reductions in benefit levels, while an increase in benefits in 2015 in Italy was diluted by a simultaneous deregulation of regular contract EPL.

Table 2: Unemployment Benefit Reforms

		Sign	Imp Lag	SC	θ_t	Y_t	E_t	W_t
DE	2006 Q1	+	13	1.00	‡ 3.37	1.70	1.59	-0.79
IE	2010 Q1	+	1	.86	0.74	0.41	0.32	0.00
IE	2011 Q1	+	1	.71	0.40	0.04	0.12	0.06
IT	2015 Q1	−	1	.84	-0.84	-0.18	-0.18	0.04
NL	2006 Q4	+	2	1.00	‡ 2.70	0.89	0.73	-0.21
NL	2016 Q1	+	7	1.00	‡ 1.85	0.57	0.50	-0.15

'Sign' is the direction of the event with a + indicating a deregulation, 'Imp Lag' is the implementation lag. 'SC' is the sign concordance between innovations and the instrument, i.e. the share of draws for which innovations have the correct sign. The remaining columns show the medians of innovations θ_t and of the responses of GDP (Y_t) and employment (E_t) after 40 quarters and compensation per employee (W_t) after 4 quarters, respectively. ‡ denotes significance of θ_t at the 5% level.

For EPL reforms some cross-validation of the estimates is offered by the annual OECD indicators on the strictness of employment protection legislation of regular and temporary employment. These two series measure the regulatory stance based on expert judgement, aggregating categorial assessments of different aspects of the statutory laws with equal weights. They are therefore not strictly quantitative. Table 3 shows Spearman rank correlations between the estimates from the narrative VAR and the corresponding change in the OECD indicators.[4] The table also includes correlations adjusted for the sign of

[4]The indicators measure the regulatory stance on 1, January of a given year. In most cases, the change in the subsequent year is therefore to be taken. The sign of the indicators has been reversed such that a positive change stands for a deregulation. The OECD offers various versions of the indicators. I use

individual events. For regular contract EPL reforms the results indicate a fairly high correspondence of the estimates from the narrative VAR with the OECD indicators with sign-adjusted rank correlations of above 0.5 suggesting that the former is successful in identifying relevant innovations. For temporary contract EPL the numbers are lower reflecting the overall weaker effects found for this reform category.

Table 3: Comparison with OECD Indicators

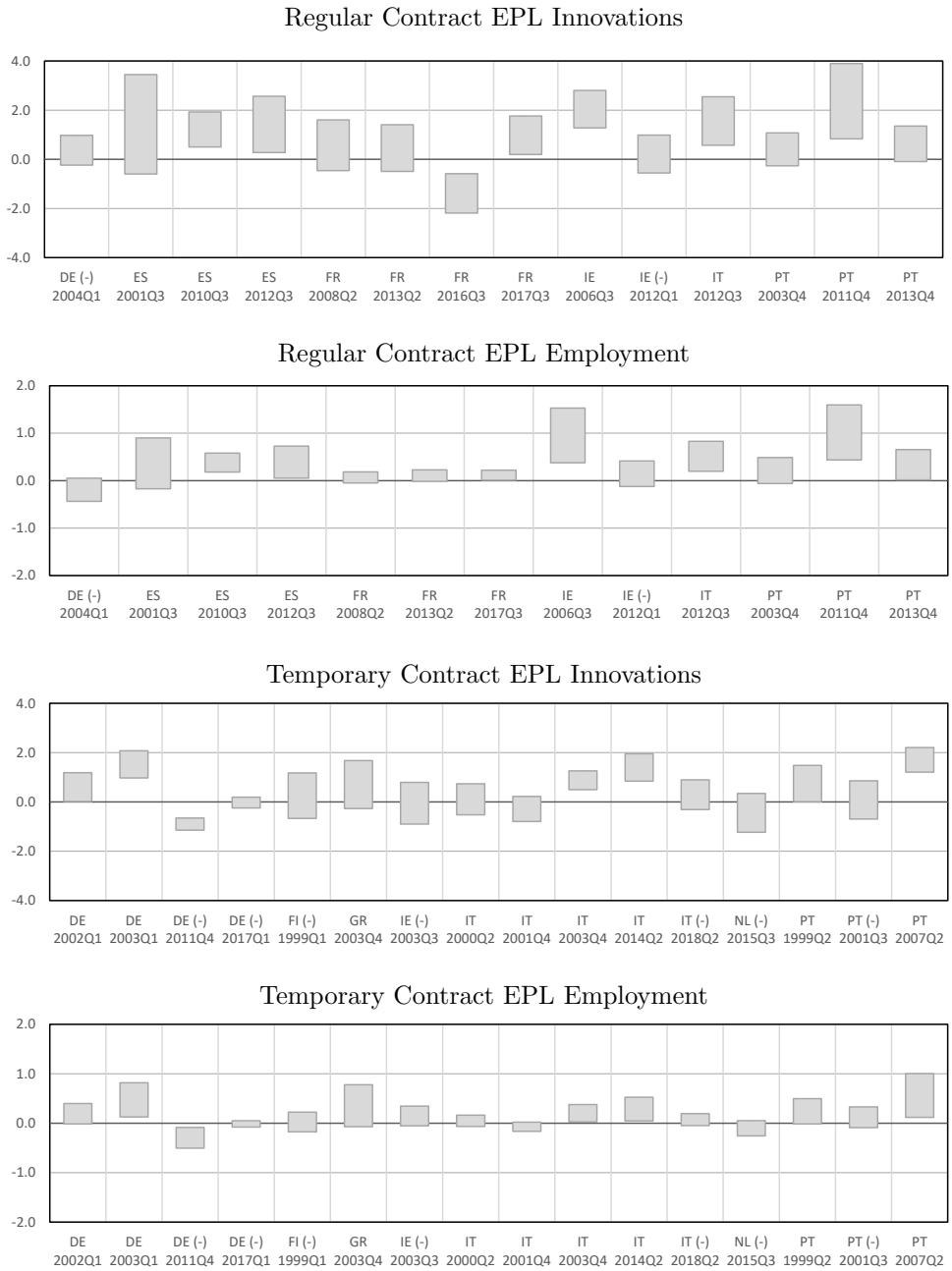
	EPL Regular				EPL Temporary			
	θ_t	Y_t	E_t	W_t	θ_t	Y_t	E_t	W_t
Original	.61	.66	.68	-.75	.51	.50	.52	-.57
Sign-adjusted	.46	.61	.55	-.60	.24	.36	.30	-.25

The table shows Spearman rank correlations of the median estimates of innovations θ_t and responses of individual series with the change in OECD indicators on the strictness of EPL. 5% and 10% critical values are .46 and .37 for EPL Regular and .43 and .34 for EPL Temporary, respectively. See Table 2 for further explanations.

Figures 4 plot the innovations and employment responses of individual EPL reforms, while Table 4 shows more detailed results for selected reforms. Among regular contract EPL reforms, the largest effects emerge for Portugal with a combined impact of two reforms in 2011 and 2013 on employment after 40 quarters of about 1.5%. For Spain and Italy the impact of reforms in between 2010 and 2012 is estimated with about 0.8% and 0.5%, respectively. Spain and Portugal shared a comparatively tight regulatory stance ahead of the reforms, which imposed reductions in severance payments and the installation of capital-funded components for the latter, together with shortenings in the terms of notice of dismissals. Among temporary contracts, several events induced a significant empoly-ment response, including the German Hartz reforms in 2003 together with a tightening reform in 2011, reforms in Italy in 2003 and 2014, and a reform in Portugal in 2007.

the earliest ones, as these are the only ones that cover the entire sample. For further information see https://www.oecd.org/employment/emp/oecdindicatorsofemploymentprotection.htm.

Figure 4: Impact of Individual EPL Reforms



The graphs show the [0.16, 0.84] interquantile ranges of reform innovations θ_t and the employment response after 40 quarters. Tightening reforms are indicated by a (-) sign.

Table 4: Impact of Selected Individual EPL Reforms

		Sign	Imp Lag	SC	θ_t	Y_t	E_t	W_t	OECD EPL
Regular Contract EPL Reforms 2010 - 2018									
ES	2010 Q3	+	2	.98	‡ 1.43	0.27	0.41	-0.41	0.14
ES	2012 Q3	+	1	.89	1.44	0.21	0.35	-0.35	0.25
FR	2017 Q3	+	0	.93	1.11	0.09	0.12	-0.14	0.06
IT	2012 Q4	+	1	.98	‡ 1.91	0.92	0.51	-0.61	0.17
PT	2011 Q4	+	0	.93	‡ 2.80	1.89	1.16	-1.41	0.53
PT	2013 Q4	+	1	.80	0.80	0.54	0.35	-0.41	0.25
Temporary Contract EPL Reforms 2000 - 2007									
DE	2002 Q1	+	1	.84	0.63	0.14	0.16	-0.15	0.50
DE	2003 Q1	+	1	.97	‡ 1.61	0.45	0.49	-0.41	0.50
IT	2003 Q4	+	1	.98	‡ 0.94	0.20	0.16	-0.14	0.38
PT	1999 Q2	+	1	.84	0.83	0.24	0.21	-0.21	0.00
PT	2007 Q2	+	1	.98	‡ 1.83	0.68	0.50	-0.54	0.63

See Table 2 for explanations. The table shows reforms of regular contract EPL in between 2010 and 2018 and of temporary contract EPL in between 2000 and 2007 for which innovations are beyond the [.16,.84] uncertainty band. ‡ denotes significance of θ_t at the 5% level. In all these cases, GDP and employment responses are significant at least at the 10% level. Column 'OECD EPL' shows the OECD indicator.

4.3 State Dependence

It has been argued that the effects of EPL reforms are smaller and subject to long delays during episodes of low economic growth. Boeri and Garibaldi (2007) claim that temporary contract EPL reforms generate no more than a transient 'honeymoon' effect on employment in good business conditions, while being ineffective during recessions as firms are constrained by the stock of employees on permanent contracts. Similarly, New Keynesian models with search frictions suggest a more delayed employment response to a reduction in firing costs during recessions (Cacciatore et al., 2016). Empirical evidence for weaker

effects of reforms during low growth is provided by Gehrke and Weber (2017) for Germany and by Duval, Furceri and Jalle (2020) for EPL reforms across a panel of countries.

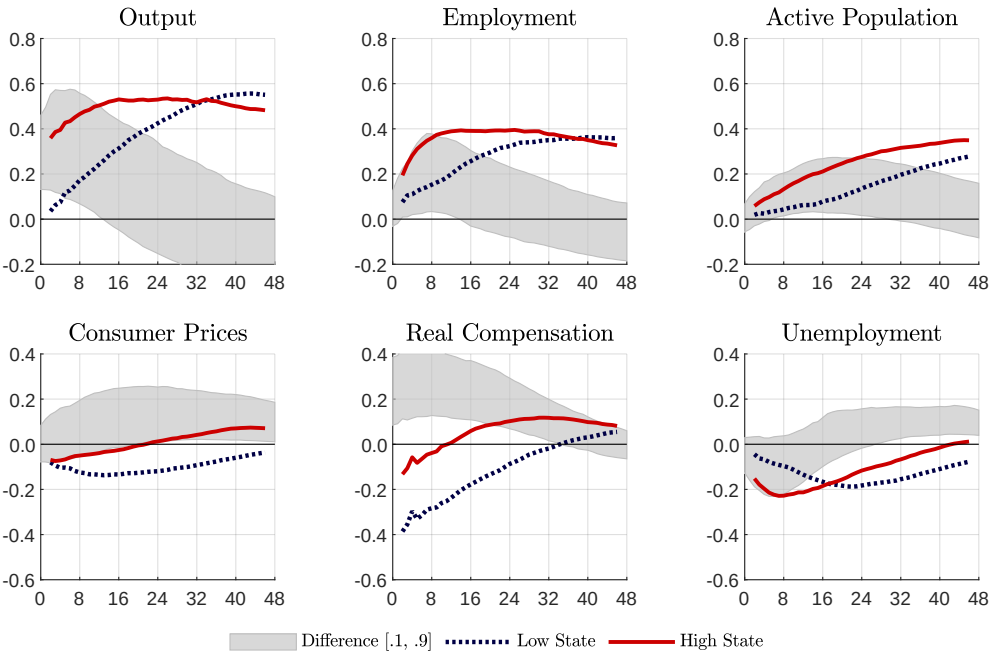
To assess state dependencies I re-estimate the model for subsets of reforms that took place in either low or high states of economic growth. I divide the events into two groups of about equal size depending on the annual growth rate of euro area GDP at the implementation date.[5] Since the number of events in each state becomes rather small for the individual reform categories, I consider regular and temporary contract EPL reforms jointly in these estimates, setting the instrument to non-zero values at the implementation date and the subsequent quarter. Joint estimation is warranted by the fact that the two reforms categories yield similar standardised impulse responses. Moreover, events happen to be about equally split across the two states for both types of reforms. Separate state-dependent estimates for the two reform categories yield similar patterns as well, as shown in Figures B.7 to B.9 in Annex B.

Figure 5 shows the average impact of reforms across the two states. To avoid that results reflect differences in the scale of reform events across states, the responses are rescaled to reflect an average unit reform innovation.[6] The results support the conjecture of state dependence in the effects of EPL reforms for the short-term. Economic activity and employment respond very sluggishly under low growth, but are subject to an immediate response under high growth. By contrast, the wage response is larger under low growth. However, the differences vanish at longer horizons leaving the medium-term impact unaffected. Correspondingly, as shown in Table B.2 in Annex B, the impacts of individual events remain similar to those presented in section 4.2.

[5]Using euro area GDP avoids endogeneity issues that may arise with defining states from the GDP of individual countries, but is still informative about demand conditions in the latter. As an alternative measure of the state I considered the level of the euro area business cycle at the implementation date, as estimated from a one-sided Christiano-Fitzgerald (2003) bandpass filter, with similar results.

[6]For each draw, I rescale the reform innovations such that their expected absolute value γ equals one and calculate the corresponding average impact of reforms.

Figure 5: Impact of EPL Reforms in High and Low Growth States



The graphs show the impact of EPL reforms implemented in low and high states of euro area annual GDP growth. The responses are standardised to reflect unit average innovations. The shaded areas represent [.1,.9] uncertainty bands for the difference between the two estimates.

4.4 Robustness

Figures B.1 to B.6 in Annex B present various estimates exploring the timing of reform innovations and further robustness issues. First, in combining the implementation date and the subsequent quarter, the main estimates implicitly assume that innovations at these two dates generate identical impulse responses. Estimates defining the instrument separately for the two periods confirm that this is the case. Second, using the legal adoption date as a reference point for defining the instrument yields small effects. Hence, there is little indication of announcement effects.

Third, the condition of contemporaneous exogeneity of the instrument in equations (3)

requires the instrument to be independent of other shocks that occur within the same quarter. This condition is likely to be satisfied on a priori grounds, as labour market reforms typically respond to imbalances that have accumulated over the past, while their legal adoption is delayed by consultations with social partners. The condition holds by construction for the estimates related to UB and temporary contract EPL, as reform innovations are all set in periods subsequent to legal adoption. For regular contract EPL, a robustness check can be performed by removing the implementation date from the instrument for events with zero implementation lag. This has little effect on the results.

Fourth, I explore the presence of reform innovations at higher lags by setting instrument $z_{c,t}$ to non-zero values at individual higher lags. As discussed in Annex B, these specifications have an interpretation as robustness check against a violation of the invertibility condition. Again I find only small effects, although there is some indication of a more persistent wage response and a smaller short-term employment response for regular contract EPL. Finally, the results are fairly robust against excluding reforms in specific countries from the sample. Specifically, excluding regular contract EPL reforms of Portugal, Spain, or Italy has little effect, although the responses are necessarily of somewhat smaller scale.

5 Conclusions

While the narrative approach so far has been underutilized in studying the effects of labour market reforms, the results of this paper suggest that quarterly narrative indicators are useful for this purpose. Even though identification is based on minimal information, it allows for estimating the effects of individual events in a granular way.

The time series perspective complements micro-econometric evidence by aligning short-run and medium-term effects and permitting the exploration of state dependencies. The delayed employment response to EPL reforms, in particular, creates difficulties for micro-

econometric designs to fully gauge their impact and may partly explain the ambiguous findings of studies on the short-term effects of reforms. Moreover, quasi-experimental designs allow only for indirect conclusions on the aggregate effects of reforms.

From a policy perspective, the results underline the importance of distinguishing between different types of reforms. After the mixed experiences with reforms of temporary contract EPL in the early years of the euro area, reforms undertaken since 2010 with a focus on regular contracts appear to have been more successful in improving labour market performance, despite the arguments of Eggertson et al. (2014) against implementing structural reforms at the zero lower bound. However, the long delays in achieving their full effect render EPL reforms rather ineffective as a counter-cyclical policy tool.

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Online Annex A: Estimation

Discriminant Regression

Discriminant analysis is e.g. described in Maddala (2013: 79ff). Consider a dichotomous variable z_t that takes the value $z_t = 1$ for m observations and $z_t = 0$ for the remaining $T-m$ observations. The objective of discriminant analysis is to estimate function $\psi(u_t)$ predicting z_t from a set of random variables $u_t = (u_{1,t}, \ldots, u_{n,t})$ based on the rule $\hat{z}_t = 1$ if $\psi(u_t) > 0$ and $\hat{z}_t = 0$ otherwise. Under the assumption that $u_t|z_t = j \sim N(\mu_j, \Sigma)$ for $j = 0, 1$, the optimal discriminant function is linear, $\psi(u_t) = a'u_t$.

Under the assumption that the cost of misclassifying an observation in category j is inversely proportional to the relative number of observations in the category, the maximum likelihood estimate of vector a maximizes the ratio of the squared difference in means between groups and the variance within groups, $(a'\Sigma a)^{-1}\left[a'(\mu_1 - \mu_0)\right]^2$. This is equivalent up to scale to estimating a via OLS from the regression $z_t = a_0 + a'u_t + \zeta_t$. The OLS estimates $\hat{a}$ and $\hat{\sigma}^2_\zeta$ are subject to standard inference, $\hat{a} \sim N(a, \sigma^2_\zeta)$ and $\sigma_\zeta^{-2}\sum \zeta_t^2 \sim \chi^2_{T-n-1}$ (Maddala, 2013: 79ff).

In a Bayesian framework, this allows for direct sampling from the parameters of the regression, based on a Normal-Gamma conjugate prior. Specifically, let $U = (u_1', \ldots, u_T')'$ and $Z = (z_1, \ldots, z_t)'$. Assuming an improper prior $p(a, \sigma^2_\zeta) \propto \sigma_\zeta^{-2}$ results in the posterior $\sigma_\zeta|U, Z \sim IG(v, \hat{\sigma}_\zeta v)$, and $a|\sigma_\zeta, U, Z \sim N(\hat{a}, \sigma^2_\zeta S_u^{-1})$, where $v = (TC\text{-}n\text{-}1)/2$, $\hat{a}$ and $\hat{\sigma}_\zeta$ are the OLS estimates from equation (6), and $S_u = \Sigma_{t=1}^T u_t u_t'$.

While there are other means to solve the above classification problem, such as probit and logit regressions, linear DCA is efficient in case that the explanatory variables u_t are normally distributed, as is the case with sign-adjusted VAR forecast errors. The regression is robust against moderate deviations from normality (Maddala, 2013: 79ff).

Coefficients α_c are then found as $\alpha_c = (a'\Sigma_c a)^{-1}a$ to achieve unit variance of innovations $\theta_{c,t}$. Given a it remains to construct the full decomposition $A_c u_{c,t} = \epsilon_{c,t}$. I use the Gram-Schmidt orthogonalization proposed by Arias, Rubio Ramirez and Waggoner (2018). Equation (2) together with the requirement $\mathbb{E}\epsilon_{c,t}\epsilon_{c,t}' = I_n$ implies $A_c\Sigma_c A_c' = I_n$ and, hence, $\Sigma_c^{-1} = A_c'A_c$. Further, from the QR decomposition, A_c' can be expressed as $A_c' = A_c^* Q_c$, where A_c^* is the Choleski decomposition of Σ_c^{-1}. With $\alpha_c = A_c^* q_{c,1}$, where $q_{c,1}$ is the first column of Q_c, the latter is found as

$q_{c,1} = (A_c^*)^{-1}\alpha_c$, followed by the normalization $q_{c,1}/||q_{c,1}||$. The remaining columns of matrix Q_c are constructed without any further constraints.[1]

Some modification is required in case that standardised residuals $u_{c,t}^* = \delta_{c,t}S_c^{-1}$ are used in the DC regression. I set S_c as diagonal matrices with country-specific residual variances on the diagonal, $S_{c,ii} = 1/T\sum_t u_{c,ti}^2$ for i $= 1, \ldots, n$. The standardisation requires a corresponding adjustment of the mapping of vector a into matrices A_c that accounts for the different re-scaling of residuals across countries, as it implies $\alpha' S_c^{-1/2} u_{c,t} = \theta_{c,t}$. With $A_{c,0} = A_c^* S_c^{-1/2} Q_c$, it is easily verified that the mapping results in $q_{c,1} = (A_c^*)^{-1} S_c^{-1/2} a$.

Panel VAR with Partial Shrinkage Prior

The reduced form VAR for country c can be written as $y_c = Y_c B_{c,+} + XF_c + U_c$ with $y_c = (y_{c,1}, \ldots, y_{c,t})'$ and $Y_c = (y_{c,t-1}, \ldots, y_{c,t-p})$. The matrix of predetermined variables $X = (x_1, \ldots, x_t)'$ is defined accordingly, where vector x_t includes the constant term. Denote with $\beta_c = \text{vec}(B_{c,+})$ and $f_c = \text{vec}(F_c)$ the vectors of coefficients of the VAR and assume that $\beta_c|\beta, \lambda, f_c \sim N(\beta, \lambda\Omega)$. Further, assume diffuse priors for pooled coefficients $\beta \sim N(\overline{\beta}, \Psi)$ with $\Psi^{-1} \equiv 0$, covariances $p(\Sigma_c) \propto |\Sigma_c|^{-\frac{1}{2}(n+1)}$, and f_c.

For a given $\lambda\Omega$ this specification amounts to the random coefficient VAR model introduced by Canova (2005). The coefficients emerge as weighted averages of those estimated for country c in isolation and of the common mean β, with the weights depending on the degree of shrinkage. Jarocinski (2010) extends the model by using an inverse-gamma prior for shrinkage parameter $p(\lambda) \propto \lambda^{-(v+1)} \exp(-\frac{s}{\lambda})$ and presents a Gibbs sampler to draw from the model parameters. I set $v + 1 = s = 0$ resulting in an uninformative prior and specify the diagonal element $\Omega_{c,kk}$ related to coefficient $B_{cp,ij}$ as $\Omega_{kk} = \widehat{\sigma}_i^2/\widehat{\sigma}_j^2$, where $\widehat{\sigma}_i^2$ is the median across countries of sample residual variances from univariate 4^{th}-order autoregressions of series $y_{c,ti}$. By noting that the posterior of the reduced form VAR for the individual countries is independent of α, draws from the full model can be obtained by extending the Gibbs sampler of Jarocinski (2010) with an additional step to draw α conditional on draws of β_c and the related residuals U_c. Hence, after each draw of U_c the DC regression is re-estimated and a draw from the resulting posterior is obtained.

[1] Arias, J., J. Rubio-Ramirez, and D. Waggoner. 2018. Inference based on SVARs identified with sign and zero restrictions: theory and applications. *Econometrica* 86(2), 685-720.

Online Annex B: Robustness Checks

This annex discusses the timing of reform innovations. To see the rationale for multi-period innovations in case of rare events, note that, based on the moving average representation of the VAR, the impact of an event Θ_t on the VAR series in period $t+h$ can be expressed as

$$\mathbb{P}(y_{t+h} - y_t|\Theta_t) = \sum_{s=0}^{h} C_s \mathbb{P}(u_{t+h-s}|\Theta_t) = \sum_{s=0}^{h} C_s \theta_{t+h-s,t},$$

where $y_t = \sum_{s=0}^{\infty} C_s u_{t-s}$. $\mathbb{P}$ denotes the linear projection operator, and $\theta_{t+h-s,t}$ is the innovation in period $t+h-s$ triggered by event Θ_t.

The invertibility condition as defined by Stock and Watson (2018) amounts to $\mathbb{P}(\Theta_t|u_t) = \theta_{t,t}$ ensuring that the entire effect of the event is represented in $\theta_{t,t}$. The condition is equivalent to $\mathbb{P}(u_{t+h}|\Theta_t) = 0$ for $h > 0$. The case of a sparse binary instrument has yet two specific implications. First, lagged innovations $\theta_{t+h,t}$ are straightforwardly identified for $h > 0$. Non-invertibility issues may therefore be explored and corrected by estimating the DC regression with the instrument set to non-zero values at periods other than t. Second, the emergence of lagged innovations does not necessarily indicate misspecification of the VAR, as rare events may trigger specific dynamics that is not predictable, precisely because these events are rare.

I start with estimating equation (6) with instrument z_t set to non-zero values at implementation dates only and then inspect the size of the resulting innovations in the preceding and subsequent quarters. The top panel of Table B.1 shows the average of sign-adjusted innovations $\gamma_h = m^{-1} \sum \theta_{t+h} z_t$ across events for positive and negative values of h.[2] For regular contract EPL and UB reforms, significant innovations arise at the implementation date, but there is also some indication of further lagged effects in the subsequent quarters. For temporary contract EPL sign-adjusted innovations are insignificant in all periods.

The second hand panel of Table B.1 shows results from estimates where z_t is set to non-zero values in the subsequent quarter, while in the third panel z_t takes non-zero values in both periods. The last case yields fairly consistent patterns on the timing of innovations. For temporary

[2]In calculating γ_h for higher lags, I remove periods that are affected by other reforms. This applies, in particular, to reforms in Portugal in 2011 Q4 and 2013 Q4, which were followed by an UB reform in 2012 Q2 and a cut in public wages in 2014 Q2, respectively, and in Ireland in 2011 Q1. I also exclude higher lags for a reform in France in 2008 Q2 because of the financial crisis.

contract EPL and UB reforms the innovations in the subsequent quarter exceed those at the implementation date, while the results are more balanced for regular contract EPL. Figures B.1 and B.2 show that the average impact of reforms is of similar shape across different specifications of the instrument, which justifies pooling the iatter over the two periods.

Table B.1: Reform Innovations Around Implementation Dates

Lags h	-2	-1	0	1	2	3	4	5	6
Implementation Date									
EPL Regular	-0.07	0.17	‡ 0.64	0.27	0.19	-0.10	-0.08	0.12	0.05
EPL Temporary	0.15	0.05	0.07	-0.08	0.07	0.04	-0.13	-0.01	0.04
Unemployment Benefits	-0.28	-0.06	‡ 0.55	0.43	0.16	0.28	-0.63	0.02	0.12
Subsequent Quarter									
EPL Regular	0.17	‡ 0.42	0.32	‡ 0.59	0.05	-0.17	-0.08	-0.08	0.10
EPL Temporary	0.08	-0.01	0.05	‡ 0.53	-0.21	0.14	-0.08	0.14	-0.16
Unemployment Benefits	-0.21	-0.25	0.27	‡ 1.27	-0.23	‡ 0.26	-0.25	0.07	0.01
Implementation Date plus Subsequent Quarter									
EPL Regular	0.07	‡ 0.36	‡ 0.58	‡ 0.56	0.17	-0.16	-0.11	0.03	0.09
EPL Temporary	0.16	0.02	-0.00	‡ 0.42	-0.14	0.16	-0.13	0.12	-0.10
Unemployment Benefits	-0.28	-0.21	‡ 0.47	‡ 1.20	-0.08	‡ 0.33	-0.49	0.07	0.07

The table shows the mean values γ_h of sign-adjusted innovations $\theta_{t+h} z_t$. For the upper two panels z_t is set to non-zero values only at the implementation date and the subsequent quarter, respectively. In the third panel, z_t is set to non-zero values in both periods. ‡ indicates significance at the 5% level.

Figures B.3 show the results from using the legal adoption date as a reference point for the instrument. This yields small outcomes. There is some indication of negative anticipation effects for temporary contract EPL, but this may well be related to state dependence, as there is some overlap of adoption with with implementation dates (see Table 1) and those events with an implementation lag of one happen to occur predominantly in low states.

Figures B.4 explore further related issues for regular contract EPL. First, as discussed in the main text, contemporaneous exogeneity holds by construction for estimates related to UB and temporary contract EPL, as the instrument refers entirely to periods subsequent to legal adoption. For regular contract EPL, the validity of the condition an be examined by removing the adoption date from the instrument. Figure B.4 shows that this yields almost identical results.

Second, a small effect in the previous quarter is suggested by the results shown in Table B.1. Adding the previous quarter to the instrument yields however very similar results.

Table B.1 shows little evidence for additional effects at higher lags, although the results for UB reforms are somewhat erratic due to the small number of events. These results assume that reform innovations at higher lags trigger impulse responses similar to those from the baseline estimates. To relax this assumption, I follow Budnik and Rünstler (2020) in re-estimating the model with the instrument set to non-zero values at individual higher lags. The lags are randomly drawn, while a sign concordance (SC) prior is used to identify lags with important effects. Define the sign concordance of θ_t with z_t as the share of events with equal sign,

$$\varphi(B_+, \alpha, Y, Z) = m^{-1} \sum_{z_t \neq 0} \mathbf{I}(\theta_t z_t > 0). \tag{7}$$

where $\mathbf{I}()$ is the indicator function and m is the number of non-zero observations in z_t. Given the independence of u_t over time, the number of correct signs follows a binomial distribution,

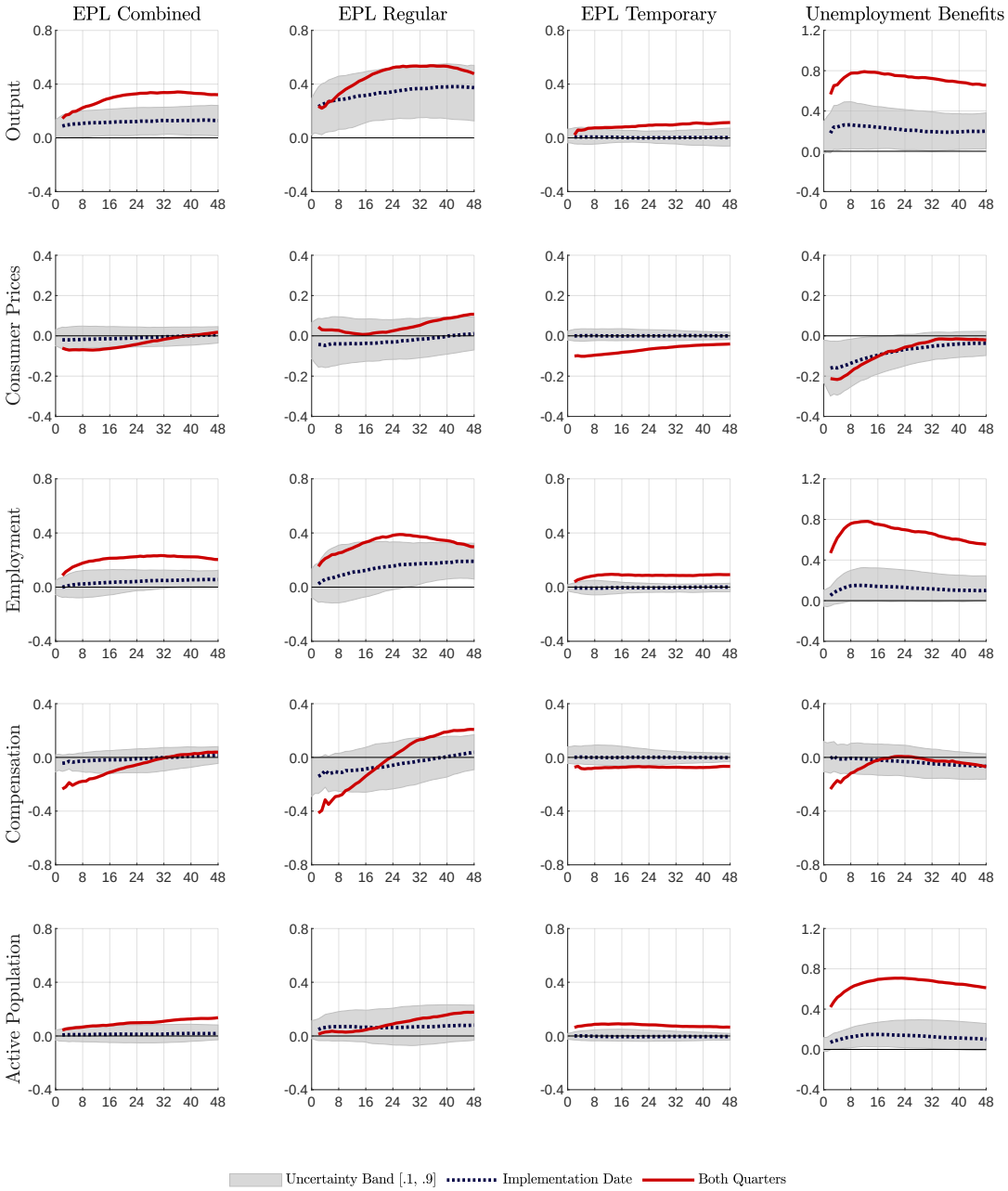
$$p(m\varphi|\alpha, \pi, B_+, Y, Z) = f_z(m\varphi; m, \pi), \tag{8}$$

where π is the unknown probability of the correct classification of a single event. In combination with an appropriate prior on π that supports high values of the latter, the SC statistics can be used for model selection purposes. The prior attaches higher weights to those draws of α that generate high sign concordance and thereby selects between model variants. The extended model is estimated by proceeding with sampling as described in Annex A with a rejection sampling step based on the SC prior added.

As discussed in Annex A, this specification has an interpretation as robustness check against a violation of the invertibility condition. I draw the number of lags from a discrete uniform distribution in the range of 2 to 4 and use a uniform distribution for π over support $[\pi_0, 1]$ with $\pi_0 = 0.7$ for regular contract EPL and $\pi_0 = 0.6$ otherwise (see Budnik and Rünstler, 2020). The results in Figures B.5 indicate only minor indications for lagged effects. For regular contract EPL I find mild negative effects on all series at higher lags with some amplification of wage and price responses and a mitigation of short-term GDP and responses.

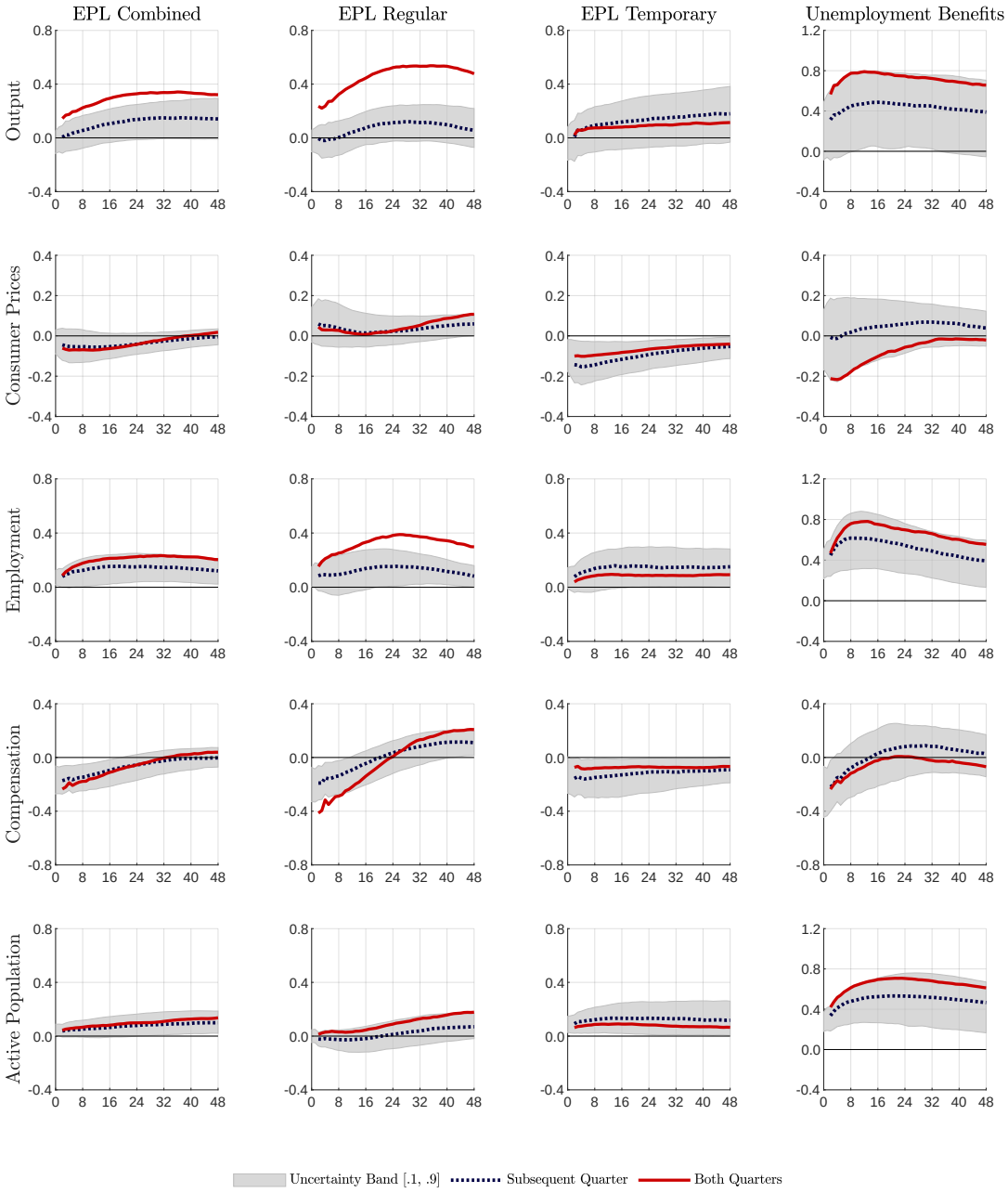
Finally, Figures B.6 show the effects of excluding regular contract EPL reforms in Portugal, Spain, Italy and Ireland from the DC regression (6).

Figure B.1: Instrument only at Implementation Date



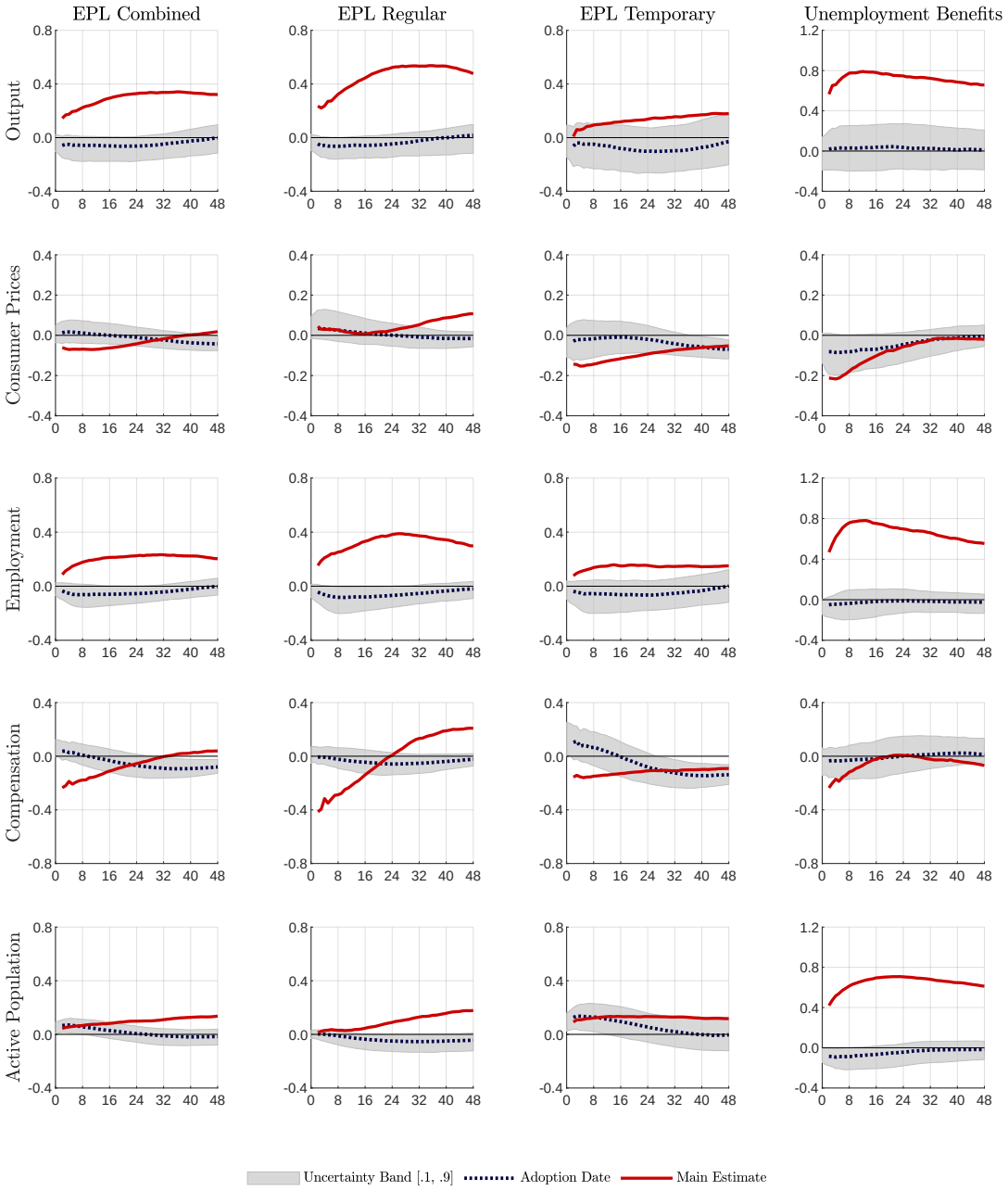
The instrument is set to non-zero values at either both the implementation date and the subsequent quarter or only at the former. The graphs show the average impact of reforms as defined in the main text. The shaded areas represent [.1,.9] uncertainty bands.

Figure B.2: Instrument only in Subsequent Quarter



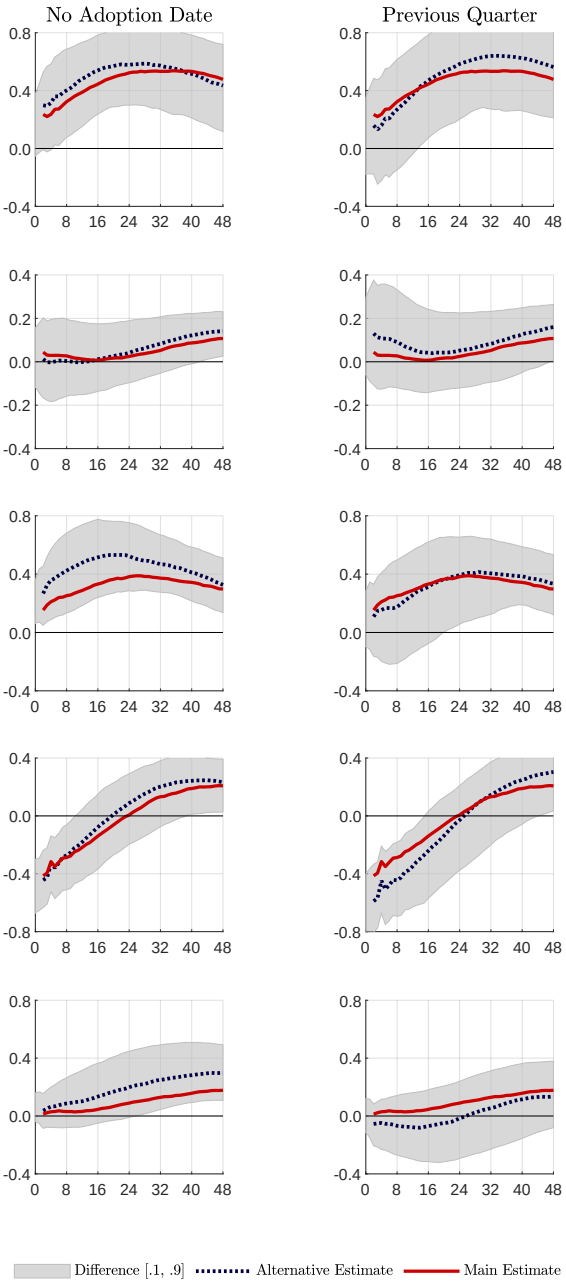
The instrument is set to non-zero values either at both the implementation date and the subsequent quarter or only for the subsequent quarter. See Figure B.1 for further explanations.

Figure B.3: Instrument only at Legal Adoption Date



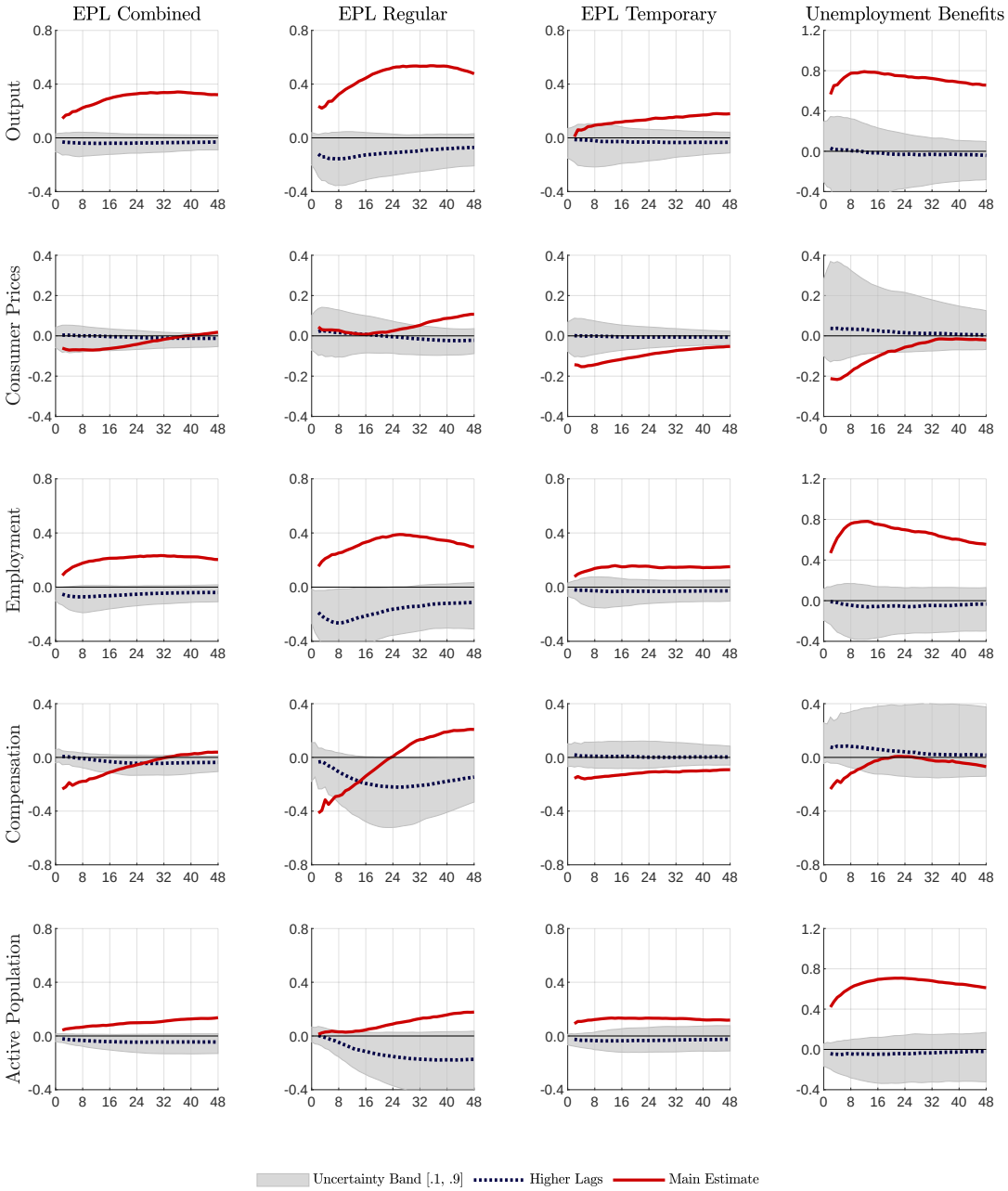
The instrument is set to non-zero values at the legal adoption date. See Figure B.1 for further explanations.

Figure B.4: Specific Estimates for Regular Contract EPL



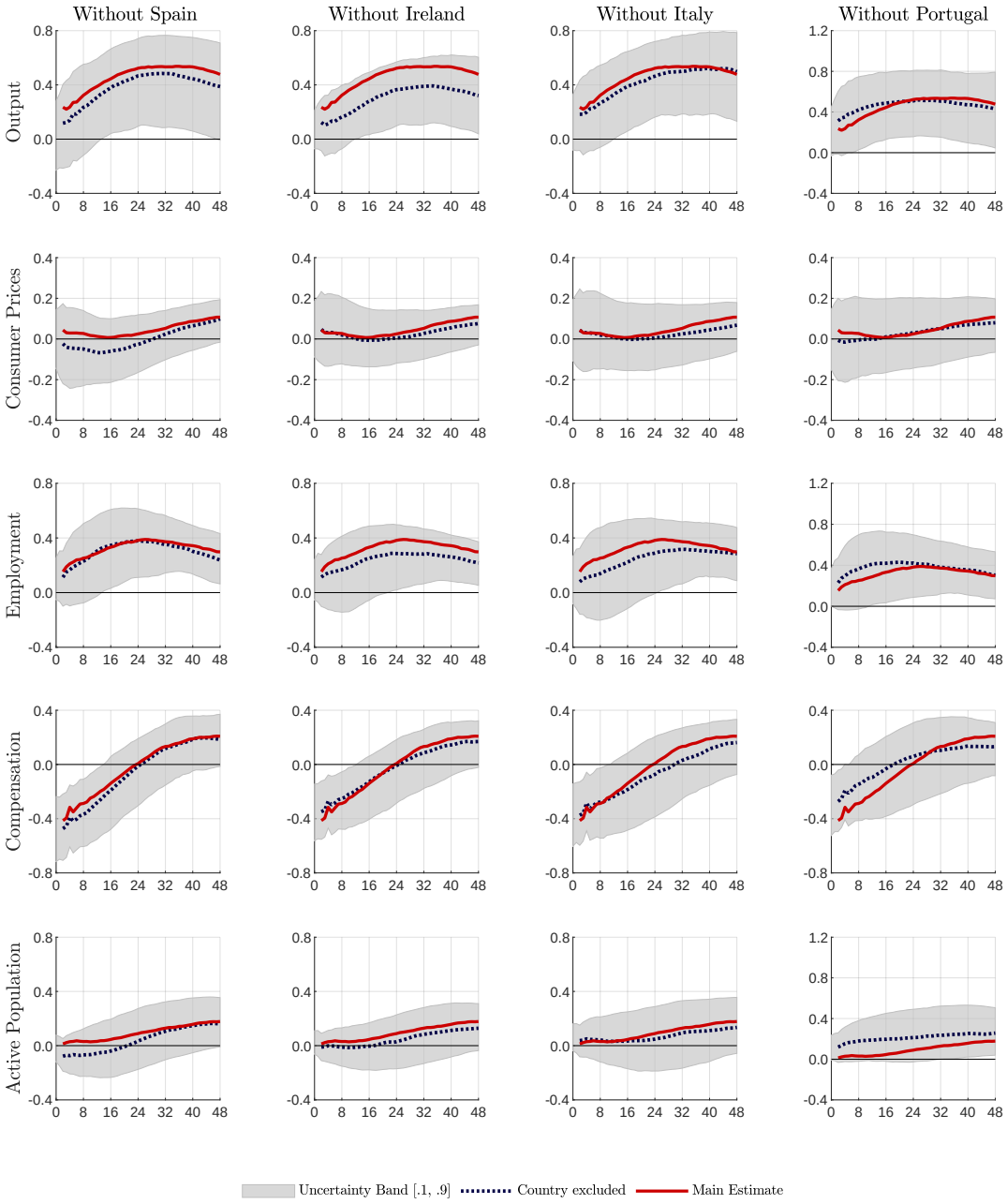
The left-hand graph uses the instrument as in the main estimate, but with the implementation date excluded for events with zero implementation lag. The right-hand graph adds the preceding quarter to the instrument. See Figure B.1 for further explanations.

Figure B.5: Lagged Effects from Individual Quarters 2 to 4



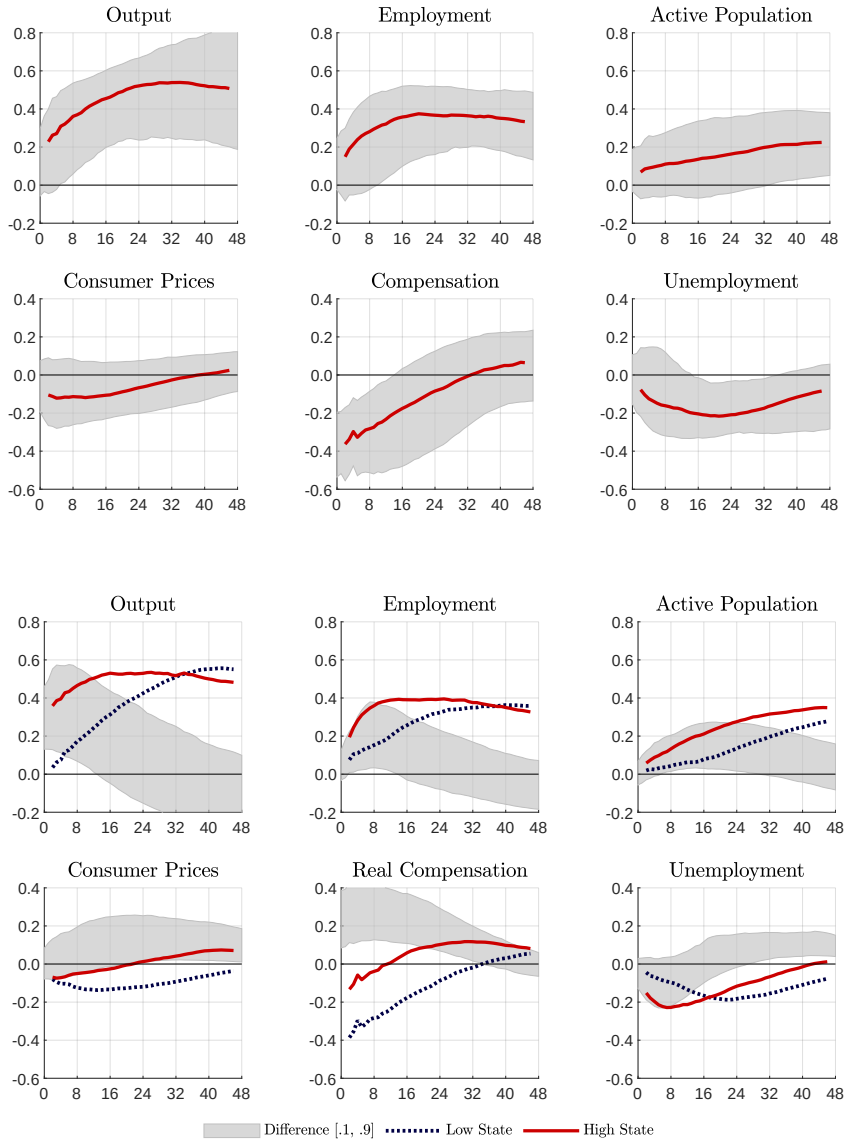
The instrument set to non-zero values at individual quarters randomly drawn from a discrete uniform distribution over support 2 to 4. A sign concordance prior is used to weight the draws. See the main text and Figure B.1 for further explanations.

Figure B.6: Individual Countries Excluded from DC Regression



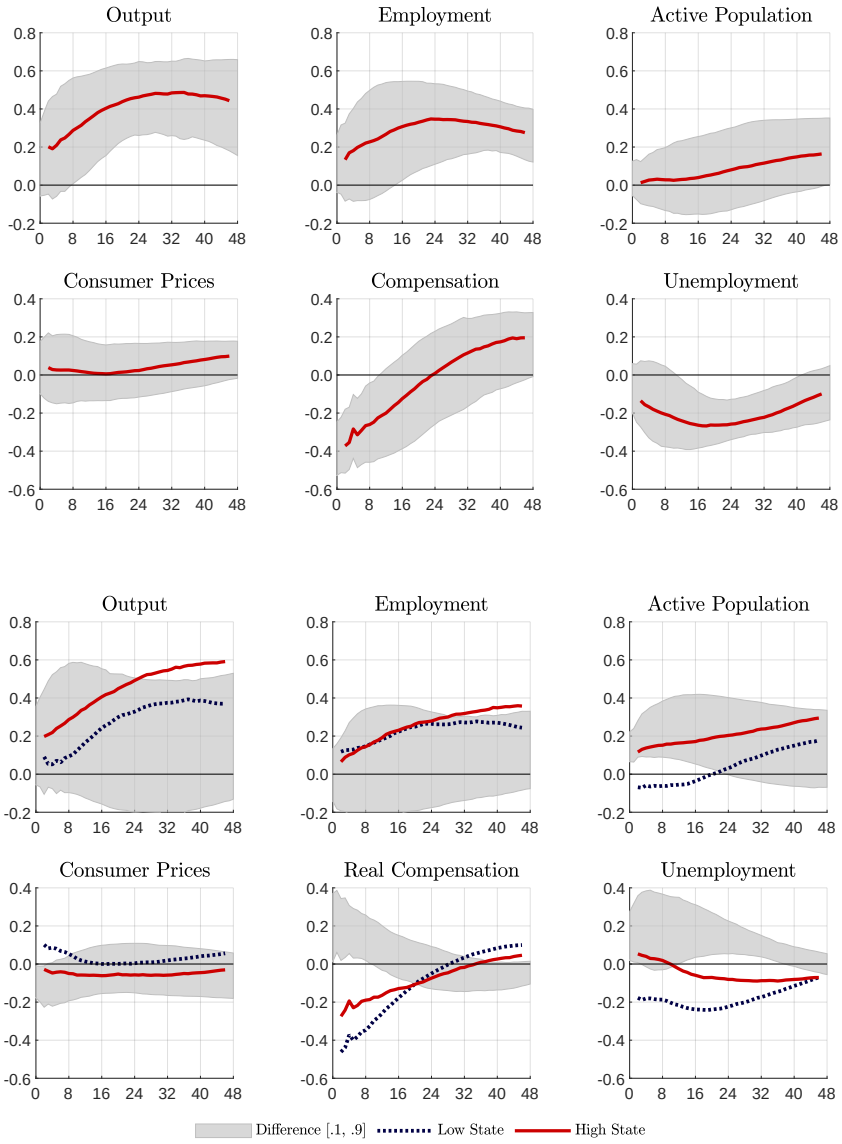
The graphs show estimates from the baseline model for regular contract EPL with reform estimates for individual countries excluded from the DC regression. See Figure B.1 for further explanations.

Figure B.7: Standardised Average Impact of EPL Reforms (Combined)



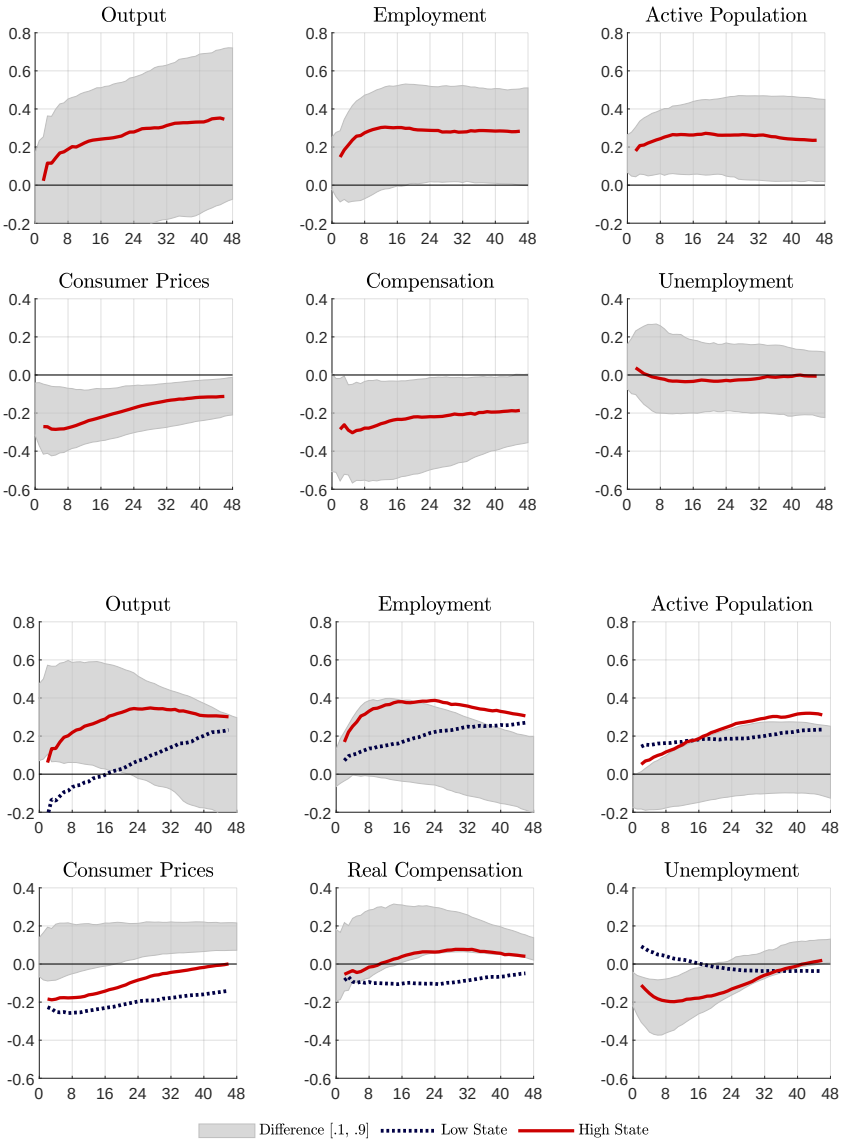
The upper graphs show the average impact of EPL reform events with temporary and regular contract EPL combined. Shaded areas represent [0.1,0.9] uncertainty bands. The lower graphs show the average impact of reforms implemented in low and high states of euro area annual GDP growth. Shaded areas show [.1,.9] uncertainty bands for the difference between the two estimates. In all cases, the responses are standardised to reflect unit average reform innovations.

Figure B.8: Standardised Average Impact of Regular Contract EPL Reforms



The upper graphs show the average impact of regular contract EPL reforms. Shaded areas represent [0.1,0.9] uncertainty bands. The lower graphs show the average impact of reforms implemented in low and high states of euro area annual GDP growth. The shaded areas represent [.1,.9] uncertainty bands for the difference between the two estimates. In all cases, the responses are standardised to reflect unit average reform innovations.

Figure B.9: Standardised Average Impact of Temporary Contract EPL Reforms



The upper graphs show the average impact of temporary contract EPL reforms. Shaded areas represent [0.1,0.9] uncertainty bands. The lower graphs show the average impact of reforms implemented in low and high states of euro area annual GDP growth. Shaded areas show [.1,.9] uncertainty bands for the difference between the two estimates. In all cases, the responses are standardised to reflect unit average reform innovations.

Table B.2: Employment Responses to Individual EPL Reforms

		Sign	State	OECD EPL	Baseline	Combined	State Dependent
Regular Contract EPL Reforms							
DE	2004 Q1	−	1	.00	-0.17	-0.31	-0.32
ES	2001 Q3	+	1	.00	0.28	0.49	0.50
ES	2010 Q3	+	1	-.14	0.41	0.34	0.33
ES	2012 Q3	+	0	-.25	0.36	0.21	0.00
FR	2008 Q1	+	1	-.13	0.05	0.09	-0.01
FR	2013 Q1	+	0	.00	0.06	0.11	0.03
FR	2017 Q2	+	1	-.06	0.12	0.09	0.11
IE	2006 Q3	+	1	-.17	0.85	0.94	0.80
IE	2012 Q1	−	0	.13	0.11	0.07	-0.04
IT	2012 Q3	+	0	-.08	0.51	0.53	0.45
PT	2003 Q4	+	0	-.17	0.20	0.14	0.14
PT	2011 Q4	+	0	-.53	1.16	0.45	0.56
PT	2013 Q4	+	0	-.25	0.35	0.59	0.61
Temporary Contract EPL Reforms							
DE	2002 Q1	+	1	-.50	0.16	0.22	0.13
DE	2003 Q1	+	0	-.50	0.49	-0.07	0.30
DE	2011 Q4	−	0	.13	-0.29	-0.25	-0.23
DE	2017 Q1	−	0	.15	-0.01	-0.28	-0.28
FI	1999 Q1	−	1	.00	0.02	-0.34	0.00
GR	2003 Q4	+	0	-2.00	0.23	0.07	-0.02
IE	2003 Q3	−	0	.40	0.04	0.15	-0.08
IT	2000 Q2	+	1	-.38	0.02	0.43	0.45
IT	2001 Q3	+	1	-.88	-0.04	-0.22	-0.02
IT	2003 Q4	+	0	-.38	0.16	0.76	0.70
IT	2014 Q2	+	0	-.88	0.25	0.38	0.16
IT	2018 Q3	−	0		0.04	-0.12	-0.17
NL	2015 Q3	−	1	.25	-0.06	-0.14	-0.14
PT	1999 Q2	+	1	.00	0.21	-0.15	-0.21
PT	2001 Q3	−	1	.00	0.06	-0.67	-0.66
PT	2007 Q2	+	1	-.63	0.50	0.72	0.85

The table shows the employment response after 40 quarters for the main estimates, the model combining regular and temporary contract EPL and the model estimated separately for low and high states of euro area GDP growth, presented in section 4.3. 'Sign' is the direction of the event with a + indicating a deregulation, 'State' is state of euro area GDP growth as defined for the state dependent estimates. 0 and 1 indicate low and high states, respectively. The remaining columns show the median employment response after 40 quarters. For Italy 2018 Q3 different versions of the OECD EPL indicators give grossly different numbers, the entry is therefore excluded in calculating the correlations in Table 3.

Annex C: The Narrative Indicators

EPL on Regular Contracts

2004 Q1 Germany (−)

Reform of the legislation on "socially justified" dismissals

2001 Q1 Spain (+)

Liberalisation of court procedures for dismissals with an aim to reduce severance payments of firms from 45 days to 20 days of the wage. A capital-funded component of severance payments is introduced to further reduce the one-time costs of dismissal.

2010 Q3 Spain (+)

Reduction of the upper range of severance payments for permanent contracts. Severance payments are cut by about one half, while a capital-funded component is introduced, further reducing the one time costs of dismissal. Conditions for dismissals to be accepted by the courts are eased.

2012 Q3 Spain (+)

Reforms aim at reducing the duality in the Spanish labour market. New regulations eliminate the need for administrative authorisation of collective dismissal and introduce a new type of permanent contract for companies with fewer than 50 employees with an extended trial period.

2008 Q2 France (+)

The layoff law is simplified by introducing the possibility of mutually agreed termination.

2013 Q2 France (+)

Simplification of collective layoff procedures in case of proven economic difficulties. The law also allows for temporary suspension of contracts or collective agreements.

2017 Q3 France (+)

Major reforms aim at increasing incentives to hire on permanent contracts: they clarify the definition of real and serious cause for the dismissal on economic grounds of employees on permanent contracts, and aimed to improve the predictability of compensation in the event of unfair dismissal.

2006 Q3 Ireland (+)

Revision of the 1973 Minimum Notice and Terms of Employment Act, which had introduced and defined minimum notice period for dismissal. The act now applies also to employees in the Civil Service.

2012 Q1 Ireland (−)

Revocation of the government rebate paid to employers for redundancy payouts to employees. The rebate is cut from 60% to 15%.

2012 Q3 Italy (+)

Relaxation of employment protection rules to increase incentives to hire on permanent contacts, in particular limiting the possibility of reinstatement following unfair dismissal.

2003 Q4 Portugal (+)

Replacing individual and collective labour legislation with a unified text. Employers get the right to oppose the reinstatement of workers in dismissal cases under certain conditions. Liberalisation of the procedures for collective dismissal with shortened time frames and elimination of priorities given to worker representatives.

2011 Q4 Portugal (+)

Reduction in severance pay from 30 to 20 days per year of tenure. Elimination of the distinction between severance pay for expiry of the employment contract and for lawful termination of the employment contract, resulting in further reductions in severance pay.

2013 Q4 Portugal (+)

Reduction in severance pay from 20 to 12 days per year of tenure. Instalment of a capital-funded guarantee system for severance pay.

EPL on Temporary Contracts

2002 Q1 Germany (+)

New legislation for temporary work agreements came into force in January 2002 extending the maximal continuous time period of sub-contracts with the same user enterprise from 12 to 24 months.

2003 Q1 Germany (+)

The Hartz I-II reforms create new opportunities for temporary work.

2011 Q4 Germany (-)

Tightening regulation of temporary agency workers.

2017 Q1 Germany (−)

The duration of employment on jobs filled by temporary work agency workers is limited to 18 months.

2010 Q3 Spain (+)

The reform focuses on facilitating intermediation in the job market by authorizing not-for-profit matching agencies to operate and eliminating operational restrictions placed on Temporary Employment Agencies. This seeks to increase the effectiveness of the matching process.

1999 Q1 Finland (−)

Job protection raised for atypical work forms

2003 Q4 Greece (+)

The renewal of a fixed term contract is permitted without any limitation if it is justified by certain reasons.

2003 Q3 Ireland (-)

The Protection of Employees Act tightens the restrictions on use of temporary contracts.

2000 Q2 Italy (+)

New regulations for part-time work, targeted for less than 40 working hours per week. Any change in working hours of part-time contracts is allowed under the agreement of social actors (the so-called elastic or flexible clauses). The act also introduces regulations for additional working hours on part-time jobs.

2001 Q4 Italy (+)

New regulations facilitate the hiring of fixed-time work and reduces sanctions for breaches of the law. A contract renewal is possible only if the original contract lasts three years, and the renewal must last three years as well.

2003 Q4 Italy (+)

Reforms with the objective of increasing employment among youth, women, and older workers by improving conditions for non-standard forms of employment, such as flexible job contracts related to specific projects and occasional work, incl. part-time employment.

2014 Q2 Italy (+)

Reforms facilitate the use of fixed-term contracts through abolishment of the need for justifying reasons and an increase in the maximum number of extensions from 1 to 5; by shortening the interval between two consecutive fixed term contracts with the same employer; and by allowing collective agreements to extend the length of fixed-term contracts above the statutory maximum of 36 months.

2018 Q3 Italy (−)

Various restrictions on temporary contracts are imposed. Contracts with a duration of longer than 12 month are allowed only in cases of temporary needs unrelated to ordinary administration, replacements, and unforeseeable temporary and significant increments in demand. Further, the maximum number of renewals is reduced from 5 to 4 and social security contributions on temporary contracts are raised.

2015 Q3 Netherlands (−)

Reforms to increase the protection of employees on temporary contracts and to reduce the protection of permanent contracts. The duration of consecutive temporary contracts is shortened from 3 to 2 years and the period between two consecutive contracts extended from 3 to 6 months.

1999 Q2 Portugal (+)

The need for greater flexibility of labour is recognized by the "Short-term Social Pact" of1996 had called, inter alia, for a more flexible organization of working time, largely based upon reduced job demarcation. The Social Pact was transposed into law in 1999.

2001 Q2 Portugal (−)

New legislation for fixed-term employment contracts to ensure that workers who, de facto, are employed on permanent jobs would benefit from a standard permanent contract. The regulations impose a set of rules to restrict the scope and termination conditions of temporary contracts.

2003 Q4 Portugal (+)

New regulations provide more flexibility in the use of fixed-term contracts, which can now be renewed up to a maximum six years (instead of three years previously); at the same time it gives more transparency to these forms of contracting.

Unemployment Benefit Reforms

2006 Q1 Germany (+)

The maximum duration of unemployment benefits is reduced from 32 months to 12 months (18 months for unemployed aged 55 or older). Subsequent welfare benefits are made subject to tighter eligibility requirements.

2010 Q1 Ireland (+)

Unemployment benefits and other social welfare payments reduced by 4.1%.

2011 Q1 Ireland (+)

Job seekers' allowances are reduced by about 25% for unemployed aged between 22 and 26. Higher rates are applied if the jobseekers return to education or training.

2015 Q1 Italy (−)

Increase in the coverage and duration of unemployment benefits. Duration is made dependent on the number of weeks of contributions over a longer reference period and is extended to project workers. The reforms are combined with a liberalisation of EPL and temporary cuts to employers' social security contributions in a "flexicurity" approach.

2006 Q4 Netherlands (+)

The maximum duration of unemployment benefit is lowered from 60 to 38 months.

2016 Q1 Netherlands (+)

Reduction of the duration of unemployment benefits as a function of the contribution period with a maximum duration of unemployment benefits of 2 years.

Notes

More detailed descriptions of the individual events together with references are provided by Aumond, Di Tommaso, and Rünstler (2021). Various reforms in the original database have been removed for the present study for different reasons.

- As noted in the main text, I exclude three EPL reforms in Greece in between 2010 and 2012 and an UB reform in Portugal in 2012, as these events interfere with cuts in either public sector or minimum wages in the same or subsequent quarter in the course of fiscal consolidation programmes.
- Further, among regular contract EPL reforms, an event in Portugal in 2009 Q1 was removed as it coincides with the exceptionally large macroeconomic shock at the onset of the 2009 financial crisis. Another reform in Austria in 2003 Q1 was removed, because it coincides with a structural break in the employment and active population series due to a change in data collection procedures.
- Three reforms in the original database, i.e. 1998 Q3 in the Netherlands, 2003 Q1 in Portugal, and 2010 Q4 in Spain, combine legislation on both regular and temporary contracts. In all cases, legislative changes are more substantive on regular contracts and I therefore classify these reforms accordingly.
- Similarly, a reform in Italy in 2015 Q1 combines an increase in unemployment benefits with a liberalisation in regular contract EPL. Preliminary estimates suggest that the first effect is more relevant and I classify it accordingly.

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